TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

EDINBURGH.

VOL. XXIV.

EDINBURGH:
PUBLISHED BY ROBERT GRANT & SON, 82 PRINCES STREET.
AND WILLIAMS & NORGATE, 14 HENRIETTA STREET, COVENT GARDEN, LONDON.

MDCCCLXVII.

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THE KEITH, BRISBANE, AND NEILL PRIZES.

The above Prizes will be awarded by the Council in the following manner:—

I. KEITH PRIZE.

The Keith Prize, consisting of a Gold Medal and from £40 to £50 in Money, will be awarded in the Session 1867–68, for the "best communication on a scientific subject, communicated, in the first instance, to the Royal Society during the Sessions 1865–66 and 1866–67." Preference will be given to a paper containing a discovery.

II. MAKDOUGALL BRISBANE PRIZE.

This Prize is to be awarded biennially by the Council of the Royal Society of Edinburgh to such person, for such purposes, for such objects, and in such manner as shall appear to them the most conducive to the promotion of the interests of science; with the *proviso* that the Council shall not be compelled to award the Prize unless there shall be some individual engaged in scientific pursuit, or some paper written on a scientific subject, or some discovery in science made during the biennial period, of sufficient merit or importance in the opinion of the Council to be entitled to the Prize.

1. The Prize, consisting of a Gold Medal and a sum of Money, will be awarded at the commencement of the Session 1868–69, for an Essay or Paper having reference to any branch of scientific inquiry, whether Material or Mental.

2. Competing Essays to be addressed to the Secretary of the Society, and transmitted not later than 1st June 1868.

3. The competition is open to all men of science.

4. The Essays may be either anonymous or otherwise. In the former case, they must be distinguished by mottoes, with corresponding sealed billets superscribed with the same motto, and containing the name of the Author.

5. The Council impose no restriction as to the length of the Essays, which may be, at the discretion of the Council, read at the Ordinary Meetings of the Society. They wish also to leave the property and free disposal of the manuscripts to the Authors; a copy, however, being deposited in the Archives of the Society, unless the Paper shall be published in the Transactions.

6. In awarding the Prize, the Council will also take into consideration any scientific papers presented to the Society during the Sessions 1866–67 and 1867–68, whether they may have been given in with a view to the Prize or not.

III. NEILL PRIZE.

The Council of the Royal Society of Edinburgh having received the bequest of the late Dr Patrick Neill of the sum of £500, for the purpose of "the interest thereof being applied in furnishing a Medal or other reward every second or third year to any distinguished Scottish Naturalist, according as such Medal or reward shall be voted by the Council of the said Society," hereby intimate,

1. The Neill Prize, consisting of a Gold Medal and a sum of Money, will be awarded at the commencement of the Session 1868–69.

2. The Prize will be given for a Paper of distinguished merit, on a subject of Natural History, by a Scottish Naturalist, which shall have been presented to the Society during the three years preceding the 1st May 1868,—or failing presentation of a Paper sufficiently meritorious, it will be awarded for a work or publication by some distinguished Scottish Naturalist, on some branch of Natural History, bearing date within five years of the time of award.

AWARDS OF THE KEITH, MAKDOUGALL BRISBANE, AND NEILL PRIZES, SINCE 1862.

AWARD OF THE KEITH PRIZE.

19th Biennial Period, 1863–65. Principal Forbes, St Andrews, for his "Experimental Inquiry into the Laws of Conduction of Heat in Iron Bars," published in the Transactions of the Society.

MAKDOUGALL BRISBANE PRIZE.

4th Biennial Period, 1864–66. Not awarded.

AWARD OF THE NEILL PRIZE.

3d Triennial Period, 1862–65. Andrew Crombie Ramsay, F.R.S., Professor of Geology in the Government School of Mines, and Local Director of the Geological Survey of Great Britain, for his various works and memoirs published during the last five years, in which he has applied the large experience acquired by him in the Direction of the arduous work of the Geological Survey of Great Britain to the elucidation of important questions bearing on Geological Science.

[illegible]

[illegible] Ramsay, F.R.S., [illegible]

[illegible] the Geological Survey of Great [illegible]

works and [illegible]

which he has applied [illegible]

him in the [illegible]

survey of Great Brit[illegible]

question bearing on Geological [illegible]

DIRECTIONS TO THE BINDER FOR PLACING THE PLATES IN THIS VOLUME.

LAWS

OF THE

ROYAL SOCIETY OF EDINBURGH,

AS REVISED 31st OCTOBER 1866.

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

LAWS.

[By the Charter of the Society (printed in the *Transactions*, Vol. VI. p. 5), the Laws cannot be altered, except at a Meeting held one month after that at which the Motion for alteration shall have been proposed.]

I.

THE ROYAL SOCIETY OF EDINBURGH shall consist of Ordinary and Honorary Fellows. Title.

II.

Every Ordinary Fellow, within three months after his election, shall pay Two Guineas as the fee of admission, and Three Guineas as his contribution for the Session in which he has been elected; and annually at the commencement of every Session, Three Guineas into the hands of the Treasurer. This annual contribution shall continue for ten years after his admission, and it shall be limited to Two Guineas for fifteen years thereafter.* The fees of Ordinary Fellows residing in Scotland.

III.

All Fellows who shall have paid Twenty-five years' annual contribution shall be exempted from farther payment. Payment to cease after 25 years.

IV.

The fees of admission of an Ordinary Non-Resident Fellow shall be £26, 5s., payable on his admission; and in case of any Non-Resident Fellow coming to reside at any time in Scotland, he shall, during each year of his residence, pay the usual annual contribution of £3, 3s., payable by each Resident Fellow; but after payment of such annual contribution for eight years, he shall be exempt from any farther payment. In the case of any Resident Fellow ceasing to reside in Scot- Fees of Non-Resident Ordinary Fellows. Case of Fellows becoming Non-Resident.

* At the Meeting of the Society, on the 5th January 1857, when the reduction of the Contributions from £3, 3s., to £2, 2s., from the 11th to the 25th year of membership, was adopted, it was resolved that the existing Members shall share in this reduction, so far as regards their future Annual Contributions.

A modification of this rule, in certain cases, was agreed to 3d January 1831.

land, and wishing to continue a Fellow of the Society, it shall be in the power of the Council to determine on what terms, in the circumstances of each case, the privilege of remaining a Fellow of the Society shall be continued to such Fellow while out of Scotland.

V.

Defaulters.

Members failing to pay their contributions for three successive years (due application having been made to them by the Treasurer) shall be reported to the Council, and, if they see fit, shall be declared from that period to be no longer Fellows, and the legal means for recovering such arrears shall be employed.

VI.

Privileges of Ordinary Fellows.

None but Ordinary Fellows shall bear any office in the Society, or vote in the choice of Fellows or Office-Bearers, or interfere in the patrimonial interests of the Society.

VII.

Numbers Unlimited.

The number of Ordinary Fellows shall be unlimited.

VIII.

Fellows entitled to Transactions.

The Ordinary Fellows, upon producing an order from the TREASURER, shall be entitled to receive from the Publisher, gratis, the Parts of the Society's Transactions which shall be published subsequent to their admission.

IX.

Mode of Recommending Ordinary Fellows.

No person shall be proposed as an Ordinary Fellow without a recommendation subscribed by *One* Ordinary Fellow, to the purport below.* This recommendation shall be delivered to the Secretary, and by him laid before the Council, and shall afterwards be printed in the circulars for three Ordinary Meetings of the Society, previous to the day of the election, and shall lie upon the table during that time.

X.

Honorary Fellows, British and Foreign.

Honorary Fellows shall not be subject to any contribution. This class shall

* "A. B., a gentleman well skilled in several branches of Science (*or Polite Literature, as the case* "*may be*), being to my knowledge desirous of becoming a Fellow of the Royal Society of Edin- "burgh, I hereby recommend him as deserving of that honour, and as likely to prove a useful and "valuable Member."

This recommendation to be accompanied by a request of admission signed by the Candidate.

consist of persons eminently distinguished for science or literature. Its number shall not exceed Fifty-six, of whom Twenty may be British subjects, and Thirty-six may be subjects of foreign states.

XI.

Royal Personages.

Personages of Royal Blood may be elected Honorary Fellows, without regard to the limitation of numbers specified in Law X.

XII.

Recommendation of Honorary Fellows.

Honorary Fellows may be proposed by the Council, or by a recommendation (in the form given below*) subscribed by three Ordinary Fellows; and in case the Council shall decline to bring this recommendation before the Society, it shall be competent for the proposers to bring the same before a General Meeting. The election shall be by ballot, after the proposal has been communicated *viva voce* from the Chair at one meeting, and printed in the circular for the meeting at which the Ballot is to take place.

Mode of Election.

XIII.

Election of Ordinary Fellows.

The election of Ordinary Fellows shall take place at the Ordinary Meetings of the Society. The election shall be by ballot, and shall be determined by a majority of at least two-thirds of the votes, provided Twenty-four Fellows be present and vote.

XIV.

Ordinary Meetings.

The Ordinary Meetings shall be held on the first and third Mondays of every month from November to June inclusive. Regular Minutes shall be kept of the proceedings, and the Secretaries shall do the duty alternately, or according to such agreement as they may find it convenient to make.

XV.

The Transactions.

The Society shall from time to time publish its Transactions and Proceedings. For this purpose the Council shall select and arrange the papers which they shall

* We hereby recommend ______________________________
for the distinction of being made an Honorary Fellow of this Society, declaring that each of us from our own knowledge of his services to (*Literature or Science, as the case may be*) believe him to be worthy of that honour.

(To be signed by three Ordinary Fellows.)

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____

To the President and Council of Royal Society of Edinburgh.

deem it expedient to publish in the *Transactions* of the Society, and shall superintend the printing of the same.

XVI.

How Published.

The Transactions shall be published in Parts or ***Fasciculi*** at the close of each Session, and the expense shall be defrayed by the Society.

The Council.

There shall be elected annually, for conducting the publications and regulating the private business of the Society, a Council, consisting of a President; Six Vice-Presidents, two at least of whom shall be resident; Twelve Councillors, a General Secretary, Two Secretaries to the Ordinary Meetings, a Treasurer, and a Curator of the Museum and Library.

XVII.

Retiring Councillors.

Four Councillors shall go out annually, to be taken according to the order in which they stand on the list of the Council.

XVIII.

Election of Office-Bearers.

An Extraordinary Meeting for the Election of Office-Bearers shall be held on the fourth Monday of November annually.

XIX.

Special Meetings; how called.

Special Meetings of the Society may be called by the Secretary, by direction of the Council; or on a requisition signed by six or more Ordinary Fellows. Notice of not less than two days must be given of such Meetings.

XX.

Treasurer's Duties.

The Treasurer shall receive and disburse the money belonging to the Society, granting the necessary receipts, and collecting the money when due.

He shall keep regular accounts of all the cash received and expended, which shall be made up and balanced annually; and at the Extraordinary Meeting in November, he shall present the accounts for the preceding year, duly audited. At this Meeting, the Treasurer shall also lay before the Council a list of all arrears due above two years, and the Council shall thereupon give such directions as they may deem necessary for recovery thereof.

XXI.

Auditor.

At the Extraordinary Meeting in November, a professional accountant shall be chosen to audit the Treasurer's accounts for that year, and to give the necessary discharge of his intromissions.

XXII.

The General Secretary shall keep Minutes of the Extraordinary Meetings of the Society, and of the Meetings of the Council, in two distinct books. He shall, under the direction of the Council, conduct the correspondence of the Society, and superintend its publications. For these purposes, he shall, when necessary, employ a clerk, to be paid by the Society. General Secretary's Duties.

The Secretaries to the Ordinary Meetings shall keep a regular Minute-book, in which a full account of the proceedings of these Meetings shall be entered; they shall specify all the Donations received, and furnish a list of them, and of the donors' names, to the Curator of the Library and Museum: they shall likewise furnish the Treasurer with notes of all admissions of Ordinary Fellows. They shall assist the General Secretary in superintending the publications, and in his absence shall take his duty. Secretaries to Ordinary Meetings.

XXIII.

The Curator of the Museum and Library shall have the custody and charge of all the Books, Manuscripts, objects of Natural History, Scientific Productions, and other articles of a similar description belonging to the Society; he shall take an account of these when received, and keep a regular catalogue of the whole, which shall lie in the Hall, for the inspection of the Fellows. Curator of Museum and Library.

XXIV.

All Articles of the above description shall be open to the inspection of the Fellows at the Hall of the Society, at such times and under such regulations, as the Council from time to time shall appoint. Use of Museum and Library.

XXV.

A Register shall be kept, in which the names of the Fellows shall be enrolled at their admission, with the date. Register Book

XVII.

[illegible] Minutes of the Extraordinary Meetings of [illegible]
[illegible] and [illegible] the Council, in the [illegible]
[illegible] correspondence of the [illegible]
[illegible] publications. For these purposes, as [illegible]
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[illegible] to the Ordinary Meetings shall keep a regular [illegible] which, in [illegible]
[illegible] they
[illegible] and furnish a list of [illegible] of
[illegible] of the Library and Museum: they shall [illegible]
[illegible] with notes of all admissions of Ordinary Fellows. They
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[illegible] duties.

XVIII.

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[illegible], Manuscripts, objects of Natural History, Scientific Productions, and
[illegible] to the Society; he shall take [illegible]
[illegible] when received, and keep a regular catalogue of the whole, which
[illegible] the Hall, for the inspection of the Fellows.

XIX.

[illegible] open to the inspection of the [illegible]
[illegible] Hall of the Society [illegible]
[illegible] from time to time shall [illegible]

XX.

[illegible] Fellows shall be entitled [illegible]
[illegible]

LIST OF THE ORDINARY FELLOWS OF THE SOCIETY.

N.B.—*Those marked * are Annual Contributors.*

1846 *Alex. J. Adie, Esq., Rockville, Linlithgow
1866 *Col. Sir James E. Alexander of Westerton
1867 *Rev. Dr W. Lindsay Alexander, 17 Brown Square
1848 Dr James Allan, Inspector of Hospitals, Portsmouth
1856 *Dr G. J. Allman (SECRETARY), Professor of Natural History, 21 Manor Place
1849 *David Anderson, Esq., Moredun, Edinburgh
1845 *Dr Thomas Anderson, Prof. Chemistry, Univ., Glasgow
1823 Warren Hastings Anderson, Esq., Isle of Wight
1867 *Thomas Annandale, Esq., 3 Hope Street
1840 James Anstruther, Esq., W.S. 10
1862 *T. C. Archer, Esq., Director of the Museum of Science and Art, 9 Argyle Square
1849 *His Grace the Duke of Argyll (HON. VICE-PRESIDENT), Inverary Castle
1822 Dr G. Walker Arnott, Prof. Botany, Univ., Glasgow
1820 Charles Babbage, K.H., London
1843 David Balfour, Esq., Trenaby
1835 Dr J. H. Balfour (GENERAL SECRETARY), Professor of Medicine and Botany, 27 Inverleith Row
1867 *George F. Barbour, Esq., 11 George Square
1862 *Hon. Lord Barcaple, 3 Ainslie Place
1830 Dr Thomas Barnes, Carlisle
1858 Edmund Chisholm Batten, M.A., Lincoln's Inn, London 20
1844 *Dr Begbie, 10 Charlotte Square
1843 Dr Bennett, Professor of Institutes of Medicine, 1 Glenfinlas Street
1861 *George Berry, Esq., 2 Windsor Terrace, Portobello
1866 *Adam Black, Esq., 38 Drummond Place
1850 *Hugh Blackburn, Esq., Prof. Mathematics, University, Glasgow.
1863 *Professor Blackie, 24 Hill Street
1857 *John Blackwood, Esq., 3 Randolph Crescent
1862 *Rev. Dr W. G. Blaikie, Pilrig Manse
1854 Ernest Bonar, Esq.
1863 *William Brand, Esq., 5 Northumberland Street 30
1808 Principal Sir D. Brewster, K.H., (PRESIDENT), College
1864 *Dr Alex. Crum Brown, 4 Rillbank Terrace
1859 *Dr John Brown, 23 Rutland Street
1861 *Rev. Thomas Brown, 16 Carlton Street
1835 William Brown, Esq., 25 Dublin Street
1861 *W. A. F. Browne, Esq., Post-Office Buildings
1867 *A. H. Bryce, Esq., D.C.L., LL.D., 13 Salisbury Road
1856 *David Bryce, Esq., Architect, 131 George Street
1833 His Grace the Duke of Buccleuch, K.G., Dalkeith Palace
1857 *Dr W. M. Buchanan, 3 Carlton Terrace 40
1845 *Dr Burt, 88 George Street
1847 *J. H. Burton, LL.D., Advocate, Craig House
1863 *Robert Campbell, Esq., Advocate
1865 *Alfred R. Catton, B.A., College
1866 *David Chalmers, Esq., Kate's Mill, Slateford
1840 Robert Chambers, LL.D., St Andrews
1860 *William Chambers, Esq. of Glenormiston, 13 Chester Street
1862 *Henry Cheyne, Esq., W.S., 6 Royal Terrace
1823 Dr Christison, Professor of Materia Medica (VICE-PRESIDENT), 40 Moray Place
1863 Dr H. F. C. Cleghorn, Madras 50
1856 *Thomas Cleghorn, Esq., Advocate, 26 Queen Street
1812 Right Hon. Sir George Clerk, Bart., Penicuik House
1844 Dr Thomas R. Colledge, Lauriston House, Cheltenham
1829 The Right Honourable Lord Colonsay, London
1829 A. Colyar, Esq.
1850 *Dr James Scarth Combe, 36 York Place
1866 *Thomas Constable, Esq., 11 Thistle Street
1843 Dr John Rose Cormack, Orleans, France.
1843 Andrew Coventry, Esq., Advocate, 29 Moray Place
1863 *Charles Cowan, Esq., Valleyfield, Penicuik 60
1854 *Sir James Coxe, M.D., Kinellan
1830 J. T. Gibson-Craig, Esq., W.S., 24 York Place
1829 Sir William Gibson-Craig, Bart., Riccarton
1853 Rev. John Cumming, D.D., London
1852 *James Cunningham, Esq., W.S., 50 Queen Street
1823 Liscombe J. Curtis, Esq., Ingsdown House, Devonshire
1851 *E. W. Dallas, Esq., 125 Princes Street
1841 James Dalmahoy, Esq., 9 Forres Street
1852 *Allen Dalzell, M.D., The Lodge, North Berwick
1862 *Nicholas Alexander Dalzell, Esq., Bombay 70
1867 *David Davidson, Esq., Bank of Scotland
1848 *Henry Davidson, Esq., Muirhouse
1842 Dr John Davy, Lesketh How, Ambleside
1867 Henry Dircks, Esq., C.E., London
1867 *Francis Deas, Esq., LL.B., Advocate, 32 Heriot Row
1863 *W. Dittmar, Esq., College
1867 *James Donaldson, Esq., LL.D., 8 Mayfield Street
1866 *David Douglas, Esq., 11 Salisbury Road
1839 Francis Brown Douglas, Esq., Advocate, 21 Moray Pl.
1867 *G. Stirling Home Drummond, Blair-Drummond 80
1860 *Patrick Dudgeon, Esq. of Cargen
1863 *Dr J. Matthews Duncan, 30 Charlotte Square
1851 *Sir David Dundas, Bart. of Dunira
1863 *The Right Hon. Lord Dunfermline, Colinton House
1859 *Rev. Dr John Duns, 2 Mansion-House Road, Grange
1866 *Dr James Dunsmure, 53 Queen Street
1864 *Professor Robert Dyce, Aberdeen
1856 *W. Mitchell Ellis, Esq., Wellington Lodge, Portobello
1855 Robert Etheridge, Esq., Clifton, Bristol
1866 *William Euing, Esq., Glasgow 90
1863 *J. D. Everett, Esq., M.A., Glasgow
1866 *James Falshaw, Esq., C.E., 26 Castle Street

1859 *Dr Fayrer, Professor of Surgery, Calcutta
1858 Frederick Field, Esq., Chili
1852 Dr Andrew Fleming, H.M.I.S., Bengal
1831 Principal Forbes (VICE-PRESIDENT), St Andrews
1859 Major James George Forlong, Bombay
1828 John Forster, Esq., Liverpool
1864 *Dr John Foulerton, Manila
1858 *Professor Fraser, 20 Chester Street 100
1867 *Dr Thomas R. Fraser, College
1867 *Frederick Fuller, Esq., Prof. Math., Univ., Aberdeen
1867 Dr Charles Gainer, Oxford
1867 *Dr Arthur Gamgee, 27 Alva Street
1861 *Archibald Geikie, Esq., 16 Duncan Street, Newington
1845 *L. D. B. Gordon, Esq., C.E., London
1850 *Lieut-Col. W. D. Gosset, R.E.
1867 *Dr Andrew Graham, R.N., 35 Melville Street
1851 *Rev. Dr James Grant, 18 Great King Street
1824 Dr Robert E. Grant, Prof. Comp. Anat., Univ. Coll., London 110
1860 *Dr Frederick Guthrie, M.A., Prof. of Chemistry, Roy. Coll., Mauritius
1867 *Dr D. R. Haldane, 22 Charlotte Square
1867 *Frederick Hallard, Esq., Advocate, 7 Whitehouse Terrace
1867 *James H. B. Hallen, Esq.
1833 Alexander Hamilton, LL.B., W.S., The Elms, Whitehouse Loan
1824 Dr Robert Hamilton, 11 North Merchiston Place
1837 Dr P. D. Handyside, 11 Hope Street
1864 *Rev. Dr Hannah, Glenalmond
1854 Professor Robert Harkness, Queen's College, Cork
1867 *Sir George Harvey, 21 Regent Terrace 120
1859 *G. W. Hay, Esq. of Whiterigg
1855 *James Hay, Esq., 5 Links Place, Leith
1862 *Dr James Hector, New Zealand
1854 Dr William Bird Herapath, Bristol
1859 Lieut. John Hills, Bombay Engineers
1828 David Milne Home, Esq. of Wedderburn (VICE-PRESIDENT), 10 York Place
1839 Dr Adam Hunter, 18 Abercromby Place
1864 *Robert Hutchison, Esq., Carlowrie Castle
1855 *The Right Hon. John Inglis, Lord Justice-General, 30 Abercromby Place
1858 *Professor Innes (VICE-PRESIDENT), Inverleith House 130
1840 Edward J. Jackson, Esq., 6 Coates Crescent
1863 William Jameson, Esq., Surgeon-Major, Saharunpore
1860 *George A. Jamieson, Esq., 58 Melville Street
1825 Sir William Jardine, Bart., LL.D., of Applegarth, Jardine Hall, Lockerby
1865 *Charles Jenner, Esq., Easter Duddingston Lodge
1863 *Hon. Charles Baillie, LL.D., Lord Jerviswoode, 10 Strathearn Road
1850 *Alex. K. Johnston, LL.D., March-Hall Park, Dalkeith Road
1867 *T. B. Johnston, Esq., 9 Claremont Crescent
1867 *William Keddie, Esq., Glasgow.
1866 *Dr Alexander Keiller, 21 Queen Street 140
1839 Rev. Professor Kelland 20 Clarendon Crescent
1863 *Charles Lawson, Esq., 35 George Square
1865 *Charles Lawson, jun., Esq., 34 George Square
1856 *Dr Laycock, Professor of the Practice of Medicine, 13 Walker Street
1853 *Rev. Dr Robert Lee, Professor of Biblical Criticism, 24 George Square
1863 *Hon. G. Waldegrave Leslie, 4 Heriot Row
1858 *James Leslie, Esq., C.E., 2 Charlotte Square
1861 *Dr W. Lauder Lindsay, Gilgal, Perth
1864 *William Lindsay, Esq., Hermitage-Hill House, Leith
1857 Thomas Login, Esq., C.E., Pegu 150
1861 *Professor Lorimer, Advocate, 21 Hill Street
1849 *Dr W. H. Lowe, Balgreen, Slateford
1855 *Dr Stevenson Macadam, 25 Brighton Place, Portobello
1861 *Dr James M'Bain, R.N., Logie Villa, York Road, Trinity
1867 *John M. M'Candlish, Esq., 4 Doune Terrace
1866 *John M'Culloch, Esq., Banker, 11 Duke Street
1820 Dr Wm. Macdonald, Prof. Civ. and Nat. Hist., St Andrews
1847 *W. Macdonald Macdonald, Esq., St Martins
1860 *Professor MacDougall, 9 Buckingham Terrace
1861 *Dr A. E. Mackay, R.N. 160
1840 John Mackenzie, Esq., 11 Abercromby Place
1843 Dr A. Douglas Maclagan (CURATOR), Prof. of Medical Jurisprudence, 28 Heriot Row
1853 Lieut.-Col. R. Maclagan, Royal Engineers, Bengal
1864 *Peter M'Lagan, Esq. of Pumpherston, M.P.
1866 *John Macnair, Esq., 33 Moray Place.
1840 Sir John M'Neill, G.C.B., Granton House
1834 Patrick Boyle Mure Macredie, Esq., Perceton
1858 *Dr R. B. Malcolm, 126 George Street
1838 Thomas Mansfield, Esq., 26 Abercromby Place
1828 Dr Manson, Nottingham 170
1864 *J. D. Marwick, Esq., 10 Bellevue Crescent
1866 *Professor David Masson, 3 Rosebery Crescent
1856 *James Clerk Maxwell, Esq., late Prof. Nat. Phil., King's College, London
1849 *Sir William Stirling-Maxwell, Bart., Keir, M.P.
1835 R. Mayne, Esq., 3 Merchiston Place
1863 *Edward Meldrum, Esq., Bathgate
1853 *Græme Reid Mercer, Esq., Ceylon Civil Service
1841 John Miller, Esq., C.E., 2 Melville Crescent
1818 Dr P. Miller, Exeter
1852 *Thomas Miller, Esq., A.M., LL.D., Rector, Perth Academy 180
1833 Rear-Admiral Sir Alexander Milne, R.N., Inveresk
1866 *Dr Arthur Mitchell, 6 Laverock Bank Villas
1843 *Joseph Mitchell, Esq., C.E., Inverness
1865 *Dr John Moir, 52 Castle Street
1866 *Dr Charles Morehead, 34 Melville Street
1866 *Right Rev. Bishop Morrell, Greenhill House
1861 *John Muir, D.C.L., LL.D., 16 Regent Terrace
1824 Rev. Dr William Muir, Ormslie Villa, Murrayfield
1857 *Dr John Ivor Murray, Colonial Surgeon, Hong Kong
1850 Dr Sheridan Muspratt, Liverpool 190
1842 Robert Nasmyth, Esq., 5 Charlotte Square
1856 *Hon. Lord Neaves (VICE-PRESIDENT), 7 Charlotte Square
1866 *Thomas Nelson, Esq., Abden House, Prestonfield
1847 *James Nicol, Esq., Prof. Nat. Hist., Aberdeen
1860 *Rev. Leonard Shafto Orde
1863 *Hon. Lord Ormidale, 14 Moray Place
1863 *David Page, LL.D., 44 Gilmore Place

1837 Dr Richard Parnell, 7 James' Place, Leith
1863 *Dr Alexander Peddie, 15 Rutland Street
1856 *Dr Penny, Glasgow 200
1849 *W. Pirrie, Esq., Prof. Surg., Marischal Coll., Aberdeen
1859 *Dr Lyon Playfair, C.B., LL.D. (VICE-PRESIDENT), Prof. Chemistry, 14 Abercromby Place
1834 Mungo Ponton, Esq., W.S., Clifton, Bristol
1852 Eyre B. Powell, Esq., Madras
1865 *James Powrie, Esq., Reswallie, Forfar
1849 *Hon. B. F. Primrose, 22 Moray Place
1827 Very Rev. E. B. Ramsay, LL.D., 23 Ainslie Place
1850 *W. J. M. Rankine, Esq., C.E., Prof. Civil Engineering, University, Glasgow
1865 *Rev. Francis Redford, M.A., Silloth
1836 David Rhind, Esq., 54 Great King Street 210
1867 *James Richardson, Esq., 16 Coates Crescent
1859 Professor Richardson, Durham
1818 William Richardson, Esq., Cheltenham
1840 Martyn J. Roberts, Esq., Crickhowell, South Wales
1859 *George Robertson, Esq., C.E., 47 Albany Street
1832 Dr Montgomery Robertson, Mortlake, Surrey
1860 *Dr William Robertson, 28 Albany Street
1862 *Dr E. Ronalds, Bonnington Bank
1852 *Alex. James Russell, Esq., C.S., 9 Shandwick Place
1837 J. Scott Russell, Esq., 5 Westminster Chambers, London 220
1859 *Robert Russell, Esq., Pilmuir, Leven, Fife
1863 *James Sanderson, Esq., Surgeon-Major, 17 Claremont Crescent
1864 *Rev. D. F. Sandford, 19 Rutland Street
1849 *Edward Sang, Esq., 2 George Street
1846 *Dr Schmitz, London
1853 *Hugh Scott, Esq. of Gala, Galashiels
1840 Sir William Scott, Bart., Ancrum
1864 *Professor Sellar, 15 Buckingham Terrace
1850 *Dr William Seller, 18 Northumberland Street
1834 Dr Sharpey, Prof. Anatomy, Univ. Coll., London 230
1844 *Sir James Y. Simpson, Bart., Prof. of Midwifery, 52 Queen Street
1829 Ven. Archdeacon Sinclair, Kensington
1859 *William F. Skene, LL.D., W.S., 20 Inverleith Row
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TRANSACTIONS.

I.—*On the Principle of Onomatopœia in Language.* By Professor BLACKIE.

(Read 19th December 1864.)

By ὀνοματοποιΐα the Greek grammarians understood that principle, or tendency in the growth of language, according to which certain words are formed by an imitation of the sounds which they signify. Thus, ὀγκ, the root of the Greek word ὀγκᾶσθαι, to *bray*, may be considered to have been formed of a human mimicry of that animal to which human beings of the lowest cerebral capacity are peculiarly compared; and in the same way, *laogh*, the Gaelic for a *calf*, seems to contain a sound to which only the throats of Highland calves, Highland chieftains, and Highland crofters are competent. The word *onomatopœia*, like some other technical terms of the old grammarians, is not particularly happy, for it means only and generally *word-making*, or rather *name-making*, and says nothing of the principle by which the special class of words in question is made. Instead of this term, therefore, I should prefer to speak of the imitative or pictorial principle in the formation of human speech; and I should contrast the whole class of words in which the operation of this principle can be traced, with another class, derived from ideas or notions about the thing to be named in the mind of the word-maker. Thus, the modern Greeks call a cock πετεινό, that is, the *fowl*, or *flying* animal, from πέτομαι, to fly; and the Latin word, *equus*, a horse, if it comes, as Professor MÜLLER says, from the Sanscrit root *â'su*, *swift*, will be another word formed on the same principle. The roots of these words I propose to call notional roots, as contrasted with the onomatopœtic class of roots, which I propose to call pictorial roots, or roots formed by *phonic imitation.*

Professor MÜLLER, in his valuable work on the Science of Language, has, in both volumes, either denied altogether the existence of this class of words, or treated them with such marked disfavour, that in his system they do not appear at all as effective agents in the formation of reasonable speech, but merely play a subordinate and scarcely human part in the precincts of the poultry-yard and the pig-sty. If, in the central table-land of Asia, before the divarication of the great Aryan races, a Persian pig gave a grunt, the learned Professor might perhaps be willing to admit, or might be forced to admit, that there was some connection in the way of mimetic reproduction between the sound uttered by that animal and the words γρύζω in Greek, *grunnio* in Latin, *grunt* in English, and *grumphie* in Scotch. If, when the sacred chickens were observed by the Roman augurs in their cages to give forth an attenuated indication of the approaching fates, according to their vocal capacity, and if the speakers of the Latin dialect of the Aryan family agreed to designate the sound then emitted by the root *pipi*, familiarly known as a verb of the fourth conjugation, *pipire*, with the variety *pipilare*, applied to sparrows—in this case also, we presume, those who disown the pictorial principle would be inclined to concede some pretty mimicry of the small unreasoning by the great reasoning animal. Or, to take an example from an altogether different quarter, in the word "*chirumvurumvuru*, used by the Africans on the Zambesi river, to designate a sudden violent tornado, with lightning, thunder, and rain, who can refuse to recognise a beautiful imitation of the long-continued roll of peals of thunder in a mountain district?"* But then they would say that in forming such words a man acts as a parrot and not as a man; and in the philosophy of human speech we can take no account of an element which denies the distinctive character—namely, reason—of the being who forms it. It is against this view of the part played by the imitative principle of our nature in the formation of language that I now submit a few observations.

In treating this matter, I shall first state the arguments in favour of the extensive operation of this principle, which appear to me conclusive, and then shortly consider the nature of the objections that have been brought against it. But, before making a regular muster of the arguments for or against any position, it appears to me to be of the utmost consequence to see how the presumptions lie. When a man is tried before a jury for a special act of felonious appropriation, the fact that he is *habit* and *repute* a *thief*, although no part of the evidence on which he can be convicted, will certainly operate against him to some extent in the minds of the most impartial jury. In the same way, it must have been observed that in the discussion of the most famous literary, scientific, and philosophical questions, there is an under-current of presumption of some kind or other, which secretly determines which side the reasoner will take, more powerfully than all the arguments that are articulately brought forward,—a presumption of which these arguments are sometimes only the servile satellites. So, in the present case, I ask, first, is there any presumption why words should not be formed by the human voice, in imitation of certain sounds emitted by or connected with objects in the external world? Man has, no doubt, been well defined a reasonable, or at least a reasoning animal; but he is no less truly, and no less largely, an imitative animal. It may be said that there are more persons in the world who can give true pictures of things by word or line, than there are who can argue

* On the Zambesi, Notes of a long Journey. By JAMES STEWART. (Good Words, Feb. 1865.)

about them soundly; for one instance of false portraiture in common conversation, you shall have a hundred exhibitions of bad logic. From the earliest words and actions of the child to the ripest productions of dramatic genius, you have the principle of imitation constantly and intensely at work. Many a literary reputation, exercising a powerful sway over thousands and tens of thousands of delighted readers, rests in a great measure on mimicry, on what may be called a sort of *parrot* work, in the service of reason, no doubt, but not at all dependent upon any high function of reason for its potency or its popularity. It has seldom been heard that the most effective mimics are the most profound reasoners; and, on the other hand, a profound reasoner is often found deficient in that vivid power of imitating the striking points of detail which is the strength of the popular novelist, and the best spice of convivial conversation. There is therefore no presumption against the action of this so universal principle in the formation of language, but rather the contrary. And if the element by which sounds in the external world are signified in human speech is itself sound, how should we more naturally expect the one to express the other, than by some sort of imitation, more or less complete, according to the character of the vocal organ? I go, then, to nature, prepared to expect imitative phenomena in human speech; and I find them, not one here and one there, but everywhere in the richest abundance. Can any one hear the English words *smash, dash, thump, dumb, squeak, creep, clatter, chatter, click-clack, ding-dong, sigh, sob, moan, groan, hurry-skurry, skimble-skamble, wiggle-waggle,* and not believe that these words were framed by the human voice, with the express intention, more or less successfully realised, of giving a dramatic representation of the thing signified? This is so obvious, that, as already stated, Professor MÜLLER has been forced to admit it, to a certain extent; but, at the same time, watches with the sternest jealousy that the action of such a principle shall not be allowed to travel beyond the narrow precincts of the poultry-yard and the pig-sty. But, however he may wish to circumscribe the operation of the principle, it is quite certain that it acts not only most powerfully in the low region here indicated, but that this pictorial power of words is one of the most powerful instruments by which human speech is made to affect the human imagination, and becomes an instrument in the skilful wielding of which one of the great merits of a great poet has always been felt to consist. When, for instance, HOMER says:—

Δουπῆσεν τε πεσὼν ἀραβῆσε δὲ τέυχεα ἐπ ἀυτῷ.

"With a hollow sound he smote the ground, and his armour rattled o'er him;"

or GÖTHE—

"Aus dem hohlen dunklen Thor
Drängt sich ein buntes Gewimmel hervor,"

every one feels that the poet, under the influence of the rhythmical instinct which is an expression of reason, is only using the materials of language for producing an æsthetical effect, on the same imitative principle by which these materials themselves were originally framed. And we can prove the actual making of words on this principle from observation. A happy father calls his child "*little goo-goo!*" Why? Because the little creasy-armed, chubby-faced Hopeful has a throat, and *g* is a guttural letter; and, therefore, as naturally as a chicken cries *pip, pip,* the baby sends forth *goo, goo*, as the first notice of its march into the realm of articulate speech; and the delighted parent, by the exercise of the parrot faculty, immediately forms a name for his son, which might have remained for ever, as the only name it should get, did not the conventional rights of baptism interfere, not to mention the long prescriptive claim in favour of *baby* and *boy*, which the labial letters from old Greek and Roman days have succeeded in establishing against the guttural. For I certainly do believe, whatever may be said to the contrary, that the Hebrew word *em*, the Greek μαῖα and μήτηϛ, the German *Amme*, and the common English *ma'*, *pa'*, and *baby*, have something to do with the use of the labial letters, so natural to the toothless gums of children, and so obvious in the cries of certain animals. Of the consonants indeed, which brutes use to modify their vocal cries, of which the vowel is always the grand element, the labials and gutturals, along with the snarling R, the rolling L, and the sibilant S, seem to be the most common. We shall not therefore be surprised to find an ox called *Bo* in Latin, Greek, and Gaelic, or to hear the bellow of oxen called μυκᾶσθαι in Greek, while the bleat of sheep is called μηκᾶσθαι, and the cry of goats, in German *meckern*, for which I do not know that we have a specific word in English. And if the Greeks say ὑλακτῶ for the bark of a dog, it is not because their language is not mimetic in this case, while ours certainly is, but because ὑλακτῶ is merely a lengthened derivative form of the root ὑλ, which is only a feebler form of our English *howl*, German *heulen*. In the same way that the letter R in the Greek κορώνη, the Latin *corvus*, the Hebrew ערב, and the English *crow*, has something to do with the sound uttered by that class of animals, I shall continue to believe, without any reference to GRIMM's Law, so long as in the world of animated sound neither swallows shall have been heard to grunt on the eaves, nor pigs to twitter in the sty, nor bulls to mew in Bashan, nor cats to bellow at the fireside.

Let so much therefore be allowed,—be held as admitted,—though not without manifest unwillingness by those who disown the principle we now advocate. But now comes the more important question, for the sake of which alone the preceding examples have been given, as a sort of postulate, rather than as demanding proof. Is this all? If only a few names of animals, and certain phenomena in nature always accompanied by sound, are to be explained by the principle of pictured articulation, we are advanced but a very short way, and the great body of the roots of a language, expressing not *sounds* but *notions*, remains unexplained. When I express the idea of thinking in Latin by the root *med*, in Greek by μητ, and in English by *think*, what possible connection can such words have with screaming, or grunting, or twittering, or with the cry of any unreasoning animal? For man, as a reasoning animal, must have a method of proceeding in forming his language, altogether different from the procedure which would suffice for unreasoning brutes; his discourse is not only φωνή, mere *voice*, but it is λόγος, that is simply the outside of reason, and expressed in Greek (as all the world knows) by the word which likewise signifies reason. Depend upon it, all the important roots of a language must be *notional;* otherwise, we suppose man acting without reason, and our philosophy sinks into the lowest sensationalism of the French school of the last century.

Now, before answering this argument, I must again protest distinctly against the presumption here implied, that the assertion that we do any thing without the intervention of conscious notions and ideas is degrading to man, and ignores that reason which is his characteristic. We eat, drink, sleep, love, hate, dance, fly into sublime passions, and write lofty poetry, not without reason, indeed, but certainly in nowise by virtue of consciously worked out products of reason, called abstract ideas. If it should be found, therefore, that certain words denoting mental action are only a secondary application of words originally painting an outward mechanical action or position, or even a mere sound, I see nothing to be ashamed of in the matter. A man may make himself a pig, or worse than a pig in many ways, but certainly not merely by painting a pig-sty, or by ventriloquizing a grunt, or even by borrowing a grunt, for the expression of some moral or metaphysical idea. The degradation to a reasonable being in the matter of language consists, not in the borrowing from physical sources, but in not submitting the borrowed physical material to a native metaphysical treatment.

This premised, we remark that it is a known tendency of language to grow, not by the creation of new roots, when they are not necessary, but by a dexterous use of the stock already acquired. In harmony with this fact, we have a right to suppose that the original framers of language having succeeded, by the principle of phonic imitation, in making a vocabulary to express the sounds made by animals or sounding bodies, and the related names by which these should be known, would not stop here, but would proceed to apply the same principle to a much wider and more important range of ideas. Nor was the stepping-stone far to seek, by which they soon learned to pass from the domain of single imitated sounds to the domain of actions generally, and of all sorts of ideas. For if we attend to the process of nature in such cases, we shall observe three facts which would necessarily help to work out the original stock of strictly pictorial words imitating mere sounds, to a large class of words, including all the most important verbs which language in its early stages required. The first of these facts is, that most actions which attract the notice of men are, in the first place, accompanied by certain sounds or noises, which serve to indicate the approach, and to express the manner and intensity of their energy. The second fact is, that between sounds and certain feelings and ideas, not accompanied by any sound, there are certain strong analogies, such as that which the blind man indicated, when he said that he thought scarlet colour was like the sound of a trumpet; and these analogies, taken advantage of by the dexterous and economic framers of language, would necessarily lead to the designation of a number of ideas expressive of noiseless vision or touch, by words possessing some vocal and audible analogy. The third fact is, that all external impressions made upon our senses, which, if not the cause, are certainly one of the necessary factors of all human knowledge, are never expressed without the production of certain pleasant or unpleasant feelings, and certain affection of the nervous system, on which the utterance of articulate sound depends; and as effects always correspond to causes, it cannot but be that the vocal utterance from within educed by any strong impression from without, shall in some way or other represent the character of the source from which it sprang. Let us examine these three facts separately, and see to what classes of results in the formation of language they unavoidably lead. Take the word *Kill* to begin with. You ask what connection is there between the sound of this word and the action signified? I reply, that I do not know, because there are many words in all languages, derivative both in meaning and form, whose original type is not now recoverable; but there is another *English* word, *slay*, signifying the same thing, the original form of which is the German word, *schlagen*, to *strike*, and here I distinctly see a phonic congruity between the rough action signified, and the rough word *Schlag*, by which it is expressed. The act of striking is generally accompanied by a hard, sharp noise; and so, hard, sharp syllables, as in the English words, *knock*, *rap*, are used to express that act. Or take the Sanscrit root MAR, of which MÜLLER has made so much, and who does not see that it expresses something rude and harsh, as much as the English word *crush*, and the French word *ecraser?* In the same way the root AR, signifying to plough, and which appears in the Hebrew ארץ the earth, as well as in the Greek adverb ἔραζε, is evidently a phonetic expression of the rough sound of earth or gravel when stirred, containing a combination of letters which, when inverted, appears in *gravel*, *grain*, γράφω, *scratch*, χαράσσω.

In the same way, actions accompanied by slender soft sounds are expressed by weak vowels, as to *creep*, to *sneak*, and to *slink*. Is it not also plain, that whether we take the Greek κλέπτω or the English *steal*, we find that these words are so formed as to present a dramatic contrast to ἁρπάζω and *rob*, which signify the same kind of abstraction, accompanied with violence and noise? And if you say that the Latin *fūr* does not express anything of this kind, I thank you for the observation, and reply that the Greek verb φωράω, from which *fūr* is derived, does not originally imply the silent stealthiness of felonious appropriation, but rather the sudden, rude act, by which a thief is apprehended. Contrast with these words the English word *tumble*, and you will observe that the awkward, clumsy, hollow roll with which the act of accidental falling is generally accompanied, finds expression here to such a degree that the words to *tumble* and to *stand* seem as much opposed to one another as a round rowley-powley pudding is to the sharp, thin, clear knife which cuts it. And this brings me to my second great fact,—Why has the word *knife* a *k* in it? Why the Gaelic *sgian*, why the Latin *culter?* Is this altogether accidental? Certainly not. K is a sharp letter, perhaps the sharpest in the alphabet, and therefore in all languages appears in words which signify sharpness, as in the Latin word *acies*, Greek ἄκρος, Sanscrit *krit, to cut*, with the Latin *cœdo*, and probably the Gaelic *cath*, a *battle*. The Greek κόπτω contains the same initial letter, although from the intrusion of the labial π it is a less perfect word to express a clean, sharp stroke than the simple dental which appears in the other roots. For the labials, being uttered by rounded, unpointed organs, are naturally used to express bluntness, as the very word *blunt*, Greek ἀμβλύς, plainly proves. Hollow vowels and hard consonants will in all cases be applied to express the reverse of what is sharp and thin. So *tundo* in Latin is to beat, not sharply, like our word *rap*, but broadly and bluntly, as with a mallet. Hence *obtundere aures*, to bore a person with talking, to be constantly beating, and thumping, and drumming your crotchets upon the tympanum of his ear. So, when a man's intellect is not very sharp, he is said to be *muddled* or *fuddled;* and if muddled is only a verbal form of *mud*, you will easily understand that something soft, broad, round, not at all clear, and not very stable, is understood by the verb as well as by the noun. We thus see how not only sound, but everything perceptible to vision or to touch—that is to say, the whole range of phenomenal knowledge—comes under the derided principle of ὀνοματοποιία; and if there can be any stronger proof given of the unlimited range of articulate sound, in mimetically expressing things which have nothing to do with sound, the English word *mum*, for silence, contains that proof. M is the labial which most completely closes the lips, and sends the breath up through the nose; hence it appears in the Latin *mutus*, the Greek μύω for closing or shutting, not the mouth, but the eyes, and in the English *dumb*, which in German is *dumm*, stupid, because stupid people have often the sense to sit silent in company, and thus not betray their stupidity. I conclude these illustrations of the second of the three great facts by a remark on the word *stand*, previously used. This word, which is a bastard present, formed from the old past tense, like the Alexandrian Greek στέκω, has for its root the Sanscrit *sthâ*, in Latin *stare*. Now, any one may see that this word stands more firmly on its legs than the word *tumble*, with which we contrasted it. Why is this? There is no firmness or decision in any part of this word, just as in the cognate word *mumble* there is a plain want of determinative emphasis in the conglomeration of the letters. But when I say *sta*, I bring my teeth together with a decision which shows that I am suiting the word to the action, and that the firmness which I exhibit in the muscles of my legs is not to be accompanied with any looseness in the action of my jaws. And that this is not a mere fancy will be obvious to any one who considers the wide application which this combination of letters enjoys in words expressive of strength and decision in all languages. Thus in English, *stop*, *strength*, *strike*, *stride*, *sturdy*, *start;* in Greek, στράγγω, στρέφω, στρήνης, στρυφνός, στείβω, most of which have their Latin representatives, as *stringo*, *strenuus*, *stipo*. So in German, *starr*, *streng*, *stössig;* and many others. There remains now, to complete the pictorial process by which language is formed, the third fact mentioned above—according to which all external expressions necessarily affect in a certain way the whole nervous system and mental economy, and through the motion in the vital spirits thereby produced, modify in a corresponding way the articulation of human speech. Here we have a different principle altogether, as it would appear at first sight, from mere ὀνοματοποιία; for to imitate an internal sound, and to express an internal feeling, seem not only different, but quite contrary actions. Nevertheless, they are in their effects, as in their origin, substantially one; and Professor MÜLLER has accordingly put what he calls the POOH! POOH! theory as much under his ban as the Bow-wow! For the fact of the matter is, that an interjection, such as *ah!* or ὄιμοι, or *eheu*, and all such vocal expressions of pleasure or pain, must, by the laws of vitality, exhibit a certain correspondence with the sensations of which they are the expression. Thus any oppressive, heavy feeling in the chest will naturally cause a slow, protracted, dull flow of breath to proceed from the throat. The vowels α and ω, the diphthongs *ai* and *oi*, are exactly such a flow of breath. Hence the interjections ω, αι, οι, amplified into the verbs ὤζω, ἀιάζω, ὀιμώζω.

There is here, therefore, a sort of natural drama enacted—a correspondence of the *within* and *without*—which springs fundamentally out of the same root as the ὀνοματοποιία proper. When Aristotle called all poetry mimetic, he probably meant something of this kind; for while dramatic poetry only is strictly imitative of outward objects, lyric poetry is dramatically expressive of inward feelings; and to this the *Bow-wow* and the *Pooh! pooh!* departments of early word-making plainly correspond.

If we now inquire what the objections are that are brought against these facts, indicative of the operation of the pictorial principle in the world of vocal utterance, we find that they require no very laboured refutation, but resolve themselves into a few misunderstandings and prejudices, which a single touch can brush aside. In the first place, if it ever was asserted by any writer that all the presently existing roots in any language are onomatopœtic, and that all current words are to be explained on this principle alone, with such assertion I have nothing to do. I only maintain that the original stock of which language was made up consisted of such roots, and that a great proportion of them, after the changes of thousands of years, bear their origin distinctly on their face. I do not say, however, that all the words now existing in a language are to be dealt with on the supposition that they contain some pictorial element of the original phonic drama of human speech. Syllables are like sixpences, and are apt to be rubbed down in the course of time, till their original image and superscription can no more be traced.

Besides, as in the Greek language the word ἀδελφός, signifying *uterinus*, or born of the same womb, took the place of φράτωρ, which no doubt originally was used as *frater* in Latin, *bhrātṛi* in Sanscrit, and *brother* in English, so many of the oldest dramatically significant roots of language may have been replaced by secondary roots, in which the real character that belonged to the first pictorial roots is lost. I do not therefore deny that *equus* may come from the root *á'su*, *swift*, and a horse signify the swift animal. Though I have no doubt that *bo*, an ox, is merely a human imitation of the bovine sound, I by no means insist that all animals should have received their names from the cries which they make. I only say that, in the original formation of language, this was one of the simplest and most obvious methods of designation, and a method that extended a great deal further than superficial observation might lead the modern speculator to believe.

As little can I see why Professor MÜLLER should feel it his duty to declare war wholesale against onomatopœia in language, because on this or the other occasion some men have handled it wildly, and ridden rough-shod with it over GRIMM'S law, and the whole body of ascertained facts with regard to phonic transmigrations and transmutations. A man may talk ingenious nonsense on any branch of philological science with the utmost ease, in the teeth of GRIMM'S law, or even with the help of it; but that great principle of interlingual change has nothing to do with the question how roots, variable according to certain laws of phonic change, were originally formed. The Sanscrit *pitṛi* may become the English *father*, and the Scotch *fader*, without touching the question whether *PA* and *MA* have anything to do with imitation by parents of the first untutored labial utterances of a child. Finally, I must be allowed to express my conviction that the opposition to onomatopœia seems to arise in the minds of some speculators partly from a certain horror of a sort of merely animal element in the creation of language, which in ancient times had found acceptance with the low sensuous philosophy of EPICURUS,* and partly, so far as the Germans are concerned, from a certain instinct in them which leads them to prefer what is remote to what is obvious, what is conceptional to what is sensational, what is fanciful to what is real, what is mystical to what is plain. If they blame us, not unjustly altogether, for having no ideas in our scholarship, we may with equal reason retort that they have too many, and use them often with a wild ingenuity, rather than with a sober discretion. If we do not make such brilliant discoveries as they do beyond the flaming walls of the universe, we do not, on the other hand,

* MÜLLER, vol. ii. p. 87, quotes a passage of PROCLUS from EPICURUS as having suggested his soubriquet of the *Bow-wow* theory.

so often fail to see what directly lies before us. The same national habit of thought which led FORCHHAMMER to find in the Iliad a geological account of the struggle betwixt land and water in the Troad, and leads Professor MÜLLER to discover in the same great historic tradition a mythological fight between light and darkness, seems to determine the position of this distinguished philologer in reference to the original formation and growth of roots in language. How they were formed he nowhere tells us; he does not pretend to know; but of one thing he feels assured, that there is more of mystery in the matter than the easy mimicry of natural sounds can explain. "Are not Abana and Pharpar rivers of Damascus? may I not wash in them and be clean?" He will have nothing to do with word-painting, because it is too simple a process, seems to deal with facts rather than with ideas, and is not at all mysterious. For my own part, I think all is mysterious with language in one sense, nothing in another. It is as natural for men to speak, as for birds to sing, and fountains to flow; and that when they did speak, they spoke originally from imitation of natural sounds, and a cunning adaptation of the expressive power of the audible element, not only to things audible, but also to things visible and tangible, I shall continue to believe till some principle shall be propounded that may explain all known facts in a manner equally obvious and satisfactory.

I have only to say in conclusion, that my faith in imitation as the great principle in the formation of the original stock of human speech, is not in any degree affected by the vexed question whether man was originally created full-grown or a baby, whether he made language for himself, or got it, as some think there is a peculiar piety in imagining, ready-made from the Deity. I do not believe that Adam got language ready-made from his Creator, for the very plain reason that we get nothing ready-made from the Creator, but we make it ourselves after a fashion, by the indwelling power of His infinite virtue and grace, who is never far from the meanest of His creatures. But even if the Supreme Being did make a present to our primal sire of a ready-made language (though I think this contrary to the words of Moses in Genesis ii. 19), still the fact remains that the grand vocal organism so presented, bears on its front the most evident marks of an onomatopœtic or imitative construction. Those, therefore, who hold that God made human language must maintain that He made it on the same principle on which I maintain that man made it; for the facts are undeniable; and surely it cannot be more pious to suppose that the Father of all men coined words for the use of His reasonable children in a manner altogether arbitrary, rather than on the principle of a reasonable congruity, and a beautiful adaptation.

II.—*On the Cause and Cure of Cataract.* By Sir David Brewster, K.H., F.R.S.

(Read 16th January 1865.)

My attention was called to the subject of Cataract, in consequence of having, about forty years ago, experienced an incipient attack of that complaint, and studied its progress and cure.

While engaged in a game at chess with Sir James Hall, who was a very slow player, I amused myself in the intervals with looking at the streams of light which radiated from the flame of a candle in certain positions of the eyelids. In one of these observations I was surprised by a new phenomenon, of which I did not immediately see the cause. The flame of the candle was surrounded with lines of light, of an imperfectly triangular form, some parts of which were deeply tinged with the prismatic colours. Upon going home from the chess club, this optical figure was seen more distinctly round the moon, and of course it appeared, with more or less brightness, round every source of light.

Having been engaged in examining the structure of the crystalline lens in animals of all kinds, I soon discovered the cause of the phenomenon which I have described. The laminæ of the crystalline lens had separated near its centre, and the separation had extended considerably towards its margin. The albuminous fluid, the *liquor Morgagni*, which so wonderfully unites into one transparent body, as pure as a drop of water, the mass of toothed fibres which compose the crystalline lens, had not been sufficiently supplied, and if this process of desiccation had continued, the whole laminæ of the lens would have separated, and that state of white opacity induced, which no attempt has ever been made to remove.

The continuance of this affection of the lens was naturally a subject of much anxiety, and I never entertained the slightest hope of a cure. My medical friends recommended the use of what were then called Eye Pills, but having received no benefit from them, and having learned from experience the sympathy between the eye and the stomach, I used every day, and copiously, the *Pulvis salinus compositus*, and at the end of about eight months, when playing at chess in the same apartment, I had the happiness of seeing the laminæ of the lens suddenly brought into optical contact, and the entire disappearance of the luminous and coloured apparition with which I had been so long haunted.

In speculating on the process by which the crystalline lens is supplied with the necessary quantity of fluid, it occurred to me that it might be derived from the aqueous humour, and that cataract might be produced when there was too little water and too much albumen in the fluid which filled the aqueous chamber.

Upon this hypothesis, incipient cataract might be cured in two ways:—

1st, By discharging a portion of the aqueous humour, in the hope that the fresh secretion, by which the loss is repaired, may contain less albumen, and counteract the desiccation of the lens.

2d, By injecting distilled water into the aqueous chamber, to supply the quantity of humour discharged from it.

The first of these methods I knew to be practicable and safe, from the fact that a surgeon in the Manchester Infirmary, many years ago, tapped the aqueous chamber of a female patient forty times, in the vain hope of curing a case of conical cornea, which he attributed to an excess of aqueous humour. The frequent repetition of this operation shows how rapidly the humour is secreted, and how reasonable it is to suppose that, in the case of cataract, a healthier secretion might be produced under medical treatment.

Although the second method of injecting distilled water into the aqueous chamber presents greater difficulties, yet they do not appear to be insuperable. In 1827, when I happened to be in Dublin, I mentioned this method to the celebrated comparative anatomist, Dr MACARTNEY, who considered it quite practicable. He mentioned to me that a foreign oculist, whose name I forget, had actually injected distilled water into the eye of a patient with the view of supplying the aqueous humour that was lost during the extraction of the lens.

My attention was recalled to these suggestions for treating incipient cataract, by the results of a series of experiments on the changes which take place in the crystalline lenses of the sheep, the cow, and the horse, after death. In these experiments, which were published in the Philosophical Transactions for 1837, the lenses were placed in a glass trough of distilled water, and exposed to polarised light; and the changes thus produced were indicated by variations in the number and character of the polarised rings, and more palpably by the gradual enlargement of the lens. The distilled water passed through the elastic capsule of the lens. The lens increased in size daily, and at the end of several days the capsule burst, leaving the lens in a disorganised state, the outer laminæ being reduced to an albuminous pulp by the water admitted through the capsule.

These experiments have an obvious importance in reference to the cause and cure of the two kinds of cataract to which the human eye is subject. The aqueous humour is in immediate contact with the capsule of the crystalline lens. When the humour, therefore, contains too little water, the lens has not a sufficient supply of the fluid which keeps its fibres and laminæ in optical contact, and hence the laminæ separate, and the lens becomes opaque and hard. When, on the contrary, the aqueous humour contains too much water, the capsule introduces the excess into the lens, and produces the more dangerous affection of soft cataract, in which it is difficult either to depress or extract the lens.

In order to cure the first of these kinds of cataract, we must discharge a portion of the aqueous humour, and either supply its place by injecting distilled water, or leave it to nature to supply a more healthy secretion. In order to cure the second kind, we must supply the place of the discharged humour with a solution of albumen; or, as in the first case, leave to nature the production of a more albuminous secretion.

These views have received a remarkable confirmation from recent experiments on the artificial production and removal of cataract in the eyes of animals. Dr KIND, a German physiologist, whom I met at Nice in 1857, informed me that he had produced cataract in guinea-pigs, by feeding them with much salt, and that the cataract disappeared when there was no salt in their food. More recently, Dr MITCHELL,* an American physician, produced cataract by injecting syrup into the subcutilar sacs of frogs; and Dr RICHARDSON† did the same by injecting syrup into the aqueous chamber of the recently dead eye of a sheep. In the experiment of Dr MITCHELL, the cataract was removed from the living eye of the frog by surrounding the animal with water; and in that of Dr RICHARDSON, the cataract was removed from the dead eye of the sheep by replacing the syrup with distilled water.

Neither Dr MITCHELL nor Dr RICHARDSON seem to have been acquainted with my experiments on the changes in the lens after death, published in 1837, and with the theory of the cause and cure of cataract there referred to; nor with the distinct statement of it published in 1836,‡ and twenty years later, in 1856.§ Dr RICHARDSON, however, has borne ample testimony to its practicability and safety, when he suggests, almost in my own words, "that it would be worth while, in the earliest stage of cataract in the human subject, to let out the aqueous humour, and to refill the chamber with simple water." And he has borne a still stronger testimony to its value by congratulating "Dr MITCHELL in having been the earliest experimentalist to elucidate the synthesis of cataract, and to take the first steps towards a rational interpretation of the disease."

The tendency of the human crystalline lens to indurate or soften, by a defect or excess of water in the aqueous humour, may occur at any period of life, and may arise from the general state of health of the patient; but it is most likely to occur at that age, between 40 and 60, and often much earlier, when the lens experiences that change in its condition which requires the aid of spectacles. This change commences at one part of the margin of the lens, where its density is either increased or diminished. Its action in forming a picture on the retina thus becomes unsymmetrical, and vision is sensibly impaired. But when the change has taken place round the margin of the lens, its symmetrical action is

* American Journal of the Medical Sciences. January 1860.
† Medical Times and Gazette. March 31, 1860.
‡ Report of the British Association, 1836, p. 111; and 1837, p. 12.
§ North British Review, vol. xx. p. 167. November 1856.

restored, and by the use of spectacles the vision becomes as perfect as it was before the change. If glasses are not used when the change is completed, the eye must either strain itself, or use a strong light, to produce distinct vision in reading the small type and the imperfect printing which characterises the daily press; and by both these processes it will, in a greater or less degree, be injured.

It is a strange delusion, arising either from ignorance or vanity, which induces most people to put off the use of spectacles as long as possible. From the instant they are required, spectacles of different focal lengths ought to be used for the different purposes for which distinct vision is required, and the eyes should never do any work, unless they can do it with perfect distinctness and satisfaction. There is no branch of the healing art where science comes so directly and immediately to the relief of impaired functions as that which relates to vision, and none where science has been so imperfectly applied. When the change in question takes place, the eye requires to be carefully watched, and used with the greatest caution; and if there is any appearance of a separation of the fibres or laminæ, those means should be adopted which, by improving the general health, are most likely to restore the aqueous humour to its usual state. Nothing is more easy than to determine the condition of the crystalline lens; and by the examination of a small luminous object placed at a distance, and the interposition of small apertures, and small opaque bodies of a spherical form, we can ascertain the exact point in the lens where the fibres and laminæ have begun to separate, and may observe from day to day whether the disease is gaining ground or disappearing.

[Since the preceding paper was read I have seen a remarkable work, entitled "*Etudes Cliniques sur l'evacuation de l'Humeur Aqueuse dans les Maladies de l'Œil*," par Casimir Spirino, Turin, 1862. Pp. 500. M. Spirino had, in the course of little more than a year, operated upon forty-five cases of cataract. In many of these the cataract was perfectly cured, and in others the sight was improved. The first case was that of a lady of eighty-one, who had cataract in both eyes. After thirty-two evacuations of the aqueous humour by the same aperture, and almost always two or three times at the same sitting, both cataracts disappeared, the lady was able to read, without glasses, Nos. 3 and 4 of Jaeger's scale, at the distance of 4 or 5 inches, and even to thread a small needle.]

III.—*On Hemiopsy, or Half-Vision.* By Sir DAVID BREWSTER, K.H., F.R.S.

(Read 20th February 1865.)

The affection of Half-vision, or Half-blindness as it has been called, was first distinctly described by Dr WOLLASTON, in a paper "On Semidecussation of the Optic Nerves," published in the Philosophical Transactions for 1824. "It is now more than twenty years," he says, "since I was first affected with this peculiar state of vision, in consequence of violent exercise I had taken for two or three hours before. I suddenly found that I could see but half the face of a man whom I met, and it was the same with every object I looked at. In attempting to read the name JOHNSON over a door, I saw only SON, the commencement of the name being wholly obliterated from my view. In this instance, the loss of sight was towards my left, and was the same, whether I looked with my right eye or my left. This blindness was not so complete as to amount to absolute blackness, but was a shaded darkness, without definite outline. The complaint lasted only about a quarter of an hour." In 1822, Dr WOLLASTON had another attack of hemiopsy, with this difference, that the blindness was to the right of the centre of vision, and he has referred to three other cases among his friends; but in these, the affection was accompanied with headache and indigestion.

In republishing Dr WOLLASTON'S paper in the "*Annales de Chimie et Physique,*"* M. ARAGO says, that he knows *four* cases of hemiopsy, and that he himself had experienced three attacks of it, followed by headache above the right eye.

In the "Cyclopædia of Practical Surgery," published in 1841, Mr TYRRELL describes Hemiopsy as "Functional amaurosis from general disturbance." He informs us that "he has experienced this form of amaurosis several times," and that he has been consulted by several fellow-sufferers of both sexes. In all these cases the affection was attended with severe headache, giddiness, and gastric irritation, sometimes preceding, and sometimes following, the attack.

In the accounts which have been given of these different cases of hemiopsy, no attempt has been made to ascertain the optical condition of the eye when it is said to be half-blind, or to determine the locality and immediate cause of the complaint. Dr WOLLASTON describes the blindness as a shaded darkness without definite outline. M. ARAGO says nothing about darkness; and the insensibility of the retina, of which he speaks, must mean its insensibility to visual and not to luminous impressions. Mr TYRRELL, on the other hand, simply states, that the

* 1824, vol. xxvii. p. 109.

obscurity takes place in different portions of the retina, and varies in its extent at different times.

Having myself experienced several attacks of hemiopsy, I have been enabled to ascertain the optical condition of the retina when under its influence, and to determine the extent of the affection, and its immediate cause.

In reading the different cases of hemiopsy, we are led to infer that there is vision in one-half of the retina, and blindness in the other. But this is not the case. The blindness, or insensibility to distinct impressions, exists chiefly in a small portion of the retina to the right or left hand of the *foramen centrale*, and extends itself irregularly to other parts of the retina on the same side, in the neighbourhood of which the vision is uninjured. In some cases the upper half of the object is invisible, the part of the retina paralysed being a little below the *foramen centrale*. On some occasions, in absolute darkness, when a faint glow of light was produced by some uniform pressure upon the whole of the retina, I have observed a great number of black spots, corresponding to parts of the retina upon which no pressure was exerted.

In the case of ordinary hemiopsy, as observed by myself, there is neither darkness nor obscurity, the portion of the paper from which the letters disappear being as bright as those upon which they are seen. Now, this is a remarkable condition of the retina. While it is sensible to luminous impressions, it is insensible to the lines and shades of the pictures which it receives of external objects; or, in other words, the retina is in certain parts of it in such a state that the light which falls upon it is irradiated, or passes into the dark lines or shades of the pictures upon it, and obliterates them. This irradiation exists to a small degree, even when the vision is perfect at the *foramen centrale*, and it may be produced artificially in a sound eye, on parts of the retina remote from the foramen, and as completely, though temporarily, as in hemiopsy. In order to prove this, we have only to look obliquely at a narrow strip of paper placed upon a green cloth, that is, to fix the eye upon a point a little distant from the strip of paper. After a short time the strip of paper will disappear partially or wholly, and the space which it occupied will be green, or the colour of the ground upon which it is laid.*

This temporary insensibility of the retina in the part of it covered by the picture of the strip of paper, or its inability to maintain constant vision of it, can arise only from its being paralysed by the continued action of light, an effect not likely to be produced, and never observed, in the ordinary use of the eye.

The insensibility of the retina, in cases of hemiopsy, and the consequent irradiation of the light into the space occupied with the letters, or the objects which disappear, though a phenomenon of the same kind as that which takes place in

* Letters on Natural Magic. Let. II. p. 13.

oblique vision, has yet a very different origin. The parts which are in these cases affected extend irregularly from the *foramen centrale* to the margin of the retina, as if they were related to the distribution of its blood-vessels, and hence it was probable that the paralysis of the corresponding parts of the retina was produced by their pressure. This opinion might have long remained a reasonable explanation of hemiopsy, had not a phenomenon presented itself to me, which places it beyond a doubt. When I had a rather severe attack, which never took place unless I had been reading for a long time the small print of the *Times* newspaper, and which was never accompanied either with headache or gastric irritation, I went accidentally into a dark room, when I was surprised to observe that all the parts of the retina which were affected were slightly luminous, an effect invariably produced by pressure upon that membrane. If these views be correct, hemiopsy cannot be regarded as a case of amaurosis, or in any way connected, as has been supposed, with cerebral disturbance.

Dr WOLLASTON endeavoured to explain the phenomena of hemiopsy, and the fact of single vision with two eyes, by what he calls the semidecussation of the optic nerves, a doctrine which Sir ISAAC NEWTON had suggested, and employed to account for single vision.* A fibre of the right-hand side of the optic nerve is supposed to decussate or divide itself into two fibres, sending *one* to the right side of the right eye, and *another* to the right side of the left eye, while a fibre on the left-hand side of the optic nerve also decussates, sending *one* fibre to the left side of the left eye, and *another* to the left side of the right eye. Hence, Sir ISAAC NEWTON drew the conclusion, that an impression on each of the two half fibres would convey a single sensation to the brain; and hence, Dr WOLLASTON concluded that hemiopsy in one eye must be accompanied with hemiopsy in the other.

Ingenious as these explanations are, the anatomical facts by which alone they could be supported have not been established. Dr ALISON,† who has adopted the opinion of NEWTON, and reasoned upon it, admits that the anatomical evidence is still defective; and the late Mr TWINING‡ has adduced nine cases of disease in the optic nerves and thalami, which stand in direct opposition to the hypothesis of semidecussation. Dr MACKENZIE, too, adopting the same view of the subject as Mr TWINING, distinctly asserts that "the great mass of facts in Pathology and Experimental Anatomy, touching this question, go to prove that injuries and diseases affecting one side of the brain, instead of *hemiopsia* in both eyes, produce amaurosis only in the opposite eye."

The two great facts of hemiopsy in both eyes, and of what is called single vision with two eyes, do not require the hypothesis of semidecussation to explain

* Optics, p. 320.

† Edinburgh Transactions, vol. xiii. p. 479.

‡ Trans. Med. Soc., Calcutta, vol. ii. p. 151; or, Edin. Journal of Science, July 1828, vol. ix. p. 143.

them. If hemiopsy is produced by the distended blood-vessels of the retina, these vessels must be similarly distributed in each eye, and similarly affected by any change in the system; and, consequently, must produce the same effect upon each retina, and upon the same part of it.

In explaining single vision with two eyes, we have no occasion to appeal to double fibres in the optic nerves, or to corresponding points on the retina. There is, in reality, no such thing as single vision, that is a single image seen by both eyes. With two sound eyes every object is seen double, and it appears single only when, by the law of visible position, the one image is placed above the other. But even in this case the object is seen double, by means of two dissimilar images of it which are not coincident. By shutting the right eye, we lose sight of a part on the right side of the double image, which is seen only by the right eye; and by shutting the left eye, we lose sight of a part on the left side of the double image, which is seen only by the left eye. If one eye gives a better picture than the other, the duplicity of the apparently single image is more easily seen. By shutting the good eye the imperfect picture is seen, and by shutting the bad eye we insulate the perfect picture. It is difficult to understand how optical writers and physiologists should have so long demanded a single sensation for the production of a single picture from the two pictures imprinted on the two retinas. If we had the hundred eyes of Argus, the production of an apparently single picture would have been the necessary result of the Law of Visible Position.

IV.—*Miscellaneous Observations on the Blood.* By John Davy, M.D., F.R.S., Lond. & Ed., &c.

(Read 6th March 1865.)

On a fluid of so much importance as the blood, observations with any pretension to accuracy can hardly be too often made and repeated, more especially when we consider its great instability, its little uniformity, and the differences of opinion entertained by physiologists respecting some of its most remarkable properties.

Such is the persuasion which has influenced me in engaging in the present inquiry, and in submitting its results to the Society.

I. *On the Action of Water on the Red Corpuscles of the Blood.*

As is well known, the red corpuscles are altered in form and appearance on admixture with water, the most obvious change being, that from discs they expand into globules.

In some trials made with the view to ascertain something more precise, I have selected the blood of birds, that chiefly of the common fowl and duck, the corpuscles of their blood, from their elliptical shape, being peculiarly fit, as it seemed, for the inquiry.

The first trials made were to ascertain the proportion of water that was required to effect any material change. The results obtained were the following:—

When one measure of water was added to one of serum holding red corpuscles in suspension, but few of them experienced an immediate change of form and became globular.

On the addition of two of water, the majority of the corpuscles underwent this change, a few only retaining their normal form.

On the addition of three of water, none of a normal form could any longer be seen; all that were visible were rounded, much reduced in apparent size, and were much less distinct; indeed, a nice adjustment was required to detect them. Many of them had a jagged outline; and from some there was a slight projection, suggestive of a rupture of their capsule.

Dried by evaporation at about 100° Fahr., very many of them were found to have recovered their original form and size. Some of them, however, appeared to be ruptured, the excluded nuclei adhering to their surface; others retained their nuclei, of irregular appearance; all appeared to be wasted.

On the addition of four of water, the corpuscles were seen less distinctly, yet they were to be seen, the adjustment being as accurate as possible, and using a warm object-glass, a precaution needed to prevent the dimming of the glass ($\frac{1}{8}$th inch power) from the vapour rising from the fluid in such close proximity.

When more water was added, the only material difference that I am aware of was not in the effect on the corpuscles, but in their wider diffusion, thus increasing the difficulty of observing them. To counteract this, a portion of cruor was mixed with water in a tall vessel, stirred occasionally, and after having been some hours left at rest, the greater part of the coloured fluid—coloured by the solution of the colouring matter of the corpuscles—was drawn off. What remained afforded an interesting result. A drop of this fluid under the microscope exhibited much the same appearance as that from the addition of four parts of water; and on drying at the same temperature, the appearances were also similar but more strongly marked, suggestive of ruptured capsules and the loss of their contents, with the exception, as in that instance, of some of them retaining their nuclei, these more or less altered. Most of the corpuscles, if that term be applicable to their remains, were circular or portions of circles, portions of them having been broken of. Some showed a rent, a few were elliptical, and with the exception of being wasted, but little altered in appearance.

The agency of water on the red corpuscles has commonly been attributed to imbibition or endosmosis, to solution of the soluble matter which these cells contain, and to exosmosis. The appearances which I have described seem to harmonise well with this view, with the addition of rupture of the cell-wall or capsule, and the occasional exclusion of the neuclei. They accord, too, tolerably with those noticed by Professor LEHMANN, in his Physiological Chemistry,* with the exception of two particulars. He states, that when largely diluted, the corpuscles become invisible under the microscope, which he attributes to their refractive power, after the action of water, differing but little from that of water itself. As already mentioned, when using certain precautions, I have found them, only much less distinct. The other particular relates to their remains. According to him, these are mere shreds, and not empty and more or less broken capsules, as I have found them to be. The subject, it must be admitted, is one in the investigation of which it is not easy to obtain uniform and satisfactory results, there are so many interfering and disturbing circumstances concerned, not omitting the influence of the serum, and especially keeping in mind the powerful attraction the corpuscles have for water, and their hygroscopic properties; and further, the changes to which they are liable as dead matter, from the influences to which they are exposed.

As to the last mentioned, I have found that the longer the blood is kept, the

* Vol. ii. p. 184.

smaller is the quantity of water that is required to alter their form. As to their hygroscopic property, this is shown by the simple experiment of breathing on them, or by exposing them over water for a few hours, keeping them, of course, out of contact with the water. In the instance of the warm vapour of the breath, one expiration is sufficient to deprive them, previously dried, of their elliptical form.

It is worthy of remark, that when the corpuscles are coloured by the addition of a weak solution of iodine, not only the action of the warm vapour of the breath is in a great degree arrested, but even the action of water, and this after immersion in water on a glass support for twelve hours, when they were found to retain their normal form, only slightly contracted, with their nuclei distinct. May it not be conjectured from this, that iodine medicinally used may operate in a degree similarly, and thus may arrest undue metamorphic disintegration?

II. *On the Changes which take place in the Blood when excluded from the Air.*

The changes to which the blood is subject when exposed to the air, at ordinary atmospheric temperatures, are pretty well known. To endeavour to ascertain what would happen were air as much as possible excluded, the following experiments were made:—

A bottle full of water, deprived of air by the air-pump, was emptied the instant before receiving blood from the divided cervical vessels of a barn-door fowl; so soon as full to overflowing, it was closed with a glass stopper lubricated with oil, and inverted in water.

During the first hour the blood retained its original hue, bright vermilion, and this throughout. After two hours the colour had lost something of its brightness. The following day the colour had become uniformly chocolate brown. The day after there was no appreciable change. The serum which had separated was of a wine yellow, and the crassamentum had contracted considerably. On the third day the serum was beginning to show a reddish tinge. From this day, viz., the 9th of November, to the 4th of December, the serum became of a darker red, and, like the crassamentum, was almost black, as seen by reflected light. During the time mentioned, the temperature of the room in which the blood was kept varied from about 55° to 58°. The bottle was now taken out of the water, which was as clear as at first, showing that the closure was complete. The stopper was drawn out with ease, proving—as it was introduced when the blood was warm—that there was pressure from within rather than from without, though there was no appearance of any gas evolved. The serum decanted was of a dark purplish-red, as seen in thin layers by transmitted light, but black by the same light, and opaque, in a tube of one-half inch diameter. The crassamentum, of the same colour, was soft and easily broken up, and had, as well as the serum, an offensive putrid smell, but less so than if air had been allowed access

With hydrate of lime, both yielded a strong ammoniacal odour. The blood corpuscles, as seen under the microscope, were found to have become rounded and globular. The fibrin seemed to be permanently dyed red ; it retained this colour after having been well washed, and after maceration in water for many hours. Its structure was finely granular ; it showed no appearance of fibres under gentle pressure, and viewed with a high power.

The experiment was repeated, using the blood of a turkey and also of a bullock. The results were similar. That on the blood of the turkey was of the same duration as the preceding. That on the bullock's blood was begun on the 14th of December, and ended on the 7th of January. In the latter instance, though no gas was evolved, the putrid blood, when subjected to the air-pump, entered into violent ebullition from the copious disengagement of air, and this even before the vacuum was nearly complete. It may also be mentioned, that a silver probe plunged into the clot became, after a few minutes, strongly discoloured, indicative thus of the presence of sulphuretted hydrogen.

A fourth experiment was made with the blood of a duck. In this instance, instead of emptying the bottle of water before receiving the blood, the water, deprived of air, was to a certain amount expelled by the blood as it flowed from the divided vessels. The specific gravity of the mixture was 1033. After having been kept from the 23d of November to the 12th of January, at a temperature varying from about 40° to 50° and 55°, the changes observed on examination were so similar to those already specified that they need not be described.

A fifth experiment was made with the blood of a fowl. As in the last the blood was mixed with water; but it differed from the last in being subjected to the air-pump as soon as it had become sufficiently cool. No air was thus extricated. The bottle was again closed and inverted in water. This was on the 16th of February; it was examined on the 17th of March. Some difficulty was experienced in withdrawing the stopper. The blood bore marks of an incipient putrefaction ; its smell was offensive, and some muriate of ammonia was formed on a plate of glass moistened with hydrochloric acid put over it during a few hours.

These results, all so well marked, seem to be nearly identical with those which occur when blood of the same temperature is exposed to the air, almost the only difference that I am aware of being in degree. The change of colour is the same, with the exception, that when exposed to the atmosphere, the blood at the surface, especially if it be venous, becomes florid before it darkens; the change of form of the corpuscles is the same, and the solution in the serum of their colouring matter. The same gases likewise are formed, and the same alkali is generated, accompanied by the characteristic putrid odour.

That blood should thus undergo change when air is excluded, is no more, perhaps, than might be expected when we reflect on its composition, and that oxygen is contained in it in a state, it is presumed, free to act and give rise to putrefactive fermentation.

What is more remarkable is the fact, that blood may be retained in the living body, stagnant, at rest, without undergoing similar changes, at a temperature so favourable to these changes. I may refer to HEWSON's collected works, edited by Mr GULLIVER, for instances of the kind. In a note, page 17, to mention one, the editor remarks, "occasionally blood is extravasated and stagnant in the living body for an indefinite time, and yet retains fluidity, as Mr HUNTER and Mr CÆSAR HAWKINS have noticed." He adds, "I saw a case in a soldier, who had received a bruise in his loins, from his horse bolting with him over a bridge in Hyde Park; the injured part quickly swelled, evidently from effused fluid, which was let out twenty-eight days afterwards. It measured five ounces, was as liquid as blood just drawn from a vein, and coagulated in a cup in less than three minutes. The corpuscles were observed to be unchanged, and readily collected together in the usual way by their broad surfaces. Next day the clot was moderately firm, scarlet at the top, somewhat contracted, and surrounded by a little serum." What a contrast this presents to the blood from which atmospheric air was excluded in the experiments detailed! Can the difference have been owing to the stagnant blood in the living body having been exposed to the action of the surrounding tissues, by which it is possible that, though a change may have been going on slowly in the blood, the degraded or altered particles may have been carried away as they were produced, leaving the residue in its normal state? I have witnessed something analogous when a mass of fibrin, enclosed in a muslin bag, has been immersed in water under a cock, from which there was a constant small stream keeping the water round the included fibrin in motion. During about a month that the fibrin was thus exposed at a temperature of about 40°, it had undergone little change; it was firm and only slightly tainted. In instances of aneurism, it is well known that not only the fibrin, but also the crassamentum enclosed in the sac resist for a long time putrid decomposition. May not this resistance be referred to the same cause? This explanation is submitted conjecturally. The fact that extravasated blood, from contusion and vascular rupture, is commonly absorbed with discoloration of the bruised part, may be adduced as somewhat in its favour. The physiologists of the School of Hunter would doubtless refer the liquidity of the blood, in the case in question, to the vitality of the blood; but that is a doctrine which at present is hardly tenable.

III. *On the Action of the Air-Pump on the Blood.*

The air-pump I have used in the trials I am about to describe is the same as that with which I made some former experiments on the blood,* and, as then, it was in excellent order.

* Anatom. and Physiolog. Res. vol. ii. p. 214.

The chief precautions taken were to receive the blood as it flowed from the divided vessels of the animal killed into phials, immediately after they had been emptied of water from which the air had been expelled by the action of the air-pump, and after closing with a glass stopper, cooling the blood rapidly by immersion in water.

Though these precautions were taken, I believe they were not absolutely necessary for good results, as I find that when water exhausted of air is poured into a carefully washed phial from which water containing air has been poured out, on submitting it to the air-pump, no air is extricated either from the water or from the side of the phial.

The experiments on exhaustion have been made on the blood of the common fowl, of the duck, of the sheep, bullock, and pig; they have most of them been several times repeated.

The results have varied more than I could have expected, tending to show that the quantity of air extricable from the blood by the air-pump is far from constant, and depends on circumstances, some of which are appreciable, others obscure.

1. From the blood of the common fowl, the quantity of air disengaged has commonly been less than from that of the duck, sheep, bullock, and pig.

2. The blood of all the animals, when taken from them shortly after feeding, has commonly afforded more air than from animals of the like kind when fasting.

3. Florid blood, which it may be inferred is chiefly arterial, has yielded less air than dark blood, which probably is chiefly venous, and, accordingly, that which flows first, when an animal has been blooded to death, less than that which flows last.

4. In a small number of instances, those of animals killed after a fast of many hours, the fresh blood yielded no air. In some of the trials which gave this result, the blood was mixed as it flowed with an equal quantity of water deprived of air.

5. In no instance have I witnessed the disengagement of air from fresh serum, proving that the air, when extricated from the blood, is derived from the clot, and it may be presumed, from the red corpuscles which are entangled in it.

6. As might be expected, I have found the disengagement of air from the action of the pump more copious in summer than in winter; and also more copious from blood, the fibrin of which has been broken up by having been agitated with shot previously freed from adhering air, than from the clot left entire. In the instance of the blood of the common fowl, which coagulates rapidly, affording a firm coagulum, even the puncturing of it makes a difference; air then escapes, which before was retained.

7. In many instances, blood which had yielded air on exhaustion, has, after exposure for a few hours to the atmosphere, on repetition of the exhaustion,

ceased to yield air, and this when the first trial was stopped before the exhaustion of the air was nearly complete. This result, seemingly paradoxical, may have been owing to ammonia formed, which may have fixed carbonic acid; and that ammonia was formed, was proved by the hydrochloric test and the production of muriate of ammonia. It has been witnessed in the instance of both venous and arterial blood, but most remarkably in the latter, and in warm weather oftener than in cold. In support of the explanation offered, I may mention an experiment on the blood of a calf, which had no food for about twenty hours before it was killed. This blood,—it was arterial,—even at first gave off no air on careful exhaustion. It was kept under an exhausted receiver from the 23d of April to the 13th of May, during the whole of which time it gave off no air, though the vacuum was as perfect as it could be made, and the pump was worked daily. At the end of this time the serum had become dark red, and on examination the blood was found in a state of incipient putrefaction and giving off ammonia.

What struck me as most remarkable in these experiments with the air-pump, was the comparatively small quantity of air, in most instances, disengaged from the blood, and its total absence in others, taking into account the quantity of carbonic acid liberated in the lungs during life in normal respiration, and also the quantity of air, both oxygen and carbonic acid, found in the blood by the German physiologists. Difference of temperature, comparing that of the hot blood circulating in the lungs in birds as high as 106°–108°, and in the sheep, ox, and pig, as high as 104°–106°,* with that of the blood of the same animals cooled to 50°–55°, may partly account for the result first referred to, but the second adverted to I cannot attempt to explain.

Besides the foregoing trials with the air-pump, I have made some on the blood-corpuscles, using very small quantities suspended in serum on a glass support. The corpuscles were from the blood of the animals already mentioned, and also of the frog and common trout. The results were all nearly similar: so long as the corpuscles were floating in serum there was no appreciable change of form, but if they were kept some hours under the exhausted receiver until they were left apparently dry on the object-glass, then a change was perceptible in them. Under the microscope they were found to have become greatly reduced in size, so as to be seen with difficulty, and not without the nicest adjustment, and also altered in form—the elliptical, as those of the bird, the fish, and batrachian, having become rounded. These changes were very similar to those produced by the action of water, and they may be accounted for, perhaps, on the idea that they were owing to the hygroscopic quality of the corpuscles. I may further remark that the effects on the corpuscles of the blood of the several animals tried some-

* I have found the temperature of the blood of a pig, flowing in a full stream, 106°. The pig was in high condition; the blood used was from it.

what varied; it seemed greatest in those of the common fowl, least in those of the ox.

IV. *On the Effect of a Low Temperature on the Blood.*

It was ascertained by HEWSON that the blood, by rapid freezing, is not deprived of its property of coagulating when thawed;* besides this and the change of form of the corpuscles from refrigeration which I have observed,† I am not aware that any thing has hitherto been published respecting the agency of a low temperature on this fluid.

During the frost which prevailed in the Lake District the winter before last, from the 2d to the 10th of January, I had an opportunity of renewing the inquiry. The blood used was that of the turkey, of the common fowl, and of the sheep. The most remarkable result obtained was that a low temperature, like a high temperature, appears to promote not only a change of form of the red corpuscles, but also a change of composition, as indicated by the production of ammonia and the solution in the serum of the colouring matter of the blood. As the changes were the same whichever blood was the subject of experiment, I shall restrict myself to what was observed in the trials on that of the common fowl. On the 4th of January a wine-glass was nearly filled with the blood of a full-grown fowl as it flowed from the divided great cervical vessels; it coagulated in less than two minutes. A plate of glass, moistened with a drop of dilute hydrochloric acid, was placed over it. After ten minutes there was a copious deposition of dew on its inner surface, vapour from the warm blood beneath, and there condensed. On examination with microscope, after evaporation, not a trace of muriate of ammonia could be detected. The trial was repeated, and for six hours in the open air, at the temperature of 28° Fahr.; now a trace barely of the salt was found. The blood was moderately florid, preserving its original appearance, and was not yet frozen. It was left out during the night. The temperature during the time, as shown by a register thermometer, was as low as 12°; on the following morning, at 9 A.M., it had risen to 18°. The blood was found to be frozen hard and thoroughly; it was greatly darkened in colour, and had lost entirely its florid hue. Distinct crystals of muriate of ammonia, and these not a few, were detected on the covering glass after the evaporation of the acid, and the red corpuscles from elliptical had become circular and globular.

The observations were continued until the morning of the 10th, when a thaw set in. The blood was examined twice daily, viz., at 9 A.M. and at 3 P.M. During the period the temperature was always below 20°, but not lower than 15°, excepting once, as already mentioned. The day temperature ranged as high as 27°, it was never lower than 22°. At the former temperature, a softening of the blood was

* HEWSON'S Works, p. 25.
† Physiological Researches, p. 369.

observed from incipient thaw. During the whole time, as denoted by the test employed, ammonia was evolved, and, as well as I could judge, the lower the temperature the larger was the quantity. I need hardly remark that there was no appreciable contraction of the crassamentum, no further separation of serum after congelation had taken place. The serum which first exuded before congelation—a very small quantity—after having been frozen, became coloured, and, finally, of a red nearly as dark as the general mass, and this owing in part to blood corpuscles suspended in it of altered form, and in part to solution of their colouring matter. After thawing, the blood had no smell indicating putridity, nor did it discolour silver; yet it continued, at a temperature of 50°, to evolve ammonia, and much in the same proportion as when frozen. Now, however, the contraction of the crassamentum, *i.e.*, of its fibrin, before arrested, took place, and to an extent seemingly differing but little from what would have occurred had the blood not been frozen. The blood corpuscles now were so reduced in size, and had become so transparent, that unless dried, they were seen with difficulty, and not without the most accurate adjustment.

These results, viz., the disengagement of ammonia, and, we must infer, its formation, when blood is frozen, are hardly such as could be expected; and they are the more remarkable, as seeming to be independent of putrefaction and the action of oxygen, and owing to a new arrangement of elementary parts produced by a low temperature, one ranging from about 50° to many degrees below the freezing point. That congelation was not essential to the formation of the ammonia was shown in other experiments, in which, when blood was exposed to a temperature ranging in one trial from 32° to 34°, the volatile alkali was produced, and in others at a temperature varying from 40° to 50°; and, in the latter, even when continued several days, without any indications of putridity, judging from the absence of the smell such as denotes putrefaction, and from silver immersed remaining untarnished.

At first view what has been described may seem anomalous, yet the results are not without analogies. The potato, as is well known, becomes sweet from the conversion of starch into sugar by "frosting;" and the ripening of the grape, the sweetening of its juice, it is also well known, is hastened by the setting in of frost at the time of the vintage in Switzerland, and in other countries with a similar climate. The formation of peat is another example of the efficiency of a comparatively low temperature in producing new compounds. Familiar with the effects of heat—*i.e.*, of a high temperature—as an active agent, it is not perhaps surprising that cold—*i.e.*, a low temperature—should be little thought of except as the opposite and the antagonist of heat, disregarding the fact that they differ merely in degree; and how inconsiderable that is, whether measured by our sensations or by the thermometer.

After witnessing the effects of congelation on blood, the question occurred, Is meat liable like it to change from freezing—that change which the evolution of ammonia indicates? As it is well known that meat may be kept for weeks frozen without being spoiled as an article of diet, the obvious answer was in the negative. The only experiments I have made have afforded results leading to the same conclusion. A portion of fresh mutton, cut into small pieces, was exposed on the 6th of January to the open air. During the following night the register thermometer was as low as 16°; the next morning it was 22°. The meat was not frozen; its fibre was soft and flexible; a bare trace of muriate of ammonia was found on the glass above it prepared as a test of the volatile alkali. Before night it became frozen and rigid, and it continued so until the thaw began on the 10th. Examined twice daily, no traces could be detected of the production of ammonia. The fibre, indeed, was evidently softened, and its striated structure, as seen under the microscope, was less conspicuous, so much so, that without a good light and a careful adjustment it could not be seen.

A like question occurred respecting manures—Does frost arrest their decomposition? I have made trial of stable-dung, and have found it when frozen to exhale ammonia in an unmistakable manner, proving that a low temperature, as in the instance of blood, promotes its decomposition, or that change on which the evolution of ammonia depends. A similar result has been obtained from the exposure of impure lithate of ammonia (the urinary excrements of the pelican), of the mixed excrements, partly urinary, partly alvine, of the barn-door fowl, and of guano. From all of them, using the same test, the production of ammonia was conspicuous. Should not these results suggest the propriety of reconsidering the treatment of manures, and if not the time of their application to the land, at least whether an addition should not be made to them to fix the ammonia?

V. *On the Action of Ammonia on the Blood.*

Since the hypothesis has been advanced, that the escape of ammonia from the blood is the cause of its coagulation, additional interest is attached to the action of the volatile alkali on this fluid.

The following experiments have been made with a view not so much to test the correctness of that hypothesis, as to show what are the effects of ammonia on the blood as a whole, and on its several parts:—

1. *On the Entire Blood.*—On the 8th of December 241 grs. of the blood of a duck were received, as it flowed from the divided cervical vessels, into a bottle containing 72·5 grs. of aqua ammoniæ of sp. gr. ·95. The bottle was immediately closed with a glass stopper. This was at 10.37 A.M. At 12.15 P.M. a semi-fluid viscid coagulum had formed, of a rich Turkey-red colour. A glass rod applied to it, it yielded to gentle pressure, without adhering to, or in the slightest degree soiling the rod. At 2.30 P.M. it was somewhat firmer. At 10 P.M. it was more

so; now, when the bottle was turned on its side, it ceased to flow. On the following day it was so firm that it bore inversion without flowing; no serum had separated. Examined on the 13th of December, the only change perceptible was that it was rather firmer. On the 1st of January it was more carefully examined. On withdrawing the stopper, as might have been expected, the smell of the ammonia was very powerful, indeed unendurable. The coagulum was found of the consistence of a pretty firm jelly, readily yielding to pressure, but not adhering to the glass rod impressing it. The whole mass was easily removed, retaining its form unbroken; and such was the adhesiveness of its substance—*i.e.*, of its particles to one another—that the mass admitted of being divided with a scissors without its soiling the instrument. A portion of it put into water did not immediately colour the water; from black it became dull brown. Examined with the microscope under compression, it exhibited a finely granular surface, through which were scattered globules of a less diameter than the blood corpuscles—these, it may be inferred, contracted.

In other experiments, made within a few days of each other, with smaller proportions of the volatile alkali, the effect has been found to vary.

When 12 grs. of aqua ammoniæ, of the same strength as that last mentioned, were mixed with 465 grs. of the blood of a turkey, the instant it was shed, the coagulation was retarded about 20 minutes. On withdrawing the stopper the following morning there was a strong smell of ammonia; the crassamentum was found of tolerable consistence, and was surrounded and covered with red serum, which owed its colour chiefly to red corpuscles suspended in it of a globular form, a change from their normal form evidently owing to the action of the alkali. Examined again after twenty days the coagulum was found firmer; it admitted of being taken out as an entire mass.

In a third experiment 2·5 grs. of aqua ammoniæ were mixed, as in the former instances, with 572 grs. of the blood of a fowl. After two hours the blood was found feebly coagulated and viscid, in a semifluid state, sluggishly flowing like tar. When a portion of it was poured into water, it did not mix with the water, but kept entire, retaining its viscidity. What remained, examined the following morning, was found divided into a somewhat denser crassamentum, still semifluid and viscid, and a red somewhat viscid serum, abounding in red corpuscles, more or less altered in form, many of them diminished in volume, and almost all of them rounded. Both the soft coagulum and the serum smelt of ammonia.

A fourth experiment was made on the blood of a sheep, after the same manner as the preceding. The quantity of the aqua ammoniæ was 1 gr., of the blood 587 grs. Examined after about half-an-hour, the blood, still warm, was found pretty firmly coagulated, and already some serum had separated. The glass stopper was withdrawn, and instantly after a plate of glass, moistened with dilute hydrochloric acid, was placed over the mouth of the bottle, and left for two minutes.

Now, after evaporation, crystals of muriate of ammonia, of a large size and in abundance, were found on it.

A fifth experiment, similar in manner to the last, with the exception that the aqua ammoniæ (1 gr.) was spread as much as possible over the inside of the phial, was made on the blood of a fowl (590 grs.) The blood coagulated in eight minutes, and pretty firmly. Another portion, caught in a wine-glass, coagulated in about a minute. Each was tested for ammonia, as in the preceding trial. The blood in the wine-glass, after five minutes—the time that the acid was kept over it—afforded no distinct trace of ammonia. The blood in the bottle, after one minute, afforded ample proof of the evolution of ammonia in the large crystals of the muriate which were formed on evaporation on the incumbent glass. The contrast, indeed, was very striking, comparing the blood with and without the addition of ammonia, as thus tested, and also by test-paper; the one, the former, having no effect during a minute that moistened test-paper was held over it; the other, in the same time, producing a decided alkaline reaction. On the following morning the crassamentum in the bottle was found slightly contracted, though less than that in the wine-glass; some reddish serum had separated; the blood corpuscles, whether suspended in the serum or retained in the clot, were little if at all altered.

From these experiments it would appear that the effect of aqua ammoniæ varies as to the quantity used, and this in a manner that could hardly be expected; 31 per cent. occasioning a thick adhesive coagulum, with a change of form of the red corpuscles, without the separation of any serum; 2·5 per cent. retarding the coagulation many minutes, but not preventing the separation of serum and a certain contraction of the crassamentum; 0·44 per cent. retarding the coagulation and rendering the coagulum soft and viscid, barely semifluid, with little separation of serum, and that viscid; lastly, 0·17 per cent. had little effect, except that of retarding for a few minutes the coagulation,—the coagulum, when formed, having very much its normal appearance.

2. *On the Fibrin of the Blood.*—The fibrin used was obtained by washing the clot which had formed in the first experiment. In its moist state it was slightly viscid. By drying it lost 93 per cent.; 2·5 grs. thus dried were put into a phial with 273 grs. of aqua ammoniæ of sp. grav. ·89, and secured by a glass stopper. After eleven days it was not apparently diminished in volume: 41 grs. of the clear fluid decanted and evaporated yielded only ·1 gr. As the fluid became concentrated during the process, which was conducted at a low temperature, its fluidity diminished, and when reduced to a drop it was still transparent. In its dry state it appeared as a transparent film, and as seen under the microscope with a high power it had a finely granular appearance.

A second experiment was made on the same fibrin in its moist state, using in place of aqua ammoniæ alone a dilute solution, consisting of 65·4 grs. of the alkali, and of 305 grs. of water. The fibrin was equal to 31 grs. After ten days its volume seemed little diminished—58·4 grs. of the clear fluid evaporated left ·1 gr. The undissolved residuary portion, constituting so large a proportion of the whole, was soft, glutinous, and adhesive; it might be called ropy, as it allowed of being drawn out, and when agitated by a circular motion, it rose spirally in the liquid. It thus differed from the dried fibrin, which was softened in a slight degree, but not rendered glutinous. Examined after thirty-four days, it seemed little altered in bulk, and nowise in its properties. The fluid was slightly viscid: a portion of it, 41·4 grs., evaporated to dryness, yielded only ·05 gr. It became slightly turbid during evaporation at a temperature of about 180°, and when the ammonia was driven off, it lost the little viscidity it before had. The smaller proportion of residue in this instance might have been owing to the circumstance that the phial holding the fibrin and the dilute aqua ammoniæ not being firmly corked, some of the ammonia might have escaped.

These results demonstrate how feeble is the solvent power of ammonia on fibrin. Many other experiments which I have made, of which an account is hardly needed, have been amply confirmatory of the fact, and also of the well-known effect of ammonia in rendering fibrin viscid and glutinous, and of increasing its transparency. This last effect should be kept in mind, otherwise, as the refractive power of fibrin differs but little from that of water, it may in some instances be imagined to be dissolved, when it is only diffused.*

3. *On the Serum of the Blood.*—On this fluid the effect of ammonia is less distinct. It appears to diminish rather than to increase the viscidity of the serum, as is shown by the following experiment: a portion of the serum of the blood of a pig, equal to 314 grs., was mixed with 274·4 grs. of aqua ammoniæ of sp. gr. ·89, in a glass-stoppered phial; and about an equal quantity of the serum of the same blood was poured into a similar phial. This was on the 30th March. Each was shaken daily: froth was produced in each instance, but that from the ammoniacal mixture subsided more rapidly than that from the serum alone; and the longer the trial was continued—it was continued more than a month—the more marked was the difference.

At the end of this time the ammoniacal mixture had deposited a white

* When well-washed fibrin, still slightly coloured by the colouring matter of the blood, is placed under the microscope, it appears to consist of translucent granules forming under gentle pressure a connected tissue. On the addition of aqua ammoniæ it becomes clear and transparent, like jelly, with a brightening of its colour. Compressed, it shows elasticity, and when extended by continued pressure, so as to be very thin, its appearance is hyoloid; no granules are to be seen in it except a few scattered ones, which, it may be, were derived from blood corpuscles.

Fibrin which has been rendered viscid by ammonia, after the removal of the ammonia by repeated washing, gradually contracts, and from being transparent becomes, in consequence of condensation, opaque, or nearly so. Thus contracted it often exhibits an imitative form, like that of hydatids. Its retention of the colouring matter of the blood is remarkable; it is greater even than that of the capsule or walls of the corpuscles.

matter, which was readily diffused on gentle agitation, rendering the fluid, which was before transparent, turbid.

Under the microscope the deposit exhibited thin crystalline plates, their length exceeding their width about a third, some minute spicula, somewhat like raphides, and some granules. As the matter was not viscid, it may be inferred that fibrin did not form a part of it.

The transparent fluid separated by decantation from this sediment yielded a coagulum with the sulphuric, muriatic, nitric, and acetic acids, added each in slight excess, that is, in a quantity a little more than was sufficient to neutralise the ammonia. The precipitate was redissolved by the sulphuric and muriatic acids, and in great part by the nitric—these acids concentrated—but not by the acetic.

When the clear ammoniacal fluid was boiled until the whole of the volatile alkali was expelled, it was rendered gelatinous, that is, the coagulum formed was soft and transparent, like the albumen of the eggs of some birds similarly treated.

The same fluid, evaporated at a low temperature, left a brownish transparent matter, which was soluble almost entirely in water. The solution frothed when boiled and gelatinised. It had a slight alkaline reaction, like serum, and had no unpleasant smell. Evaporated again, little of it was redissolved on the addition of water, and still less on repeating the operation—thus resembling ordinary serum.*

The serum without the addition of ammonia, kept during the same time, had also yielded a deposit, which was of a greyish hue, and under the microscope exhibited only amorphous particles The fluid had acquired a reddish hue, and had an offensive putrid smell, and it afforded when boiled a firm coagulum.

Comparing, then, the two, it appears that ammonia renders serum less viscid, prevents its putrefaction,† and modifies in some degree its coagulable property. Whether the serum of the blood of other animals under the influence of ammonia would show the same properties, I have not ascertained with sufficient accuracy. From the few comparative trials I have made, I am disposed to infer that there would be no material difference.

4. *On the Red Corpuscles of the Blood.*—On these the effect of the volatile alkali is more decided, as is shown by the following experiment,—one of the

* Serum of blood, such as I have tried, and I have made many trials, on first evaporation affords a residue which is almost entirely soluble in water, but on repetition again and again, it is so altered as to become insoluble.

† Ammonia does not appear to arrest entirely the putrefactive decomposition of the blood: thus a mixture of 257 grs. of blood, and of 2·5 grs. of aqua ammoniæ, the subject of the third experiment, after having been kept twenty days, had, besides an ammoniacal odour, an offensive smell, indicative of incipient putrefaction. In great excess, it certainly retards the change in the instance of the entire blood, and in a great degree in the instance of the fibrin, and in that of the serum. A portion of the coagulum left from the first experiment and kept three months, had, after the ammonia had been rapidly expelled, an offensive odour, only in a slight degree.

many which I have made. The cruor used was from fresh bullock's blood, its fibrin separated in the usual way; 45 grs. of it were mixed with 48 grs. of aqua ammoniæ. The colour was immediately darkened, so much so, that by reflected light it appeared almost black; by transmitted, of a garnet-red, similar to the change of colour observed when the entire blood was used, and it was accompanied by the same alteration in the corpuscles, these being reduced in size and rendered globular. Another obvious effect was an increase of viscidity. Examined after eight days, and again after twenty-four, the only further change noticeable was the disappearance of the corpuscles, as if they had in great part been dissolved; they were not to be seen under the microscope; minute granules only were visible, and these were seen only after evaporation.

It is worthy of remark, that when the whole of the volatile alkali was expelled by heat at a temperature below 160°, and the residuary fluid was tried by test-paper, only the feeblest alkaline reaction was observable; in this respect, differing from the serum, which under the same circumstances showed a distinct alkaline reaction. The solution was coagulated at a temperature of about 160°. Another portion evaporated at a low temperature was resoluble, *i.e.*, the colouring matter; seeming to show that this matter had suffered no change from the action of the volatile alkali.

The bearings of the results of these several experiments on the hypothesis adverted to, hardly need be dwelt on, they are so obvious. Seeing that ammonia, in so large a quantity as that used in the first experiment, did not prevent the coagulation of the blood, or, in other words, of its fibrin—its coagulable part—it would be strange, indeed, if the escape of a very minute quantity of the volatile alkali, hardly an appreciable one at most, should be the cause of the phenomenon.

Considering that ammonia renders the fibrin viscid and alters the shape of the red corpuscles, is there not ground for caution as regards its medicinal use, and of more than doubt of its efficacy when administered with the intention of dissolving a coagulum in cases of thrombosis? The marked difference as to alkaline reaction of the serum and cruor, as already mentioned, was suggestive of analogy between the blood and the contents of the egg. It is stated that an aqueous solution of the colouring matter of the former is neutral.* Whatever care I have taken in preparing it, draining off the serum as much as possible from the clot before the action of water, I have always found it feebly alkaline.† Nor is this surprising, considering the impossibility of getting rid of all the serum by drain-

* Brande and Taylor's "Chemistry," 1863, p. 833.

† In one experiment, the clot, from six ounces of bullock's blood, after draining off as much as possible of the serum, was cut into small pieces and macerated in water, using the ordinary means to separate the fibrin. The solution formed, loaded with colouring matter, was evaporated at a temperature below 160°, until reduced to the sp. gr. 1033; its alkaline reaction then was very slight, so as to be hardly discernible when the delicate test-paper used was dried.

After evaporation of the solution to dryness, the residue was exposed to the fire in a platina capsule. In its charred state, after it had ceased to burn with flame, its particles were slightly ing. Further, I have found the ash of hæmatine prepared by a more elaborate process also feebly alkaline; and the latest analyses of the several ingredients of the blood, those most to be depended on, indicate the same.* May not this difference, slight though it appears, warrant the conjecture, that as in the egg, so in the blood, there may be an action of a galvanic kind between its several proximate parts? And may not the differences which are known to exist between the serum and the red corpuscles be adduced in favour of the conjecture?

VI. *On the Coagulation of the Blood.*

Of the many hypotheses which have been advanced at different times to account for the coagulation of the blood, each has been supported, as hypotheses usually are, by some facts, but few of them have for any length of time maintained their ground, facts having been adduced hostile to them.

Of the latest hypotheses brought forward, one is that of Dr RICHARDSON, briefly designated the ammonia-theory, to which I have already adverted; another is that of Professor LISTER, in which he considers the phenomenon as mainly depending, out of the body, on a kind of catalytic action produced by the contact of any foreign substance, and, within the body, as owing to an analogous cause, contact with a part, either dead or *quasi* dead,—as he supposes a tissue to be under the influence of inflammation.†

This hypothesis, as it appears to me, is open to certain objections. I shall now notice merely a few of the facts which seem to me most opposed to it.

1. Were it true, ought not the phenomenon of coagulation to take place in every instance in which dead matter comes in contact in the living body with the blood? Instances of ossification, in which concretions of phosphate of lime are formed in the arterial coats, and often project into the vessels themselves,—concretions differing but little from the "tartar," deposited so often on the teeth, and inorganic,—are familiar to every one acquainted with pathological anatomy, and yet in the majority of these cases the coagulation of the blood has not taken place.

2. In instances of aneurism, with a rupture of the vessel, the seat of it, a coagulum of blood is invariably formed, though in contact with parts which, it may be presumed, until the contrary is proved, still retain their vitality.

3. Examples of the coagulation of the blood in the veins, in the arteries, and in the ventricles of the heart, during life, in persons reduced to a feeble state by

attracted by the magnet, the coal after cooling having been reduced to powder. The particles of its ash, after the charcoal had been burnt off, were also similarly attracted. The magnet used, it may be mentioned, was a needle that had been magnetised by a fœtal torpedo, and which (as the result showed) still retained its power, after the elapse of 32 years. The residuary ash, on the addition of a little water, showed so feeble an alkaline reaction, that it was hardly as well marked as that of the saliva.

* LEHMANN'S "Physiological Chemistry," ii. pp. 160, 212.

† Proceedings Roy. Soc. Vol. xii. p. 580.

disease, are not of unfrequent occurrence, and this often without any apparent lesion in the coats of the vessels themselves, or in the lining membrane of the ventricles.

4. Confirmatory of the last, many examples are on record of the blood, in its coagulated state after death, having been found broken up in the left ventricle of the heart, proving that its coagulation must have taken place whilst the heart was still forcibly acting, and this in cases in which the organ appeared to be sound.*

5. Certain poisons influence the coagulation, some accelerating it, some retarding it. As an example of both, may be mentioned the poison of a snake, the tic-polonga of Ceylon (*Daboia Russellii*, Gray), which on fowls acts with extreme rapidity, so much so, that simultaneously with their death, it occasions the coagulation of the blood in the heart and great vessels, and this even before the former has ceased to act; whilst, in larger animals, such as the dog, in which it takes effect less rapidly, causing death in an hour instead of about a minute, it has a contrary influence, that of preventing the coagulation of the blood.† There are other considerations which seem to me to cast a doubt on the accuracy of this hypothesis. To reconcile it with certain facts, its author is under the necessity of assuming that a clot is a "living tissue in relation to the blood;" if so, then does it not follow, in strictness of reasoning, that such must be its state under all conditions, whether formed within the body during life, or in blood abstracted by the ordinary operation of blood-letting; and he is further under the necessity of assuming that inflamed parts are *quasi* dead parts, or, in other words,—and they are his—"have lost for a time their vital properties, and comport themselves like ordinary solids."

The vagueness, moreover, of the hypothesis renders it open to objection. The referring the phenomenon to a catalytic action, seems to be little more than the accounting for what is obscure by that which is equally or hardly less obscure.

To conclude, I fear it must be confessed that, strictly speaking, the theory of the coagulation of the blood, its *vera causa*, is still an unsolved problem, there being, to all the hypotheses which have hitherto been propounded, opposing facts logically in strictness prohibiting the establishment of any one of them.

* See the author's Anatom. and Physiolog. Research., ii. 196.

† Idem, vol. i. p. 123.

V.—*A Study of Trilinear Co-ordinates: being a Consecutive Series of Seventy-two Propositions in Transversals.* By the Rev. HUGH MARTIN, M.A., Free Greyfriars', Edinburgh. Communicated by Professor KELLAND.

(Read 20th March 1865.)

INTRODUCTORY REMARKS.

The following series of theorems is given as an illustration of the modern method of trilinear co-ordinates, having been wrought out after perusal of Mr FERRAR'S very lucid and elegant treatise on that subject. The demonstrations present no difficulty, requiring nothing more complicated than the formation of determinants of two and three places. Accordingly, after exhibiting the method of proof in a few instances, I have merely given the enunciations of the remaining propositions. As the series of theorems advances the manipulation becomes, of course, a little more complicated; but the co-ordinates and co-efficients always appear in such symmetry as very greatly abbreviates the task, and guarantees its accuracy. Two, or perhaps three, of these seventy-two theorems are known mathematical truths; but that so many new consecutive propositions should be so easily found, and so easily proved, is a convincing evidence of the simplicity, fertility, and power of this new and beautiful method.

Treated according to the ancient geometry, the contents of the following pages would constitute a volume of no mean dimensions; and some of the propositions, such as those which affirm that the six points P_1, P_2, P_3, P_4, P_5, P_6 range in a straight line, and that the seven straight lines U_1U_2; R_1R_2, R_3R_4; S_1S_2, S_3S_4; Q_1Q_3, Q_4Q_5, Q_2Q_6 all meet in a point, would probably have been undiscoverable.

In the admirable treatises of MULCAHY and TOWNSEND a few analogous propositions are demonstrated geometrically. Mr TOWNSEND, in particular, has a chapter in his *first* volume, on concurrent lines and co-linear points, which falls in very closely with the kind of propositions which the following series embraces. His *second* volume I have not been fortunate enough to see; but the subject is only ripening for a systematic gathering-up of the propositions that have been discovered in this line of investigation, and the following pages are presented as a humble contribution towards that desirable result.

A word or two may be permitted in reference to the additions to the terminology which must be made, and generally sanctioned by mathematicians, ere such systematic digest can be successfully accomplished. I have ventured—of course only provisionally—on one or two such additions. When the co-ordinates of two points are respectively the algebraical inverses of each other, I have called these points, in reference to each other, "inverse points;" and it is evident that a very fine vein of mathematical truth opens up in reference to them, which it needs only a little ingenuity to work advantageously. Thus, at a glance, it is evident that if a point moves in the straight line $l\alpha+m\beta+n\gamma=0$, its inverse moves in the locus, $\frac{l}{\alpha}+\frac{m}{\beta}+\frac{n}{\gamma}=0$; which is a conic passing through the angular points of the triangle of reference. Since writing the following pages I find Mr TOWNSEND has a chapter entitled "Theory of inverse points with respect to a circle;" and although not treated according to the trilinear method, these points, so called, will be found, I rather think, *if* so treated, to present only a case of what I have called "inverse points" in general. In the same manner I have called two lines "inverse" with respect to each other when the co-efficients of the co-ordinates are respectively the algebraical inverses of each other.

There is another relation between a special point and line which I have not ventured to designate, but to which I would respectfully call attention as requiring designation. When lines from the angular points of a triangle are drawn through any point to intersect the opposite sides, the intersections constitute the angular points of an inscribed triangle, whose sides are known to meet the corresponding sides of the original triangle in points which range in a straight line. Instead of giving a particular designation to this line, I have used the general functional symbol; and, as its position depends exclusively on the point —say P, I have called the line ϕ (P), in a few theorems in reference to it (Theorems XXXI.—XXXV.). Of course the inverse functional symbol ϕ^{-1} indicates the point in reference to the line, as the direct symbol indicates the line with reference to the point. This *point* and *line* are, indeed, with respect to each other, a species of pole and polar,—the line being the ordinary polar, not of the point but of its inverse,—to the imaginary conic $\alpha^2+\beta^2+\gamma^2=0$. Manifestly a special designation is necessary in a case like this, in order to secure that ease of reference and that brevity of treatment without which the pioneering work of farther investigation is brought to a stand.

Theorem LXVI. is the prize question of the "Gentleman's Diary" for 1841; and some long but good geometrical demonstrations of it are given. The proof is perfectly simple according to the trilinear method, and the co-ordinates of the point appear in a form so elegant that one could not help seeing that it must have some singular relations and be worthy of a name. I have accordingly ventured to call it the Anapole of the two given points; and, connecting it with some of the preceding results, I find a few propositions easily deducible, such as that the anapole of two inverse points and the line joining them are pole and polar, to the imaginary conic, $\alpha^2+\beta^2+\gamma^2=0$. For the three concluding theorems I am indebted to a young mathematical friend—destined, I believe and trust, to scientific eminence—Mr GEORGE M. SMITH, student in the Aberdeen University. On proposing to him the problems of finding the locus of the anapole of a central body and its planet, and the locus of the anapole of two points which should move away from each other in a straight line with uniform velocities, I was delighted to receive demonstrations, perfectly simple and elegant, to the effect that, in the former case, the anapole moves in a straight line, let the planet move as it may; and that, in the latter case, the locus of the anapole is a conic section, and becomes a straight line if the uniform velocities are equal; and farther, that the anapole of any two points in an ellipse circumscribing the triangle of reference is invariable. Geometers, I am sure, will admire these theorems of a rising young mathematician, and will recognise the vein thus struck as promising to be a fertile one. Mr SMITH added another very beautiful property of the anapole, which turned out, on investigation, to be identical with Theorem LXV. of the following series.

Instead of defining a point by the equations $\frac{\alpha}{l}=\frac{\beta}{m}=\frac{\gamma}{n}$, we shall say the point is—(l, m, n). Instead of defining a straight line by the equation $l\alpha+m\beta+n\gamma=0$, we shall say the line is—(l, m, n).

The straight line joining the points (l_1, m_1, n_1), (l_2, m_2, n_2) is;—

$$\begin{vmatrix} m_1, & n_1 \\ m_2, & n_2 \end{vmatrix}, \quad \begin{vmatrix} n_1, & l_1 \\ n_2, & l_2 \end{vmatrix}, \quad \begin{vmatrix} l_1, & m_1 \\ l_2, & m_2 \end{vmatrix}.$$

The intersection of the two straight lines (l_1, m_1, n_1), (l_2, m_2, n_2) is defined by the same expression. This identity of form is, *in reality*, the earliest germ of the doctrine of pole and polar; and gives rise to what is *usually* regarded as the first promise of that doctrine, namely, the identity of the condition that the three points (l_1, m_1, n_1), (l_2, m_2, n_2), (l_3, m_3, n_3) shall range in a straight line, with the condition that the three straight lines (l_1, m_1, n_1), (l_2, m_2, n_2), (l_3, m_3, n_3) shall intersect in a point; which is, in both cases,

$$\begin{vmatrix} l_1, & m_1, & n_1 \\ l_2, & m_2, & n_2 \\ l_3, & m_3, & n_3 \end{vmatrix} \doteq 0.$$

THEOREMS.

THEOREM 1.

On the sides of the triangle A B C, as bases, are constructed three triangles, A_1BC, AB_1C, ABC_1, similar to each other, and so placed that the angles $A_1BC=AB_1C=ABC_1$; $B_1CA=BC_1A=BCA_1$; and $C_1AB=CA_1B=CAB_1$. Then AA_1, BB_1, CC_1 meet in a point.

The sides of A B C, taken as the triangle of reference, being a, b, c, the perpendiculars from A_1 on a, b, c are, respectively,

$$\frac{a \,.\, \sin B_1 \,.\, \sin C_1}{\sin A_1}, \quad \frac{a \,.\, \sin B_1 \,.\, \sin (C+C_1)}{\sin A_1}, \quad \frac{a \,.\, \sin C_1 \,.\, \sin (B+B_1)}{\sin A_1}.$$

$$\begin{array}{llll}
\text{Hence the trilinear co-ordinates of } A_1 \text{ are} & -1, & \dfrac{\sin(C+C_1)}{\sin C_1}, & \dfrac{\sin(B+B_1)}{\sin B_1}. \\[2ex]
\quad ,, \qquad ,, \qquad \text{of } B_1 \text{ are} & \dfrac{\sin(C+C_1)}{\sin C_1}, & -1, & \dfrac{\sin(A+A_1)}{\sin A_1}. \\[2ex]
\quad ,, \qquad ,, \qquad \text{of } C_1 \text{ are} & \dfrac{\sin(B+B_1)}{\sin B_1}, & \dfrac{\sin(C+C_1)}{\sin C_1}, & -1.
\end{array}$$

For $\dfrac{\sin(A+A_1)}{\sin A_1}$, $\dfrac{\sin(B+B_1)}{\sin B_1}$, $\dfrac{\sin(C+C_1)}{\sin C_1}$, put f, g, h. Then ;—

$$\left.\begin{array}{llll} A_1 \text{ is} & -1, & h, & g. \\ B_1 \text{ is} & h, & -1, & f. \\ C_1 \text{ is} & g, & f, & -1. \end{array}\right\} . \quad \text{Also,} \quad \left\{\begin{array}{llll} A \text{ is} & 1, & 0, & 0. \\ B \text{ is} & 0, & 1, & 0. \\ C \text{ is} & 0, & 0, & 1. \end{array}\right\} \text{ Hence ;—}$$

$$\begin{array}{l} A\,A_1 \text{ is} \\ B\,B_1 \text{ is} \\ C\,C_1 \text{ is} \end{array} \left|\begin{array}{ccc} 0, & g, & -h. \\ -f, & 0, & h. \\ f, & -g, & 0. \end{array}\right| \equiv 0. \quad \text{Therefore } A\,A_1,\ B\,B_1,\ C\,C_1 \text{ intersect in a point—say } P_1.$$

P_1 is gh, hf, fg, or f^{-1}, g^{-1}, h^{-1}.

THEOREM 2.

$$\left.\begin{array}{llll} B_1C_1 \text{ is} & 1-f^2, & h+fg, & g+hf \\ C_1A_1 \text{ is} & h+fg, & 1-g^2, & f+gh \\ A_1B_1 \text{ is} & g+hf, & f+gh, & 1-h^2 \end{array}\right\} . \quad \text{Also,} \quad \left\{\begin{array}{llll} BC \text{ is} & 1, & 0, & 0. \\ CA \text{ is} & 0, & 1, & 0. \\ AB \text{ is} & 0, & 0, & 1. \end{array}\right\} \text{ Hence ;—}$$

$$\begin{array}{llll} BC,\ B_1C_1 & \text{meet in} & \mathfrak{A}_1 & \text{which is} \\ CA,\ C_1A_1 & ,, & \mathfrak{B}_1 & ,, \\ AB,\ A_1B_1 & ,, & \mathfrak{C}_1 & ,, \end{array} \left|\begin{array}{ccc} 0, & g+hf, & -(h+fg). \\ -(f+gh), & 0, & h+fg. \\ f+gh, & -(g+hf), & 0. \end{array}\right| \equiv 0. \quad \text{Therefore ;—}$$

$\mathfrak{A}_1\mathfrak{B}_1\mathfrak{C}_1$ is a straight line, viz., $(f+gh)^{-1}$, $(g+hf)^{-1}$, $(h+fg)^{-1}$.

THEOREM 3.

$$\left.\begin{array}{l} B\,C_1 \text{ is } 1,\ 0,\ g \\ C\,A_1 \text{ is } h,\ 1,\ 0 \\ A\,B_1 \text{ is } 0,\ f,\ 1 \end{array}\right\} . \ \text{Also} \left\{\begin{array}{l} B_1C \text{ is } 1,\ h,\ 0 \\ C_1A \text{ is } 0,\ 1,\ f \\ A_1B \text{ is } g,\ 0,\ 1 \end{array}\right\} . \ \text{Hence} \left\{\begin{array}{llll} B\,C_1,\ B_1C & \text{meet in} & A_2, & \text{viz.} \\ C\,A_1,\ C_1A & ,, & B_2, & ,, \\ A\,B_1,\ A_1B & ,, & C_2, & ,, \end{array}\right. \left|\begin{array}{ccc} -hg, & g, & h \\ f, & -hf, & h \\ f, & g, & -fg \end{array}\right|$$

$$\begin{array}{l} A\,A_2 \text{ is} \\ B\,B_2 \text{ is} \\ C\,C_2 \text{ is} \end{array} \left|\begin{array}{ccc} 0, & -h, & g \\ h, & 0, & -f \\ -g, & f, & 0 \end{array}\right| \equiv 0. \quad \text{Therefore } A\,A_2,\ B\,B_2,\ C\,C_2 \text{ intersect in a point—say } P_2.$$

P_2 is f, g, h.

P_1 and P_2 may be called inverse or reciprocal points.

THEOREM 4.

$$\begin{array}{l} BC \text{ is} \\ B_1C_1 \text{ is} \\ B_2C_2 \text{ is} \end{array} \left|\begin{array}{ccc} 1, & 0, & 0. \\ 1-f^2, & h+fg, & g+hf. \\ -gh(1-f^2), & f(h+fg), & f(g+hf). \end{array}\right| \equiv 0. \quad \text{Therefore ;—}$$

$$\begin{array}{llll} & BC,\ B_1C_1,\ B_2C_2 & \text{intersect in} & \mathfrak{A}_1. \\ * & CA,\ C_1A_1,\ C_2A_2 & ,, & \mathfrak{B}_1. \\ & AB,\ A_1B_1,\ A_2B_2 & ,, & \mathfrak{C}_1. \end{array}$$

* By symmetry.

Theorem 5.

$$\begin{array}{l} A_1A_2 \text{ is} \\ B_1B_2 \text{ is} \\ C_1C_2 \text{ is} \end{array} \begin{vmatrix} g^2-h^2, & -h\,(1-g^2), & g\,(1-h^2) \\ h\,(1-f^2), & h^2-f^2, & -f\,(1-h^2) \\ -g\,(1-f^2), & f\,(1-g^2), & f^2-g^2 \end{vmatrix} \equiv 0. \text{ Therefore ;—}$$

A_1A_2, B_1B_2, C_1C_2 intersect in a point,—say P_3, viz., $f-gh$, $g-hf$, $h-fg$.

Theorem 6.

$$\begin{array}{l} P_1 \text{ is} \\ P_2 \text{ is} \\ P_3 \text{ is} \end{array} \begin{vmatrix} gh, & hf, & fg. \\ f, & g, & h. \\ f-gh, & g-hf, & h-fg. \end{vmatrix} \equiv 0. \text{ Therefore } P_1,\ P_2,\ P_3 \text{ range in a straight line.}$$

$$P_1P_2P_3 \text{ is } f\,(g^2-h^2),\ g\,(h^2-f^2),\ h\,(f^2-g^2).$$

Theorem 7.

$$\begin{array}{lll} A_1B,\ BC \text{ meet in } \mathfrak{A}_2, \text{ viz.,} \\ B_1C,\ CA \quad ,, \quad \mathfrak{B}_2, \quad ,, \\ C_1A,\ AB \quad ,, \quad \mathfrak{C}_2, \quad ,, \end{array} \left\{\begin{array}{rrr} 0, & 1, & -f \\ -g, & 0, & 1 \\ 1, & -h, & 0 \end{array}\right\}. \quad \text{Also} \left\{\begin{array}{l} C_1A,\ BC \text{ meet in } \mathfrak{A}_3 \text{ viz.,} \\ A_1B,\ CA \quad ,, \quad \mathfrak{B}_3 \quad ,, \\ B_1C,\ AB \quad ,, \quad \mathfrak{C}_3 \quad ,, \end{array}\right. \left\{\begin{array}{rrr} 0, & f, & -1 \\ -1, & 0, & g \\ h, & -1, & 0 \end{array}\right\}.$$

$$\text{Hence ;—} \quad \begin{array}{l} \mathfrak{B}_2\mathfrak{C}_2 \text{ is} \\ \mathfrak{C}_2\mathfrak{A}_2 \text{ is} \\ \mathfrak{A}_2\mathfrak{B}_2 \text{ is} \end{array} \left\{\begin{array}{lll} h, & 1, & gh \\ hf, & f, & 1 \\ 1, & fg, & g \end{array}\right\}; \quad \begin{array}{l} \mathfrak{B}_3\mathfrak{C}_3 \text{ is} \\ \mathfrak{C}_3\mathfrak{A}_3 \text{ is} \\ \mathfrak{A}_3\mathfrak{B}_3 \text{ is} \end{array} \left\{\begin{array}{lll} g, & gh, & 1, \\ 1, & h, & hf, \\ fg, & 1, & f, \end{array}\right\};$$

$$\text{Also ;—} \quad \begin{array}{l} \mathfrak{B}_2\mathfrak{C}_2,\ \mathfrak{B}_3\mathfrak{C}_3 \text{ meet in } A_3, \text{ viz.,} \\ \mathfrak{C}_2\mathfrak{A}_2,\ \mathfrak{C}_3\mathfrak{A}_3 \quad ,, \quad B_3, \quad ,, \\ \mathfrak{A}_2\mathfrak{B}_2,\ \mathfrak{A}_3\mathfrak{B}_3 \quad ,, \quad C_3, \quad ,, \end{array} \left\{\begin{array}{lll} 1-g^2h^2, & h\,(g^2-1), & g\,(h^2-1) \\ h\,(f^2-1), & 1-h^2f^2, & f\,(h^2-1) \\ g\,(f^2-1), & f\,(g^2-1), & 1-f^2g^2 \end{array}\right\}.$$

$$\text{Farther ;—} \quad \begin{array}{l} \mathfrak{B}_2\mathfrak{C}_3 \text{ is} \\ \mathfrak{C}_2\mathfrak{A}_3 \text{ is} \\ \mathfrak{A}_2\mathfrak{B}_3 \text{ is} \end{array} \left\{\begin{array}{lll} 1, & h, & g \\ h, & 1, & f \\ g, & f, & 1 \end{array}\right\}; \quad \begin{array}{l} \mathfrak{B}_3\mathfrak{C}_2 \text{ is} \\ \mathfrak{C}_3\mathfrak{A}_2 \text{ is} \\ \mathfrak{A}_3\mathfrak{B}_2 \text{ is} \end{array} \left\{\begin{array}{lll} gh, & g, & h \\ f, & hf, & h \\ f, & g, & fg \end{array}\right\};$$

$$\text{Hence ;—} \quad \begin{array}{l} \mathfrak{B}_2\mathfrak{C}_3,\ \mathfrak{B}_3\mathfrak{C}_2 \text{ meet in } A_4, \text{ viz.,} \\ \mathfrak{C}_2\mathfrak{A}_3,\ \mathfrak{C}_3\mathfrak{A}_2 \quad ,, \quad B_4, \quad ,, \\ \mathfrak{A}_2\mathfrak{B}_3,\ \mathfrak{A}_3\mathfrak{B}_2 \quad ,, \quad C_4, \quad ,, \end{array} \left\{\begin{array}{lll} h^2-g^2, & h\,(g^2-1), & g\,(h^2-1) \\ h\,(f^2-1), & f^2-h^2, & f\,(h^2-1) \\ g\,(f^2-1), & f\,(g^2-1), & g^2-f^2 \end{array}\right\}.$$

$$\text{Now ;—} \quad \begin{array}{l} A \text{ being} \\ A_3 \quad ,, \\ A_4 \quad ,, \end{array} \begin{vmatrix} 1, & 0, & 0 \\ 1-g^2h^2, & h\,(g^2-1), & g\,(h^2-1) \\ h^2-g^2, & h\,(g^2-1), & g\,(h^2-1) \end{vmatrix} \equiv 0. \text{ Therefore ;—}$$

$A\,A_3A_4$ is a straight line.
$B\,B_3B_4$,, ,,
$C\,C_3C_4$,, ,,

Theorem 8.

$$\begin{array}{l} A\,A_3A_4 \text{ is} \\ B\,B_3B_4 \text{ is} \\ C\,C_3C_4 \text{ is} \end{array} \begin{vmatrix} 0, & g\,(1-h^2), & -h\,(1-g^2) \\ -f\,(1-h^2), & 0, & h\,(1-f^2) \\ f\,(1-g^2), & -g\,(1-f^2), & 0 \end{vmatrix} \equiv 0. \text{ Therefore ;—}$$

$A\,A_3A_4$, $B\,B_3B_4$, $C\,C_3C_4$ meet in a point P_4, viz., $\dfrac{1-f^2}{f}$, $\dfrac{1-g^2}{g}$, $\dfrac{1-h^2}{h}$.

Theorem 9.

$$\begin{array}{l} A_1 \text{ is} \\ A_2 \text{ is} \\ A_3 \text{ is} \end{array} \left| \begin{array}{ccc} -1, & h, & g \\ -gh, & g, & h \\ 1-g^2h^2, & h(g^2-1), & g(h^2-1) \end{array} \right| \equiv 0. \quad \text{Therefore ;—}$$

$A_1A_2A_3$ is a straight line.
$B_1B_2B_3$ „ „
$C_1C_2C_3$ „ „

Theorem 10.

$$\left.\begin{array}{llll} B_3C_3 \text{ is } 1+fgh, & h+fg, & g+hf; & \text{meeting } BC \text{ in } \mathfrak{A}_1 \\ C_3A_3 \text{ is } h+fg, & 1+fgh, & f+gh; & \quad ,, \quad CA \;,,\; \mathfrak{B}_1 \\ A_3B_3 \text{ is } g+hf, & f+gh, & 1+fgh; & \quad ,, \quad AB \;,,\; \mathfrak{C}_1 \end{array}\right\}. \quad \text{Therefore ;—}$$

BC, B_1C_1, B_2C_2, B_3C_3 meet in $\mathfrak{A}_1$.
CA, C_1A_1, C_2A_2, C_3A_3 „ $\mathfrak{B}_1$.
AB, A_1B_1, A_2B_2, A_3B_3 „ $\mathfrak{C}_1$.

Theorem 11.

$$\begin{array}{l} BC,\ B_4C_4 \text{ meet in } \mathfrak{A}_4, \text{ viz.,} \\ CA,\ C_4A_4 \quad ,, \quad \mathfrak{B}_4, \;,, \\ AB,\ A_4B_4 \quad ,, \quad \mathfrak{C}_4, \;,, \end{array} \left| \begin{array}{ccc} 0, & (f-gh).(h+fg), & -(f+gh).(g-hf) \\ -(g+hf).(h-fg), & 0, & (g-hf).(f+gh) \\ (h-fg).(g+hf), & -(h+fg).(f-gh), & 0 \end{array} \right| \equiv 0.$$

Therefore $\mathfrak{A}_4\mathfrak{B}_4\mathfrak{C}_4$ is a straight line, viz., $\dfrac{1}{(g+hf).(h-fg)}$, $\dfrac{1}{(h+fg).(f-gh)}$, $\dfrac{1}{(f+gh).(g-hf)}$.

Theorem 12.

$$\begin{array}{l} B_1C,\ A_1B \text{ meet in } A_5, \text{ viz.,} \\ C_1A,\ B_1C \quad ,, \quad B_5, \;,, \\ A_1B,\ C_1A \quad ,, \quad C_5, \;,, \end{array} \left\{ \begin{array}{ccc} -h, & 1, & gh \\ hf, & -f, & 1 \\ 1, & fg, & -g \end{array} \right\}. \quad \begin{array}{l} BC_1,\ CA_1 \text{ meet in } A_6, \text{ viz.,} \\ CA_1,\ AB_1 \quad ,, \quad B_6, \;,, \\ AB_1,\ BC_1 \quad ,, \quad C_6, \;,, \end{array} \left\{ \begin{array}{ccc} -g, & gh, & 1 \\ 1, & -h, & hf \\ fg, & 1, & -f \end{array} \right\}.$$

$$\begin{array}{l} A_5A_6,\ BC \text{ meet in } \mathfrak{A}_5, \text{ viz.,} \\ B_5B_6,\ CA \quad ,, \quad \mathfrak{B}_5, \;,, \\ C_5C_6,\ AB \quad ,, \quad \mathfrak{C}_5, \;,, \end{array} \left| \begin{array}{ccc} 0, & g(1-h^2), & -h(1-g^2) \\ -f(1-h^2), & 0, & h(1-f^2) \\ f(1-g^2), & -g(1-f^2), & 0 \end{array} \right| \equiv 0. \quad \text{Therefore ;—}$$

$\mathfrak{A}_5\mathfrak{B}_5\mathfrak{C}_5$ is a straight line, viz., $\dfrac{1-f^2}{f}$, $\dfrac{1-g^2}{g}$, $\dfrac{1-h^2}{h}$.

Theorem 13.

$$\begin{array}{l} BC \text{ is} \\ B_5C_6 \text{ is} \\ B_6C_5 \text{ is} \end{array} \left| \begin{array}{ccc} 1, & 0, & 0 \\ f^2-1, & f(g+hf), & f(h+fg) \\ hg(1-f^2), & g+hf, & h+fg \end{array} \right| \equiv 0. \quad \text{Therefore ;—}$$

BC, B_5C_6, B_6C_5 meet in a point: $\mathfrak{A}_6$
CA, C_5A_6, C_6A_5 „ „ $\mathfrak{B}_6$
AB, A_5B_6, A_6B_5 „ „ $\mathfrak{C}_6$

Theorem 14.

$$\begin{array}{r} \mathfrak{A}_6 \text{ is} \\ \mathfrak{B}_6 \text{ is} \\ \mathfrak{C}_6 \text{ is} \end{array} \begin{vmatrix} 0, & h+fg, & -(g+hf) \\ -(h+fg), & 0, & f+gh \\ g+hf, & -(f+gh), & 0 \end{vmatrix} \equiv 0. \quad \text{Therefore ;—}$$

$\mathfrak{A}_6\mathfrak{B}_6\mathfrak{C}_6$ is a straight line, viz., $f+gh,\ g+hf,\ h+fg.$

$\mathfrak{A}_1\mathfrak{B}_1\mathfrak{C}_1$ and $\mathfrak{A}_6\mathfrak{B}_6\mathfrak{C}_6$ are inverse or reciprocal lines.

Theorem 15.

$$\begin{array}{r} \mathfrak{B}_1\mathfrak{C}_6 \text{ is} \\ \mathfrak{C}_1\mathfrak{A}_6 \text{ is} \\ \mathfrak{A}_1\mathfrak{B}_6 \text{ is} \end{array} \left| \begin{matrix} (h+fg)\,.\,(f+gh), & (g+hf)\,.\,(h+fg), & (f+gh)^2 \\ (g+hf)^2, & (f+gh)\,.\,(g+hf), & (h+fg)\,.\,(f+gh) \\ (f+gh)\,.\,(g+hf), & (h+fg)^2, & (g+hf)\,.\,(h+fg) \end{matrix} \right| \begin{array}{l} \text{and meets B C in } \mathfrak{A}_7 \\ \quad ,, \quad \text{C A in } \mathfrak{B}_7 \\ \quad ,, \quad \text{A B in } \mathfrak{C}_7 \end{array}$$

$$\begin{array}{r} \mathfrak{A}_7 \text{ is} \\ \mathfrak{B}_7 \text{ is} \\ \mathfrak{C}_7 \text{ is} \end{array} \begin{vmatrix} 0, & (f+gh)^2, & -(g+hf)\,.\,(h+fg) \\ -(h+fg)\,.\,(f+gh), & 0, & (g+hf)^2 \\ (h+fg)^2, & -(f+gh)\,.\,(g+hf), & 0 \end{vmatrix} \equiv 0. \quad \text{Therefore ;—}$$

$\mathfrak{A}_7\mathfrak{B}_7\mathfrak{C}_7$ is a straight line, viz., $\dfrac{g+hf}{h+fg},\ \dfrac{h+fg}{f+gh},\ \dfrac{f+gh}{g+hf}.$

Theorem 16.

$$\text{Similarly,} \left\{ \begin{array}{lll} \mathfrak{B}_6\mathfrak{C}_1 & \text{meeting} & \text{B C in } \mathfrak{A}_8 \\ \mathfrak{C}_6\mathfrak{A}_1 & \quad ,, & \text{C A in } \mathfrak{B}_8 \\ \mathfrak{A}_6\mathfrak{B}_1 & \quad ,, & \text{A B in } \mathfrak{C}_8 \end{array} \right\}, \ \mathfrak{A}_8\mathfrak{B}_8\mathfrak{C}_8 \text{ is a straight line, viz., } \frac{h+fg}{g+hf},\ \frac{f+gh}{h+fg},\ \frac{g+hf}{f+gh}.$$

$\mathfrak{A}_7\mathfrak{B}_7\mathfrak{C}_7$ and $\mathfrak{A}_8\mathfrak{B}_8\mathfrak{C}_8$ are reciprocal lines.

Theorem 17.

$$\begin{array}{r} \text{A A}_1 \text{ is} \\ \text{B B}_5 \text{ is} \\ \text{C C}_6 \text{ is} \end{array} \begin{vmatrix} 0, & g, & -h \\ -1, & 0, & hf \\ 1, & -fg, & 0 \end{vmatrix} \equiv 0. \quad \text{Also} \left\{ \begin{array}{r} \text{A A}_2 \text{ is} \\ \text{B B}_6 \text{ is} \\ \text{C C}_5 \text{ is} \end{array} \right. \begin{vmatrix} 0, & h, & -g \\ -hf, & 0, & 1 \\ fg, & -1, & 0 \end{vmatrix} \equiv 0. \quad \text{Therefore ;—}$$

A A_1, B B_5, C C_6 meet in a point A_7	viz.,	fgh,	h,	g.
B B_1, C C_5, A A_6 ,, ,, B_7	,,	h,	fgh,	f.
C C_1, A A_5, B B_6 ,, ,, C_7	,,	g,	f,	fgh.

Theorem 18.

A A_2, B B_6, C C_5 meet in a point A_8	viz.,	1,	fg,	hf.
B B_2, C C_6, A A_5 ,, ,, B_8	,,	fg,	1,	gh.
C C_2, A A_6, B B_5 ,, ,, C_8	,,	hf,	gh,	1.

But instead of continuing the manipulation, we shall gather up these results, and continue the series of propositions.

Theorem I.

AA_1, BB_1, CC_1 intersect, in a point, P_1.

Theorem II.

$$\left.\begin{array}{llcl}\text{Let } BC, & B_1C_1 & \text{meet in} & \mathfrak{A}_1 \\ CA, & C_1A_1 & ,, & \mathfrak{B}_1 \\ AB, & A_1B_1 & ,, & \mathfrak{C}_1\end{array}\right\}. \quad \text{Then ;—}\mathfrak{A}_1\mathfrak{B}_1\mathfrak{C}_1 \text{ is a straight line.}$$

Theorem III.

$$\left.\begin{array}{llcl}\text{Let } BC_1, & B_1C & \text{meet in} & A_2 \\ CA_1, & C_1A & ,, & B_2 \\ AB_1, & A_1B & ,, & C_2\end{array}\right\}. \quad \text{Then ;—}AA_2,\ BB_2,\ CC_2 \text{ meet in a point, } P_2.$$

P_1 and P_2 are reciprocal points.

Theorem IV.

$$\begin{array}{llllll} BC, & B_1C_1, & B_2C_2 & \text{intersect in a point, and that point is} & & \mathfrak{A}_1 \\ CA, & C_1A_1, & C_2A_2 & ,, \quad ,, \quad ,, & & \mathfrak{B}_1 \\ AB, & A_1B_1, & A_2B_2 & ,, \quad ,, \quad ,, & & \mathfrak{C}_1\end{array}$$

Theorem V.

A_1A_2, B_1B_2, C_1C_2 intersect in a point, P_3.

Theorem VI.

$P_1P_2P_3$ is a straight line.

Theorem VII.

$$\left.\begin{array}{llcl}\text{Let } A_1B, & BC & \text{meet in} & \mathfrak{A}_2 \\ B_1C, & CA & ,, & \mathfrak{B}_2 \\ CA, & AB & ,, & \mathfrak{C}_2\end{array}\right\}; \quad \left.\begin{array}{llcl}\text{And let } C_1A, & BC & \text{meet in} & \mathfrak{A}_3 \\ A_1B, & CA & ,, & \mathfrak{B}_3 \\ B_1C, & AB & ,, & \mathfrak{C}_3\end{array}\right\}.$$

$$\left.\begin{array}{llcl}\text{Also let } \mathfrak{B}_2\mathfrak{C}_2, & \mathfrak{B}_3\mathfrak{C}_3 & \text{meet in} & A_3 \\ \mathfrak{C}_2\mathfrak{A}_2, & \mathfrak{C}_3\mathfrak{A}_3 & ,, & B_3 \\ \mathfrak{A}_2\mathfrak{B}_2, & \mathfrak{A}_3\mathfrak{B}_3 & ,, & C_3\end{array}\right\}; \quad \left.\begin{array}{llcl}\text{And let } \mathfrak{B}_2\mathfrak{C}_3, & \mathfrak{B}_3\mathfrak{C}_2 & \text{meet in} & A_4 \\ \mathfrak{C}_2\mathfrak{A}_3, & \mathfrak{C}_3\mathfrak{A}_2 & ,, & B_4 \\ \mathfrak{A}_2\mathfrak{B}_3, & \mathfrak{A}_3\mathfrak{B}_2 & ,, & C_4\end{array}\right\}.$$

$$\begin{array}{lll}\text{Then,—}AA_3A_4 & \text{is a straight line.} & \\ BB_3B_4 & ,, & ,, \\ CC_3C_4 & ,, & ,,\end{array}$$

Theorem VIII.

AA_3A_4, BB_3B_4, CC_3C_4 meet in a point, P_4.

Theorem IX.

$$\begin{array}{lll}A_1A_2A_3 & \text{is a straight line.} & \\ B_1B_2B_3 & ,, & ,, \\ C_1C_2C_3 & ,, & ,,\end{array}$$

THEOREM X.

$BC,\ B_1C_1,\ B_2C_2,\ B_3C_3$ all meet in $\mathfrak{A}_1$.
$CA,\ C_1A_1,\ C_2A_2,\ C_3A_3$ „ $\mathfrak{B}_1$.
$AB,\ A_1B_1,\ A_2B_2,\ A_3B_3$ „ $\mathfrak{C}_1$.

THEOREM XI.

$$\left.\begin{array}{lll} \text{Let } BC,\ B_4C_4 & \text{meet in} & \mathfrak{A}_4 \\ CA,\ C_4A_4 & „ & \mathfrak{B}_4 \\ AB,\ A_4B_4 & „ & \mathfrak{C}_4 \end{array}\right\}. \quad \text{Then ;—}\mathfrak{A}_4\mathfrak{B}_4\mathfrak{C}_4 \text{ is a straight line.}$$

THEOREM XII.

$$\left.\begin{array}{lll} \text{Let } B_1C,\ A_1B & \text{meet in} & A_5 \\ C_1A,\ B_1C & „ & B_5 \\ A_1B,\ C_1A & „ & C_5 \end{array}\right\};\text{—} \qquad \left.\begin{array}{lll} \text{And let } BC_1,\ CA_1 & \text{meet in} & A_6 \\ CA_1,\ AB_1 & „ & B_6 \\ AB_1,\ BC_1 & „ & C_6 \end{array}\right\}.$$

$$\left.\begin{array}{lll} \text{Also let } BC,\ A_5A_6 & \text{meet in} & \mathfrak{A}_5 \\ CA,\ B_5B_6 & „ & \mathfrak{B}_5 \\ AB,\ C_5C_6 & „ & \mathfrak{C}_5 \end{array}\right\}. \quad \text{Then ;—}\mathfrak{A}_5\mathfrak{B}_5\mathfrak{C}_5 \text{ is a straight line.}$$

THEOREM XIII.

$BC,\ B_5C_6,\ B_6C_5$ meet in a point, $\mathfrak{A}_6$.
$CA,\ C_5A_6,\ C_6A_5$ „ „ $\mathfrak{B}_6$.
$AB,\ A_5B_6,\ A_6B_5$ „ „ $\mathfrak{C}_6$.

THEOREM XIV.

$\mathfrak{A}_6\mathfrak{B}_6\mathfrak{C}_6$ is a straight line; reciprocal to $\mathfrak{A}_1\mathfrak{B}_1\mathfrak{C}_1$.

THEOREM XV.

$$\left.\begin{array}{lll} \text{Let } BC,\ \mathfrak{B}_1\mathfrak{C}_6 & \text{meet in} & \mathfrak{A}_7 \\ CA,\ \mathfrak{C}_1\mathfrak{A}_6 & „ & \mathfrak{B}_7 \\ AB,\ \mathfrak{A}_1\mathfrak{A}_6 & „ & \mathfrak{C}_7 \end{array}\right\}. \quad \text{Then ;—}\mathfrak{A}_7\mathfrak{B}_7\mathfrak{C}_7 \text{ is a straight line.}$$

THEOREM XVI.

$$\left.\begin{array}{lll} \text{Let } BC,\ \mathfrak{B}_6\mathfrak{C}_1 & \text{meet in} & \mathfrak{A}_8 \\ CA,\ \mathfrak{C}_6\mathfrak{A}_1 & „ & \mathfrak{B}_8 \\ AB,\ \mathfrak{A}_6\mathfrak{B}_1 & „ & \mathfrak{C}_8 \end{array}\right\}. \quad \text{Then ;—}\mathfrak{A}_8\mathfrak{B}_8\mathfrak{C}_8 \text{ is a straight line; reciprocal to } \mathfrak{A}_7\mathfrak{B}_7\mathfrak{C}_7.$$

THEOREM XVII.

$AA_1,\ BB_5,\ CC_6$ meet in a point, A_7.
$BB_1,\ CC_5,\ AA_6$ „ „ B_7.
$CC_1,\ AA_5,\ BB_6$ „ „ C_7.

THEOREM XVIII.

$AA_2,\ BB_6,\ CC_5$ meet in a point, A_8.
$BB_2,\ CC_6,\ AA_5$ „ „ B_8.
$CC_2,\ AA_6,\ BB_5$ „ „ C_8.

THEOREM XIX.

$A_7A_8,\ B_7B_8,\ C_7C_8$ meet in a point P_5.

Theorem XX.

BC, B_7C_7, B_8C_8 meet in a point, $\mathfrak{A}_9$.
CA, C_7A_7, C_8A_8 „ „ $\mathfrak{B}_9$.
AB, A_7B_7, A_8B_8 „ „ $\mathfrak{C}_9$.

Theorem XXI.

$\mathfrak{A}_9\mathfrak{B}_9\mathfrak{C}_9$ is a straight line.

Theorem XXII.

BC, AA_2, A_8P_1, A_7P_2 all meet in a point, $\mathfrak{A}_{10}$.
CA, BB_2, B_8P_1, B_7P_2 „ „ $\mathfrak{B}_{10}$.
AB, CC_2, C_8P_1, C_7P_2 „ „ $\mathfrak{C}_{10}$.

Theorem XXIII.

$A\mathfrak{A}_9$, AC, $A\mathfrak{A}_{10}$, AB is a harmonic pencil.
$B\mathfrak{B}_9$, BA, $B\mathfrak{B}_{10}$, BC „ „
$C\mathfrak{C}_9$, CB, $C\mathfrak{C}_{10}$, CA „ „

Theorem XXIV.

BC, A_1P_2, A_2P_1 meet in a point, $\mathfrak{A}_{11}$.
CA, B_1P_2, B_2P_1 „ „ $\mathfrak{B}_{11}$.
AB, C_1P_2, C_2P_1 „ „ $\mathfrak{C}_{11}$.

Theorem XXV.

$A\mathfrak{A}_{11}$, $B\mathfrak{B}_{11}$, $C\mathfrak{C}_{11}$ intersect in a point, P_6.

Theorem XXVI.

BC, A_1A_8, A_2A_7 meet in a point, $\mathfrak{A}_{12}$.
CA, B_1B_8, B_2B_7 „ „ $\mathfrak{B}_{12}$.
AB, C_1C_8, C_2C_7 „ „ $\mathfrak{C}_{12}$.

Theorem XXVII.

$A\mathfrak{A}_{12}$, $B\mathfrak{B}_{12}$, $C\mathfrak{C}_{12}$ intersect in a point, Q_1.

Theorem XXVIII.

$$\text{Let } \left.\begin{array}{llll} BC, & A_7A_8 & \text{meet in} & \mathfrak{A}_{13} \\ CA, & B_7B_8 & \text{,,} & \mathfrak{B}_{13} \\ AB, & C_7C_8 & \text{,,} & \mathfrak{C}_{13} \end{array}\right\} \text{;—Then, } A\mathfrak{A}_{13},\ B\mathfrak{B}_{13},\ C\mathfrak{C}_{13} \text{ intersect in a point, } Q_2.$$

Theorem XXIX.

$$\text{Let } \left.\begin{array}{llll} B_5B_6, & C_5C_6 & \text{meet in} & A_9 \\ C_5C_6, & A_5A_6 & \text{,,} & B_9 \\ A_5A_6, & B_5B_6 & \text{,,} & C_9 \end{array}\right\} \text{;—Then, } AA_9,\ BB_9,\ CC_9 \text{ intersect in a point, } Q_3.$$

Theorem XXX.

$$\text{Let } \left.\begin{array}{llll} A_1A_2, & A_5A_6 & \text{meet in} & A_{10} \\ B_1B_2, & B_5B_6 & \text{,,} & B_{10} \\ C_1C_2, & C_5C_6 & \text{,,} & C_{10} \end{array}\right\} \text{;—Then, } AA_{10},\ BB_{10},\ CC_{10} \text{ intersect in a point, } Q_4.$$

Theorem XXXI.

$$\text{Let} \left.\begin{array}{lll} BC,\ AA_7 & \text{meet in} & l \\ CA,\ BA_7 & ,, & m \\ AB,\ CA_7 & ,, & n \end{array}\right\}; \quad \text{And let} \left.\begin{array}{lll} BC,\ mn & \text{meet in} & l_1 \\ CA,\ nl & ,, & m_1 \\ AB,\ lm & ,, & n_1 \end{array}\right\}.$$

Then $l_1m_1n_1$ is a straight line, as is well known. As it depends entirely on the position of A_7 let it be called $\phi\ (A_7)$. Then ;—

$$\begin{array}{llll} \phi\ (A_7) \text{ is the polar of } & A_8 & \text{to the imaginary conic,} & \alpha^2+\beta^2+\gamma^2=0. \\ \phi\ (B_7) \quad ,, & B_8 & ,, \quad ,, & ,, \\ \phi\ (C_7) \quad ,, & C_8 & ,, \quad ,, & ,, \end{array}$$

Theorem XXXII.

$$\begin{array}{llll} \phi\ (A_8) \text{ is the polar of } & A_7 & \text{to the imaginary conic,} & \alpha^2+\beta^2+\gamma^2=0. \\ \phi\ (B_8) \quad ,, & B_7 & ,, \quad ,, & ,, \\ \phi\ (C_8) \quad ,, & C_7 & ,, \quad ,, & ,, \end{array}$$

Theorem XXXIII.

$$\begin{array}{llll} \phi\ (P_1) \text{ is the polar of } & P_2 & \text{to the imaginary conic,} & \alpha^2+\beta^2+\gamma^2=0. \\ \phi\ (P_2) \quad ,, & P_1 & ,, \quad ,, & ,, \end{array}$$

Theorem XXXIV.

$$\begin{array}{llll} \phi^{-1}\ (\mathfrak{A}_1 \mathfrak{A}_1 \mathfrak{C}_1) \text{ is the pole of } & \mathfrak{A}_6 \mathfrak{B}_6 \mathfrak{C}_6 & \text{to the conic,} & \alpha^2+\beta^2+\gamma^2=0. \\ \phi^{-1}\ (\mathfrak{A}_6 \mathfrak{B}_6 \mathfrak{C}_6) \quad ,, & \mathfrak{A}_1 \mathfrak{B}_1 \mathfrak{C}_1 & ,, & ,, \end{array}$$

Theorem XXXV.

$$\begin{array}{llll} \phi^{-1}\ (\mathfrak{A}_7 \mathfrak{B}_7 \mathfrak{C}_7) \text{ is the pole of } & \mathfrak{A}_8 \mathfrak{B}_8 \mathfrak{C}_8 & \text{to the conic,} & \alpha^2+\beta^2+\gamma^2=0. \\ \phi^{-1}\ (\mathfrak{A}_8 \mathfrak{B}_8 \mathfrak{C}_8) \quad ,, & \mathfrak{A}_7 \mathfrak{B}_7 \mathfrak{C}_7 & ,, & ,, \end{array}$$

The principle of reciprocation would introduce here a number of Propositions which it is unnecessary to enunciate.

Theorem XXXVI.

$$\text{Let} \left.\begin{array}{lll} AA_1,\ B_7C_7 & \text{meet in} & l_2 \\ BB_1,\ C_7A_7 & ,, & m_2 \\ CC_1,\ A_7B_7 & ,, & n_2 \end{array}\right\}; \quad \text{And let} \left.\begin{array}{lll} BC,\ m_2n_2 & \text{meet in} & \alpha_1 \\ CA,\ n_2l_2 & ,, & \beta_1 \\ AB,\ l_2m_2 & ,, & \gamma_1 \end{array}\right\}.$$

Then ;—$\alpha_1\beta_1\gamma_1$ is a straight line.

Theorem XXXVII.

$$\text{Let} \left.\begin{array}{lll} AA_2,\ B_8C_8 & \text{meet in} & l_3 \\ BB_2,\ C_8A_8 & ,, & m_3 \\ CC_2,\ A_8B_8 & ,, & n_3 \end{array}\right\}; \quad \text{And let} \left.\begin{array}{lll} BC,\ m_3n_3 & \text{meet in} & \alpha_2 \\ CA,\ n_3l_3 & ,, & \beta_2 \\ AB,\ l_3m_3 & ,, & \gamma_2 \end{array}\right\}.$$

Then ;—$\alpha_2\beta_2\gamma_2$ is a straight line.

Theorem XXXVIII.

$$\text{Let} \left.\begin{array}{lll} \beta_1\gamma_2,\ \beta_2\gamma_1 & \text{meet in} & \alpha_3 \\ \gamma_1\alpha_2,\ \gamma_2\alpha_1 & ,, & \beta_3 \\ \alpha_1\beta_2,\ \alpha_2\beta_1 & ,, & \gamma_3 \end{array}\right\};\text{—Then, } \alpha_3\beta_3\gamma_3 \text{ is a straight line.}$$

Theorem XXXIX.

Let $\left.\begin{array}{lll} \alpha_1\beta_2,\ \gamma_1\alpha_2 & \text{meet in} & \alpha_4 \\ \beta_1\gamma_2,\ \alpha_1\beta_2 & ,, & \beta_4 \\ \gamma_1\alpha_2,\ \beta_1\gamma_2 & ,, & \gamma_4 \end{array}\right\}$; And let $\left.\begin{array}{lll} \alpha_2\beta_1,\ \gamma_2\alpha_1 & \text{meet in} & \alpha_5 \\ \beta_2\gamma_1,\ \alpha_2\beta_1 & ,, & \beta_5 \\ \gamma_2\alpha_1,\ \beta_2\gamma_1 & ,, & \gamma_5 \end{array}\right\}$.

Then ;— $\begin{array}{lll} A\,\alpha_4\alpha_5 & \text{is a straight line.} & \\ B\,\beta_4\beta_5 & ,, & ,, \\ C\,\gamma_4\gamma_5 & ,, & ,, \end{array}$

Theorem XL.

$A\,\alpha_4\alpha_5$, $B\,\beta_4\beta_5$, $C\,\gamma_4\gamma_5$ meet in a point.

Theorem XLI.

Postulating again, similarly as in Theorems XXXVI. and XXXVII.,—

$\left.\begin{array}{l} \text{Let } A\,A_2,\ B_7C_7 \text{ meet in } \bar{l}_2\text{; and similarly} \\ \text{Also } A\,A_1,\ B_8C_8 \quad ,, \quad \bar{l}_3\text{; and similarly} \end{array}\right\}$. Then ;—

$\bar{\alpha}_1\,\bar{\beta}_1\,\bar{\gamma}_1$ is a straight line.

Theorem XLII.

$\bar{\alpha}_2\,\bar{\beta}_2\,\bar{\gamma}_2$ is a straight line.

Theorem XLIII.

$\bar{\alpha}_3\,\bar{\beta}_3\,\bar{\gamma}_3$ is a straight line.

Theorem XLIV.

$\begin{array}{lll} A\,\bar{\alpha}_4\,\bar{\alpha}_5 & \text{is a straight line.} & \\ B\,\bar{\beta}_4\,\bar{\beta}_5 & ,, & ,, \\ C\,\bar{\gamma}_4\,\bar{\gamma}_5 & ,, & ,, \end{array}$

Theorem XLV.

$A\,\bar{\alpha}_4\,\bar{\alpha}_5$, $B\,\bar{\beta}_4\,\bar{\beta}_5$, $C\,\bar{\gamma}_4\,\bar{\gamma}_5$ meet in a point.

Theorem XLVI.

Let $\begin{array}{lll} \alpha_1\alpha_2,\ \bar{\alpha}_1\bar{\alpha}_2 & \text{meet in} & \alpha_6 \\ \beta_1\beta_2,\ \bar{\beta}_1\bar{\beta}_2 & ,, & \beta_6 \\ \gamma_1\gamma_2,\ \bar{\gamma}_1\bar{\gamma}_2 & ,, & \gamma_6 \end{array}$ Then ;—

$A\,\alpha_6$, $B\beta_6$, $C\gamma_6$; A_7A_8, B_7B_8, C_7C_8 all intersect in P_5.

Let P_3Q_3 and P_6Q_1 meet in Q_5 ; and let P_5Q_5 and P_4Q_4 meet in Q_6.
Then the points P_1, P_2, P_3, P_4, P_5, P_6 ; Q_1, Q_2, Q_3, Q_4, Q_5, Q_6 have very remarkable relations.

Theorem XLVII.

P_1, P_2, P_3, P_4, P_5, P_6 all range in a straight line.

Theorem XLVIII.

Q_1Q_4 and Q_3Q_5 intersect in P_3.

Theorem XLIX.

Q_2Q_5 and Q_4Q_6 intersect in P_4.

Theorem L.

Q_1Q_3 and P_1P_2 intersect in P_5.

Theorem LI.

Q_1Q_5 and Q_3Q_4 intersect in P_6.

Theorem LII.

P_4Q_5 and P_5Q_4 intersect in Q_2.

Theorem LIII.

P_1P_2, Q_1Q_2, Q_3Q_6 intersect in a point.

Theorem LIV.

Q_1Q_3, Q_4Q_5, Q_2Q_6 intersect in a point,—say $\mathfrak{P}$.

Theorem LV.

Let $\left.\begin{array}{l} Q_3Q_5 \text{ and } Q_5P_5 \text{ meet in } S_1 \\ Q_1Q_5 \text{ and } Q_4P_5 \quad ,, \quad\; S_2 \end{array}\right\}$; And let $\left.\begin{array}{l} Q_3Q_5 \text{ and } Q_4Q_6 \text{ meet in } S_3 \\ Q_1Q_4 \text{ and } Q_2Q_5 \quad ,, \quad\; S_4 \end{array}\right\}$. Then ;—

$S_1S_2\mathfrak{P}$ is a straight line.

Theorem LVI.

$S_3S_4\mathfrak{P}$ is a straight line.

Theorem LVII.

Let $\left.\begin{array}{l} P_1Q_1 \text{ and } P_2Q_3 \text{ meet in } R_1 \\ P_1Q_3 \text{ and } P_2Q_1 \quad ,, \quad\; R_2 \end{array}\right\}$; And let $\left.\begin{array}{l} P_1Q_4 \text{ and } P_2Q_5 \text{ meet in } R_3 \\ P_1Q_5 \text{ and } P_2Q_4 \quad ,, \quad\; R_4 \end{array}\right\}$. Then ;—

$R_1R_2\mathfrak{P}$ is a straight line.

Theorem LVIII.

$R_3R_4\mathfrak{P}$ is a straight line.

Theorem LIX.

Let P_1Q_6 and P_2Q_2 meet in U_1 \
P_1Q_2 and P_2Q_6 ,, U_2 } Then ;—

$U_1U_2\,\mathfrak{P}$ is a straight line.

Theorem LX.

U_1U_2; R_1R_2, R_3R_4; S_1S_2, S_3S_4; Q_1Q_3, Q_4Q_5, Q_2Q_6; all intersect in $\mathfrak{P}$.

Theorem LXI.

$\mathfrak{P}$ is the pole of $P_1P_2P_3P_4P_5P_6$ to the imaginary conic, $\alpha^2+\beta^2+\gamma^2=0$.

Theorem LXII.

The lines AA_1, BB_1, CC_1; AA_2, BB_2, CC_2 cut the sides of the triangle ABC in six points which lie in the conic;—

$$\alpha^2+\beta^2+\gamma^2-\left(\frac{g}{h}+\frac{h}{g}\right)\beta\gamma-\left(\frac{h}{f}+\frac{f}{h}\right)\gamma\alpha-\left(\frac{f}{g}+\frac{g}{f}\right)\alpha\beta=0.$$

For the six points are inverse or reciprocal points. Substituting the co-ordinates of five of them (the third of the *second* set being omitted) in the general equation of the second degree, and eliminating the arbitrary constants, gives the conic as above. To substitute the co-ordinates of five of the points, omitting now the third of the *first* set, amounts evidently to inverting the separate terms in the constants of the above equation; and as this leaves it unaltered, the proof of the theorem is obvious.

Theorem LXIII.

The lines AA_5, BB_5, CC_5; AA_6, BB_6, CC_6 cut the sides of the triangle ABC in six points which lie in the conic;—

$$\alpha^2+\beta^2+\gamma^2-\left(gh+\frac{1}{gh}\right)\beta\gamma-\left(hf+\frac{1}{hf}\right)\gamma\alpha-\left(fg+\frac{1}{fg}\right)\alpha\beta=0.$$

Theorem LXIV.

The points $\mathfrak{A}_1\mathfrak{B}_1\mathfrak{C}_1$; $\mathfrak{A}_2\mathfrak{B}_2\mathfrak{C}_2$ lie in the conic;—

$$\alpha^2+\beta^2+\gamma^2+\left(f+\frac{1}{f}\right)\beta\gamma+\left(g+\frac{1}{g}\right)\gamma\alpha+\left(h+\frac{1}{h}\right)\alpha\beta=0.$$

Theorem LXV.

If the point P_1 move in a straight line, the line $\phi\,(P_1)$ will always touch a conic which touches the three sides of the triangle ABC.

Let the equation of the line in which P_1 moves be $l\alpha+m\beta+n\gamma=0$; or substituting the co-ordinates of P_1 now supposed variable (see Theorem 1),

$$\frac{l}{f}+\frac{m}{g}+\frac{n}{h}=0 \qquad \ldots \qquad (1).$$

$\phi\,(P_1)$ is
$$f\alpha+g\beta+h\gamma=0=u \qquad \ldots \qquad (2).$$

Now h may be considered as constant, since it is the ratios only that are concerned. Take f as the independent variable, then g varies with f by (1); and by the theory of envelopes we have

$$\frac{du}{df}=0=\left(\frac{du}{df}\right)+\left(\frac{du}{dg}\right)\cdot\frac{dg}{df} \qquad \ldots \qquad (3).$$

Elimination gives
$$\pm(l\alpha)^{\frac{1}{2}}\pm(m\beta)^{\frac{1}{2}}\pm(n\gamma)^{\frac{1}{2}}=0;—$$

the equation of a conic touching the sides of the triangle of reference.—*Q. E. D.*

Theorem LXVI.

Let lines be drawn from the angles of a triangle through any two points and terminating in the opposite sides; by joining the extremities of each set of lines so drawn, two other triangles will be formed. The three lines joining the intersections of corresponding sides of these two triangles with the corresponding angle of the original triangle meet in a point.

If the co-ordinates of the two assumed points be $a_1\, b_1\, c_1$ and $a_2\, b_2\, c_2$ those of the third point are

$$a_1\,a_2\begin{vmatrix} b_1, b_2 \\ c_1, c_2 \end{vmatrix}, \quad b_1\,b_2\begin{vmatrix} c_1, c_2 \\ a_1, a_2 \end{vmatrix}, \quad c_1\,c_2\begin{vmatrix} a_1, a_2 \\ b_1, b_2 \end{vmatrix}.$$

Let this point be called the anapole of the two assumed points.

Theorem LXVII.

The anapoles of A_7, A_8; of B_7, B_8; and of C_7, C_8 are in a straight line,—say $\mathbb{A}_1\,\mathbb{B}_1\,\mathbb{C}_1$.

Theorem LXVIII.

The straight line joining the anapoles of P_1, A_8 and P_2, A_7 passes through A; cutting BC (say) in $\mathbb{A}_2$.
" " " P_1, B_8 " P_2, B_7 " B; " CA " $\mathbb{B}_2$.
" " " P_1, C_8 " P_2, C_7 " C; " AB " $\mathbb{C}_2$.

$\mathbb{A}_2\,\mathbb{B}_2\,\mathbb{C}_2$ is a straight line, identical with $\mathfrak{A}_5\mathfrak{B}_5\mathfrak{C}_5$ of Theorem XII.

Theorem LXIX.

The lines $\mathbb{A}_1\,\mathbb{B}_1\,\mathbb{C}_1$, $\mathbb{A}_2\,\mathbb{B}_2\,\mathbb{C}_2$ intersect in the anapole of P_1, P_2, which is also the pole of the line $P_1\,P_2$ to the imaginary conic, $\alpha^2+\beta^2+\gamma^2=0$. So that the anapole of inverse points is the pole of the line joining them—to the same imaginary conic.

Theorem LXX.

If one of two points remain fixed and the other move in any manner whatever, the locus of the anapoles is a straight line.

Theorem LXXI.

If two points move away from each other along a straight line with uniform velocities, their anapole will describe a conic section; and if the uniform velocities be equal, a straight line.

Theorem LXXII.

The anapole of any two points in a conic section passing through the angles of the triangle of reference is invariable, and its co-ordinates are proportional to the sides of the triangle.

VI.—*Note on Confocal Conic Sections.* By H. F. Talbot, Esq.

(Read 17th April 1865.)

A short paper of mine on Fagnani's theorem, and on Confocal Conic Sections, was inserted in the twenty-third volume of the Transactions of the Royal Society. Some of the conclusions of that paper can, however, be obtained more simply, as I will now proceed to show.

I will, in the first place, resume the problem—

"To find the intersection of a confocal ellipse and hyperbola."

Since the curves have the same foci, and therefore the same centre, let the distance between the centre and focus be called *unity*, since it is the same for both curves. Let a, b, be the axes of the ellipse, A, B, those of the hyperbola. Then we have $1 = a^2 - b^2 = A^2 + B^2$, which equation expresses the condition of confocality.

The equation of the ellipse will be $\frac{x^2}{a^2} + \frac{y^2}{b^2} = 1$, and that of the hyperbola $\frac{x^2}{A^2} - \frac{y^2}{B^2} = 1$. But at the point of intersection x and y are the same for both curves. We have therefore two equations from which to determine two unknown quantities. The result is one of unexpected simplicity. (See Vol. XXIII. p. 295.)

$$x = Aa\,, \qquad y = Bb\,.$$

The theory of the Conic Sections has been so much studied, that I can scarcely suppose that a result of such extreme simplicity, and so fruitful in remarkable results, should not have occurred to some previous mathematician. I have no had the opportunity of late of consulting many treatises on the Conic Sections, but in those which I have examined I have not found this theorem.

I will not here repeat the proof which I gave of it in my former paper, since it suffices to show that these values of x and y satisfy both the given equations. In fact, if we put $x = Aa$ and $y = Bb$, the equation $\frac{x^2}{a^2} + \frac{y^2}{b^2} = 1$ becomes $A^2 + B^2 = 1$, which is true, and the equation $\frac{x^2}{A^2} - \frac{y^2}{B^2} = 1$ becomes $a^2 - b^2 = 1$, which is also true.

This fundamental theorem being thus established, I shall proceed to show how easily the theorem of page 296 follows from it, viz., "If two ellipses and two hyperbolas have all of them the same centre and foci, and therefore intersect in four points, forming a curvilinear quadrilateral, the diagonals of this quadrilateral are equal."

There will, of course, be a similar quadrilateral on the other side of the axis major.

In proof of this theorem, it is sufficient to calculate the value of *one diagonal*, for since that is found to be a *symmetrical function* of the four greater axes of the given curves, the second diagonal has necessarily the same value.

This may be shown thus (Vol. XXIII., fig. 15, p. 296). Adopting the former notation, the square of one of the diagonals, or $D^2 = (x - x_{,})^2 + (y - y_{,})^2$

where

$$x = \mathrm{A}a \qquad y = \mathrm{B}b$$
$$x_{,} = \mathrm{A}_{,}a_{,} \qquad y_{,} = \mathrm{B}_{,}b_{,}$$
$$\therefore\ D^2 = (x^2 + y^2) + (x_{,}^2 + y_{,}^2) - 2xx_{,} - 2yy_{,}$$

But $xx_{,} = \mathrm{AA}_{,}aa_{,}$, which is a symmetrical quantity, being the product of the four major axes:—and $yy_{,} = \mathrm{BB}_{,}bb_{,}$ is a symmetrical also, being the product of the four minor axes.

Therefore it remains to show that $(x^2 + y^2) + (x_{,}^2 + y_{,}^2)$ is a symmetrical quantity.

Now

$$x^2 + y^2 = \mathrm{A}^2a^2 + \mathrm{B}^2b^2$$

but

$$\mathrm{B}^2b^2 = (1 - \mathrm{A}^2)\ (a^2 - 1) = -1 + (\mathrm{A}^2 + a^2) - \mathrm{A}^2a^2$$
$$\therefore\ x^2 + y^2 = (\mathrm{A}^2 + a^2) - 1\,.$$

And similarly

$$x_{,}^2 + y_{,}^2 = (\mathrm{A}_{,}^2 + a_{,}^2) - 1$$
$$\therefore\ (x^2 + y^2) + (x_{,}^2 + y_{,}^2) = (\mathrm{A}^2 + \mathrm{A}_{,}^2 + a^2 + a_{,}^2) - 2$$

which being a symmetrical quantity, the truth of the theorem in question is demonstrated.

From this theorem many others may be deduced; some of which I have given in my first memoir. The following elegant theorem was communicated to me by CHARLES H. TALBOT, Esq.

"If the direction of one of the diagonals passes through the focus, that of the other diagonal passes through the other focus."

Demonstration.—First take the general case in which neither diagonal passes through a focus (see fig. 1). Let the diagonals be PP′, QQ′; join HP, HP′ and SQ, SQ′;—then I say that HP′—HP=SQ′—SQ.

For, by a theorem in my former paper (p. 295), if two confocals intersect, the *focal distance* of their intersection equals the distance between their vertices.

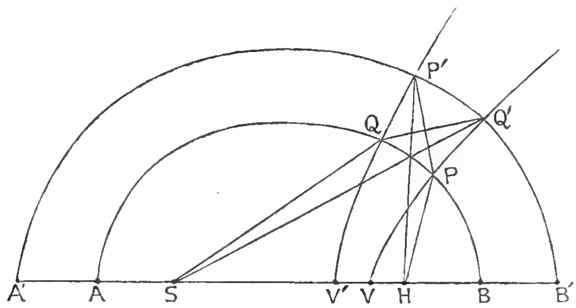


Fig. 1.

Thus, if AP be the ellipse, and VP the hyperbola (fig. 2), AB the major axis,

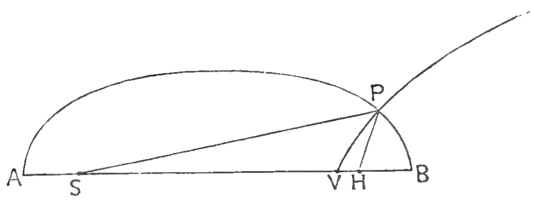


Fig. 2.

S, H, the foci; SP will be equal to AV, and HP to BV.

Therefore in fig. 1 we have

$$HP'=V'B', \quad HP=VB, \quad SQ'=A'V, \quad SQ=AV'$$
$$\therefore\ HP'-HP=V'B'-VB=V'V+BB'$$
$$\text{and } SQ'-SQ=A'V-AV'=V'V+AA'$$
$$\therefore\ (\text{since } AA'=BB')\ HP'-HP=SQ'-SQ.$$

This, of itself, is a curious theorem. The other follows immediately from it. For, in the particular case, where HPP′ is a straight line, HP′ − HP is the diagonal PP′, which is always equal to the diagonal QQ′.

Therefore, in this case, SQ′ − SQ = QQ′, and therefore SQQ′ is a straight line, which was to be proved.

Another theorem which I have found concerning these quadrilaterals is the following.

"If one of the diagonals is a tangent to the inner ellipse, the other diagonal is so likewise."

I omit, for the present, the demonstration of this, which is not difficult.

I deduced from Graves's theorem in my former paper the remarkable consequence, that if a triangle or other polygon is inscribed to the one, and circumscribed to the other, of two confocal ellipses, its perimeter is constant, at whatever point of the exterior ellipse it is supposed to commence.

But the truth of this can be shown without any reference to GRAVES's theorem, from the simple consideration that two consecutive sides of the triangle make equal angles with the periphery of the exterior ellipse. Hence if the point of departure, or vertex of the triangle, suffers a *very small* displacement, the three sides increase or diminish *at one end* by three small quantities δ, δ', δ'' (generally speaking all different).

Let us suppose this to occur at the *right* extremity of each of the three lines, then it is evident that the increments (or decrements as the case may be) which occur at their *left* extremities will be $-\delta''$, $-\delta$, $-\delta'$ respectively (because each side *gains* at one end what the *following* side *loses* there). Therefore the total increase of the perimeter $= (\delta - \delta'') + (\delta' - \delta) + (\delta'' - \delta') = 0$. A much more general theorem can be proved in the same way. "If a triangle cirumscribes an ellipse, and its three angles rest upon the peripheries of three other ellipses (all four having the same foci), the perimeter of the triangle is constant."

I find that CHASLES has given this theorem (although without proof) in his memoir, which I have already quoted (see my last paper, p. 287). The same is true of polygons of n angles resting upon n confocal ellipses.

I will conclude this short note by giving a curious property of the circle, communicated to me by C. H. TALBOT, Esq.

"If three concentric circles (fig. 3) are described from any centre S, with

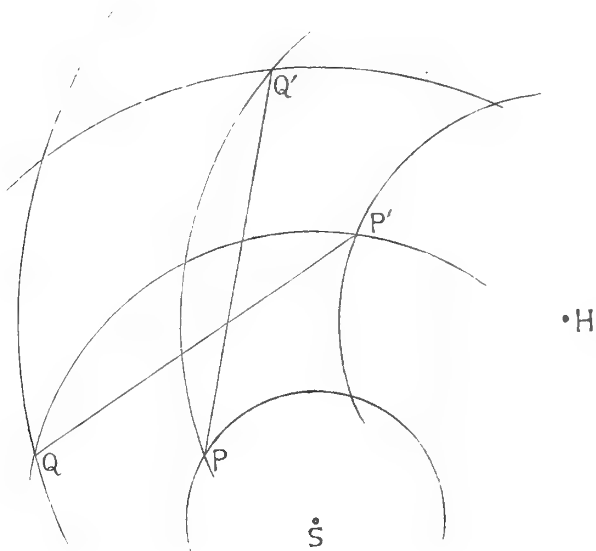


Fig. 3.

radii m, $m+h$, $m+2h$. And if three other concentric circles intersecting them are described from any other centre H, with radii n, $n+h$, $n+2h$ [m, n, h having any values]; then the chord of PQ′, the middle arc of one series, equals the chord of P′Q, the middle arc of the other series."*

Demonstration.—In each series of circles the radii have a common difference h, which may be called the *interval* between them. P and P′ are two points in the same ellipse of which S, H, are the foci, because in passing from P to P′, SP increases by one *interval* h, and HP diminishes by the same, therefore SP + HP remains constant.

By similar reasoning Q and Q′ are two points in a *second* ellipse having same foci. Moreover P and Q are two points in a hyperbola of which S, H, are foci; because in passing from P to Q, both HP and SP increase by one interval h, and therefore HP − SP remains constant, and equal to HQ − SQ.

By similar reasoning P′ and Q′ are two points in a *second* hyperbola having same foci. Therefore P, P′, Q, Q′ are the intersections of two ellipses and two hyperbolas, all confocal. Therefore the diagonals PQ′, P′Q are equal to each other.—*Q.E.D.*

This property of the circle should be readily demonstrable by EUCLID's Elements; a simple geometrical demonstration is, however, at present a desideratum.

* The second or middle circle of one series must be understood to be limited by the first and third circles of the other.

VII.—*On the Motion of a Heavy Body along the Circumference of a Circle.* By Edward Sang, Esq.

(Read 20th March 1865.)

In the year 1861 I laid before the Royal Society of Edinburgh a theorem concerning the time of descent in a circular arc, by help of which that time can be computed with great ease and rapidity. A concise statement of it is printed in the fourth volume of the Society's Proceedings at page 419.

The theorem in question was arrived at by the comparison of two formulæ, the one being the common series and the other an expression given in the "Edinburgh Philosophical Magazine" for November 1828, by a writer under the signature J. W. L. Each of these series is reached by a long train of transformations, developments, and integrations, which require great familiarity with the most advanced branches of the infinitesimal calculus; yet the theorem which results from their comparison has an aspect of extreme simplicity, and seems as if surely it might be attained to by a much shorter and less rugged road. For that reason I did not, at the time, give an account of the manner in which it was arrived at, intending to seek out a better proof. On comparing it with what is known in the theory of elliptic functions, its resemblance to the beautiful theorem of Halle became obvious; but then the coefficients in Halle's formulæ are necessarily less than unit, whereas for this theorem they are required to be greater than unit.

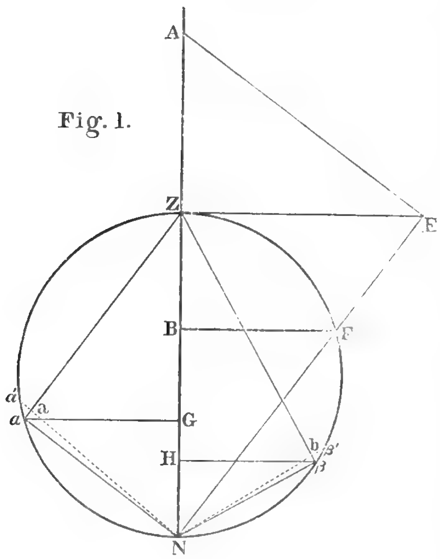
The search after the mutual relations of the two theorems has led me to the discovery of a few simple propositions which involve only the very first principles of the calculus, and the well-known law that the square of the velocity which a heavy body acquires in descending along a curve is proportional to the vertical distance, and to the intensity of gravitation jointly; and which, yet, contain the whole theory of motion in a circle whether that motion be oscillatory or continuous. I am thus enabled to present this hitherto intricate theory in a form which renders it intelligible to junior students of mechanical science.

By a well-known method of extension, the doctrine of the motion of a heavy physical point along the circumference of a circle can be made to include that of the rotation of any mass of matter on an axis not passing through its centre of gravity, whether that axis be horizontal or be inclined; hence, in the following investigation, I may confine my attention to the motion of a physical point in the circumference of a circle placed vertically.

2. Let N be the nadir and Z the zenith point of a circle, along the circumference of which a minute heavy body is free to move. If that body be projected

from N with some given velocity V, it will ascend along the circumference, losing velocity as it rises. When the initial velocity exceeds that which is due to a descent along the diameter ZN, the body will rise to the zenith-point Z, and will proceed onwards to descend along the other semicircumference; after that it will continue (all resistance being supposed away) to repeat revolution after revolution. But when the initial velocity is less than that which is due to a descent along ZN, the heavy body will have lost the whole of its velocity at some point below Z, from that point it will descend again to N, pass to the other side, on which it will reach to the same height, and thence descending it will continue to oscillate as in the familiar example of a pendulum. There are thus two distinct cases of circular motion, viz., the *continuous* and the *oscillatory*.

Fig. 1.



3. These two cases may be connected in the following manner :—

Let us suppose that a heavy body a, has been projected at N, with a velocity due to its descent from some point A beyond Z, and that it has now reached to the point marked a. Having drawn the horizontal line aG, we see that its velocity at the point a is that which is due to a fall through the distance AG; so that if we put V_A for the initial velocity, and v_a for the velocity at the point a, we must have the proportion

$$V_A^2 : v_a^2 :: NA : AG :: NZ . NA : NZ . AG$$
$$:: NZ . NA : NZ . NA - NZ . NG .$$

Through Z draw the horizontal line ZE, and make it a mean proportional between NZ and ZA; join NE, EA, then the trigons NZE, NEA are similar, so that NE is a mean proportional between NZ and NA, wherefore the above analogy may be written,—

$$V_A^2 : v_a^2 :: NE^2 : NE^2 - Na^2 .$$

4. F being the intersection of NE with the circumference of the circle, draw FB horizontally, then the five lines NA, NE, NZ, NF, and NB, are in continued proportion; so that NA : NZ : : NZ : NB.

If a second body β be projected from N, with a velocity due to a descent from B, it will rise along the curve only to the point F, its velocity there being entirely exhausted. The greatest distance, then, which β can reach from N, viz., NF, is to the greatest distance to which a can attain, viz., NZ, in the ratio of NE to NA. Let us take an intermediate point β to correspond with a, by making Na : Nβ

: : NA : NE : : NE : NZ : : &c.; and seek the ratio of the two velocities, viz., of a at the point a, and of β at the point β.

5. Putting V_B for the initial velocity of β, and v_β for its velocity at the point β we have,—

$$\begin{aligned} V_B^2 : v_\beta^2 &: : BN : BH : : BN : BN - NH \\ &: : NZ \,.\, BN : NZ \,.\, BN - NZ \,.\, NH \\ &: : NF^2 \quad : NF^2 - N\beta^2 \,. \end{aligned}$$

But we have also

$$V_A^2 : V^2_B : : NE^2 : NF^2 \,,$$

wherefore

$$v_\alpha^2 : v_\beta^2 : : NE^2 - N\alpha^2 : NF^2 - NB^2$$

Now, from our construction,

$$NE^2 : NZ^2 : : N\alpha^2 : N\beta^2$$

wherefore

$$NE^2 : NZ^2 : : NE^2 - N\alpha^2 : NZ^2 - N\beta^2$$

but

$$NZ^2 - N\beta^2 = Z\beta^2 \,,$$

wherefore

$$NE^2 - N\alpha^2 : NE^2 : : Z\beta^2 : NZ^2$$

and similarly

$$NE^2 : NA^2 : : NF^2 - N\beta^2 : NZ^2 - N\alpha^2 \,;$$

or,

$$NE^2 : NF^2 - N\beta^2 : : NA^2 : Z\alpha^2 \,.$$

Compounding these ratios we obtain,—

whence

$$NE^2 - N\alpha^2 : NF^2 - N\beta^2 : : NA^2 \,.\, Z\beta^2 : NZ^2 \,.\, Z\alpha^2 \,,$$

$$v_\alpha : v_\beta : : NA \,.\, Z\beta : NZ \,.\, Z\alpha \,.$$

6. Let us now suppose that the body a moves through an exceedingly minute distance, represented by $\alpha\alpha'$, and let us make the proximate chord $N\beta'$, in the same ratio to $N\alpha'$ as before; then, since $N\alpha$ may be held equal to $N\alpha$ and Nb to $N\beta$, we have $a\alpha' : b\beta : : N\alpha : N\beta$.

The minute trigons $\alpha a\alpha'$ and $b\beta\beta'$ are similar, respectively, to αNZ and βNZ, wherefore

$$\begin{aligned} \alpha\alpha' : a\alpha' &: : NZ : Z\alpha \\ b\beta' : \beta\beta' &: : Z\beta \;: NZ \,. \end{aligned}$$

By compounding these three ratios we obtain

$$\begin{aligned} \alpha\alpha' : \beta\beta' &: : N\alpha \,.\, Z\beta : N\beta \,.\, Z\alpha \\ &: : NZ \,.\, Z\beta : NF \,.\, Z\alpha \,. \end{aligned}$$

7. On comparing the lengths of the arcs $\alpha\alpha'$ and $\beta\beta'$, and also the velocities with which they are passed over, we find that the minute intervals of time are in the ratio

$$\text{time in } \alpha\alpha' : \text{time in } \beta\beta' : : \frac{NZ}{NA} : \frac{NF}{NZ} : : NZ : NE \,.$$

Now, if we suppose that the semicircumference $N\alpha Z$ is divided into a multitude of minute portions, of which $\alpha\alpha$ may be taken as one; and if we divide the arc $N\beta F$ into as many corresponding portions by making the chords $N\beta$ always to the chords $N\alpha$ in the constant ratio NZ to NE; the time of describing each element $\alpha\alpha'$ of the semicircumference NZ is to that of describing the corresponding element $\beta\beta'$ of the arc NF in the constant ratio NZ to NE; and, consequently, the time of describing any portion as $N\alpha$ must be to that of the describing the corresponding portion $N\beta$ in the same ratio; and so also must be the periodic times of the two motions.

Hence, if we can discover the law of the motion in the arc NF, we shall be able thence to deduce the law of the continuous motion due to the velocity obtained by descent from the point A; and contrariwise.

For the sake of convenience, we shall call these two motions *conjugate* to each other.

8. It will conduce greatly to the clearness of our subsequent investigations to introduce here another consideration. The time of describing the arc NF is greater than that of describing the conjugate arc NZ in the ratio of NZ to NF; the oscillatory motion will thus fall behind the continuous motion. Now, if we were to suppose that the body β is acted on by gravitation of an intensity greater than that which acts on α in the ratio duplicate of the ratio of the actual periodic times, the two motions would be rendered alike.

We shall then suppose that the gravitation acting on α, which we may designate by G_α, is proportional to NZ, while the intensity of the gravitation acting on β, appropriately denoted by G_β, is proportional to NA. And, as we have to do with the subduplicate of the ratio of these intensities, we shall, for the sake of additional convenience, put

$$G_\alpha = NZ \;.\; NZ\,; \qquad G_\beta = NZ \;.\; NA\,.$$

9. The height from which a body has fallen being denoted by h, and the intensity of gravitation being G, the velocity acquired is, according to the well-known law of motion, proportional to $\sqrt{(Gh)}$; we shall, therefore, put the general formula for that velocity thus:—

$$v = \sqrt{(G \;.\; NZ \;.\; h)}\,.$$

10. On inserting the above value of G_α in this general formula, and at the same time making $h = AG = NA - NG$ we have

$$v_\alpha = \sqrt{\{NZ \;.\; NZ \;.\; NZ\,(NA - NG)\}} = NZ \;.\; \sqrt{(NE^2 - N\alpha^2)}\,.$$

And similarly for the body β

$$v_\beta = \sqrt{\{NZ \;.\; NA \;.\; NZ\,(NB - NH)\}} = NE \;.\; \sqrt{(NF^2 - N\beta^2)}\,.$$

But since

$$NE : NZ :: NZ : NF :: N\alpha : N\beta$$

$$NZ \,.\, \surd(NE^2 - N\alpha^2) = NE \,.\, \surd(NZ^2 - N\beta^2) = NE \,.\, Z\beta \,,$$

and

$$NE \,.\, \surd(NF^2 - N\beta^2) = NZ \,.\, \surd(NZ^2 - N\alpha^2) = NZ \,.\, Z\alpha$$

wherefore

$$v_\alpha = NE \,.\, Z\beta \,; \qquad v_\beta = NZ \,.\, Z\alpha \,.$$

Hence, under this supposition of two distinct intensities of gravitation, we have

$$v_\alpha : v_\beta :: NE \,.\, Z\beta : NZ \,.\, Z\alpha \,,$$

but we have shown in Article 6 that the minute distance $\alpha\alpha'$ is to the corresponding distance $\beta\beta'$ in the very same ratio, wherefore the time in which the body α describes the distance $\alpha\alpha'$ is now equal to that in which β describes the distance $\beta\beta'$. And consequently if α and β start at the same instant from N, they will reach the points α and β simultaneously; and just when α has reached the highest point Z, β will have reached its highest point F; so that the periodic times of the two conjugate motions have been made alike.

11. In the figure 1 hitherto referred to, the points α and β have been placed on opposite sides of the diameter NZ for the sake of perspicuity. We shall now, in figure 2, suppose that they are both projected in the same direction from N and at the same instant, so that when α has reached the point α, β has reached β. Proceeding onwards, when α comes to Z, β arrives at F, the velocity of α being then that which is due to a fall from A to Z, and the velocity of β being zero. Subsequently, while α returns to N along the other semicircumference, β returns to N by retracing its previous path FN. In this way both bodies arrive at N at the same instant, but moving in opposite directions. While α, for the second time, describes the entire circumference of the circle, β ascends to L and thence returns to N at the same instant that α reaches that point. The two bodies are now moving in the same direction as at first, and these phases, all resistance being set aside, are again and again reproduced.

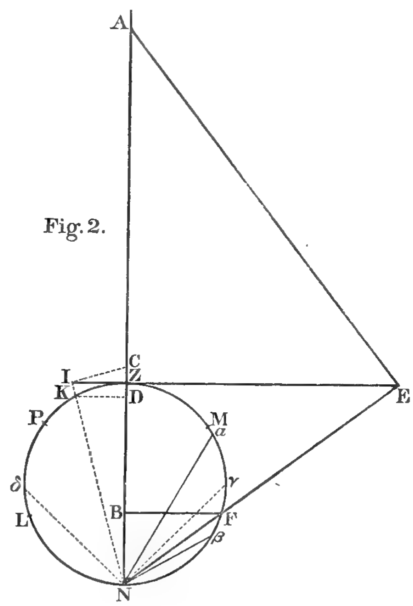


Fig. 2.

12. Let us now imagine that the arc $\alpha\beta$ is continually bisected in γ, and let us trace the motion of this middle point.

When α has reached Z and β has come to F, the point γ must be at M the middle of the arc FZ: when α has passed Z and β is descending from F towards

N, the point γ must be approaching to Z, and it must reach Z just when α and β have met at N. Thus the time in which γ describes the arc MZ is just equal to that in which it passes from N to M. By the time that α has reached Z again, β has reached L the extreme point of its motion on the other side, and therefore γ is at P the middle of ZL; lastly, when α has once more descended along ZPLN to N, β has descended along LN to the same point, and so γ also has come to N.

It thus appears that while the point α makes two complete revolutions, the point γ makes only one. In the progress of this revolution the velocity of γ varies; when at N it is half the sum of the initial velocities V_A and V_B, and when at Z it is reduced to be their difference; so that the motion of γ has the general characteristic of one due to the action of gravity upon a heavy body.

13. If the motion of γ can be truly represented by the action of gravitation upon a heavy body, we may determine the point from which γ may be supposed to have descended, and the intensity of the gravitation which must act upon it, by comparing the velocities at the lowest and highest points of its path. Let C be the point from which γ must have descended in order to acquire at N the velocity $\frac{1}{2}(V_A + V_B)$, or at Z the velocity $\frac{1}{2}(V_A - V_B)$, and put G_γ for the intensity of the gravitation to which it is subjected; then

$$\sqrt{(G_\gamma \,.\, NZ \,.\, CN)} = \tfrac{1}{2}(V_A + V_B); \quad \sqrt{(G_\gamma \,.\, NZ \,.\, CZ)} = \tfrac{1}{2}(V_A - V_B) .$$

Now according to Article 10,

$$V_A = NE \,.\, NZ; \; V_B = NE \,.\, NF$$

while if we make ZI a mean proportional between CZ and ZN, and join NI, we have

$$NZ \,.\, CN = NI^2, \qquad NZ \,.\, CZ = IZ^2 ,$$

so that

$$\sqrt{(G_\gamma)} \,.\, NI = \tfrac{1}{2} NE\,(NZ + NF); \qquad \sqrt{(G_\gamma)} \,.\, IZ = \tfrac{1}{2} NE\,(NZ - NF) ,$$

whence $NI : IZ :: NZ + NF : NZ - NF$; a proportion which enables us to determine the position of the point I, and, consequently, that of C.

Taking the square of each term of that proportion we have

$$NI^2 : IZ^2 :: NZ^2 + 2NZ \,.\, NF + NF^2 : NZ^2 - 2NZ \,.\, NF + NF^2$$

whence

$$NI^2 : NZ^2 :: (NZ + NF)^2 : 4NZ \,.\, NF$$

and consequently

$$NI^2 = \frac{NZ^2\,(NZ + NF)^2}{4NZ \,.\, NF}; \; NI = NZ\,\frac{NZ + NF}{2\sqrt{(NZ \,.\, NF)}} = NZ\,\frac{NE + NZ}{2\sqrt{(NE \,.\, NZ)}} ,$$

whence also

$$NC = \frac{(NZ + NF)^2}{4NF}; \ 4NF \ . \ NC = (NZ + NF)^2 .$$

14. Having thus determined the position of the point C, we can determine also the intensity of the gravitation for, putting for NI the value just found,

$$\sqrt{G_\gamma} \ . \ NZ \frac{NF + NZ}{2\sqrt{(NZ . NF)}} = \tfrac{1}{2} NE \ (NZ + NF)$$

whence $G_\gamma = NZ . NE$; so that the intensity of the gravitation for γ must be a mean proportional between those for α and β.

15. A descent from C under the influence of gravitation of the intensity NZ . ZE would cause a heavy body to have at Z and at N the very velocities which the moveable point γ has at those places; and we have now to inquire whether the same influence would give to that body, when at any intermediate point, the corresponding velocity. Before treating of this matter generally, it may be instructive to inquire into the velocity of the point γ when it is at M the middle of FZ; the moveable point γ is at M when α is at Z and β at F, now at that instant the velocity of β is zero, while the velocity of α, proportional to the square root of NZ . ZA, is represented by NZ . ZE, so that the velocity of γ must then be $\frac{1}{2}$ NZ . ZE.

But the velocity of the heavy body when at M is given by the general formula,

$$\sqrt{(G_\gamma \ . \ NZ \ . \ QC)} = \sqrt{\{G_\gamma \ . \ NZ \ (NC - NQ)\}} .$$

Substituting for G_γ the value above found we have

$$v_M{}^2 = NZ . NE \ (NI^2 - NM^2)$$

but since M is the middle of the arc FZ

$$2NZ : NZ + NF \ \text{ or } \ 2NE : NE + NZ :: NZ^2 : NM^2$$

wherefore

$$NM^2 = NZ^2 \frac{NE + NZ}{2NE}; \text{ but } NI^2 = NZ^2 \frac{(NE + NZ)^2}{4NE . NZ}$$

wherefore

$$NI^2 - NM^2 = NZ^2 \left\{ \frac{NE^2 + 2NE . NZ + NZ^2}{4NE . NZ} - \frac{2NE . NZ + 2NZ^2}{4NE . NZ} \right\}$$

$$= NZ^2 \frac{NE^2 - NZ^2}{4NE . NZ} = NZ^2 \frac{ZE^2}{4NE . NZ}$$

and consequently

$$v_M{}^2 = \tfrac{1}{4} \ . \ NZ^2 \ . \ ZE$$

or

$$v_M = \tfrac{1}{2} NZ \ . \ ZE$$

and thus the velocity at M due to a descent from the level of C is exactly that which the moveable point γ has at the same place.

16. We may now examine the velocity which this same heavy body would have at any intermediate point as γ. The general expression for that velocity is

$$v_\gamma = \surd\{G_\gamma(\mathrm{NI}^2 - \mathrm{N}\gamma^2)\} = \surd\{\mathrm{NZ}\ .\ \mathrm{NE}(\mathrm{NI}^2 - \mathrm{N}\gamma^2)\}$$

but since γ is the middle of the arc $\alpha\beta$,

$$\mathrm{N}\gamma^2 = \tfrac{1}{2}\{\mathrm{NZ}^2 + \mathrm{N}\alpha\,.\,\mathrm{N}\beta - \mathrm{Z}\alpha\,.\,\mathrm{Z}\beta\}$$

subtracting this from the value of NI^2 and simplifying

$$\mathrm{NI}^2 - \mathrm{N}\gamma^2 = \frac{\mathrm{NE}^2\,.\,\mathrm{NZ}^2 + \mathrm{NZ}^2\,.\,\mathrm{NZ}^2 + 2\mathrm{NE}\,.\,\mathrm{NZ}\,.\,\mathrm{Z}\alpha\,.\,\mathrm{Z}\beta - 2\mathrm{NE}\,.\,\mathrm{NZ}\,.\,\mathrm{N}\alpha\,.\,\mathrm{N}\beta}{4\mathrm{NE}\,.\,\mathrm{NZ}}.$$

Now $\mathrm{NE}\,.\,\mathrm{N}\alpha = \mathrm{NZ}\,.\,\mathrm{N}\beta$ so that the continued product $\mathrm{NE}\,.\,\mathrm{NZ}\,.\,\mathrm{N}\alpha\,.\,\mathrm{N}\beta$ may be written either $\mathrm{NE}^2.\,\mathrm{N}\alpha^2$ or $\mathrm{NZ}^2.\,\mathrm{N}\beta^2$; writing it once each way we obtain

$$\mathrm{NI}^2 - \mathrm{N}\gamma^2 = \frac{\mathrm{NE}^2\,.\,\mathrm{Z}\alpha^2 + 2\mathrm{NE}\,.\,\mathrm{NZ}\,.\,\mathrm{Z}\alpha\,.\,\mathrm{Z}\beta + \mathrm{NZ}^2\,.\,\mathrm{Z}\beta^2}{4\mathrm{NE}\,.\,\mathrm{NZ}}$$
$$= \frac{(\mathrm{NE}\,.\,\mathrm{Z}\alpha + \mathrm{NZ}\,.\,\mathrm{Z}\beta)^2}{4\mathrm{NE}\,.\,\mathrm{NZ}}$$

wherefore

$$v_\gamma = \surd\left\{\mathrm{NE}\,.\,\mathrm{NZ}\,\frac{(\mathrm{NE}\,.\,\mathrm{Z}\alpha + \mathrm{NZ}\,.\,\mathrm{Z}\beta)^2}{4\mathrm{NE}\,.\,\mathrm{NZ}}\right\} = \tfrac{1}{2}(\mathrm{NE}\,.\,\mathrm{Z}\alpha + \mathrm{NZ}\,.\,\mathrm{Z}\beta);$$

but we have seen that $\mathrm{NE}\,.\,\mathrm{Z}\alpha$ is the velocity of β at the point β, $\mathrm{NZ}\,.\,\mathrm{Z}\beta$ that of α at the point α, so that

$$v_\gamma = \tfrac{1}{2}\{v_\alpha + v_\beta\}$$

and thus, at every point of the circumference, the velocity of a body projected from N with a velocity due to a descent from Z, and acted on by a gravitation having its intensity represented by NZ . NE, is equal to the velocity of the middle of the arc $\alpha\beta$.

17. The motion of the body γ round the circumference has for its *conjugate* that of a fourth, which we may name δ ascending from N to K, and thence returning to N, while γ rises from N to Z, and proceeding onwards, returns to N; the conjugation being analogous to that which connects the motions of α and β.

Hence, if we inflect the chord $\mathrm{N}\delta$, a fourth proportional to NZ, NK, and $\mathrm{N}\gamma$, we shall obtain the point at which the body δ is found when γ is at γ; and if we make G_δ a fourth proportional to NZ, NC, and G_γ, we shall obtain for the intensity of the gravitation to which δ must be subjected

$$\mathrm{NZ} : \mathrm{NC} :: \mathrm{NZ}\,.\,\mathrm{NE} : \mathrm{NC}\,.\,\mathrm{NE} = G_\delta.$$

18. In this way we have obtained two pair of conjugate motions, the periodic time of the second pair being double of that of the first, and the intensities of gravitation being

$$G_\alpha = NZ \, . \, NZ$$
$$G_\beta = NZ \, . \, NA$$
$$G_\gamma = NZ \, . \, NE$$
$$G_\delta = NC \, . \, NE$$

such that any one of the four motions being known the other three may be found.

19. Here it is to be observed, that the periodic time of γ and δ is double that of α and β; in order to bring it to be the same, we must quadruple the intensities of the gravitation acting on these bodies, so that for all the

Periodic Times alike

$$G_\alpha = NZ \, . \, NZ \; , \qquad G_\beta = NZ \, . \, NA$$
$$G_\gamma = 4NZ \, . \, NE \; , \qquad G_\delta = 4NC \, . \, NE \; .$$

20. And if the intensities of gravitation be supposed the same for all the four bodies, their periodic times will then be proportional to the square roots of the preceding intensities: so that if we put T_α, T_β, T_γ, T_δ for the periodic times on the supposition of one gravitation, we have

Gravitations alike

$$T_\alpha = NZ \; ; \qquad T_\beta = NE$$
$$T_\gamma = 2\sqrt{(NZ \, . \, NE)} \; ; \qquad T_\delta = 2\sqrt{(NC \, . \, NE)} \; .$$

21. These two pairs of conjugate motions are so connected, that from one of them the other can be found, the law of connection being contained in the proportion

$$NI : IZ :: NE + NZ :: NE - NZ$$

or in

$$NI + IZ : NI - IZ :: NE : NZ \; .$$

If then from the pair α, β, we deduce the pair γ, δ; we may again from this latter deduce another pair of conjugate motions which we may mark γ_1, δ_1; and from this again another pair γ_2, δ_2, and so on without end. Or if we regard γ, δ, as the original pair, and deduce α, β, from it, we may thence deduce a new pair α_1, β_1, and from that again, another α_2, β_2; and so on, so that we have a progression of conjugate motions extending indefinitely each way, and such that any one of the series being known, all the others can be thence deduced. The latter branch of the progression, viz., that from γ, δ, to α, β, and thence onwards is that which is available in our research.

22. The periodic time of a body descending from C is deducible from that of one which has the velocity belonging to a descent from A; that again is deducible from the motion of a body supposed to have fallen from A_1, and so on. Now, the distances NA, NA_1, NA_2, &c, increase with greater and greater rapidity as we proceed, so that after a few terms NA may become enormously great as compared with NZ. But EZ is proportional to the velocity at Z, while NE is proportional to that at N; and the ratio of NE to EZ must approach nearer and nearer to a ratio of equality as the point A rises; so that when NA is very great, the velocity becomes almost uniform, and the periodic time of the motion becomes the quotient of the circumference by that velocity. In this way, the study of the law of this progression may conduct us to a knowledge of the periodic time of the motion of γ.

23. Analogously the distance NB is deduced from NC, from NB again we may deduce NB_1, thence NB_2, and so on; and in this manner, we may reduce the question of the time of descent in the arc KN, to that of the time of ascent in an excessively minute arc.

24. Attending first to the case of oscillatory motion, let it be proposed to compute the periodic time of a body having its velocity due to a descent from the level of D.

For this purpose, let us put B_0 for the angle NZK measured by half of the extreme arc NK, and B_1 for the angle NZF measured by half of the arc NF; then

$$NK = NZ \, . \sin B_0, \qquad KZ = NZ \, . \cos B_0 \;;$$

but

$$NZ + ZK : NZ - ZK :: NZ : NF \,,$$

wherefore

$$1 + \cos B_0 : 1 - \cos B_0 :: (\cos \tfrac{1}{2} B_0)^2 : (\sin \tfrac{1}{2} B_0)^2 :: 1 : \sin B_1$$

and

$$\sin B_1 = (\tan \tfrac{1}{2} B_0)^2 \,.$$

But it has been shown in article 20 that the times of descent from K and from F, there marked by the symbols T_δ and T_β are in the ratio $2\sqrt{(NC \, . \, NE)} : NE$ or of $2\sqrt{NC} : \sqrt{NE}$. It is now convenient to indicate these times by the characters T sin. B_0 and time B_1, so that the above proportion may be written

$$\begin{aligned}
\text{time } B_0 : \text{time } B_1 \;&::\; \sqrt{NC} : \sqrt{NE} \\
&::\; 2NI : NI + IZ \\
&::\; 2NZ : NZ + ZK \\
&::\; 2 : 1 + \cos B_0 \\
&::\; 1 : (\cos \tfrac{1}{2} B_0)^2 \\
&::\; (\sec \tfrac{1}{2} B_0)^2 : 1
\end{aligned}$$

so that the time of descent from K is

$$\text{time } B_0 = \text{time } B_1 \, . \, (\sec \tfrac{1}{2} B_0)^2 \,.$$

Hence the following very simple construction:—Having made NS the fourth part of the arc NK and drawn a horizontal line through N, join OS and produce it to meet the horizontal tangent at T; draw TU perpendicular to OT, meeting ON produced in U; lastly inflect NF double of NU; then the time of descent along KN is to that of descent from F to N in the ratio of OU to ON.

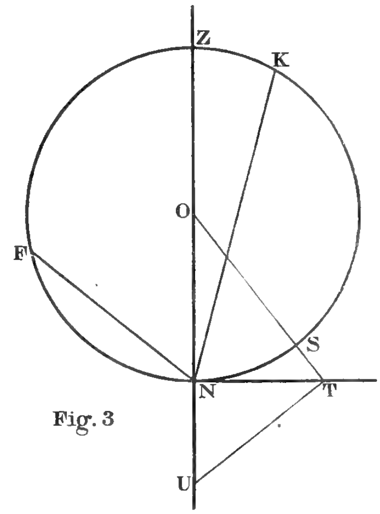


Fig. 3

By repeating this operation in regard to the arc NF, we should obtain a much smaller arc, and thence again one still smaller, and so on; the periodic times in these successive arcs bearing known ratios to each other. Now it is obvious, from a glance at the figure, that these arcs, which we may denote by $2B_0$, $2B_1$, $2B_2$, &c., form a very rapidly decreasing progression; and that the ratios of which ON : OU is the first, approach at the same time to a ratio of equality; hence the time of descent along KN may be deduced from the time of oscillation in an exceedingly minute arc. A very familiar investigation shows that the time of oscillation in a small arc is almost independent of the extent of the arc; but instead of founding on this well-known proposition, I prefer to deduce the truth of it from our present considerations.

25. If we suppose the arc NF of figure 2 to be very minute, the height NA, which is inversely proportional to NZ, must become very great in proportion to the diameter NZ, and hence the velocity at the point Z must be nearly equal to that at N, the two being in the ratio of ZE to EN; and the time in which a body descending from A describes the circumference must always be greater than that in which another moving with a uniform velocity equal to that at N would describe the same circumference. Putting g for the actual intensity of gravitation, the velocity acquired by falling from A to N is

$$V_A = \sqrt{(2g \, . \, AN)}$$

so that, as πNZ is the length of the circumference, the value of T_A must be greater than

$$\frac{\pi NZ}{\sqrt{2g \, . \, AN}} = \pi\sqrt{\left(\frac{NZ}{2g}\right)} \cdot \sqrt{\frac{NZ}{AN}} = \pi\sqrt{\left(\frac{NZ}{2g}\right)} \cdot \frac{NF}{NZ}$$

while, since the velocity at Z is to that at N as ZE to NE, the same time must be less than

$$\pi\sqrt{\left(\frac{NZ}{2g}\right)} \, \frac{NF}{NZ} \cdot \sec NZF.$$

26. Now we have seen that

$$T_\alpha : T_\beta :: NF : NZ$$

wherefore the time of descending from F to N, and thence rising to L on the other side, is between the limits

$$\pi\sqrt{\left(\frac{\mathrm{NZ}}{2g}\right)} \text{ and } \pi\sqrt{\left(\frac{\mathrm{NZ}}{2g}\right)} \cdot \sec \mathrm{NZF},$$

which limits approach closer to each other when the arc NF is made smaller.

27. Resuming now the investigation as in article 24, we find that the time of oscillation from K is

$$\text{time } \mathrm{B}_0 \gtrsim (\sec \tfrac{1}{2}\mathrm{B}_0)^2\, \pi\sqrt{\left(\frac{\mathrm{NZ}}{2g}\right)}$$

$$\lesssim (\sec \tfrac{1}{2}\mathrm{B}_0)^2\, \pi\sqrt{\left(\frac{\mathrm{NZ}}{2g}\right)} \cdot \sec \mathrm{B}_1.$$

Continuing the same progression another step by making

$$\sin \mathrm{B}_2 = (\tan \tfrac{1}{2}\mathrm{B}_1)^2$$

we find

$$\text{time } \mathrm{B}_0 \gtrsim (\sec \tfrac{1}{2}\mathrm{B}_0)^2\, (\sec \tfrac{1}{2}\mathrm{B}_1)^2\, \pi\sqrt{\left(\frac{\mathrm{NZ}}{2g}\right)}$$

$$\lesssim (\sec \tfrac{1}{2}\mathrm{B}_0)^2\, (\sec \tfrac{1}{2}\mathrm{B}_1)^2\, \pi\sqrt{\left(\frac{\mathrm{NZ}}{2g}\right)} \cdot \sec \mathrm{B}_2;$$

these limits being now closer to each other, since B_2 is a smaller angle than B_1.

If, then, we continue the progression indefinitely, by making

$$\sin \mathrm{B}_3 = (\tan \tfrac{1}{2}\mathrm{B}_2)^2; \ \sin \mathrm{B}_4 = (\tan \tfrac{1}{2}\mathrm{B}_3)^2; \ \&c.$$

we shall obtain for the entire time of an oscillation in the arc $4\mathrm{B}_0$,

$$\text{time } \mathrm{B}_0 = \pi\sqrt{\left(\frac{\mathrm{NZ}}{2g}\right)} \cdot (\sec \tfrac{1}{2}\mathrm{B}_0)^2 \cdot (\sec \tfrac{1}{2}\mathrm{B}_1)^2 \cdot (\sec \tfrac{1}{2}\mathrm{B}_2)^2 \cdot \&c.$$

28. As an example of the calculation we may require the time of oscillation over an arc of 180°, which is the extreme limit of a pendulum with a flexible thread. In this case $\mathrm{B}_0 = 45°$, whence log tan $\tfrac{1}{2}\mathrm{B}_0$ = log tan 22° 30′ = 9·617 2243; wherefore log sin B_1 = 9·234 4486; B_1 = 9° 52′ 45″·42; from this again we have log tan $\tfrac{1}{2}\mathrm{B}_1$ = log tan 4° 56′ 22″·71 = 8·936 6506; log sin B_2 = 7·873 3012, giving B_2 = 0° 25′ 40″·74. And once again, log tan $\tfrac{1}{2}\mathrm{B}_2$ = log tan 0° 12′ 50″·37 = 7·572 2861; log sin B_3 = 5·144 5722, B_3 = 0° 0′ 02″·88. Here we observe that the log-secant of B_3 does not differ from zero by unit in the seventh decimal place; and that, therefore, we have brought our limits so close together that the difference cannot be appreciated by help of the ordinary logarithmic tables. The

logarithm of the ratio of the time of oscillation in a semicircumference to that in a small arc is thus

$$
\begin{array}{rrl}
 & \text{Log sec } \tfrac{1}{2}\text{B}_0 = & 0{\cdot}034\ 3847 \\
 & \text{Log sec } \tfrac{1}{2}\text{B}_1 = & \quad\ 1\ 6160 \\
 & \text{Log sec } \tfrac{1}{2}\text{B}_2 = & \qquad\ \ 30 \\
\hline
 & & 0{\cdot}036\ 0037 \times 2 \\
\hline
1{\cdot}180341 & & 0{\cdot}072\ 0074
\end{array}
$$

In other words, the time of oscillation in a semi-circumference is to that for a minute arc as 72 to 61 very nearly.

When the arc is nearly a whole circumference, the steps of the progression are more numerous, and the computation may, with advantage, be systematically arranged. I subjoin the work for an arc of 320°, with logarithms to ten places.

n	$\frac{1}{2}\text{B}_n$	2 log sec $\frac{1}{2}\text{B}_n$	2 log tan $\frac{1}{2}\text{B}_n$	B_{n+1}
	° ′ ″			° ′ ″
0	40 00 00·000	·23149 20670	9·84762 70684	44 45 21·339
1	22 22 40·669	6800 53042	9·22920 47378	9 45 34·359
2	4 52 47·180	315 40158	7·86265 75152	25 03·441
3	12 31·721	57684	5·16071 41918	2·986
4	1·493	0		
		·30265 71554=log 2·00750740		

Thus, even in this extreme case, four terms of the progression are sufficient.

These examples show that for all cases in ordinary clock-work, or in experiments on the length of the pendulum, the time of oscillation is sufficiently well represented by the formula

$$\text{time B} = \pi \sqrt{\left(\frac{l}{g}\right)} \cdot (\sec \tfrac{1}{2}\text{B})^2$$

B being one-fourth part of the entire arc of oscillation. Hence the number of beats per day is proportional to the square of the cosine of the eighth part of the arc, and the daily retardation to the square of the sine of the same eighth part.

29. From the periodic time of an oscillation that of the conjugate continuous motion may be readily deduced; and thus, so far as the entire motions are concerned, the theory may be said to be complete. The investigation of the time at which the moving body arrives at any proposed point in its path is carried on by the application of the principle just explained; but as it is of comparatively little interest, and, at the same time, more tedious, I shall not, for the present, go into its details.

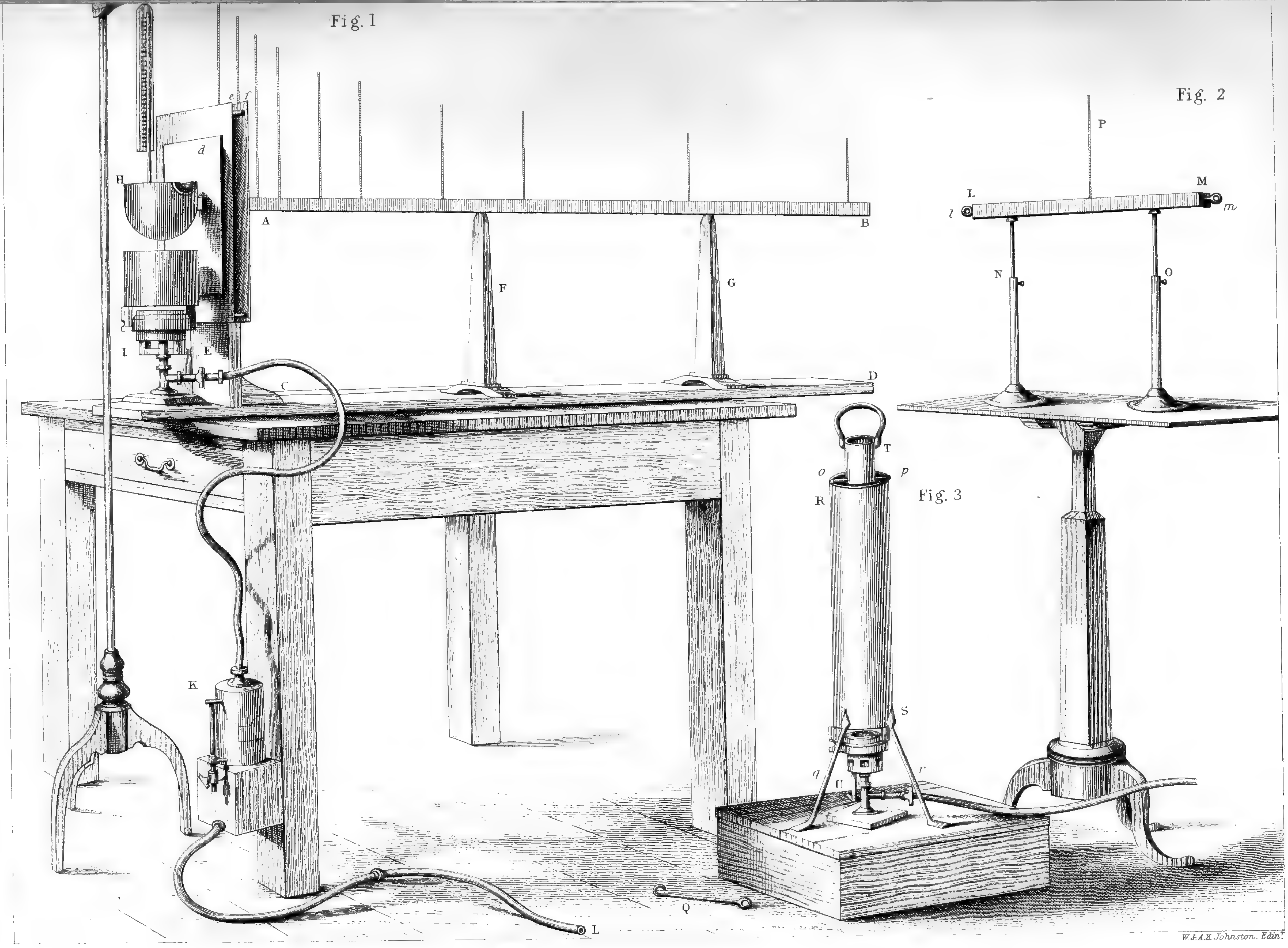


Fig. 1
Fig. 2
Fig. 3
A
B
C
D
E
F
G
H
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K
L
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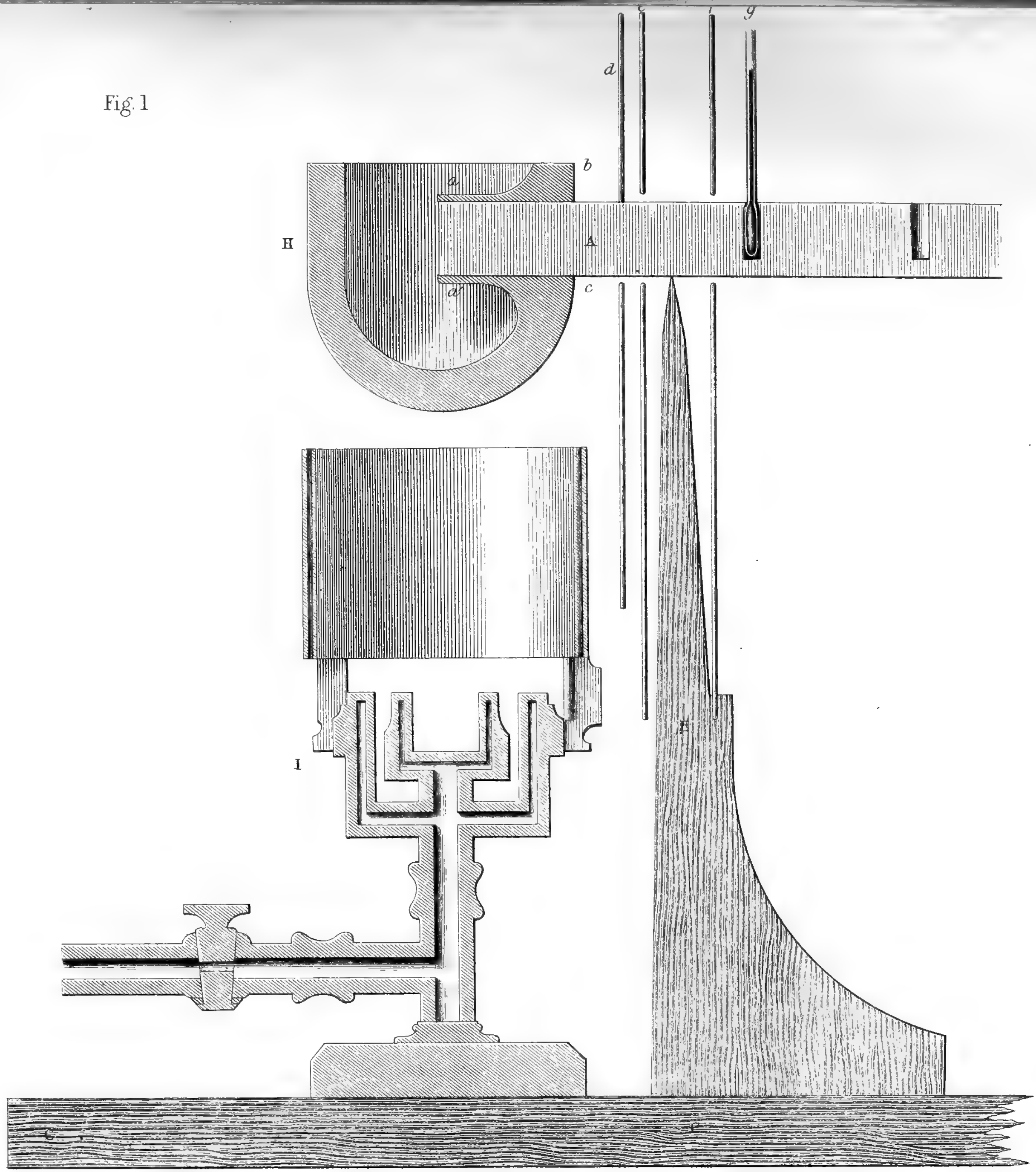
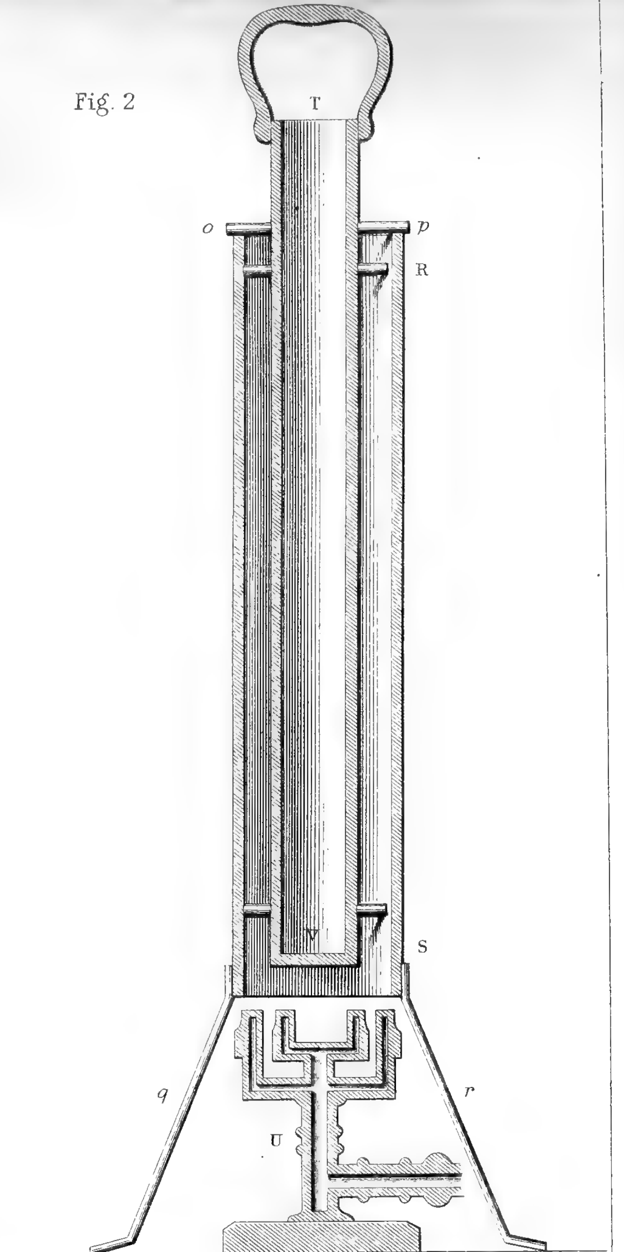
Fig. 1



Scale 1:3

Fig. 2



Scale 1:6

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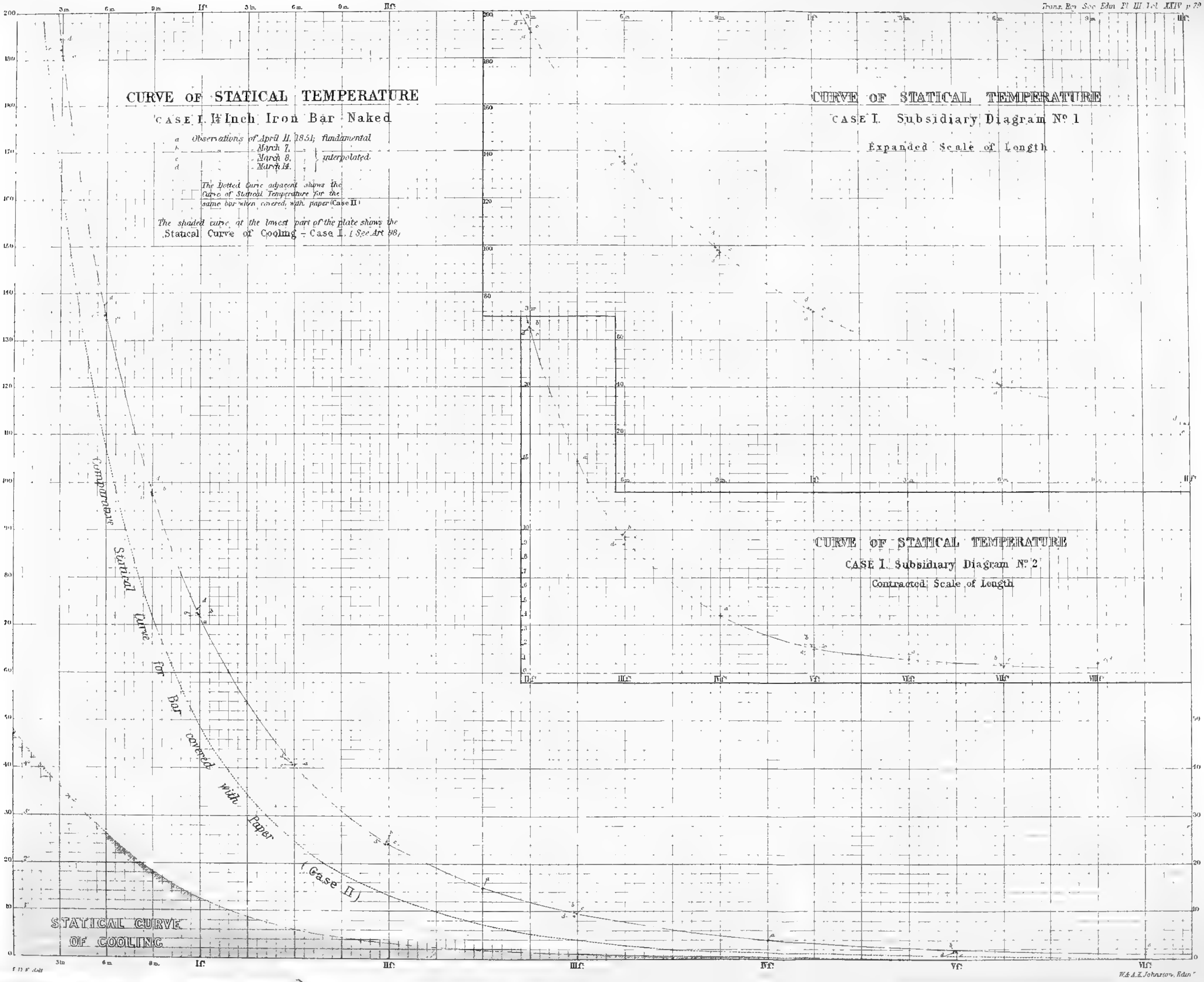

CURVE OF STATICAL TEMPERATURE
CASE I. 1¼ Inch Iron Bar Naked
a Observations of April 11. 1851; fundamental
b March 7.
c March 8.
d March 14.
interpolated.
The Dotted Curve adjacent shows the Curve of Statical Temperature for the same bar when covered with paper (Case II)
The shaded curve at the lowest part of the plate shows the Statical Curve of Cooling – Case I. (See Art. 98)
CURVE OF STATICAL TEMPERATURE
CASE I. Subsidiary Diagram No. 1
Expanded Scale of Length
CURVE OF STATICAL TEMPERATURE
CASE I. Subsidiary Diagram No. 2
Contracted Scale of Length
Comparative Statical Curve for Bar covered with Paper (Case II)
STATICAL CURVE OF COOLING
J.D.F. del.
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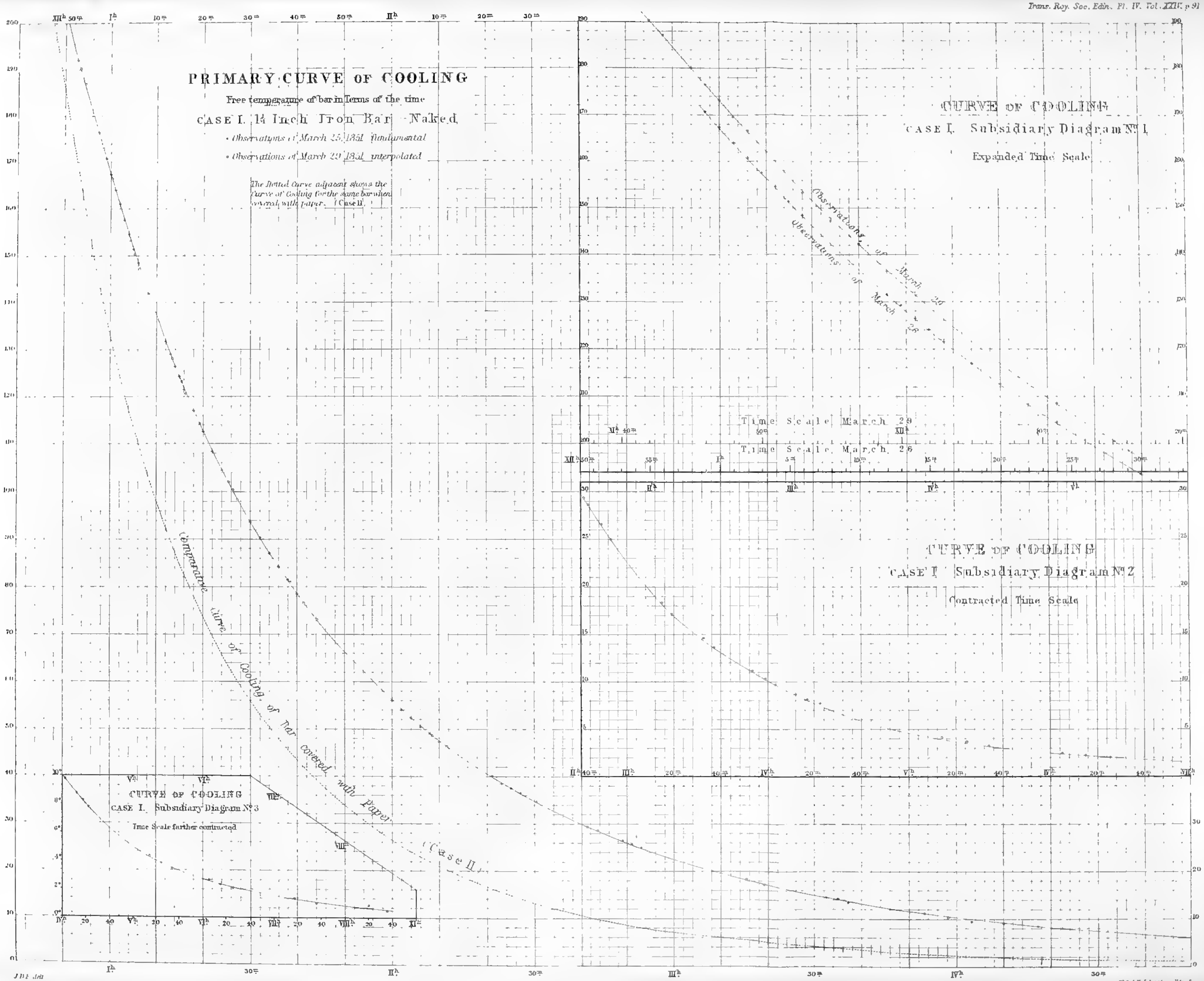
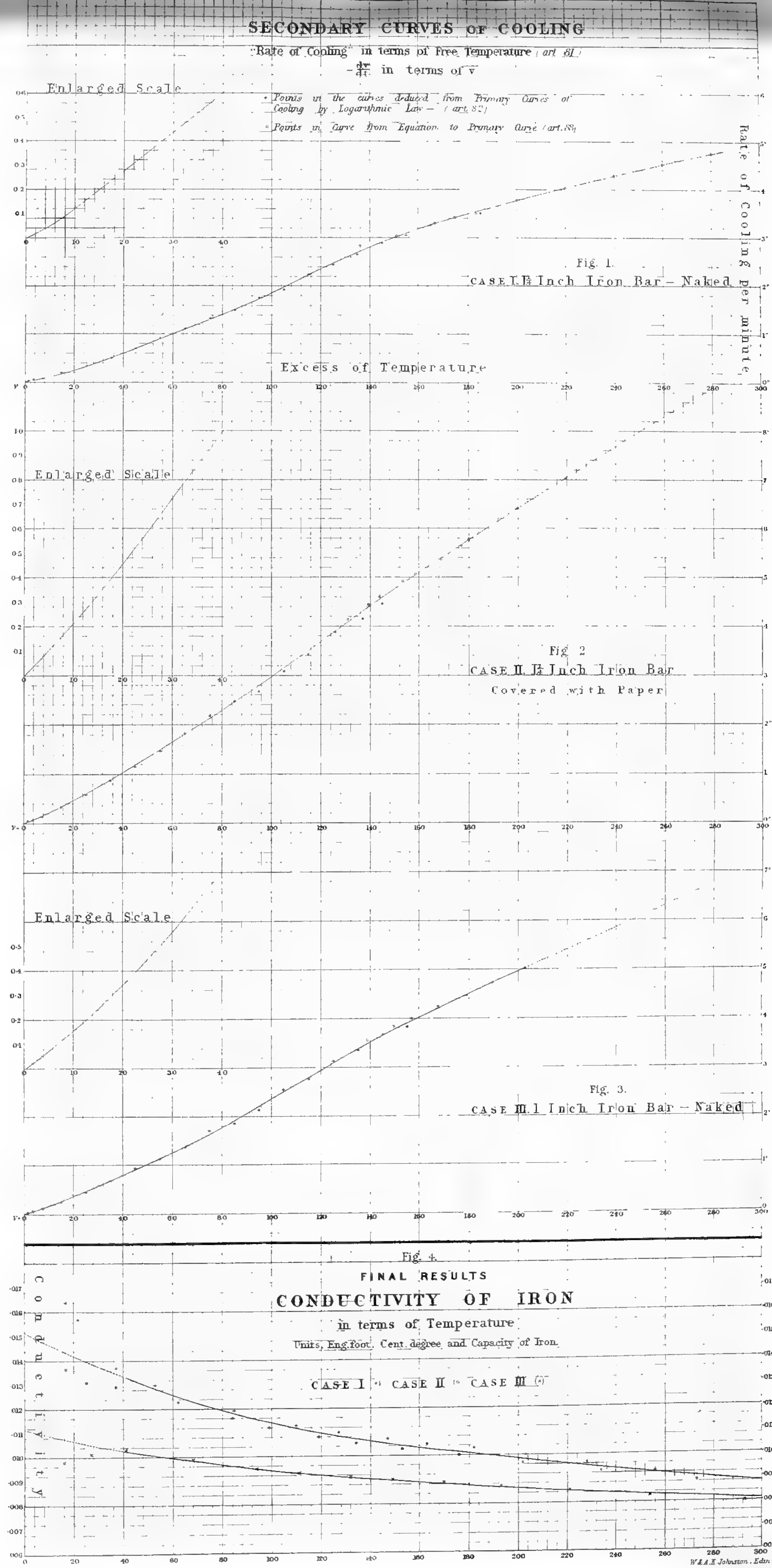

PRIMARY CURVE OF COOLING
Free temperature of bar in Terms of the time
CASE I. 1¼ Inch Iron Bar Naked
• Observations of March 25. 1851 fundamental
• Observations of March 29. 1851 interpolated
The Dotted Curve adjacent shows the Curve of Cooling for the same bar when covered with paper. (Case II)
Comparative Curve of Cooling of Bar covered with Paper (Case II)
CURVE OF COOLING
CASE I. Subsidiary Diagram No. 1
Expanded Time Scale
Observations of March 29
Observations of March 26
Time Scale March 29
Time Scale March 26
CURVE OF COOLING
CASE I. Subsidiary Diagram No. 2
Contracted Time Scale
CURVE OF COOLING
CASE I. Subsidiary Diagram No. 3
Time Scale further contracted



SECONDARY CURVES OF COOLING
"Rate of Cooling" in terms of Free Temperature (art. 81.)
$-\frac{dv}{dt}$ in terms of v
Points in the curves deduced from Primary Curves of Cooling by Logarithmic Law — (art. 82)
Points in Curve from Equation to Primary Curve (art. 83)
Enlarged Scale
Rate of Cooling per minute
Fig. 1.
CASE I. 1¼ Inch Iron Bar — Naked
Excess of Temperature
Enlarged Scale
Fig. 2
CASE II. 1¼ Inch Iron Bar
Covered with Paper
Enlarged Scale
Fig. 3.
CASE III. 1 Inch Iron Bar — Naked
Fig. 4.
FINAL RESULTS
CONDUCTIVITY OF IRON
in terms of Temperature
Units, Eng. foot, Cent. degree and Capacity of Iron.
CASE I · CASE II · CASE III
Conductivity
J D F del.
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VIII.—*Experimental Inquiry into the Laws of the Conduction of Heat in Bars. Part II. On the Conductivity of Wrought Iron, deduced from the Experiments of* 1851. By James D. Forbes, D.C.L., LL.D., F.R.S., V.P.R.S. Ed., Principal of St Salvator and St Leonard's College, St Andrews, and Corresp. Member of the Institute of France. (Plates I., II., III., IV., and V.)

(Read 20th February 1865.)

CONTENTS.

INTRODUCTION.

The Articles are numbered in continuation of those in the First Part of this Paper.

39. In the first part of this paper, read to the Royal Society of Edinburgh in April 1862, and published in their Transactions,† I explained the principles of a method devised by me in 1850 for ascertaining the absolute conducting power of substances capable of being formed into long bars; and I also stated the general results of experiments made in 1851 on the Conductivity for heat of wrought Iron.

40. I explained in Art. 14 of that paper, that the publication of the results had been for ten years withheld, partly in consequence of the state of my health which completely interrupted the experiments, but still more from the defective graduation of some of the thermometers used, which made it necessary to submit the instruments to a careful scrutiny, and to repeat with the duly corrected numbers the whole of the elaborate projections of the curves and calculations from them, on which the accuracy of the final results of course depends.

41. I stated that the friendly aid and exemplary patience of the late Mr Welsh of the Kew Observatory had supplied me with data for correcting the readings of the most important, and at the same time the most inaccurately graduated of the series of French thermometers employed in these experiments.

* Sections III. and VII. have been added to this paper since it was read.

† Vol. XXIII. p. 133.

Without his help the present corrected reduction of the observations could never have been made; and even with the aid of the tables kindly prepared by him, it has been a work of no small labour and anxiety to bring to one strictly accordant scale the whole of the observations made with eight or ten thermometers, none of them deserving of being called standards, and in most of which the zero appears to have oscillated at different periods.

42. I have thought it unnecessary, as it would certainly have been most tedious, to print in this paper the crude observations and the numerous tables of reduction formed for the scales of the several thermometers. I have thought it sufficient to give the corrected results, which, in many cases, are the mean of independent readings of different thermometers.

43. Besides the correction of scale errors, an important correction required to be applied in order to reduce the readings to what they would have been had the column of the mercury in the thermometer partaken of the temperature of the bulb. Owing to the small transverse dimensions of the bars, whose temperatures were to be ascertained, the bulb of the thermometers was often little more than covered by the mercury with which the holes in the bars were filled (Art. 20). The stems were therefore necessarily exposed in their whole length to the temperature of the surrounding air. In the case of the higher temperatures to be measured, this correction was not only large (amounting sometimes to 3° Cent., always additive), but also in some degree uncertain, owing to the ascending currents of warm air in the neighbourhood of the heated bar, and enveloping the stem of the thermometer.* However, I believe that the formula in the note below leads to pretty accurate results, checked, as it has been, by occasional observations of a small auxiliary thermometer suspended in the air, touching the stem of the thermometer to be reduced, about its middle.

44. The hotter thermometers are probably slightly *over* corrected. I have stated that in extreme cases this correction amounts to about 3° Cent., a quantity which may possibly be erroneous in some cases to one-tenth of its amount, but

* The form of the correction is very simple, being

$$\frac{\text{Degrees exposed} \times \text{Excess of Temp. shewn over air.}}{\text{Dilatation of Merc. in Glass for 1° Cent.}}$$

always additive. If T be the temperature as read, t the temperature of the air, and a the scale reading of the commencement of the stem of the particular thermometer, the correction is very nearly

$$+\frac{(T-a)\quad(T-t)}{6400}.$$

Since t and a are usually small numbers, the correction increases nearly as the square of the temperature to be measured.

Fortunately, the precision of this correction is not very important to the result. It chiefly affects the actual temperatures; for it will be more fully seen hereafter, that if the same instrument be used in the dynamical and statical experiments, being exposed in precisely the same way, the measures will be relatively correct, and the deduction of the conductivity will not thereby be sensibly affected.

I hope rarely so. Much larger corrections would have been inevitable at the highest temperatures (about 200° Cent.), had I not invariably employed for these a thermometer in which about 110° of the mercury was expelled from the bulb into the cavity at the top of the stem. The corrections for the reading of this thermometer were determined by Mr WELSH with extraordinary care. As its indications only commenced about the boiling-point of water, the length of the column exposed to the air was comparatively short.

45. For the principles on which the experimental investigations are founded, I refer to Art. 5, &c., of the former part of this paper. It will be recollected that there are two distinct classes of experiments, in one of which (the statical) the permanent temperatures at different points of a bar are to be observed; in the other (the dynamical) the velocity of cooling of a short bar of similar section, uniformly heated at first, is to be ascertained. I shall now proceed to describe these experiments severally more in detail than I have yet done, and to classify and discuss the results.

§ I. *Statical Experiments.*

46. *The Apparatus.*—A general account of this has been given in Arts. 17–20. It will, however, be rendered more intelligible by a reference to Plate I., fig. 1. The long wrought-iron bar AB was supported on a wooden frame CD by means of one fixed support E, and two moveable props F, G, which were all of wood, and were brought to a blunt edge at top, on which the bar rested, at about 15 inches above the top of a massive table, which stood in a spacious apartment attached to the Natural Philosophy Class-room (Edinburgh University). No fire was allowed during the experiments, and the south shutters being closed, the room was lighted from the north. At the end of the bar, towards the left side of the figure, was attached the heating apparatus, a cast-iron crucible H, usually filled with just-melting lead. It was kept hot by means of the powerful gas-furnace I, with a double metal chimney and two concentric rows of burners. The gas was derived from the main pipe by a flexible tube L, and passed through one of Milne's patent gas regulators, K, with a view to obtaining a uniform flame, which, however, remained subject to occasional fluctuation. The connection of the crucible with the conduction bar will be best understood from the sectional diagram in Plate II. fig. 1. An internal flange $a\,a'$ was cast on the crucible, leaving a square cavity 2·5 inches long, into which the extremity A of the conduction bar was thrust, and was retained there by friction only. The exterior face of the crucible $b\,c$ is almost vertical, and determines the position from which the distances of the thermometers along the bar are reckoned. Supposing the crucible itself to be maintained at the constant temperature of melting lead, it seems reasonable to assume that the bar A, so far as encased within it—that is, up to the zero line $b\,c$—may be regarded as having nearly

the same temperature.* The gas flame and the violently heated currents of air thence arising were prevented from playing against any part of the bar by a piece of metal plate fastened by wire to the crucible against the face *a b* (but to prevent confusion not shown in the figure), while the whole conduction bar was farther protected from heated currents, and from radiation from the crucible and gas chimney, by three polished metal screens *d, e, f,* placed parallel to one another, two to the left and one to the right of the wooden support E. The square apertures in the screens were 0·25 inch wider than the dimensions of the bar, and they were supported in such a manner as not usually to touch it in any part. These screens very effectually defend the thermometers, as well as the bar, from extraneous heat. The first thermometer only—that at 3 inches distance from the zero line *b c*—is seen at *g* in the section, fig. 1.

47. The conduction-bars have already been described in Arts. 17, 18. The more perfect one was 1·25 inch square, and fully 8 feet long, reckoning from the zero line above mentioned. It was used in two states, *first,* with a naked polished surface, and, *secondly,* when coated with thin paper. The other bar, also of wrought-iron, was 1 inch in the side and 7 feet long. In the present paper I shall discuss separately the results obtained with these two bars, presenting three quite independent cases, but which, as they ought to lead to an identical value of the conductivity of iron (assuming the quality of the bars to be alike), put the method here proved to a severe trial.

48. Throughout the remainder of this paper, when I speak of Case I., I mean the $1\frac{1}{4}$-inch bar with moderately *polished* surface; Case II. is the same bar with *paper* surface; Case III. is the 1-inch bar with naked, but less brightly polished surface.

49. The thermometers were inserted in holes in the bar 0·28 inch diameter and about $\frac{3}{4}$ inch deep. They were surrounded by mercury or (in the hotter holes) by fusible metal. (See Art. 20, and also Plate II. fig. 1.) Nine or ten thermometers were usually employed, and in the case of the principal bar (Cases I. and II.) they were usually (though not always) spaced as follows, reckoning from the zero line *a b* on the face of the crucible :—

0·25, 0·5, 0·75, 1, 1·5, 2 or 2·5, 4, 6, 8 feet.

50. The method of using a single standard thermometer for obtaining final results by the method of *stepping,* with its advantages, have been fully explained at Art. 22.

51. " The *free temperature,* or that to be deducted from the readings of the thermometers, in order to get the true excess of statical temperature along the bar, was obtained by inserting a well-compared thermometer into a hole containing mercury, drilled in a similar but short bar of iron, supported in the free air of the

* See, however, note to Art. 70, below.

room in the neighbourhood of the long bar, and similarly exposed, but without artificial heat." (Art. 23.) The arrangement is shown in Plate I. fig. 2.

52. The gas furnace was usually lighted about 8 A.M., when the lead in the crucible was gradually melted. The readings of the thermometers were not recorded until about four hours had elapsed, and the experiment seldom lasted altogether less than eight hours, generally ten or eleven hours. It was difficult to keep the flame of the gas furnace steady, the "regulator" used for the purpose being apparently of little use. The lead in the crucible, after being quite fluid, sometimes solidified a little over the interior flange which grasps the bar. I at length found the best way of regulating the temperature to be, to keep the eye very constantly on the first thermometer in order, and whenever the slightest rise commenced, to dip a little cold lead into the crucible, and either let it melt or chill the mass by its contact, or the gas might be cautiously lowered. If the temperature was seen to be falling, the gas had to be raised. With experience I learned to keep the temperature of the three-inch hole within a range of 2° Cent. under favourable circumstances, the temperature being nearly 200° Cent. In some experiments in which solder was employed instead of lead, I used a thermometer whose bulb dipped into the crucible, where it stood about 460° Fahr.* My able assistant, Mr JAMES LINDSAY, learned to regulate this with great nicety.

53. When the temperature had been for a long time quite steady at the three-inch (or hottest) hole, the thermometers, disposed, as has been explained, along the bar, were successively read, and the readings recorded. This was done from left to right along the bar with all deliberation, without regard to any possible change during the process in the temperature at the hot end of the bar, since any such change is comparatively slowly propagated along the bar. In like manner, in "stepping" with one thermometer from point to point of the bar (Art. 2), a slight change in the temperature of the source is immaterial, since the "stepping" is performed faster than the wave of heat can follow.

54. A careful examination of the whole record of simultaneous readings was made, and those corresponding to the most stationary conditions of temperature were selected; and these being corrected for scale errors and temperature of column (Arts. 42, 43) were entered, after the free temperature indicated by the little bar (Art. 51) had been deducted, in the following tables as the Corrected Excesses of Statical Temperatures in centigrade degrees.

55. Looking cursorily over Table I., we may observe, *First*, that each day's observations are comparable only amongst one another, as no *exact* coincidence of the temperature of the crucible, or source of heat on different days, was attempted. *Secondly*, the bracketed observations are made with independent thermometers. *Thirdly*, comparing the first series for the bar covered with paper

* By an oversight in the first part of this paper (Art. 19, *note*), it was stated that in this instance the thermometer was dipped in melted *lead*.

TABLE I.—Corrected Excesses of Statical Temperature, Origin at Outer Surface of Crucible—Degrees Centigrade.

(The numbers bracketed together were obtained with different thermometers.)

	3 inch.	6 inch.	9 inch.	1 ft. 0 in.	1 ft. 6 in.	2 ft. 0 in.	2 ft. 6 in.	3 ft. 0 in.	4 ft. 0 in.	5 ft. 0 in.	6 ft. 0 in.	7 ft. 0 in.	8 ft. 0 in.
Case I.—1¼ inch Iron Bar, naked. 1851, March 7 (Series A.)	191·1°	134·6°	97·7°	72·2°	March 7. Series B.	24·6°	...°	9·7°	...°	1·88°	...°	0·53°	0·34°
March 8, ...	192·3	135·3	97·95	{ 72·4 72·4	{ 40·9° 40·6*	{ 24·1* 24·15	...	9·5	...	1·78	...	0·43	0·28
March 14, . . .	194·6	137·65	{ 99·65 99·45	73·4	41·35	{ 24·25 24·15	...	{ 9·05 8·9*	...	{ 1·70 1·61*	...	...	{ 0·25* 0·35 0·45
(Standard) April 11, ...	191·0	{ 134·7 134·3	{ 97·15 97·4 97·25	71·85	{ 40·9 40·65*	...	{ 14·8 14·8	...	{ 4·05 4·05	...	{ 0·95 0·95	...	{ 0·30 0·28
Case II.—1¼ inch Iron Bar, covered with Paper. (Standard) 1851, April 15, . .	162·9	{ 105·7 105·65	{ 71·45 71·45	49·35	{ 25·0 24·65*	[13·0?]	{ 7·08 6·98	[3·85?]	{ 1·28 1·28	0·50	{ 0·18 0·18	...	0·0
April 16,† . .	{ 120·55 120·3 120·5	{ 80·0 80·15	{ 54·95 54·65 54·8*	{ 38·4 38·15*	19·45	10·3	{ 5·65 5·65	...	{ 1·10 1·10	...	{ 0·15 0·15	...	0·0

	4 inch.	7 inch.	10 inch.	1 ft. 1 in.	1 ft. 7 in.	1 ft. 10 in.	2 ft. 4 in.	2 ft. 7 in.	4 ft. 4 in.	4 ft. 7 in.	4 ft. 10.	6 ft. 7 in.
Case III.—1 inch Iron Bar, naked. 1850, Dec. 21, . . .	149·9	100·6	69·95	...	25·62*	...	10·2	7·65	1·25	{ 1·0 1·0	0·78	0·07
1851, Jan. 11 (Series *a.*)	156·6	105·1	72·42	50·8	...	19·0	...	7·62	1·02	{ 0·77 0·86	0·52	0·0
(Standard) ... ... (Series *b.*)	159·0	{ 105·75 105·8	72·8	51·0	...	19·4	...	7·83	1·2	{ 0·92	0·67	0·0

* Defective thermometer, or otherwise less reliable observations.

† The crucible was this day filled with a mixture of lead and tin (*solder*, which melts at a much lower temperature than pure lead), heated considerably above its melting-point.

(April 15) with the last of the same bar naked (April 11), which were made in almost similar circumstances, we notice the prodigious effect of the increased radiation due to the paper casing. Though the heat at the origin may be considered as the same, at a distance of only three inches the temperature in the second case was less by nearly 30° Cent.; at thirty inches distance, the proper heat of the bar was but *one-half* of what it was in Case I.; at four feet, *one-third;* and, at eight feet, it had vanished in the second case, while it was still sensible in the first. *Fourthly,* it may be remarked that in Case I. the bar scarcely fulfilled, as to length, the implied condition of the experiment, which assumes that the extreme end of the bar shall be sensibly of the temperature of the air. As 0°·3 of heat remained at eight feet, and as the bar extended only a few inches beyond that point, it would appear that the conducted heat was not absolutely dissipated by radiation. The effect, which is to render the decrement of heat in the extreme holes rather too slow, is, however, barely appreciable in the deductions.

56. *Graphical Interpolation of the Statical Experiments.*—As it was desirable to combine the results of the independent experiments in each of the three Cases, and to deduce the most probable temperature for any point of the bar, a graphical method was adopted as follows:—Large sheets of drawing-paper were provided covered with engraved squares one-tenth of an inch in the side. A horizontal line was taken to represent the distances reckoned along the bar on a scale of four inches to a foot, and at the proper intervals the observed temperatures (or rather excesses of temperature) were set off as vertical ordinates on a scale of 10° to 1 inch. The general arrangement of the observations in this way is shown in Plate III. on a reduced scale for Case I.

57. It is plain, however, that this primary projection could only apply to a single and comparable series of observations under each Case of Table I., since the temperature of the origin might vary from one experiment to another. One set under each Case was assumed as a standard series to which the others were to be referred. In Case I., April 11; in Case II., April 15; in Case III., January 11 (*b*). But as it is plain that for one and the same bar the curve of temperature has the same form, though it may deviate in position to the right or left along the bar, each of the other days' observations was separately projected on transparent cloth, and then laid on the engraved squares over the first projection. By moving the system of projected points to the right or left (taking care to keep the line of abscissæ in each case accurately coincident), a position was easily found where the points to be interpolated accommodated themselves best to the general curvature of the fundamental series.

58. This method of interpolating independent series belonging to different fundamental temperatures has very great advantages. Had circumstances allowed me to continue these observations, I should have applied it more extensively. A clear instance of its utility will be seen by comparing the first and second row

of figures in Case II. of Table I. The observations of April 16 were made with melted solder as a source of heat, which fuses at a much lower temperature than lead. The result is, that the temperatures in the 3, 6, 9 inch holes and those which follow, are *intermediate* between the temperatures shown in those holes in the other experiment. Thus, by varying the temperature of the source of heat, we may multiply indefinitely points in the curve without increasing the number of holes with which the bar is pierced, which is evidently undesirable. It would have been very serviceable for the interpolation of the numbers in Case I., had the temperature of the origin been expressly varied for this purpose.

59. The general agreement of the independent interpolated observations has been highly satisfactory, as may be seen from Plate III., where the several sets of temperatures belonging to Case I. are distinguished by marks.

60. A continuous curve was next to be drawn through the extremities of the ordinates, so as best to conciliate the whole of the observations. To draw this curve was a matter requiring great nicety and judgment, owing to the limited number of ordinates disposable. It is well known to every one who has used such projections, that to draw an interpolating curve advantageously requires that the rate of increment of the two variables shall not be *excessively* unequal. In curves like those of Plate III., which rise very rapidly at one end, and become almost or quite asymptotic at the other, it is indispensable to make subsidiary projections of different parts of the curve, in which the relative scale of the vertical and horizontal co-ordinates shall be altered. For the part of the curve between 0 and 2 feet, the temperatures had to be contracted in scale and the abscissæ expanded; while for the right-hand branch of the curve the contrary was done, even to the extent of magnifying the vertical scale of degrees tenfold, whilst the horizontal scale of feet was diminished fourfold, compared with the first projection. For each of the three Cases (Art. 48), the statical curve was thus subdivided and partially projected on four different scales, three of which are exhibited on the engraved Plate. The result of this close analysis and comparison has been highly favourable to the assurance of accuracy in the final results, since the interpolated temperatures for any abscissæ are the result of two, if not three, projections of the observations on different relative scales. No numerical or other casual error could thus possibly escape detection.

61. I believe that these curves, as now obtained with the ordinates immediately to be given, are favourable specimens of numerical accuracy and geometrical definition, considering the difficulties attending the experiments. Throughout a great part of the curves (and that by far the most important for the results), the temperature excesses of the bar, or vertical ordinates, may, I hope, be esteemed correct within a very small fraction of their amount. The following table (which may be regarded as summing up the whole Statical data) contains the ordinates of the curves corresponding to the three Cases of Art. 48.

TABLE II.—STATIONARY EXCESSES OF TEMPERATURE (FROM ALL THE PROJECTIONS) ADOPTED.

Distance from Origin at Crucible.	Excess of Temperature (Centigrade) of Bar above Air.		
	Case I. 1¼ inch Bar, naked.	Case II. 1¼ inch Bar, covered.	Case III. 1 inch Bar, naked.
0 inch	275·5 c.	260·5 c.	282·2 c.
1	242·9 c.	221·7 c.	243·2 c.
2	214·8 c.	189·5 c.	210·2 c.
3	190·5	162·9	182·2 c.
4	168·3*	140·0*	159
5	150·6*	121·5	137·5*
6	134·7	105·9	120·5*
7·5	114·1*	86·6	99·8
9	97·3	71·3	82·8*
10·5	84·0*	59·0	68·6
I. foot 0 inch	72·0	49·2	57·1
3	53·6	34·5	40·9*
6	40·8	24·6	29·5
9	31·0	17·7	21·6
II. feet 0	24·2	13·0	15·65*
3	18·9*	9·45	11·5*
6	14·8	7·0	8·55*
III. " 0	9·33	3·8	4·95*
6	6·15*	2·1*	2·78*
IV. " 0	4·0	1·28	1·56
V. " 0	1·8	0·47	0·55
VI. " 0	0·9	0·165‡	0·13*
VII. " 0	0·50	...	...
VIII. " 0	0·28†	...	...

The numbers marked c. are derived from calculation. See Art 68 below.
The numbers marked thus * belong to points in the curve not closely adjacent to points of observation; and, therefore, are less certain than the others.
† 0·32 by mean of 7 observations. ‡ Mean of 4 observations.

Adjacent to the principal curve of Plate III. is a dotted curve, which exhibits the remarkable change of character in the curve when the bar is coated with a highly radiating surface of paper (Case II., Art. 48).

62. *Formulæ of Interpolation for the Statical Curves.*—It was not originally my intention to have entered on the thorny enterprise of seeking equations to satisfy the statical curves of temperature. My original plan (Art. 6 of former paper) was to deal with Curves alone, or almost entirely. And when we do not wish to exceed the limits of direct observation, it is perhaps the safest, as well as by far the easiest plan. I wished, however, to throw all possible light on the problem, for the benefit of those who may hereafter extend these observations. I also wished to obtain the greatest amount of information from the data at my disposal; and by means of formulæ, to extend the results somewhat (though not far) beyond the limits of observation. It will

be seen farther on (Art. 71) that a formula, however empirical, enables us to execute promptly and with confidence the otherwise tentative and uncertain process of drawing tangents to the curve in its higher part, in other words, of obtaining values of $\frac{dv}{dx}$ upon which the final deductions mainly depend. (See Arts. 6 and 31.)

63. It had been acutely perceived by LAMBERT nearly a century ago,* that the temperatures of a bar heated in the manner of our experiment would diminish in a regular geometrical progression. A more rigorous analysis led BIOT† and FOURIER‡ to the same result, according to the physical assumptions with which they started. BIOT and DESPRETZ§ subjected various metallic bars to experiment, but they assumed the logarithmic law to be true, and endeavoured to accommodate their numerical results to it, as well as they might. BIOT, in particular, applied to his (apparently excellent) observations, the method of least squares to enable him to draw a logarithmic curve through his points of observation, giving no attention to the fact, that the temperatures found did not conform themselves by any means to the *a priori* geometrical law, and that the laws of Probability could not be applied to them without ascribing extravagant and improbable errors to a large part of the curve of observation.‖

64. That the temperatures deviate systematically from the law of continued progression, will appear from the following table of the ratios of successive ordinates, taken three inches apart in the three Cases of Table II.

MEAN RATIO¶ BETWEEN TWO CONSECUTIVE ORDINATES 3 INCHES APART, FROM THE NUMBERS IN TABLE II.

INTERVALS.	CASE I.	CASE II.	CASE III.
3 to 6 inches, . . .	·707	·650	...
6 to 9 „	·722	·673	·687
9 inches to I. foot, . .	·733	·690	·690
I. foot to I. foot 6 inches,	·753	·707	·719
I. foot 6 inches to II. feet,	·770	·727	·728
II. to III. feet, . . .	·787	·735	·750
III. to IV. „	·809	·762	·755
IV. to VI. „	·830	·774	·731

* Pyrometrie. Berlin, 1779, p. 185.

† Traité de Physique, vol. iv. p. 669.

‡ Théorie Analytique de la Chaleur. 1822.

§ Traité Elémentaire de la Physique. 1836, p. 197.

‖ Compare Note to Art. 3 of this paper.

¶ By "mean ratio," I intend to express, that where more than one 3-inch space is included in the Interval specified in the first column, the number which follows is the average decrement throughout that space. Thus, in Case I. the whole interval from II. to III. feet, shows a decrement from 24°·2 to 9°·33, which would result from the mean ratio of 0·787, four times multiplied into itself.

65. It will be observed that in every instance, with the single exception of the final number of the Table under Case III., the decrement of temperature becomes materially slower as we recede from the heated end of the bar. The exception is of little weight, as it depends on the residual temperature of the one-inch bar, six feet from the crucible, at which point the warmth was barely perceptible. The statical temperatures of the bar therefore increase more rapidly than in a geometrical progression. I have found that there is a sufficient analogy between the curve of statical temperature which we are here investigating, and that of the tension of steam at different temperatures, to afford some assistance in the selection of empirical formulæ in the present instance; being in each case a modified geometrical progression, though here the progression of ordinates is more rapid than a simple continued proportion, while in the tension of steam it is less rapid.

66. The most complete discussion of this class of formulæ, and the methods calculating from them, is to be found in M. REGNAULT'S *Relation des Expériences sur la Vapeur*. The available formulæ are reducible to three; YOUNG'S,* ROCHE'S,† and BIOT'S.‡ The two former contain three constants, the latter five. The last has been found the most satisfactory for the empirical representation of the elasticity of steam; and it is the only one of the three which can be regarded as applicable throughout the entire limits of experiment. The same is, I believe, true for the Conduction-curves with which we are now occupied. With five constants, five points of the curve may be accurately represented, and the intermediate deviations are of course inconsiderable. As none of the formulæ (except the simple logarithmic which is found to be inapplicable) have any foundation in principle, the whole matter is purely one of convenience. For simplicity's sake, I used only the formulæ of YOUNG and ROCHE, but I now think that the greater labour involved in the application of BIOT'S formula would have been repaid by the directness and certainty of the results. M. REGNAULT has given rules to facilitate its numerical calculation.

67. I have found the formula,—

$$\log v = \mathrm{A} - \frac{bx}{1 + cx} \quad \ldots \ldots \quad \text{Eq. (1.)}$$

(where v is the excess of temperature above the air at a point of the bar, whose abscissa, *in feet*, is x, and A, b, and c are constants), to represent tolerably well the temperature curve of Case I., as represented by the numbers in Table II.,

* $p = \mathrm{A}\,(1 + at)^n$, where p is the elasticity, and t the temperature.

† $\log p = \log a + \dfrac{bt}{1 + ct}$.

‡ $\log p = a + b\alpha^t + c\beta^t$.

throughout the whole extent of the experimental curve.* But in order to follow the observations more closely, it seemed desirable to divide the curve into two parts, one between 0 and 1·5 feet, and the other beyond 1·5 feet, and to employ distinct constants for each. A like process was applied to the numbers of Table II., for Cases II. and III. In these, the variations of temperature along the bar being more rapid, the approximation of the formulæ was less exact than in the first case, and a formula with three constants was insufficient to represent the curve throughout its whole extent. I found it advisable, in the case of the bar covered with paper (No. II.), to use modified formulæ for the upper, middle, and lower part of the curve. I may add that it was found convenient to adapt the formula (Eq. (1.) of this article) to the calculation of the lower temperatures, by changing the origin to an arbitrary point some feet to the right, and by reckoning the abscissæ in the opposite direction, thus rendering the second term of the equation positive instead of negative. To this end the equation was written,—

$$\log v = \mathrm{A} + \frac{bz}{1+cz},$$

when

$$z = n - x.$$

68. The coincidence of the various formulæ with the experimental numbers of Table II. is shown in the foregoing Table.

* The formula in this case would be,—

$$\log v = 272.7 - \frac{\cdot 63374\, x}{1 + \cdot 0956\, x}.$$

The following adaptation of YOUNG's formula also represents the observations in Case I. very approximately.

$$v = (\cdot 43027 + \cdot 09539\, x)^{-6\cdot 65}.$$

x in feet.	v by Experimental Curve.	v by Formula $(a+bx)^p$.	Difference.	v by Formula $\log v = \log a - \frac{bx}{1+cx}$	Difference.
	°	°	°	°	°
0	...	272·66	...	272·7	...
0·25	190·5	190·5	0·0	191·0	+0·5
0·5	134·7	135·5	+0·8	135·9	+1·2
0·75	97·3	98·04	+0·74	98·23	+0·93
1·0	72·0	72·0	0·0	72·0	0·0
1·25	53·6	53·6	0·0	...	...
1·5	40·8	40·41	−0·39	40·21	−0·59
2·0	24·2	23·75	−0·45	23·53	−0·67
2·5	14·8	14·52	−0·28	...	...
3·0	9·33	9·18	−0·15	9·08	−0·25
4·0	4·0	4·0	0·0	4·0	0·0
5·0	1·8	1·91	+0·11	1·96	+0·16
6·0	0·9	1·00	+0·10	...	...
8·0	0·28	0·31	+0·03	0·36	+0·08

TABLE III.—Showing the Comparison of the Graphical Curves of Statical Temperature with different Formulæ of Interpolation.

Distance from Origin in		Case I.				Case II.					Case III.			
Inches and Feet.	Feet and Decimals $= x$.	v by Experim. Curve.	Form. A.	Form. B.	Differ-ence.*	v by Curve.	Form. C.	Form. D.	Form. E.	Differ-ence.*	v by Curve.	Form. F.	Form. G.	Differ-ence.*
0 inch	0	...	275·5	...	...	...	260·5	...	...	...	...	282·2	...	...
1 ,,	$0{\cdot}08\dot{3}$	...	242·9	...	...	...	221·7	...	...	...	...	243·2	...	...
2 ,,	$0{\cdot}1\dot{6}6$	...	214·8	...	...	...	189·5	...	...	...	...	210·2	...	...
3 ,,	0·25	190·5	190·5	...	0·0	162·9	162·9	162·9	...	0·0	...	182·2	...	...
4 ,,	$0{\cdot}33\dot{3}$	168·8	169·3	...	+0·5	140·0	140·6	140·6	...	+0·6	159·0	158·4	...	−0.6
5 ,,	$0{\cdot}41\dot{6}$	150·6	150·9	...	+0·3	121·5	121·9	121·9	...	+0·4	137·5	138·2	...	+0·7
6 ,,	0·5	134·7	134·7	...	0·0	105·9	105·9	[106·0]	...	0·0	120·5	120·8	...	+0·3
7·5 ,,	0·625	114·1	114·2	...	+0·1	86·6	86·52	[86·53]	...	−0·08	99·8	99·36	...	−0·44
9 ,,	0·75	97·3	97·3	...	0·0	71·3	71·32	[71·14]	...	+0·02	82·8	82·18	...	−0·62
1 ft. 0 ,,	1·0	72·0	71·6	...	− 0·4	49·2	[49·48]	49·08	...	−0·12	57·1	57·19	[56·8]	+0·09
3 ,,	1·25	53·6	53·6	53·6	0·0	34·5	...	[34·7]	34·4	−0·1	40·9	40·67	[40·9]	−0·23
6 ,,	1·5	40·8	40·77	40·75	−0·04	24·6	...	[25·1]	24·64	+0·04	29·5	29·52	[29·61]	+0·02
2 ft. 0 ,,	2·0	24·2	[24·61]	24·17	−0·03	13·0	...	...	12·94	−0·06	15·65	...	15·76	+0·11
6 ,,	2·5	14·8	...	14·80	0·0	7·0	...	...	7·00	0·0	8·55	...	8·55	0·0
3 ft. 0 ,,	3·0	9·33	...	9·33	0·0	3·8	...	...	3·89	+0·09	4·95	...	4·72	−0·23
4 ft. 0 ,,	4·0	4·0	...	4·00	0·0	1·28	...	...	1·29	+0·01	1·56	...	1·52	−0·04
5 ft. 0 ,,	5·0	1·8	...	1·87	+0·07	0·47	...	..	0·47	0·0	0·55	...	0·52	− 0·03
6 ft. 0 ,,	6·0	0·9	...	0·95	+0·05	0·165	...	...	0·185	+0·02	0·13	...	0·189	+0·06
7 ft. 0 ,,	7·0	0·50	...	0·51	+0·01	...	...	...	...	...	...	...	...	...
8 ft. 0 ,,	8·0	0·28	...	0·279	0·00	...	...	...	...	...	...	...	...	...

* The bracketed numbers are not used in taking the differences.

69. The formulæ used in the preceding calculations are the following :—

$$\text{Case I.}\left\{\begin{array}{ll}\text{(A)} & \log v = \log 275{\cdot}5 - \dfrac{{\cdot}66184x}{1+{\cdot}13093x} \\[2ex] \text{(B)} & \log v = \log \quad 4{\cdot}0 + \dfrac{{\cdot}3472z}{1-{\cdot}0556z}, \text{ where } z=4-x.\end{array}\right.$$

$$\text{Case II.}\left\{\begin{array}{ll}\text{(C)} & \log v = \log 260{\cdot}5 - \dfrac{{\cdot}85265x}{1+{\cdot}1819x} \\[2ex] \text{(D)} & \log v = \log 259{\cdot}08 - \dfrac{{\cdot}83855x}{1+{\cdot}1606x} \\[2ex] \text{(E)} & \log v = \log \quad 0{\cdot}47 + \dfrac{{\cdot}4217z}{1-{\cdot}0405z}, \text{ where } z=5-x.\end{array}\right.$$

$$\text{Case III.}\left\{\begin{array}{ll}\text{(F)} & \log v = \log 282{\cdot}2 - \dfrac{{\cdot}7872x}{1+{\cdot}1275x} \\[2ex] \text{(G)} & \log v = \log \quad 0{\cdot}52 + \dfrac{{\cdot}4521z}{1-{\cdot}0282z}, \text{ where } z=5-x.\end{array}\right.$$

70. With reference to the preceding numerical Table, I may remark, *First*, that the differences shown are not in all cases deviations from *direct* observations, but between the formulæ and the graphical interpolation of the data. There is a difficulty (which will be understood from Art. 57) in comparing compendiously the formulæ with the *single* data of Table I. When the points of the curve are somewhat distant from points of observation, the numbers in the preceding Table, obtained from the formulæ, may be, and probably are more reliable than those assigned from the curve. *Secondly*, the curve of Case I. appears to be the most reliable in all respects. And in particular I consider the portion of the curve which includes the highest temperatures, or those corresponding to points on the bar between 0 and 3 inches, to be very nearly accurate. From numerous independent calculations, I conclude that the value of v at the origin, or in contact with the crucible, is pretty exactly 275°·5 Cent., as there assigned. If we add to this 12°·5 for the approximate temperature of the apartment, we have 288° for that of the bar where it enters the crucible, and is supposed to have very nearly the temperature of melting lead. This is a considerably lower temperature than is usually attributed to melting lead.*

* Usually stated at from 320° to 330° Cent., 608° to 626° Fahr. Biot, indeed, gives it as only 260°, inferentially derived from his conduction experiments (Traité de Physique, iv. 677); but this is on the supposition of the logarithmic law prevailing. Crichton, junior, gives 606°·5 Fahr. (T. Thomson); Daniell, 612°; Kupffer, 633°. Supposing any of these last numbers to be correct, the inference must be, that in the conduction experiments described in the present paper, the temperature of melting lead did not extend to the outside of the iron crucible when the origin of the co-ordinates has been taken, but must be sought somewhere in the interior. This conclusion is strengthened by some other, though indirect considerations.

71. I cannot too distinctly repeat that the formulæ adopted in the preceding Table are only to be regarded as a means of more conveniently grouping the observations. The most important use to be made of these formulæ, however, yet remains to be mentioned. It will be seen by reference to Arts. 6, 28, &c., of the former part of this paper, or to § IV. of the present paper, that it is not the ordinates themselves of the statical curve of cooling which are to be used in obtaining the conductivity of the bar, but the values of the differential coefficient $-\frac{dv}{dx}$ for each part of the bar. In other words, we must be able to draw a tangent to the curve of statical temperature at any point of the curve. This may be roughly done mechanically, or it may be done by dividing the curve into short elementary portions, and treating each portion as if it were part of a logarithmic curve (see below, Art. 82, on the Analogous Treatment of the Dynamical Curve); or, finally, it may be obtained from the equations above given. The two last methods have been used in the reductions, and especially the last of all, which is the only satisfactory one for the higher parts of the statical curve. The general form of the empirical equation being,

$$\text{Tab. log } v = \mathrm{A} - \frac{bx}{1+cx}$$

when reduced to Napierian logarithms, gives

$$0{\cdot}4343 \text{ hyp. log } v = \mathrm{A} - \frac{bx}{1+cx}$$

and

$$\frac{dv}{dx} = -2{\cdot}3036 \frac{bv}{(1+cx)^2}$$

whence the numbers which will be given in § IV. of the present paper are computed, the values of b and c being taken from the formulæ of Art. 69.

§ II. *Experiments on Cooling.*

72. *The Apparatus.*—It will be seen, by reference to the former part of this paper (Arts. 5, 24), that, in order to interpret the indications of the permanent temperature of a bar, and to deduce its conductivity, we must have an independent set of observations on the cooling of a similar bar, or a portion of a similar bar. For this purpose, the apparatus shown in fig. 2 of Plate I. was employed. The same short bar, L M, which has been already referred to (Art. 51), as being used in the statical experiment for determining the temperature the bar would have had independently of the heat applied at one end, was supported on the props N, O, after being duly heated. It is now to be used to ascertain the rate of loss of heat from a bar having the Section and Surface proper to each of the three Cases of Art. 48, in terms of the scale of the thermometer P, inserted at or near its middle point.

73. I have so fully described, in Art. 24, the manner of performing the

Cooling experiment, that I need here do little more than refer to the figures by which it is now illustrated, and give the corrected results as to the "law of cooling."

74. Fig. 2 of Plate I. shows the small iron bar employed, which in Case I. and Case II. (Art 48.) was 20 inches long and $1\frac{1}{4}$ inch square, first naked and polished, and afterwards covered with paper; it was marked C. In Case III. it was a polished (or at least a bright) bar, 20 inches long, 1 inch square, and marked E. Each bar had a ring at each end, *l*, *m*, and could be handled by seizing either end by the hook Q, fig. 3. Having been covered with several folds of stout paper to prevent a sudden chill of the metal bath into which it was to be introduced, it was lowered vertically and lengthwise into the cylindrical iron vessel shown at fig. 3, and in section in Plate II. fig. 2. It consists of a stout iron tube TV, about two feet long, with a bottom at V, and a handle at T. It rests by means of two iron pins, *o*, *p*, on the upper edge of a cylindrical iron chimney RS, supported by three feet, of which two are seen at *q* and *r* over the gas furnace U, the powerful flame of which, playing between the two cylinders, keeps a quantity of solder or of "fusible metal" in the interior one, not only melted, but heated considerably above the melting point. The bar under experiment, after being coated with several folds of paper, having usually also been well warmed over a hot-air stove, was introduced by the hook Q into the metal bath, then turned end for end several times, until it was believed that the heat had well penetrated its entire thickness. It was then withdrawn, shaken, the paper covering rapidly cut off, the bar wiped with a cloth,* and placed horizontally on the two ivory-topped props N, O (Plate I. fig. 2), the thermometer P inserted in the central hole,† into which heated mercury had already been placed, and the reading of the thermometer from minute to minute immediately commenced, the times being given by an assistant. The free temperature was determined by a thermometer sunk in a cold bar in the neighbourhood, or by one suspended in the air, or by both.

75. *The Observations.*—As in the statical observations there are three cases.

Case I.	Iron bar, $1\frac{1}{4}$ inch square, roughly polished.		
Case II.	Do.	do.	covered with paper.
Case III.	Do.	1 inch square, roughly polished.	

76. Two independent sets of observations of the law of cooling on different days have been obtained for each case. Moreover, as more than one thermometer was observed in the holes of each bar (as in the example which follows), except for the very highest temperatures, use has from time to time been made of these auxiliary series. The whole of these observations have been most carefully corrected for the

* The *wiping* of the bar I believe to have been unnecessary and injurious. It lowered the temperature, and interfered with the distribution of the heat in the bar.

† The $1\frac{1}{4}$ inch bar had a central hole, and others 1·5 inch distant, right and left. The 1 inch bar had only two holes equidistant from the centre of the bar.

scale errors and for the temperature of the stem. Where the temperature of the air of the room has not been quite steady, the variations have been interpolated and allowed for in deducing the excess of temperature of the bar above that of the room.

77. I shall give the details of one experiment as a specimen (all reductions being first made).

TABLE IV.—COOLING OF SHORT $1\frac{1}{4}$-INCH BAR, C.—26TH MARCH 1851.

Hour.	Hole (2), Centre. Corrected Excess.	Hole (1), to the Left. Corrected Excess.	Hole (3), to the Right. Corrected Excess.
h. m. s.	°	°	°
12 59	170·7		
1 0	167·4		
1	164·0		
2	160·85		
3	157·6		
4	154·5		
5	151·45		
6	148·45		
12	131·7		
13	129·2		
14	126·6		
15	124·2		
16	121·7	119·85	
17			
18	116·95		
19			114·25
20	112·65		
21		110·85	
22	108·35		
23			106·0
24	104·5		
25		103·05	
26	100·65		
29			95·10
30	93·75		
31		92·05	
32	90·25		
33		88·9	
34	87·0		
35			85·5
36	84·10		
37			82·5
38	81·25		
39		79·95	
40	78·50		
2 0	56·10		

Hour.	Hole (2), Centre. Corrected Excess.	Hole (1), to the Left. Corrected Excess.	Hole (3), to the Right. Corrected Excess.
h. m. s.	°	°	°
2 1		55·20	
2	54·15		
3			53·2
4	52·4		
5		51·65	
6	50·6		
7			49·75
8	49·0		
9		48·35	
10	47·4		
21	40·1	40·15	
22	39·35		39·40
24	38·25	38·20	
26	37·10		37·10
28	36·0	36·05	
48	26·9	26·95	
50	26·15		26·2
52	25·4	25·4	
54	24·6		24·6
3 31	14·95	14·92	
34	14·35	14·32	
36	14·0		13·92
38 30	13·40		13·25
4 10	8·98	9·00	8·92
15	8·48	8·50	8·41
20	7·98	7·9	7·8
25	7·53	7·55	7·44
6 0*	2·79	2·6	
10*	2·54	2·4	
20*	2·15	2·1	
30*	2·0	1·9	
8 10*	0·9	0·7	
20*	0·9	0·7	
30*	0·85	0·7	

* Read by an assistant. The scale of the thermometer in hole (3) being liable to mistake in reading, its results are omitted.

78. *Graphical Interpolations.*—The observed excesses of temperature (as obtained, for example, in the preceding experiment for the central hole) were projected in a curve of which the times were taken as abscissæ, and the independent temperatures of the bar as ordinates. When more than one series of observations (on the same bar at different times) were to be combined, a procedure exactly similar to that described in Art. 57 for the stationary temperatures was employed; that is to say, one series being first projected on the engraved paper as fundamental, any other series was next similarly projected on tracing cloth, and the system of points thus obtained was moved to the right or left over the first, until the points in the two curves appeared to be superposed satisfactorily. The interpolated observations were then pricked through, and a curve drawn through the whole.

TABLE V.—Curves of Cooling (in Terms of Time).

Time from Arbitrary Origin.	Case I. 1¼ inch Bar.		Case II. 1¼ inch Bar, covered.	Case III. 1 inch Bar.
	March 26.	March 29.		
−10 Min.	...	*242·3	...	...
− 5	...	*221·4	*263·9	...
− 2·5	...	*211·5	*243·6	...
0	...	*201·9	*225·0	*258·5
2·5	...	192·0	*207·7	*243·3
5	...	183·5	*191·8	*228 9
7·5	...	174·8	177·0	*215·3
10	167·3	166·5	163·6	202 4
12·5	159·1	158·2	150·6	190·05
15	151·4	150·6	139·1	178·7
20	137·0	136·5	119·25	157·55
25	124·2	124·2	102·95	138·9
30	112·6	112·8	88·8	122·45
35	102·6	102·5	77·1	108·6
40	93 7		67·0	96·45
50	78·6		50·95	77·25
60	65·9		39·4	62·2
70	55·8		30·65	50·5
80	47·3		24·25	41·3
90	40·5		19·2	33·95
100	34·9		15·27	28·2
125	24·2		8·9	18·0
150	17·1		5·45	11·95
175	12·3		3·42	8·1
200	8 95		2·15	5·55
300	2·95		0·4	1·4
400	1·12		...	0·4

The numbers marked thus * are deduced from the Equations of Art. 88.

79. A specimen of the Curves of Cooling is given in Plate IV. The subsidiary curves in the same plate show different sections of the main curve projected on different scales (as in the case of the Statical Curves, Art. 60), for convenience of interpolation. The main curve corresponds to Case I. The dotted line adjacent to the main curve in the Plate shows the modification of the law of cooling introduced by covering the bar with paper, as in Case II. The results of the whole are shown in the preceding Table. The origin of the abscissæ (the times) is of course wholly arbitrary in each case.

80. The continuity of the curves thus obtained was in general satisfactory, though in one or two instances it seemed desirable to project part of two curves as distinct, as in Case I.

81. It is of little use, however, to possess merely a knowledge of the free temperature of the bar in terms of the time. The valuable information which we require in the deduction of conductivity is the "rate of cooling," or the proportional momentary loss of heat corresponding to a given excess of temperature. This is expressed mathematically by $\frac{dv}{dt}$, and might be directly obtained by discovering the equation to the primary curve of cooling, and then differentiating it.

82. There is, however, not less difficulty in finding a formula of interpolation to represent the curve of Cooling throughout its extent, than we have already found in the case of the curve of Statical Temperature, and it would evidently require the introduction of as many constants. I therefore preferred, in the first instance, (seeing that from the multiplied observations of cooling, the ordinates of this curve are more perfectly known than in the other case), to subdivide it into elementary arcs, and treating each of these as a portion of a logarithmic curve (to which it approximates), to find the value of $\frac{dv}{dt}$, or the "rate of cooling," corresponding to successive values of v,* and by projecting these in curves to study their inflections in detail in each of the three forms of experiment already often referred to.

83. The three upper figures of Plate V. represent the "rates of cooling" of each bar in terms of its temperature-excess. From the study of these the peculiarities of the law of cooling above adverted to will become evident, and the harmony of the three cases is exhibited to the eye.

84. *First*, For very small excesses of temperature, the rate of cooling is comparatively slow, but increases much more rapidly than the temperature. To illus-

† By the formula $\frac{dv}{dt} = 2{\cdot}3026\,\frac{\log v' - \log v}{t - t'} \times \frac{v' + v}{2}$, where v and v' are the excesses of temperature corresponding to the times t and t'. $\frac{v' + v}{2}$ is the mean ordinate to which the result corresponds. The logarithms are tabular.

trate this, a portion of each curve near its origin has been drawn separately on an exaggerated scale in a subsidiary figure, where its deviation from a straight line is abundantly manifest. In each case it may be adequately represented for the first 4° or 5° by an arc of a common parabola—more accurately perhaps by a semicubical parabola. However, taking the former as the simplest, I find the following equations to represent the part of the three curves nearest their origin:—

$$\text{Case I.} \quad \frac{dv}{dt} = \cdot 00836\,v + \cdot 000615\,v^2$$

$$\text{Case II.} \quad \frac{dv}{dt} = \cdot 01626\,v + \cdot 00065\,v^2$$

$$\text{Case III.} \quad \frac{dv}{dt} = \cdot 01046\,v + \cdot 00091\,v^2$$

85. *Secondly*, The concavity upwards of these curves of the "rate of cooling"—showing that the cooling increases faster than the temperature rises—gradually diminishes; and in all the three curves we find between 110° and 120° (centigrade), a space nearly straight, indicating a point of contrary flexure. Above 150° the curve is in all the three cases slightly convex upwards, showing a rate of cooling slower in proportion than the rise of temperature.

86. *Thirdly*, This last circumstance appeared to me to be deserving of an elaborate verification. I therefore applied the same formula of interpolation which I had used with success to represent considerable arcs of the statical curve of temperature (see Art. 67, Eq. (1.)), being what has been called ROCHE's formula, to represent the temperatures of the cooling bar in the higher parts of the primary curve of cooling.

87. In this I was successful, and I deem the matter of sufficient importance to show the coincidence between the original thermometric observations and the formulæ employed in each of the three cases. The times (t) are in each case reckoned from an arbitrary origin; and v is the excess of temperature above that of the air actually observed. [See Table VI.]

88. *Fourthly*, It will be seen that, within the limits of these tables, the observations are, upon the whole, well represented by the equations. Moreover, they confirm a result at which I had previously arrived from the projections, as to the law of cooling at higher temperatures, namely, that above 140° or 150° there is a gradual falling off in the rate of cooling, compared to the measure of temperature. For it is to be observed, that the equations employed to represent the primary curve of cooling coincide with the simple geometrical or logarithmic law, when the co-efficient of t in the *denominator* of the fraction (c of Art. 67) vanishes. When this co-efficient is positive, the progression is faster than geometrical; when negative, it is slower. Now, in each of the three cases c is negative,

TABLE VI.

Case I.—$1\frac{1}{4}$-inch Bar, Naked.				Case II.—$1\frac{1}{4}$-in. Bar, Covered.				Case III.—1-inch Bar, Naked.			
Formula; $\log v = 2{\cdot}30471 - \frac{{\cdot}008133t}{1-{\cdot}00262t}$.				Formula; $\log v = 2{\cdot}35215 - \frac{{\cdot}01385t}{1-{\cdot}00007t}$.				Formula; $\log v = 2{\cdot}41255 - \frac{{\cdot}01051t}{1-{\cdot}00113t}$.			
t	v observed.	v calc.	Diff.	t	v observed.	v calc.	Diff.		observed.	v calc.	Diff.
minutes.	°	°	°	minutes.	°	°	°	minutes.	°	°	°
2·5	192·1	192·45	+0·35	7·5	177·0*	177·1	+0·1	9	207·4	207·5	+0·1
3	190·3	190·55	+0·25	8	174·3	174·3	0·0	10	202·4	202·4	0·0
4	187·15	187·0	−0·15	9	168·7	168·8	+0·1	11	197·45	197·4	−0·05
5	183·5*	183·45	−0·05	10	163·6	163·5	−0·1	12	192·65	192·6	−0·05
6	179·95	179·95	0·0	11	158·25	158·4	+0·15	13	187·9	187·9	0·0
7	176·55	176·55	0·0	12	153·4	153·4	0·0	14	183·25	183·3	+0·05
8	173·1	173·15	+0·05	13	148·4	148·5	+0·1	15	178·7	178·7	0·0
9	169·7	169·65	−0·05	14	143·75	144·0	+0·25	17·5	167·5*	167·8	+0·3
10	166·55	166·4	−0·15	15	139·3	139·3	0·0	20	157·55	157·55	0·0
14	153·45	153·7	+0·25	16	135·0	135·0	0·0	21	153·75	153·5	−0·25
15	150·45	150·55	+0·1	17	130·9	130·8	−0·1	22	149·9	149·7	−0·2
16	147·65	147·5	−0·15					23	146·1	146·0	−0·1
								25	138·9*	138·7	−0·2
* From interpolating curve.				* From curve.				* From curve.			

consequently the progression is slower than geometrical, and the curve of the "rate of cooling," in terms of v, is convex upwards, as already stated.

89. *Fifthly*, By satisfying the observations by equations, we have farther these advantages—(1.) We can, with approximate accuracy, extend the law of cooling somewhat beyond the limits of observation, though with caution; (2.) We can also obtain the values of $\frac{dv}{dt}$ in a ready and continuous manner. The higher parts of the curves in Plate V. have been deduced in this way, and thus the "rate of cooling" has been tabulated for temperatures higher than those actually observed; but such numbers, being more or less hypothetical, are distinguished by asterisks in the following Table, which in other respects includes the results obtained from the observations treated as has been already described.

TABLE VII.—SHOWING THE "RATE OF COOLING," $-\frac{dv}{dt}$ FOR DIFFERENT EXCESSES OF TEMPERATURE (v).

v.	CASE I. 1¼ inch Bar, naked.	CASE II. 1¼ inch Bar, papered.	Ratio to I.	CASE III. 1 inch Bar, naked.	Ratio to I.
1	0·009	·017		0·0115	
2	·019	·035		·0245	
3	·031	·054	1·74	·0395	1·28
4	·043	·075		·056	
5	·057	·096		·072	
10	0·124	·203	1·64	·158	1·27
20	·275	·44	1·60	·34	1·24
30	·43	·72	1·67	·55	1·28
40	·60	1·01	1·68	·78	1·30
50	·80	1·30	1·62	1·01	1·26
60	1·01	1·62	1·60	1·25	1·24
70	1·21	1·95	1·61	1·52	1·26
80	1·42	2·27	1·60	1·77	1·25
90	1·63	2·60	1·59	2·04	1·25
100	1·84	2·95	1·60	2·33	1·27
120	2·27	3·67	1·62	2·92	1·28
140	2·80	4·40	1·57	3·50	1·25
160	3·18	5·08	1·60	4·03	1·27
180	3·48	5·75	1·65	4·50	1·29
200	3·78	*6·38	1·69	4·95	1·31
220	*4·04	*7·00	1·73	5·40	1·34
240	*4·29	*7·65	1·78	*5·85	1·36
260	*4·52	*8·28	1·83	*6·30	1·39
280	*4·75	*8·90	1·88	*6·72	1·42

The numbers marked thus * being the results of calculation, are to be regarded as more or less hypothetical, and increasingly so at the higher temperatures.

90. *Sixthly*, I will not attempt to account for the inflections of the curves of Plate V. on physical principles, farther than to remark that the rapid increase in the velocity of cooling with temperature in the lowest part of the scale is perhaps owing to the separate effects of cooling by radiation, and cooling by convection. It seems probable that a certain excess of temperature of the bar above the air is necessary to determine efficient atmospheric currents, and thus to accelerate the rate of cooling; that, in fact, there is an amount of viscosity in air, which it requires a certain elevation of temperature properly to overcome. I would also observe, that the cooling in Case I. is (at higher temperatures) less regular than in the two other cases, while in Case III. the logarithmic law is almost accurately observed at those temperatures. This is no doubt to be ascribed to the greater mass of the Bar No. I., compared to its radiating power, occasioning probably

sensible irregularities, depending on the primitive distribution of heat in the bar, and on the want of uniformity in the temperature of its transverse section. The nearer that we approach to the ideal of an infinitely slender bar, the more shall we escape those periodical irregularities (see Art. 25 of the former part of this paper), arising from the primitive distribution of heat in its substance, which no doubt gives rise to some of the peculiarities of the inflections in the curves of "rates of cooling." In particular, we may naturally ascribe, in part at least, to the fact that the bar is heated first of all to a uniform temperature throughout in the fusible metal bath, the relatively diminished rate of cooling observed at the highest temperatures. At the same time I would repeat the caution, that the hypothetical or dotted portion of those curves cannot be relied on as expressing an actual fact, at least to more than a little way beyond the range of experiment.

§ III. *On the Proportion of Heat dissipated from the Bar by Radiation and Convection.*

91. Although not of direct importance to the determination of conducting power, I will indicate shortly how the numbers in Table VII., may be used to ascertain the relative amount of heat lost by radiation and convection at any or all points of the surface of the bar in Cases I. and II. The method was originally due to Sir JOHN LESLIE, but was stated more clearly by DALTON (System of Chem. Philosophy, p. 115), and was happily applied by DULONG and PETIT. Suppose the total "rate of cooling" of the same bar to be ascertained in air, first, when it is naked, and, secondly, when covered with paper, and let the ratio of the first case to the second be as $1:p$. Next, by comparing after the manner of LESLIE's canister-experiments the "emissive power" of the same two surfaces, iron and paper, let it be as $1:q$. Let the required ratio of the heat lost by convection to that lost by radiation be as $1:x$ in the first case; then, of course, it will be in the proportion of $1:qx$ in the second. But as the heat dissipated in each case is the sum of the effects due to convection (which is always $=1$), and that due to radiation, we have

$$1:p=1+x:1+qx$$

and

$$x=\frac{p-1}{q-p}$$

92. I have given in Table VII. the ratios of cooling, at different temperatures, for Cases I. and II., that is, for the same bar covered with paper and naked iron; and though the ratios vary somewhat,* yet they agree pretty nearly within the

* Since this was written, I have observed that a like diminution of the ratios of cooling from glass and silver up to a certain point, and afterwards an increase, was noticed by DULONG and PETIT, in their admirable Memoir on the Law of Cooling, page 102.—*Mem. Acad. Sci. Par.*

safe limits of observation. In fact, if we compare the average ratio from 10° to 100° Cent., and again from 100° to 200° Cent., we shall find them to be almost identical. They give for the value of p, the number 1·6023. This represents the proportion in which the papered bar dissipates its heat more rapidly than the naked bar.

93. For the direct radiating or emissive power of the two surfaces, I had recourse to the kind aid of Mr BALFOUR STEWART, not having had recently conveniences for making the experiment myself. He used the thermo-electric pile, and he found the experiment to be attended with considerably greater difficulty than is commonly attributed to it. I believe that Mr STEWART is not yet satisfied as to the reliability of his methods of observation; but the four best series of experiments made in February and March 1864, gave the emissive power of paper compared to iron as 5·8 to 1.* The value of q is therefore 5·8.

94. Hence by the previous investigation—

The value of x, the heat dissipated by radiation from naked iron (the dissipation by convection being always $=1$) is $x=\frac{\cdot 6023}{5\cdot 8-0\cdot 60}=0\cdot 116$. In the case of the paper surface, x is 5·8 times greater, or $=0\cdot 673$. In other words, of the heat dissipated from the bar in Case I., nearly $\frac{9}{10}$ths are lost by convection, and $\frac{1}{10}$th by radiation. In the paper-covered bar (Case II.), only $\frac{6}{10}$ths are lost by convection, and $\frac{4}{10}$ths by radiation.†

95. From this it appears that the principal agent in the dissipation of heat in these experiments is Convection and not Radiation; nay, that the effect of the latter is comparatively almost insensible, when naked metallic bars are used. This of itself tends to explain the systematic deviation of the statical curve of temperature (Art. 64) from the logarithmic law. The experiments of DULONG and PETIT show that the dissipation of heat due to Convection increases not as the excess of temperature simply, but as its $\frac{5}{4}$th power nearly (more exactly 1·233). This accords so far with what has been said of the variation in the rate of cooling in Art. 84; but it gives no adequate explanation of the inflections of the curves of Plate V. at higher temperatures. Were it not for the unquestionable precision of DULONG'S admirable experiments, in which the law of cooling due to the contact of air was verified as high as 260° Cent., one might have not unreasonably supposed that the energy of convection was *relatively* less at higher temperatures.

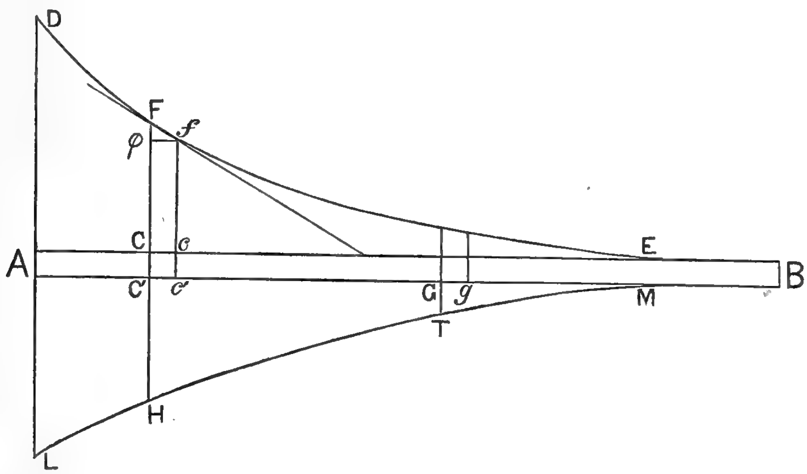
* This corresponds nearly to the relative emissive power of glass and polished silver used by DULONG.

† For in Case I. the whole heat lost from a point having a given temperature being represented by the number 1·116, that due to Convection is 1, that due to Radiation is ·116. In Case II. the total loss is 1·673, whereof 1 is due to Convection, and ·673 to Radiation.

§ IV.—*The "Statical Curve of Cooling." Recapitulation and Application of the Method of Deducing the Conductivity.*

96. It will be convenient here to recapitulate, from Arts. 5, &c. of the former part of this paper, the use which is to be made of the data obtained from the two fundamental experiments described in the previous sections, namely, the determination of the Curve of Statical Temperatures (Table II. Art. 61), and the rate or velocity of Cooling of the bar at any temperature (Table VII. Art. 89).

97. Let A B be the bar, kept hot at the extremity A, and left to assume a per-



manent temperature at its various points under natural causes. Let the upper curve, or DFE, represent by its ordinates (as FC) these temperatures. All the heat that enters the bar at A, and is propagated along it, has to be accounted for. Since the bar is so long that at the end B the heat has become insensible, the entire heat entering the bar at A has been dissipated from its surface in various proportions, according to its temperature, between the point A and some remote point E where the elevation of temperature is practically insensible. In like manner, if we take any point C in the bar, the heat transmitted from the hotter end, by conduction across the transverse section of the bar at C, is dissipated by the cooling of the bar between C and E. To know the quantity of heat passing across this transverse section, we have therefore to ascertain the aggregate loss of heat from the surface of the bar to the right hand of C.

98. To do this, we must construct what I call the *Statical Curve of Cooling*, which is represented in the same figure by the curve LHM, beneath the bar AB. The ordinate C′H represents the heat lost by the bar *per minute*, from the portion CC′, whose temperature is represented by CF in the upper curve. This loss or C′H, is found from Table VII., by entering it with the thermometer reading CF, which again is known from Table II. in terms of the position of the point C in the length of the bar. Thus all the ordinates of the Statical Curve of Cooling, LHM, can

be constructed. Dividing the length of the bar into sections, in the three experimental cases so often referred to, the ordinates of the curve of statical cooling, or values of $-\frac{dv}{dx}$, appropriate to every point of the bar, will be found as in the following Table:—

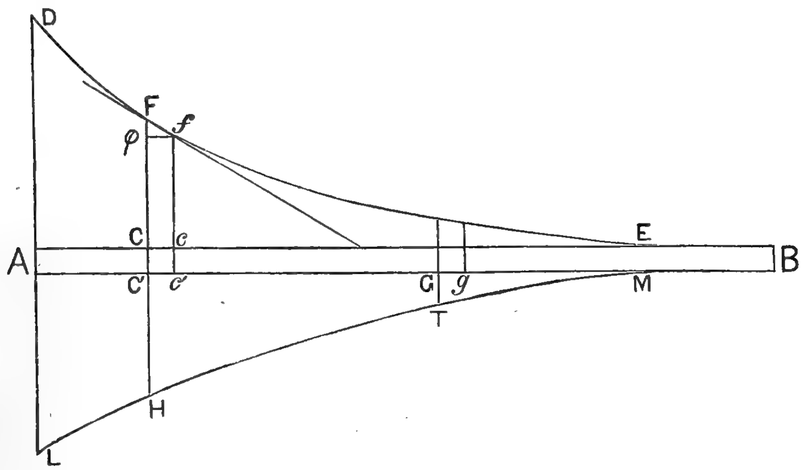
TABLE VIII.—SHOWING THE RATE OF COOLING PROPER TO EACH POINT OF THE LENGTH OF THE BAR (OR ORDINATES OF THE *Statical Curve of Cooling*), CONTAINING ALSO THE VALUES OF $-\frac{dv}{dx}$.

Distance from Origin in Feet and Inches.	Case I.		Case II.		Case III.	
	$-\frac{dv}{dt}$	$-\frac{dv}{dx}$	$-\frac{dv}{dt}$	$-\frac{dv}{dx}$	$-\frac{dv}{dt}$	$-\frac{dv}{dx}$
Ft. In.	°	°	°	°	°	°
„ 0	4·71	420	8·30	512	6·75	512
„ 1	4·32	362	7·06	423	5·92	432
„ 2	3·97	314	6·05	351	5·19	366
„ 3	3·64	272	5·20	292	4·54	310
„ 4	3·32	237	4·41	245	4·00	264
„ 5	3·01	206·8	3·73	206·5	3·44	226
„ 6	2·70	181·0	3·14	175·2	2·93	193·6
„ 7·5	2·20	148·8	2·48	137·6	2·33	154·6
„ 9	1·80	123·0	1·98	109·0	1·85	124·1
I. 0	1·245	85·35	1·28	69·9	1·185	81·6
6	0·620	43·70	0·58	35·2	0·55	38·0
II. 0	0·342	24·47	0·282	16·3	0·258	19·64
III. 0	0·114	8·15	0·070	4·47	0·070	5·52
IV. 0	0·043	3·34	0·021	1·36	0·0185	1·69
VI. 0	0·008	0·57				

N.B.—The values of $-\frac{dv}{dx}$ are computed by the method explained in Art. 71. Only at some of the lower temperatures a mixed method of calculation and projection has been used.

99. It is evident that, if we can effect the quadrature of successive sections of the statical curve of cooling, continued until it vanishes in the direction of the cool end of the bar, we shall have got the "flux of heat" across the section of the bar at which the quadrature commences. The measure of the heat expressed by the area of the curve in question will have for unit the amount of heat required to raise unit of volume (1 cubic foot) of iron by 1° Cent. The shaded curve, in the lower part of Plate III., shows the Statical Curve of Cooling proper to Case I. The ordinates of the curve are related to those of the curve of statical temperature immediately above it, by the relation of $-\frac{dv}{dt}$ to v, shown in the secondary curve of cooling in the upper figure of Plate V.

100. The flux of heat is greatest in the hottest part of the bar, because the temperature of the bar varies most rapidly there, and the heat is more rapidly drawn towards the cold end. To give exact expression to the tendency of the



heat to traverse the section of the bar at C, we will take Cc to represent the thickness of a plate, bounded by imaginary parallel surfaces, situated transversely within the bar through which the flow of heat is to be considered. This is to be compared with the flow of heat across any other plate, Gg, of equal thickness, in a different part of the bar. Then, according to FOURIER, the flow of heat across Cc will be proportional to the small decrement of temperature Fϕ, by which the side of the plate nearest to A is hotter than the farther side, and to the Conductivity jointly. The value of this decrement, Fϕ, is evidently nothing else than the differential coefficient $\frac{dv}{dx}$, which has been given in the last Table, as derived from the equations to the curve of statical temperature in Art. 71.

101. Hence (in conformity with Arts. 7, 31, and 35 of the first part of this paper),

$$\text{Flux of heat, or area CFE} = -\frac{dv}{dx} \times \text{conductivity},$$

or

$$\text{Conductivity} = \frac{\text{Area CFE}}{-\frac{dv}{dx}}.$$

§ V.—*The Method of this paper applied, under the usual assumptions made in the Theory of Conduction, as a first approximation to the determination of Conductivity.*

102. The area of the statical curve of cooling to the right hand of any ordinate is therefore to be found. It will be convenient, for this purpose, to show what the nature of this curve would be were the *usual assumptions* of the mathematical theory of Heat adopted. These assumptions are (1.) That the superficial

loss of heat follows NEWTON'S law, or that the loss of heat in unit of time varies simply as the excess of temperature; (2.) That the same law holds for the internal communication of heat, or that the quantity of heat conducted is proportional simply to the difference of temperature of two adjacent elementary portions of a bar.

103. From the *first* assumption it follows, of course, that the temperature of a cooling body of small dimensions varies in a decreasing geometrical progression with the time. The dynamical Curve of Cooling on this assumption is a logarithmic curve, t and v being the variables.

104. From the *second* assumption, taken along with the first, we learn from a well-known analysis, that what we have called the Curve of Statical Temperature is also a logarithmic, x and v being the variables.

105. Now the Statical Curve of Cooling ($\frac{dv}{dt}$, in terms of x) must, on these assumptions, be also logarithmic; for its ordinates—the velocities of cooling—are everywhere proportional to the temperature. Hence also the subtangent to these two last curves* is the same. Let it be M. Then by a property of the logarithmic curve (M being the modulus) the area of the curve bounded by an ordinate y, and carried to infinity, is $\mathrm{M}y$. Also the flux of heat corresponding to the position of the ordinate y is (Art. 99)$=\mathrm{M}y$, y being, as we have seen, $=-\frac{dv}{dt}$. But, by Art. 102, $-\frac{dv}{dt}$ is everywhere *assumed* (for the present) to be proportional to v, or $-\frac{dv}{dt}=pv$. Also since the dynamical curve of cooling is a logarithmic (103), let its modulus be m. Then, by the property of the curve, $-\frac{dv}{dt}=\frac{v}{m}$. Hence, comparing the last two equations $p=\frac{1}{m}$. And,

$$\mathrm{F} = \text{Flux of heat} = \mathrm{M}y = -\mathrm{M}\frac{dv}{dt} = \mathrm{M}\frac{v}{m}$$

and (by Art. 101).

$$\text{Conductivity} = \frac{\mathrm{F}}{-\frac{dv}{dx}} = \frac{\mathrm{M}\cdot v}{-m\cdot\frac{dv}{dx}}$$

But the curve of statical temperature being also assumed to be logarithmic (104); and consequently

$$-\frac{dv}{dx}=\frac{v}{\mathrm{M}};$$

we finally get

$$\text{Conductivity} = \frac{\mathrm{M}v}{m\,.\,\frac{v}{\mathrm{M}}} = \frac{\mathrm{M}^2}{m}$$

106. A first approximation to the conductivity of the bar may therefore be found *by dividing the square of the modulus or subtangent of the statical curve of*

* Namely, the Curve of Statical Temperature and the Statical Curve of Cooling, being the two curves shown in the wood-cut of last page.

Temperature (assumed to be logarithmic) *by the modulus of the Dynamical Curve of Cooling.*

107. Thus, to illustrate this by a numerical example, were we to attempt to reduce the statical curves of temperature of Table II. to logarithmics after the manner of BIOT, we should probably find the following approximate values of the subtangent M:—

	CASE I.	CASE II.	CASE III.
M	0·9 foot	0·7 foot	0·8 foot

And from Table V. of the Dynamical Curves of Cooling, the subtangents might be nearly

m	60 min.	40 min.	50 min.

whence

$\frac{M^2}{m}$	·0135	·0122	·0128

which, it is seen, give nearly approaching values of the conductivity.

§ VI. *Final Determinations of the Conductivity of Iron at various Temperatures.*

108. The results given in the last section are in the highest degree rude, and are introduced merely to illustrate the general form of the method. The curves of Temperature and Cooling are neither of them sensibly logarithmic, and therefore we have found the necessity of dividing them into small portions, and taking their elements from point to point. Therefore, in continuation of what has been said in Art. 99, we must proceed to the quadrature of the Statical Curve of Cooling whose elements are given in Table VIII. This is a curve which though not logarithmic, may, like the other curves we have already discussed, be treated as if it had been, when divided into numerous elements bounded by parallel ordinates. Every one of these segments may have its area estimated by the simple formula proper to a logarithmic curve,* and for the infinite branch a similar formula must be adopted.

109. The following Tables contain the determination of the total Flux of Heat (F) across any section of the bar by the summation of the areas of the statical curve of cooling, commencing from the colder end of the bar, where this curve is (like the primary curve of temperatures) apparently asymptotic. In these Tables (corresponding to the three experimental Cases discussed in this Memoir, the chief uncertainty attaches to the two extremities of the curve. There are difficulties inherent in the precise determination of very small excesses of temperature of a bar, whether in a statical or a cooling condition, above the surrounding air, itself not absolutely constant in temperature. These difficulties have been previously referred to. Moreover, when we have to take the ratio of two quantities, both to be experi-

* Namely, area between ordinates y and $y' = M\ (y'-y)$ where M the subtangent equals

$$\frac{0{\cdot}4343\ \&c.\ (x-x')}{\log y' - \log y}$$

mentally determined, and both in an almost evanescent state (as is the case in the extreme portion of the curve of statical temperature and of the statical curve of cooling), the quotient may be sensibly in error. To this I add, that in Case I. the length of the bar was certainly not quite sufficient to allow the conducted heat to be entirely spent by dissipation. Consequently there is, as it were, a slight *congestion* of heat towards the extremity—very slight indeed, but still sufficient to give to the subtangent there a too large value, and consequently to the decrement of the primary curve of temperature too small a one. Hence the ratio $\frac{F}{\frac{dv}{dx}}$ is somewhat too great, both in consequence of the numerator being too large and the denominator too small. But how little any such ambiguity can effect the general evaluation of the flux of heat in the succeeding lines of the Table, either in the case of this or of the two succeeding experiments, will be seen by noticing the minuteness of the areas representing the flux which correspond to the extreme portions of the curves. They are so small, that an error amounting to one-half their amount, would hardly affect by $\frac{1}{300}$th or $\frac{1}{400}$th part the measure of the conductivity in the middle and more important part of the Tables.

TABLE IX.—CASE I. $1\frac{1}{4}$-INCH IRON BAR, NAKED. CALCULATION OF AREA OF STATICAL CURVE OF COOLING (F), AND OF THE CONDUCTIVITY AT DIFFERENT TEMPERATURES.

Limits of Abscissæ.		Limits of Ordinates.		M=Sub-tangent.*	Area $M(y'-y)$.	Total Area F.	$-\frac{dv}{dx}$	Conductivity, $\frac{F}{-\frac{dv}{dx}}$	Corresponding Actual Temp. Cent. $(v+13)$.
x.	x'.	y.	y'.						
Ft. Inch.	Ft. Inch.	°	°						
∞	VI. 0	0·	0·008	1·662	0·0133	...	...	...	...
VI. 0	IV. 0	·008	·043	1·189	·0416	0·0549	3·34†	·0164	17
IV. 0	III. 0	·043	·114	1·026	·0728	·1277	8·15†	·0157	22
III. 0	II. 0	·114	·342	·9104	·2075	·3352	24·47	·0137	37
II. 0	I. 6	·342	·620	·8403	·2336	·5688	43·7	·0130	53
I. 6	I. 0	·62	1·245	·7175	·4484	1·0172	85·35	·0119	85
I. 0	0 9	1·245	1·80	·6777	·3762	1·3934	123·0	·0113	110
0 9	,, 7·5	1·80	2·20	·6233	·2493	1·6427	148·8	·0110	127
,, 7·5	,, 6	2·20	2·70	·6100	·3050	1·9477	181·0	·0107	147
,, 6	,, 5	2·70	3·01	·7669	·2378	2·1855	206·8	·0105	163
,, 5	,, 4	3·01	3·32	·8515	·2640	2·4495	237·1	·0103	182
,, 4	,, 3	3·32	3·64	·9047	·2895	2·7390	272·4	·0100	203
,, 3	,, 2	3·64	3·97	·960	·3168	3·0558‡	313·7	·0097‡	228
,, 2	,, 1	3·97	4·32	·986	·3451	3·4009‡	362·5	·0093‡	256
,, 1	,, 0	4·32	4·71	·965	·3764	3·7773‡	420·0	·0090‡	288
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)

* From the formula $0{\cdot}4343 \times \frac{x - x'}{\log y' - \log y}$

† From curve; the rest from equation. ‡ More or less uncertain.

TABLE X.—CASE II. $1\frac{1}{4}$-INCH IRON BAR, COVERED WITH PAPER. CALCULATION OF AREA OF STATICAL CURVE OF COOLING (F), AND OF THE CONDUCTIVITY AT DIFFERENT TEMPERATURES.

Limits of Abscissæ.		Limits of Ordinates.		M.*	Area M $(y'-y)$.	Total Area F.	$-\frac{dv}{dx}$	Conductivity, $\frac{F.}{-\frac{dv}{dx}}$	Corresponding Actual Temp. Cent. $(v+13)$.
x	x'	y	y'						
Ft. In.	Ft. In.	°							°
∞	IV. ,,	0·	·021	·980	·0206	...	...	...	...
IV. ,,	III. ,,	·021	·070	·8305	·0407	·0613	4·47†	·01372	17
III. ,,	II. ,,	·070	·282	·7177	·1521	·2134	16·3	·01310	26
II. ,,	I. 6	·282	·58	·6935	·2066	·4200	32·5	·01292	37
I. 6	I. ,,	·58	1·28	·6317	·4422	·8622	69·9	·01234	62
I. ,,	,, 9	1·28	1·98	·5728	·4020	1·2642	109·0	·01160	84
,, 9	,, 7·5	1·98	2·48	·5551	·2776	1·5418	137·6	·01120	99
,, 7·5	,, 6	2·48	3·14	·5301	·3499	1·8917	175·2	·01080	119
,, 6	,, 5	3·14	3·73	·4840	·2855	2·1772	206·5	·01054	134
,, 5	,, 4	3·73	4·41	·4978	·3385	2·5157	245	·01027	153
,, 4	,, 3	4·41	5·20	·5055	·3993	2·9150	292	·00998	176
,, 3	,, 2	5·20	6·05	·5500	·4675	3·3825‡	351	·00964‡	202
,, 2	,, 1	6·05	7·06	·5401	·5455	3·9280‡	423	·00929‡	234
,, 1	,, 0	7·06	8·30	·5147	·6383	4·5663‡	512	·00892‡	273
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)

* $M=0{\cdot}4343\,\dfrac{x-x'}{\log y'-\log y}$ ‡ More or less uncertain.

† The values of $\frac{dv}{dx}$ are all from equations, and they all agree satisfactorily with projection.

TABLE XI.—CASE III. 1-INCH IRON BAR, NAKED. CALCULATION OF AREA OF STATICAL CURVE OF COOLING (F), AND OF THE CONDUCTIVITY AT DIFFERENT TEMPERATURES.

x	x'	y	y'	M	Area M $(y'-y)$.	Total Area F.	$-\frac{dv}{dx}$	Conductivity, $\frac{F.}{-\frac{dv}{dx}}$	Actual Temp. Cent. $(v+11)$.
Ft. In.	Ft. In.	°							°
∞	IV. ,,	0·	0·0185	·820	·0152	...	...	...	...
IV. ,,	III. ,,	·0185	·070	·7515	·0387	·0539	5·52	·00977	16
III. ,,	II. ,,	·070	·258	·7667	·1441	·1980	19·64	·01008	27
II. ,,	I. 6	·258	·55	·6605	·1928	·3908	38·0	·01029	41
I. 6	I. ,,	·55	1·185	·6515	·4137	·8045	81·6	·00986	68
I. ,,	,, 9	1·185	1·85	·5612	·3733	1·1778	124·1	·00949	94
,, 9	,, 7·5	1·85	2·33	·5419	·2600	1·4378	154·6	·00930	111
,, 7·5	,, 6	2·33	2·93	·5456	·3274	1·7652	193·6	·00912	132
,, 6	,, 5	2·93	3·44	·5192	·2648	2·0300	226·0	·00898	149
,, 5	,, 4	3·44	4·00	·5526	·3094	2·3394	264·4	·00885	170
,, 4	,, 3	4·00	4·54	·6580	·3553	2·6947	310·5	·00868	193
,, 3	,, 2	4·54	5·19	·6229	·4048	3·0995*	366	*·00847	221
,, 2	,, 1	5·19	5·92	·6339	·4627	3·5622*	432	*·00824	254
,, 1	,, 0	5·92	6·75	·6349	·5270	4·0892*	512	*·00799	293
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)

* More or less uncertain.

110. Uncertainty, I have already said, attends the determinations of conductivity for the higher as well as those at the lowest temperatures. In fact, the former are (as will have been seen from the details already given) the results of analogies rather than of direct experiments. The experiments, whether Statical or Dynamical, rarely extended beyond a temperature of 200°, or at most 220° Cent. The results have been here carried out by the analogies afforded by the equations to the curves to nearly 300°. Nevertheless, the continuity of the law of conductivity diminishing with temperature, is consistently brought out by these approximations.

111. In the preceding Tables the conductivity is expressed in terms of the amount of heat as unity, which is required to raise the temperature of one cubic foot of iron, by one degree Cent. It expresses the amount of heat reckoned in such units which would traverse in one minute across an area of one square foot, a plate of iron one foot thick, with the two surfaces maintained at temperatures differing by 1° Cent.

112. If we now project the values of the conductivity of iron found in the last column but one of the three preceding Tables in terms of the thermometric temperatures (Centigrade) in the last columns, we are enabled to trace easily the connected results of the whole inquiry. (See Plate V. fig. 4.)

113. We find that in each case *the conductivity diminishes as the temperature increases;* and *that*, for the next part, in a progressive manner. The variation with temperature is clearly most rapid at the lower temperatures.

114. The two first series agree very closely in their numerical results, with the exception of certain irregularities in the part of the curve where the temperatures are lowest; which have already been in part accounted for (Arts. 55, 65, 109). These two series belong to one and the same bar, though cooling under very different circumstances, owing to the largely increased radiating power conferred upon it by coating it with paper. And the value of the striking coincidence in the numerical results in Tables I. and II. is enhanced by the consideration, that the numbers expressing the conductivity are obtained by taking the ratios of two different columns (7 and 8), which in the two Tables differ most widely, and the result cannot be even guessed at until the ratio is actually taken.

115. The third series (Table X.) leads to numbers very sensibly differing from the two first series, yet following the same general law, the conductivity decreasing with temperature (excepting at the lowest part of the scale, where we find an anomaly corresponding to that noted in an early part of this paper (Art. 65), showing that the lowest portion of the statical curve has not in this instance been satisfactorily determined). The conductivity in Table X. is smaller throughout than in the two former cases. It is believed that this can be satisfactorily accounted for by the different quality of the iron of which this

bar was made, which came from a different manufactory, and was probably inferior in quality.*

116. Tracing an interpolating curve through the projected observations of Cases I. and II., which run nearly parallel and at no great distance, at temperatures superior to 40° and do not diverge even in the higher and more hypothetical part of the diagram, and doing the same separately for Case III., we obtain the following numbers, purely as results of observation:—In the first column of each division of Table XII., we have the ratio $\frac{F}{-\frac{dv}{dx}}$, which expresses the conductivity in terms of the heat required to raise a cubic foot of iron by one degree Centigrade. In the two following columns, we have the same reduced to the usual standard of conductivity in French and English measures respectively.†

TABLE. XII.

Temp. Cent.	Cases I. and II.			Case III.		
	$k=\frac{F}{-\frac{dv}{dx}}$	Conductivity. Units: Foot, Minute and Cent. Deg.	Conductivity. Units: Centimetre, Minute, Cent. Deg.	$k=\frac{F}{-\frac{dv}{dx}}$	Conductivity. Units: Foot, Minute, Cent. Deg.	Conductivity. Units: Centimetre, Minute, Cent. Deg.
0	·01506	·01337	12·42	·01117	·00992	9·21
25	·01391	·01235	11·48	·01062	·00943	8·79
50	·01288	·01144	10·63	·01014	·00904	8·37
75	·01205	·01070	9·94	·00974	·00865	8·04
100	·01140	·01012	9·40	·00940	·00835	7·76
125	·01088	·00966	8·98	·00916	·00813	7·56
150	·01052	·00934	8·68	·00895	·00795	7·38
175	·01018	·00904	8·39	·00877	·00779	7·23
200	·00987	·00876	8·14	·00860	·00764	7·10
225	·00958	·00851	7·90	·00844	·00749	6·96
250	·00930	·00826	7·67	·00826	·00736	6·84
275	·00902	·00801	7·44	·00815	·00724	6·72

117. The coincidence of the results in the second column with the results of the provisional reduction in the case of the $1\frac{1}{4}$-inch bar, made in 1852, and printed at Art. 33, page 144, of the former part of this paper, is both striking and satisfactory. For it shows, as I there anticipated (Art. 38), that the method is, to a great extent, independent of the ordinary instrumental errors,

* Dr Matthiesson in his Experiments on the Electric Conductivity of Iron (Phil. Trans., 1863), has found nearly equally wide variations in different specimens.

† If the numbers in the first column of each division of the Table be called A, then A × ·888 will express the conductivity in water-measure for the foot, minute, and Cent. degree; and A × 825 gives the numbers in the third column, where the centimetre is substituted for the foot.

and even of the laborious computations which have formed the basis of the present paper.

118. In the preceding Table I have completed the series for lower temperatures, where the observations were less accordant, in the following way:—I have assumed that the most trustworthy part of the observational curves are those between the actual temperatures of 40° or 50° and 150° or 160°, and that within moderate limits, the conductivities (k) may be represented in terms of the temperature (t), by such a formula as

$$k = A + at + bt^2$$

In the case of the $1\frac{1}{4}$-inch bar, I find for these constants

$$A = \cdot 01506 \quad a = - \cdot 0000488 \quad b = + \cdot 000000122$$

From which the conductivities corresponding to 0° and 25° have been interpolated. In the case of the 1-inch bar the constants are—

$$A \cdot 01117 \quad a = - \cdot 0000235 \quad b = + \cdot 000000058.$$

119. I must here observe, however, that the above form of relation between k and t, which has been applied by Dr MATTHIESSEN, in his extensive and important researches on electric conductivity, does not satisfy the form of our conductive curves, Plate V. fig. 4, except through a limited range. I have reason, however, to think, that down to 0° of temperature it may be sufficiently exact. The "percentage decrement" of the conductivity between 0° and 100° is 24·5 for the larger bar of iron, and 15·9 for the smaller one. As in the case of Dr MATTHIESSEN's electrical experiments, the "percentage decrement" diminishes with the conducting power, and in almost exactly the same proportion.* The numerical values in either case are, however, considerably smaller for heat than those obtained by Dr MATTHIESSEN for electricity.

120. With this exception, however, there is an agreement in the character of the metals (so far as is yet known) in conducting heat and electricity. (See Art. 2 of this paper.)

§ VII.—*Concluding Remarks and Suggestions.*

121. In Art. (15) of the first part of this paper, I expressed my desire to afford to future experimenters every aid I possibly could to resume and extend my observations (confined, unfortunately, to only one metal—iron), and to furnish them with such advantages as my experience afforded, as well in methods of observation as of reduction.

122. It was especially with this view that I have spent what may perhaps appear an undue amount of labour on the reduction of the experiments considered

* Phil. Trans. 1863, p. 380.

in the present paper. I do not, however, regard this labour as wasted, for the knowledge thus acquired of the nature of the remarkable curves of which it treats will enable a future observer to attack the question in a far more direct manner, and to obtain, with comparatively little trouble, numerical determinations of the conductivity of the metals under ordinary circumstances, and adapted to most purposes of theory or practice.

123. *Suggestions as to Experiments.*—After mature consideration, I do not think that the experimental methods require almost any modification. The independence of the results of any moderate error in the thermometers seems satisfactorily proved (Arts. 38 and 117); and if the object be merely to ascertain the conductivity and "percentage decrement" for a number of metals, it may easily be done without pushing the observations to the high temperatures used in my experiments, which are always a fertile source of difficulty and error. If, for instance, an extreme temperature of 120° or 140° Cent. only was aimed at, shorter bars might be used; the heat would be more manageable and more quickly attained; the thermometers would be more easily made, more easily used, and subject to far smaller corrections; and the dynamical experiments especially, would be freed from an anxious and troublesome source of error, arising from the irregularity of the primitive distribution of the heat in the cooling bar (Arts. 25, 26, and 90).

124. A more exact knowledge of the form of the statical curve of temperature in any case may be obtained by using sources of heat of progressively lower temperature, as explained in Arts. 27 and 58.

125. It is probable that very good results might be obtained by simply using boiling water as a source of heat at the hottest end of the bar, than which nothing can be more manageable. The duration of the statical experiments could thus be much reduced, and the temperature of the air of the apartment rendered more stable. The difficulties referred to in Arts. 65, 109, as to the determination of very small excesses of temperature next the cool end of the bar might thus be in a great measure removed. Indeed, it would be a worthy object of study, in a theoretical point of view, to determine the form of the Statical and Dynamical curves for those low temperatures more accurately than I have done. I cannot but suspect an anomaly in the conduction of heat when the temperature varies with extreme slowness from point to point, which my observations rather indicate than establish.*

126. Another experimental point of interest for the theory would be to estab-

* I may be allowed to state here generally, that this anomaly would apparently assign a too great conducting power to iron at low temperatures than we can readily admit. [The case of the 1-inch bar might rather lead to an opposite conclusion, but I have less confidence in the observations made on it for very small excesses of temperature] Both the statical curve and the curve of cooling deviate more and more from the logarithmic form as the temperature-excesses diminish.

lish, for a few points of a metallic bar, the difference between the superficial and the internal temperature of the bar in any transverse section. This might be done by thermo-electric methods, such as, I think, were used by the late M. LANGBERG of Christiania in his experiments on the conduction of heat in bars. I made some attempts (which were not unpromising) in a different way, by applying to the surface of the bar small portions of fusible alloys or other substances, liquefying at definite temperatures. There did not appear to be much difficulty by gently sliding these proof-pieces along the bar from the cooler towards the hotter part, of ascertaining with considerable precision the co-ordinate of the superficial point, corresponding to the fusing temperature of the alloy or other substance used. The five following substances, in a descending scale, were found to have tolerably definite fusing-points, and to be sufficiently suitable for the experiment:—Tin; solder (tin 9 parts, lead 5 by weight); fusible metal (consisting of bismuth 2 parts, lead 1 part, and tin 1 part by weight); napthalic acid; and bees-wax. The fusing temperatures of the *three* first were carefully ascertained by direct experiment to be—

Tin,* .	229°·0 Cent.	=444°·2	Fahr.
Solder, .	181°·6 „	=358°·9	„
Fusible metal,	94°·15 „	=201°·4	„

The fusing points of the others were not ascertained by me.

127. The experiments which I made in this manner were entirely tentative and preliminary. The following is a specimen:—Statical experiment; 1851, March 14. $1\frac{1}{4}$-inch bar, naked [see Table I., page 78 of this paper.] "At $1^h\ 40^m$ I tried the following experiment to test the difference of temperature of interior and exterior of bar. Taking small sharp-pointed pieces of tin [and] fusible metal (prepared on purpose, bismuth 2, lead 1, tin 1 by weight), I rubbed them gently on the surface of the iron bar till I found the melting point, keeping them gently in motion so as not to allow the surface to heat beneath them. I fixed these points with very considerable exactness, in the case of the fusible metal (the best observation), to perhaps within $\frac{1}{20}$th inch. I did not find the position sensibly [to] vary on the centre of the top, and on the centre of the side of the bar, nor even towards the angle of the bar (with the fusible metal). These experiments deserve repetition.

"$1^h\ 40^m$ Tin melted when rubbed on the centre of one side of the bar, at . . .	0 ft.	0·65 in.	from origin at the edge of the crucible.
Fusible metal, . .	0	10·45 „	„ „ „ „
Bees-wax, . .	1	4 7 „	„ „ „ „

* The melting point of tin seems to be one of the best determined of the higher temperatures. According to CRICHTON, Senior (of Glasgow), it is 442° Fahr. [T. THOMSON]; KUPFFER, 446°; DANIELL, 441°. On the melting point of lead, see Art. 70.

128. *Suggestions as to Reductions.*—Were any one desirous of pursuing the subject of the theory of conduction into its details, I should be disposed to recommend the employment of BIOT'S formula of 5 constants (used to express the elasticity of steam), instead of ROCHE'S, containing 3 constants, which we have here used, see Art. 66. The method of calculation (which is necessarily laborious), is given in REGNAULT'S large treatise on the Theory of the Steam Engine.* For any merely practical purpose, however, this is not required. An experimenter desiring to compare the conductivity and "percentage decrement" of different metals, may reasonably confine his attention between the useful limits of 20° and 120°, or at most 140° Centigrade. For that interval, ROCHE'S formula will suffice. And the chief use of the formula is to obtain readily and accurately the differential co-efficient $\frac{dv}{dx}$ (see Arts. 71 and 78), on the determination of which the value of the conductivity mainly depends.

129. Though I would not recommend the attempt to proceed by graphical methods *alone*, they are an invaluable help, and also serve as a check to the calculations. Where these are not made throughout in duplicate, the use of curves ensures the detection of any material error of the computer. The check by taking first and second differences should also not be disregarded. The curves of cooling may be treated in a similar way.

130. I believe, however, that very fair results might be rapidly and approximately obtained by graphical methods alone. The curves of Statical Temperature and of Cooling being first projected in the usual way, tangents might be drawn mechanically for ordinates successively differing by 10°. The ordinate divided by the subtangent found would give the numerical values of $\frac{dv}{dx}$ and $\frac{dv}{dt}$. They would no doubt be somewhat irregular from the clumsiness of the graphical process; but being projected in terms of x and v respectively, and equalizing curves drawn through them, fair results would be obtained.† The "statical curve of cooling" is then constructed without any calculation whatever; and for evaluating its area up to any limiting ordinate, it might be sufficient that the curvilinear space it encloses should be defined on writing paper and cut out with scissors: the successive portions being weighed, would represent the flux of heat in known

* I ought perhaps to mention the formula which Professor Rankine has applied with success to express the elasticity of steam at all temperatures (Edin. Phil. Journ. 1849, vol. xlvii. p. 28, and Philos. Mag. 1854, vol. viii. p. 530). It is as follows:—

$$\log P = A - \frac{B}{\tau} - \frac{C}{\tau^2}$$

where P is the elasticity of vapour, and τ the temperature reckoned from an absolute zero (−274° cent). In applying the formula to the temperature of a bar, there can be no natural zero from which the lengths are reckoned along the bar; and therefore the constants, instead of three in number, may be reckoned as four; putting v instead of P in the above formula, and, instead of τ, writing $x+D$, D being some fourth constant. (See article 67.)

† This method was used by me in 1852.

units. I have little doubt that the results would come out within one or two hundredths of those obtained by elaborate calculations.

131. I have only to add, that the greater part of the computations in this paper were executed by Mr ALEXANDER PIRIE of St Andrews. Every part of the projections and graphical interpolations was performed by my own hand; and my thanks are especially due to the Messrs JOHNSTON for the unusual care with which they have been reduced in scale, and transferred to copper, as seen in the Plates.

ST ANDREWS, *April* 1865.

IX.—*Some Observations on the Cuticle in relation to Evaporation.* By John Davy, M.D., F.R.S. London and Edinburgh.

(Read 1st May 1865.)

Though it is generally admitted that the cuticle performs an important part in retarding and regulating evaporation from the surface of the body, yet I am not aware of any inquiry hitherto made to determine the fact with exactness.

On account of the importance of the subject, I have been induced to engage in it. The experiments instituted for the purpose have all been of a very simple kind and easily made. They were on the similar parts of dead animals, detached immediately or very soon after the animals had been killed. From one specimen in each instance, the cuticle with the cutis, or the cuticle alone, was removed; whilst from the other these parts of the integuments were left entire. Each was carefully weighed, and then suspended, exposed to the air, side by side. Day after day, with occasional interruptions, or hour after hour, the weighing was repeated, and the result in the loss sustained was noted down. The experiments were made, when not otherwise mentioned, as just described, in a room in which, except in the height of summer, there was commonly a fire by day, its temperature during the day and night varying from about 50° of Fahr. to 55° and 58°. The animals affording the subjects of the trials were the trout, frog, toad, hare, rabbit, pig, thrush, common fowl, blue tit. Even at the risk of tediousness, I shall give the results of the weighing in some detail,—exactness in such trials being the first thing necessary.

1. The Common Trout (*Salmo fario*). Two trouts were selected of the same size. From one (No. 1) the greater part of the skin was removed, when it weighed 112 grs. The skin of the other (No. 2) was left on entire; it weighed 267 grs. This was on the 20th October.

October	21.	No.	1	had lost	32·0	grs., or	28·5	per cent.
,,	,,	,,	2	,,	31·7	,,	19·0	,,
,,	22.	,,	1	,,	55·3	,,	50·6	,,
,,	,,	,,	2	,,	66·0	,,	39·0	,,
,,	24.	,,	1	,,	80·0	,,	71·4	,,
,,	,,	,,	2	,,	108·0	,,	64·6	,,
,,	27.	,,	1	,,	82·8	,,	74·0	,,
,,	,,	,,	2	,,	120·8	,,	72·0	,,
,,	28.	,,	1	,,	83·0	,,	74·2	,,
,,	,,	,,	2	,,	121·3	,,	72·6	,,
,,	30.	,,	1	,,	83·1	,,	74·28	,,
,,	,,	,,	2	,,	121·8	,,	72·90	,,

Weighed again on the 31st, there was no further loss. Both were dry and rigid, and free from any unpleasant smell.

Another trial was made in the following manner:—a trout, just after it had been taken, October 26th, was divided in the line of the spine. From one moiety (No. 1) the skin was removed, when it weighed 117·7 grs. On the other (No. 2) the skin was left; it weighed 83·7 grs. The head had previously been detached and the fish eviscerated.

October	27.	No.	1	had lost	59·7	grs.,	or	50·7	per cent.
,,	,,	,,	2	,,	43·2	,,		50·4	,,
,,	28.	,,	1	,,	88·7	,,		75·3	,,
,,	,,	,,	2	,,	63·2	,,		75·5	,,
,,	30.	,,	1	,,	90·0	,,		76·4	,,
,,	,,	,,	2	,,	63·6	,,		75·9	,,

On the following day the weight of No. 2 was the same, that of No. 1 was ·1 gr. less; both were dry and rigid.

2. The Frog (*Rana temporaria*). A female on the 7th April was killed by decapitation. After the application of a ligature to each thigh, just at the junction with the pelvis, they were detached. From one (No. 1) the integuments were removed; it weighed 55·9 grs. On the other (No. 2) they were left; it weighed 69·9 grs.

April	8.	No.	1	had lost	17·2	grs.,	or	30·7	per cent.
,,	,,	,,	2	,,	20·4	,,		29·2	,,
,,	9.	,,	1	,,	34·6	,,		61·9	,,
,,	,,	,,	2	,,	47·3	,,		60·5	,,
,,	10.	,,	1	,,	39·0	,,		70·0	,,
,,	,,	,,	2	,,	48·2	,,		69·0	,,
,,	11.	,,	1	,,	39·5	,,		70·6	,,
,,	,,	,,	2	,,	49·6	,,		70·9	,,
,,	12.	,,	1	,,	39·7	,,		71·0	,,
,,	,,	,,	2	,,	50·0	,,		71·5	,,
,,	13.	,,	1	,,	40·0	,,		71·4	,,
,,	,,	,,	2	,,	50·2	,,		71·8	,,

After this they sustained no further loss; on the contrary, the air being damper they gained slightly in weight. In the dry and rigid state to which they were reduced they were put into water. Taken out after three hours and wiped to remove adhering water, No. 1 had gained 10·8 grs., No. 2, 14·8 grs. Immersed again and left in twenty-four hours, each had recovered its original weight.

3. The Toad (*Bufo vulgaris*). A similar trial was made with the lower extremities of a large toad, killed on the 11th July, when in full vigour. The extremity (No. 1), deprived of its integuments, weighed 80 grs.; the other, with the integuments on (No. 2), 82·3 grs.

July 12.	No.	1	had lost	39·0	grs., or	48·7	per cent.	
”	” ”	2	”	37·60	”	45·5	”	
” 13.	”	1	”	51·90	”	64·9	”	
”	” ”	2	”	54·00	”	65·2	”	
” 17.	”	1	”	53·07	”	66·3	”	
”	” ”	2	”	56·85	”	69·0	”	

Neither sustained any further loss from exposure. The thermometer during the time ranged from 65° to 68°.

4. The Rabbit (*Lepus cuniculus*). From a wild one recently killed the skin was removed from the under surface of one ear and left on the upper; it (No. 1) weighed 34·5 grs. Of the other (No. 2) the skin was left on both surfaces; this weighed 43·1 grs. They were placed on a stove, side by side, the under surface of each uppermost; a thermometer close to them was 93°.

In 1 hour 35 minutes	No.	1	had lost	15·5	grs., or	44·9	per cent.	It has become rigid.
” ” ” ” ”	”	2	”	3·4	”	7·8	”	
” 3 ” 35 ”	”	1	”	19·1	”	55·2	”	
” ” ” ” ”	”	2	”	6·0	”	13·9	”	It is rigid only at its edge.
” 10 ” 35 ”	”	1	”	20·9	”	60·5	”	
” ” ” ” ”	”	2	”	8·5	”	19·7	”	
” 24 ” 0 ”	”	1	”	22·3	”	64·6	”	
” ” ” 0 ”	”	2	”	16·2	”	37·6	”	

No. 1 sustained no further loss; No. 2 continued to lose weight, gradually diminishing in flexibility up to 289 hours, when it had lost 28·1 grs., or 65·2 per cent., and had become hard and rigid.

5. The Hare (*L. timidus*). On the 5th November, from a hare recently killed, one ear (No. 1) was immersed in boiling water for a minute, after which, when cold, the integument was easily removed, this from the outer surface only; it weighed 81·8 grs. On the other ear (No. 2) the integuments were left entire; it weighed 109·7 grs. They were placed on paper on a stove, when the temperature was about 100°.

In 20 hours	No.	1	had lost	57·0	grs., or	69·6	per cent.	It had become shrivelled and hard.
” ” ”	”	2	”	7·7	”	7·0	”	
” 24 ”	”	1	”	57·8	”	70·6	”	It sustained no further loss.
” ” ”	”	2	”	37·5	”	34·2	”	
” 48 ”	”	2	”	47·7	”	43·5	”	
” 72 ”	”	2	”	58·9	”	53·7	”	Is still tolerably soft and supple.
” 96 ”	”	2	”	61·5	”	56·	”	Suppleness much impaired.
” 192 ”	”	2	”	67·6	”	61·6	”	It is now hard and little flexible.

It should be mentioned that it was only by day that the temperature was kept up to about 100°.

6. The Pig (*Sus vulgaris*). As soon as killed, March 24th, a ligature was applied to one ear, and the portion included cut off; it (No. 1) weighed 128·5 grs. From the other ear a portion similarly included (No. 2) was cut off; after the application of boiling water and the removal of the cuticle, it weighed 159 grs. They were suspended fully exposed to the air.

March 30.	No. 1	had lost	8·1 grs.,	or	6·6	per cent.	
,, ,,	,, 2	,,	56·0	,,	36·6	,,	
,, 31.	,, 1	,,	18·1	,,	14·0	,,	
,, ,,	,, 2	,,	107·0	,,	67·2	,,	It is shrunk and rigid.
April 3.	,, 1	,,	36·0	,,	28·1	,,	
,, ,,	,, 2	,,	114·3	,,	71·7	,,	It sustained no further loss.
,, 9.	,, 1	,,	61·0	,,	47·4	,,	Its softness and suppleness diminishing.
,, 12.	,, 1	,,	71·6	,,	55·6	,,	Is now rigid.

7. The Barn-door Fowl (*Gallus domesticus*). As soon as killed, the wattles of a cock two years old were cut off, a ligature having been previously applied at the base of each. One (No. 1), from which the cuticle was scraped off, weighed 62·2 grs.; the other (No. 2), on which the cuticle was left, weighed 79·5 grs.

October 21.	No. 1	has lost	25·3 grs.,	or	40·6	per cent.	
,, ,,	,, 2	,,	14·8	,,	18·6	,,	
,, 22.	,, 1	,,	35·6	,,	57·2	,,	It is hard and rigid.
,, ,,	,, 2	,,	26·4	,,	33·2	,,	
,, 24.	,, 1	,,	41·7	,,	67·0	,,	
,, ,,	,, 2	,,	42·5	,,	53·4	,,	
,, 27.	,, 1	,,	44·7	,,	71·8	,,	
,, ,,	,, 2	,,	52·8	,,	66·4	,,	Excepting margin, still retains some flexibility.
,, 30.	,, 1	,,	45·2	,,	72·3	,,	
,, ,,	,, 2	,,	55·6	,,	70·0	,,	Is quite rigid.

8. The trial was repeated on the legs of a fowl on the 18th May, when the temperature of the room was 65°, without a fire. They were separated at both their junctions, viz., femur and tarsus. One (No. 1), stripped of its integuments, weighed 835·5 grs.; the other (No. 2) its integuments on, the skin drawn over each stump and secured by a ligature, weighed 889 grs. This without the feathers, which had been removed.

May 19.	No. 1	has lost	115·5 grs.,	or	13·8	per cent.	
,, ,,	,, 2	,,	8·5	,,	·9	,,	
,, 20.	,, 1	,,	201·5	,,	24·0	,,	
,, ,,	,, 2	,,	14·5	,,	1·6	,,	
,, 21.	,, 1	,,	266·0	,,	31·8	,,	
,, ,,	,, 2	,,	20·0	,,	2·2	,,	
,, 22.	,, 1	,,	320·0	,,	38·3	,,	Is free from any unpleasant smell.
,, ,,	,, 2	,,	25·5	,,	2·8	,,	Is becoming putrid.
June 5.	,, 1	,,	81·5	,,	10·0	,,	Not again weighed, owing to its putrid state.
,, ,,	,, 2	,,	487·0	,,	58·0	,,	
,, 17.	,, 2	,,	522·	,,	62·4	,,	No further loss.

Comparing the results of the one covered with integument with those of the other deprived of it, apart from the vastly greater loss of water by evaporation, the other changes were strikingly contrasted. No. 1, that deprived of integument, excepting its loss of water and its hard, rigid state in consequence, seemed little altered; when moistened it was quite free from any putrid taint, and its muscles exhibited their striated structure with undiminished distinctness. No. 2, on the contrary, that on which the integument was left—that still retaining its toughness and little changed—was found, when an incision was made into the contained muscles, to be undergoing the putrefactive change, denoted by the sickening, putrid smell, the softening of fibre and the loss of striated structure, with the appearance of many crystals, chiefly four-sided prisms, which were pretty readily dissolved in dilute acetic acid.

9. The Martin (*Hirundo urbica*). A young bird, fledged, on the 14th July, was found dead, thrown out of its nest by the female bird, which had been forsaken by her mate, probably killed. The nestling weighed 230 grs. One of its thighs (No. 1), stripped of integuments, weighed 3·6 grs.; the other limb (No. 2), the entire lower extremity with integuments on, weighed 7·6 grs.

July 15.	No. 1	had lost	2·3	grs., or	63·8	per cent.	It had no further loss.
„ „	„ 2	„	1·4	„	18·4	„	
„ 16.	„ 2	„	2·3	„	30·2	„	
„ 19.	„ 2	„	3·6	„	47·3	„	
„ 22.	„ 2	„	3·9	„	51·3	„	
„ 25.	„ 2	„	4·0	„	52·6	„	It sustained no further loss; proportionally it was so much less than that of No. 1, from having less muscle, more bone, &c.

10. The Thrush (*Turdus musicus*). A male was shot on the 15th July. Its leg, deprived of its integuments (No. 1), weighed 32·2 grs.; the other leg, with foot, the integuments left on, but without the feathers (No. 2), weighed 42·1 grs.

July 16.	No. 1	had lost	13·2	grs., or	41·0	per cent.	
„ „	„ 2	„	2·8	„	6·8	„	
„ 17.	„ 1	„	19·2	„	59·6	„	
„ „	„ 2	„	4·6	„	10·9	„	
„ 19.	„ 1	„	20·7	„	64·2	„	
„ „	„ 2	„	12·6	„	30·0	„	
„ 27.	„ 1	„	21·0	„	65·1	„	No further loss.

It was rigid, and the muscles were well preserved; perfectly free from putrid taint. No. 2 was not weighed after the 19th; then examined, it was found full of magots of the flesh-fly, twenty-seven in number, all of about the same size, about ·4 inch in length; their weight was 16·6 grs. The muscles of the leg were entirely devoured; what remained, namely, skin and bone, weighed 12·9 grs. The larvæ were partially distended with putrid matter of muscle, in a semi-fluid state, of a reddish hue, and like chyme in appearance. As seen under the microscope, it was found to consist of extremely minute granules, amongst which were dispersed oil globules of different sizes, and some crystals, mostly prismatic. In these larvæ we have a striking example, it may be remarked, of highly organised beings, structurally consisting of striped and unstriped muscles, of tracheæ, of nerves and various glands, &c., formed in so short a time by assimilation of dead putrid matter.

11. Of another thrush, killed on the 27th July, one thigh and leg (No. 1), deprived of integuments, weighed 50·1 grs.; the other leg and foot, stripped of

feathers, and without the thigh (No. 2), weighed 42·6. The integuments were left on, and to prevent access to the muscles and the deposition of the ova of the flesh-fly, sufficient skin from the thigh was drawn over the stump and secured from ingress by a ligature of fine silk.

July 28.	No.	1 had lost	22·2	grs., or	44·3	per cent.	
„ „	„	2 „	2·7	„	6·0	„	
„ 29.	„	1 „	31·6	„	63·0	„	
„ „	„	2 „	4·5	„	10·0	„	
„ 31.	„	1 „	33·4	„	66·6	„	
„ „	„	2 „	6·75	„	15·8	„	
Aug. 4.	„	1 „	33·55	„	66·9	„	No further loss.
„ „	„	2 „	14·6	„	34·0	„	
„ 7.	„	2 „	21·6	„	50·0	„	
„ 10.	„	2 „	25·2	„	59·1	„	

In both instances, in the dried state to which they were reduced, little change had taken place. Even when examined now, after eight months, the muscles are found to retain their striated structure. In the instance of No. 2, this was probably owing to the pretty rapid drying from the small bulk of the limb.

12. I will mention one example more, a trial made in winter, between January the 4th and March the 14th, in a room the temperature of which seldom exceeded 50°. The Blue Tit (*Parus cœruleus*) was the subject of the experiments. One, deprived of its skin (No. 1), weighed 169 grs.; another (No. 2), deprived merely of its feathers, weighed 122·3 grs. Without giving the details of the weighing at short intervals, it may suffice to state that No. 1 had lost in twenty-six days 105 grs., or 62 per cent.; whilst No. 2 had lost 51·4 grs., or only 37 per cent. The first had become quite rigid and hard, and sustained no further loss; the second continued to lose weight, but so very slowly, that on the 14th March it was not thoroughly desiccated. It had lost 83·1 grs., or 60 per cent.

Whilst the results which have been described sufficiently show the powerful influence of the integuments in moderating evaporation, if we compare those obtained in the experiments on different animals a marked difference is notable. The moderating or retentive power of the integuments of the frog and toad is seen to be lowest, that of the trout next, that of the mammalia higher, and that of birds highest.

In the instance of the common fowl the thigh showed a much more retentive power than that of the wattle; and it can hardly be doubted that were trials made of different parts of any other animal, a variety of moderating influences would be witnessed, according to the degree of thickness of the covering and difference of physical structure.

As the cuticle is considered anorganic, may not the part it performs in relation to the checking of evaporation in the living body be held to be much the same as in the dead body?

In the majority of the experiments described, the cutis was removed with the cuticle. . The results might appear more satisfactory if the cuticle alone had been abstracted—this a difficult matter, so difficult, that I rarely attempted it—but, inasmuch as the cutis does not seem to exercise any limiting power on evaporation, may it not be regarded as inoperative or impassive, and to have no material vitiating effect on the results?

Viewing the function of the cuticle physiologically, must it not be considered as intimately connected with animal heat? Thus, where its retentive, moderating power is lowest, as in the instance of the batrachians, is it not operative in preserving these comparatively cold-blooded animals cool; and *vice versâ* in the instances in which its power is highest, in birds, is it not conducive to the preservation of the elevated temperature for which they are remarkable?

A more important function, it may be inferred, is performed by it, associated with the preceding, namely, of preventing a too rapid loss of water from the system, and especially from the blood, thus preserving this vital fluid of a proper degree of dilution, and the solid parts of a proper degree of moisture and flexibility. In cases of extensive burns, when a large surface of integument has been destroyed, the loss of the aqueous portion of the blood is remarkable. In those cases which have been fatal, the blood has been found by M. Baraduc dark and inspissated, and the viscera surprisingly dry, with an absence of fluid in all the serous cavities. This inquirer, indeed, considers the gravity of burns in proportion to the amount of abstraction of fluid or the drying; and accordingly in the treatment he holds it to be a principle to counteract this as much as possible by keeping the patients many hours in a bath daily, aided by the use of diluents, and by the dressing of the burnt parts with cerate.

In the experiments on the limb of the fowl and of the thrush, as well as in all the others, the results show how much the rapid drying of the parts deprived of their integuments checks and prevents putrefaction at a certain temperature, and *vice versâ*, how a retardation of drying, from the integuments being left on, favours putrefaction. And this it may be inferred, much in the same manner as an atmosphere loaded with moisture promotes the same change.*

* A simple experiment illustrates this. Two portions of recently killed lamb were selected. One (No. 1) weighing 168·5 grs. was suspended by a thread, freely exposed to the air of a room varying in temperature from 60° to 65°, *i.e.*, the day and night temperature. The other (No. 2), weighing 160·1 grs., was suspended hanging free in a small glass receiver, in which was a little water, and was so covered as to allow ingress of air, and yet almost to prevent any evaporation. The results were strongly marked. No. 1 lost weight rapidly, and soon became hard and dry without acquiring any putrid taint. No. 2, on the contrary, softened and actually liquefied, at the same time becoming extremely putrid. In an experiment similarly conducted over water—but the water exhausted of air and in vacuo—the muscle escaped putridity. This, from the 11th July to the 20th August, illustrating in addition, I may remark, the difference as regards tendency to putrefaction between muscle and blood; the latter, as I have shown elsewhere (p. 25 of this volume), undergoing the putrid decomposition, even in vacuo, being impregnated with oxygen.

Why muscle deprived of its integuments should escape putrefaction, most other conditions favouring, is not very obvious to reason. Whether electricity is concerned in any way in the prevention is open to question. Be this as it may, the property is an important one, economically considered, and deserving, I cannot but think, of more attention than it has commonly received. The fact that meat, when cut into thin slices, can by drying be kept in a state fit for food even within the tropics, where putrefaction proceeds at so rapid a rate, is well known. The Boucaniers, we are informed, who, in the beginning of the last century, were such formidable pirates in the West Indies, depended very much for subsistence on meat thus prepared. PÈRE LABAT, in his abridged history of St Domingo, describes this meat, calling it by its popular name, "Viandes boucannés," as excellent; and he details the exact method of preparing it, as obtained from the wild hog, and from cattle run wild in the forests of that island.* Now, considering the qualities of such meat, free from the defects of salted meat, the concentrated nourishment it affords, and that of an agreeable kind, and easily cooked when softened by water, it seems peculiarly fitted for the army and navy in protracted campaigns and in long voyages, and also for the use of travellers in countries where subsistence is precarious. A method very similar to the preceding, I am informed by Sir JOHN RICHARDSON, is employed by the North American Indians, in summer and autumn, for preserving the flesh of deer; they, like the Boucaniers, bring in the aid of smoke, but chiefly for the purpose of protecting the meat from flies.† The dried meat powdered, mixed with lard or marrow, forms pemican, which has been of such inestimable value to arctic explorers.‡ The same Indians are well acquainted with the effect of skinning an animal in retarding its putrefaction. It is a practice of theirs to remove the skin with as little delay as possible, eviscerating their game at the same time.

That thorough desiccation should have the effect of preserving meat from putrefactive change is obviously owing to an arrest of chemical action, the presence of a certain portion of water being essential to such action.§

* Nouveau Voyage aux Isles de l'Amerique, &c. Par R. P. LABAT. Tom. iii. p. 132.

† When dry, even muscle no longer attracts the flesh-fly; it is the moist putrefying flesh which allures it, that being alone suitable to the development of its ova.

‡ A specimen of pemican (for which I was indebted to Sir JOHN RICHARDSON), about twelve months old, of the best quality, I found composed of—

84·88 fat,
11·77 muscular fibre chiefly,
3·35 water.

The fat consisted of oleine or elaine chiefly, and stearine. The muscular fibre, moistened, was found unaltered as to striated structure.

§ In 1852 I put by, merely wrapped in paper, portions of pork, mutton, beef, fowl, common trout, pollack (*Gadus pollachius*). They were left in the drawer of a table, in a room in which, during three-fourths of the year, there was a fire. Examined in December 1864; in all of them, with one exception, the muscular fibre exhibited the original striated structure distinctly. The exception was that of the trout, in the muscular fasciculi of which the striæ were less distinct Pemican, which had been kept two years (a portion of that of which the composition is given in the preceding note), exhibited the striated muscle with perfect distinctness.

The same protection from change is witnessed, as is well known, in vegetables, from the removal of their aqueous portion. And in them, too, the cuticle appears to act a part in many instances similar to that of the animal cuticle in retarding evaporation. I may mention an instance or two in illustration, selecting a tuber, the potato, and a fruit, the apple, these being striking examples:—

On the 22d June, three potatoes of the kidney kind were taken from the ground: one (No. 1) had its skin left on—it weighed 295·2 grs.; another (No. 2) had its outer skin removed—it weighed 181·7 grs.; a third (No. 3) had its outer and very fine inner skin both entirely removed—it weighed 263·2 grs. They were placed on the chimney-piece, where the temperature throughout the year varied inconsiderably, ranging from about 50° to 60°. The three were weighed from time to time; the loss per cent., as found on each weighing, is given in the following table:—

June	24.	No. 1 lost	7·8	per cent.,	No. 2, 46·5,	No. 3,	54·0
,,	27.	,,	8·8	,,	,, 66·4,	,,	70·2
,,	30.	,,	10·4	,,	,, 72·6,	,,	73·3
July	6.	,,	11·4	,,	,, 74·9,	,,	75·8
,,	11.	,,	12·6	,,	,, 75·7,	,,	77·8
,,	22.	,,	13·1	,,	,, 75·8,	,,	78·6
August	5.	,,	13·9	,,	,, 75·9,	,,	79·8*
September	10.	,,	15·8	,,	No further loss.	,,	85·7†
December	1.	,,	21·0‡	,,		,,	86·2
February	2.	,,	27·5§	,,		No further loss.	
March	14.	,,	32·7‖	,,			

In these instances it is seen, that not only is the loss of water, owing to evaporation, retarded by the cuticular covering, but also that the vitality or germinating power of the tuber is destroyed by its removal; and further, that the removal of the outer delicate cuticle has much the same effect in promoting

To what extent these portions of meat and fish might otherwise be altered, and their nutritive quality impaired, is a question I am not prepared to answer. I may mention that white of egg, which, on thorough desiccation at a low temperature, is again for most part soluble, appears, if long kept, to become insoluble, judging from a trial of some put by after desiccation in 1852, and recently examined.

* Now, August 5, No. 1, where least exposed to light, has become greenish; where most exposed, brownish green.

No. 2 has become dark brown, almost black, and has acquired a crescentic form, contracted, without being shrivelled.

No. 3 has become of a light brown, and is much shrunk and shrivelled.

† Now, September 10, on No. 1, three small greenish sprouts have appeared tipped with black; general hue the same. Nos. 2 and 3 of the same colour and appearance as before.

‡ Now, December 1, the sprouting buds of No. 1 have grown a very little, showing a very feeble vitality, and very slow progress. In Nos. 2 and 3 no apparent change.

§ Now, February 2, the bud of No. 1 has grown into a stalk, with terminal greenish leaflets, and three lateral long roots, with delicate spongioles.

‖ Now, March 14, the tuber No. 1 is slightly shrunk; the stem from it is ·9 inch in length, and ·3 inch in diameter where thickest; is of a dark purple, and is surmounted by several small green leaflets; the roots from its side, numbering five, vary in length; the longest is 1·7 inch. Weighed again on the 28th of May, the tuber was only a little more shrunk; it was reduced to 161·5 grs. The growth from it, the stem, leaflets, and other offshoots, had all a healthy appearance.

evaporation as the removal of the whole of the integuments, and is as fatal to life; the colour, too, which the tuber acquires, with change of form from its removal, are circumstances which seem worthy of note.*

On the 26th September, two apples of the same kind, not sweet, were selected; one unpeeled (No. 1), weighed 933·5 grs.; the other peeled (No. 2), weighed 1051 grs. They were suspended by their stalk in a room, the temperature of which varied from about 60° to 55° and 50°, during the time of trial. The results of the weighing from time to time are given in the following table, viz., the loss per cent.:—

October	1.	No. 1 had lost	1·8	per cent.,	No. 2,	13·9
„	11.	„	8·1	„	„	61·5
„	30.	„	14·2	„	„	80·8
November	6.	„	17·7	„	„	82·4†
„	12.	„	18·9	„		
„	20.	„	21·8	„		
„	26.	„	24·2	„		
December	2.	„	26·5	„		
„	14	„	30·1	„		
„	30.	„	33·7	„		

On the 30th December, No. 1 was shrivelled; it retained its colour, a greenish hue, and, cut into, was found free from decay. No. 2 had become very much shrunk, had acquired a brown colour, and a slight degree of sweetness.

I need not dwell further on the remarkable manner in which the dessication of vegetable substances preserves them from chemical change, and in many instances for a long period; but I must express regret that a process so simple, and in other countries, especially France and the United States of America, so much used, is not more employed in Great Britain. By means of it, the families of the labouring class might secure to themselves throughout the year a greater variety of food at a cheap rate; the apple for instance; several vegetables, such as the carrot, potato, &c.—a variety equally recommended by two qualities, which happily are seldom disjoined, those of agreeableness and wholesomeness.‡

* In another experiment, begun on 27th October 1863, the results were much the same, with this difference, that, on the 17th March 1864, the unpeeled potato was removed from the light into a dark cupboard, and covered with a small inverted porcelain jar. There it has vegetated; it has shot out many branches, all but the largest of which are white; it is of a light purple; attached to them are many well-formed tubers. Now, March 15, 1865, the weight of the potato is reduced from what it was at first, viz., 900·5 grs., to 331 grs. It has no terminal leaflets. There are seventeen small tubers connected with it; all are of an oval form, like the parent tuber: the largest is ·4 inch in length. All of them are throwing out shoots, and they are most easily detached.

† Its weight afterwards fluctuated a little, according to the hygroscopic state of the atmosphere.

‡ Sliced apples exposed to the air dry rapidly, as do also sliced potatoes and carrots; and if put up in paper bags in a dry place, they will keep fit for use for a long time. No vegetable that I am acquainted with undergoes change more rapidly than the sweet potato (*Batatas edulis*), yet when sliced and dried, as I have found by experience, it may be kept for years unaltered. I have some thus preserved, which I brought from the West Indies in 1848.

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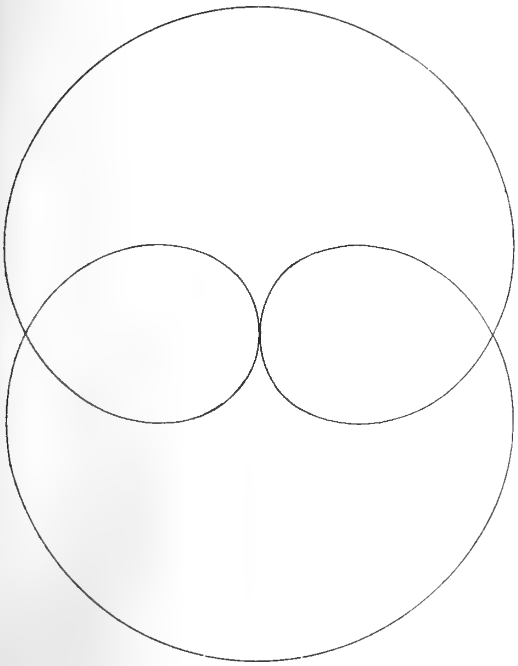
$\alpha : \beta :: 1 : 3$

Fig. 1

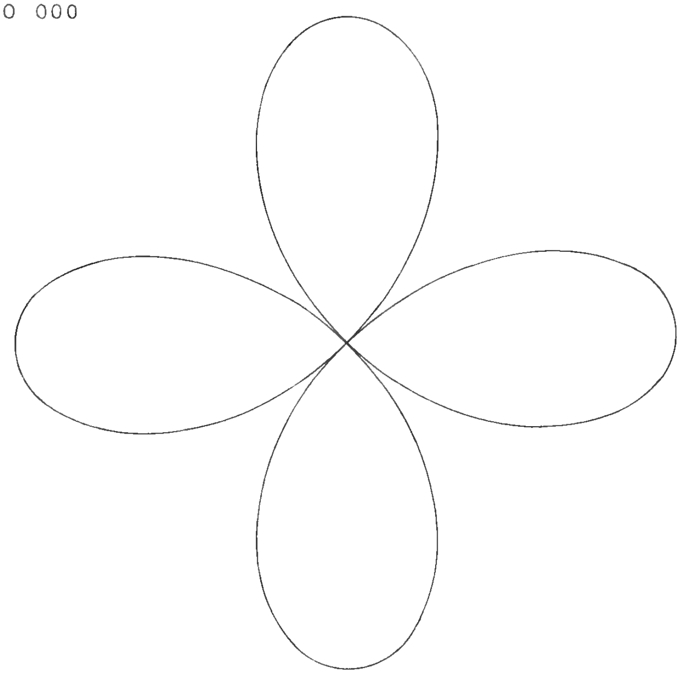


A = .500 000

B = .500 000

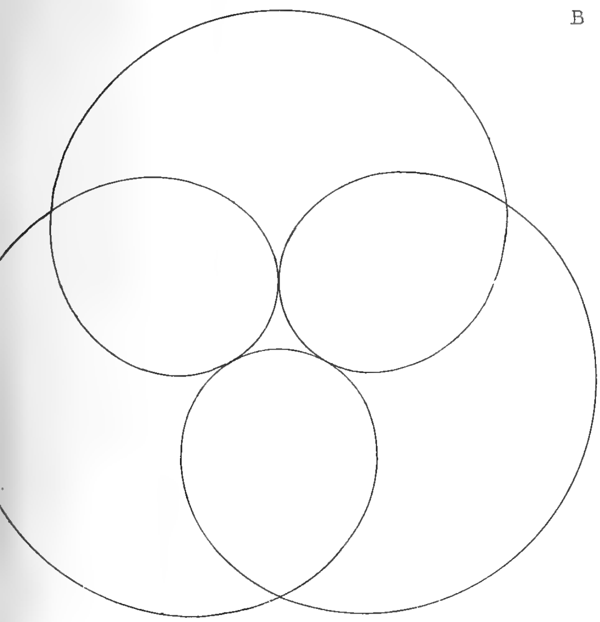
$\alpha : \beta :: 1 : -3$

Fig. 2



$\alpha : \beta :: 1 : 4$

Fig. 3

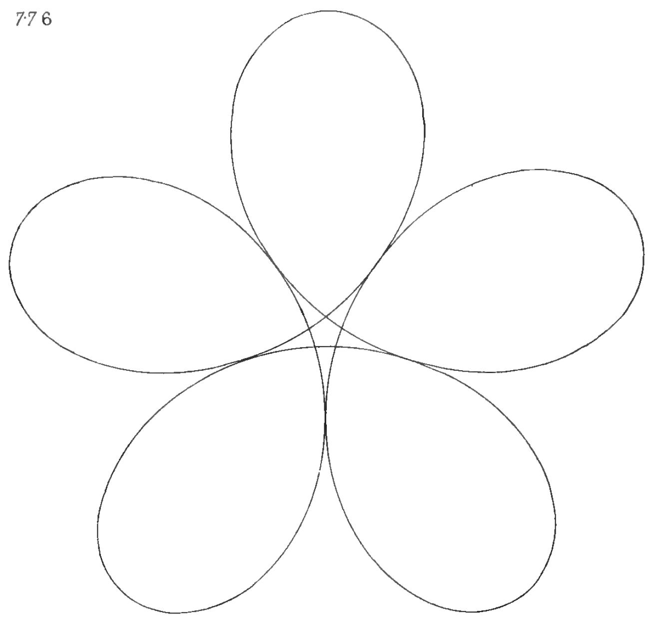


A = .521 224

B = .478 776

$\alpha : \beta :: 1 : -4$

Fig. 4



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$\alpha : \beta :: 1 : 5$

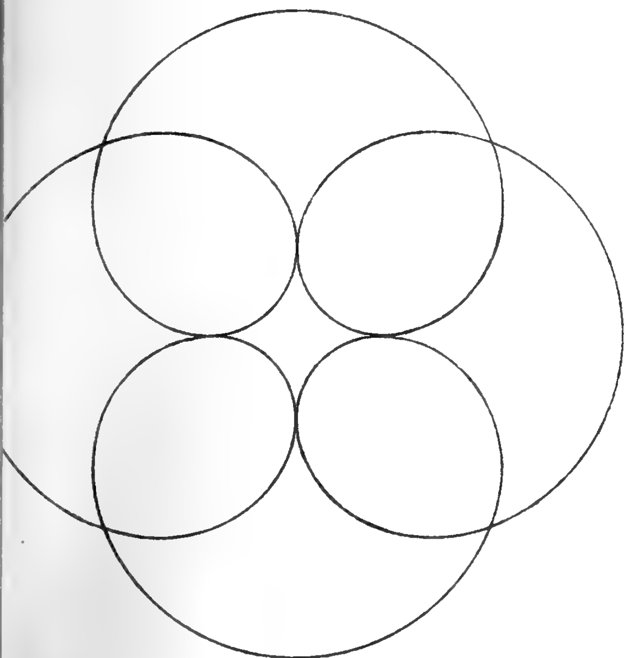
Fig. 5

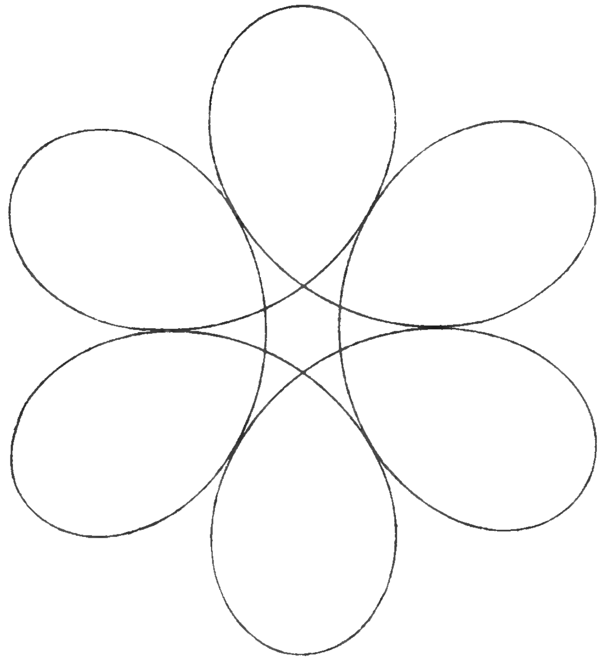
A = .555 556

B = .444 444

$\alpha : \beta :: 1 : -5$

Fig. 6





A = .500000

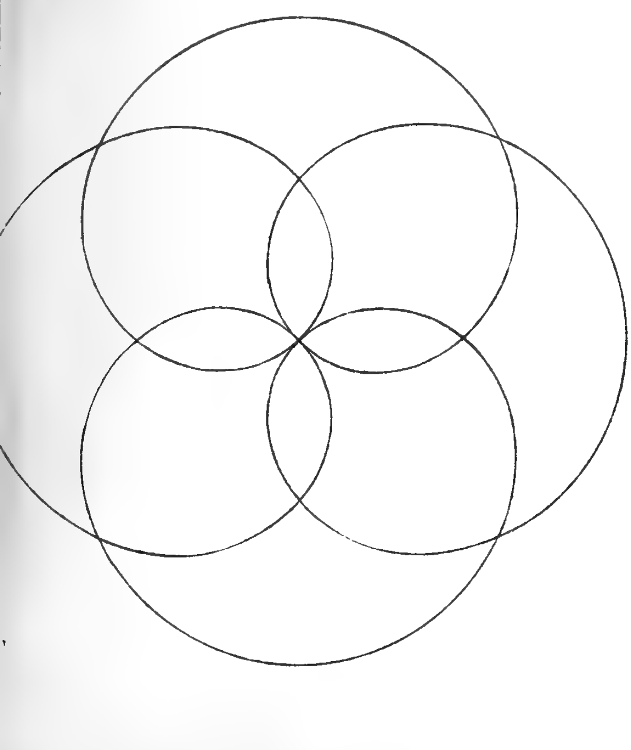
B = .500000

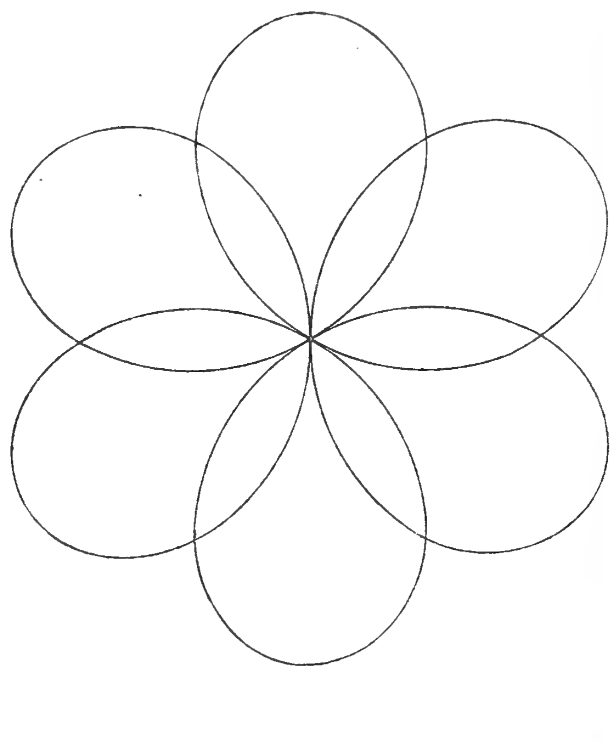
$\alpha : \beta :: 1 : 5$

Fig. 7

$\alpha : \beta :: 1 : -5$

Fig. 8





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$\alpha : \beta :: 1 : 6$

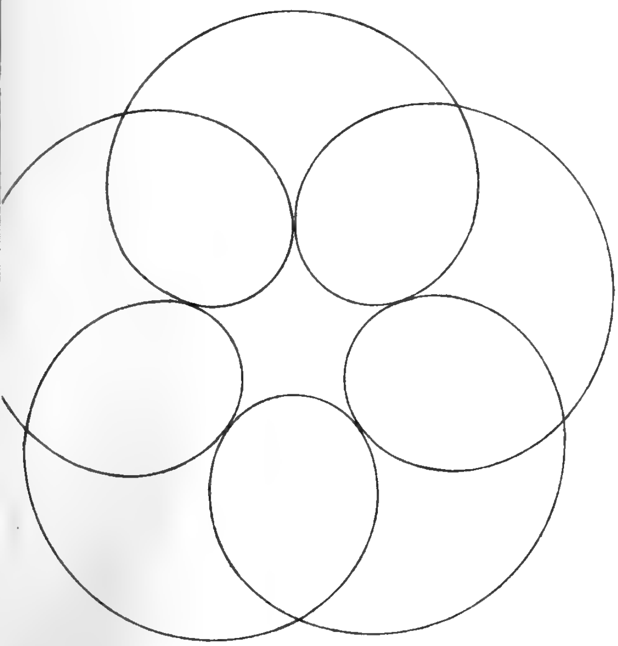
Fig. 9

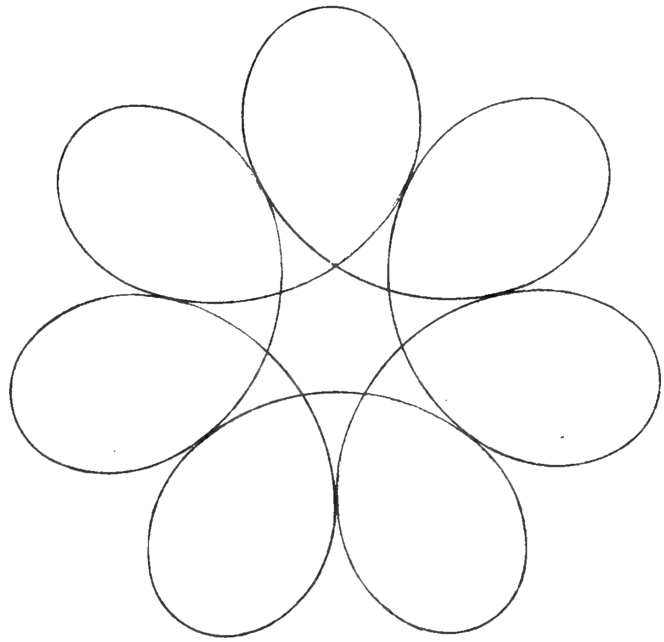
A = .589 335

B = .410 665

$\alpha : \beta :: 1 : -6$

Fig. 10





$\alpha : \beta :: 1 : 6$

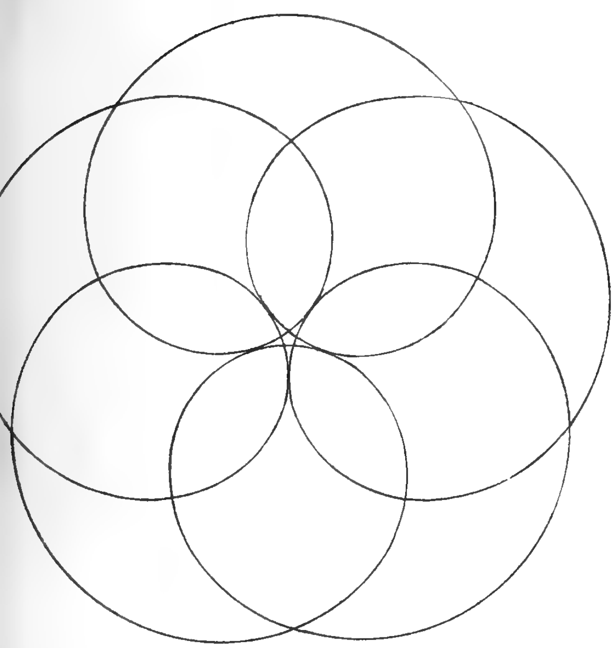
Fig. 11

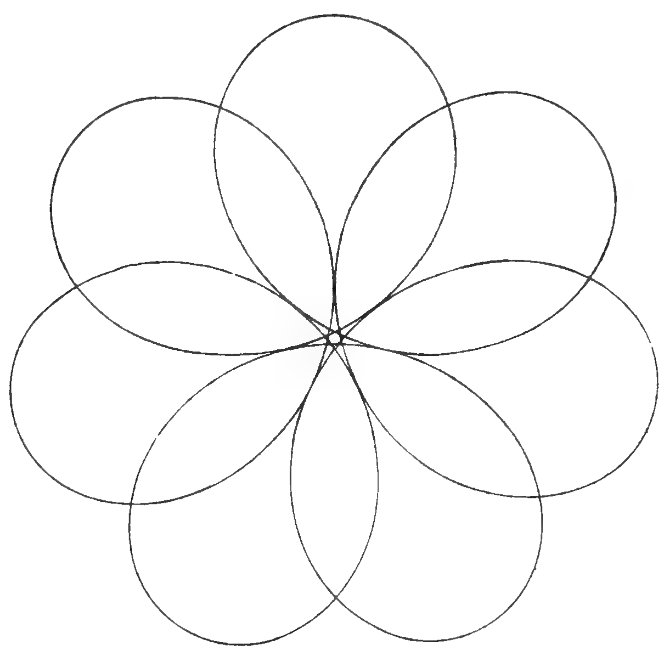
A = .508923

B = .491077.

$\alpha : \beta :: 1 : -6$

Fig. 12





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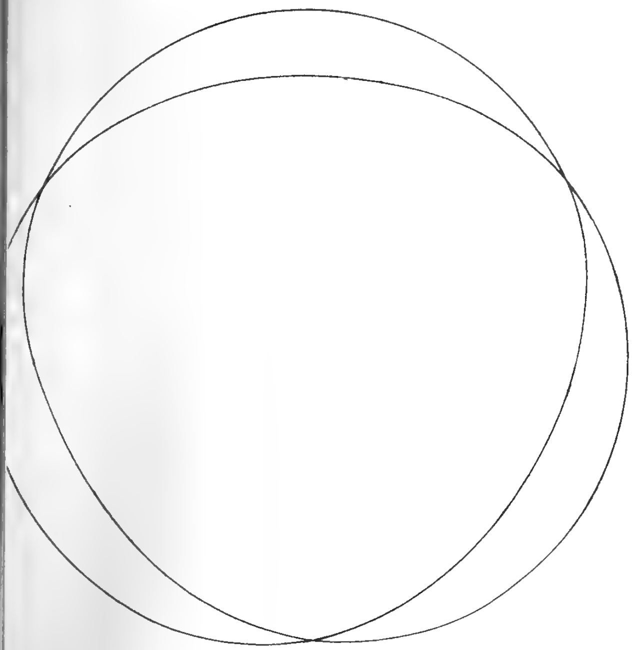
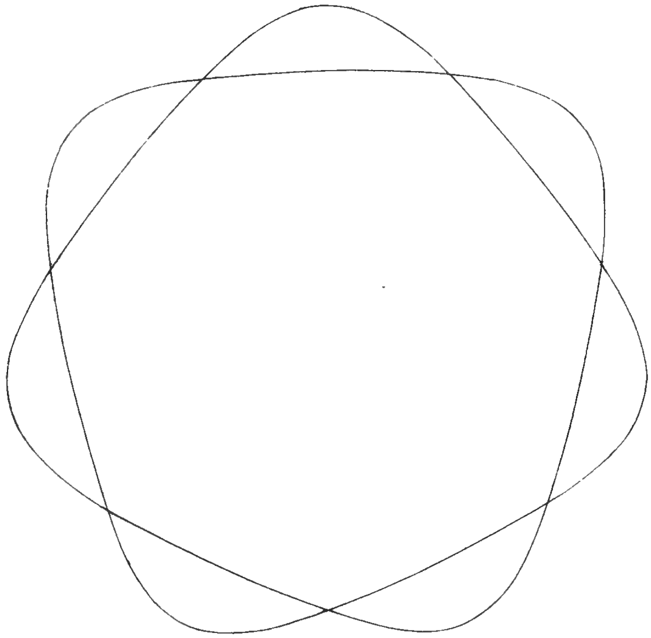
A = .900 000
B = .100 000

$\alpha : \beta :: 2 : 5$
Fig. 13

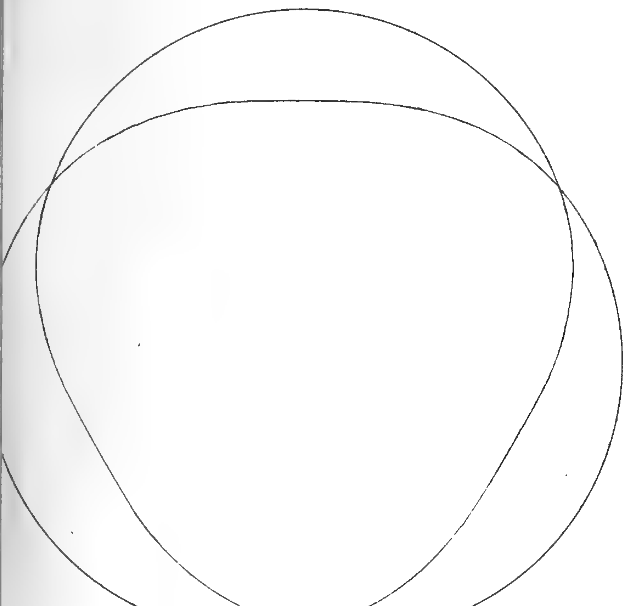


$\alpha : \beta :: 2 : -5$
Fig. 14

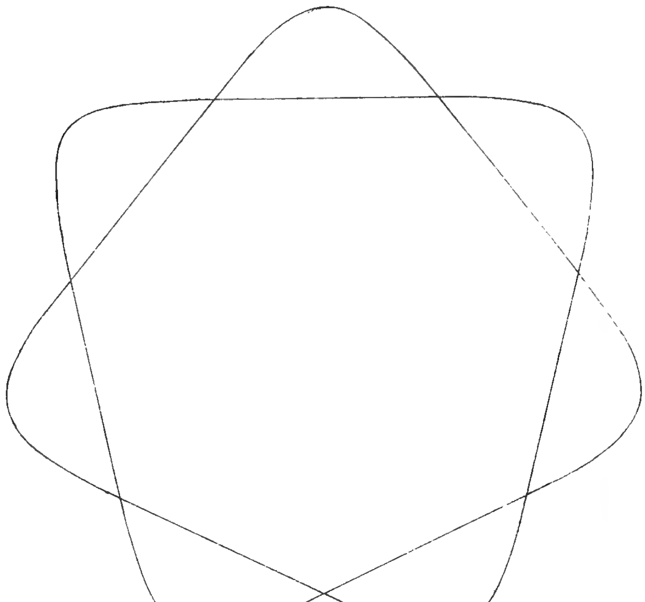


A = .862 069
B = .137 931

$\alpha : \beta :: 2 : 5$
Fig. 15



$\alpha : \beta :: 2 : -5$
Fig. 16



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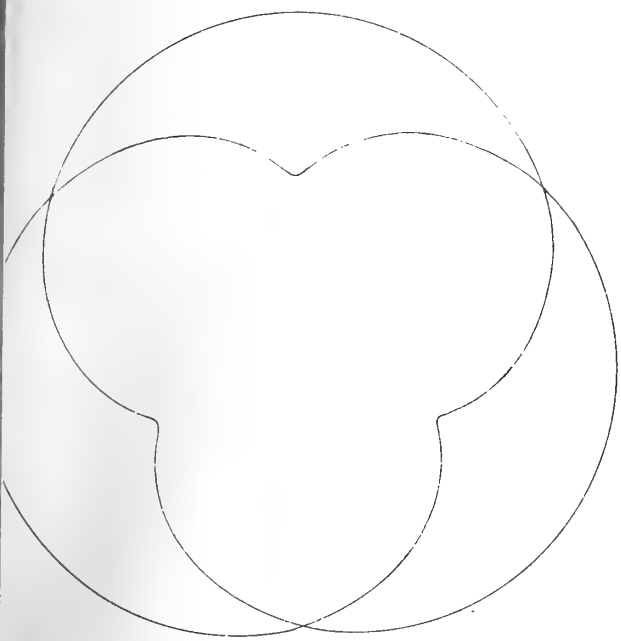
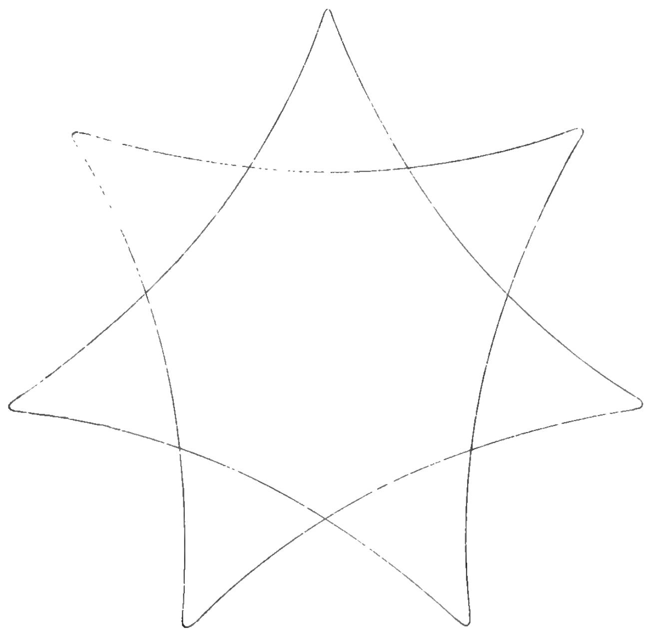
A = .750 000
B = .250 000

$\alpha : \beta :: 2 : 5$
Fig. 17

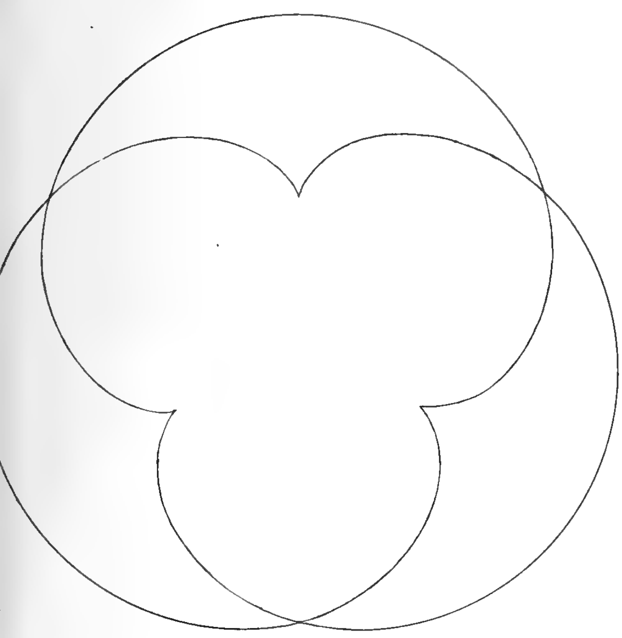


$\alpha : \beta :: 2 : -5$
Fig. 18

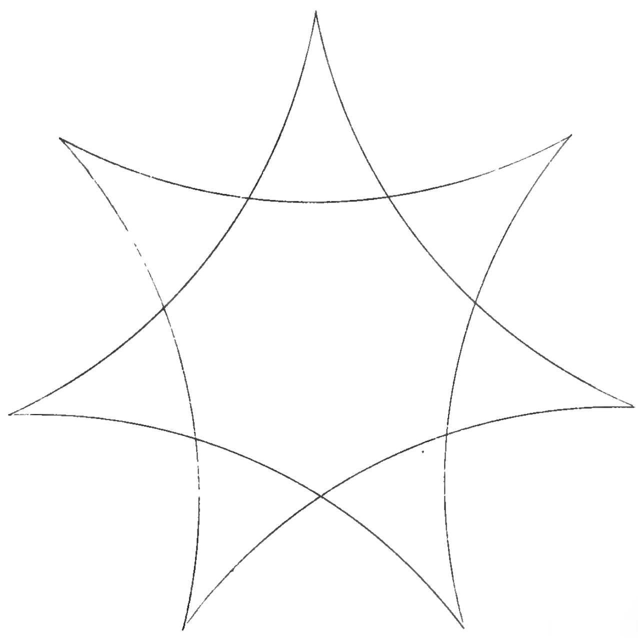


A = .714 286
B = .285 714

$\alpha : \beta :: 2 : 5$
Fig. 19



$\alpha : \beta :: 2 : -5$
Fig. 20



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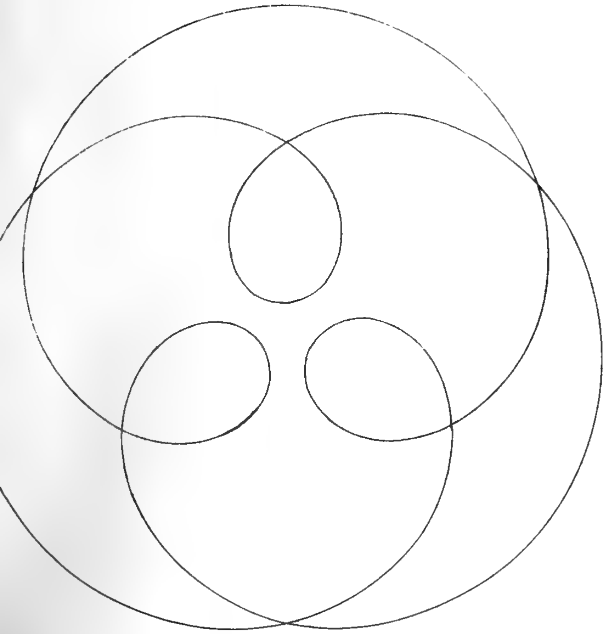
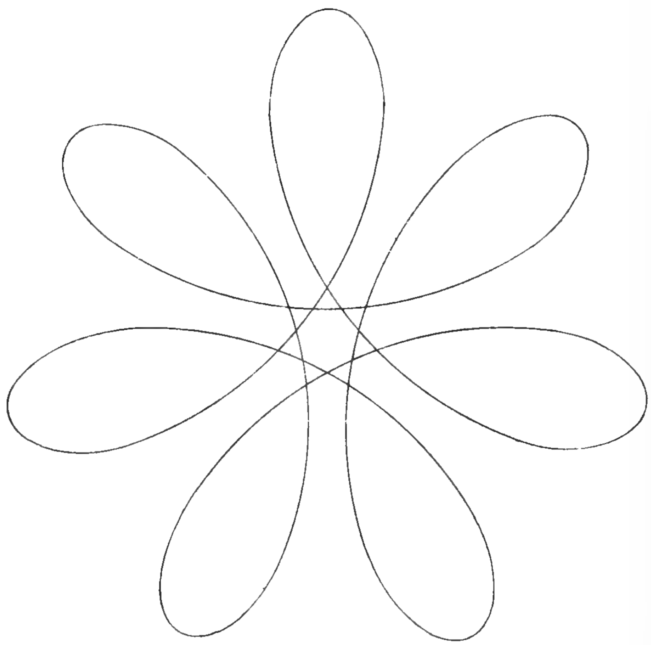
A = . 540 000
B = . 460 000

$\alpha : \beta :: 2 : 5$
Fig. 21

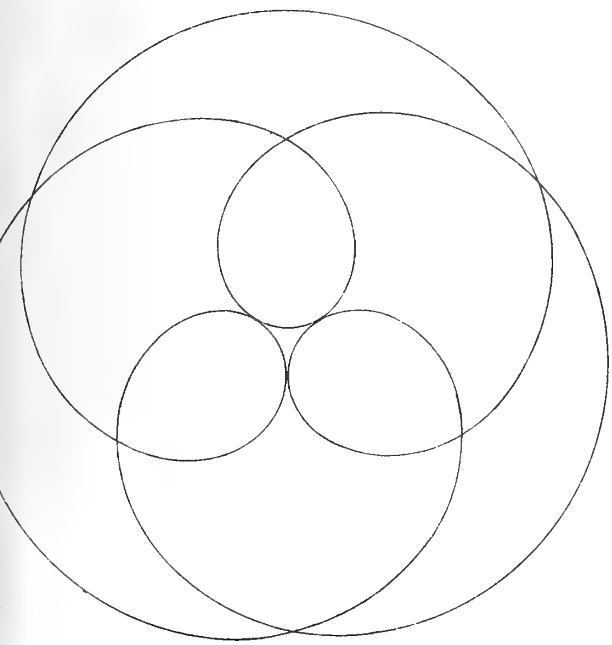


$\alpha : \beta :: 2 : -5$
Fig. 22

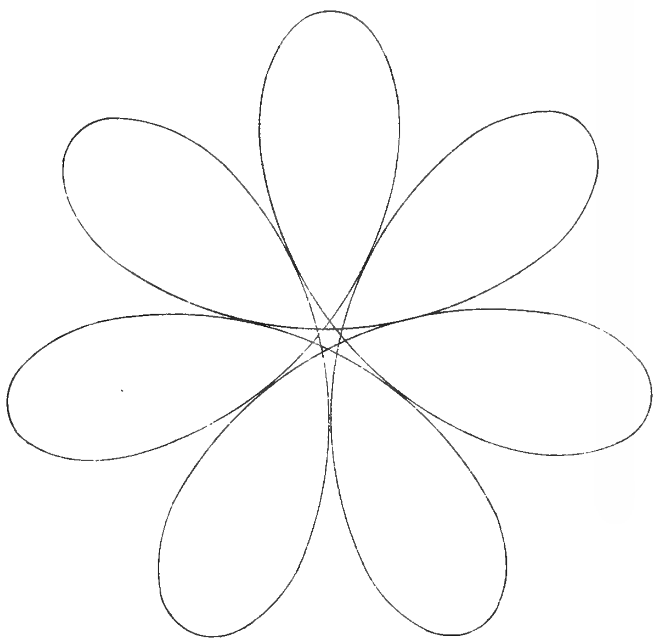


A. = 515 100
B = . 484 900

$\alpha : \beta :: 2 : 5$
Fig. 23



$\alpha : \beta :: 2 : -5$
Fig. 24



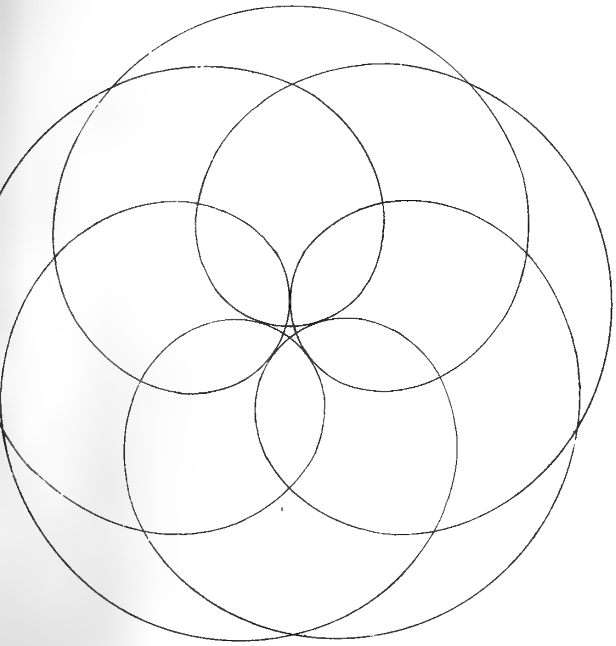
W & A.K. Johnston, Edinburgh

A = .506 929

B = .493 071

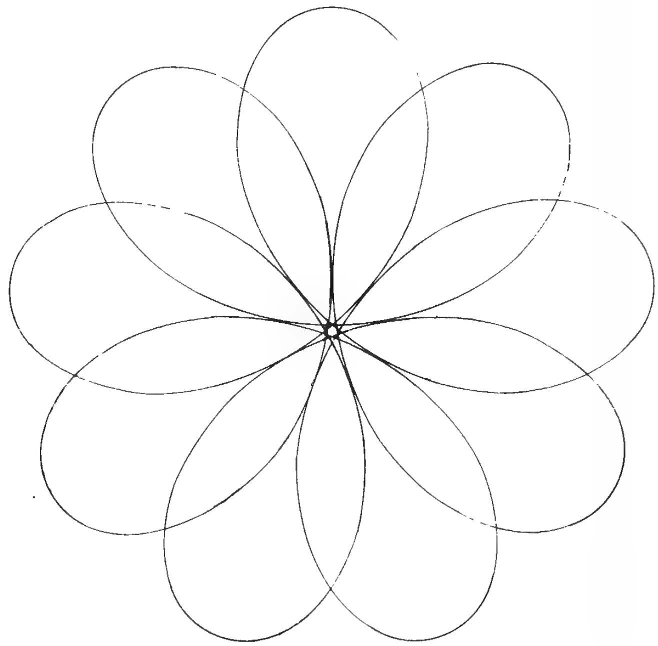
$\alpha : \beta :: 2 : 7$

Fig. 25



$\alpha : \beta :: 2 : -7$

Fig. 26

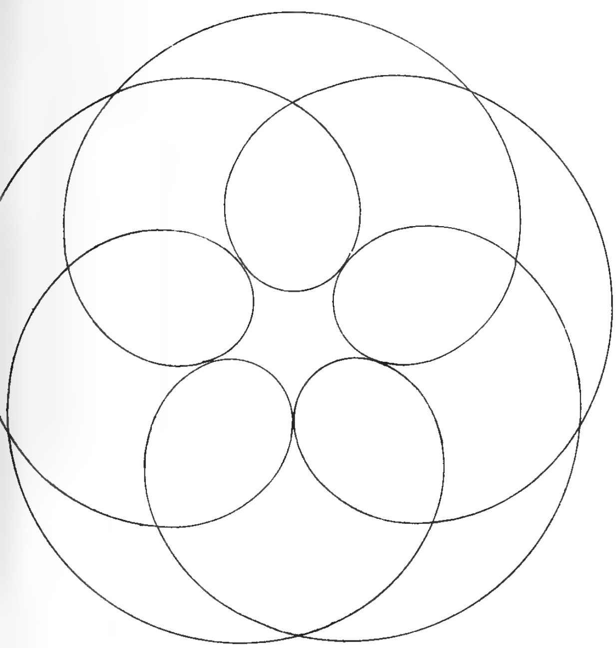


A = .568 535

B = 431 465

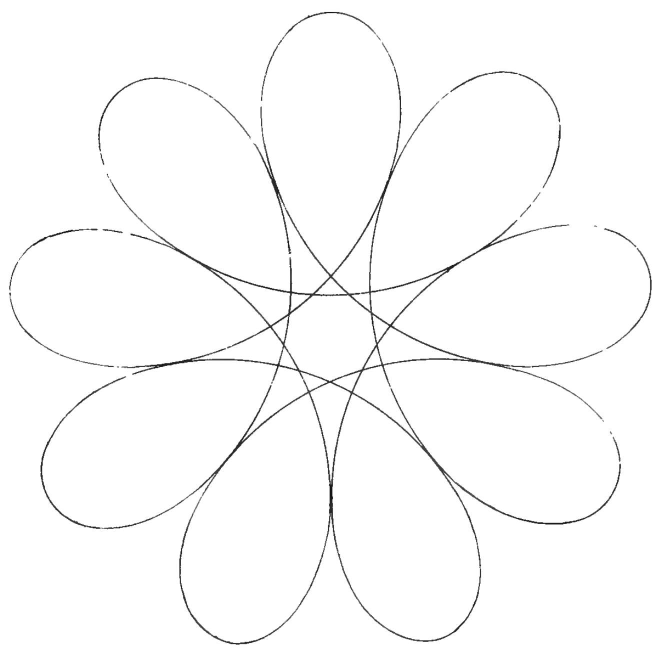
$\alpha : \beta :: 2 : 7$

Fig. 27



$\alpha : \beta :: 2 : -7$

Fig. 28



A.K. Johnston Edinburgh

X.—*On the Contact of the Loops of Epicycloidal Curves.* By EDWARD SANG, Esq. (Plates VI. to XII.)

(Read 3d January 1865.)

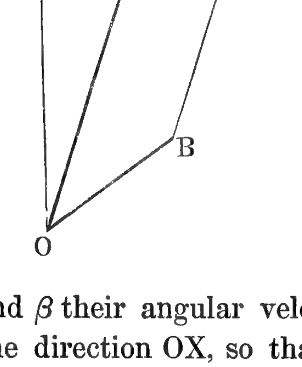
During the summer vacation, Mr HENRY PERIGAL of London proposed to me the following problem :—

"To determine the proportions of an epicycloid of which the loops touch each other."

The solution of this problem contains some points of interest to the general analyst, and exhibits relations between certain trigonometric formulæ and algebraic equations. I, therefore, offer an outline of it to the attention of the Royal Society.

Mr PERIGAL had obtained the solution, in a considerable variety of cases by the method of trial, aided by mechanical appliances, and has exhibited them in his beautiful series of machine-engraved epicycloids.

1. If we suppose two radii OA and OB to turn on a common centre O with uniform velocities, in the manner of the two hands of a watch, and if, at each instant, we complete the parallelogram OAPB, the opposite corner P describes an *epicycloid.* This curve may be obtained by causing the line AP to turn on A as a centre, while A itself describes a circle round O ; or by causing the arm BP to turn on B as a centre, while B moves round the fixed centre O. These are the ordinary arrangements by wheel-work.



There are other arrangements by help of which epicycloids may be produced, but they all result in giving, for the equation of the curve referred to rectangular co-ordinates, the formulæ

$$x = \mathrm{A} \cdot \cos \alpha t + \mathrm{B} \cdot \cos \beta t$$
$$y = \mathrm{A} \cdot \sin \alpha t + \mathrm{B} \cdot \sin \beta t$$

in which A and B represent the length of the arms, α and β their angular velocities, and t the time elapsed since both arms were in the direction OX, so that αt and βt are the angles XOA and XOB respectively.

2. β being supposed to be the greater of the two angular velocities, if the arm OB were minute as compared with OA, the curve described by P would be nearly circular and slightly undulated ; as OB is augmented the waves become deeper, as shown in figs. 13 and 14, and when OB reaches the magnitude determined

by the proportion $\beta^2 : \alpha^2 :: \mathrm{A} : \mathrm{B}$, the curve becomes flat at certain points, that is, the radius of curvature there becomes infinite; this phase is exemplified in figs. 15, 16. When OB increases beyond this value the curve becomes sinuous, its concavity being turned inwards and outwards alternately, as is seen in figs. 17, 18; and when OB becomes so great as to satisfy the condition $\beta : \alpha :: \mathrm{A} : \mathrm{B}$, the curve becomes cusped, and assumes that form to which the name *epicycloid* is sometimes restricted; this form is exemplified in figs. 19, 20. When the arm OB is made still longer the epicycloid is looped, the loops being arranged at regular intervals, as in figs. 21, 22; and if OB be made sufficiently long, the loops come to touch each other, as in figs. 23, 24. Mr PERIGAL'S problem is to determine the conditions under which this contact of the loops takes place. The loop may touch those adjacent to it on either side, or, if OB be made sufficiently long, those separated from it by two, three, or more intervals; so that the problem may have more than one solution.

3. From the very genesis of the curve it follows that the contact of the loops must occur either on the *major* or on the *minor radius-vector;* now the angular motions of the arms may be either in the same or in opposite directions, wherefore there are four cases to be examined.

4. When the arms turn in the same direction, and when the contact is to be on the major radius-vector, we may use the formulæ of Article 1, unchanged; and since, at the instant of contact, the curve must touch the radius-vector OX, we must have both $y=0$, and its derivative $\frac{dy}{dt}=0$; hence, if T denote the time at which the tracing-point is in this position, we must have

$$\begin{aligned} 0 &= \mathrm{A} . \sin \alpha\mathrm{T} + \mathrm{B} . \sin \beta\mathrm{T} \\ 0 &= \alpha\mathrm{A} . \cos \alpha\mathrm{T} + \beta\mathrm{B} . \cos \beta\mathrm{T} \end{aligned}$$

whence we obtain the two proportions

$$\begin{aligned} \alpha : \beta &:: \tan \alpha\mathrm{T} : \tan \beta\mathrm{T} \\ \mathrm{A} : -\mathrm{B} &:: \sin \beta\mathrm{T} : \sin \alpha\mathrm{T} \end{aligned}$$

5. When the arms turn in the same direction, and when the contact is to be on the minor radius-vector, we have to change the sign of B in the preceding formulæ, which are thereby converted into

$$\begin{aligned} 0 &= \mathrm{A} . \sin \alpha\mathrm{T} - \mathrm{B} . \sin \beta\mathrm{T} \\ 0 &= \alpha\mathrm{A} . \sin \alpha\mathrm{T} - \beta\mathrm{B} . \sin \beta\mathrm{T} \end{aligned}$$

whence

$$\begin{aligned} \alpha : \beta &:: \tan \alpha\mathrm{T} : \tan \beta\mathrm{T} \\ \mathrm{A} : \mathrm{B} &:: \sin \beta\mathrm{T} : \sin \alpha\mathrm{T} \end{aligned}$$

6. If the arms turn in opposite directions, we have to change the sign of β in the preceding formulæ. By this we merely reverse the order of the occurrence on the major or on the minor radius, and thus the general condition of the contact of two loops is contained in the proportion

$$\alpha : \beta : : \tan \alpha\mathrm{T} : \tan \beta\mathrm{T};$$

in other words, we have to find two arcs αT and βT in the ratio of α to β, and having their tangents in the same ratio.

7. If we put x for the tangent of the arc T, the above proportion becomes,

$$\beta \frac{\frac{\alpha}{1} x - \frac{\alpha}{1}\frac{\alpha-1}{2}\frac{\alpha-2}{3} x^3 + \&c.}{1 - \frac{\alpha}{1}\frac{\alpha-1}{2} x^2 + \frac{\alpha}{1}\frac{\alpha-1}{2}\frac{\alpha-2}{3}\frac{\alpha-3}{4} x^4 - \&c.}$$

$$= \alpha \frac{\frac{\beta}{1} x - \frac{\beta}{1}\frac{\beta-1}{2}\frac{\beta-2}{3} x^3 + \&c.}{1 - \frac{\beta}{1}\frac{\beta-1}{2} x^2 + \frac{\beta}{1}\frac{\beta-1}{2}\frac{\beta-2}{3}\frac{\beta-3}{4} x^4 - \&c.}$$

On developing and subtracting the common term $\alpha\beta x$ from each side, the result is divisible by x^3, and we obtain an equation into which only the even powers of x enter. When α and β are prime to each other and both odd, the order of the equation, x^2 being regarded as the unknown quantity, is $\frac{1}{2}(\alpha + \beta - 4)$; and when one of them is even the equation is of the order $\frac{1}{2}(\alpha + \beta - 3)$; and it is to be observed that when $\alpha + \beta$ is even there is always the solution $\mathrm{T} = 90°$, $\mathrm{B} = \mathrm{A}$, which corresponds to a contact at the centre of the epicycloid.

8. A table of the values of T and B (A being taken as unit) for all cases up to $\beta = 10$, is subjoined. These values were readily obtained by the process which I published in 1829 (Solution of Algebraic Equations of all orders); the values of x^2 having been taken to ten places of decimals; and those of T and B having been thence computed by the ordinary seven-place tables.

It may be noticed that the only case in which the ratio of A to B can be expressed by integer numbers is that of $\alpha : \beta : : 1 : 5$; this rationality being connected with the fact that 1 and 5 are the only two component parts of the perfect number 6 which express a ratio in its lowest terms. And farther, that the ratio of A to B is the same whether the arms turn in the same or in opposite directions; this fact is exhibited in the accompanying figures.

α	β	A	B	Log B	T		
					°	′	″
1	3	1	1·000 0000	0·000 0000	90	00	00·00
1	4	1	·915 5587	9·963 1069	54	05	41·40
1	5	1	·800 0000	9·903 0900	37	45	40·50
1	5	1	1·000 0000	0·000 0000	90	00	00·00
1	6	1	·696 8284	9·843 1257	43	19	48·35
1	6	1	·964 9338	9·984 4975	74	33	38·71
1	7	1	·613 0718	9·787 5114	37	02	50·10
1	7	1	·898 7860	9·953 6563	63	42	32·40
1	7	1	1·000 0000	0·000 0000	90	00	00·00
1	8	1	·545 4784	9·736 7775	32	21	17·20
1	8	1	·828 5136	9·918 2997	55	38	21·84
1	8	1	·980 4880	9·991 4423	78	34	08·48
1	9	1	·490 4087	9·690 5581	28	43	36·00
1	9	1	·762 6565	9·882 3290	49	23	43·62
1	9	1	·938 9196	9·972 6284	69	44	20·53
1	9	1	1·000 0000	0·000 0000	90	00	00·00
1	10	1	·444 9490	9·648 3102	25	48	58·54
1	10	1	·703 5140	9·847 2728	44	25	02·25
1	10	1	·889 8366	9·949 3102	62	42	16·54
1	10	1	·987 5693	9·994 5676	80	53	15·78
2	5	1	·837 3864	9·922 9259	55	47	59·35
2	7	1	·972 6638	9·987 9628	37	59	16·80
2	7	1	·758 9075	9·880 1889	66	24	11·65
2	9	1	·858 4477	9·933 7138	29	07	49·32
2	9	1	·984 0117	9·993 0002	50	15	45·72
2	9	1	·617 9169	9·790 9301	71	52	23·40
3	5	1	1·000 0000	0·000 0000	90	00	00·00
3	7	1	·876 5924	9·942 7977	40	43	39·83
3	7	1	1·000 0000	0·000 0000	90	00	00·00
3	8	1	·977 5973	9·990 1600	34	22	28·47
3	8	1	·799 5282	9·902 8338	76	32	18·95
3	10	1	·986 4601	9·994 0800	26	42	01·12
3	10	1	·699 6930	9·825 8758	47	02	27·70
3	10	1	·879 2330	9·944 1040	80	01	10·72
4	7	1	·962 3744	9·983 3441	62	39	57·36
4	9	1	·828 3048	9·918 1901	32	10	43·41
4	9	1	·981 0198	9·991 6778	70	37	31·13
5	7	1	1·000 0000	0·000 0000	90	00	00·00
5	8	1	·968 0850	9·985 9134	57	44	43·17
5	9	1	·910 5817	9·959 3189	48	02	17·95
5	9	1	1·000 0000	0·000 0000	90	00	00·00
7	9	1	1·000 0000	0·000 0000	90	00	00·00
7	10	1	·975 5140	9·989 2335	61	43	23·76

9. When one of the radii, say OA, becomes indefinitely large the epicycloid merges into the cycloid produced by carrying the centre of a revolving wheel along a straight line; and the extension of PERIGAL's problem leads naturally to this one:—

"To construct a cycloid of which the loops may touch each other."

10. If we suppose the centre of the revolving circle to be carried along the axis Y with a linear velocity v, while the radius B turns with an angular velocity β, the co-ordinates of the tracing-point are

$$x = \mathrm{B} . \cos \beta t ; \qquad y = vt + \mathrm{B} . \sin \beta t$$

and for the point of contact we must have

$$\begin{aligned} 0 &= v\mathrm{T} + \mathrm{B} . \sin \beta \mathrm{T} \\ 0 &= v + \beta \mathrm{B} . \cos \beta \mathrm{T} \end{aligned}$$

wherefore all such points are determined by the solution of the trigonometrical equation

$$\beta \mathrm{T} = \tan \beta \mathrm{T} ;$$

that is to say, we must discover all those arcs which are equal in length to their own tangents.

11. If we put $v = \beta = 1$, we obtain the following solutions for the first ten cases, the first of these being that of the common or cusped cycloid.

T			Log sec T	B
°	′	″		
0	00	00·00	0·000 0000	1·000 00
257	27	12·24	0·663 0732	4·603 34
442	37	27·57	0·891 5209	7·789 70
624	45	36·54	1·039 4093	10·949 88
805	56	00·77	1·149 2717	14·101 71
986	40	35·75	1·237 7832	17·289 54
1167	11	22·88	1·309 5424	20·395 88
1347	33	55·30	1·371 8187	23·540 67
1527	51	08·52	1·426 2642	26·684 82
1708	04	43·65	1·474 6296	29·828 38

XI.—*Researches on Malfatti's Problem.* By H. F. TALBOT, Esq.

(Read 20th March 1865.)

The problem which bears the name of the Italian geometer MALFATTI, by whom it was first proposed and solved, has long attracted the attention and exercised the ingenuity of mathematicians, and has been made the subject of many careful and elaborate researches.

The great attention which has been bestowed upon this problem has arisen partly from its intrinsic difficulty, but chiefly on account of the extreme simplicity of the solution finally obtained by MALFATTI, which seemed to open new views of geometrical research, and gave reason to hope that simple solutions might in like manner be found of many other geometrical problems usually accounted very difficult or insoluble.

The problem of MALFATTI offers another singularity. Although it is a question of elementary geometry which can be solved by a simple and elegant geometrical construction, yet no *geometrical* proof has ever been given, as far as I am aware, of the truth of this construction. It has been established hitherto only by a very elaborate use of algebraic analysis, in the course of which, however indisputable the result may be, all *geometrical* perception of its truth is lost. And yet there can be little doubt, it should seem, that a *geometrical* reason must exist for any simple series of facts belonging to elementary geometry.

The necessity of calling in the aid of analysis can only arise from the true connection of the geometrical principles involved in the problem being imperfectly understood.

I now offer to the Royal Society a purely geometrical solution of the problem; and, for the sake of clearness, I have divided it into several parts, which I have called Lemmas. Some of these are well deserving of attention for their own sake, and irrespective of MALFATTI's problem. When these theorems have been established, their combination affords a lucid proof of the truth of the solutions which mathematicians have hitherto only obtained by the help of analysis.

History of the Problem.

In the year 1803, a distinguished Italian geometer, Signor MALFATTI, proposed the following problem in the Memoirs of the Italian Society of Sciences, vol. x. part 1:—*

* See GERGONNE, vol. i. p 347.

"In a given triangle to inscribe three circles touching each other, and each of them touching two sides of the triangle."

He gave at the same time a remarkably simple geometrical solution which he had discovered, but unaccompanied by any *geometrical* demonstration of its truth. He contented himself with showing that, if we calculate the algebraic values of the three radii which result from the above-mentioned geometrical construction, these three values, when substituted in the analytical equations deduced from the original conditions of the problem, do in fact satisfy them, and are therefore demonstrated to be true. But he gives no indication of any process of reasoning by which he arrived at the knowledge of these values.

In the year 1810, GERGONNE proposed this problem for solution in the "Annales des Mathématiques," vol. i. p. 196, without knowing that it had been previously solved by MALFATTI; and no solution of it being sent to him by his correspondents, he took up the inquiry himself.* He makes the following preliminary statement:—

"Il y a plus de 10 ans que ce difficile problême s'est offert pour la première fois aux rédacteurs de ce recueil, mais bien qu'ils l'aient attaqué un grand nombre de fois ils n'ont pu pendant longtemps parvenir à le resoudre ni même à s'assurer s'il était resoluble par la ligne droite et le cercle. . . . .

"Ils ont cru devoir faire encore de nouvelles tentatives, et plus heureux cette fois que les précédentes ils sont parvenus sinon à trouver une construction du problême, du moins à l'abaisser au premier dégré."

Then follows an analytical investigation, which finally gives an algebraic value for the radius of any one of the circles in terms of known quantities. But this value does not lead to any *simple* geometrical construction, nor is it easy to show that it agrees with that previously found by MALFATTI, which, however, must necessarily be the case.

Having succeeded to this extent, M. GERGONNE did not believe the problem to be susceptible of much further simplification, when he first became acquainted with the previous researches of MALFATTI. Having procured and perused the memoir of that author, he found that it threw no light upon the point of chief interest, viz., the mode of investigation by which a result so unexpectedly simple had been obtained.†

Nothing further appears to have been done till the year 1820, when M. LECHMÜTZ, of Berlin, published a memoir in GERGONNE'S Annales, vol. x. p. 289, in which he succeeded for the first time in solving the problem, by a course of *à priori* reasoning. His investigation is algebraical, and his results coincide with those of MALFATTI. In the year 1826, STEINER, a distinguished geometer of Berlin, threw an entirely new light upon the problem, by giving, in CRELLE'S Annals (vol. i. p.

* See GERGONNE, vol. i. p. 343.

† Ibid. vol. ii. p. 60.

178), a geometrical construction of singular simplicity, but entirely unaccompanied with proof. His solution is as follows:—

Let ABC be the triangle in which it is required to inscribe three circles touching each other, and each of them touching two sides of the triangle. Bisect the angles A, B, C, by three lines AO, BO, CO, which will meet in the point O. In the three triangles AOB, BOC, COA, inscribe three circles, touching the sides of the given triangle in the points D, E, F, which letters will serve to denote those circles respectively. From the point of contact D, draw a line DG, touching the circle E on its inner side. Similarly from the point of contact E, draw a line EG touching the circle D, on its inner side. And let DG, EG, intersect in G. Then we have a trapezium BDEG, and STEINER affirms, *first*, that a circle can always be inscribed in this trapezium; and, *secondly*, that the circle so inscribed will be one of the three required circles.

But no doubt a difficulty will be observed immediately. STEINER directs that the line DG shall be tangent to the circle E; and plainly there is no reason why the circle E should be selected rather than the circle F. But the reply to this is, according to STEINER, that if the line DG touches the circle E, it will also *necessarily* touch the circle F.

But of this most remarkable theorem he gives no demonstration whatever, although there is assuredly no theorem in the whole of geometry which has less claim to be considered as an axiom. Moreover, he affirms that the same line, DG, touches two of the required circles of the problem, at the point where they touch each other. This being admitted, the construction of the problem follows at once, as it is only requisite to describe a circle touching AB, BC, two sides of the given triangle, and also the known line DG, and this circle will be one of the three circles required, the others being found with equal facility.

STEINER'S solution, therefore, would have left nothing more to be wished for, if it had been accompanied with a demonstration. But of such his memoir contains not a single syllable. He says, indeed (page 178), that this solution of a difficult problem shows "the fruitfulness of the preceding theory;" but the critical researches of subsequent inquirers have not failed to discover the singular circumstance, that there is no connection whatever between this solution of MALFATTI'S problem and the theories set forth in the preceding part of STEINER'S memoir.

The great simplicity and elegance of this solution discovered by STEINER rendered a demonstration of it very desirable, which was at length accomplished by ZORNOW of Konigsberg, in CRELLE'S Annals for 1833 (vol. x. p. 300). The demonstration of ZORNOW is remarkably elegant, but it chiefly depends upon some very dexterous algebraic transformations, in the course of which, however, all perception of a geometric proof of the construction necessarily disappears.

In the following year, PLÜCKER of Bonn resumed the subject in CRELLE'S "Annals," xi. p. 121. His memoir, which bears date October 1831, throws a great deal of new light upon the subject. His object was, like that of ZORNOW, to demonstrate the truth of STEINER'S construction, in which attempt he succeeds up to a certain point, by a well-conducted train of geometrical reasoning; but beyond that point he cannot proceed without the help of analysis. In fact, he shows geometrically that there exists a certain point O within any given triangle ABC (see former figure), which possesses the property, that if AO, BO, CO, are joined, and three circles are inscribed in the three triangles AOB, BOC, COA, as in that figure, then the line DG will touch both the circles E, F, and also two of the required circles. It remained to discover what point of the triangle the point O was, and to verify STEINER'S assertion that it was the centre of the inscribed circle, or that the lines AO, BO, CO, respectively bisect the three angles of the given triangle. But of this capital point PLÜCKER was unable to find any geometrical proof. He has recourse, therefore, to a very free and prolix use of trigonometry and algebra, through which I doubt whether any of his readers have had the courage to follow him, but which finally conducts him to the conclusion that STEINER'S assertion is true. It will be observed that PLÜCKER'S memoir, though published subsequently to that of ZORNOW, preceded it in point of date; that of ZORNOW being dated in October 1832. He was, therefore, the first who succeeded in demonstrating STEINER'S theorem. PLÜCKER concludes his memoir with the following remarks upon the mode in which STEINER has treated the question:—*

"The construction which I have given is essentially the same with that proposed by STEINER in vol. i. of this journal, p. 178. There is, however, at that place no indication of a demonstration. The introductory words of the author—'*to show the fruitfulness of the theorems set forth in paragraphs* 1, 2, 3, *by a suitable example, we add the geometrical solution, and also a greater generalisation of Malfatti's problem, omitting the proof*,'—might cause a person who (as I must confess to be my own case) has no idea how the construction of that problem can depend upon the well-known theorems explained in the above quoted paragraphs concerning points of similitude, &c., &c., to think that the given construction is not proved."†

I have said that PLÜCKER'S recourse to a difficult and very prolix analysis in order to justify the assertion that O is the centre of the inscribed circle, is the weak point of his able investigation. He has admitted this himself, for he says (p. 126), "So soon as this theorem is brought into its proper connection there

* Vol. xi. p. 126.

† Die einleitenden Worte des Verfassers; "*Um die Fruchtbarkeit*," &c. &c., könnten demjenigen, der, wie ich von mir bekennen muss, keine Idee davon hat, wie die Construction, &c., &c . . . . . den Gedanken aufdrängen, dass die gegebene Construction nicht bewiesen sei.

can be no doubt that an easier proof will be found of it." And he adds a wish for "a simple geometrical proof."

In the demonstration which I now submit, I shall follow PLÜCKER's geometrical proof in a general way, up to the point where he breaks away from geometry into the regions of analysis, and then give a proof, by geometry alone, of the remaining portion of the investigation.

Lemma 1.—If a circle is inscribed in a triangle, the difference of the sides equals the difference of the segments of the base.

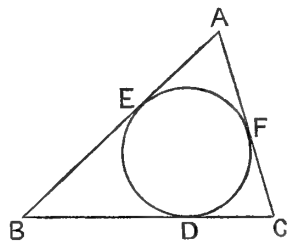


Fig. 1.

This is evident. D, E, F, being the points of contact, AB — AC = BE — CF = BD — DC.

Lemma 2.—Let two circles touch each other at O, and let BFGC be their common external tangent, and AOD their common internal tangent. Let A be any point in the tangent AOD, and let AEB, AHC, be drawn touching the circles; then if a circle be inscribed in the triangle ABC, it will touch the base BC at D.

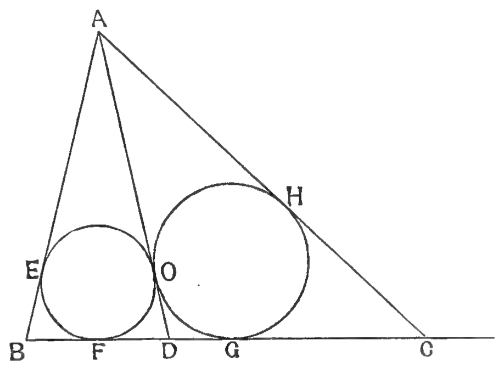


Fig. 2.

Demonstration.—We have manifestly the equal tangents AE = AH, DF = DG, BE = BF, and CG = CH. Therefore AB — AC = BE — HC = BF — GC = BD — DC. Therefore by Lemma 1 the inscribed circle touches the base in D.

Lemma 3.—This is only another case of Lemma 2, when the point A is taken so near to O that the tangents AE, AH, diverge from the base BC, but their prolongations AB, AC, intersect the base at B, C.

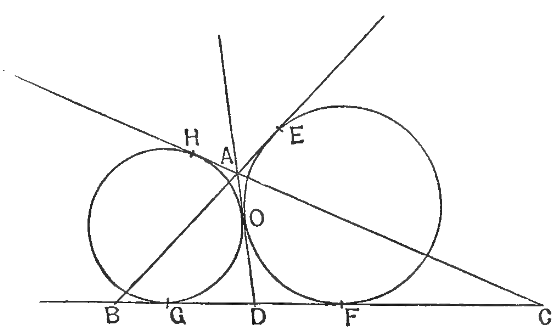


Fig. 3.

In this case also we have the equal tangents AE = AH, DF = DG, BE = BF, and CG = CH. Therefore AB — AC = BE — HC = BF — GC = BD — DC.

Lemma 3 is the case which occurs in the solution of MALFATTI's problem, but as the same demonstration applies to Lemma 2, I have given both of them.

Lemma 4.—If tangents of equal length are drawn to a circle, the locus of their extremities is a circle concentric to the first.

Lemma 5.—Let there be two circles A, B, and let RS and its equal ŔŚ be their two common internal tangents; then if OP, OQ are two tangents drawn from any point O not situated either in RS produced, or in ŔŚ produced, OQ — OP is not equal to RS.

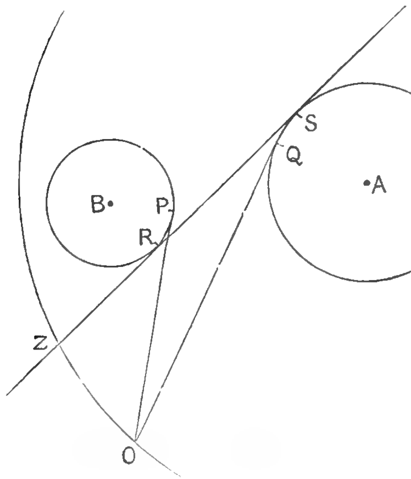


Fig. 4.

Demonstration.—Let the points A, B, be the centres of the given circles. From centre A with radius AO describe a circle cutting RS produced in Z. Then by Lemma 4, OQ = ZS, whence OQ — OP = ZS — OP. But OP is not equal to ZR, because O lies on the circumference ZO, which is not concentric to the circle B. Therefore OQ — OP is not equal to ZS — ZR; therefore it is not equal to RS. *Q. E. D.*

Corollary.—If OQ — OP is equal to RS, O must either lie in RS produced or in ŔŚ produced.

Lemma 6.—Let A, B, C, be three circles. Let DE be the internal common

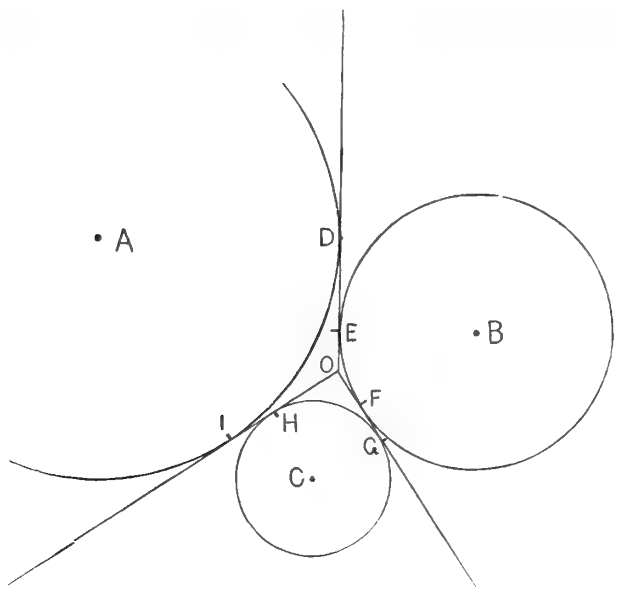


Fig. 5.

tangent of A and B; FG of B and C; and HI of C and A. Then if these three tangents, when produced, meet in a single point O,

$$DE = FG + IH$$

or the greatest common tangent equals the sum of the two others.

For,

$$IH = OI - OH = OD - OG = DE - FG.$$

Lemma 7.—Let A, B, C, be three circles. Let DE be the external tangent of A and B. Let FG be the external tangent of A and C; and let HI be the internal tangent of B and C. Then if these three tangents, when produced, meet in a single point O,

$$\mathrm{FG} - \mathrm{DE} = \mathrm{HI}$$

For,

$$\mathrm{FG} - \mathrm{DE} = \mathrm{OG} - \mathrm{OE} = \mathrm{OI} - \mathrm{OH} = \mathrm{HI}.$$

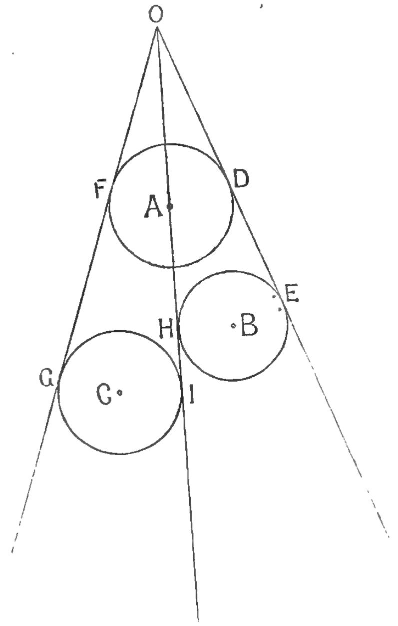


Fig. 6.

Lemma 8.—If three internal common tangents of three circles meet in a point O, their other three internal common tangents meet in another point P.

Demonstration.—In figure 5, suppose the common tangents meeting in O to be effaced, and replaced by the three other internal common tangents, it is required to show that these also meet in a point.

Let two of them, viz. those touching the circle A, meet in the point P. Denoting the new tangents by the same letters as before, but accentuated, we have of course

$$\mathrm{DE} = \acute{\mathrm{D}}\acute{\mathrm{E}}, \ \mathrm{FG} = \acute{\mathrm{F}}\acute{\mathrm{G}}, \ \mathrm{HI} = \acute{\mathrm{H}}\acute{\mathrm{I}}$$

Now we proved in Lemma 6 that

$$\mathrm{DE} = \mathrm{FG} + \mathrm{HI}, \text{ or } \mathrm{ED} - \mathrm{HI} = \mathrm{FG}$$

therefore we have

$$\acute{\mathrm{E}}\acute{\mathrm{D}} - \acute{\mathrm{H}}\acute{\mathrm{I}} = \acute{\mathrm{F}}\acute{\mathrm{G}}$$

The new common tangents meeting in P will be P$\acute{\mathrm{E}}\acute{\mathrm{D}}$, P$\acute{\mathrm{H}}\acute{\mathrm{I}}$; but these have a part which is equal in each, namely PE′ = PH′, which are tangents to the same circle A. Therefore subtracting this part, we have

$$\mathrm{P}\acute{\mathrm{E}}\acute{\mathrm{D}} - \mathrm{P}\acute{\mathrm{H}}\acute{\mathrm{I}} = \acute{\mathrm{E}}\acute{\mathrm{D}} - \acute{\mathrm{H}}\acute{\mathrm{I}}$$

which we proved to be equal to $\acute{\mathrm{F}}\acute{\mathrm{G}}$.

Therefore P is a point from which tangents PD′, PI′ have been drawn to the circles B and C, and their difference has been found equal to $\acute{\mathrm{F}}\acute{\mathrm{G}}$ the internal common tangent of B and C. Therefore by the corollary to Lemma 5, P is a point in $\acute{\mathrm{F}}\acute{\mathrm{G}}$ produced. *Q. E. D.*

Lemma 9.—If two external and one internal common tangents of three circles meet in a point O, the three other *corresponding* tangents meet in another point P.

The demonstration of this is the same as the last, employing Lemma 7 instead of Lemma 6. These theorems may be called PLÜCKER'S tangents, from the name of their discoverer (CRELLE'S *Annals*, tom. xi.). It is evident that there are several more cases besides those considered in Lemmas 6, 7, 8, 9; but I omit them, because they are not required for the solution of MALFATTI'S problem. The demonstration of each would be nearly in the same words.

Lemma 10.—If three lines issue from a point A, and contain the angles DAX, EAX, which may be called θ and ϕ; and if two circles, with centres B and C, are

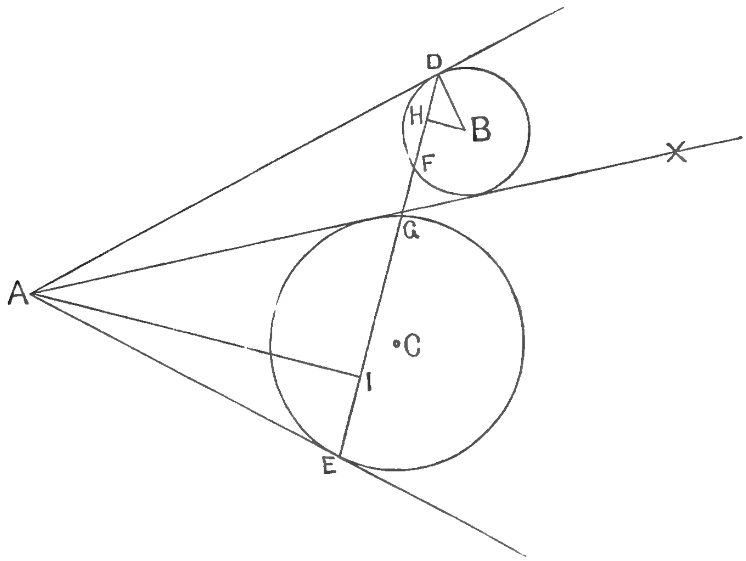


Fig. 7.

inscribed anywhere in these angles, touching the outer sides at D and E; then if DE is joined, the intercepted chords DF, GE are in a contant ratio; namely, in the ratio of tan $\frac{1}{2}\theta$ to tan $\frac{1}{2}\phi$.

Demonstration.—Join BD, and draw the perpendicular BH, dividing the chord DF into two equal parts. Draw AI perpendicular to DE.

Then, since the triangles ADI, BDH are similar,

$$\mathrm{AD} : \mathrm{AI} :: \mathrm{BD} : \mathrm{DH}$$

$$\therefore \ \mathrm{DH} = \mathrm{AI} \, . \, \frac{\mathrm{BD}}{\mathrm{AD}} = \mathrm{AI} \, . \tan \tfrac{1}{2} \theta$$

$$\therefore \ \text{chord } \mathrm{DF} = 2 \, \mathrm{AI} \, . \tan \tfrac{1}{2} \theta$$

By similar reasoning it may be shown that chord GE $= 2\mathrm{AI} \tan \frac{1}{2}\phi$

$$\therefore \ \mathrm{DF} : \mathrm{GE} :: \tan \tfrac{1}{2} \theta : \tan \tfrac{1}{2} \phi$$

Corollary 1.—If $\theta = \phi$, DF = GE. That is:—"If the circles subtend equal angles at A, the intercepted chords are equal."

Corollary 2.—If θ is greater than ϕ, then DF is greater than GE.

Lemma 11.—If any angle DAE is bisected by the line AX, and two circles are inscribed anywhere in the semi-angles DAX, EAX touching the sides at D and E; then the tangent DK is equal to the tangent EL.

Demonstration.—Join DE. We have shown in Lemma 10 that in this case the

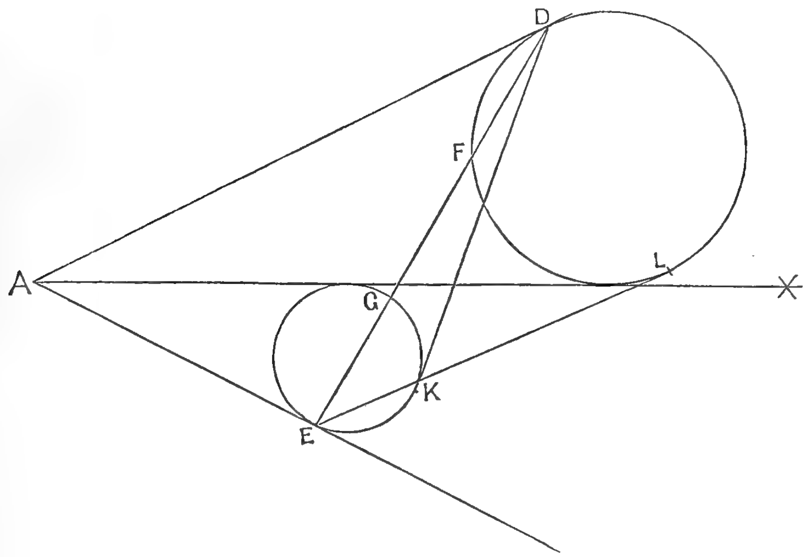


Fig. 8.

intercepted chords DF, GE are equal. Subtract them from the whole line DE, and the remainders DG, EF will be equal. Therefore

$$DG \, . \, DE = EF \, . \, ED \quad \therefore \; DK^2 = EL^2$$

and

$$\therefore \; DK = EL$$

Corollary.—Conversely, if $DK = EL$ it follows that the angle $DAX =$ angle EAX.

For, if those angles are not equal, let DAX be the greater. Then because the angle DAX is greater than the angle EAX, the chord DF is greater than the chord EG (by Lemma 10, corollary 2). Subtract them successively from the line DE, and the remainder EF will be smaller than the remainder DG, therefore EF . ED is less than GE . DE; therefore EL^2 is less than DK^2; and EL is less than DK. But on the contrary $EL = DK$ by hypothesis. Consequently it is not true that the angle DAX is greater than the angle EAX. In the same way it is shown that it is not less; consequently it is equal to EAX. *Q.E.D.*

Lemmas 10 and 11 are particular cases of a much more general theorem which I propose to give on another occasion. In the first nine Lemmas I have chiefly followed PLÜCKER, but have endeavoured to make his demonstrations more rigorous by going more into detail than he has done. But Lemmas 10 and 11, and the more general theorem of which they are particular cases, are original; at least I am not aware of their having been published elsewhere. They supply the link that was missing in PLÜCKER'S investigation, and singularly facilitate the demonstration of STEINER'S elegant construction.

By the help of the preceding Lemmas, we can show the truth of that construction in the following manner:—

Malfatti's Problem.

It is required to inscribe in the triangle ABC three circles touching each other, and each of them touching two sides of the triangle.

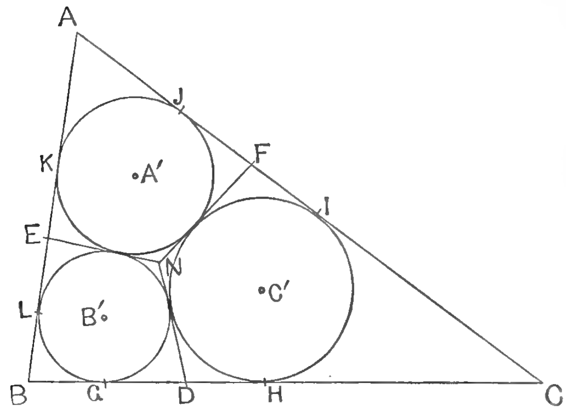


Fig. 9.

Solution.—Suppose the thing done, and the three circles A′, B′, C′, found, it is plain that their three internal common tangents meet in a point N, and bisect the external tangents GH, IJ, KL.

Produce the lines EN, FN beyond the point N until they meet the base BC in two points Q, R. Then if in the triangle QRN so formed, we inscribe a circle which may be* called α, it will touch the base BC in the point D (by Lemma 3).

By an exactly similar process we obtain a circle β, touching DN, EN produced, and the side AC in F; and a circle γ touching DN, FN produced, and the side AB in E.

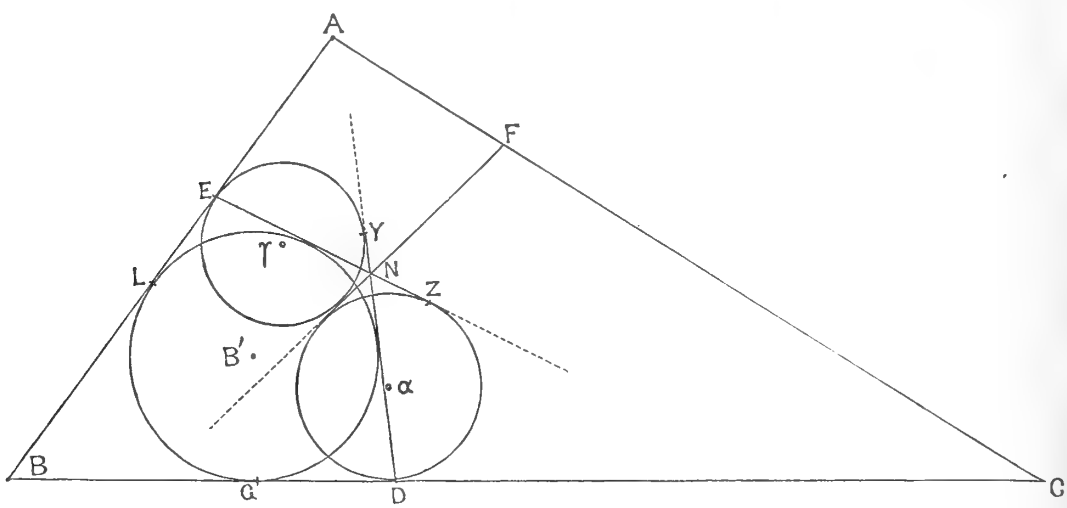


Fig. 10.

* I have called it α, because it stands opposite to the angle A of the original triangle. Similarly for the names of β and γ. I have called α, β, γ the *secondary* circles.

This will be seen better by referring to fig. 10, in which, to avoid confusion, I have only represented one of the required circles B′, and the two secondary circles a, γ, which belong to it. I have represented the prolongations of the three tangents DN, EN, FN by dotted lines. The secondary circles touch these dotted lines, and also touch the sides of the triangle at D and E, where the tangents intersect them. A simple inspection of the figure suffices to show that the tangent DY drawn from D, which touches both the circles B′ and γ, is the sum of two parts, which equal the external tangents DG and EL respectively. And that the tangent EZ, drawn from E, which touches both the circles B′ and a, is the sum of two parts, which equal the external tangents EL and DG respectively. Therefore, these two tangents DY, EZ are equal to each other, since each of them equals DG + EL.

But the three lines DN, EN, FN meet in one point at N; that is, the three internal common tangents of the circles a, β, γ meet in one point. Therefore, by Lemma 8, their *other* three common tangents must also meet in one point, which point may be called O.

Moreover, the line DN produced is the external tangent of the circles B′ and γ: and EN produced is the external tangent of the same circle B′ and circle a; and (as we said before), FN produced is the *internal* tangent of the circles a and γ. But DN, EN, FN concur in a point; that is, two external and one internal common tangents of the circles a, γ, and B′ concur in a point. Therefore, by Lemma 9 the other three *corresponding* tangents of those circles meet in a point. And it is easy to see what that point is. For, the *second* external common tangent of the circles B′ and γ, is AB, one of the sides of the given triangle; and the *second* external common tangent of the circles B′ and a is BC, one of the sides of the triangle.

But we know the point of concourse of AB and BC, to be at B, one of the vertices of the triangle. Consequently, we attain this important result, that the second common internal tangent of the circles a and γ passes through the angular point B of the triangle. In a similar way, it may be shown that the second common internal tangent of the circles a and β passes through the angle C; and that the second common internal tangent of the circles β and γ passes through the angle A.

These three tangents are therefore three lines proceeding from A, B, C, the three angles of the given triangle, and meeting *in a single point* (which a few lines previously we named O). Now STEINER affirms that this point O is the centre of the inscribed circle of the given triangle ABC; and this is the theorem which PLÜCKER and other geometers have been unable to prove except by the use of analytical methods of investigation. But here we call to our assistance the Lemma 11, which we have demonstrated above; and we proceed as follows. Since we have shown that the line BO touches the circle a, and also the circle γ. And since we have also shown that the line DY drawn from the point of contact

D to the circle γ, is equal to the line EZ drawn from the point of contact E to touch the circle a. Therefore by the corollary to Lemma 11, the line BO necessarily bisects the angle B of the original triangle. Similarly it is shown that CO bisects the angle C, and that AO bisects the angle A. Therefore, O is the centre of the inscribed circle of the triangle ABC:—which is STEINER'S Theorem. The solution of MALFATTI'S Problem is therefore as follows:—

Bisect the angles of the triangle ABC, by the lines AO, BO, CO. In two of the smaller triangles thus made AOB, BOC, inscribe the circles γ and a. From D, the point of contact of a with the side BC, draw a line DY, touching the circle γ. Then DY will touch one of the required circles also; which circle also touches AB, BC, two sides of the triangle, and is therefore wholly determined.

XII.—*On the Law of Frequency of Error.* By Professor TAIT.

(Read 3d January 1865.)

1. It has always appeared to me that the difficulties which present themselves in investigations concerning the Frequency of Error, and the deduction of the most probable result from a large number of observations by the *Method of Least Squares* (which is an immediate consequence of the ordinary "Law of Error"), are difficulties of reasoning, or logic, rather than of analysis. Hence I conceive that the elaborate analytical investigations of LAPLACE, POISSON, and others, do not in anywise present the question in its intrinsic simplicity. They seem to me to be necessitated by the unnatural point of view from which their authors have contemplated the question. It is, undoubtedly, a difficult one; but this is a strong reason for abstaining from the use of unnecessarily elaborate analysis, which, however beautiful in itself, does harm when it masks the real nature of the difficulty it is employed to overcome. I believe that, so far at least as mathematics is concerned, the subject ought to be found extremely simple, if we only approach it in a natural manner.

2. It occurred to me lately, while I was writing an elementary article on the Theory of Probabilities, that such a natural process might possibly be obtained by taking as a basis one of the common problems in probabilities, viz.:—*To find the relative probabilities of different combinations of mutually exclusive simple events in the course of a large number of trials.*

3. In fact, this is really the basis of LAPLACE's investigation, an elegant, but very troublesome piece of analysis. With the view, apparently, of attaining the utmost possible generality, he considers an error to be made up of an infinite number of contributions, each from a separate source. But he assumes at starting, that these separate contributions are as likely to be of one magnitude as another, which is, to say the least, questionable; as it seems to be inconsistent with the result finally arrived at. For instance, by far the larger part of the probability of a given finite error is thus made to depend upon a great number of infinite positive contributions, combined with a proper allowance of infinite negative ones. Now, though it is not a harsh assumption to suppose that finite effects should be, in certain cases, the results of additive and subtractive operations with infinite quantities, it does appear unlikely in the extreme, that finite effects should be due to such operations in a far greater measure than to operations with finite quantities. It is true that LAPLACE subsequently shows that the same law will be arrived at by assuming any law of probability for the contributions to the error from each separate cause, provided positive and negative errors of equal amount are equally likely; but it is the complexity, not the sufficiency, of his processes, which I think requires attention.

4. GAUSS' investigation is founded on the assumption, that *the arithmetical mean, of the results deduced from equally trustworthy observations, is the most probable value of the quantity sought.* So far as I can see, ELLIS* has satisfactorily shown that this, however apparently natural, is not justifiable as an *à priori* assumption. In fact, it would seem that we have no right to assume that, because errors of equal magnitude and opposite signs are equally likely, their *sum* will vanish in a large number of trials, any more than that the sum of their third or fifth powers will vanish. Why the first powers should be chosen, appears to arise from the extreme simplicity of the requisite operations; yet, though complexity of calculations is undesirable, it must be submitted to, if necessary for the evolution of truth. The principle of the arithmetical mean has been adopted, among a multitude of others equally likely, just as we might suppose a calculator to insist on gravity varying as the direct distance instead of its inverse square, on the ground that the problem of Three Bodies would then become as simple and its solution as exact, as they are now complicated, and at best only approximate. "*La nature ne s'est pas embarrassée des difficultés d'analyse, elle n'a évité que la complication des moyens*," in the words of FRESNEL.

5. It is with some hesitation that I communicate the present paper to the Society; for I have not devoted much time to the study of the Theory of Probabilities; and I know well how easy it is to fall into the gravest errors of reasoning on such a subject, from the fact that D'ALEMBERT, IVORY, and many others, have published investigations and proofs (sometimes in its most elementary parts), which are now seen to be entirely fallacious.

6. I proceed to show how I think the principle, above (§ 2) enunciated, may be applied. The most direct method would be, of course, to assume any one set of causes of error whatever, and to determine what will, in the long run, be the chance of each separate amount of error as due to their joint action. Supposing this to be determined, let us try to combine the probabilities of error from any indefinite number of sets of possible causes; and, if this process should lead to a definite law of error, such will be the law to which, by an inverse application of the Theory of Probabilities, we should expect each separate observation to be subject. But this process, which is analogous to that of LAPLACE, though not identical with it, cannot easily be carried out, for it essentially involves in its first steps the assumption of a law of error which it is the object of the investigation to determine. We must try a less direct method.

7. We shall, therefore, investigate what must be, in the long run, the chance

* Cambridge Phil. Trans. viii. p. 205.

of any combination whatever of independent events, and *consider the deviation of this combination from the most probable combination as the Error, and the ratio of its probability to that of the most probable combination, as the function which expresses the Law of Error.* If we find, as we proceed, that the law thus arrived at, is (in *form* at least) totally independent of the number, variety, &c., of the several simultaneously acting causes, we shall thus have a very strong argument in favour of the correctness of the process; whose real difficulty, be it remembered, is logical and not mathematical. The mathematical processes to be employed below are, of course, known, and will be found in most treatises on Algebra; but, for the present application, it will be convenient to put them in a form slightly different from the usual one.

8. Taking the simplest case, let us suppose a bag to contain white and black balls, whose numbers are as $p : q$, where $p+q=1$. The chance of drawing α white, and β black, balls in n $(=\alpha+\beta)$ drawings, replacing before each drawing, and disregarding the order in which they appear, is,—

$$\frac{\lfloor n}{\lfloor \alpha \lfloor \beta} p^{\alpha} q^{\beta} \qquad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

This is a maximum, when $\alpha : \beta :: p : q$; which, when n is indefinitely great, can always be exactly attained. This maximum value is,—

$$\frac{\lfloor n}{\lfloor pn \lfloor qn} p^{pn} q^{qn} \qquad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The ratio of these two numbers is,—

$$\frac{\lfloor pn \lfloor qn}{\lfloor \alpha \lfloor \beta} p^{\alpha-pn} q^{\beta-qn} \qquad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Now, according to the principle above assumed, we must treat $\alpha-pn$, the deviation from the most probable result, as measuring the error in some observation, while the expression (3) measures the probability of it, as compared with that of the most probable result. To introduce the ordinary notation, let x be the error, and y the (indefinitely small) probability of that error; then, A and m being constants,—

$$\alpha-pn=mx, \qquad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

while y may be expressed as the product of (3) into A, that is, by (4),

$$y=\mathrm{A}\frac{\lfloor pn \lfloor qn}{\lfloor pn+mx \lfloor qn-mx} p^{mx} q^{-mx} \qquad . \quad . \quad . \quad . \quad (5)$$

When n is a large number, the value of this is easily found from STIRLING'S Theorem, viz.—

$$1.2.3 \ldots . n=\lfloor n=\sqrt{2\pi}\, n^{n+\frac{1}{2}}\epsilon^{-n}\left(1+\frac{1}{12n}+\&c.\right)$$

where the inverse powers of n may be neglected if n is large. For (5) thus becomes,

$$y=\mathrm{A}\frac{(pn)^{pn+\frac{1}{2}+mx}(qn)^{qn+\frac{1}{2}-mx}}{(pn+mx)^{pn+mx+\frac{1}{2}}(qn-mx)^{qn-mx+\frac{1}{2}}}$$

$$=\mathrm{A}\frac{1}{\left(1+\frac{mx}{pn}\right)^{pn+mx+\frac{1}{2}}\left(1-\frac{mx}{qn}\right)^{qn-mx+\frac{1}{2}}}$$

Hence, $\log y-\log \mathrm{A}=-(pn+mx+\frac{1}{2})\log\left(1+\frac{mx}{pn}\right)-(qn-mx+\frac{1}{2})\log\left(1-\frac{mx}{qn}\right)$

$$=-(pn+mx+\tfrac{1}{2})\left\{\frac{mx}{pn}-\frac{m^2x^2}{2p^2n^2}+\frac{m^3x^3}{3p^3n^3}-\&\text{c.}\right\}-(qn-mx+\tfrac{1}{2})\left\{-\frac{mx}{qn}-\frac{m^2x^2}{2q^2n^2}-\frac{m^3x^3}{3q^3n^3}-\&\text{c.}\right\}$$

$$=-\frac{m^2x^2}{2n}\left(\frac{1}{p}+\frac{1}{q}\right)+\frac{m^3x^3}{6n^2}\left(\frac{1}{p^2}-\frac{1}{q^2}\right)-\frac{m^4x^4}{12n^3}\left(\frac{1}{p^3}+\frac{1}{q^3}\right)+\&\text{c.}$$

$$-\frac{mx}{2n}\left(\frac{1}{p}-\frac{1}{q}\right)+\frac{m^2x^2}{4n^2}\left(\frac{1}{p^2}+\frac{1}{q^2}\right)+\&\text{c.}$$

The first term of this expression is finite when mx is of the order $n^{\frac{1}{2}}$; and in this case the other terms in the first line are infinitely small, being of the orders $n^{-\frac{1}{2}}$, n^{-1}, &c. respectively. The latter remark applies to the second line of the expression, which depends upon the $\frac{1}{2}$ in the exponents. When mx is of an order higher than $n^{\frac{1}{2}}$, it is obvious from the undeveloped form that the expression must be infinitely large, and negative. Hence, generally, we may neglect all but the first term, and we have therefore

$$y=\mathrm{A}\epsilon^{-\frac{m^2x^2}{2pqn}}$$

$$=\mathrm{A}\epsilon^{-\mu x^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6),$$

which is the ordinary expression.

9. This shows that, as is well known, the chance of a result differing x from the most probable combination is, in this very simple case, represented by a number proportional to $\epsilon^{-\mu x^2}$ times that of the most probable event. But if we now consider, not *one* but, *any number of* causes conspiring to produce the observed result, we find that the law is still precisely the same in *form*, and this *whether the most probable event be the same as regards each cause or not.* And it is this fact which appears completely to justify the proposed method of regarding the question.

10. For, if the various causes all tend to produce the *same* most probable event, *its* probability will be, by (6),

$$\mathfrak{A}=\mathrm{A}_1\mathrm{A}_2\mathrm{A}_3 \ldots \mathrm{A}_\nu, \quad . \quad . \quad . \quad . \quad . \quad (7)$$

while that of a result, whose error is x, will be

$$y=y_1y_2y_3 \ldots y_\nu=\mathfrak{A}\epsilon^{-(\mu_1+\mu_2+\ldots+\mu_\nu)x^2}=\mathfrak{A}\epsilon^{-\mathrm{M}x^2} \quad . \quad . \quad (8)$$

$$(\text{where } \mathrm{M}=\mu_1+\mu_2+\mu_3+\ldots+\mu_\nu)$$

which is the same form as (6).

If the most probable result, as depending on the several sets of causes, be different for each, the formula (6) becomes, for any one cause,

$$y = A\epsilon^{-\mu(x-\gamma)^2} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (9),$$

where A is the (small) chance of the most probable result, which is, of course, $x=\gamma$.

The chance of any particular value of x, as due to the simultaneous action of all the causes, is now

$$y = A_1 \;.\;.\;.\;.\; A_\nu \epsilon^{-\mu_1(x-\gamma_1)^2 - \;.\;.\;.\;.\; - \mu_\nu(x-\gamma_\nu)^2} \quad . \quad . \qquad (10),$$

which may, of course, be put in the form

$$y = \mathfrak{A}\epsilon^{-M(x-\Gamma)^2} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (11),$$

where the most probable result is now

$$x = \Gamma = \frac{\mu_1\gamma_1 + \mu_2\gamma_2 + \;.\;.\;.\;.\; + \mu_\nu\gamma_\nu}{\mu_1 + \mu_2 + \;.\;.\;.\;.\; + \mu_\nu},$$

while

$$\mathfrak{A} = A_1 \;.\;.\;.\;.\; A_\nu \epsilon^{-(\mu_1\gamma_1^2 + \;.\;.\;.\;.\; + \mu_\nu\gamma_\nu^2) + M\Gamma^2}$$

(where, as before, $M = \mu_1 + \mu_2 + \;.\;.\;.\;.\; + \mu_\nu$)

is its probability.

If we take this as our point of departure for the error x, we must write x for $x - \Gamma$, and we have

$$y = \mathfrak{A}\epsilon^{-Mx^2} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (12),$$

for the form of the law of error, which is precisely that of (6) deduced from the simplest conceivable case.

11. Another remarkable confirmation of the validity of the process suggested above, is to be found in the fact that not only are the *curves* expressed by equations such as (6) and (9) compounded, by multiplication of corresponding ordinates, into another of the same class, whatever be the positions of their axes of symmetry, but that the same principle holds good in three, four, &c., dimensions also.

Thus, any number of hills on the plane of xy, represented by equations such as

$$z = A\epsilon^{-\mu[(x-\alpha)^2 + (y-\beta)^2]} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (13),$$

give, by multiplication of their corresponding ordinates, another hill of the same general form, the *values* only of the constants being changed.

[Many curious geometrical results may be derived from this construction. One of the most singular is the fact that the projection on $x\,y$ of the line of intersection of any two surfaces whose equations are of the form (13) is a *circle*, and that another such surface (viz., that whose ordinates are mean proportionals between those of the former) can be described, passing through the curve of double curvature of which this circle is the projection. But, besides being foreign to our subject, these theorems follow at once from well-known properties of circles.]

12. Returning to equation (12), it is obvious that $\mathfrak{A}$ and M must be connected, since we have to satisfy the condition that the probability that the error lies between infinite positive and negative limits is certainty. Hence, as we may write

$$\mathfrak{A}\epsilon^{-Mx^2}\delta x \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14),$$

for the chance that the error lies between x and $x+\delta x$; we must have

$$\mathfrak{A}\int_{-\infty}^{+\infty}\epsilon^{-Mx^2}dx=1. \quad . \quad . \quad . \quad . \quad . \quad . \quad (15),$$

But we know that

$$\int_{-\infty}^{+\infty}\epsilon^{-y^2}dy=\sqrt{\pi},$$

which reduces (15) at once to the form

$$\mathfrak{A}\sqrt{\frac{\pi}{M}}=1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16),$$

the required relation.

13. It is obvious from (12) that large errors have less probability when M is large; that is when h is small, if we put

$$M=\frac{1}{h^2}.$$

Hence h becomes an indication of the comparative accuracy of the process whose errors we are testing, and it is thus desirable to retain it in the expression for the law of error.

By (16) we have

$$\mathfrak{A}=\frac{1}{h\sqrt{\pi}},$$

and therefore, by (14), we obtain

$$\frac{1}{h\sqrt{\pi}}\epsilon^{-\frac{x^2}{h^2}}\delta x$$

for the chance that the error lies between x and $x+\delta x$, the usual expression.

14. It only remains that we give an idea of the accuracy with which this law of error is approximated to, in cases such as we have assumed as the basis of our reasoning, even in a very small number of trials. For this purpose we take the case of 20 tosses of a coin. Here the most probable result is, of course, 10 heads and 10 tails, and the chances of the various possible combinations are the terms of the expansion of

$$\left(\frac{1}{2}+\frac{1}{2}\right)^{20}.$$

If we erect these as ordinates at successive distances, each equal to unit, along a line, we may graphically represent their relative values by a curve drawn, *liberâ manu,* through their extremities. The area of this curve will evidently approximate to unity, which is the exact value of the sum of the areas of the rectangles of unit breadth, each of which is bisected by one of the ordinates laid down from the expansion.

To find the corresponding curve of error, notice that the maximum ordinate is

$$\frac{20\,.\,19\,\ldots\ldots\,11}{1\,.\;\;2\,\ldots\ldots\,10}\cdot\frac{1}{2^{20}}=\frac{184756}{1048576}=0\cdot1762.$$

Taking this as the value of $\frac{1}{h\sqrt{\pi}}$ we have for (12) the expression

$$y=\frac{1}{5\cdot675}\epsilon^{-\frac{x^2}{10\cdot253}} \quad \ldots \ldots \quad (17).$$

The following table shows a few of the values of y from this formula, compared with the corresponding terms in the binomial: it is sufficient for our purpose, as it would not be worth while to take the trouble of calculating the *areas* of the curve of error corresponding respectively to the rectangles above mentioned.

x.	y from (17).	y from Binomial.	Difference.
0	0·1762	0·1762	0·0000
1	0·1598	0·1602	−0·0004
2	0·1193	0·1201	−0·0008
3	0·0733	0·0739	−0·0006
4	0·0370	0·0369	+0·0001
5	0·0154	0·0148	+0·0006
6	0·0053	0·0046	+0·0007

15. Nothing is better calculated to show the general soundness of the method we have adopted in this paper, than the fact of the excessive closeness of the above approximation: the case having been specially chosen as one in which we could hardly have expected more than a rude resemblance to the law of error.

XIII.—*On the Application of Hamilton's Characteristic Function to Special Cases of Constraint.* By Professor Tait.

(Read 20th March 1865.)

1. One of the grandest steps which has ever been made in Dynamical Science is contained in two papers, "*On a General Method in Dynamics,*" contributed to the Philosophical Transactions for 1834 and 1835 by Sir W. R. Hamilton. It is there shown that the complete solution of any kinetical problem, involving the action of a given conservative system of forces, and constraint depending upon the reaction of smooth guiding curves or surfaces, also given, is reducible to the determination of a single quantity called the *Characteristic Function* of the motion. This quantity is to be found from a partial differential equation of the first order, and second degree; and it has been shown that, from any *complete* integral of this equation, all the circumstances of the motion may be deduced by differentiation. So far as I can discover, this method has not been applied to inverse problems, of the nature of the Brachistochrone for instance, where the object aimed at is essentially the determination of the constraint requisite to produce a given result. It is easy to see, however, that a large class of such questions may be treated successfully by a process perfectly analogous to that of Hamilton; though the characteristic function in such cases is not the same function (of the quantities determining the motion) as that of the Method of *Varying Action.*

2. It is unnecessary to enter into any great detail with reference to the present subject; because any one who is familiar with Hamilton's beautiful investigations will have no difficulty in applying them, with the requisite slight modifications, to the subject of this paper. I shall therefore content myself with a brief explanation of the application of the method to the problem of the Brachistochrone, and a mere indication of some other curious problems which are easily solved in a similar manner.

3. The problem of the Brachistochrone for a single particle is, in its simplest form, as follows:—

Find the form of the (smooth) constraining curve along which a particle will pass, under the action of a given conservative system of forces, from one given point to another in the least possible time, the initial velocity being given.

The problem may easily be complicated by supposing, for instance, the terminal points not to be definitely assigned, but to lie each on a given surface:

still farther, by supposing the initial velocity to depend, according to some given law, upon the coördinates of the initial point, and so forth. But such complications introduce analytical difficulties of the quasi-arithmetical kind merely, not of a physical nature; and we leave them to those who are curious in such matters.

4. In symbols, if τ be the time of passing from x_0, y_0, z_0 to x, y, z, we must have

$$\tau = \int_{x_0, y_0, z_0}^{x, y, z} \frac{ds}{v}$$

a minimum: subject to the sole condition

$$v^2 = 2\,(\mathrm{H} - \mathrm{V})$$

where H is the whole energy, and V the potential of the system of forces on unit mass at the point x, y, z.

Hence, taking the variation,

$$\delta\tau = \int\left(\frac{d\delta s}{v} - \frac{ds\delta v}{v^2}\right).$$

But $$ds d\delta s = dx d\delta x + dy d\delta y + dz d\delta z\,;$$

and $$v\delta v = \delta(\mathrm{H} - \mathrm{V}) = \mathrm{X}\delta x + \mathrm{Y}\delta y + \mathrm{Z}\delta z + \delta\mathrm{H},$$

if X, Y, Z be the component forces on unit mass at x, y, z. Thus we have

$$\delta\tau = \left[\frac{1}{v^2}\left(\frac{dx}{dt}\delta x + \frac{dy}{dt}\delta y + \frac{dz}{dt}\delta z\right) - \delta\mathrm{H}\int\frac{ds}{v^3}\right]$$

$$-\int\left\{\delta x\left[d\left(\frac{\frac{dx}{dt}}{v^2}\right) + \frac{\mathrm{X}dt}{v^2}\right] + \&c.\right\}\,;$$

where the whole, integrated or not, is to be taken between the given limits.

If the limits and the initial velocity be fixed, the first part of the expression for $\delta\tau$ disappears; and, that the integral may vanish, we must have

$$d\left(\frac{\frac{dx}{dt}}{v^2}\right) + \frac{\mathrm{X}dt}{v^2} = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{A}).$$

with similar equations in y and z. This is simply the ordinary result given in treatises on kinetics.

But if we consider the effect of the alteration of the limits, or of the initial energy, we have

$$\left.\begin{aligned} &\frac{\delta\tau}{\delta x} = \frac{1}{v^2}\frac{dx}{dt}, \qquad \frac{\delta\tau}{\delta x_0} = -\left(\frac{1}{v^2}\frac{dx}{dt}\right)_0, \\ &\quad \&c. \qquad\qquad\qquad \&c. \\ \text{and} \qquad &\frac{\delta\tau}{\delta \mathrm{H}} = -\int_{x_0, y_0, z_0}^{x, y, z} \frac{ds}{v^3}. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (1).$$

5. Hence, if τ could be found as a function of x, y, z, x_0, y_0, z_0, and H, it is obvious that its partial differential coefficients with respect to these quantities would give the motion completely.

But, neglecting altogether the initial limit, we see that

$$\left(\frac{d\tau}{dx}\right)^2 + \left(\frac{d\tau}{dy}\right)^2 + \left(\frac{d\tau}{dz}\right)^2 = \frac{1}{v^4}\left(\left(\frac{dx}{dt}\right)^2 + \left(\frac{dy}{dt}\right)^2 + \left(\frac{dz}{dt}\right)^2\right)$$

$$= \frac{1}{v^2} = \frac{1}{2(\mathrm{H}-\mathrm{V})} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2).$$

6. It can be easily shown, by a process similar to that employed for *Varying Action*,* that, if any integral of this equation can be found, its partial differential coefficients with respect to x, y, z are respectively equal to the corresponding components of the velocity, in a curve which is a brachistochrone for the given forces, *each divided by the square of the whole velocity.*

A *complete* integral of (2) must of course contain, besides H, two arbitrary constants α, β. If, then, τ be a complete integral, the equations of the brachistochrone are easily shown to be

$$\frac{d\tau}{d\alpha} = \mathfrak{A}, \qquad \frac{d\tau}{d\beta} = \mathfrak{B} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3);$$

where $\mathfrak{A}$ and $\mathfrak{B}$ are two new arbitrary constants.

Also we have the relation

$$\frac{d\tau}{d\mathrm{H}} = -\int\frac{dt}{v^2} = -\int\frac{ds}{v^3} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4).$$

7. Before proceeding farther with the theory, we may apply the results already obtained to one or two well-known problems; commencing with the original case proposed by Bernoulli.

8. *To find the brachistochrone, when gravity is the only impressed force, and the particle has the velocity due to a fall from a given horizontal plane.*

Taking the axis of y vertically downwards, we have

$$\mathrm{V} = -gy.$$

Also, we may write

$$\mathrm{H} = g\alpha.$$

* Thomson and Tait's Natural Philosophy, § 323, or Tait and Steele's Dynamics of a Particle (2d edition), §§ 252, 253.

Hence

$$\left(\frac{d\tau}{dx}\right)^2+\left(\frac{d\tau}{dy}\right)^2+\left(\frac{d\tau}{dz}\right)^2=\frac{1}{2g(a+y)}.$$

This equation is obviously satisfied by

$$\left(\frac{d\tau}{dx}\right)=\text{M},\qquad \left(\frac{d\tau}{dz}\right)=\text{N},\qquad \left(\frac{d\tau}{dy}\right)^2=\frac{1}{2g(a+y)}-\text{M}^2-\text{N}^2.$$

But

$$\frac{\left(\frac{d\tau}{dx}\right)}{\left(\frac{d\tau}{dz}\right)}=\frac{\frac{dx}{dt}}{\frac{dz}{dt}}\ (\text{by § 6})\ =\frac{dx}{dz}.$$

Hence $\frac{dx}{dz}=\frac{\text{M}}{\text{N}}$, that is *the path is in a vertical plane.* We may take this as the plane of xy. Hence our equation becomes

$$\left(\frac{d\tau}{dx}\right)^2+\left(\frac{d\tau}{dy}\right)^2=\frac{1}{2g(a+y)}.$$

We may now write

$$\left.\begin{aligned}\frac{d\tau}{dx}&=\frac{1}{\sqrt{2gb}}\\ \left(\frac{d\tau}{dy}\right)^2&=\frac{1}{2g}\left(\frac{1}{a+y}-\frac{1}{b}\right),\end{aligned}\right\}\quad\ldots\ldots\ldots\quad(5).$$

where b is an arbitrary constant.

By (5) we have, at once,

$$\sqrt{2g}\,\tau=\frac{x}{\sqrt{b}}+\int dy\sqrt{\frac{1}{a+y}-\frac{1}{b}}\quad\ldots\ldots\quad(6).$$

Hence the equation of the brachistochrone is (by § 6)

$$\frac{d\tau}{db}=\text{const.}$$

or

$$\text{C}=-\frac{x}{b^{\frac{3}{2}}}+\frac{1}{b^2}\int\frac{dy}{\sqrt{\frac{1}{a+y}-\frac{1}{b}}};$$

that is, changing the constant, and effecting the integration,

$$\text{C}_1=-x-\sqrt{(b-\overline{a+y})\,(a+y)}+\frac{b}{2}\,\text{vers.}^{-1}\frac{2(a+y)}{b}\quad\ldots\ldots\quad(7).$$

the common equation of the *Cycloid,* the velocity at any point being that due to a fall from the base.

In this case we have evidently

$$\frac{d\tau}{dH} = -\int \frac{ds}{v^3} = \frac{1}{g}\frac{d\tau}{da} = -\frac{1}{2\sqrt{2g^3}}\int \frac{dy}{(a+y)^2\sqrt{\frac{1}{a+y}-\frac{1}{b}}}$$

$$= \frac{1}{\sqrt{2g^3}}\sqrt{\frac{1}{a+y}-\frac{1}{b}} + C_2 .$$

The above (at first sight apparently too limited) assumptions

$$\frac{d\tau}{dx} = M, \quad \frac{d\tau}{dz} = N,$$

and the consequent reduction of the question to a *plane* problem, may seem to require some justification. This is easily supplied, thus: In the equation

$$\left(\frac{d\tau}{dx}\right)^2 + \left(\frac{d\tau}{dy}\right)^2 + \left(\frac{d\tau}{dz}\right)^2 = F^2,$$

the direction-cosines of the tangent to the brachistochrone, at the point x, y, z, are, by (1.),

$$l = \frac{1}{F}\frac{d\tau}{dx}, \quad m = \frac{1}{F}\frac{d\tau}{dy}, \quad n = \frac{1}{F}\frac{d\tau}{dz} .$$

At the adjacent point $x + \delta x$, $y + \delta y$, $z + \delta z$, where we have, of course,

$$\frac{\delta x}{l} = \frac{\delta y}{m} = \frac{\delta z}{n} = \delta s,$$

the value of l becomes

$$l' = \frac{\frac{d\tau}{dx} + \frac{d^2\tau}{dx^2}\delta x + \frac{d^2\tau}{dxdy}\delta y + \frac{d^2\tau}{dxdz}\delta z}{F + \delta F}$$

$$= \frac{\frac{d\tau}{dx} + \frac{\delta s}{F}\left(\frac{d\tau}{dx}\frac{d^2\tau}{dx^2} + \frac{d\tau}{dy}\frac{d^2\tau}{dxdy} + \frac{d\tau}{dz}\frac{d^2\tau}{dxdz}\right)}{F + \delta F}$$

$$= \frac{\frac{d\tau}{dx} + \left(\frac{dF}{dx}\right)\delta s}{F + \delta F} .$$

But in the above problem F *is a function of* y *only*, and we must therefore have

$$\frac{l'}{n'} = \frac{l}{n},$$

which shows that the curve is in a plane parallel to the axis of y.

9. *To find the Brachistochrone when the force is central, and proportional to a power of the distance; the velocity being also proportional to a power of the distance, that is, being the velocity from infinity if the force is attractive, from the centre if it is repulsive.*

Here

$$v^2 = 2(\mathrm{H}-\mathrm{V}) = \frac{\mu}{r^n},$$

and the central force at distance r is evidently

$$-\frac{d\mathrm{V}}{dr} = -\frac{n\mu}{2r^{n+1}}.$$

Thus (2) becomes

$$\left(\frac{d\tau}{dx}\right)^2 + \left(\frac{d\tau}{dy}\right)^2 + \left(\frac{d\tau}{dz}\right)^2 = \frac{r^n}{\mu}$$

or, changing to polar co-ordinates,

$$\left(\frac{d\tau}{dr}\right)^2 + \frac{1}{r^2}\left(\frac{d\tau}{d\theta}\right)^2 + \frac{1}{r^2\sin^2\theta}\left(\frac{d\tau}{d\phi}\right)^2 = \frac{r^n}{\mu}.$$

It is obvious that we must take

$$\frac{d\tau}{d\phi} = 0,$$

which shows that the path is in a plane passing through the centre of force. The above equation will then be satisfied by

$$\frac{d\tau}{d\theta} = a, \qquad \frac{d\tau}{dr} = \sqrt{\frac{r^n}{\mu} - \frac{a^2}{r^2}}.$$

Hence we have

$$\tau = a\theta + \int dr\sqrt{\frac{r^n}{\mu} - \frac{a^2}{r^2}},$$

$$= a\theta + \frac{2a}{n+2}\left\{\sqrt{\frac{r^{n+2}}{\mu a^2} - 1} - \cos^{-1}\frac{\sqrt{\mu}a}{r^{\frac{n+2}{2}}}\right\} + \mathrm{C}.$$

And the equation of the brachistochrone, which is evidently a plane curve, is

$$\mathfrak{A} = \theta + \frac{2}{n+2}\left\{\sqrt{\frac{r^{n+2}}{\mu a^2} - 1} - \cos^{-1}\frac{\sqrt{\mu}a}{r^{\frac{n+2}{2}}}\right\}$$

$$+ \frac{2a}{n+2}\left\{-\frac{\frac{r^{n+2}}{\mu a^3}}{\sqrt{\frac{r^{n+2}}{\mu a^2} - 1}} + \frac{\sqrt{\mu}}{r^{\frac{n+2}{2}}}\frac{1}{\sqrt{1 - \frac{\mu a^2}{r^{n+2}}}}\right\}$$

$$= \theta - \frac{2}{n+2}\cos^{-1}\frac{\sqrt{\mu}a}{r^{\frac{n+2}{2}}}:$$

or

$$r^{\frac{n+2}{2}} = \sqrt{\mu}a\sec\frac{n+2}{2}(\theta - \mathfrak{A}),$$

while the equation of the *free* path is

$$\left(\frac{r}{a}\right)^{\frac{n-2}{2}} = \cos\frac{n-2}{2}(\theta+\beta) .$$

The above integration fails in the case of $n = -2$; that is, when the force is repulsive and directly as the distance, the velocity vanishing at the centre of force. But in this case

$$\tau = a\theta + \sqrt{\frac{1}{\mu} - a^2}\log Cr,$$

and the equation of the brachistochrone is

$$\mathfrak{A} = \theta - \frac{a}{\sqrt{\frac{1}{\mu} - a^2}}\log Cr,$$

the logarithmic spiral. Eliminating r between these equations, we see that the time is proportional to the polar angle.

Since a definite form has been assigned to the expression for the velocity in this problem, it is obvious that H is given, and therefore that there is no $\frac{d\tau}{dH}$.

The assumption

$$\frac{d\tau}{d\phi} = 0$$

is easily justified, in the case of any equation of the form

$$\left(\frac{d\tau}{dr}\right)^2 + \frac{1}{r^2}\left(\frac{d\tau}{d\theta}\right)^2 + \frac{1}{r^2\sin^2\theta}\left(\frac{d\tau}{d\phi}\right)^2 = F^2,$$

if F be a function of r only. For

$$\delta\left(\frac{d\tau}{d\phi}\right) = \frac{d^2\tau}{drd\phi}\delta r + \frac{d^2\tau}{d\theta d\phi}\delta\theta + \frac{d^2\tau}{\delta\phi^2}\delta\phi.$$

But

$$\frac{d\tau}{dr} = F^2\frac{dr}{dt}, \qquad \frac{d\tau}{rd\theta} = F^2\frac{rd\theta}{dt}, \qquad \frac{d\tau}{r\sin\theta d\phi} = F^2\frac{r\sin\theta d\phi}{dt}.$$

Hence

$$\delta\left(\frac{d\tau}{d\phi}\right) = \frac{\delta t}{F^2}\left\{\frac{d\tau}{dr}\frac{d^2\tau}{drd\phi} + \frac{1}{r^2}\frac{d\tau}{d\theta}\frac{d^2\tau}{d\theta d\phi} + \frac{1}{r^2\sin^2\theta}\frac{d\tau}{d\phi}\frac{d^2\tau}{d\phi^2}\right\} = \frac{\delta t}{F}\left(\frac{dF}{d\phi}\right) = 0.$$

That is, unless F contains ϕ, $\frac{d\tau}{d\phi}$ is necessarily a constant, β suppose.

But, in the present case, *if we give this constant any value but zero, we introduce a problem much more general than that proposed,* for the expression for the reciprocal of the square of the velocity becomes

$$\frac{r^n}{\mu} - \frac{\beta^2}{r^2\sin^2\theta}.$$

10. As an example of a tortuous curve we take the following:

Determine the form of the brachistochrone when the velocity at any point of space is proportional to the distance from a given line.

Taking the line as the axis of z, our equation obviously becomes

$$\left(\frac{d\tau}{dx}\right)^2+\left(\frac{d\tau}{dy}\right)^2+\left(\frac{d\tau}{dz}\right)^2=\frac{a^2}{x^2+y^2}.$$

Hence

$$\frac{d\tau}{dz}=a,$$

and, substituting this, and changing to polar co-ordinates in a plane parallel to xy,

$$\left(\frac{d\tau}{dr}\right)^2+\frac{1}{r^2}\left(\frac{d\tau}{d\theta}\right)^2=\frac{a^2}{r^2}-a^2.$$

Hence we may take

$$\frac{d\tau}{d\theta}=\beta,$$

and there remains

$$\frac{d\tau}{dr}=\frac{1}{r}\sqrt{a^2-\beta^2-a^2r^2}.$$

Integrating, we have

$$\tau=az+\beta\theta-\sqrt{a^2-\beta^2}\log.\left[\frac{\sqrt{a^2-\beta^2}}{r}+\sqrt{\frac{a^2-\beta^2}{r^2}-a^2}\right]+\sqrt{a^2-\beta^2-a^2r^2}.$$

By equating to constants the partial differential coefficients of τ with respect to a and β, we obtain the two equations of the brachistochrone

$$\mathfrak{A}=z-\frac{ar^2}{\sqrt{a^2-\beta^2}+\sqrt{a^2-\beta^2-a^2r^2}},$$

and

$$\mathfrak{B}=\theta+\frac{\beta}{\sqrt{a^2-\beta^2}}\log.\left[\frac{\sqrt{a^2-\beta^2}}{r}+\sqrt{\frac{a^2-\beta^2}{r^2}-a^2}\right].$$

The former of these is the equation of a sphere, as may be seen at once by putting it in the form

$$a\,(z-\mathfrak{A})=\sqrt{a^2-\beta^2}-\sqrt{a^2-\beta^2-a^2r^2}.$$

The remaining equation, by altering the value of $\mathfrak{B}$, may be reduced to the form

$$2\,\frac{\sqrt{a^2-\beta^2}}{a}=r\left(\epsilon^{\frac{\sqrt{a^2-\beta^2}}{\beta}(\theta-\mathfrak{B})}+\epsilon^{-\frac{\sqrt{a^2-\beta^2}}{\beta}(\theta-\mathfrak{B})}\right)$$

which is at once recognised as a cylinder, whose base is one of COTES' Spirals.

Also, if we remark that, by (1),

$$r\frac{d\theta}{dt}=v^2\frac{d\tau}{rd\theta}=\frac{r^2}{a^2}\cdot\frac{\beta}{r}=\frac{\beta v}{a}$$

we see that

$$\cos\psi = \frac{r\dfrac{d\theta}{dt}}{v} = \frac{\beta}{a} = \text{const.}$$

where ψ is the inclination of the element $r\delta\theta$ to the corresponding element δs of the brachistochrone. That is, the brachistochrone cuts all circles on the above sphere, whose planes are parallel to xy, at a constant angle. (*Loxodrome.*)

11. It is easily seen that

$$\tau = \text{C}$$

is the equation of an *Isochronous* surface.

Also, since

$$\frac{\left(\dfrac{d\tau}{dx}\right)}{\dfrac{dx}{dt}} = \frac{\left(\dfrac{d\tau}{dy}\right)}{\dfrac{dy}{dt}} = \frac{\left(\dfrac{d\tau}{dz}\right)}{\dfrac{dz}{dt}}$$

the brachistochrone cuts all such surfaces at right angles.

And the normal distance between two consecutive isochronous surfaces is proportional to the velocity in the brachistochrone of which it forms an element. For, of course,

$$\delta s = v\delta\tau.$$

12. Generally, putting

$$\mathfrak{T} = \left(\frac{d\tau}{dx}\right)^2 + \left(\frac{d\tau}{dy}\right)^2 + \left(\frac{d\tau}{dz}\right)^2 = \frac{1}{2(\text{H}-\text{V})} \quad . \quad . \quad . \quad . \quad (7),$$

we have

$$2(\text{H}-\text{V}) = \frac{1}{\mathfrak{T}},$$

and

$$\text{X} = -\left(\frac{d\text{V}}{dx}\right) = -\frac{1}{2\mathfrak{T}^2}\frac{d\mathfrak{T}}{dx} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8),$$

with similar expressions for Y and Z.

Also, by (1), we have

$$\left.\begin{aligned} \frac{d\tau}{dx} &= \mathfrak{T}\frac{dx}{dt}, \text{ \&c.} \\ \text{and} \qquad \frac{d\tau}{d\text{H}} &= -\int \mathfrak{T} dt \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9),$$

Hence

$$\begin{aligned} \frac{d^2x}{dt^2} &= \frac{d}{dt}\left(\frac{1}{\mathfrak{T}}\frac{d\tau}{dx}\right) \\ &= \frac{1}{\mathfrak{T}}\frac{d}{dt}\left(\frac{d\tau}{dx}\right) - \frac{1}{\mathfrak{T}^2}\frac{d\tau}{dx}\frac{d\mathfrak{T}}{dt} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10). \end{aligned}$$

But
$$\frac{d}{dt}\left(\frac{d\tau}{dx}\right) = \frac{d^2\tau}{dx^2}\frac{dx}{dt} + \frac{d^2\tau}{dydx}\frac{dy}{dt} + \frac{d^2\tau}{dzdx}\frac{dz}{dt}$$
$$= \frac{1}{\mathfrak{T}}\left\{\frac{d^2\tau}{dx^2}\frac{d\tau}{dx} + \frac{d^2\tau}{dydx}\frac{d\tau}{dy} + \frac{d^2\tau}{dzdx}\frac{d\tau}{dz}\right\} = \frac{1}{2\mathfrak{T}}\frac{d\mathfrak{T}}{dx}, \qquad (11),$$

which is the ordinary form of the equation of the brachistochrone, (A) in § 4.

Also,
$$\frac{d\mathfrak{T}}{dt} = 2\left\{\frac{d\tau}{dx}\frac{d}{dt}\left(\frac{d\tau}{dx}\right) + \frac{d\tau}{dy}\frac{d}{dt}\left(\frac{d\tau}{dy}\right) + \frac{d\tau}{dz}\frac{d}{dt}\left(\frac{d\tau}{dz}\right)\right\}$$
$$= \frac{1}{\mathfrak{T}}\left\{\frac{d\tau}{dx}\frac{d\mathfrak{T}}{dx} + \frac{d\tau}{dy}\frac{d\mathfrak{T}}{dy} + \frac{d\tau}{dz}\frac{d\mathfrak{T}}{dz}\right\} \quad . \quad . \quad . \quad . \quad . \quad (12).$$

The above value of $\frac{d^2x}{dt^2}$ becomes therefore
$$\frac{d^2x}{dt^2} = \frac{1}{2\mathfrak{T}^2}\frac{d\mathfrak{T}}{dx} - \frac{\frac{d\tau}{dx}}{\mathfrak{T}^3}\left\{\frac{d\tau}{dx}\frac{d\mathfrak{T}}{dx} + \frac{d\tau}{dy}\frac{d\mathfrak{T}}{dy} + \frac{d\tau}{dz}\frac{d\mathfrak{T}}{dz}\right\} \quad . \quad . \quad . \quad . \quad (13),$$

which (8) reduces to the form
$$\frac{d^2x}{dt^2} = -X + 2\frac{\frac{d\tau}{dx}}{\mathfrak{T}}\left\{X\frac{d\tau}{dx} + Y\frac{d\tau}{dy} + Z\frac{d\tau}{dz}\right\} \quad . \quad . \quad . \quad . \quad (14).$$

And we have, of course, similar expressions for $\frac{d^2y}{dt^2}$ and $\frac{d^2z}{dt^2}$.

13. We may thus easily prove the fundamental property of brachistochrones given in most treatises on dynamics.

The pressure on the curve, due to the motion, is equal to that due to the impressed forces.

For (14) may be written
$$\frac{d^2x}{dt^2} = -X + 2\frac{dx}{dt}\mathfrak{T}\left\{X\frac{dx}{dt} + Y\frac{dy}{dt} + Z\frac{dz}{dt}\right\}$$
$$= -X + 2\frac{dx}{ds}\left\{X\frac{dx}{ds} + Y\frac{dy}{ds} + Z\frac{dz}{ds}\right\}$$
$$= X - 2\left\{X - \frac{dx}{ds}\left(X\frac{dx}{ds} + Y\frac{dy}{ds} + Z\frac{dz}{ds}\right)\right\}.$$

Now $X\frac{dx}{ds} + Y\frac{dy}{ds} + Z\frac{dz}{ds}$ is the component of the impressed forces along ds. Hence
$$X - \frac{dx}{ds}\left(X\frac{dx}{ds} + Y\frac{dy}{ds} + Z\frac{dz}{ds}\right),$$
$$Y - \frac{dy}{ds}\left(X\frac{dx}{ds} + Y\frac{dy}{ds} + Z\frac{dz}{ds}\right), \qquad Z - \frac{dz}{ds}\left(X\frac{dx}{ds} + Y\frac{dy}{ds} + Z\frac{dz}{ds}\right),$$

are the rectangular components of the component of the impressed force perpendicular to the path.

But, if R be the force of constraint, λ, μ, ν, its direction-cosines, we have by ordinary kinetics

$$\frac{d^2x}{dt^2} = \mathrm{X} - \mathrm{R}\lambda, \quad \&c.$$

Hence $$\mathrm{R}\lambda = 2\left(\mathrm{X} - \frac{dx}{ds}\left(\mathrm{X}\frac{dx}{ds} + \mathrm{Y}\frac{dy}{ds} + \mathrm{Z}\frac{dz}{ds}\right)\right), \quad \&c., \&c.,$$

and therefore the whole pressure is *double* that due to the impressed forces.

From the above follows also the well-known theorem, that *the osculating plane of the brachistochrone contains, at each point, the resultant of the impressed forces.* For it has been shown that this resultant coincides in direction with the centrifugal force, and the latter of course lies in the osculating plane.

14. Another, and perhaps simpler proof of the theorem above is furnished directly by (10). Thus, squaring and adding the three equations of that form, after substituting in them from (11), we have

$$\left(\frac{d^2x}{dt^2}\right)^2 + \left(\frac{d^2y}{dt^2}\right)^2 + \left(\frac{d^2z}{dt^2}\right)^2 = \frac{1}{4\mathfrak{T}^4}\left\{\left(\frac{d\mathfrak{T}}{dx}\right)^2 + \left(\frac{d\mathfrak{T}}{dy}\right)^2 + \left(\frac{d\mathfrak{T}}{dz}\right)^2\right\}$$

$$- \frac{1}{\mathfrak{T}^4}\frac{d\mathfrak{T}}{dt}\left\{\frac{d\tau}{dx}\frac{d\mathfrak{T}}{dx} + \frac{d\tau}{dy}\frac{d\mathfrak{T}}{dy} + \frac{d\tau}{dz}\frac{d\mathfrak{T}}{dz}\right\}$$

$$+ \frac{1}{\mathfrak{T}^4}\left(\frac{d\mathfrak{T}}{dt}\right)^2\left\{\left(\frac{d\tau}{dx}\right)^2 + \left(\frac{d\tau}{dy}\right)^2 + \left(\frac{d\tau}{dz}\right)^2\right\}$$

$$= \frac{1}{4\mathfrak{T}^4}\left\{\left(\frac{d\mathfrak{T}}{dx}\right)^2 + \left(\frac{d\mathfrak{T}}{dy}\right)^2 + \left(\frac{d\mathfrak{T}}{dz}\right)^2\right\} - \frac{1}{\mathfrak{T}^4}\frac{d\mathfrak{T}}{dt}\left(\mathfrak{T}\frac{d\mathfrak{T}}{dt}\right) + \frac{1}{\mathfrak{T}^4}\left(\frac{d\mathfrak{T}}{dt}\right)^2(\mathfrak{T})$$

[by (12) and (7)]

$$= \frac{1}{4\mathfrak{T}^4}\left\{\left(\frac{d\mathfrak{T}}{dx}\right)^2 + \left(\frac{d\mathfrak{T}}{dy}\right)^2 + \left(\frac{d\mathfrak{T}}{dz}\right)^2\right\} = \mathrm{X}^2 + \mathrm{Y}^2 + \mathrm{Z}^2, \quad . \quad . \quad \text{by (8).}$$

Hence *the whole acceleration is equal to the resultant of the impressed forces;* and therefore the component of the acceleration, normal to the curve, must be equal to that of the resultant of the impressed forces; from which the theorem follows at once if we can show independently that the resultant of the impressed forces lies in the osculating plane. This is easily done as follows. We have

$$\delta x = \frac{\delta t}{\mathfrak{T}}\frac{d\tau}{dx}, \quad \&c., \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{by (9).}$$

Hence $$\delta^2 x = \frac{\delta t}{\mathfrak{T}}\delta\left(\frac{d\tau}{dx}\right) - \frac{\delta x}{\mathfrak{T}}\delta\mathfrak{T}, \quad \&c.$$

Now, by (8) and (11), $\delta\left(\frac{d\tau}{dx}\right)$ &c., are proportional to the direction-cosines of the resultant force, which therefore lies in the common plane of two consecutive elements of the curve.

15. The equation of the surfaces which are orthogonal to the path is

$$\tau = C\,;$$

and that of equipotential surfaces

$$V = C_1\,.$$

That these may coincide we must have

$$\tau = \phi(V)\,,$$

where ϕ is any function whatever.

Hence

$$\left\{\phi'(V)\right\}^2\left(\left(\frac{dV}{dx}\right)^2 + \left(\frac{dV}{dy}\right)^2 + \left(\frac{dV}{dz}\right)^2\right) = \frac{1}{2(H-V)}\,.$$

If we write

$$\mathfrak{V} = \int\sqrt{2(H-V)}\,\phi'(V)\,dV = \psi(V)\,, \quad . \quad . \quad . \quad (15).$$

this becomes

$$\left(\frac{d\mathfrak{V}}{dx}\right)^2 + \left(\frac{d\mathfrak{V}}{dy}\right)^2 + \left(\frac{d\mathfrak{V}}{dz}\right)^2 = 1\,, \quad . \quad . \quad . \quad . \quad (16).$$

A complete primitive of this equation is, of course,

$$\mathfrak{V} = lx + my + nz - p\,,$$

where p is any function of l, m, n, and

$$l^2 + m^2 + n^2 = 1.$$

The general primitive, equated to a constant, is therefore obviously the equation of a series of surfaces such that the normal distance between any two consecutive members of the series is everywhere the same. It is evident from (15) that the surfaces thus found are identical with the isochronous and equipotential surfaces, when these coincide. The equations of their orthogonal trajectory, that is, of the free path which is also a brachistochrone, are therefore,

$$\frac{\delta x}{\left(\frac{d\mathfrak{V}}{dx}\right)} = \frac{\delta y}{\left(\frac{d\mathfrak{V}}{dy}\right)} = \frac{\delta z}{\left(\frac{d\mathfrak{V}}{dz}\right)} = \frac{\left(\frac{d\mathfrak{V}}{dx}\right)\delta x + \left(\frac{d\mathfrak{V}}{dy}\right)\delta y + \left(\frac{d\mathfrak{V}}{dz}\right)\delta z}{\left(\frac{d\mathfrak{V}}{dx}\right)^2 + \left(\frac{d\mathfrak{V}}{dy}\right)^2 + \left(\frac{d\mathfrak{V}}{dz}\right)^2} = \delta\mathfrak{V} = \delta C, \quad (17).$$

Hence,

$$\delta x = \delta C\left(\frac{d\mathfrak{V}}{dx}\right)\,, \text{ \&c.,}$$

and, therefore,

$$\delta^2 x = \delta C \left\{ \left(\frac{d^2\mathfrak{V}}{dx^2}\right) \delta x + \left(\frac{d^2\mathfrak{V}}{dxdy}\right) \delta y + \left(\frac{d^2\mathfrak{V}}{dxdz}\right) \delta z \right\} + \delta^2 C \left(\frac{d\mathfrak{V}}{dx}\right).$$

But, substituting the values of δx, &c., from (17), this becomes

$$\delta^2 x = (\delta C)^2 \left\{ \left(\frac{d\mathfrak{V}}{dx}\right)\left(\frac{d^2\mathfrak{V}}{dx^2}\right) + \left(\frac{d\mathfrak{V}}{dy}\right)\left(\frac{d^2\mathfrak{V}}{dxdy}\right) + \left(\frac{d\mathfrak{V}}{dz}\right)\left(\frac{d^2\mathfrak{V}}{dxdz}\right) \right\} + \delta^2 C \left(\frac{d\mathfrak{V}}{dx}\right),$$

and the first part vanishes, by (16).

Hence

$$\frac{\delta^2 x}{\delta x} = \frac{\delta^2 y}{\delta y} = \frac{\delta^2 z}{\delta z} = \frac{\delta^2 C}{\delta C},$$

which show that when the path is simultaneously a free path and a brachistochrone, it is necessarily rectilinear.

This might have been inferred at once, from the theorem of § 13, which shows that if the free path be a brachistochrone, there can be no pressure due to the motion, *i.e.*, no curvature. But the above investigation is given as containing curious additional information. It shows, for instance, that if the force be the same at all points of each of a series of equipotential surfaces, the lines of force are rectilinear. Also, that if the flux of heat be constant per unit of area over each one of a series of isothermal surfaces, though not necessarily the same for all, the propagation of heat takes place in straight lines. And, as particular cases of these theorems, if the force or the flux of heat be the same throughout a given space, the attraction, or the flux, therein takes place in parallel lines.

16. HAMILTON'S equation for the determination of the Characteristic Function (A) in the case of the free motion of a single particle is

$$\left(\frac{dA}{dx}\right)^2 + \left(\frac{dA}{dy}\right)^2 + \left(\frac{dA}{dz}\right)^2 = 2(H - V) \quad . \quad . \quad . \quad . \quad (18).$$

The comparison of this with (2) suggests a useful transformation. Introducing in that equation a factor θ^2, an undetermined function of x, y, z, we have

$$\left(\theta\frac{d\tau}{dx}\right)^2 + \left(\theta\frac{d\tau}{dy}\right)^2 + \left(\theta\frac{d\tau}{dz}\right)^2 = \frac{\theta^2}{2(H - V)} \quad . \quad . \quad . \quad (19).$$

If we make

$$\theta = \phi'(\tau) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20),$$

and

$$\frac{\theta^2}{2(H - V)} = 2(H_1 - V_1) \quad . \quad . \quad . \quad . \quad . \quad (21),$$

(19) becomes

$$\left(\frac{d\phi(\tau)}{dx}\right)^2 + \left(\frac{d\phi(\tau)}{dy}\right)^2 + \left(\frac{d\phi(\tau)}{dz}\right)^2 = 2(H_1 - V_1) \quad . \quad . \quad . \quad (22).$$

Here it is obvious, by (18), that $\phi(\tau)$ is the action in a *free* path coinciding with the brachistochrone, and that $2(H_1-V_1)$ is the square of the velocity in this path.

Hence the curious result that, *if* τ *be the time through any arc of a given brachistochrone, the same path will be described freely under the action of forces whose potential is* V_1, *where*

$$2(H_1 - V_1) = \frac{(\phi'(\tau))^2}{2(H - V)},$$

ϕ' *being any function whatever; and* $\phi(\tau)$ *representing the action in the free path.*

17. The simplest supposition we can make is that $\phi'(\tau)$ is constant. In this case the velocity in the free path is inversely proportional to that in the brachistochrone at the same point; and the action in the one is proportional to the time in the other. In fact, as Professor W. THOMSON has pointed out to me, in this case the investigation may be made with extreme simplicity, thus—

In the brachistochrone we have

$$\int \frac{ds}{v} \text{ a minimum.}$$

Putting $\nu = \frac{1}{v}$, and considering ν as the velocity in the same path due to another (easily determinable) potential; we must have

$$\int \nu ds \text{ a minimum.}$$

This is the ordinary condition of *Least Action*, and belongs, therefore, to a free path.

Hence, since the cycloid is the brachistochrone for gravity, and since in it $v^2 = 2gy$, it will be a free path if $\nu^2 = \frac{1}{2gy}$, that is for a system of force where the potential is found from

$$H_1 - V_1 = \frac{1}{4gy}.$$

This gives

$$-\frac{dV_1}{dx} = 0, \quad -\frac{dV_1}{dy} = -\frac{1}{4gy^2}.$$

In other words, a cycloid may be described freely under the action of a force towards, and inversely as the square of the distance from, the base; and the velocity at any point will be the reciprocal of that in the same cycloid when it is the common brachistochrone.

This result is easily verified by a direct process.

18. But we have, by § 16, an infinite number of other systems of forces under which this cycloid will be described freely.

For by § 8 we have, putting $a=0$, since the base is now the axis of x,

$$\sqrt{2g}\,\tau = \frac{x}{\sqrt{b}} + \int dy \sqrt{\frac{1}{y} - \frac{1}{b}}$$

$$= \frac{x}{\sqrt{b}} - \sqrt{b} \overset{-1}{\cos} \sqrt{\frac{y}{b}} + \sqrt{\frac{y}{b}}\sqrt{b-y} + \mathrm{C}.$$

Hence, whatever be ϕ', the cycloid is a free path for the system

$$v^2 = 2(\mathrm{H}_1 - \mathrm{V}_1) = \frac{\left\{\phi'\left(\frac{x}{\sqrt{b}} - \sqrt{b}\overset{-1}{\cos}\sqrt{\frac{y}{b}} + \sqrt{\frac{y}{b}}\sqrt{b-y} + \mathrm{C}\right)\right\}^2}{2gy}.$$

19. The converse of the proposition in § 16 is also curious. Taking HAMILTON's equation (18), we have,

$$(\phi'(\mathrm{A}))^2\left\{\left(\frac{d\mathrm{A}}{dx}\right)^2 + \left(\frac{d\mathrm{A}}{dy}\right)^2 + \left(\frac{d\mathrm{A}}{dz}\right)^2\right\} = 2(\mathrm{H}-\mathrm{V})(\phi'(\mathrm{A}))^2 \quad . \quad . \quad (23).$$

Comparing this with (2), we see that $\tau = \phi(\mathrm{A})$ is the brachistochronic expression for the time in a path which is a free path for potential V. The requisite potential is now found from

$$\frac{1}{2(\mathrm{H}_1 - \mathrm{V}_1)} = 2(\mathrm{H}-\mathrm{V})(\phi'(\mathrm{A}))^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (24).$$

Hence, *if* A *be the action in a given free path, the same path will be a brachistochrone for forces whose potential is* V_1, *determined by* (24), V *being the potential in the free path.*

Thus, the parabola

$$(x - \mathfrak{A})^2 = 4a\,(y - a)$$

is the free path for $v^2 = 2gy$. And the action is given by

$$\frac{1}{\sqrt{2g}}\mathrm{A} = x\sqrt{a} + \frac{2}{3}(y-a)^{\frac{3}{2}}.$$

Hence this parabola is the brachistochrone for

$$2(\mathrm{H}_1 - \mathrm{V}_1) = \frac{1}{2gy(\phi'(\mathrm{A}))^2}.$$

In the simplest case $\phi'(\mathrm{A}) = 1$, and we have

$$-\frac{d\mathrm{V}_1}{dx} = 0, \qquad -\frac{d\mathrm{V}_1}{dy} = -\frac{1}{4gy^2}.$$

Hence, by § 17, the parabola is a brachistochrone when a cycloid is the free path.

20. Again, if

$$v^2 = 2\left(\frac{\mu}{r} - \mathrm{H}\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad (25).$$

where H and μ are essentially positive, the free path is an ellipse of which the origin (the centre of force) is a focus.

This ellipse is the brachistochrone for the potential V_1, and whole energy H_1, where

$$\frac{C}{2(H_1 - V_1)} = 2\left(\frac{\mu}{r} - H\right),$$

or

$$V_1 = H_1 - \frac{Cr}{4(\mu - Hr)}.$$

This corresponds to a central force

$$-\frac{dV_1}{dr} = \frac{C}{4(\mu - Hr)} + \frac{CHr}{4(\mu - Hr)^2}$$

$$= \frac{C\mu}{4(\mu - Hr)^2}.$$

The velocity at any point is

$$\sqrt{\frac{Cr}{2(\mu - Hr)}}.$$

In the ellipse, we know by ordinary kinetics that

$$v^2 = \mu\left(\frac{2}{r} - \frac{1}{a}\right).$$

Comparing this with the above formula (25) we have

$$\frac{\mu}{H} = 2a.$$

Hence the velocity in the free ellipse is

$$v = \sqrt{\frac{\mu}{a}}\sqrt{\frac{2a - r}{r}} \quad \ldots \quad (26).$$

That in the same ellipse, when it is a brachistochrone, is, as above,

$$v_1 = \sqrt{\frac{Cr}{2(\mu - Hr)}} = \sqrt{\frac{Ca}{\mu}}\sqrt{\frac{r}{2a - r}}.$$

But if we refer it to the other focus of the ellipse we have

$$r_1 = 2a - r.$$

Hence

$$v_1 = \sqrt{\frac{Ca}{\mu}}\sqrt{\frac{2a - r_1}{r_1}} \quad \ldots \quad (27).$$

Comparing (26) and (27), we have the singular result that *a planet moving freely about a centre of force in the focus of its elliptic orbit is describing a brachistochrone* (*for the same law of velocity as regards position*) *about the other focus.* The reason of this remarkable property, as well as of the connected one that *while the time in an elliptic orbit is (of course) measured by the area described about one focus, the action is measured by that described about the other,** is easily traced to the fact that the rectangle under the perpendiculars from the foci on any tangent is constant.

21. It follows from HAMILTON'S investigations, that in the free ellipse we have

$$A = \int \frac{2\left(\frac{\mu}{r} - H\right) dr}{\sqrt{2\left(\frac{\mu}{r} - H\right) - \frac{a^2}{r^2}}},$$

where a depends upon the excentricity of the ellipse by the formula

$$a^2 = \frac{\mu^2}{2H}(1 - e^2).$$

The theorem may therefore be generalized as follows:—The free ellipse will be a brachistochrone, if the velocity be given by

$$v^2 = 2(H_1 - V_1) = \frac{1}{2\left(\frac{\mu}{r} - H\right)\left\{\phi'(A)\right\}^2}$$

where ϕ' is any function, and A is the integral last written. By differentiation with respect to r, we get the law of central force requisite.

But results of this nature may be deduced to any desired extent, without more trouble than the requisite integrations involve.

22. The examples immediately preceding are but particular cases of the following general theorem, which is easily seen to be involved in the results of §§ 16, 19. *If we have two curves, P and Q, of which P is a free path, and Q a brachistochrone, for a given conservative system of forces; P will be a brachistochrone for a system of forces for which Q is a free path—and the action and time in any arc of either, when it is described freely, are functions of the time and action respectively, in the same arc, when it is a brachistochrone.*

23. It is easy to see, that there exists a very singular analogy between the processes we have just given, and those suggested by certain problems in optics.

Assuming, for an instant, the exploded corpuscular theory of Light, Varying Action is at once applicable to the determination of the path of a corpuscle. On the other hand, if we assume, as our fundamental hypothesis, that light takes the least possible time to pass from one point of its path to another, the foregoing investigations would be directly applicable to find the path in a medium whose refractive index (on which the velocity depends), at any point, is a given function of the co-ordinates; in other words, in a heterogeneous singly refracting medium.

In the beautiful investigations of HAMILTON, on the *Theory of Systems of Rays*

* Proc. R.S.E. March 1865, or TAIT and STEELE'S Dynamics of a Particle (2d edition) § 258.

(Trans. R.I.A., 1824–32), the path of a ray is assumed to be a straight line in any one medium. Here the velocity depends only upon the *direction* of the ray, as in homogeneous doubly refracting media, and the problem has no analogy with the conservative case which is treated above.

24. As an instance of an optical problem I take the following, due I believe to MAXWELL.* *If the refractive index of a medium be such a function of the distance from a given point that the path of any one ray is a circle, the path of every other ray is a circle; and all rays diverging from any one point converge accurately in another.* Or, in another form, find the relation between the velocity and the distance from the centre of force that the brachistochrone may always be a circle.

The symmetry shows that our investigations need only involve two dimensions. Taking the centre of force as pole, the equation of a circle is

$$r^2 - 2ar\cos(\theta - \mathfrak{A}) = \varrho^2 - a^2, = b^2 \text{ suppose.}$$

Hence

$$\mathfrak{A} = \theta - \cos^{-1}\frac{b^2 - r^2}{2ar}.$$

This is obviously the equation before written (3) in the form

$$\frac{d\tau}{da} = \mathfrak{A}.$$

Hence

$$\tau = a\theta - \int da \cos^{-1}\frac{b^2 - r^2}{2ar}.$$

But, if v be the velocity (the reciprocal of the refractive index in the optical problem),

$$\left(\frac{d\tau}{dr}\right)^2 + \frac{1}{r^2}\left(\frac{d\tau}{d\theta}\right)^2 = \frac{1}{v^2}.$$

Hence

$$\frac{d\tau}{dr} = \sqrt{\frac{1}{v^2} - \frac{a^2}{r^2}} = -\frac{d}{dr}\int da \cos^{-1}\frac{b^2 - r^2}{2ar} = -\int da \frac{b^2 + r^2}{r\sqrt{(4a^2r^2 - (b^2 - r^2)^2)}}.$$

But v is not a function of a, so that we get by differentiation with respect to that quantity

$$\frac{\frac{a}{r^2}}{\sqrt{\frac{1}{v^2} - \frac{a^2}{r^2}}} = \frac{b^2 + r^2}{r\sqrt{(4a^2r^2 - (b^2 - r^2)^2)}}.$$

This is easily reduced to

$$v^2a^2 = \frac{(b^2 + r^2)^2}{4(a^2 + b^2)} = \frac{(b^2 + r^2)^2}{4\varrho^2}.$$

The condition, that v is a function of r and absolute constants only, thus leads

* Cambridge and Dublin Math. Journal, IX., p. 9.

at once to two conclusions: b is an absolute constant; and so is $2\varrho a$, for which we may write c. a is therefore inversely as the diameter of the circle; and

$$v = \frac{b^2 + r^2}{c}.$$

From the form of the equation of the path it is obvious that $-b^2$ is the rectangle under the segments of any chord drawn through the centre of force.

Hence, in the optical problem, if a ray leave, in any direction, a point distant r from the origin, it will pass through another point in the prolongation of r, distant $\frac{b^2}{r}$ from the origin; and, in the kinetic problem, there is an infinite number of brachistochrones (circles all, and the time being the same for all) when two points thus related are taken as the initial and final points.

25. Such examples might be multiplied indefinitely. For instance, if the refractive index of a medium be inversely proportional to the square root of the distance from a given point, the path is a parabola about the point as focus; that every ray may be a cardioid whose cusp is at the point, the square of the refractive index must be inversely as the cube of the distance: and so on.

26. The processes of § 4 may of course be applied to innumerable problems besides the determination of the form and properties of brachistochrones, but I shall content myself with an example or two. Thus, if we take

$$\Phi = \int f(v)\,ds$$

as the characteristic function, we have

$$\frac{d\Phi}{dx} = \frac{f(v)}{v}\frac{dx}{dt}, \text{ \&c., and } \frac{d\Phi}{dH} = \int f'(v)\,dt.$$

Of this, besides the cases $f(v) = v$, and $f(v) = \frac{1}{v}$, which we have already considered, the most curious is that where

$$f(v) = \frac{v^2}{2};$$

that is, when *the space average of the kinetic energy is a minimum.* In this case,

$$\left(\frac{d\Phi}{dx}\right)^2 + \left(\frac{d\Phi}{dy}\right)^2 + \left(\frac{d\Phi}{dz}\right)^2 = \frac{v^4}{4} = (H - V)^2,$$

and

$$\frac{d\Phi}{dH} = s.$$

Again, if we take

$$\Phi = \int F(x, y, z)\, f(v)\,ds$$

$$\frac{d\Phi}{dx} = \frac{Ff}{v}\frac{dx}{dt}, \text{ \&c., and } \frac{d\Phi}{dH} = \int F f'(v) dt.$$

Hence, if
$$F(x, y, z) = \frac{\text{Constant}}{f'(v)},$$

we have
$$\frac{d\Phi}{dH} = Ct,$$

so that there is an infinite number of values of the characteristic function, besides that of HAMILTON, which give the time through any arc of the orbit by their differential coefficients with respect to H.

27. Enough of this; I conclude with the remark that various investigations in Statics supply us with excellent examples in our subject.* Take the common catenary, for instance, its equation is found by the conditions

$$\int y ds = \text{minimum}, \quad \text{and} \int ds = \text{constant},$$

the axis of y being directed vertically upwards.

This gives
$$\delta \int (y + a) \, ds = 0.$$

Hence the catenary is the free path of a particle whose velocity is given by
$$v = C(y + a);$$
that is, if the force be in the direction of, and proportional to, the ordinate, and repulsive from the axis of x. In the same way we see that the catenary is the brachistochrone if the velocity be inversely as the distance from the axis; that is, if the force be attractive, and inversely as the cube of the distance from the axis.

* Compare THOMSON and TAIT's Natural Philosophy, §§ 581, 582.

XIV.—*On the Tertiary Coals of New Zealand.* By W. Lauder Lindsay, M.D., F.L.S., &c., Honorary Fellow of the Philosophical Institute of Canterbury, New Zealand.

(Read 20th February 1865.)

Coal deposits of Tertiary age have now been found more or less throughout the two great islands (north and south) of New Zealand. They are best known, and they have been chiefly worked, however, in the provinces of Otago, Nelson, Canterbury, and Auckland. Their apparent more meagre development in other provinces is probably simply due to the circumstance that the latter have not, as yet, been so thoroughly explored or so extensively colonised and peopled as the others. The explorations, however, of provincial and geological surveyors, of prospecting gold-miners and sheep-owners, and of other pioneers of civilisation, are daily adding to the number of the known coal-fields of New Zealand; and it is probably not going too far to assert, in general terms, that the whole area of its two great islands is studded over with coal-basins of various extent, depth, age, and quality of coal.

In *Otago* the following are the districts which possess coal-fields or basins of Tertiary age:—

1. District between Dunedin (the capital) and the Taeri plains; including especially what I may designate the *Saddlehill* or *Greenisland Basin;* and the Silverstream valley.
2. Tokomairiro valley; *a.* Upper (Woolshed), and *b.* Lower (Tokomairiro gorge).
3. Great valley of the Clutha river—
    - *a. Upper* (Dunstan, Kawarau, and Manuherikia districts; Cromwell, Clyde, and Alexandra townships).
    - *b. Middle* (Teviot, Tuapeka, and Waitahuna districts; Laurence and Wetherstone's townships).
    - *c. Lower* (Kaitangata and Coal Point).
4. Valley of the Upper Taeri and Shag river—
    - *a. Upper* (Mount Ida or Highlay district).
    - *b. Lower* (Shag or Vulcan Point).
5. Valleys or beds of the Waitaki and Waikawa rivers; Otepopo &c.; all mostly in the central and eastern districts.

These localities include, fortunately for the gold-mining interest, the great gold-fields of Tuapeka, Dunstan, and Mount Ida or Highlay; and, as a general rule, there are few of the gold-fields destitute of local deposits of Tertiary or brown coals. The discovery and rapid development of the rich and extensive gold-fields of Otago are the main causes why in this province the coals in question have been more largely worked than in any other part of New Zealand.*

Throughout the province of *Canterbury* similar coal-deposits occur, and more especially in—

1. The valleys or beds of the Selwyn, Upper Waikamariri, Rakaia, Rangitata, Ashburton, Northern Hinds, Potts, Tenawai, and other rivers.
2. The Malvern Hills, Mount Somers, Big Ben Range, Thirteen Mile Bush, &c.; all mostly in the central or eastern districts.

In the province of *Nelson*, Tertiary coal-deposits stretch along the—

1. *West coast*, from Cape Farewell with little interruption to the Grey river on the Canterbury boundary line—overlying unconformably the secondary Coal-measures of the Buller and Grey rivers.
2. On the *northern* and *north-western coasts*, they occur at Motupipi in Massacre Bay ;Ennerglyn, near the town of Nelson, &c.

In the province of *Auckland* they stretch along the—

1. *West coast* with little interruption from Kawhia to Hokianga (including Aotea, Raglan, Manukau, and Kaipara).
2. On the *east coast* they show themselves at Mongonui in the north, and further south at Wangeroa and Matakana, &c.
3. In the *interior* they abound in the Upper and Lower Waikato ; Waipa ; and Drury (or Hunua) districts.

Towards the close of 1861 I lived for three months in the centre of the *Saddlehill Coal Basin* in the immediate vicinity of Dunedin. Within a couple of miles of my residence (ten miles from Dunedin) an excellent artificial section of the brown coal strata was exposed in the Saddlehill Colliery, on the flank of the conical basaltic mass of Saddlehill (height 1565 feet). There is here a regular adit of considerable length, with relative tramways and other works. This colliery had been in successful, though limited, operation for several years; but at the period of my visit the superior attractiveness of the recently discovered gold field of Tuapeka had absorbed all available labour, and the works were consequently temporarily stopped. A semi-artificial section I also found in the immediate neighbourhood of my headquarters (the farm-house of Fairfield),—viz., in Abbott's Creek, where brown coal had also been worked for a very short time, and on a very limited scale. Natural outcrops or sections of similar strata I

* In all probability it will yet be found that the Tertiary coals of New Zealand are referrible to groups of *three distinct ages*,—corresponding so far to our Eocene, Miocene, and Pliocene subdivisions. Dr Hector already regards the Otago brown coals as of *three* distinct ages, and Dr Haast those of Canterbury as of at least *two*. Those which are mined as fuel are—for the most part at least—apparently referrible to the older or lower groups, or subdivisions, of the New Zealand Tertiaries.

found on Scrogg's Hill—a continuation southwards of Saddlehill—where they were included in a Government coal-reserve; in M'Coll's Creek, on the seaward base of Saddlehill; and at other points on the flanks or base of this basaltic nucleus. I was led, while on the spot, to the conclusion that the Trappean mass of Saddlehill, with adjacent minor eruptive Trappean masses, had burst through what had at one time been a continuous coal-bed or basin subsequent to the deposition of the latter; or, in other words, in newer Tertiary times. That the coal-bed in question is more or less continuous is so far proved by the fact, that since I left Otago no less than three large collieries have been established in the immediate vicinity of my former residence. The first of these—the Fairfield Colliery—was opened on the lands of Fairfield itself, very near the old workings of Abbott's Creek, which I frequently examined. The proprietor, my friend Mr Martin, late Member of the Provincial Council of Otago, tells me* there are here two main seams, the upper six and the lower four feet thick, with a "dirt-bed" between them. The other pits, all within a few hundred yards of each other, are the Shand and the Walton Park Collieries. The strata associated with the brown coal are mostly various shales; clays, bituminous, and arenaceous, some of them plastic, white, and micaceous; and sands or sandstones of various degrees of coarseness. The whole are overlaid by the newer Tertiaries so abundant in the district, consisting of variously coloured clays, sands, and conglomerates.

Daily pedestrian excursions during my residence in the Saddlehill district gave me frequent opportunities of studying all the natural and artificial sections of the brown coal strata of this basin, and of collecting hand-specimens of every quality of the coals so exposed. The latter were brought home, and a suite thereof submitted in 1862 to careful chemical analysis by Professor Murray Thomson, a Fellow of this Society, then an analytical chemist in Edinburgh, and now Professor of Experimental Science in the Thomason College, Roorkee, Bengal. The results of his analysis are embodied in Table II., and fully bear out the opinions to which, when in Otago,† I gave public expression, regarding the economical value of the Otago brown coals.

While in Otago I also visited the Kaitangata coal-field at the mouth of the Clutha, about 60 miles southward of Dunedin. Here there are works which were originated, and are now carried on, under the patronage of the Otago government. There are not only regular adits at the pit; but the pit is connected by means of a rail or tramway two miles in length, with a jetty on the Clutha

* Letters of January 1862 and September 1863.

† In a lecture on "The Place and Power of Natural History in Colonisation, with special reference to Otago (New Zealand)," prepared for and printed by the "Young Men's Christian Association of Dunedin," Dunedin, January 1862. Extracts therefrom reprinted in the "Edinburgh New Philosophical Journal" for April and July 1863.

Vide chapter on the "Geology of the Otago *Lignites*."

for the ready delivery of the fuel into small coasting traders for the Dunedin market.

At Coal Point, in the coast cliffs, two or three miles northwest of the mouth of the Clutha river, occur the best natural sections of the brown coal strata I saw in Otago. Here these strata consist of various seams of coal of different characters or qualities, separated by or associated with laminæ or beds of conglomerates, quartzose gravels, grits, and sandstones; clays, including fire-clay, pipe-clay, fine coloured clays, and carbonaceous and arenaceous clays, sometimes laminated; and carbonaceous and other shales. They contain various fossils, in the form chiefly of dicotyledonous leaves, fragments of lignite, stems of trees, and other plant impressions; as well as ironstone concretions and iron pyrites. Overlying the brown coal strata is a series of conglomerates, gravels, sands, and clays, generally more or less ferruginous; of newer Tertiary age; essentially identical with those overlying the brown coals of the Saddlehill district. These beds, too, contain nodules of clay ironstone, which are further scattered in all directions on the beach at the foot of the cliffs. Their appearance and position reminded me strongly of those of the carboniferous shales of Wardie. It does not appear that the stratigraphical relations of the Tertiary coals in other parts of Otago and New Zealand differ—save in minor and local details—from those of the brown coals of the Saddlehill and Kaitangata basins, as here roughly sketched.

Before leaving Otago I visited the Tuapeka gold-field, where I had an opportunity of seeing the relations of the brown coal, which is now being worked at Laurence, Wetherstone's, and Waitahuna. From Otago I passed northwards to the provinces of Nelson and Auckland. In neither of these, however, had I any opportunity of inspecting the Tertiary coal strata *in situ*. My examinations were confined to the suites of coal specimens contained in the Provincial Museums of these provinces, or in the hands of coal proprietors or amateur geological collectors. Unfortunately, the value of the Museum series—which, in the Auckland Museum at least, is somewhat extensive—is seriously detracted from by their careless nomenclature and classification, and by the improper or defective method of exhibition. The plan adopted in the Museum of Science and Art, Edinburgh, and in other of our own national Museums, should forthwith be copied in these, as well as other, colonial museums,—viz., to accompany each specimen with a full descriptive label, setting forth not only its locality, and date and circumstances of collection, but its chemical composition; and to classify it after some uniform plan, geological or chemical. Were this done, such series of specimens could not fail to acquire a great local as well as general value, instead of being, as at present, little more than a mass of lumber.

In the Great Exhibition at London in 1862 (New Zealand department), I was further enabled to examine a pretty complete series of all the New Zealand coals known up to that date,—especially those of the North Island, and northern portions of the South Island. Desirous of comparing the New Zealand *Tertiary* coals with local coals of greater age and superior quality, as well as with coals of all ages from every part of the world, I availed myself of the opportunities presented by the Exhibition in question; the Australian Museum, Sydney; the Museum of Economic Geology and British Museum, London; the Museums of Economic Botany at Kew and Edinburgh; the Museum of Science and Art, Edinburgh, and other minor museums, British or colonial.

I have selected the *Brown Coals* of *Otago*, as representative of the Tertiary coals of New Zealand, for three reasons,—viz., that I am more familiar with them; that they are the best known and most extensively worked in the colony; and that their stratigraphical relations and chemical constitution appear *essentially* those of all other New Zealand tertiary coals.

The general results of my inquiries as to the geology and chemistry of the Tertiary coals of New Zealand I have given in the "Abstract" [published in the Society's "Proceedings,"] of the paper which I had the honour of presenting to the Royal Society on 20th February last. The only section of the said paper which it seems desirable here to print in detail, is the tabular exhibition of the physical characters and chemical constitution of the brown coals of Otago; and as standards of comparison of certain other or older coals of other provinces of New Zealand or of Australia.

Table I. refers exclusively to specimens collected by the Geological Survey of Otago, or submitted to analysis by the chemist attached to that survey.* The majority of the Otago specimens are from the same collieries or localities from which my own collections were made. But inasmuch as the collections of the Geological Survey were made subsequently to mine, and at a period when the various works were in full operation, the survey specimens are likely to be of a quality superior to mine, which were necessarily, to a great extent, surface specimens. Moreover, the mode and scope of the analyses differ somewhat in the two tables; wherefore, and on other accounts, Table I. is to be regarded as the natural complement of Table II.†

Table II. refers exclusively to specimens collected by myself in Otago in the course of my various excursions in 1861. The chemical analyses were made, as before stated, by Professor Murray Thomson.

* It is constructed chiefly from materials contained in a Report by the Government geologist of Otago, Dr Hector—a Fellow of this Society—of date 13th April 1864.

† The natural order or sequence of these Tables has been reversed to suit the requirements of the printer.

TABLE I.

I. TERTIARY (CAINOZOIC) COALS OF OTAGO.

Locality.	Physical Characters of Coal-Specimen Analysed.	Character of Powder.	Character of Ash.	Character of Coke.[2]	Specific Gravity.[1]	Percentage[1] of Coke.	Fixed Carbon.	Volatile Hydro-carbons.	Ash.	Sulphur.	Water.[3]
I. CLUTHA—COAL POINT.	1. COMMON BROWN COAL.—*Fracture* moderately bright—conchoidal; becomes dull on exposure; *streak* deep brown; *burns* freely with a very slight *odour;* contains laminæ of *Jet.*	Deep brown.	White or light grey.	Dull.	1280	43·28	38·99	37·28	5·46	...	16·10
Do. *Top* of Main Seam.	2. BROWN COAL, apparently of very superior quality.—Distinctly laminated; traversed irregularly by thin plates of *Jet;* contains abundance of *Mineral Resin,* scattered through its substance in small specks, or occurring in large nodulated masses; also *Iron Pyrites* in its cracks.	Dull brown.	White.	Do.	1258	42·79	43·23	33·87	3·36	3·63	12·94
Do. *Middle* of Main Seam.	3. Quality and characters as in No. 2.	Do.	Do.	Do.	1267	41·10	39·12	35·67	4·43	...	16·35
Do. *Bottom* of Main Seam.	4. Blacker; more compact and massive; *Fracture* conchoidal in all directions; *streak* chocolate brown; possesses higher illuminating power than Nos. 2 and 3; and contains fewer impurities, with exception of nodules of *Iron Pyrites.*	Light chocolate brown.	Do.	Do.	1275	40·46	39·71	32·24	7·28	3·85	16·96
Do. Associated Sandstones.	5. *Jet;* bright; lustrous; compact; streak black; *burns* with very great difficulty, *caking* slightly.	Black.	Light red.	Iridescent.	...	50·35	39·04	27·61	11·31	...	16·43
II. GREENISLAND.	6. COMMON BROWN COAL.—Dull; earthy; very moist when first extracted from the mine; absorbs water readily, with a crackling noise; laminated; *fracture* subconchoidal; no trace of woody structure; *streak* dull, blackish-brown; contains thin plates of *Jet;* ignites slowly, and rather smoulders than burns; forms a useful fuel, when *mixed with a better class of coals.*	Dark brown.	Very light; almost like that of wood—Colour light buff.	Slightly metallic or iridescent.	1256	39·72	36·83	33·83	4·32	2·24	19·33
Do. from another Pit.	7. Quality and characters as in No. 6.	Dull brown.	Light buff.	Dull.	...	...	43·12	34·37	2·25	...	20·26
III. SADDLEHILL.	8. EARTHY BROWN COAL.—Soft; dull; very moist; structure homogeneous; no trace of vegetable tissue; *streak* dark brown; in burning gives off a fœtid smell.	Do.	Very light. — Colour pure white.	Do.	1294	38·97	39·07	35·93	2·78	2·22	15·95
IV. SHAG POINT.	9. PITCH COAL.—Brownish black; very compact; *fracture* conchoidal and splintery; *lustre* fatty or resinous; *streak* lustrous; does not absorb water; does not soil the fingers; *burns* with a rich oily flame and a slight odour; on distillation at high temperatures yields a rich *gas,* and at lower ones a large amount of *oils.* Probably one of the oldest tertiary coals of Otago. The freest burning quality of Brown Coal yet found (1864) in that province. Its characters correspond with Dr Percy's definition of "*Pitch Coal:*" and it holds the same relative position among the Brown Coals of Otago that Cannel does among the household coals of Britain.	Dull brownish black.	Bulky; light; argillaceous —Chocolate brown.	Light; dull or semi-metallic.	1258	50·68	41·83	37·31	9·25	4·78	10·91
V. TOKOMAIRIRO.	10. DULL BROWN COAL.—Physical characters resemble those of Clutha and Greenisland Coal. *Streak* brown; contains occasional *quartz* pebbles.	Dull brown.	Light grey.	Dull or semi-metallic.—Does not cake.	1299	41·38	39·93	37·55	5·91	...	11·40
VI. WAITAHUNA.—FLAT OR JUNCTION.	11. LIGNITE, or WOODY BROWN COAL.—Dull; friable; slightly laminated; contains a considerable amount of *ligneous tissue;* also *mineral resin* in small masses; *burns* slowly, like turf; with a heavy, fœtid odour. Perhaps of more recent geological age than the Brown Coals proper.	Light brown.	Light buff.	Imperfect; lustrous; slightly iridescent.	...	43·74	35·20	37·63	11·15	6·21	11·06

Preservation Inlet; South-West Coast.	splintery, cubical, rarely conchoidal; *streak* black, shining; approaches in character the coals of the West Coast of Nelson and Canterbury, and of Newcastle, New South Wales; has a greater resemblance to British palæozoic coal than any other hitherto found in Otago.	occasional faint brownish tinge.		changed; does not cake.						
II. NELSON. 1. Pakawau (or Seaford); Golden (or Massacre) Bay; North-West Coast.	13. Pitch Coal.—Pitch black; compact; heavy; *burns* with a large amount of flame and heat, and without disagreeable odour (Hochstetter). Lithological characters those of true coal. Quality of specimens exhibited to me, while at Nelson, by Mr Lewthwaite, proprietor of the colliery, seemed inferior to that of Grey River coal; but specimens in the Great Exhibition (London, 1862), labelled *Gas Coal*, appeared of superior quality.	Brownish black.	Light red (Hector); Clean and white (Hochstetter)	Semi-metallic.	1330	...	50·10	38·08	8·26	1·04	3·56
2. Buller River; West Coast.	14. Pitch Coal.—The only specimens I have seen were some water-worn fragments from the river's bed in the Exhibition of 1862 (London; Nelson section of the New Zealand department). They had the external characters of No. 13.	Do.	White.	Do.	1255	...	60·72	33·76	4·62	1·85	0·87
3. Grey River; West Coast, at or near River's mouth.	15. Pitch Coal.—By far the best quality of coal I saw in New Zealand; has all the lithological attributes of a true coal, and is quite comparable, *quoad* external characters, with the best English Newcastle qualities.	Black.	Light brown.	Cakes.	1330	...	62·37	29·44	6·20	...	1·99
	Mean of the foregoing 4 Classes of *Semi-Bituminous* or *Bituminous Coals*, ...	...	...	...	1305	64·43	58·15	32·19	6·05	1·44	3·37

III.—PRIMARY (PALÆOZOIC) COALS OF AUSTRALIA.

I. NEW SOUTH WALES. Newcastle; Hunter River.	16. Pitch Coal: with all the lithological characters of British Palæozoic Coals. Of true carboniferous age (according to Clarke, Keene, Daintree, and others).	Dull black.	Light buff.	Semi-metallic: cakes.	1385	...	56·28	31·68	9·09	...	2·94

ABSTRACT.

I. Tertiary (Cainozoic) Coals—Otago, ...	Maximum,	1308	50·98	46·77	46·77	12·13	6·21	20·26
	Minimum,	1229	37·45	33·13	27·61	1·92	2·22	10·91
	Average,	1273	43·24	39·64	34·84	6·13	3·82	15·24
II. Secondary (Mesozoic) Coals—Nelson and Otago, ...	Maximum,	1330	68·67	64·43	38·08	11·20	1·85	15·60
	Minimum,	1250	58·85	50·10	25·55	1·98	1·04	0·70
	Average,	1305	64·43	58·15	32·19	6·05	1·44	3·39
III. Primary (Palæozoic) Coals—New South Wales, ...	Maximum,	...	...	57·20	31·78	10·05	...	3·00
	Minimum,	...	...	55·36	31·59	8·14	...	2·88
	Average,	1385	...	56·28	31·68	9·09	1·60	2·94

[1] The *mean* of several examinations of, or experiments on, a variety of specimens. Up to October 1864, the Geological Survey of Otago had made fifty analyses of its *Brown Coals* alone.
[2] *None* of the Tertiary Coals of Otago, as here analysed, *cake*.
[3] Water *of constitution* as contradistinguished from *accidental water* (that given off at ordinary temperatures). Dr Hector informs me that he regards the Otago Brown Coals as of three distinct ages, the age being in each case indicated by the relative percentage of water of constitution. (*Letter*, October 1864).
[4] *Precise age* for the most part as yet undetermined.

TABLE II.

Locality.	Physical Characters of Coal—Specimen analysed.	Quantity[1] per Ton.						Percentage.[1]			
		Coke.	Carbon in Coke.	Ash.	Volatile Matter.	Gas.	Oil.	Coke.	Carbon in Coke.	Volatile Matter.	Ash.
		lbs.	lbs.	lbs.	lbs.	cub. ft.	gals.				
	I. TERTIARY COALS OF OTAGO.										
I. SADDLEHILL COLLIERY—on the property of Mr Jaffrey of Saddlehill Park, about 10 miles southward from DUNEDIN, on the Great South or Invercargill Road.	1. From Main Adit. COMMON BROWN COAL.—Brown; compact; massive; earthy; comparatively heavy;[2] brittle; *lustre* dull; *fracture* uneven; no vegetable structure apparent to naked eye; cracks on exposure as if baked, apparently from desiccation[2] and shrinkage; subsequently splinters into fragments or "small."	1021	659	362	1217	3175	...	45	29	54	16
	2. BROWN COAL.—Blackish-brown; contains occasional *quartz* pebbles or grains; physical characters otherwise as in No. 1.	972	887	85	1267	2954	17·	43	39	56	3
	3. From Roof of Main Adit. Transition state between BROWN COAL and LIGNITE.—Obscurely fibrous or ligneous; laminated; associated with arenaceous shales.	1262	681	586	977	3024	7·4	56	30	43	26
	4. LIGNITE.—Consists of, or contains, much vegetable débris; very friable; bears a close resemblance, save in colour, to the *Mineral Charcoal* of the Scotch coal measures.	1046	964	82	1194	3175	15·5	46	43	53	3
	Mean of four foregoing classes of Coal, which include *Brown Coal, Lignite,* and their transition states, ...	1075	797	278	1163	3082	13·3	47	35	51	12
	5. From Roof of Main Adit. CARBONACEOUS SHALE.[3]—Largely intermixed with arenaceous clays; very impure; not used as fuel.	1099	733	366	1140	1889	...	48	32	50	16
II. SADDLEHILL—M'Coll's Creek, on its seaward base: near the Coast, and about 10 miles southward of Dunedin.	6. BROWN COAL.—Laminated; earthy; ochrey; friable; of light specific gravity; resembles more than do most of the Otago Tertiary Coals, the *Brown Coals of Bohemia.*	943	632	303	1296	3931	...	42	28	53	13
III. ABBOT'S CREEK[4]—on the Kaikorai Stream, Chain Hills, Greenisland, about 6 miles southward from Dunedin, on the Great South Road.	7. Transition state between BROWN COAL, LIGNITE, and JET: partaking of the lithological characters of all. Black; *texture* fibrous, partly woody; of light specific gravity; contains occasional *quartz* pebbles, and *iron pyrites;* associated with *pyritous shales,* which yield on decomposition with water considerable quantities of *sulphate of iron.*	1068	737	331	1170	3024	...	47	32	52	14
IV. MOUTH OF THE CLUTHA.—KAITANGATA COAL-FIELD—Lewis' Colliery, Coast Cliffs, south of Coal Point, about 60 miles southward of Dunedin.	8. Transition state between LIGNITE, BROWN COAL, and PITCH COAL.—Black; compact; *texture* sub-fibrous; *lustre* dull; *fracture* sub-conchoidal; of light specific gravity; graduates into a *Pitch Coal,* resembling in external characters the *Scotch Cannels* or the Mesozoic coals of Nelson and Canterbury.	861	787	73	1378	3250	...	38	35	61	3
	Mean of the last three groups of specimens, which include *Brown Coals* and *Pitch Coals,* and their transitions into *Lignite* and *Jet,*	957	718	235	1281	3401	...	42	31	55	10
	Mean of seven varieties of Otago Tertiary Coals, including the transitions between *Jet* and *Pitch Coal,*	1016	758	257	1222	3241	...	44	33	52	11
	ABSTRACT.										
	Maximum, ...	1262	964	586	1378	3931	17·0	56	43	61	26
	Minimum, ...	861	632	73	977	2954	7·4	38	28	43	3
	Average, ...	1016	758	257	1222	3241[5]	13·3[5]	44	33	52	11
	II. TERTIARY COALS OF NELSON.										
V. ENNERGLYN—up the Waimea Valley, on a hill about 4 miles south of the town of NELSON; Jenkins' Colliery.	9. PITCH COAL: having the external characters of the "*Glanzkohle,*" of the Germans and of the English "Newcastle." Black; *lustre* vitreous or splendent; *fracture* conchoidal. Much used in the town of Nelson as a domestic coal; but not used for steam purposes, avowedly on account of its large proportion of *ash.*	973	911	62	1266	4400	...	43	40	56	2

([1]). In round numbers (without decimals).

([2]). The Otago Brown Coals generally contain, when first extracted from their beds, from 20 to 30 per cent. of *water,* of which sometimes as much as one-half is given off in the process of natural desiccation.

([3]). Among these carbonaceous sandy shales I found *Lignite,* closely resembling the Icelandic "*Surturbrand.*" It could only be distinguished from recent or fresh wood by its greater darkness, density, and weight. In other localities, *e. g.,* near Coal Point, I have found the same form of Lignite, always in strata *overlying* the Brown Coal proper, and referrible, I suspect, to the *Newer or Upper Tertiaries.*

([4]). In the immediate vicinity of the Fairfield, Shand's, and Walton Park Collieries, now in successful operation, and probably part of the bed on which these works are situated.

([5]). As a standard of comparison, I append the following Analyses attached to certain specimens of

Description of Coal.	Quantity per Ton.	
	Gas, in cubic feet.	Crude Paraffin Oil, in gallons.
1. Torbanehill Mineral, Boghead, Linlithgowshire,	15,000	120
2. Lesmahagow, Duke of Hamilton's Pit, . .	12,000	...
3. Brown Cannel, Methil, Fifeshire, . . .	10,000	90
4. Cappeldbrae, do. . . .	...	80
5. Cannel, Rochsoles, Lanarkshire, . . .	...	72
6. Overton, do. . . .	...	64
7. Black Cannel, Methil, Fifeshire, . . .	...	50

XV.—*On Variability in Human Structure, with Illustrations, from the Flexor Muscles of the Fingers and Toes.* By WM. TURNER, M.B. (Lond.), F.R.S.E., Senior Demonstrator of Anatomy in the University of Edinburgh.

(Read 19th December 1864.)

Deviations from the usually described arrangements of the parts, of which the human body is composed, have from time to time attracted the attention of the anthropotomist. In many anatomical text-books, as well as in sundry memoirs specially devoted to the subject, numerous examples of such variations have now been recorded. To the scientific anatomist these have always had a certain value, but of late years this department of anatomical inquiry, more especially in connection with variations in the muscular system, has had additional importance and interest attached to it, on account of the attention which has been directed to the correspondence, or want of correspondence, in the muscular arrangements in man and the other mammalia, more particularly the apes.

Into this aspect of the question it is not my intention to enter in this communication. My object on this occasion is rather to compare certain structures in one human body with corresponding structures in others, and to point out the extent of variability which may occur in similar parts in different individuals.

Every one is conscious that of the multitude of individuals he may meet with in the course of a day's experience, no two are alike. Leaving altogether out of consideration all mental differences, each possesses some peculiarity of form and gait which enables him at once to be distinguished from those around him, and that these external manifestations of variability are in their prominent features correlated with internal structural differences will, I suppose, be generally admitted. That diversities in the shape of the skull, for example, occasion corresponding diversities in the form of the head and face, so as to impart to them characters diagnostic not only of the race, but of the individual, have been recognised from the time of BLUMENBACH and CAMPER. But the osseous is not the only organic system in which distinct evidence of structural variability may be traced; the muscular, vascular, nervous, and visceral systems all exhibit it. In some cases, undoubtedly, the departure from what may be termed the standard method of arrangement, as set forth by descriptive writers, is greater than in others, but evidence of its existence to a greater or less degree in every individual may be obtained not only by the examination of the systems taken as a whole, but of the separate structures of which they are composed. But though some of the best marked examples of internal structural variations are correlated with corresponding variations in the external configuration of the body, yet there are a large number which, either from their minuteness, or from being situated in the deeper seated parts, give no sign externally, and to distinguish them requires close and careful dissection. For many years I have been in the habit of preserving a record of the most remarkable "irregularities," as they are often called, which have come under my notice; and I could cite many cases from my note-book in which, in the course of dissection, variations in the different organic systems were noted. During the present winter session, for example, four arms have been met with in which that very curious process of bone, known as the supra-condyloid process, projected from the inner part of the shaft of the humerus. In all, this process was connected to the inner condyle by a ligament. The process, the ligament, and the shaft of the humerus, covered by the brachialis anticus muscle, formed collectively the boundaries of a supra-condyloid foramen. In all the median nerve went through the foramen. So far these limbs, though varying greatly from the usual arrangement of parts in the human upper arm, corresponded closely with each other, but in other respects they differed considerably amongst themselves. In three specimens the pronator radii teres muscle arose from the process and the ligament connecting it to the condyle; in the fourth the pronator muscle did not arise from these structures, but the ligament gave origin to some of the fibres of the brachialis anticus muscle. In one specimen the brachial artery, after giving off an accessory radial artery high up in the limb, accompanied the median nerve through the supra-condyloid foramen; in another the brachial artery, after giving off its ulnar branch of bifurcation high up in the limb, also passed along with the median nerve behind the process; in the third, the brachial artery pursued its usual course along the inner margin of the biceps to the bend of the elbow previous to its bifurcation, and sent simply a small branch through the foramen along with the median nerve; in the fourth not only was the brachial artery not deflected from its customary course, but it did not even send a small branch through the foramen, through which, consequently, the median nerve proceeded unaccompanied by any vessel.* In three of the specimens, also, a muscular slip arose along with the

* The four cases described in the text of the occurrence of a supra-condyloid foramen in the human upper arm are not the only specimens which have come under my notice in the dissecting-room. In former years I had observed five specimens, in three of which both brachial artery and median nerve passed through the foramen, in the remaining two I had unfortunately not preserved a note of the arrangement. But by far the most complete account of the anatomy of the supra-condyloid foramen which has yet appeared has been drawn up by Professor WENZEL GRUBER in an elaborate memoir presented to the Imperial Academy of St Petersburg. Vol. viii. 1859. This anatomist has collected from the works of previous writers, as well as from material which has come under his own observation, sixty-two cases in which this foramen was noticed in the human body, and in which there was at the same time a greater or less amount of variation in the arrangement of the pronator teres, the median nerve, the brachial artery or some of its branches. One of the chief features of interest connected with the supra-condyloid foramen is the circumstance that it furnishes, as an occasional occurrence in human structure, an approximation to an arrangement

flexor sublimis digitorum from the coronoid process of the ulna. This slip terminated on a tendon, which in one specimen became blended with the tendon of the flexor profundus passing to the middle finger; in another with the tendon of the same muscle going to the ring finger; and in the third with the tendon going to the little finger. In the fourth specimen the flexor sublimis was not connected to the flexor profundus by any intercommunicating structures. In one specimen the middle, ring, and little finger tendons of the flexor profundus were connected together by a network of intertendinous bands; in another such bands only connected the middle and ring finger tendons of that muscle; in the remainder these structures were absent. In one of the specimens the abductor pollicis received a distinct slip of origin from the styloid process of the radius, whilst in another the abductor minimi digiti received a slender muscular slip, which arose in the lower part of the forearm from an accessory palmaris longus tendon.

It would be quite possible for me to multiply examples to serve as additional illustrations of variations occurring in several of the most important organic systems in the same body,—variations so well marked, indeed, that though, as in the cases above cited, it is probable no outward evidence of their existence was manifested, yet they furnished the individuals in whom they occurred with characters as distinctive as any peculiarities of external configuration. Hence we may conclude that in the development of each individual a morphological

which is very frequently met with in some of the Mammalia. For there has now been recorded a considerable number of instances in which a distinct canal, generally with bony walls, existed in this locality in various Quadrumana, in Galeopithecus, in the Edentata and Monotremata, in many Carnivora, Marsupialia, Rodentia, and in some of the Pinnepedia; whilst it would appear to be absent in the Ruminantia, Solidungula, Multungula, and Cetacea. But though the canal would seem to occur almost constantly in all the genera of some orders and families of the Mammalia, yet it by no means follows that in other orders and families, though it may occur in one genus, that it exists in all, or even though it may occur in one species of a genus, that all the species of the same genus should possess it. Thus, as Professor OWEN has shown (Article *Marsupialia* in "Cyclopædia of Anatomy and Physiology"), whilst it exists generally amongst the Marsupialia, yet it is absent in Dasyurus and Thylacinus; and though most of the species of Phalangista possess it, yet it does not exist in *Phalangista Cookii;* and whilst GRUBER saw it in *Erinaceus auritus*, he did not find it, and I have not seen it, in *Erinaceus europæus.* In the Pinnepedia also it has been described by various anatomists as present in the *Phoca vitulina*, and GRUBER has seen it in other species of the same genus. In a common seal which I dissected, I found that it only transmitted the median nerve, neither the brachial artery, nor any of its branches passed through it; in the Walrus (Trichechus), however, anatomists agree in stating that it does not occur, a fact which I have observed in three skeletons of that animal which have come under my observation. Again, in some of the Mammalia variations in its occurrence take place in individuals of the same species, a circumstance which has been noticed not unfrequently amongst the Quadrumana, though it has not as yet been seen, I believe, in the humeri of any of the Anthropoid Apes. Thus, whilst TIEDEMANN describes it as present in *Cercopithecus sabæus* and *Cercocebus fuliginosus*, MECKEL and OTTO state that it is wanting in those species; a discrepancy of statement which may probably be explained by regarding the arrangement as a variety present in one individual but absent in another. Of the skeletons of the Quadrumana personally examined, I have found the foramen absent in two specimens of the Orang, in a Chimpanzee, in two specimens of the Gibbon, in *Cercocebus fuliginosus* (agreeing thus with MECKEL and OTTO), in *Macacus cynomolgus*, in *Cynocephalus maimon*, *Hapale jacchus*, and *Ateles paniscus;* whilst I have found it present in a species of Cebus, and in the prosimian *Stenops tardigradus.*

specialisation occurs both in internal structure and external form, by which distinctive characters are conferred, so that each man's structural individuality is an expression of the sum of the individual variations of all the constituent parts of his frame.

But it is not essential that, for the demonstration of this specialisation of structure in the individual, we should extend our inquiries over all the organic systems. Any one, if carefully examined, will afford us sufficient evidence of its existence. The muscular system is the one I have especially selected for illustration. There are some parts of this system in which, from the mode of arrangement of single muscles, and from the manner in which they are collected into groups, we are enabled to study more precisely than in other localities the extent of variation which is permitted, and the various forms which it assumes. None are better fitted for this purpose than the flexor muscles of the fingers and toes; for not only can we define with great exactness the arrangement of these muscles and their tendons, but we can employ in connection with them a method of description precise enough to convey a conception, not only of the stronger and best marked varieties, but of the more minute deviations from their usually recognised disposition. During the past twelve months, I have made a series of special dissections of these groups of muscles, and I shall now record the general results which I have arrived at in the examination of these parts. It must be understood that all the dissections were made on the bodies of the inhabitants of these islands, natives of either Great Britain or Ireland.

The flexor muscles in the forearm and hand, to which my attention has especially been directed are the flexor longus pollicis, the flexor sublimis digitorum, the flexor profundus digitorum, and the lumbricales. The long flexor muscles exhibited numerous variations in their bulk, in the extent of their attachment to the bones of the forearm (the extent of the radial origin of the superficial flexor was especially variable), and to the interosseous or other fibrous membranes from which they arose. The superficial and deep flexors of the fingers also varied as to the mode in which they divided into their terminal bundles; in some cases the division took place lower down in the forearm than in others. This was especially the case with the deep flexor, in which it was not unfrequent to see the separation between the more internal of its terminal tendons still incomplete at the carpal end of the forearm, or beneath the annular ligament. In one specimen in my possession, the muscle divided into five bundles, two of which afterwards united to form the tendon for the ring finger.* But, in addition, other variations were met with of a more remarkable

* Multiplication of the bundles of this muscle has been recognised by ARNOLD, HENLE (Muskellehre, p. 196, 1858), and THEILE (Traité de Myologie), 1843, p. 246. THEILE also states that it sometimes receives a special head of origin from the inner condyle of the humerus; and THEILE, HALLETT (Ed. Med. and Surg. Journ. vol. lxxii. p. 12), and HENLE state that it sometimes receives fibres from the radius.

character. A more clear conception of their nature may perhaps be formed if we conceive the long flexors of the digits as composed of muscles situated on two planes, a superficial and a deep, and then bear in mind that both sets of muscles are subdivided into bundles, each of which terminates in a tendon possessing a distinct attachment to its proper digit. Now, between the different subdivisions of the muscle or muscles, situated on the same plane, and between the muscles situated on different planes, tendinous or musculo-tendinous bands not unfrequently proceed so as to connect them together. Thus, whilst it is customary to consider the flexor sublimis as dividing into four distinct bundles, each ending in a tendon, I not unfrequently saw a tendinous or musculo-tendinous slip proceed between adjacent bundles, and keep up a lateral communication between the divisions of the muscular mass situated on the same superficial plane. In a similar manner, the divisions of the muscular mass situated on the deeper plane were not unfrequently connected together by lateral bands. In the flexor profundus digitorum these lateral connecting bands presented various arrangements in different individuals. Sometimes the three inner tendons were closely tied together in the forearm, either by simple bands passing from one to the other, or by a more complicated reticular structure. At others only the two inner tendons; at others again the tendons for the middle and ring fingers were intimately connected (fig. 1), whilst the little and index tendons were quite free. As a rule, indeed, the index tendon appeared to be less liable to form a connection with the tendon of the same muscle than was the case with the other subdivisions of the flexor profundus. But on the other hand, I saw several specimens in which the index division of the deep flexor was intimately connected to the flexor longus pollicis,* a junction of considerable interest, as it approximates in their arrangement these muscles in the forearm and hand with the flexor hallucis and flexor communis digitorum in the foot. The nature of this union varied considerably in different specimens. In some it consisted of a muscular bundle, passing obliquely downwards from the fleshy part of the flexor of the thumb to the fleshy part of the index division of the deep flexor; in others it consisted of a musculo-tendinous slip proceeding obliquely downwards from the muscular part of the former to the tendon of the latter (fig. 1); and in some of these

Fig. 1.†

* Various anatomists have recognised the occasional connection of these tendons, without, however, specialising its different forms. See THEILE, p. 246; M'WHINNIE, Lond. Med. Gaz., vol. xxxvii. p. 191; HENLE, p. 196; WOOD, Proc. Roy. Soc. of London, p. 301, 1864. I am disposed to regard the connection in one or other of its forms as more common than is usually supposed.

† Fig. 1, *t*, flexor longus pollicis; *p*, flexor profundus digitorum. It shows the connection of the index tendon of the latter muscle with a strong musculo-tendinous band from the former, also the close union for some distance of the middle and ring-finger tendons of the deep flexor. This, and the other illustrative figures, have been drawn from the dried preparations of my dissections by my pupil, Mr RICHARD CATON.

cases the connecting slip was in great measure, though not altogether, formed of the fibres of the rounded head of the flexor longus pollicis, which arose from the coronoid process of the ulna. In one case the connecting slip received almost one-half the fibres of the long flexor muscle, a specimen which illustrates how large and important this intermuscular tendon may at times become. In one very remarkable specimen the bond of union passed in the opposite direction from those above described—viz., from the index tendon of the flexor profundus to the tendon of the flexor longus pollicis (fig. 2).

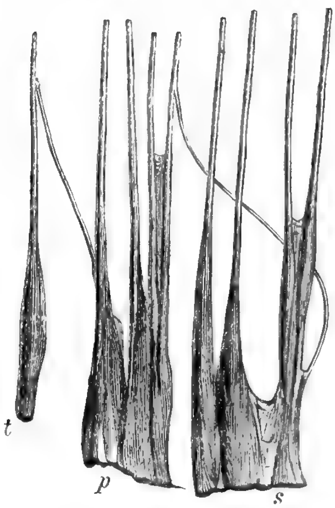


Fig. 2.*

Amongst the intermuscular structures which not unfrequently connect together the superficial and deep flexor muscles of the forearm, I am disposed to place that rounded musculo-tendinous band, which is so often met with, as a second head of origin of the flexor longus pollicis, for it arises along with the flexor sublimis from the coronoid process of the ulna, and ends inferiorly in the inner part of the long flexor of the thumb. But the superficial is also not unfrequently connected to the deep flexor of the fingers by intermuscular bands. For I have frequently seen a slip of muscle arise from the coronoid process, along with, and apparently forming a part of, the flexor sublimis, which, after it became tendinous, blended in five specimens with the tendon of the flexor profundus going to the little finger (*e.g.* fig. 2), in one with the tendon passing to the ring-finger, in four with the tendon of the same muscle going to the middle finger, and in one it divided into three slips which joined the deep tendons for the middle, ring, and little fingers. The blending usually occurred opposite, or slightly below, the carpal articulations.†

The lumbricales muscles exhibit many forms of variation in size, number, extent, surface of origin, and mode of insertion; but as both Theile and Henle have entered fully into these varieties, I need do no more than state that I have seen, in addition to most of the forms which they have described, a variety in which an accessory first lumbricalis arose tendinous from the flexor sublimis.

* Fig. 2, *t*, flexor longus pollicis; *p*, flexor profundus digitorum; *s*, flexor sublimis digitorum. The connection of the first and second muscles by a tendinous band passing from the index tendon of the latter to the long flexor of the thumb, is shown; also a tendon connecting the ulnar side of the superficial with the tendon of the deep flexor for the little finger; also a close connection low down the limb between the ring and little finger tendons of both the superficial and deep flexor muscles.

† The presence of slips proceeding between the flexor sublimis and F. profundus, though without precise statement as to their connections, has been recognised by Cowper (Myotomia reformata), Theile, and Wood.

I may in this place also refer to an arrangement which I saw on one occasion in the left forearm. A slender fasciculus of muscular fibres proceeded from the flexor sublimis immediately to the inner side of the palmaris longus. It ended on a tendon which passed beneath the palmaris, and joined the tendon of the supinator radii longus at the lower end of the forearm.

After a course of about two inches it became muscular, and then ran parallel to the first lumbricalis, and was inserted along with it.

The flexor muscles of the toes, the arrangements of which I have more especially studied, were the flexor brevis digitorum, the flexor communis digitorum, the flexor longus hallucis, the flexor accessorius, and the lumbricales,—an important group of muscles, the different members of which are more or less intimately related to each other in the sole of the foot. In order to form as precise a conception as possible of their mode of arrangement in the foot I carefully dissected fifty specimens, taking them without selection from the subjects which came in my way in the ordinary course of my anatomical work, so that the variations described must not be regarded as unusual or abnormal forms. The results I have arrived at differ in many respects from the descriptions of these muscles usually given in treatises on anatomy. Of these fifty specimens no two were exactly alike, so that it would be necessary, in order properly to bring out the extent of individual variation which they presented, that each should have a separate description; but as this would be tedious both to writer and reader, it may suffice if I adopt some method of arrangement which may exhibit their most important variations.

In all the specimens the tendon of the flexor longus hallucis gave off, in the sole of the foot, a slip or band which connected that tendon either to one or more of the subdivisions of the flexor communis digitorum, or in part to that tendon, and in part to the flexor accessorius. In its size this connecting slip varied somewhat, and though at times flattened and membrane-like, yet was mostly in the form of a rounded band. In every specimen it took a more or less important part in the formation of the deep flexor tendons for one or more of the four outer toes. In eleven specimens it ended solely in the deep flexor tendon for the second toe; in twenty specimens it bifurcated and ended in the deep flexor tendons for the second and third toes; in eighteen specimens it trifurcated, and ended in the deep flexor tendons for the second, third, and fourth toes; in one specimen it divided into four parts, and ended in the deep flexor tendons for the four outer toes.

Of the eleven specimens in which the connecting band went solely to the second toe, it formed about one-half the deep flexor tendon for that toe in four cases (fig. 9), the remaining half being formed partly by the flexor communis and partly by the flexor accessorius. A much larger proportion than one-half in six cases (fig. 11); and in one case it and the flexor accessorius together formed the whole of the deep flexor tendon for the second toe, in the construction of which the flexor communis did not consequently enter (fig. 3).

Of the twenty specimens in which the connecting slip went to the second and third toes, it contributed a larger share to the second than the third toes in twelve specimens, in one of which it formed, with the addition of a few fibres from the flexor accessorius, the whole of the deep flexor tendon for the second toe;* it was divided almost equally between the two in six specimens (fig. 4); and in two specimens it and the flexor accessorius together formed almost the whole of the deep tendons for these toes, the share taken in their construction by the common flexor being limited to a few fibres (fig. 5).

Of the eighteen specimens in which the connecting slip went to the second, third, and fourth toes, it contributed a larger share to the second than to either

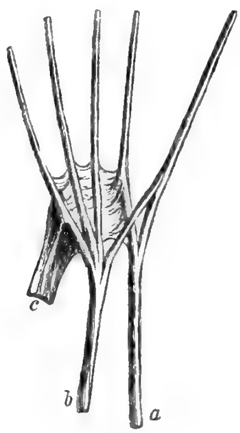


Fig. 3.†

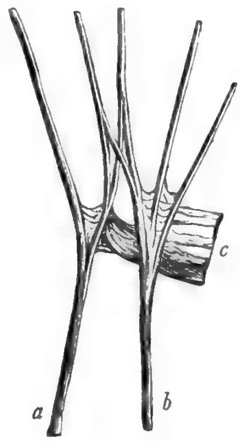


Fig. 4.

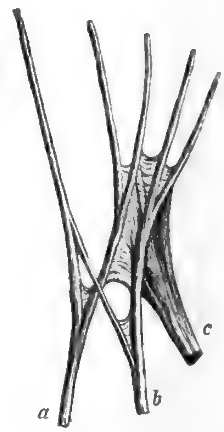


Fig. 5.

the third or fourth in seven specimens (fig. 6), in one of which the process for the deep flexor tendon of the second toe was much larger than that supplied by the flexor communis; a larger share to the second and third than to the fourth in five specimens; a larger share to the second and fourth than to the third in one specimen; and about equally to these three toes in the remainder.

In the solitary specimen in which the connecting slip went to the four outer toes, the subdivisions for the second and third toes were larger than those for the fourth and fifth, that for the fifth being a comparatively slender thread (fig. 7).

In none of the fifty specimens did the connecting band join the tendon of the common flexor previous to the subdivision of the latter tendon. In every instance it proceeded either single, bifurcated, trifurcated, or in four subdivisions, to its appropriate toe or toes, and in its course joined the divisions of the flexor communis, or the portions of the flexor accessorius passing to the same toe or toes. To the deep tendons for the second and third toes, more especially, it not unfrequently contributed quite as much as the flexor communis, and occasionally it and the flexor accessorius together, entirely or almost entirely, were substituted for

* The flexor communis in this case trifurcated for the third, fourth, and fifth toes.

† Fig. 3. In this and the succeeding figures, *a*, is the flexor hallucis longus tendon; *b*, the flexor communis digitorum tendon; *c*, the flexor accessorius. In figure 3 the flexor communis forms no portion of the deep tendon for the second toe, and after giving a slip to the flexor hallucis tendon, trifurcates for the three outer toes.

the flexor communis in the construction of the deep flexor tendons for those toes.*

In nine specimens a band, sometimes of considerable size, proceeded from the

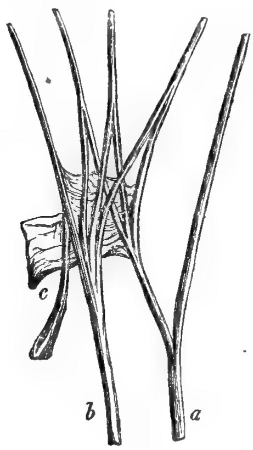


Fig. 6.

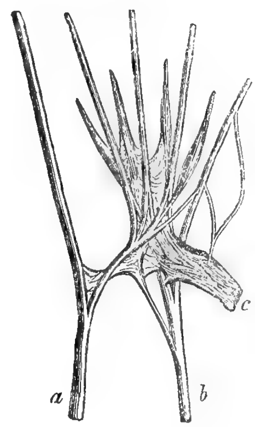


Fig. 7.

common flexor tendon previous to its subdivision, which joined the tendon of the flexor hallucis longus beyond the origin of the connecting slip for the common

* That the tendon of the long flexor of the great toe gives off a band more or less strong to the common flexor of the toes in the sole of the foot has been almost universally recognised by anatomists, but the exact nature of the connection between them has not at all times been clearly expressed. Amongst the older anatomists, VESALIUS describes this band as passing from the tendon of the great toe to the tendon proceeding to the second toe, and sometimes in an equal degree to the tendon of the common flexor for the middle toe. DIEMERBROECK again states that sometimes the long flexor of the great toe is divided in the sole into two parts, one of which goes to the great, the other to the second toe, and then the common flexor sends but three tendons to the other toes. COWPER and BIDLOO simply describe a connecting band passing from the proper to the common flexor, without specialising its mode of termination, and this method of description has been followed by most systematic writers in the latter part of the last century, and in the present, as INNES, MONRO, SABATIER, BICHAT, BOYER, JOHN BELL, FYFE, CLOQUET, CRUVEILHIER, DODD, QUAIN, HARRISON, HYRTL, LEDWICH, ELLIS, KNOX, HOLDEN, HEATH, and GRAY. MECKEL employs, in his description of the long flexor of the great toe almost the same method as DIEMERBROECK, but, in addition, states that the long flexor tendon for the second toe is for the most part formed by the connecting band and the flexor accessorius. THEILE follows very closely the latter statement of MECKEL, but, under the head of anomalies, he describes the connecting band as dividing for the second and third toes. ARNOLD gives the connecting band as strengthening the tendon for the second toe, though it often goes also to the third toe. HENLE states that the strong process from the proper to the common tendon is for the most part, and at times altogether, continued into the tendon destined for the second toe. Mr CHURCH, in a recent monograph on the myology of the Orang (*Natural History Review*, 1862), has also directed attention to the connection of the band from the flexor hallucis with the second and third toes. Professor ROLLESTON has advanced evidence to the same effect. Last of all, Mr HUXLEY (*Reader*, 13th February 1864) states, as the results of his dissections, that the tendon of the flexor hallucis longus, besides giving off the tendon to the great toe, furnishes distinct slips to the two or three succeeding digits, uniting with the tendons of the flexor digitorum and flexor accessorius. That considerable variability occurs in the mode of termination of the connecting band might almost be inferred from the different descriptions given of it by the numerous anatomists just quoted, each apparently, of those at least who go into details, basing his description on the specimen or specimens he may more particularly have examined. A more exact conception, however, of the extent of this variability may be gathered from the analysis of the fifty specimens recorded in the text.

flexor tendon from it (figs. 3, 5, 8, 10). In these cases, therefore, the tendons were doubly connected by intertendinous bands.*

The flexor accessorius varied greatly in its mode of termination on the flexor tendons. In but a few instances (fig. 5, for example) could it be said to end in the manner usually described in the text-books, by joining the outer border and upper, and sometimes the under surface of the tendon of the flexor communis. In many cases it had no connection whatever with the outer border of that tendon; in several of these it contributed no fibres to the tendon for the little toe, and in a few it had no connection with the tendons for the fourth and fifth toes. In other cases, however, it gave off a distinct tendinous or musculo-tendinous bundle, sometimes of considerable size, to the deep tendon for the little toe (figs. 3, 6, 7). In a few cases the deep flexor tendon for that toe was almost entirely (fig. 10), and in one case (fig. 8) apparently, entirely formed of a tendon proceeding from the flexor accessorius, the common flexor tendon sparingly in the former (fig. 10), and not at all in the latter case (fig. 8), entering into its construction. In most cases the accessory flexor ended partly on the flexor communis, and partly on the connecting slip from the flexor hallucis, and through one or both of these contributed materially to the formation of the deep tendons for the second, third, and fourth toes. In one case it sent, in addition, a few fibres to the primary tendon of the flexor hallucis, and in another all its fibres terminated on the connecting slip, and through it were transmitted to the deep flexor tendons of the second and third toes. In one case it gave off a distinct slip, which, separating into two parts, gave one to each process of bifurcation of the tendon of the flexor brevis digitorum for the third toe.

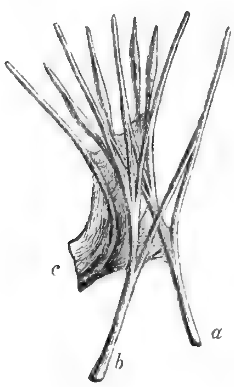


Fig. 8.†

In two cases the flexor accessorius had an accessory muscle connected to it, which arose from the deep fascia of the back of the leg in its lower third, concealing at its origin the posterior tibial vessels and nerve. It passed downwards, and ended in a rounded tendon, which extended through the inner ankle beneath the abductor pollicis, and joined the inner margin of the flexor accessorius (fig. 9).‡

* It would almost appear as if some of the systematic writers of the last century had recognised the band proceeding from the flexor communis to the flexor hallucis, but not the one passing in the opposite direction. *Vide* Albinus, Winslow, Tarin, Sandifort, and Douglas. Several of the more recent writers have described an arrangement similar to the one recorded in the text.—*Vide* Sabatier, Arnold, and Theile.

† Fig. 8 shows the deep flexor tendon for the little toe entirely formed of the flexor accessorius. The flexor communis, after sending off a connecting band to the flexor hallucis, trifurcates for the second, third, and fourth toes, to which the connecting band from the flexor hallucis also proceeds.

‡ The two specimens described in the text were found amongst the fifty specimens specially analysed, but I have in former years, and in other subjects, met with additional instances of an accessory muscle in this locality. The region of the inner ankle appears, indeed, to be frequently the seat of such accessory muscular structures, *e.g.*

The lumbricales muscles presented many variations, some of the leading forms of which it may be advisable to particularise, more especially that FROMENT (with whom HENLE seems to agree) states that variations in the arrangement of these muscles are extremely rare. The first lumbricalis, in the specimens under analysis, arose sometimes from the tibial side of the deep tendon for the second toe, after the junction of the connecting slip from the flexor hallucis with the division of the common flexor tendon for that toe. Sometimes only from the tibial side of the connecting slip; sometimes only from the division of the common flexor to the second toe; in one case from the expanded part of the common flexor before it divided into its terminal tendons; in other cases by a continuous origin both from the tibial side of the second toe tendon, and from the expanded part of the common tendon; and in two cases by two distinct heads,—one from the tibial side of the connecting slip from the flexor hallucis, the other from the expanded part of the common flexor before its division into the terminal tendons. In one case no lumbricalis was present in the first metatarsal space, but two were situated in the second space (fig. 8). In another case the second lumbricalis was absent. In several cases, not only did the second, third, and fourth muscles arise from adjacent sides of the tendons between which they were situated, but also from special slips derived from those tendons. Sometimes their fibres of origin extended for some distance backwards over the general expansion of the common flexor tendon. In other instances the fibres of the fourth lumbricalis, or of the third and fourth lumbricales, were continuous with (or received fasciculi from) those of the flexor accessorius; in others they

Fig. 9.*

1*st*, A large slip springing from the inner side of the soleus, and passing quite distinct from the tendo Achillis, to be inserted into the inner concave surface of the os calcis.

2*d*, A muscle arising from the deep fascia of the back of the leg, and inserted into the inner side of the os calcis, close to the inner head of the flexor accessorius; this apparently constitutes the accessorius ad calcaneum of GANTZER.

3*d*, Two muscular bundles connected to the deep fascia of the back of the leg, one as high as the middle of the tibia, the other close to the origin of the flexor hallucis longus from the fibula; these bundles united to form a muscle which passed beneath the internal annular ligament to the sole where its tendon bifurcated, one slip joining the tendon of the flexor hallucis longus, the other the tendon of the flexor communis digitorum. A corresponding arrangement was found in both limbs.

4*th*, A well-marked muscle arose from the deep surface of the soleus tendon. It concealed the tendons of the deep muscles, and the posterior tibial vessels and nerves in the lower third of the leg, and was inserted into the deeper surface of the tendo Achillis, immediately above the os calcis. A similar case to this has been described by R. QUAIN.

Other irregularities in this locality have been recorded by MAYER, ROSENMÜLLER, GANTZER, MECKEL, HALLETT, THEILE, HENLE, and JOHN WOOD.

* Fig. 9. *d*, the accessory muscle to the flexor accessorius. It has been bent out of its proper direction so as to occupy less space in the wood block. *e*, the displaced fasciculus of the flexor brevis for the little toe, which simply blends, without bifurcating with the deep flexor tendon for that toe.

received a special fasciculus of fibres, arising from the process sent by the connecting slip of the flexor hallucis to the third or fourth toes; in one case the fourth lumbricalis was absent.

Variations in the mode of arrangement of the flexor brevis digitorum were also noted. The tendon passing to the little toe was sometimes not perforated by the tendon of the common flexor. In one case it was blended and inserted along with it; in others it was so thin as to be lost in the fascia of the foot; in one it was altogether absent. In five cases the short flexor tendon for the little toe was displaced at its origin, and arose from the common flexor tendon previous to the subdivision of that structure (fig. 10).* At its origin it either consisted partly of fibres continuous with those of the common flexor tendon, and partly of distinct muscular fibres attached to and springing from that tendon, or it arose tendinous, and then muscular fibres appeared in it, which again terminated

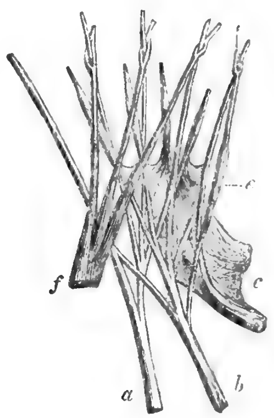


Fig. 10.*

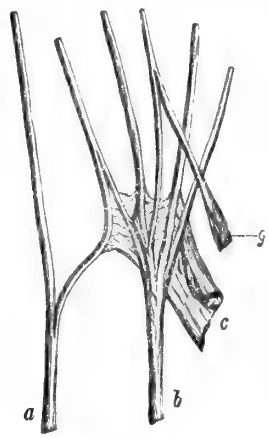


Fig. 11.†

on the tendon of insertion. In three of these cases the tendon bifurcated, to allow the common flexor tendon for the little toe to pass through and beyond (fig. 10); in the other two it blended with the common flexor tendon for that toe, and was inserted along with it (fig. 9). In one specimen the tendon for the

* Fig. 10. In this drawing, *f* is the flexor brevis digitorum, which divides into fasciculi for the second, third, and fourth toes, the fasciculus for the fifth toe, *e* arises from the tendon of the flexor communis. In this figure the deep flexor tendon for the little toe is formed almost entirely from the flexor accessorius, the flexor communis contributing but a few fibres. The connecting slip from the flexor hallucis trifurcates for the second, third, and fourth toes, and the flexor communis gives off a connecting band to the flexor hallucis. Brugnone, Meckel, Theile, Hyrtl, Henle, Church, and Huxley, have all recognised the occasional origin of the short flexor tendon for the little toe from the flexor communis. In another subject I saw the fasciculus forming the short flexor tendon for the little toe arise in part from the external inter-muscular septum, and in part through fibres continuous with the muscular part of the flexor accessorius.

† Fig. 11, *g*, the tendon of the flexor brevis for the third toe, its junction with a slip from the expanded part of the flexor communis is represented. In both feet of a subject not included in the above analysis, I saw an arrangement similar to that represented in fig. 11, except that the slip from the common flexor tendon bifurcated before joining the two branches of bifurcation of the flexor brevis.

third toe received a strong tendinous slip from the expanded part of the common flexor tendon, and the two were blended and inserted together (fig. 11). In another, the short flexor tendon for the third toe received an additional slip from the flexor accessorius.

In the foot, therefore, as in the forearm and hand, intermuscular structures not unfrequently connected together not only the flexor muscles situated on the same plane, but those situated on the superficial and deeper planes.

The fifty specimens of the flexor muscles of the foot, the special analysis of the mode of arrangement of which I have now recorded, will be sufficient, I think, to show that in the construction of this group of muscles an amount of variation existed much greater than might at first sight have been supposed. In a portion of one muscle alone, viz., the connecting band from the tendon of the flexor hallucis longus, a considerable number of modifications occurred. In the flexor accessorius also great variability was displayed, and in some proportional relation, apparently, to the extent of variation in it and the connecting band, did the flexor communis digitorum undergo certain modifications in its arrangement, so much so, indeed, in certain cases, as to permit those structures to be to a great extent substituted for it in the formation of the deep flexor tendons for some of the toes.

Variability in the construction of parts, however, was not manifested merely in different individuals, but in the same individual the corresponding structures on opposite sides of the body were by no means symmetrically disposed. Thus, in two of the four examples recorded in the earlier part of the paper in which a supra-condyloid process existed, it occurred only in one arm of each subject in two cases; whils in a third subject, though both humeri exhibited the process, yet the relations of the brachial artery to it on the two sides were by no means symmetrical. The tendons in the left foot, in many of the individuals in whom both feet were examined, varied also more or less from those in the right foot, so that in the construction of the limbs, as well as in the form and arrangement of the organs contained in the great cavities of the body, an asymmetrical disposition of parts is to be looked for. Thus, we arrive at the conclusion that the plan on which the human body is constructed, although constant in all its essential characters, yet admits of variations (within itself as it were) in certain directions and within certain limits. Neither form nor structure is absolutely stereotyped, but modifications occur which, when regarded singly, may be considered, perhaps, as slight and of comparatively little importance, yet when viewed collectively, are sufficient to give to the individual well-marked distinctive characters.

Much has been said and written of late years on the existence of structural differences between the fair and coloured races of mankind, more especially between the white man and the negro,—differences which, according to some writers, are so great as to constitute an actual specific distinction between them. But those who have advanced and supported this view seem to me to have ignored, or at least not to have taken sufficiently into consideration the fact, that in the white races themselves, nay, as we have shown in this paper, in a limited section of them even, variations occur in the arrangement of certain of the soft parts so great as to permit of the office usually performed by one muscle to be, in a great measure, or even altogether, exercised by another. But the extent of variation which the white races may exhibit is by no means exhausted by what we have detailed in this communication. Numerous isolated examples of variations, both in the muscular and other systems, have been recorded elsewhere by myself and other anatomists, and additional observations in the same profitable field of inquiry will, I have no doubt, add many other forms to our already extensive list. Until, however, the deviations from the usually described arrangements in the fair races are more systematically inquired into than has hitherto been the case, we cannot hope to reach an accurate conception of the latitude which may be allowed them.

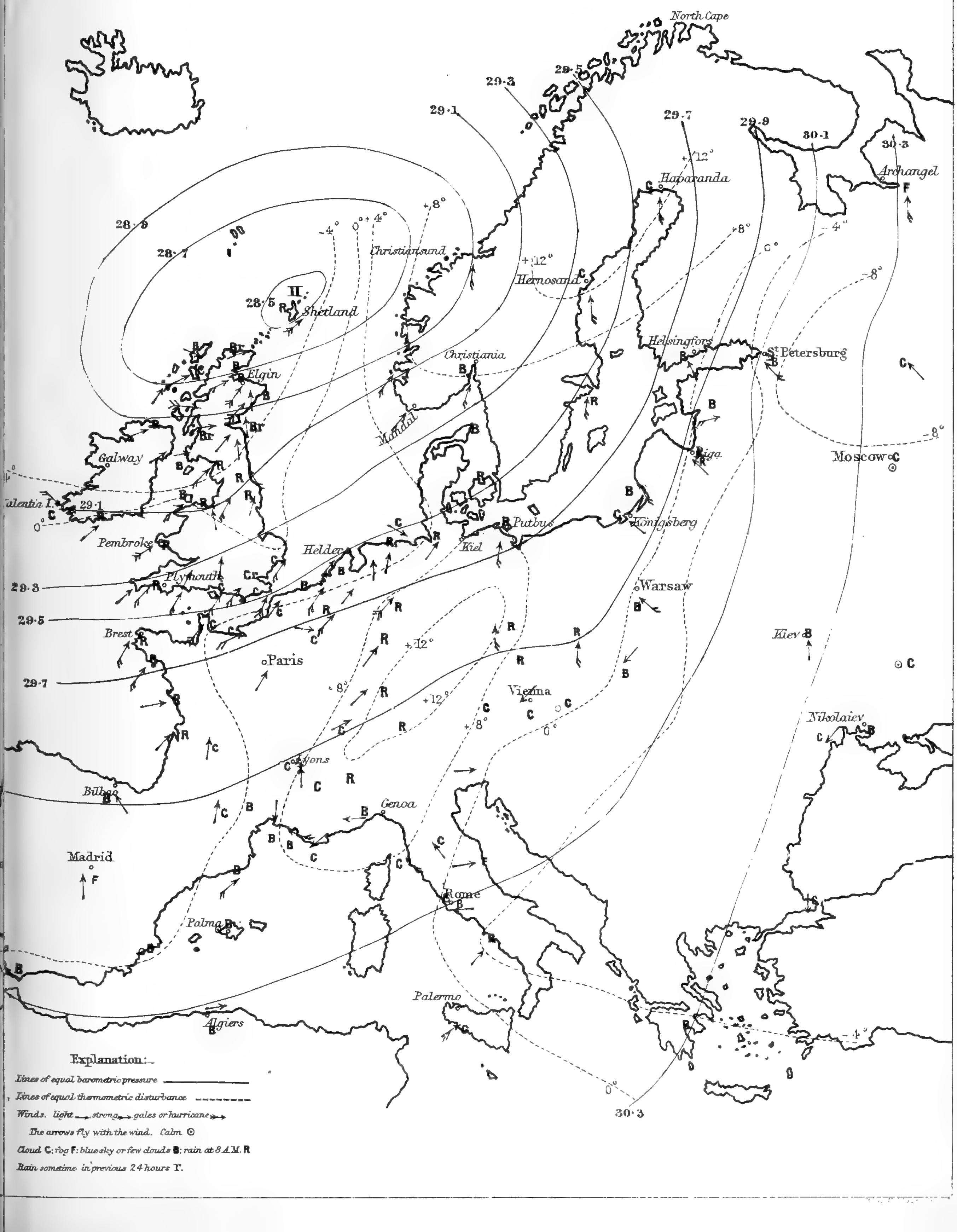
Of the extent of the structural variability which may exist in the soft parts of the dark races, we as yet know but little. It is seldom that their bodies have been critically examined; and of many of the coloured races, indeed, the number of dissections has been too small to permit of any satisfactory conclusions to be arrived at, for opportunities of making the necessary observations seldom fall in the way of the European anatomist. His dissections are made and his descriptions are based on the examination of the inhabitants of his own continent. Our knowledge of the comparative anatomy of the soft parts of the races of men is still in its infancy. To make good, indeed, the proposition that the negro is specifically distinct from the white man, it would be necessary to show that any peculiarities of arrangement which may be exhibited in the construction of his body, are either constant, or, if variable, that the variations are not in accordance with those which have been or may be met with in similar parts in the bodies of men of the fair races. For until we have determined not only the amount of structural variability in the different races, but the comparative frequency of occurrence of its principal forms, we shall not be in a position even to discuss the question of specific difference on anything like positive scientific data, still less to pronounce dogmatically on the subject.

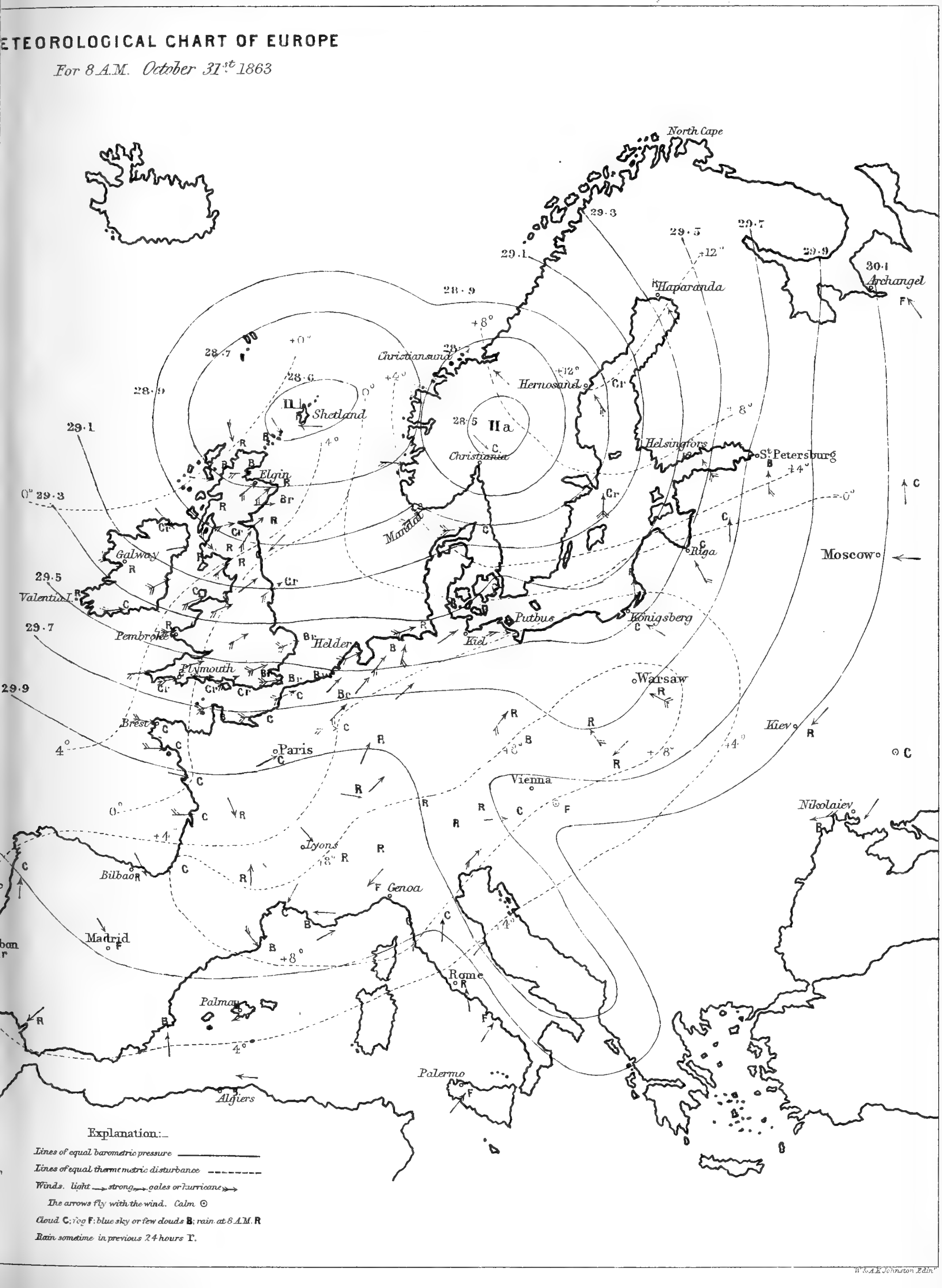
From the special difficulties which surround the study of human anatomy, it will not be an easy matter to determine with precision the laws which regulate the development of these structural variations. In some cases, indeed, it would appear that a variation in one structure is not unfrequently correlated with variations in adjacent parts. Thus in the four specimens described in the early part of this communication in which, in conjunction with a supra-condyloid process, a foramen existed above the inner condyle of the humerus, the median nerve passed through that foramen. The deflection of the nerve from its course and the existence of the process were, it is evident, not only from these but from many similar cases, correlated events. Again, in the flexor tendons of the foot, a deficiency in the size of the flexor communis digitorum was not unfrequently correlated with an increase in the size of either the connecting-band from the flexor hallucis, or of the flexor accessorius, or it might be of both; and an absence of a tendon from the flexor brevis digitorum for the little toe was not unfrequently correlated with the presence of a fifth tendon from the flexor communis digitorum.

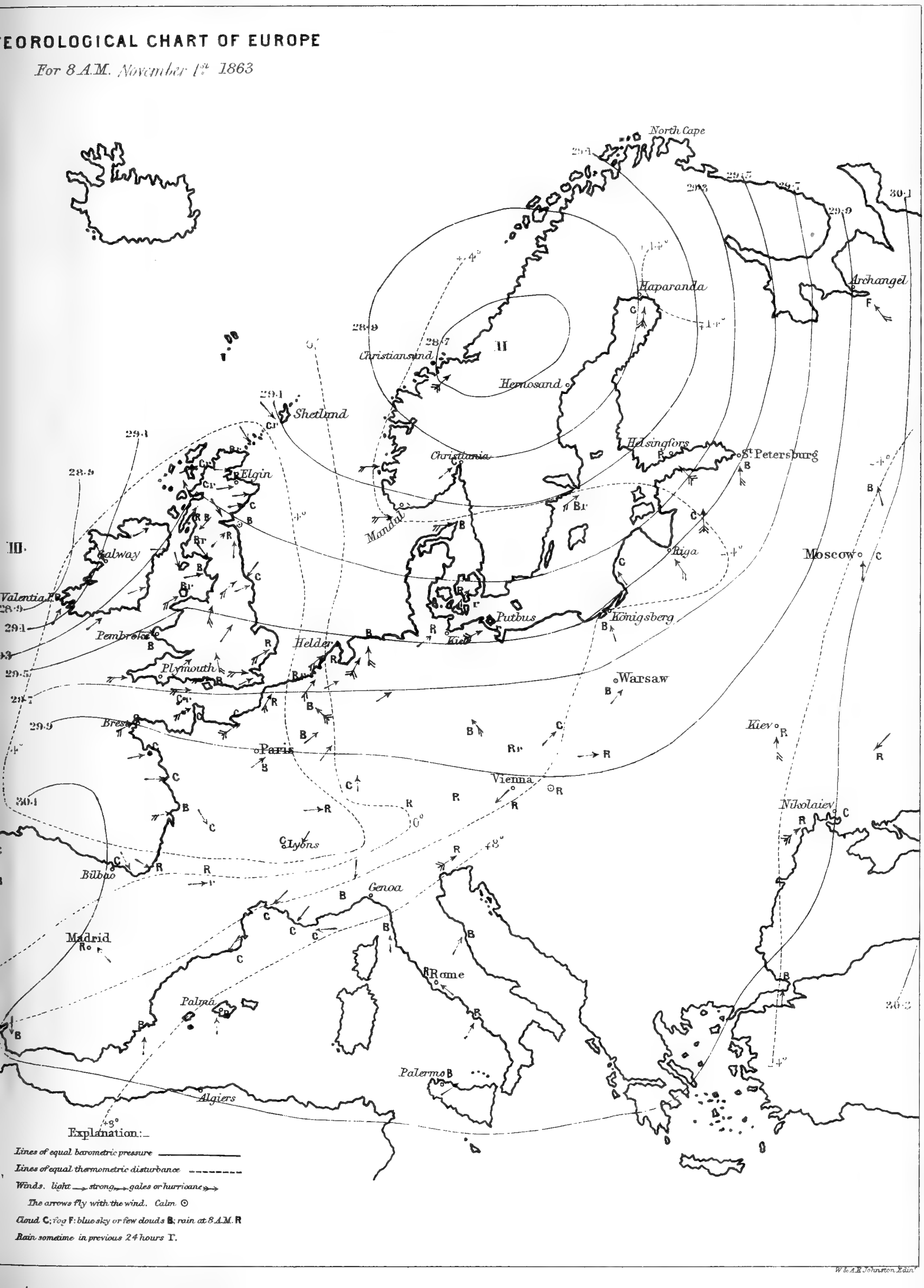
To how great an extent the conditions of life of the individual, in whom these and other varieties present themselves, may be concerned in their production, or how far they may be transmitted from parent to offspring, and thus be considered as family peculiarities, are questions which for the present at least must be left undetermined. But in regard to the variations in the muscular arrangements which have been specially illustrated in this communication, it may safely be stated that the power of performing the appropriate movements of the part must be modified in accordance with the modifications in its structure.

OROLOGICAL CHART OF EUROPE

For 8 A.M. October 30th 1863



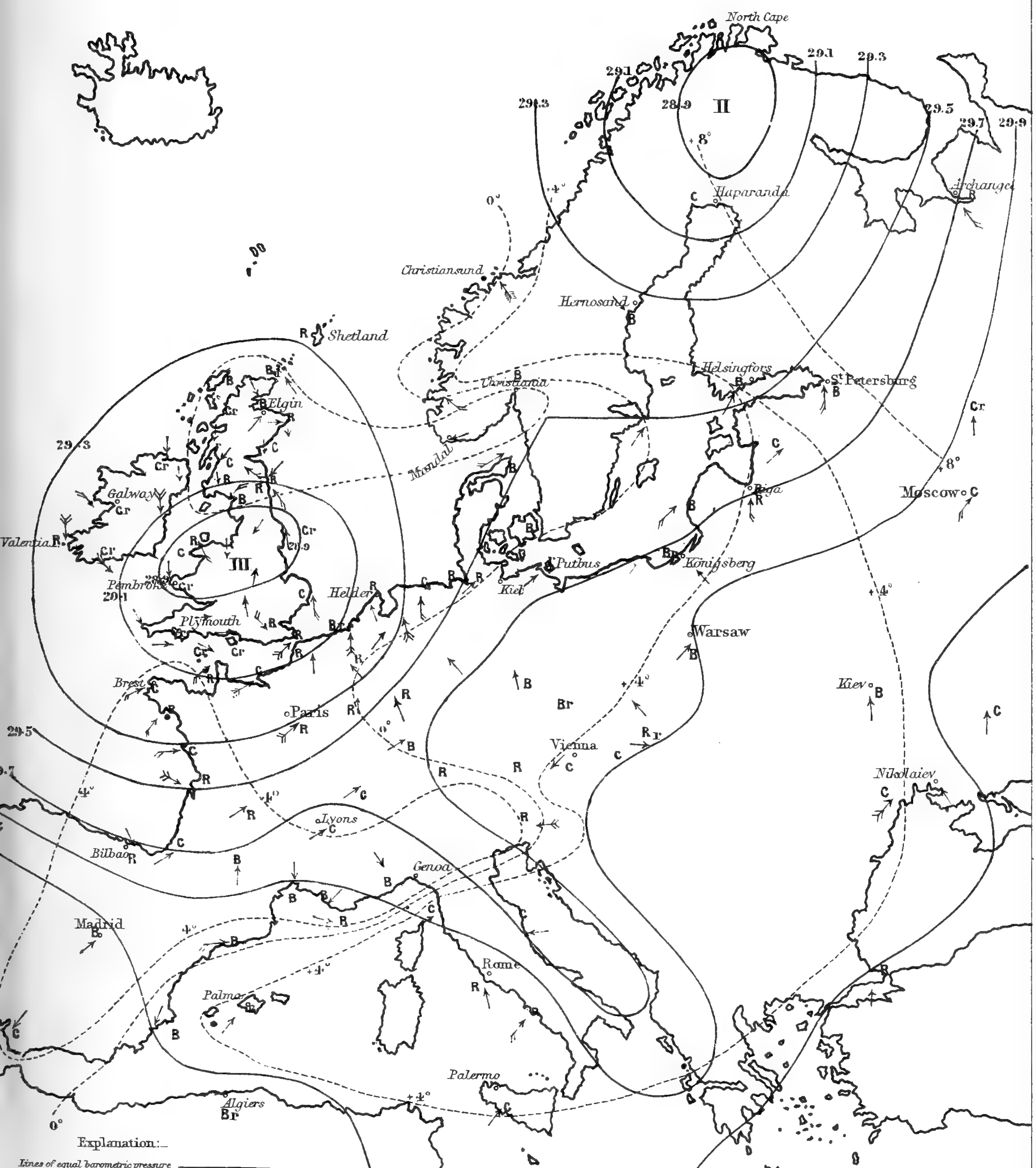

ETEOROLOGICAL CHART OF EUROPE
For 8 A.M. October 31st 1863
Explanation:_
Lines of equal barometric pressure
Lines of equal thermometric disturbance
Winds. light, strong, gales or hurricane
The arrows fly with the wind. Calm ⊙
Cloud C; fog F; blue sky or few clouds B; rain at 8 A.M. R
Rain sometime in previous 24 hours r.
North Cape
Archangel
Haparanda
Hernosand
Christiansund
Shetland
Christiania
Helsingfors
St. Petersburg
Moscow
Riga
Königsberg
Putbus
Kiel
Helder
Elgin
Galway
Valentia I.
Pembroke
Plymouth
Brest
Paris
Warsaw
Kiev
Vienna
Nikolaiev
Lyons
Bilbao
Genoa
Madrid
Rome
Palma
Palermo
Algiers
Mandal
W. & A.K. Johnston Edin.



EOROLOGICAL CHART OF EUROPE
For 8 A.M. November 1st 1863
North Cape
Haparanda
Archangel
Christiansund
Hernosand
Shetland
Elgin
Christiania
Helsingfors
St Petersburg
Mandal
Galway
Riga
Moscow
Valentia
Putbus
Königsberg
Pembroke
Kiel
Helder
Plymouth
Warsaw
Brest
Paris
Kiev
Vienna
Nikolaiev
Lyons
Bilbao
Genoa
Madrid
Rome
Palma
Palermo
Algiers
Explanation:
Lines of equal barometric pressure
Lines of equal thermometric disturbance
Winds. light, strong, gales or hurricane
The arrows fly with the wind. Calm ⊙
Cloud C; fog F; blue sky or few clouds B; rain at 8 A.M. R
Rain sometime in previous 24 hours r.
W. & A. K. Johnston Edinr

EOROLOGICAL CHART OF EUROPE

For 8 A.M. November 2nd 1863



Explanation:—

Lines of equal barometric pressure ————

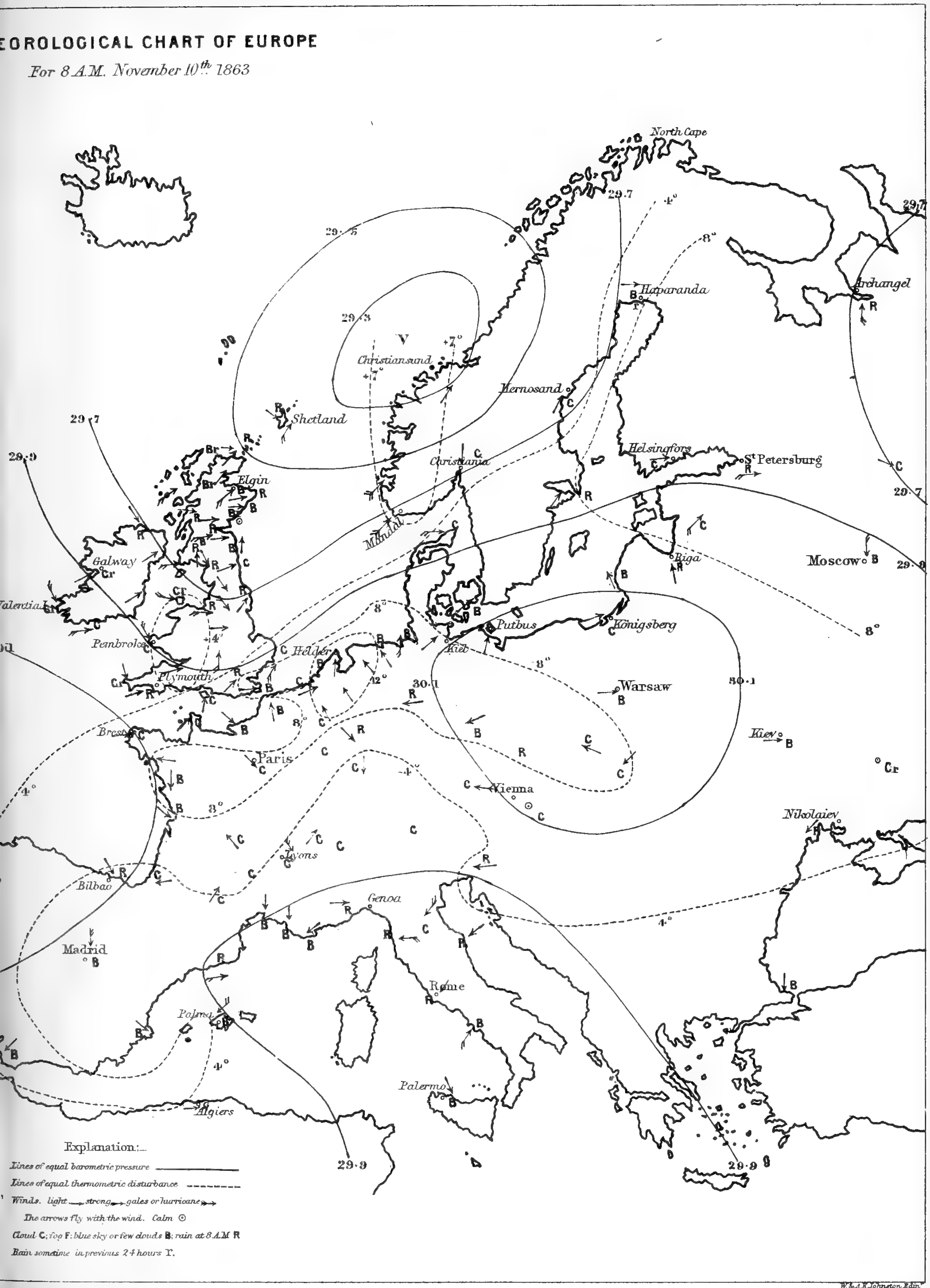
Lines of equal thermometric disturbance - - - - - -

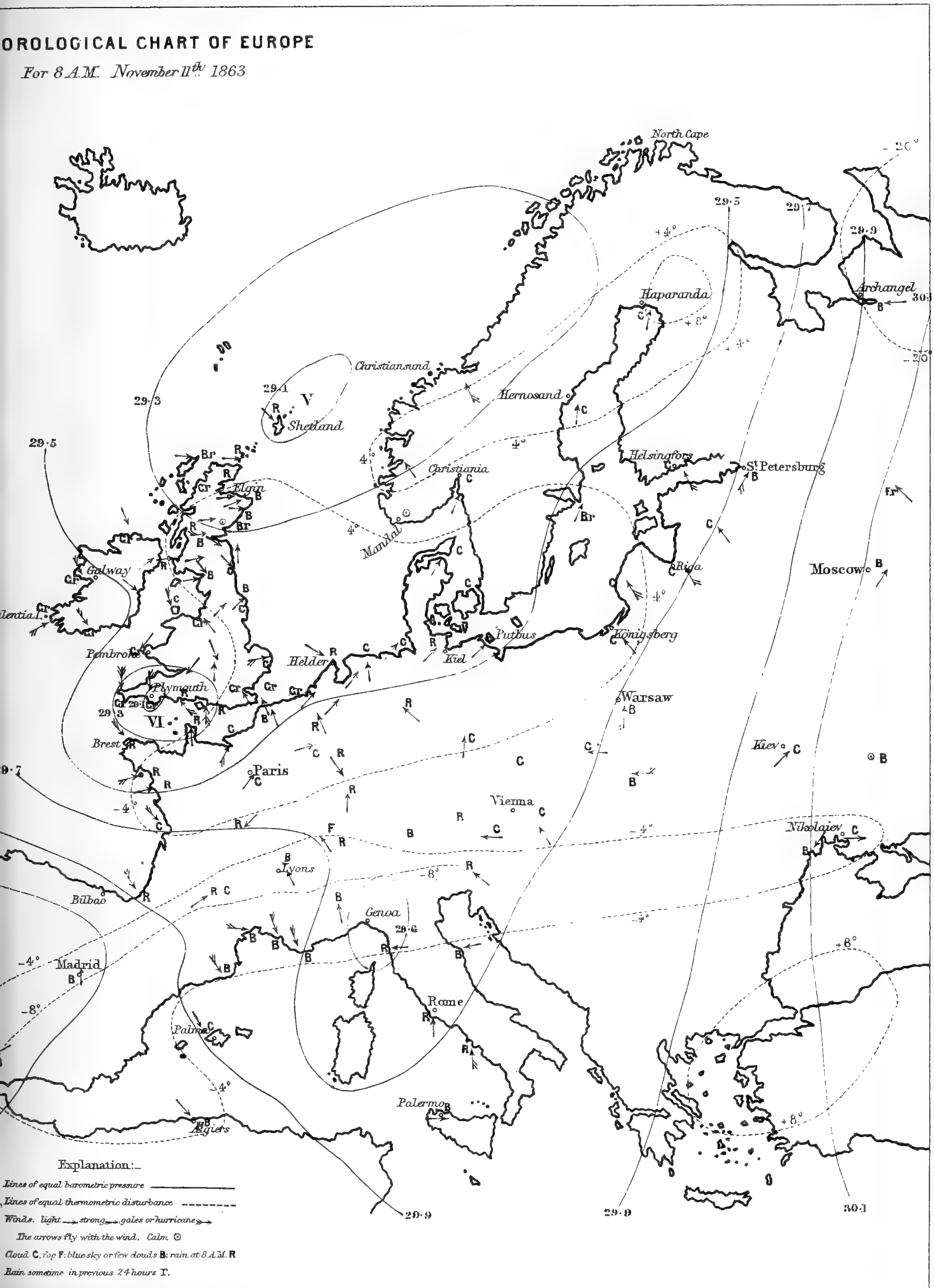
Winds. light, strong, gales or hurricane

The arrows fly with the wind. Calm ⊙

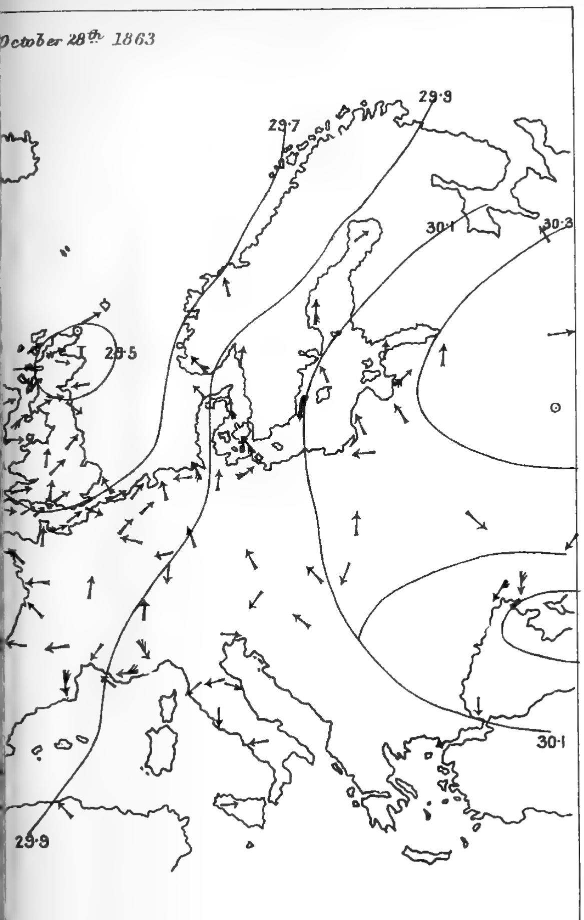
Cloud C; *Fog* F: *blue sky or few clouds* B; *rain at 8 A.M.* R

Rain sometime in previous 24 hours r.

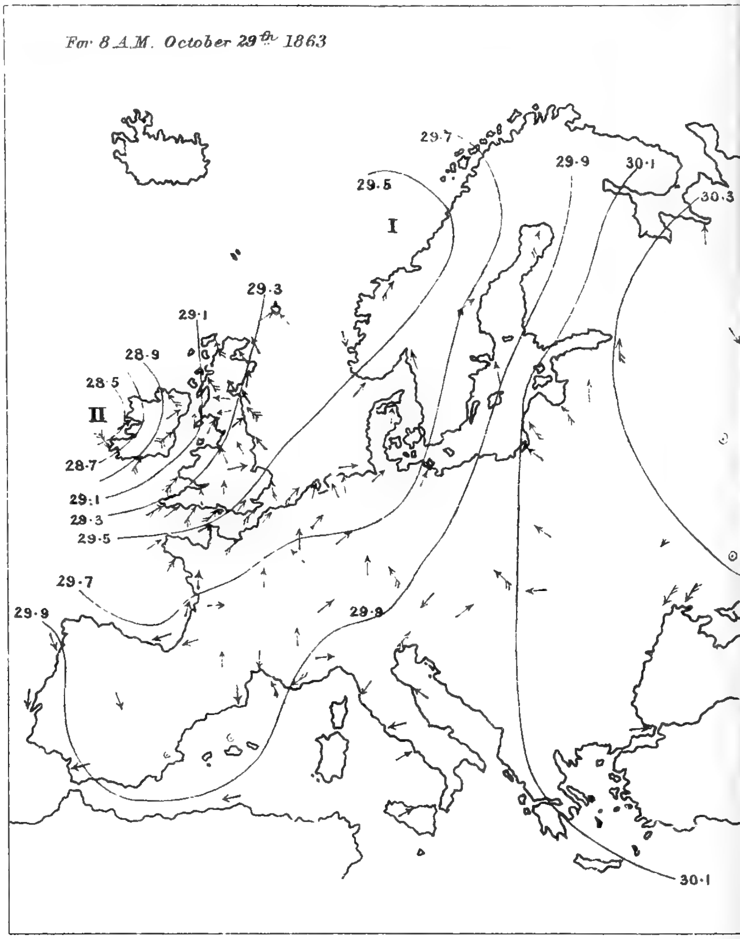

EOROLOGICAL CHART OF EUROPE
For 8 A.M. November 10th 1863
North Cape
Haparanda
Archangel
Christiansund
Hernosand
Shetland
Helsingfors
St Petersburg
Christiania
Elgin
Galway
Mandal
Riga
Moscow
Valentia I.
Putbus
Königsberg
Pembroke
Helder
Kiel
Plymouth
Warsaw
Brest
Paris
Kiev
Vienna
Nikolaiev
Lyons
Bilbao
Genoa
Madrid
Rome
Palma
Palermo
Algiers
Explanation:—
Lines of equal barometric pressure
Lines of equal thermometric disturbance
Winds. light, strong, gales or hurricane
The arrows fly with the wind. Calm ⊙
Cloud C; fog F; blue sky or few clouds B; rain at 8 A.M. R
Rain sometime in previous 24 hours Y.
W. & A. K. Johnston, Edinr.


OROLOGICAL CHART OF EUROPE
For 8 A.M. November 11th 1863
North Cape
Haparanda
Archangel
Christiansund
Hernosand
Shetland
Christiania
Helsingfors
St Petersburg
Elgin
Mandal
Riga
Moscow
Galway
Königsberg
Putbus
Kiel
Helder
Pembroke
Plymouth
Warsaw
Brest
Paris
Kiev
Vienna
Nikolaiev
Lyons
Bilbao
Genoa
Madrid
Rome
Palma
Palermo
Algiers
Explanation:—
Lines of equal barometric pressure
Lines of equal thermometric disturbance
Winds. light, strong, gales or hurricane
The arrows fly with the wind. Calm ⊙
Cloud C, Fog F; blue sky or few clouds B; rain at 8 A.M. R
Rain sometime in previous 24 hours T.
W. & A. K. Johnston, Edin.

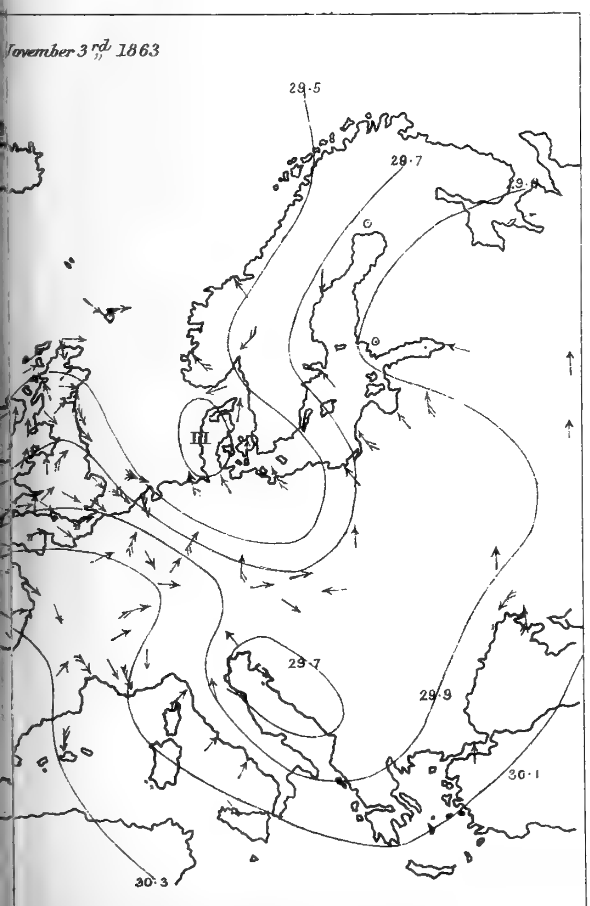
METEOROLOGICAL CHART OF EUROPE



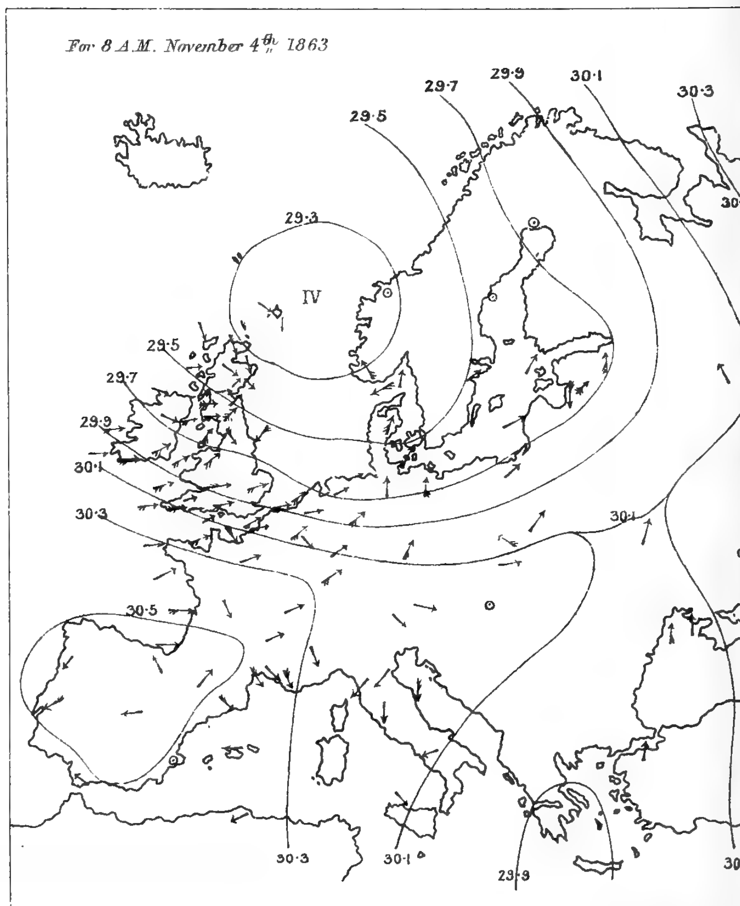
October 28th 1863



For 8 A.M. October 29th 1863



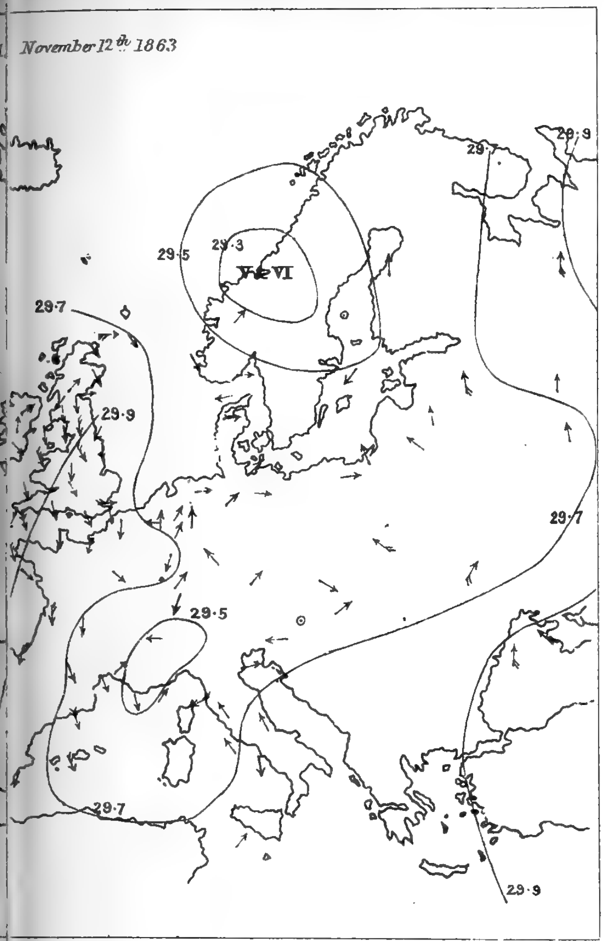
November 3rd 1863

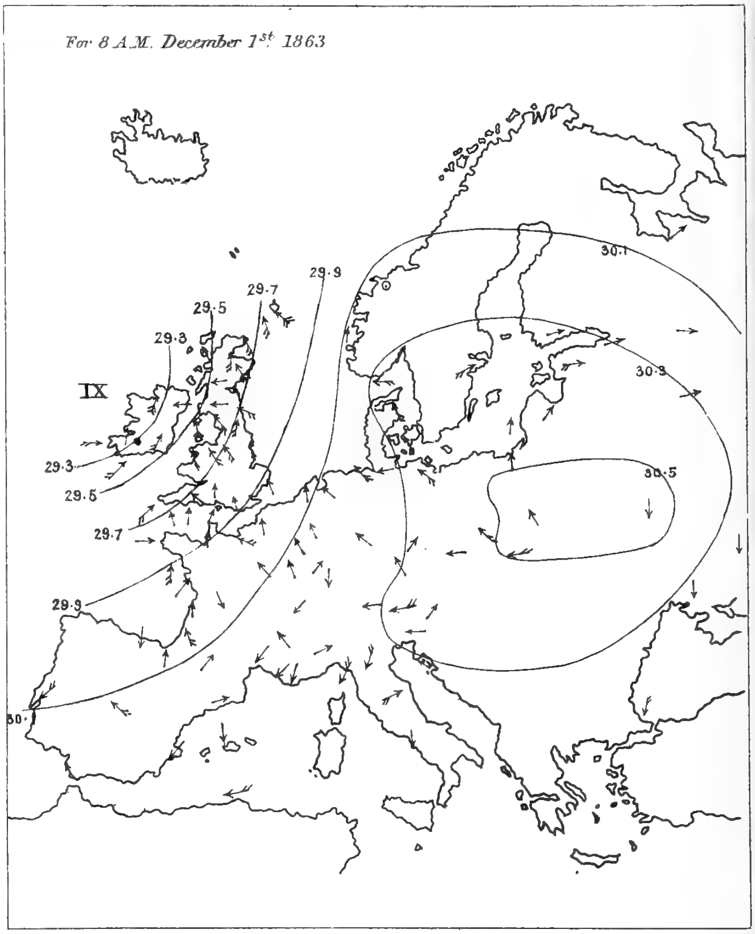


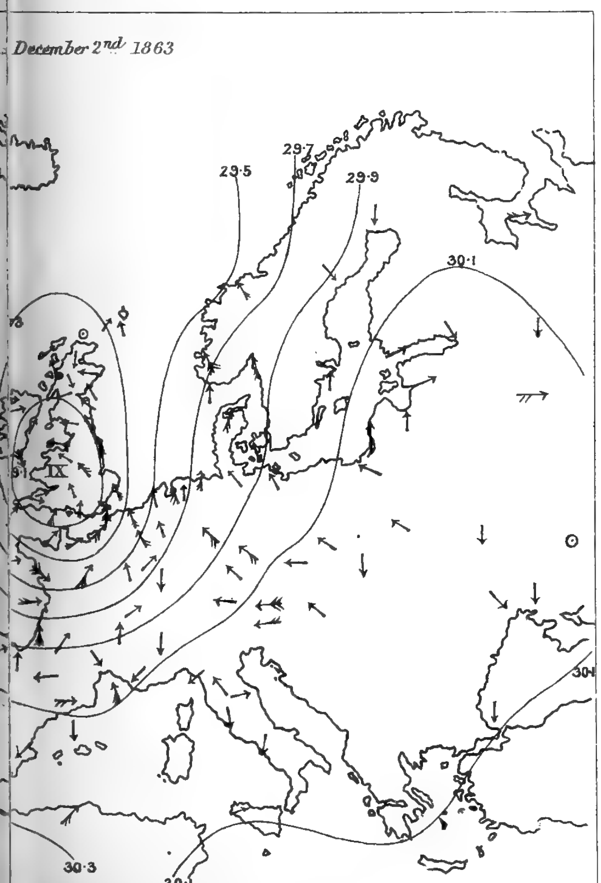
For 8 A.M. November 4th 1863

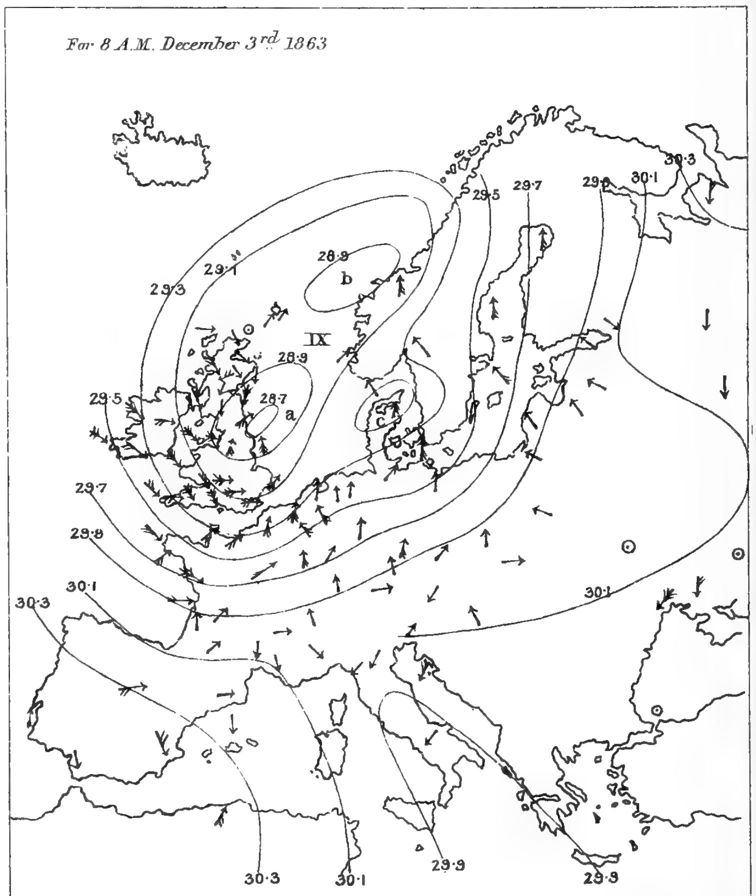
W. & A.K. Johnston, Edin.

METEOROLOGICAL CHART OF EUROPE









W. & A.K. Johnston Edin.

TABLE I.—Showing the BAROMETRIC PRESSURE, in English inches, reduced to 32° and Sea Level, at 130 places in Europe, from 26th October to 12th November, from 20th to 26th November, from 30th November to 5th December, and from 14th to 18th December, at 8 A.M.

	Lat N	Long	October 26.	27.	28.	29.	30.	31.	November 1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	20.	21.	22.	23.	24.	25.	26.	30.	December 1.	2.	3.	4.	5.	14.	15.	16.	17.	18.
IRELAND.																																						
Greencastle,	55 13	6 57 w	30·11	29·94	29·58	29·96	28·93	29·05	29·24	29·19	29·48	29·60	30·10	30·52	30·13	29·87	30·41	29·72	29·39	29·96	29·77	29·18	29·14	29·65	29·74	29·18	29·91	30·06	29·28	29·23	.	30·05	29·37	30·40	29·22	29·65	30·21	30·47
Belfast,	54 37	5 50 w	03	89	52	79	92	69	33	06	47	58	05	45	06	52	33	67	34	90	78	39	40	70	67	74	64	29·19	29	10	29·67	00	54	34	15	61	14	41
Dublin,	53 21	6 16 w	12	06	03	00	29·02	29·21	50	16	50	78	14	50	21	30·00	39	60	51	98	94	45	66	71	78	66	30·02	30·06	42	11	29·30	20	76	47	31	86	70	55
Galway,	53 17	9 3 w	11	07	72	56	06	23	20	25	49	78	14	49	24	08	41	91	54	30·04	66	16	35	13	13	48	29·88	29·75	24	31	39	20	76	44	29	91	37	56
Valencia,	51 54	10 25 w	05	87	70	96	09	42	28·98	21	61	95	10	39	34	15	39	30·03	63	06	75	24	59	67	65	52	80	84	29	44	59	30	90	43	31	30·09	77	55
Queenstown,	51 51	8 17 w	10	07	74	90	28·88	40	29·38	12	53	97	13	47	18	29·87	41	03	63	29·92	77	16	45	61	62	47	86	86	23	25	47	23	80	37	26	29·96	16	45
SCOTLAND.																																						
East Yell,	60 33	1 3 w	02	91	55	29·37	44	28·60	08	32	40	24	29·91	14	13	30·15	31	29·43	09	45	77	47	11	65	65	76	30·05	30·17	77	34	97	29·62	17	29·60	29·85	29·84	03	[illegible]
Bressay,	60 10	1 10 w	12	88	65	47	56	66	18	38	47	29	82	24	22	19	41	51	21	91	95	46	15	05	76	81	12	21	80	38	04	71	26	30·00	94	91	11	30·04
Sandwick,	59 3	3 15 w	04	82	51	24	66	69	21	23	41	25	77	24	19	14	31	47	14	84	76	39	03	51	47	59	29·98	06	76	27	29·98	67	25	05	91	96	01	05
Stornoway,	58 11	6 22 w	02	82	59	12	64	63	23	34	39	36	88	34	22	04	09	69	29	69	68	32	22	61	68	47	81	62	52	05	89	66	38	16	08	29·23	16	17
Dunrobin,	57 58	3 57 w	02	84	51	31	76	63	25	27	45	36	67	31	16	09	38	55	23	86	77	45	07	54	40	80	88	29·98	61	19	98	78	37	07	08	21	09	12
Portree,	57 25	6 12 w	02	83	48	01	66	70	23	20	40	47	92	39	12	29·91	26	54	24	88	65	31	18	73	68	37	50	97	62	14	98	81	33	22	99	30	18	27
Elgin,	57 39	3 19 w	03	84	40	21	81	68	21	20	44	34	87	34	18	04	31	53	23	83	77	42	09	75	65	11	97	30·08	51	21	96	79	35	13	30·00	22	07	16
New Pitsligo,	57 36	2 12 w	04	80	41	20	82	68	23	20	44	35	88	34	11	97	20	53	22	81	80	48	07	...	70	11	30·04	29	62	17	94	82	40	12	01	20	29·98	12
Barry,	56 31	2 45 w	09	92	44	28	93	77	22	14	65	41	30·10	49	21	94	39	62	30	91	96	55	21	64	73	73	13	20	65	15	91	95	48	17	14	41	30·10	25
Aberdeen,	57 8	2 5 w	06	90	41	22	89	75			54	39	29·97	42	17	...	...	...	26	93	88	59	10	60	72	75	09	...	57	17	95	90	40	17	06	27	01	24
Trinity Gask,	56 22	3 41 w	13	98	68	38	29·91	69	38	31	62	60	06	51	26	97	41	70	40	95	99	58	29	68	75	77	00	20	67	36	29·97	99	58	27	17	54	10	30
Oban,	56 24	5 27 w	01	88	53	14	28·87	79	20	18	61	46	29·99	44	18	65	34	67	31	89	83	46	24	59	60	64	29·93	10	45	16	28·98	97	48	26	16	47	12	32
Ball[illegible]ston,	55 52	4 0 w	11	96	52	23	29·00	90	37	19	60	63	30·10	54	33	94	44	70	37	94	96	57	36	71	76	71	30·11	20	60	21	95	30·05	62	25	22	51	4	40
Inveresk,	55 56	3 3 w	04	90	59	20	00	83	34	24	64	51	04	50	24	84	37	58	34	91	92	56	28	64	74	78	08	02	59	18	94	02	47	27	11	50	14	38
Thurston,	55 57	2 27 w	04	91	40	24	05	65	34	04	59	54	15	45	20	85	35	63	35	85	89	63	24	64	73	80	13	13	65	15	85	29·96	55	25	13	74	65	25
Milne Graden,	55 42	2 12 w	10	95	51	36	06	65	35	12	62	51	10	53	27	92	40	62	39	92	30·06	63	36	72	75	80	12	21	76	23	93	30·03	60	24	14	63	01	15
Drumlanrig,	55 17	3 48 w	05	93	60	24	06	87	34	05	58	60	07	48	20	82	37	67	34	89	01	64	29	70	73	70	09	14	58	12	73	10	67	33	22	52	03	40
Cargen,	55 2	3 43 w	03	90	40	10	06	84	30	28·97	52	61	03	44	19	76	19	63	31	89	29·91	63	34	64	67	67	08	10	51	09	76	08	61	29	18	77	06	34
South Cairn,	54 58	5 8 w	04	30·10	54	20	08	96	45	29·15	64	67	15	50	20	91	47	70	46	69	90	63	48	71	80	67	10	13	50	14	29·11	12	71	18	20	19	20	12
ENGLAND.																																						
[illegible]	54 52	3 21 w	07	29·91	49	20	02	91	37	29·97	68	63	10	52	17	61	40	68	36	90	98	59	43	68	73	72	07	13	57	07	28·77	14	68	39	35	63	09	42
Shields,	55 0	1 27 w	10	94	54	36	13	91	44	94	68	...	12	53	23	...	36	67	34	84	30·05	80	55	70	70	87	24	21	81	15	81	11	75	31	25	61	29·92	30
Scarborough,	54 12	0 23 w	08	98	55	40	27	29·03	40	67	65	68	13	48	22	50	30	60	36	96	07	36	55	70	72	91	27	22	84	17	77	11	85	34	27	67	73	42
Leeds,	53 48	1 32 w	07	95	47	25	12	13	43	69	69	72	14	50	19	79	29	69	43	89	02	62	53	69	76	85	21	13	64	01	79	13	80	14	20	67	96	43
Liverpool,	53 25	2 59 w	11	99	57	30	09	15	45	65	74	73	13	53	20	81	37	77	38	86	00	68	64	75	80	82	22	17	69	04	84	20	93	45	33	81	30·04	52
Holyhead,	53 18	4 39 w	01	94	58	13	05	15	48	53	61	77	17	48	15	95	34	79	40	84	29·97	61	73	70	76	67	10	08	47	01	29·02	25	80	44	31	83	09	49
Nottingham,	52 58	1 8 w	11	30·01	64	42	24	32	52	82	71	72	20	54	21	80	32	79	45	91	30·13	84	64	83	86	97	36	29·86	72	02	28·83	25	07	42	35	64	03	30
Yarmouth,	52 37	1 43 E	06	00	81	54	40	38	58	92	72	76	18	47	21	93	16	92	44	72	09	96	68	82	93	30·03	40	30·23	92	26	96	19	30·11	38	33	83	29·76	39
Banbury,	52 4	1 21 w	07	02	68	41	32	41	55	94	82	85	20	51	23	81	26	82	35	82	17	78	77	84	86	02	35	16	77	29·49	29·05	32	08	42	34	84	02	48
Clifton,	51 27	2 37 w	09	29·99	65	33	29	51	57	93	83	30·01	22	53	25	83	29	85	32	90	14	75	81	84	84	29·96	29	12	70	97	25	38	14	49	40	95	30·05	54
Pembroke,	51 40	4 54 w	08	99	62	21	10	64	...	89	74	29·80	17	47	28	...	34	92	29	92	06		...	77	71	84	16	05	59	07	27	30	29·88	50	30	09	14	55
London,	51 31	0 6 w	06	30·00	70	49	45	51	60	92	86	31	21	48	28	57	17	94	41	70	19	86	64	68	65	30·04	36	15	67	29·91	28·98	31	30·18	45	39	95	29·85	45
Dover,	51 7	1 18 E	08	00	72	52	61	69		29·01	9[illegible]	30·00	25	49	29	...	11	95	42	79	...		...	92	95	11	43	20	93	07	29·99	38	22	46	41	96	85	44
Portsmouth,	50 48	1 6 w	01	29·99	70	48	43	49	73	28·94	90	00	26	48	29	93	24	97	30	80	19	08	92	68	87	04	31	12	84	28·90	14	38	43	40	42	95	91	46
Weymouth,	50 37	2 27 w	07	30·00	68	44	41	58	65	06	91	06	20	49	31	69	24	04	18	83	17	53	87	88	86	29·97	24	10	78	08	25	42	23	49	42	30·00	30·01	52
Plymouth,	50 22	4 9 w	08	03	69	39	37	64	.	29·00	9[illegible]	11	29	44	34	.	28	95	10	69	14	.	.	61	84	93	22	16	71	00	34	48	25	61	44	00	10	50
Penzance,	50 7	5 33 w	09	02	73	35	35	67	72	03	92	12	30	46	26	30·07	31	30·00	3[illegible]	93	10	72	69	67	90	86	13	02	67	29·24	50	51	23	76	44	00	22	57
NORWAY AND SWEDEN.																																						
Haparanda,	65 52	24 3 E	03	02	98	82	76	13	28·80	28·98	81	29·70	29·23	29·17	29·62	29·71	29·76	29·74	41	60	29·74	41	30·[illegible]	[illegible]	[illegible]	[illegible]	11	[illegible]	11	[illegible]	[illegible]	[illegible]	29·[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Hernosand,	62 38	17 49 E	00	10	06	76	39	00	..	29·33	73	65	09	27	40	78	90	67	37	40	78	63	29·70	52	[illegible]	[illegible]	33	[illegible]	[illegible]	29·[illegible]	60	69	95	71	82	61	17	85
Christiansand,	58 7	7 46 E	29·87	29·82	70	39	28·88	60	65	36	18	29	25	40	71	30·03	30·03	28	24	31	79	71	30	31	81	29·[illegible]	13	24	12	40	29·9[illegible]	67	28	78	12	12	[illegible]	91
Skudesnæs,	59 10	5 15 E	30·14	30·02	75	44	29·09	72	29·07	30	33	20	66	30·03	00	29·98	29	66	29	60	30·04	80	23	50	04	30·00	37	62	37	2	29·1[illegible]	71	68	30·05	30·15	30	45	30·20
Mandal,	58 3	7 25 E	23	14	00	56	25	60	14	35	48	47	71	29·99	81	97	32	82	34	59	15	99	45	55	55	10	47	53	33	71	22	58	56	18	05	52	11	24
Christiania,	59 54	10 48 E	15	06	93	55	17	49	28·92	46	58	42	40	67	03	95	22	70	35	63	29·07	84	15	42	92	68	12	63	35	68	17	53	94	29·97	02	59	51	08
Stockholm,	59 17	18 3 E	26	29	30·11	84	65	29·92	29·03	51	70	57	10	43	42	74	03	90	65	55	97	84	64	44	92	91	55	[illegible]	14	29·84	60	38	30·17	54	29·84	[illegible]	31	29·86
RUSSIA.																																						
Archangel,	64 33	40 33 E	28	08	21	30·31	30·31	30·13	01	82	98	30·20	54	05	64	68	29·45	59	94	60	87	82	86	30·17	30·08	12	47	20	04	29·98	30·31	30·45	41	19	72	96	30·13	88
Helsingfors,	60 11	24 57 E	24	15	17	12	29·87	29·45	25	11	99	29·54	29·69	39	23	49	73	94	61	54	57	...	63	29·53	29·77	21	52	43	32	30·17	29·91	29·93	30	72	78	85	29·50	16
St Petersburg,	59 58	30 26 E	31	23	34	33	30·18	84	78	65	30·03	09	29·03	22	26	37	63	85	60	70	81	70	69	61	71	29·97	56	40	29	15	30·09	30·12	52	53	72	96	86	01
Dorpat,	58 17	26 17 E	30	28	34	21	29·99	50	44	79	04	60	05	40	30	43	75	94	72	59	90	70	70	60	79	30·05	56	45	37	16	29·98	00	48	72	65	04	04	68
Riga,	56 57	24 3 E	39	22	29	21	30·00	01	59	09	29·97	63	30	59	32	42	81	30·10	68	72	30·06	98	91	77	66	16	37	46	46	29	98	29·94	38	74	68	30·01	74	68
Libau,	56 30	21 0 E	32	21	[illegible]	[illegible]	29·74	[illegible]	[illegible]	67	76	63	16	46	40	48	30·11	04	[illegible]	60	[illegible]	94	82	[illegible]	71	11	40	50	48	18	[illegible]	71	30	66	71	[illegible]	51	[illegible]
Warsaw,	52 14	21 7 E	30	31	[illegible]	[illegible]	[illegible]	73	[illegible]	[illegible]	77	30·13	[illegible]	[illegible]	[illegible]	76	[illegible]	24	77	[illegible]	[illegible]	[illegible]	30·06	[illegible]	94	24	19	04	[illegible]	[illegible]	96	[illegible]	45	[illegible]	[illegible]	[illegible]	[illegible]	02
Moscow,	55 42	37 39 E	26	31	38	46	46	30·16	30·12	30·00	30·20	05	66	[illegible]	[illegible]	3	29·6[illegible]	[illegible]	30·22	65	[illegible]	29·91	29·93	[illegible]	[illegible]	[illegible]	36	[illegible]	31	12	30·14	30·47	30·54	72	74	29·[illegible]	87	30·10
[illegible]	57 44	40 7 E	22	28	[illegible]	[illegible]	42	13	[illegible]	[illegible]	[illegible]	13	[illegible]	25	[illegible]	25	41	61	10	01	29·90	69	10	15	[illegible]	87	31	24	13	29·07	08	43	78	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Catherinenburg,	56 50	60 35 E	20	00	[illegible]	[illegible]	21	44	18	35	90	52	30·11	75	77	79	72	48	[illegible]	[illegible]	25	71	4	71	30·16	29·90	29·81	29·14	52	[illegible]	29·13	29·91	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	07
Slatoust,	55 11	59 40 E	15	29·80	29·90	29·90	17	37	37	35	25	31	18	90	80	71	76	50	77	17	25	7	47	63	29·97	29·95	75	50	5	41	32	88	8	29·[illegible]	29·94	29·97	[illegible]	71
Orenburg,	51 47	55 12 E	29·57	61	72	07	29·93	29·92	17	21	18	14	29·92	61	79	72	68	[illegible]	99	[illegible]	71	71	66	76	78	61	67	52	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Kiev,	50 27	30 34 E	30·15	30·00	30·28	30·28	30·21	30·18	11	08	29·88	11	30·17	09	66	78	75	30·02	30·32	[illegible]	30·18	30·32	30·09	07	18	30·13	30·43	[illegible]	60	30·16	30·18	30·11	64	50	61	30·11	29·92	88
Lugan,	48 34	39 20 E	29·74	29·78	01	22	30	29	31	23	30·11	15	08	89	55	75	78	00	52	30·27	21	09	05	30·11	30·09	29·[illegible]	11	08	24	17	29·98	10	14	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Nicolaief,	46 58	32 0 E	46	75	29·01	20	30	20	11	05	06	06	01	98	85	61	90	07	15	29·9[illegible]	24	21	13	18	07	18	21	30·29	25	4	91	29·94	23	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Odessa,	46 28	30 43 E	39	74	95	17	25	23	29·91	29·92	29·94	29·97	04	01	85	84	90	01	00	41	21	22	16	11	29·9	30·0[illegible]	12	24	32	2	94	58	25	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Tiflis,	41 42	44 48 E	83	98	30·15	23	19	18	30·52	30·62	30·50	30·28	10	30·03	30·11	30·15	30·19	7	61	30·55	43	49	46	46	30·21	15	23	21	34	09	30·17	30·20	66	41	9	30·09	21	[illegible]
DENMARK.																																						
Ivigtut (Greenland),	61 12	48 44 w	65	92	29·50	29·80	29·98	29·03	29·90	29·72	29·91	44	15	29·60	29·84	29·21	29·51	29·01	29·29	29·51	29·43	29·12	29·11	28·81	29·7	[illegible]	29·05	29·[illegible]	28·92	29·02	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	29·53	12	29·53
Skagen, or the Skaw,	57 42	10 34 E	30·27	30·14	91	61	41	28·80	16	51	49	43	29·69	80	70	90	30·22	85	29	53	30·05	30·04	85	29·40	95	30·00	30·47	30·12	[illegible]	82	1	51	3	[illegible]	29·[illegible]	5	29·[illegible]	30·07
Copenhagen,	55 40	12 35 E	30	18	96	70	53	29·40	40	62	49	60	72	63	71	2	22	30·11	61	71	21	18	75	12	95	2	67	16	16	59	30	21	17	[illegible]	30·04	75	[illegible]	13
Kiel,	54 19	10 8 E	25	13	68	67	65	37	44	58	36	56	85	30·02	91	91	15	10	45	61	23	37	74	67	64	06	47	45	2	80	30	15	15	07	[illegible]	81	16	09
Heligoland (England),	54 11	7 53 E	11	00	81	62	60	29			27	53	04	06	99	[illegible]	15	15	31	59	25			72	5	86	45		18	[illegible]				12	[illegible]	6	37	19
PRUSSIA.																																						
Königsberg,	54 43	20 30 E	36	34	30·26	30·10	90	40	62	77	77	29·85	60	29·61	53	63	29·99	16	67	62	28	15	91	63	94	18	47	63	47	30·21	88	71	67	29·86	29·71	30·00	[illegible]	29·71
Putbus,	54 23	13 27 E	33	19	29·97	29·74	69	45	45	63	46	61	77	83	71	55	30·11	69	49	58	25	11	80	70	95	10	46	60	35	29·91	16	16	73	[illegible]	[illegible]	[illegible]	51	92
Paderborn,	51 43	8 45 E	17	17	85	65	70	63	69	61	45	30·07	30·17	71	78	29·93	06	06	53	53	27	13	88	83	30·01	67	43	34	17	77	54	73	32	29·97	27	30·05	76	30·17
Breslau,	51 7	17 2 E	21	18	30·05	64	62	70	81	69	30·00	29·89	29·98	[illegible]	[illegible]	86	14	61	55	35	21	30·00	94	03	10	33	[illegible]	45	31	30·08	83	73	09	30	29·93	29·85	05	29·86
AUSTRIAN EMPIRE.																																						
Prague,	50 5	14 26 E	17	22	05	96	92	77	67	87	75	06	30·13	30·02	30·10	30·00	93	14	68	63	45	34	17	30·05	12	20	41	42	37	15	90	60	38	30·09	30·12	16	77	93
Cracow,	50 4	19 56 E	21	21	17	30·11	30·03	29·99	57	80	74	15	09	29·87	06	29·87	97	22	71	55	45	32	15	10	09	26	37	61	54	25	97	87	40	29·77	29·95	14	81	30·02
Vienna,	48 13	16 22 E	20	19	09	02	29·99	30·11	92	92	33	16	22	30·11	04	30·03	91	12	76	58	40	25	15	10	16	38	37	40	45	18	30·00	67	45	30·03	30·06	17	84	30·02
Kremsmünster,	48 3	14 7 E	21	15	11	29·94	30·01	08	92	57	10	19	52	21	25	05	12	4	71	56	48	25	13	17	13	21	38	27	25	10	01	51	45	21	18	24	21	[illegible]
Klagenfurt,	46 37	14 19 E	14	11	02	30·00	99	29·82	77	75	76	19	21	68	12	29·95	73	29·99	63	52	34	44	32	21	20	32	38	41	42	25	10	90	42	10	19	23	93	29·91
GERMAN STATES.																																						
Leipzig,	51 21	12 21 E	32	20	00	29·83	81	71	86	83	72	29·98	15	20	20	30·06	30·07	30·11	63	73	41	25	03	00	18	12		31	32	02	29·82	78	35	17	2	12	78	30·08
Bayreuth,	49 57	11 52 E	21	14	00	80	80	81	87	80	74	30·02	22	18	27	29·77	29·77	04	04	61	42	31	11	01	14	21	45	34	21	04	65	62	33	24	3	15	81	29·99
Munich,	48 9	11 33 E	17	15	29·93	80	94	91	85	79	30·02	14	35	24	23	99	79	29·93	60	60	28	28	15	04	14	21	41	22	18	29·93	30·04	[illegible]	47	27	33	15	85	30·02
SWITZERLAND.																																						
Aarau,	47 24	8 3 E	03	04	68	81	60	84	94	72	97	18	42	38	25	30·01	69	85	66	47	33	32	11	29·98	02	15	43	10	15	90	94	40	41	[illegible]	51	[illegible]	[illegible]	29·9[illegible]
Geneva,	46 12	6 9 E	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
NETHERLANDS.																																						
Groningen,	53 13	6 34 E	15	05	72	61	57	42	53	41	40	29·65	04	20	15	29·95	30·18	30·00	43	67	26	05	29·78	82	29·95	10	48	37	08	60	37	29·78	[illegible]	[illegible]	[illegible]	[illegible]	44	[illegible]
Leeuwarden,	53 12	5 47 E	15	06	76	70	56	43	64	35	44	[illegible]	12	34	18	07	20	04	41	69	27	04	79	63	30·00	11	49	37	13	57	36	00	[illegible]	[illegible]	...	...	...	...
Helder,	52 57	4 45 E	01	02	73	69	53	38	60	26	48	66	12	34	19	05	14	00	41	67	24	00	75	82	29·96	07	46	32	05	50	41	95	[illegible]	[illegible]	...	...	...	...
Amsterdam,	52 22	4 53 E							...											...																		
Utrecht,	52 5	5 7 E	11	03	70	02	53	64	64	29	63	78	17	35	23	05	10	00	47	70	27	05	87	88	30·01	12	45	30	10	51	31	30·08	...	12	35	[illegible]	[illegible]	[illegible]
Hellevoetsluis,	51 50	4 7 E	09	03	76	03	60	54	64	19	69	81	20	39	26	95	16	00	49	70	26	04	86	88	01	11	45	28	06	44	27	13	..	35	36	47	62	11
Breda,	51 35	4 46 E	05	00	74	66	69	60	63	29	60	78	10	32	19	04	29·99	05	45	67	21	00	82	84	29·95	01	37	29	02	48	24	12	27	11	11	[illegible]	01	[illegible]
Flushing,	51 26	3 35 E	09	03	77	63	63	61	69	14	61	59	24	42	28	92	97	02	50	71	27	03	89	90	30·02	13	48	27	05	38	25	22	[illegible]	41	41	30·01	61	51
BELGIUM.																																						
Brussels,	50 52	4 21 E	12	04	63	70	70	73	60	26	91	94	28	44	32	05	07	03	55	72	32	09	00	95	07	17	47	28	10	49	39	25	10	44	39	12	75	3
Maestricht,	50 51	5 42 E			...		...		...	..	...		...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...										
Luxembourg,	49 37	6 6 E	15	09	91	80	63	87	84	62	97	30·07	43	39	35	30·04	82	04	68	70	39	23	30·06	95	14	21	45	20	16	85	57	32	17	...	46	18	84	21
FRANCE.																																						
Dunkirk,	51 7	2 39 E	00	09	76	60	04	02	72	10	56	29·91	30	44	20	29·90	30·04	04	48	76	24	29·90	29·84	84	00	13	47	28	00	27	21	27	30	47	42	04	76	75
Mézières,	49 45	4 43 E	10	05	50	76	76	87	66	42	05	30·05	38	43	36	30·03	29·80	03	60	72	32	30·16	30·03	90	07	18	48	19	11	60	31	41			43	35	63	28
Havre,	49 29	0 6 E	02	00	70	64	68	80	81	15	30·03	18	40	46	34	07	30·02	...	45	62	27	01	00	30·00	00	14	38	21	29·92	18	28	47	35	52	44	14	95	41
Cherbourg,	49 39	1 37 w	03	29·90	75	54	54	71	71	00	30·03	15	39	47	36	05	12	02	24	80	30	00	01	04	02	20	39	19	84	11	29	51	36	53	47	06	30·04	44
Paris,	48 50	2 20 E	06	99	61	70	75	87	58	29	07	17	41	42	30	98	29·84	03	55	70	9	11	29·81	29·99	04	16	10	17	30·03	7	50	41	45	14	46	15	29·9[illegible]	32
Strasbourg,	48 35	7 42 E	18	30·12	07	..	80	97	30·00	70	06	15	45	53	38	06	88	01	74	67	43	32	...	30·00	14	22	47	25	24	69	87	26	56	...	47	22	29	17
Brest,	48 23	4 27 w	00	01	76	51	68	84	29·85	22	12	32	49	51	45	19	30·18	00	37	92	12	29·85	96	29·94	29·92	29·67	21	29·92	29·82	35	52	52	37	60	43	20	30·20	47
L'Orient,	47 44	3 21 w	02	00	78	60	66	88	93	27	16	29	48	48	42	22	09	13	43	80	12	06	30·01	98	95	30·02	32	30·03	91	41	47	69	40	51	43	20	16	40
Napoléon Vendée,	46 41	1 25 w	05	01	78	74	76	98	30·00	40	30	...			47	31	00	15	61	..	...	30·11	09	30·04	30·04	13	30	05	30·03	63	62		55	62	46		15	39
Besançon,	47 17	6 5 E	07	04	68	84	84	97	02	67	13	29	63	48	36	17	29·70	29·92	69	52	36	21	11	10	09	21	35	20	18	77	90	36	60	43	41	24	29·91	00
Lyons,	45 45	4 49 E	14	29·98	93	85	94	99	10	79	25	41	60	51	42	21	88	30·00	67	61	43	32	21	16	18	18	41	27	24	67	30·06	...	57	47	20	20	30·02	09
Limoges,	45 5[illegible]	1 17 E	02	30·00	75	78	87	90	08	53	25	41	51	47	35	23	85	01	72	62	47	13	13	05	02	21	32	07	09	63	29·88	51	50	41	34	21	04	18
Rochefort,	45 56	0 57 w	00	29·96	70	76	80	96	00	43	25	41	55	51	36	...	92	05	69	70	24	01	11	00	00	05	24	26	00	57	02	61	52	43	35	28	12	25
Bordeaux,	44 50	0 31 w	.	...	.	.	.	.	...	..	...	...	...	...	...	...	...	...	...	...	17	29·93	09	08	03	14	26	03	04	72	91	63	54	42	39	26	11	30
Montauban,	44 1	1 22 E	01	93	77	85	09	30·00	08	73	28	52	56	49	41	29	84	06	62	70	29	30·15	17	13	08	18	25	00	10	82	30·05	53	67	41	38	34	06	14
Bayonne,	43 31	1 29 w	29·92	68	57	72	..	29·96	04	09	32	51	...	43	47	20	30·00	16	64	76	16	08	08	08	00	06	20	29·84	00	81		63	47	36	36	32	12	20
Montpellier,	43 36	3 54 E	30·07	30·05	69	68	97	30·04	00	89	27	39	43	35	24	29·97	29·84	29·91	74	54	39	33	24	14	13	24	34	30·17	22	57	10	42	38	27	30	29	29·97	29·93
Marseilles,	43 17	5 22 E	06	06	69	86	99	03	29·93	09	17	33	34	23	15	30·00	78	62	77	64	39	34		10	12	24	33	12	23	30·04	10	25	29	17	26	24	05	62
Toulon,	43 7	5 50 E	04	08	68	68	30·00	04	06	92	08	33	32	25	...	29·96	70	76	69	49	44	32	...	25	21	25	41	12	20	05	...	12	91	12	24	24	32	84
SPAIN AND PORTUGAL.																																						
Bilbao,	43 15	2 54 w	29·02	29·87	67	81	29·86	04	30·13	60	...	69	50	81	48	30·30	30·09	30·13	...	63	20	...	09	06	02	05	21	01	00	29·93	01		11	43	33	37	30·21	30·30
Barcelona,	41 23	2 9 E	30·02	98	78	81	96	08	29·92	94	26	40	41	31	21	11	29·83	29·87	65	55	34	30	10	13	21	17	24	08	15	30·03	12	19	24	21	24	25	29·66	29·80
Oporto,	41 7	8 33 w	29·84	83	...	00		13	30·26	30·15	50	54	44	44	35	26	30·20	30·22	...	30·14	13	29·91	16	04	0[illegible]		10	29·95		22	37	51	47	30	35	15	30·24	30·25
Lisbon,	38 41	9 10 w	77	84	64	94	30·12	19	21	23	43	49	43	41	30	23	09	14	30·27	17	16	30·20	18	00	06	12	09	93	11	26	39	47	15	36	52	34	17	42
Madrid,	40 25	3 41 w	06	97	90	93	12	19	11	15	43	63	56	54	42	32	12	10	19	29·99	45	36	25	09	19	30	32	30·00	19	25	35	01	29	45	41	41	24	2
Alicante,	38 23	0 25 w	06	88	82	87	06	13	00	07		45	49	39	...	25	29·94	29·95	...	75	36	36	26	17	14		[illegible]	06	10	21	40	35	38	31	30	31	41	61
San Fernando,	36 27	6 11 w	85	80	77	80	05	16	10	20	32	45	36	30	25	15	30·05	99	20	30·03	24	26	15	07	08	13		29·95	05	21	40	35	10	34	25	25	14	45
Palma (Majorca),	39 35	2 43 E	30·01	30·04		84	06	13	00	05	31	41	46	34	35	22	29·88	94	29·91	29·63	45	36	30	10	15	31		30·08	23	16	20	36	32	27	24	25	03	29·87
AFRICA.																																						
Algiers,	36 48	3 3 E	02	02	69	94	14	29·88	..	14	36	48	54	42	32	31	63	30·07	30·09	76	43	44	30	29	15	.		06	31	27	34	34	35	35	36	35	19	95
ITALY.																																						
Turin,	45 3	7 41 E		05	30·02	30·05	05	30·03	06	29·80	29·94	25	32	20	15	...		29·93	29·82	56	46	49	25	19	14	26	33	26	25	17	03	29·94	29	15	19	26	29·60	69
Leghorn,	43 33	10 16 E	06	10	04	29·98	03	08	29·96	69	90	22	33	21	12	07	69	80	62	52	28	31	30	13	14	24	...	...	22	16	29·96	76	16	15	21	26	9[illegible]	62
Florence,	43 47	11 14 E	09	12	04		07	10	...	...	94	25	..	...	12	...		71	81	...	59	40	32	23	...	25	34	17	20	17	96	.	22	15	24	26	30·12	56
Ancona,	43 37	13 31 E			29·83	62	29·84	29·78	76	62	64		00	01	29·91	29·82	58	81	70	60	14	16	26	21	17	25	..	19	23	26	96	30·01	10	15	22	27	10	98
Rome,	41 54	12 30 E	09	19	30·01	30·06	30·12	30·17	30·07	98	97	18	27	22	30·10	30·04	78	76	65	65	32	29	21	20	15	25	34	10	20	19	29·83	29·74	29·86	17	22	24	09	91
Naples,	40 50	14 15 E	01	04	00	03	00	22	16	30·05	00	12	25	22	12	06	84	86	81	76	30	39	31	21	19	9[illegible]	34	10	14	16	82	75	67	18	26	31	13	98
Palermo (Sicily),	38 7	13 22 E	04	03	29·95	02	07	13	07	29·99	17	11	11	12	04	03	55	63	75	76	16	19	11	17	14	15	23	02	04	10	...	57		14	19	21	06	89
TURKEY.																																						
Constantinople,	41 0	28 59 E	00	10	30·08	16	43	...	17	30·09	25	04	16	06	01	29·96	93	30·00	30·07	04	22	26	18	14	06	13	22	24	22	12	93	93	30·18	29·78	29·96	00	19	30·15
GREECE.																																						
Athens,	37 58	23 43 E	01	29·90	29·99	14	31	21	09	09	04	29·92	00	05	29·92	67	88	29·92	29·96	75	10	06	06	02	29·95	06	14	14	15	15	96	93	03	67	30·06	00	12	29·71

TABLE II.—Showing the Temperature (Fahr.) and State of the Sky in the Morning at 110 Places in Europe, from 26th October to 12th November, from 20th to 26th November, from 30th November to 5th December, and 14th to 18th December 1863.

NOTE.—B represents blue sky, or sky little more than half covered with clouds; C, a much clouded or overcast sky; F, fog; R, rain at the time of observation; and r, rain sometime during the previous 24 hours.

	OCTOBER.						NOVEMBER.																				DECEMBER.									
	26	27	28	29	30.	31	1.	2.	3.	4.	5.	6	7.	8	9.	10.	11.	12.	20.	21	22	23.	24	25.	26	30.	1.	2.	3.	4.	5	14.	15	16	17.	18
IRELAND.																																				
Greencastle,	46 c	47 c	41 cr	44 B	40 cr	44 cr	39	43 cr	41 B	[illegible]	41 cr	[illegible]	44 cr	45	37 c	43 B	41 cr	43 cr	52 cr		49	46 cr	43 cr	[illegible]	54 B	46 c	49 cr	40 cr		41 cr	47 B	45 cr	45 c	[illegible]	41 cr	44 c
Galway,	[illegible]	49 c	44 cr	51 B	42 cr	44 B	33	42 cr	52 [illegible]	58 B	48 [illegible]	37 cr	52 cr	44	[illegible]	42 cr	42 cr	41 cr	54 cr		47	[illegible]	46 cr	[illegible]	53 cr	44 c	47 cr	40 cr	32	34	45	45 cr	45 cr	41 cr	42 cr	45 B
Valencia,	[illegible]	49 c	43 cr	48	46	45 B	45 B	46 B	53	54 cr	[illegible]	54 cr	54 cr	45 r	42 cr	45 cr	44 cr	43 cr	54 cr		50	[illegible]	46 cr	[illegible]	54 B	41 cr	45	44 cr	41 B		41 cr	[illegible]	45 cr	47 cr	44 cr	41 c
Queenstown	[illegible]	51 c	45 cr	52 cr	44 cr	49 c		43 cr	51 B	55 cr	55 cr	[illegible]	53 cr		45 c	47	43 cr	41 B	55 B			49 c	[illegible]	52 B	51 c	51 c	[illegible]	42 cr		46 cr	52 B	52 c	52 c	47 c	44 cr	40 B
GREECE.																																				
Athens,	60	[illegible]	57	58	63	64	58	60	62	62	61	60	61	59	62	65	65	63	[illegible]	[illegible]	57	61	62	[illegible]	60	53										
TURKEY																																				
Constantinople,	[illegible]	[illegible]	45 B	44 B	[illegible]		52 B	54 B	53 B	55 B	58	51	57 c	64	46 B	50 B	61	60	55	54 c	57	58	65 c	63 c	65	45 B	45 B	43 c	43 c	41 B	41 B	50	34 B	32 B	34 c	45 c

TABLE III.—Showing for 69 places in Europe the Deviations from the Mean Temperature (Fahr.) of each day from 26th October to 12th November, from 20th to 26th November, from 30th November to 5th December, and from 14th to 18th December 1863.

NOTE.—The Temperatures marked with a minus sign are under the Mean, the others are above the Mean.

	OCTOBER.						NOVEMBER.																				DECEMBER.									
	26.	27.	28.	29.	30.	31.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	20.	21.	22.	23.	24.	25.	26.	30.	1.	2.	3.	4.	5.	14.	15.	16.	17.	18.
IRELAND.																																				
Greencastle,	2	3	0	0	-4	-0	-5	-1	-2	7	-1	-8	0	4	-7	-1	-3	-1	11	...	8	5	-1	10	13	5	8	-1	...	3	6	7	7	-1	1	4
Valencia,	6	2	-4	1	-1	-2	-2	-1	6	7	6	7	7	2	-4	2	-2	-3	9	...	5	5	2	6	10	5	4	1	-4	...	5	5	3	2	0	-1
SCOTLAND.																																				
Bressay,	3	1	-1	-6	-4	-2	-2	-1	-2	0	-3	-6	-5	-6	-7	1	2	1	8	8	7	8	7	6	9	4	2	-2	-4	7	4	5	5	3	-7	6
Stornoway,	1	2	-1	-1	-2	-0	-2	-4	-1	2	-2	-2	-6	-2	-7	2	-0	-5	9	9	8	6	5	7	6	2	4	-2	-4	7	5	7	6	1	7	6
Elgin,	-2	-0	-5	-5	-5	-3	-5	-11	-7	3	-6	-10	-4	-8	-8	-3	-5	-6	8	9	4	6	0	10	10	1	2	-3	-5	2	8	5	6	0	-1	4
Barry,	-1	1	5	-3	-4	-0	-4	-4	-5	7	-3	-8	-6	-2	-4	-2	-6	-3	4	12	6	3	5	8	11	2	3	3	-4	4	13	5	7	0	5	3
ENGLAND.																																				
Silloth,	1	2	3	-3	-4	1	-3	-3	2	9	3	-8	-5	-1	-3	1	-3	-4	1	14	7	8	11	15	17	-5	1	1	-1	6	12	9	8	4	0	3
Shields,	1	5	5	0	-1	4	5	7	-1	...	3	-4	-3	2	7	3	-2	-2	2	9	10	9	11	11	18	7	11	9	6	3	14	12	13	8	7	4
Holyhead,	3	3	3	1	0	2	...	1	2	11	4	1	8	...	4	4	2	3	11	...	...	10	10	13	14	5	8	6	3	6	11	12	11	7	8	4
Pembroke,	2	3	2	7	3	-3	...	2	7	10	9	0	7	...	-2	5	-1	-1	9	..	...	9	8	13	14	4	9	9	3	5	10	12	11	9	7	3
Yarmouth,	-3	-0	5	4	0	-3	-3	5	-3	2	5	-1	-5	4	2	-5	-6	-6	4	7	2	8	7	10	9	3	5	9	9	-2	6	3	4	7	3	2
London,	-6	-6	3	1	3	-2	-2	-2	-4	13	9	-6	1	6	-2	-8	-9	-7	6	10	3	4	10	13	10	-6	3	9	5	-5	8	-1	3	8	4	-4
Weymouth,	-4	5	5	3	3	1	-2	-2	1	9	8	0	7	9	-2	3	1	-4	7	11	8	10	12	12	11	4	9	10	7	3	10	9	9	11	5	-6
Penzance,	0	0	2	3	2	-3	-2	-2	4	7	7	6	7	5	1	1	-1	-3	10	9	7	8	5	11	10	7	10	3	3	4	9	9	9	8	5	-1
NORWAY AND SWEDEN.																																				
Haparanda,	9	1	2	13	12	13	14	8	3	8	1	-1	3	2	-9	-9	8	14	5	6	-19	-7	21	-26	17	18	17	18	18	21	23	0	-2	-8	14	9
Hernosand,	6	-5	0	8	13	13	...	6	4	8	6	0	1	-3	-4	-1	6	8	13	19	2	-4	-6	0	10	3	16	7	15	17	18	-4	-9	9	15	0
Christiansund,	3	3	4	6	9	9	4	1	6	5	2	-1	4	-3	-2	7	2	4	18	16	11	9	10	11	12	11	-3	13	16	9	14	7	3	8	6	4
Skudesnes,	10	9	6	6	6	4	6	4	7	6	4	-1	5	-4	-7	6	3	3	12	10	10	7	8	12	13	3	6	7	5	5	12	4	8	12	7	1
Mandal,	9	8	6	10	5	6	5	-2	7	5	3	-4	1	-6	-6	7	-2	0	13	10	12	3	2	9	13	3	5	7	6	5	11	2	9	12	5	-2
Christiania,	-1	6	3	6	8	3	-1	-4	4	4	4	1	-4	1	-13	-3	1	1	8	7	11	6	4	4	11	5	7	5	6	8	11	-8	-3	7	10	-4
Stockholm,	-11	-4	1	2	6	7	4	1	2	1	-1	-7	-7	-9	-15	-8	-6	-5	13	12	6	6	-2	5	4	9	5	7	11	14	14	-5	-2	8	14	1
RUSSIA.																																				
Archangel,	-5	4	5	5	4	-1	6	5	6	-2	8	6	5	-2	-1	-10	-21	6	4	0	-17	-19	-20	-17	10	13	16	16	7	-14	3	3	2	-11	-12	-5
St Petersburg,	-1	1	-3	-4	-9	3	2	8	6	4	9	2	-3	-1	-3	-7	-1	0	14	16	11	18	12	10	0	11	10	10	3	3	4	-2	-8	-2	-3	6
Dorpat,	-0	-3	-4	-4	-2	-1	4	5	1	8	7	2	-0	-2	-7	-9	-1	4	14	13	12	12	4	1	-1	9	8	2	-4	-4	2	-4	-2	-9	2	2
Moscow,	-18	-3	-7	-6	-7	-4	-7	-3	2	-1	11	2	1	-0	-6	-17	1	6	10	1	17	13	18	17	21	11	8	14	8	-10	-10	16	1	-6	-11	-7
Warsaw,	-3	-13	-11	-8	-3	10	6	7	9	0	19	6	-2	1	-10	-7	-2	4	12	1	1	8	10	10	11	2	0	-8	-5	1	1	-3	1	-3	8	10
Kiev,	-8	-18	-14	1	-22	-18	-9	-4	14	10	4	3	-6	-3	-8	-5	-7	4	11	10	0	-1	-2	7	2	...	-6	-3	...	-1	-2	...	-3	-13	-10	-2
Nicolaiev,	-4	-11	-10	-16	-20	-12	-6	3	7	11	-3	5	2	3	-6	-7	-6	1	6	9	2	10	12	3	-4	-3	-3	-9	-6	3	-2	-1	-4	-11	-17	-17
DENMARK.																																				
Copenhagen,	-2	-1	2	0	6	0	2	0	3	2	3	-3	-0	-10	-8	-5	-4	2	7	7	8	10	8	10	7	3	1	-3	2	6	-0	-3	-6	9	4	-1
Kiel,	-5	-5	-3	1	4	1	1	0	1	9	5	0	1	1	-3	-4	-1	0	4	3	9	11	10	8	6	-5	-5	-3	4	4	7	10	13	11	7	4
PRUSSIA.																																				
Königsberg,	-4	-7	-8	-4	4	5	5	2	4	6	8	-1	-1	-4	-4	-6	-6	-4	5	2	3	9	8	9	7	6	-0	-2	-0	-1	-2	-2	-0	1	4	4
Putbus,	-3	-2	-2	3	9	3	2	2	1	7	4	-1	1	-1	-4	-6	-3	-0	3	0	4	9	9	6	4	-2	-4	-6	2	4	2	6	10	10	6	1
Paderborn,	-4	-2	-0	8	10	3	3	2	2	11	7	-2	-2	-2	-4	-7	-2	-7	8	3	10	6	10	14	12	-8	-6	1	4	5	7	10	9	8	5	1
Breslau,	-2	-7	-2	-2	5	6	5	4	8	6	12	-1	-4	-2	-7	-10	2	1	5	5	6	8	10	10	8	-3	-6	-5	1	6	6	3	11	10	9	6
AUSTRIAN EMPIRE.																																				
Prague,	-5	-8	-5	-0	5	7	2	2	3	6	13	3	-3	-1	-4	-6	0	0	2	3	3	6	7	9	7	-4	-5	-8	5	5	2	8	13	9	7	5
Cracow,	-3	-5	-10	-7	1	7	4	2	5	4	10	1	-4	-3	-7	-8	2	3	4	0	1	2	7	8	8	-2	-6	-9	-5	2	-1	-2	2	2	2	6
Vienna,	-1	-5	-7	-6	1	5	3	3	5	-5	13	11	-6	-0	-5	-7	-1	2	3	3	2	0	1	3	4	-9	-6	-6	-4	-1	5	8	11	8	2	5
Kremsmunster,	-7	-5	-6	-2	-0	6	4	3	2	6	13	7	-0	-0	-2	-0	2	3	3	-0	-3	-2	4	5	10	-7	-7	-6	1	4	3	10	12	6	3	6
Klagenfurt,	-10	-10	-11	-8	-1	7	10	-3	-6	-6	-4	-4	-5	-6	-7	-9	-8	-2	3	-5	-5	-5	-1	0	2	-3	-3	-5	1	4	3	13	14	10	10	14
GERMAN STATES.																																				
Leipzig,	-10	-7	-10	2	14	4	0	-2	-2	7	6	-5	-7	-5	-8	-10	-4	-4	2	-2	5	3	7	5	6	-8	-8	-6	3	3	3	7	9	6	5	-1
Munich,	-7	-7	-2	8	9	7	1	2	1	6	11	4	1	7	3	-2	-0	-3	-2	-2	6	12	12	12	11	-6	-6	-0	7	1	1	10	10	5	4	5
SWITZERLAND.																																				
Aarau,	-4	-2	2	7	11	4	-1	-3	-1	4	8	4	4	8	4	-0	-2	-3	-4	-3	0	9	11	7	6	-3	-2	-3	5	-1	-3	7	1	0	1	3
Geneva,	-2	-1	1	6	11	9	-3	-2	-3	0	1	-2	1	4	2	-1	-4	-4	-4	-7	3	11	7	4	4	-3	-4	4	8	-2	-3	0	0	0	1	3
NETHERLANDS.																																				
Groningen,	-11	-10	-6	0	2	2	1	-3	2	10	4	1	2	-1	-5	-11	-2	-7	-2	-1	7	7	7	12	6	-9	-14	-0	1	7	4	12	10	11	8	3
Helder,	-11	-4	-5	0	3	2	2	-3	3	8	5	0	1	-2	-3	-15	-1	-4	-1	-7	6	7	6	9	6	-9	-12	-3	1	2	7	9	8	10	7	5
BELGIUM.																																				
Brussels,	-12	-14	-0	1	2	5	-2	-0	-1	10	10	4	-8	3	-5	-13	-5	-9	-0	1	5	4	6	13	11	-13	-10	5	5	4	4	5	3	4	2	-1
FRANCE.																																				
Havre,	-1	-1	1	5	2	2	-2	2	2	8	5	6	5	8	2	-3	-2	-1	0	4	6	4	8	8	5	-7	-1	-9	8	3	6	10	8	-5	11	5
Cherbourg,	...	4	1	2	5	2	-2	-2	0	9	7	2	7	6	4	-4	-3	3	0	4	3	1	8	6	5	-4	2	6	4	3	7	8	7	8	6	1
Brest,	-4	-7	-1	3	1	-3	-7	-6	0	5	4	3	2	3	-3	-4	-1	-5	-0	7	-1	1	7	7	2	-5	6	3	4	-1	6	5	3	6	1	-2
L'Orient,	-3	-10	-7	3	3	-2	-8	-6	3	4	2	2	3	2	-5	-13	-4	-8	-4	5	-5	-2	6	4	0	0	6	4	9	-8	4	3	-6	-3	-5	-3
Paris,	-9	-5	-4	-1	-1	-3	-3	-2	-3	8	8	5	0	5	-5	-7	-9	-4	-9	-7	5	4	5	6	3	-10	-8	6	9	-3	-2	2	3	1	1	-1
Strasbourg,	-10	-7	-1	4	9	5	3	0	4	6	11	9	1	8	8	0	2	-1	-0	1	...	16	13	14	5	-2	3	2	12	9	7	...	10	3	7	10
Rochfort,	-5	-8	1	-1	2	1	-3	-3	-5	1	0	0	...	...	-4	-9	-4	...	...	-2	2	5	2	-1	-3	-2	-1	8	12	-3	-7	2	5	5	5	5
Bayonne,	2	2	6	4	...	8	0	0	0	2	...	-2	-0	1	-9	-2	-0	-2	-4	3	3	3	3	1	0	-1	5	8	...	5	-2	6	5	3	6	4
Montauban,	-5	-5	0	6	2	5	2	-6	-3	-1	-1	-5	-4	2	-2	-5	-7	-6	-9	-5	3	3	3	1	2	-1	3	7	9	-2	-6	3	-0	-3	-0	-1
Marseilles,	5	6	7	7	8	8	8	-6	-3	1	-2	-4	-5	3	3	-1	-6	-6	-2	-2	...	5	8	9	5	-2	1	5	2	-1	-7	-1	1	6	1	-3
SPAIN AND PORTUGAL.																																				
Oporto,	6	8	1	2	...	5	-2	1	-4	-3	4	3	1	8	-1	-5	...	-0	7	9	9	4	6	...	4	5	...	6	6	...	-1	-5	-3	-11	-5	-9
Lisbon,	2	0	6	3	0	3	-2	1	-3	-4	-2	1	0	3	2	-2	-2	-0	-0	3	0	0	7	7	2	1	0	5	6	...	-5	-4	-9	-5	-0	-5
Barcelona,	4	4	7	8	6	7	6	0	0	0	0	-1	2	2	2	-1	-3	-3	-2	-1	3	2	4	0	5	8	4	7	0	3	0	1	1	1	-3	-4
Alicante,	7	8	3	4	3	4	3	2	...	-3	-4	-4	...	4	3	0	-7	-5	-3	-0	-1	4	2	...	-7	7	6	2	5	6	-6	-5	-6	-7	-7	-8
San Fernando,	3	4	2	2	2	0	2	-5	-5	-9	-2	-5	-4	2	-4	1	-8	-2	-2	2	7	12	5	-2	5	7	8	5	9	2	-7	-7	-8	-7	-8	2
Palma (Majorca),	7	7	...	7	6	8	11	5	0	2	...	0	1	6	4	-3	3	-2	-2	-1	0	1	4	...	3	3	0	-1	5	1	-10	-3	-3	1	1	-4
AFRICA.																																				
Algiers,	3	7	8	4	...	...	...	2	-2	-1	1	2	-0	-2	-0	-3	-5	-11	1	0	0	0	4	...	2	-2	3	10	9	6	0	1	1	1	-3	-6
ITALY.																																				
Leghorn,	3	-2	-3	-3	8	10	7	7	4	-4	-4	-4	-2	-5	5	-2	-4	-5	-5	-9	-6	-2	-0	-2	...	-4	-4	3	-6	...	-0	2	2	0	5	2
Rome,	-5	-8	-12	-4	-1	2	4	5	3	-6	-9	-8	-11	-11	1	0	1	-5	-1	-11	-13	-14	-4	4	-5	-4	-3	-8	-4	-2	-4	-10	-4	-14	-11	2
Naples,	-5	-7	-8	-5	-3	-6	1	6	2	-5	-6	-4	-2	-0	1	1	0	-2	-8	-5	...	-4	-3	-2	-2	-5	...	-5	-3	2	-5	-3	-0	-2	-1	-0
Palermo (Sicily),	-0	-1	-1	-2	2	1	4	4	1	-0	-0	-1	-0	-1	0	2	0	-3	2	0	-2	-2	-5	...	-2	-7	-5	-5	-2	2	...	-0	-1	-1	-1	-0
TURKEY.																																				
Constantinople,	...	-14	-14	-14	-6	-0	-4	0	4	4	5	1	-1	1	-1	1	9	7	3	3	4	5	4	2	2	-3	3	0	0	3	1	-0	-4	-2	-3	7
GREECE.																																				
Athens,	-6	-8	-9	-9	-3	-2	-0	2	4	4	3	2	2	1	4	6	7	4	0	0	-1	3	4	1	1	-5	-8	-10	-8	-4	1	-6	-7	-0	3	6

TABLE IV.—Showing the Amount of the Rainfall each Day, in English inches, at 56 places in Europe, from 26th October to 12th November, from 20th to 26th November, from 30th November to 5th December, and from 14th to 18th December 1863.

	OCTOBER.						NOVEMBER.											
	26.	27.	28.	29.	30.	31.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
Ireland.																		
Greencastle,	...	...	·50	·12	40	·43		·30	13	·15	18	·08	43	..	1·32	40	36	·27
Galway,	·17	·17	·50	·18	35	·60		80	61	·65	53	11	25		17	28	40	·16
Valencia,	·14	·14	·52			.		.		35		·93	·57		·45	·24	30	15
Queenstown,	·09	·09	·42	25	37	.		24	30	·16	36	16	20			.	61	
Scotland.																		
East Yell,			37	·92	22	·19	07	10	24	10	10	26	01	..	14	20	36	30
Sandwick,	·11	05	·08	·13	82	02	06	02	03	·34	13	08	11	07	·01	29	11	25
Stornoway,	. .	03	12	50	.		18	.		·26	23	10	05	20	·06	39	25	07
Portree,	·47	13	·10	·04	1·21	1·09	10	16	05	1·00	11	01		58		1·10	66	·16
New Pitsligo,	16	06	83	11		01		06	·08	12	10		26	29		10	..	
Barry,	06	01	16	13	02	01		·01	10	06			01	48	..	·16	...	...
Inveresk,	·01	30	·23	·40	09			03			01		21	50	60			
South Cairn,	30	55	20	30	30	35	10		35	15		25	75	20	35	·25	·35	·15
England.																		
Silloth,		·21	1·25		38	·38		·11	08	·35	·12	.	17	1·25	05	16		...
Shields,		20						.		...	..				2·80		.	25
Scarborough,						52	.	70	1·10	13	..	..		..	1·06			
Liverpool,			04	03	31	·68		1·02	20	·26	15	15	03		·59	09	31	·17
Holyhead,		20	1·20	15	50	·12	·15	·50	12	30	·45	·18	45	03	·35	·45		
Pembroke,				·46	33	15		68	35	·13	23	22	18	.	·34		1·07	13
London,				11	16	33		52	11	07	·18	06			42		·10	
Dover,				·35	36	·35		1·00	·50	·60		·25	.		1·00		·25	50
Portsmouth,				29	19	43		17	10	21		·07	. .		09	.	25	33
Weymouth,				35	20	32	·37	12	07	18			07	02	·08		50	·33
Plymouth,					17	80		50		60			13	.			60	
Penzance,			15	24	27	52		80	27	65		25	25		·60	15	55	20
France.																		
Brest,				·01	·21	·12		2·05	·71	32					75		1·18	·32
L'Orient,				.	·16	35		87	32	20	02	.	08		20		87	04
Rochfort,			04	·16	·20	·35	.	18	20		·12				08		28	·59
Dijon,				14	08	30	39	·10		32	.	.	.		26		08	28
Belgium.																		
Brussels,			·01	·19	·09	27	10	18	·11	32	06	14	.	41	55		06	
Luxemburg,					11	1·16		·36	39	07	01		·11	20		·07	·01	
Netherlands.																		
Groningen,			·10	·17		21	·08	10	26	·22	07	11		21	04	04		
Helder,			·10	·06	22	·02	20	42	30	01	09		05	52		21	05	05
Utrecht,			·01	·26	·11	01		06	16	·09	13		01	42	07	01	03	
Flushing,				11	02	07	04	10	27	·16	06	23	02	14	32		10	..
Denmark.																		
Kiel,			·06	·19		·08	·27	02	·06	·04				02	·15		·13	.
Heligoland (England),	.	..	...	·10		15	..	.	05	25			09		03			·30
Sweden.																		
Haparanda,	15	.	09			70	.			.		·17	.			09		32
Stockholm,					·06	70	·05	02	29	·10	27		·01			·02	·18	
Russia.																		
Kostroma,	.	...	...					02	...		38	·23		34	·23		·09	04
Lougan,	·62	·60	·14				..		·01							·01	.	
Orenbourg,	.				.	.			·44						·06	·20	...	..
Slatoust,		..	...			·01				04	·01					14	·04	
Catherinenbourg,		·10	·03							05						06	.	
Prussia.																		
Putbus,				..	·32				..	·59		·25	·17	...			...	.
Paderborn,				·20	35				07	·28	·02	·01	..	·43		·04	·01	.
Breslau,				24	20				02			04	·52				...	.
Austrian Empire.																		
Cracow,	·01				.	·04	·08	·31	...	.	·02	·45	·01	·03	...	...	...	·62
Prague,					11	·07	·04	..	...			·04		·13	·04	·02	..	...
Kremsmunster,			.		.		·19	. .	·18		·10	·15		1·15	.	...	...	...
Klagenfurt,						.	1·07	·86		.	.	.		.	·60	·34	·20	·02
German States.																		
Munich,		·01	·01		...	·09	·45	·20	·11	·02	·18	...	·13	·16	·21	.	·17	·04
Dresden,				.	·03	27					·11	...	·11	.	·03		..	...
Switzerland.																		
Aarau,				·06	·08	80	·50	·04	17	·79	.			·56	·05	..	...	...
Geneva,		.	·06	·19		·08	·27	·02	·06	·04				·02	·15		·13	...
Spain and Portugal.																		
Lisbon,	·20		.		·05	01	.	·06			.	..		..	.	.	·04	...
Madrid,	·37	·06	.			.	·08					.			.		...	...

	NOVEMBER.								DECEMBER.									
	20.	21.	22.	23.	24.	25.	26.	30.	1.	2.	3.	4.	5.	14.	15.	16.	17.	18.
Ireland.																		
Greencastle,	.			·66	·19	12	12	.	10	50		·18	30		.	19	29	
Galway,	..			62	35	47	10	·07	50	52				13	13	25	27	25
Valencia,	.			52	35	60	15	.		1·27			35		·20	22	·09	
Queenstown,	.			.		30		.	.	42		·22	·16				·07	
Scotland.																		
East Yell,	·10	·20	03				.	05	·35	50		·20	30	56	50	10	07	·18
Sandwick,		07	11	01		03	04			10	32	05	22	26	03	24	11	13
Stornoway,	08	10	93	03	·10		11			43	03	17	97	03	75	06	10	03
Portree,	28	80	1·02	10	20	1·12	·20		80	64	38	38	1·248	32	·18	1·20	28	32
New Pitsligo,	.	05	·41	08					07	45	22					12	11	02
Barry,	.	·12	11	02	02		02		·10	·61	64		10			02		02
Inveresk,	.	·45			06			.	10	36	67		26		15			
South Cairn,		·30	20		·10	·10	10		30	98	35	·30	20			15		
England.																		
Silloth,	.		64		41		05			·30	·29		·10			·05	09	
Shields,			36	·43						32	70	·40						
Scarborough,										60		1·02					17	
Liverpool,	.	·20	·30	23	·02			.	01	08	·30	·05	.			06	12	
Holyhead,			...	50	35		20		·09	30	42		15			07		
Pembroke,	·12		.	·75	20		·30		·15	50	53	·11	10			13		
London,			.	16	·14	09				21	·63		02					
Dover,			.	06	07	09				·50	1·02						07	
Portsmouth,				25	50	19		.		70	60		08			11		
Weymouth,		·18	03	·26	·22	·17			07	69	·87		07				07	
Plymouth,			.	25	.				·21	15	25							
Penzance,	.		.	45			20		15	85	55	23				15	·15	
France.																		
Brest,				·51	·20	04	.	...	08		1·94	16						
L'Orient,	.	. . .		28	·12		.		·04	98	35	12					08	
Rochfort,				·04					.	28		·12					12	
Dijon,				·16	·04		.			19	39	.					·14	
Belgium.																		
Brussels,				...		01				18	55					21	04	
Luxemburg,	.	.	·05	·48	·01	08	·01	.		·56	·96					21	18	
Netherlands.																		
Groningen,								..			...							
Helder,	·02	.		·03	28					11	·70	02			15	03	04	
Utrecht,		·01	01	...	07		·01		·01	03	72	·41	04	.		90	18	
Flushing,		01							04	·22	·12	·02			01		04	04
Denmark.																		
Kiel,				30	·03	.	...			·38	20	·12		·09		·21		
Heligoland (England),	.			08	05	·05	·04	.	.									
Sweden.																		
Haparanda,		23	.				03	.	.				·42			..	39	
Stockholm,		.	·01	01		·002							·16			·11	·06	
Russia.																		
Kostroma,	·06		·02	·08	·12	·06		·02	...	·02								
Lougan,	..	.	.	...	...			.	·01		.		...			.	..	
Orenbourg,	.	...	...	·06	·04	.	.	.	...	...		·14	·04	·16			·18	26
Slatoust,	·02	·57	·51	01	·02	07	03		·05	·11	20						04	
Catherinenbourg,	.			07			·02	.	01		.			01				
Prussia.																		
Putbus,	·01	.	·06		·07		08	03			...	·08	37	·11	03	11		
Paderborn,	.		01	18	·11	06	...	..	.		·43	·08	·01	10	·09	02		
Breslau,	..	..		34	.	·14	·04				35			62				
Austrian Empire.																		
Cracow,	...	...	...	...	·04	·13	·04	..			..	·15		·09	·31			
Prague,	...	.		·11	.	·19	...	...	...	...	·02	.	41	·06	.			
Kremsmunster,	...	.		·15		·16	·06	..						·28				
Klagenfurt,	...	...	...	.		.	·10	.			.						·20	
German States.																		
Munich,	.		...	...	·13	01	04	.	.	·01	..	08		01	..	·03		
Dresden,	...	...	.	62	. .	·51	·08	...			·04	·16	·28	·41	·16			
Switzerland.																		
Aarau,	...	...	...	·23	·02	.	...	.	...	..	·30	·04				...	.	
Geneva,	..	.	...	·30	·03		..	..		·38	·20	·12		.	.	·21		
Spain and Portugal.																		
Lisbon,	..	·04	...	·28		.	.	..		...		...		.				
Madrid,	.		·02	...	.	.	...	·03	·03	...	...	.	.	.	.			

TABLE V.—Showing the Direction and Force of the Wind in the Morning at 126 places in Europe, from 26th October to 12th November, from 20th to 26th November, from 30th November to 5th December, and from 14th to 18th December 1863. Light air to a moderate breeze printed thus, wnw; a fresh breeze to a fresh gale, WSW; a strong gale or hurricane, **NNE**.

	OCTOBER						NOVEMBER																				DECEMBER									
	26.	27.	28.	29.	30.	31.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	20	21.	22.	23.	24.	25.	26.	30.	1.	2.	3.	4.	5.	14.	15.	16.	17.	18
IRELAND																																				
Greencastle,	wsw	w	nw	SE	**WSW**	nw	sw	n	ssw	w	w	nw	e	w	ne	nw	nw	s	SW	SSW	nw	sw	nw	sw	SSW	SSE	**S**	w	...	WSW	WSW	wsw	wsw	N	WNW	wsw
*Belfast,	ne	ne	w	sw	sw	w	w	n	s	w	sw	sw	sw	nw	sw	sw	sw	sw	sw	sw	sw	sw	sw	sw	sw	se	e	w	nw	sw	sw	sw	sw	w	nw	sw
Galway,	ene	ssw	wsw	SSW	sw	**W**	nw	NW	**W**	SW	wsw	ene	SW	sw	nw	sw	NW	sw	SSE	SW	w	SSW	sse	SSE	s	ese	SSW	wsw	**WNW**	e	sw	w	sw	sw	WSW	w
Dublin,	n	wsw	wnw	**SW**	sw	**SW**	wsw	**N**	sw	WSW	calm	calm	w	wnw	calm	wsw	wnw	wnw	s	sse	SW	s	sw	SW	se	se	SW	wnw	W	SW	**SW**	calm	sw	wsw	wsw	wsw
Valencia,	calm	se	nne	**NW**	**WNW**	**WNW**	ssw	**N**	W	wsw	SSW	se	w	N	calm	NW	WSW	ssw	SSW	sw	SW	s	s	SSW	SSW	s	W	NW	**NNW**	SW	W	s	s	N	NW	calm
Queenstown,	se	sw	nw	SW	sw	W	...	nw	W	W	SSW	e	wsw	...	ne	W	wnw	nw	s	w	...	w	sw	SW	sw	SE	SW	wnw	.	SW	SW	sw	sw	NW	NW	sw
Cork,	SE	sw	nw	SW	NW	SW	sw	**NW**	SW	SW	sw	sw	sw	NW	nw	NW	NW	nw	S	SW	w	sw	se	S	s	SE	sw	W	NW	sw	**SW**	sw	s	W	n	n
SCOTLAND.																																				
East Yell,	sw	wsw	sw	wsw	**WSW**	E	WNW	e	wnw	wnw	w	W	N	nne	sw	wsw	wnw	W	sw	sw	ene	w	w	SSE	SSW	SSW	**S**	ssw	sw	WSW	WSW	w	**WSW**	**SW**	n	wnw
Sandwich,	sw	sw	calm	se	sw	NW	nw	se	w	NW	NW	W	n	ne	calm	w	w	nw	S	sw	n	sw	w	SE	SE	SE	SE	calm	calm	W	W	W	sw	**W**	N	W
Stornoway,	SW	sw	sw	SE	W	NW	sw	sw	sw	nw	NW	sw	ne	NE	sw	sw	w	n	SW	SW	ne	sw	sw	S	SW	S	SE	sw	w	SW	SW	W	SW	W	nw	SW
Portree,	se	calm	w	SSE	SW	W	se	calm	sw	w	w	WSW	e	e	**E**	SW	w	n	S	S	W	e	ssw	S	**S**	SE	SSE	ese	nw	SW	SW	wsw	SSW	W	N	W
Dunrobin,	w	W	W	SE	SW	W	W	NW	NW	NW	nw	nw	s	SE	ne	sw	w	sw	SW	s	NW	e	sw	S	sw	s	S	n	nw	sw	W	W	sw	NW	n	W
Elgin,	sw	w	w	SE	**SW**	SW	w	sw	sw	w	SW	w	n	n	sw	SW	w	nw	S	sw	W	SW	sw	SE	SW	SE	**SE**	e	w	sw	sw	W	w	**NW**	**NW**	NW
*New Pitsligo,	w	w	n	s	s	sw	w	w	w	nw	sw	sw	n	w	w	w	w	nw	s	s	w	sw	w	s	s	s	s	calm	nw	sw	sw	w	sw	w	nw	w
Aberdeen,	n	sw	nw	S	SW	w	w	N	w	nw	sw	w	S	e	w	w	w	W	sw	sw	w	var.	nw	S	s	S	S	se	n	sw	SW	sw	sw	w	N	w
Oban,	calm	calm	nnw	SE	**WSW**	W	sw	n	se	wnw	nw	sw	s	e	s	w	wnw	nne	S	SSW	w	ssw	calm	**SSW**	S	se	S	calm	NNW	SW	W	w	wsw	NW	N	w
Barry,	se	s	e	se	S	sw	calm	n	calm	sw	nw	w	calm	n	calm	calm	calm	n	sw	s	s	e	calm	se	s	s	se	nw	nw	w	S	calm	sw	sw	n	nw
Balliston,	w	sw	sw	E	W	SW	ne	ne	sw	W	sw	ne	ne	SE	ne	W	W	n	ne	s	SW	sw	ne	SW	sw	ne	e	e	NE	SW	W	W	sw	W	NW	n
Inveresk,	nw	s	w	se	sw	SW	sw	SE	sw	**SW**	sw	sw	sw	n	w	w	wnw	n	sw	sw	**W**	w	sw	**SW**	SW	sw	ESE	se	SW	**SW**	**SSW**	W	w	SW	n	SW
Thurston,	n	s	nw	**SE**	s	sw	s	se	s	**SW**	s	calm	s	**NE**	n	s	s	n	**SW**	**SE**	**N**	calm	s	sw	sw	n	se	se	**N**	**SW**	**SW**	**W**	sw	sw	**NW**	**S**
Cargen,	sw	w	sw	E	W		sw	NE	nw	W	NW	n	n	ne	ne	sw	nw	n	SW	S	**W**	sw	sw	S	S	nw	SE	w	n	**N**	**SW**	w	w	**NW**	N	NW
South Cairn,	n	w	WSW	S	W	NW	nnw	SSE	s	W	n	se	s	ne	e	W	w	n	S	S	**W**	w	calm	**S**	S	SSE	**S**	s	NW	SW	S	w	WNW	W	N	nw
ENGLAND																																				
Silloth,	e	calm	calm	se	SW	SW	w	NE	w	SW	w	calm	e	ne	n	w	w	n	e	s	**SW**	sw	sw	SW	sw	e	SE	e	NW	W	**SW**	w	w	e	W	n
Shields,	w	w	wnw	wnw	SSE	ne	ne	SE	NW	..	wsw	nw	wnw	n	ne	wsw	wnw	ne	wsw	sw	sw	w	sw	s	sw	sw	S	sse	SSW	nw	WSW	nw	sw	NW	**NE**	nw
Scarborough,	wnw	ssw	ssw	se	ssw	SW	w	S	wnw	NE	w	sse	sw	E	ne	sw	wsw	n	ssw	W	W	wsw	s	ssw	SSW	se	S	S	**SSE**	w	SW	wnw	w	W	**NNE**	wsw
Leeds,	nw	se	SW	SE	SW	SW	SW	ne	SW	NW	SW	SW	ne	NW	NE	NW	sw	NW	se	SE	SW	SW	SW	SE	SE	SE	SE	SE	SW	SW	SW	NW	SW	NW	SW	SW
Liverpool,	sse	se	s	se	sse	SW	SSW	n	s	WSW	ssw	se	wsw	**W**	s	W	se	n	se	SE	SE	sse	sse	ese	se	sse	ESE	ese	**W**	wsw	SSW	w	wsw	wsw	**NW**	ssw
Holyhead,	calm	n	W	SE	SW	SW	WSW	SE	SW	SW	w	ese	sw	NW	nne	W	NNW	NW	SSE	SSE	WSW	sw	sw	SSE	SSE	ese	SSE	ESE	**WNW**	WSW	SSW	wsw	sw	wsw	**NNW**	w
*Nottingham,	se	se	sw	w	sw	sw	sw	s	nw	sw	w	w	nnw	nw	n	w	nw	nnw	sw	sw	sw	sw	sw	ssw	s	ese	ese	ese	wnw	nw	sw	wsw	sw	n	wnw	nw
Yarmouth,	SE	nne	SE	S	SSW	**WSW**	WSW	S	WNW	W	NW	N	NW	ene	ENE	SSW	W	W	SSW	S	**WNW**	SW	S	s	S	SSE	SSE	SE	**SSW**	WNW	SW	NW	wsw	WSW	**NNW**	nne
Banbury,	se	n	s	s	S	**SW**	S	S	sw	W	NE	n	s	w	N	S	s	nw	s	S	S	S	s	S	S	se	s	se	**W**	W	SW	NW	w	NW	SW	n
Pembroke,	nne	sw	WNW	**SSW**	**SW**	**W**	w	NW	WSW	W	W	e	nw	N	nne	NW	nne	nsw	S	SW	WSW	sw	ssw	S	se	se	sse	**NW**	**NW**	WNW	**SSW**	wnw	sw	NW	SW	calm
Clifton,	ne	e	wsw	s	s	wsw	sw	w	s	wsw	wsw	e	wsw	w	n	wsw	ne	nnw	se	s	wsw	s	e	s	se	e	se	wsw	WNW	wnw	sw	wnw	wnw	nw	nw	nnw
London,	ene	calm	wsw	ssw	**SSW**	**WSW**	W	NNW	wnw	W	W	n	nw	sw	NE	sw	se	snw	ssw	S	SW	wsw	ssw	sw	s	ne	ese	S	**W**	W	WSW	w	w	WSW	NW	ne
Dover,	n	se	sw	SW	**SW**	**W**	...	**WSW**	**W**	wsw	w	nw	nw	...	**NE**	w	sse	n	...		W	w	ssw	ssw	sw	se	S	**S**	**W**	...	W	w	w	W	**NNW**	n
Portsmouth,	ne	ne	wsw	SW	**SW**	**WSW**	W	W	wsw	wsw	w	w	nw	W	NNE	s	S	N	sw	S	W	w	sw	sw	se	se	ssw	SW	**WNW**	WNW	SW	nw	n	WSW	NNW	nne
Weymouth,	ne	ne	wsw	SSW	SW	W	W	wsw	wsw	wsw	w	w	calm	**WNW**	nse	N	SE	NW	...	SSE	W	w	sw	sw	ESE	ESE	ese	W	**NW**	wnw	n	nsw	calm	ssw	NW	nsw
Plymouth,	**WNW**	...	sw	sw	**WSW**	**W**		W	**WSW**	W	w	E	w	W	N	WSW	N	**NNW**	ESE	S	W	SW	calm	ESE	ESE	NE	ese	SW	NW	n	**SSW**	calm	calm	ssw	NW	nsw
Penzance,	calm	calm	sw	SW	SSW	W	W	NW	WSW	W	w	calm	sw	W	NE	nsw	N	NNE	S	S	W	nsw	calm	SSW	s	SE	sw	**NW**	NW	wsw	SW	calm	calm	NW	N	calm
NORWAY AND SWEDEN																																				
Haparanda,	s	w	sw	S	S	se	S	calm	calm	calm	ne	calm	n	n	NW	w	s	S	calm	ne	n	n	n	calm	s	sw	sw	w	S	S	S	w	calm	w	s	n
Hernosand,	sw	calm	sw	S	S	ese	sw	wsw	n	calm	nw	w	nw	n	W	ssw	sw	calm	calm	W	r	calm	calm	ene	s	calm	nw	wsw	S	S	SW	calm	calm	w	sw	nw
Christiansund,	**WNW**	calm	S	SW	S	s	**SW**	w	s	calm	...	**WNW**	**NNE**	ne	wnw	se	se	nw	SW	**WSW**	calm	calm	calm	calm	se	calm	calm	S	SSE	SSE	SW	SW	calm	calm	**N**	s
Skudesnes,	S	S	S	SW	S	SW	W	SE	SE	SE	NW	**N**	**NW**	ne	nw	**SW**	SE	sw	calm	S	SE	calm	se	calm	s	calm	S	**SE**	sw	N	S	SW	calm	calm	SW	se
Mandal,	ssw	ssw	se	se	SW	**WSW**	SW	ese	ese	ENE	wnw	wsw	**WNW**	ne	ene	**SW**	calm	ene	sw	ssw	**SSW**	calm	se	ne	calm	...	E	se	se	se	S	calm	calm	SW	nw	se
Christiania,	calm	sw	se	se	S	snw	calm	sw	NE	se	ssw	ssw	calm	nne	nne	sw	ssw	nne	calm	calm	calm	nne	nsw	ene	s	wsw	ese	ese	NE	S	S	calm	calm	nw	nw	calm
Stockholm,	wsw	wsw	sse	sse	S	**S**	**SSW**	sw	ese	sw	W	wsw	**WNW**	nsw	wnw	ssw	ssw	nne	wsw	wsw	wsw	sw	n	nw	sse	W	**WSW**	s	SSE	SSE	SSE	calm	nsw	S	esw	nw
RUSSIA																																				
Archangel,	S	S	se	s	S	SE	SE	SE	W	E	s	wsw	NW	N	wnw	S	e	S	W	ne	sw	S	NE	S	W	S	SW	w	N	E	SE	e	e	ese	e	SE
Helsingfors,	calm	s	s	SE	sse	**SSE**	SE	SE	calm	se	wsw	wsw	nw	nne	wnw	W	SW	ssw	w	...	se	S	NW	calm	...	...	w	wsw	...	E	S	wnw	sw	se	ssw	W
St Petersburg,	s	w	s	S	SE	S	S	S	se	S	S	NW	e	.	n	W	SW	S	w	nw	ne	e	wnw	e	S	NW	w	nw	nw	**S**	S	w	sw	ssw	S	SW
Dorpat,	ne	SW	**SW**	s	W	s	**S**	sw	S	**SW**	sw	sw	nw	sw	NW	SW	se	s	sw	sw	sw	SW	nw	w	E	SW	W	sw	se	**SE**	S	nw	s	ssw	s	w
Riga,	w	se	se	SE	SE	SE	SE	S	se	N	SW	s	SW	sw	w	s	SE	se	sw	sw	s	se	s	sw	E	...	sw	s	se	**SE**	SE	NW	SW	s	s	sw
Libau,	e	se	ese	SSE	SSE	S	se	WSW	SE	wsw	WSW	WNW	NW	W	e	sse	SE	sse	W	W	ssw	sw	sw	ese	ESE	wnw	s	s	ese	SSE	ese	ne	ene	ese	S	
Warsaw,	**N**	n	s	se	SE	**SE**	sw	sw	s	sw	**SW**	W	**W**	W	w	w	S	se	sw	sw	SW	S	W	W	N	ne	se	...	s	W	SE	NW	SE	n	N	s
Moscow,	calm	se	calm	calm	calm	E	s	SSW	s	..	s	...	se	...	NW	N	sw	s	W	w	w	s	s	n	N	...	w	W	s	s	W	n	nw	N	N	S
*Kostroma,	nw	calm	w	nw	se	s	s	s	s	se	s	s	sse	sw	nw	nw	se	s	W	wnw	W	se	nw	sw	nne	nw	wsw	W	w	se	s	w	n	n	n	ne
Catherinenbourg,	calm	calm	nw	calm	n	w	w	calm	calm	w	sw	**SW**	SW	calm	sw	calm	w	calm	calm	calm	se	nw	calm	ne	se	calm	W	**SW**	w	se	nw	se	se	SE	[illegible]	ne
Slatoust,	se	calm	s	calm	calm	nw	calm	calm	se	NW	sw	nw	calm	se	calm	nw	calm	nw	sw	NW	calm	NW	**SE**	**NW**	nw	nw	**NW**	w	sw	sw	se	SE	se	SE	**SE**	SE
Orenbourg,	**N**	NNE	S	NNE	ene	**ESE**	ENE	E	**ESE**	N	SSE	S	s	S	WSW	S	S	N	W	S	WSW	wsw	wsw	W	WSW	**WNW**	...	...	...	...	...	...	...	...	...	...
Kiev,	NE	se	sw	se	ne	ne	S	s	s	s	sw	sw	sw	sw	w	w	sw	SW	nw	w	ne	SW	sw	sw	nw	...	n	n	calm	calm	se	nw	nw	nw	w	s
Lougan,	NE	NE	ne	calm	calm	calm	ne	s	calm	calm	calm	calm	S	calm	sw	calm	calm	calm	calm	calm	calm	SW	sw	calm	W	W	n	calm	calm	e	E	NE	NW	w	w	w
Nicolaiev,	**NE**	**NNW**	N	NE	calm	ne	SE	se	SSE	s	w	calm	NW	calm	calm	calm	w	SE	s	w	sw	W	sw	n	N	N	NE	nw	N	NE	SE	NE	**NW**	N	calm	calm
Odessa,	**NE**	**NE**	**NE**	NE	NE	E	**SW**	**SW**	NE	S	calm	calm	W	nw	w	NW	NE	S	calm	nw	calm	calm	nw	NW	calm	calm	NE	nw	NE	n	NE	NE	**N**	N	calm	calm
Tiflis,	nw	calm	sw	calm	calm	nw	calm	se	calm	calm	NW	sw	calm	calm	nw	NW	se	calm	calm	calm	calm	calm	calm	**NW**	**NW**	calm	calm	n	calm	calm	se	calm	**NW**	sw	calm	nw
DENMARK																																				
Ivigtut (Greenland),	nw	n	N	calm	ne	calm	calm	calm	calm	calm	calm	E	calm	**SE**	**NW**	n	calm	n	**E**	**E**	calm	calm	calm	NW	NE	se	NE	NE	se	NE	.	calm	calm	**E**	e	calm
Skagen,	ssw	sw	S	s	W	**W**	w	WSW	s	S	NW	NW	nw	ne	n	W	ssw	w	w	sw	SW	w	w	sw	s	se	SSE	S	s	e	SW	n	e	sw	n	n
Copenhagen,	se	ese	SE	s	esw	SW	SW	ssw	s	ssw	w	nw	wsw	calm	ne	sw	sw	wsw	wsw	sw	sw	SSW	w	sw	ssw	ese	s	s	ese	sw	wnw	ne	nw	sw	nw	NE
*Kiel,	ne	e	s	sw	sw	sw	sw	sw	se	s	w	w	nw	w	ne	sw	sw	sw	sw	W	sw	w	nw	sw	s	s	s	s	w	sw	sw	w	w	sw	sw	nw
Heligoland (England),	SE	se	SE	sw	wsw	**WSW**	...	.	SSE	wsw	**WNW**	**NNW**	**NNW**	.	E	S	SW	w	...	..	...	**W**	WSW	wsw	ssw	...	SE	S	.	..	.		...	.		
PRUSSIA.																																				
*Konigsberg,	ne	sw	e	se	se	se	se	se	se	sw	w	sw	nw	nw	n	w	se	w	w	w	w	sw	sw	w	ne	s	s	se	se	se	se	nw	sw	se	sw	w
Putbus,	sw	sw	SW	sw	S	**W**	**SW**	SW	SE	S	**NW**	**NW**	**NW**	nw	n	s	sw	w	w	s	sw	W	sw	s	n	w	SE	se	s	**SW**	w	n	**N**	w	w	N
Paderborn,	se	s	sse	sw	**SW**	sw	sw	se	SW	SW	W	w	nw	sw	nne	e	se	se	se	sw	s	s	s	sw	sw	se	se	se	ESE	s	**N**	sw	n	w	sw	n
Breslau,	se	SE	se	SE	S	SE	sw	se	w	SE	SW	w	**W**	W	e	ese	ese	nw	se	se	se	S	w	ese	nw	se	SE	se	s	s	SW	W	**W**	s	s	s
AUSTRIAN EMPIRE																																				
Cracow,	n	ne	ne	e	ne	ne	w	w	e	w	w	**W**	SW	sw	nw	ne	e	sw	w	w	w	w	w	sw	w	ne	e	n	w	n	nw	nw	nw	w	e	nw
Vienna,	n	calm	ese	wsw	calm	calm	calm	calm	wsw	calm	nw	**W**	calm	calm	w	calm	se	calm	calm	ese	s	sse	calm	calm	..	calm	sw	se	se	calm	WNW	WNW	wnw	nw	calm	calm
Kremsmunster,	ne	ne	ne	ne	ne	w	ne	ne	w	w	**W**	**W**	w	W	ne	**E**	e	w	w	w	w	w	e	e	e	e	**E**	**E**	ne	w	w	**W**	W	w	w	w
Klagenfurt,	SW	nw	w	sw	w	sw	**SW**	**E**	se	nw	w	w	w	se	**E**	e	se	w	s	s	sw	w	w	se	w	ne	e	**E**	sw	w	s	w	s	nw	w	sw
GERMAN STATES																																				
Leipzig	se	se	sse	SSE	S	SSW	SSE	S	S	SSW	WSW	wsw	SW	ssw	ene	ene	s	sw	sse	ssw	SSW	s	sw	s	...	ENE	sw	SE	SSW	WSW	**SW**	sw	wnw	sw	ssw	NW
*Bamberg,	.				..									.											...		se	se	sse	w	s	nw	nw	se	s	nw
Munich,				..	.				.	..	.	.		...		...									...		e	e	w	**W**	sw	**W**	W	s	s	W
NETHERLANDS																																				
Groningen,	e	ese	e	s	s	S	S	S	s	sw	esw	sw	w	sw	ne	se	s	s	s	s	s	ssw	s	wsw	s	e	e	se	s	**W**	sw	w	sw	s	s	nw
Leeuwarden,	ese	ese	se	sse	s	SW	**SSW**	**SE**	W	SW	w	nw	sw	ssw	ne	se	sw	sw	ese	sse	esw	esw	esw	ssw	s	e	ese	SE	ese	**WNW**	ssw	w	sw	ssw	SW	n
Helder,	ese	nne	ssw	s	SW	**WSW**	SW	ese	**WSW**	WSW	wsw	W	WNW	e	ene	s	wsw	s	s	s	sw	wsw	ssw	ssw	s	ese	ese	sse	s	nw	sw	nw	w	nw	W	w
*Amsterdam,	sse	sse	s	sw	ssw	sw	se	ese	s	ssw	wsw	n	s	sse	nne	sse	esw	sw	e	s	s	s	s	s	s	ese	ese	e	se	sw	se	w	s	se	nsw	sw
Utrecht,	se	se	esw	sw	ssw	SW	sw	SE	**NW**	SW	w	nne	sw	ssw	NE	se	sw	sw	ssw	s	sw	sw	ssw	esw	[illegible]	e	e	SE	S	NW	SW	w	w	SW	SW	N
Hellevoetsluis,	se	e	s	s	ssw	**SW**	SW	SE	**NW**	SW	sw	nnw	nnw	se	nne	ese	ssw	n	s	s	ssw	ssw	s	esw	se	e	ese	SE	SSE	NW	SSW	w	esw	SSW	NW	n
Breda,	e	se	sw	sw	sw	sw	wsw	ssw	w	w	wnw	nw	n	sw	ne	se	sw	ssw	esw	sw	wsw	wsw	sw	sw	ese	e	ese	ese	sw	wnw	sw	wnw	wnw	w	w	nne
Flushing,	se	se	SW	SW	SW	sw	SW	**S**	**NW**	W	ne	ne	w	s	E	wsw	sw	e	sw	S	sw	sw	sw	sw	se	se	sse	SSE	**SSW**	ssw	SW	wnw	w	SW	NW	NE
BELGIUM																																				
Brussels,	nne	s	ssw	S	SSW	SW	SSE	SSE	SSW	WSW	esw	wnw	SSW	s	NNE	se	se	n	s	sse	ssw	sse	se	ssw	se	ne	se	SE	SE	SW	SSW	ssw	ssw	esw	sw	n
Maastricht,	s	s	wsw	s	sw	sw	sw	S	SW	sw	sw	sw	e	n	N	e	sw	n	s	esw	sw	s	esw	sw	ese	n	s	s	SW	WNW	sw	sw	sw	esw	sw	wnw
Luxemburg,	e	e	e	sw	sw	esw	sw	sse	ssw	sw	sw	sw	n	sw	ssw	nw	nw	ssw	ne	ne	se	nnw	nsw	w	w	sw	sw	sw	esw	sw	w	w	w	sw	sw	nw
FRANCE																																				
Dunkirk,	ese	calm	nw	sw	sw	WSW	SW	S	NW	W	WNW	NE	w	wnw	**ENE**	s	s	nne	s	s	sw	sw	s	s	SE	se	ese	SW	SW	**NNW**	sw	w	wnw	sw	NNW	NE
Mezières,	e	ese	e	se	w	W	sw	S	nw	wnw	wsw	n	n	nw	ne	e	w	nne	e	e	s	e	e	se	e	e	se	ese	S	nw	esw	...	nsw	w	nnw	ne
Havre,	se	se	SW	SW	WSW	W	**WSW**	**WSW**	NW	NNW	W	NE	sw	nw	**NE**	wsw	SSW	NNE	e	se	WSW	w	sw	ese	ESE	e	ESE	WSW	WSW	**NNW**	WSW	nsw	se	sw	**NNW**	ENE
Cherbourg,	sse	nnw	s	wsw	SSW	WSW	WSW	WSW	WSW	WSW	SW	e	s	sw	NNW	sw	S	N	SSE	s	wsw	ssw	s	s	se	se	s	SW	WSW	N	SW	wsw	ssw	sw	NW	NNE
Paris,	ese	ene	ssw	s	ssw	sw	ssw	**SW**	wsw	wsw	wsw	wsw	calm	wsw	NNW	wnw	esw	nnw	se	se	sw	w	s	ese	ese	ese	se	S	SSW	w	s	calm	calm	s	w	nnw
Strasbourg,	ne	n	n	s	sw	sw	s	sw	w	SW	esw	sw	sw	n	s	n	s	n	n	se	..	s	sw	s	s	n	n	n	esw	SW	n		esw	n	wsw	n
Brest,	ne	n	esw	sw	SSW	w	wsw	**WSW**	w	SW	WSW	w	sw	nnw	n	w	NW	NNW	SE	sw	wnw	sw	ssw	se	ese	se	w	WNW	WNW	w	sw	e	ese	W	nnw	nne
L'Orient,	ne	ne	ese	esw	SW	W	W	WSW	**WSW**	wsw	W	wsw	nw	w	n	e	**W**	nnw	ese	s	wsw	se	s	ese	e	ene	S	w	SW	nw	SW	ne	ene	ese	wnw	n
Napoleon Vendée,	ne	e	e	s	wsw	wnw	w	W	nsw	...	nw	nw	nnw	N	n	w	w	...	..	se	w	w	e	ese	se	e	ne	n	nnw	nnw	n	ne	...	n	n	nne
Besançon,	n	ne	s	sw	sw	w	w	sw	WSW	sw	ese	sw	w	sw	w	sw	se	e	n	ne	wsw	sw	ne	e	n	se	ne	sw	sw	nw	nw	sw	w	w	nw	n
Lyons,	e	s	s	s	s	...	ne	sw	sw	sw	sw	nw	nw	s	w	sw	se	sw	se	ne	s	s	s	s	se	se	se	s	w	...	nw	n	w	n	s	n
Limoges,	e	se	s	w	s	nw	ne	sw	sw	sw	sw	nw	nw	s	w	nw	ne	n	sw	sw	s	w	sw	nw	sw	s	s	sw	sw	nw	n	w	w	w	ne	NE
Rochefort,	n	ne	se	s	sw	nw	sw	sw	nw	sw	ne	s	sw	w	sw	ne	NW	N	s	se	ssw	wsw	s	W	se	SE	s	W	**WSW**	nw	se	ne	NE	NW	NW	N
Bordeaux,	..	..	...	...	...	...	W	**WNW**	W	W	W	w	...	NW	nw	NW	...	...	se	se	SW	sw	se	S	s	E	SE	S	s	N	se	e	e	n	N	N
Montauban,	e	e	e	s	s	s	w	s	sw	sw	sw	w	w	w	sw	sw	sw	sw	se	se	se	s	se	s	se	se	sw	sw	e	sw	sw	se	sw	sw	sw	sw
Bayonne,	se	s	e	e	...	n	wsw	sw	nw	w	e	e	e	s	WNW	e	NW	N	se	s	se	se	s	se	se	se	s	...	e	e	e	se	e	e	e	e
Montpellier,	n	ne	ne	n	n	nne	ne	n	NW	NW	nw	n	sw	nw	nw	e	W	nw	nnw	nnw	nne	n	nnw	nne	n	nne	nne	nw	n	nw	wnw	ne	n	nw	nw	n
Marseilles,	e	e	ese	SE	ESE	e	ne	n	NW	WNW	n	n	nw	nw	nw	n	NW	ne	ne	ne	..	e	w	ne	ne	ne	ne	SE	nsw	...	n	ne	ne	e	ne	n
Toulon,	n	e	E	ENE	ene	sw	e	wnw	NW	NW	ene	nne	...	w	sw	ene	NW	sw	ene	ene	...	e	nne	ne	ene	ne	nse	ne	...	NW	sw	ene	ene	ene	nw	e
SPAIN AND PORTUGAL																																				
Bilbao,	se	se	se	e	se	nw	nnw	nw	...	se	se	se	se	se	nw	nw	..	NW	se	...	sw	se	se	se	se	SE	n	wsw	..	..	se	se	se	se	nw	nnw
Barcelona,	ne	n	N	n	SW	NW	NE	W	W	NW	w	n	w	n	NW	W	NW	**NW**	n	w	n	n	s	N	n	N	w	W	w	W	W	n	n	w	NW	W
Oporto,	n	sse	n	nnw	...	s	nnw	nw	ne	ne	e	e	e	e	n	N	...	N	e	s	s	se	ese	...	se	E	...	sw	..	...	e	e	nse	n	nw	nne
Lisbon,	e	ene	...	n	se	ssw	NNE	w	ne	NE	NE	NE	ne	ne	NNE	NE	...	N	E	ene	nne	ne	ene	...	ese	e	NE	sw	e	..	NNE	calm	NNE	n	NE	NNE
Madrid,	e	se	sse	nnw	s	sw	se	sw	ne	e	e	e	e	nne	ene	N	s	nw	e	e	...	e	n	n	e	...	SE	w	W	N	ene	ne	ne	e	ne	N
Alicante,	ne	calm	calm	calm	calm	s	s	nne	...	calm	calm	calm	...	ne	sw	sw	...	nne	se	se	se	se	ene	...	n	se	ese	ene	NW	n	ene	sw	nw	nw	wnw	nw
San Fernando,	sse	SSE	ssw	ene	se	ne	n	nne	ne	ese	ese	sse	ene	se	e	nne	ne	wnw	ese	SE	ESE	ese	se	ese	ESE	se	ese	sw	nsw	n	nsw	se	se	se	ese	nne
Palma (Majorca),	NNW	NE	...	calm	calm	ese	s	sw	N	e	nnw	nw	s	wsw	NW	ENE	nnw	NNW	n	n	n	n	n	n	...	ene	n	e	n	NNE	NNE	nnw	n	nnw	ne	ne
AFRICA.																																				
Algiers,	e	w	ese	e	w	e	...	w	w	e	sw	n	se	w	sw	w	nw	..	e	sw	ne	e	..	...	...	W	E	SW	SW	nw	N	w	w	n	w	N
ITALY.																																				
Turin,	...	...	NW	w	e	ne	n	nw	n	nw	nw	e	w	...	...	w	s	sw	E	e	W	w	W	W	W	w	w	n	nw	s	w	w	w	nw	s	nw
Leghorn,	E	E	ene	ene	SSE	s	s	wsw	WSW	ene	ne	ne	ne	ne	WSW	E	e	E	ene	ene	ne	ene	ne	ne	...	..	NE	ene	ene	e	NE	ne	ne	ne	S	ne
Florence,	ne	se	e	...	se	s	...	...	s	ne	...	...	s	...	nw	se	...	se	ne	s	sw	...	...	sw	se	NE	NE	se	ne	e	NE	se	se	e	S	e
Ancona,	...	...	nw	se	w	sw	ssw	ese	nw	N	W	w	wnw	...	ese	se	e	sse	w	e	ssw	ssw	ne	nw	..	w	W	w	NE	WNW	ENE	se	esw	nsw	nsw	ese
Rome,	n	..	n	e	e	s	se	s	sw	n	n	n	n	n	se	sse	s	se	n	n	nw	s	n	...	n	n	...	n	...	...	...	n	n	...	n	e
Naples,	ne	ne	e	w	sw	sw	sw	SW	ssw	e	NE	n	ne	n	SW	se	S	n	ne	n	...	nne	nne	e	ne	NE	n	ene	ene	SW	E	n	w	nne	nnw	wsw
Palermo (Sicily),	wsw	wsw	wsw	wsw	SW	sw	wsw	sw	wsw	nw	w	wsw	wsw	sw	wnw	nsw	w	sw	nne	wsw	wsw	wnw	wsw	wsw	wsw	wsw	wsw	wsw	...	sw	..	wsw	sw	wsw	wsw	wsw
TURKEY.																																				
Constantinople,	N	N	n	w	n	...	s	s	s	s	...	..	n	...	s	n	...	...	...	ne	...	...	calm	n	...	N	N	n	...	...	n	...	N	...	s	s

XVI.—*Examination of the Storms of Wind which occurred in Europe during October, November, and December* 1863. By ALEXANDER BUCHAN, M.A., Secretary to the Scottish Meteorological Society. (Plates XIII. to XXI.)

(Read 3d April 1865.)

A brief account of the weather of this period as regards temperature was read before the Royal Society last year. It was drawn up at the request of Professor BALFOUR, to accompany his paper "On the Remarkable State of Vegetation in the Edinburgh Botanic Garden in December 1863."

From the 26th of October to the end of December the weather was in every way remarkable. Though frost occurred in the end of October and beginning of November it was not severe, and the temperature continued on the whole seasonable till the 12th of November. From this date till the end of the month it ranged unprecedently high, being 9° above the average temperature of the season. It then fell for the next ten days, but on no occasion below the average; and again rose considerably above the average during the week ending with the 18th of December. Under this genial weather vegetation in the open air advanced rapidly to a state of forwardness not usually seen till the month of March. In December 245 plants were in flower in the Gardens in the open air, and of these 35 were spring flowers. The frost which had occurred was insufficient to damage, to any material extent, 210 autumn-flowering plants; and the high temperature of November, which was as high as what ordinarily occurs in the beginning of May, brought the spring flowers prematurely into bloom, so that there was to be seen the rare spectacle of sweet peas and hepaticas flowering together.

Thus, then, the atmosphere during this time was in a most abnormal condition in respect of temperature, which, of all the elements concerned, plays the most conspicuous part in destroying its equilibrium. It is not surprising, therefore, that the weather was equally remarkable, or even more so, for storms. From the 27th October to the 18th December, eleven well-marked storms passed over Europe in succession.

Since the space embraced by storms frequently includes the greater part of Europe, it is only recently, owing to the extension and growing popularity of meteorology, and the countenance now happily given to it by most European governments, that sufficient data could be obtained for a satisfactory treatment of the subject. For the observations of the principal observatories of Europe are too few in number, and at too great distances apart, to enable any one to lay down the isobarometric lines and general course of the winds, without drawing largely on conjecture and imagination. The recent multiplication of meteorological observatories is a great step in advance toward the discovery of the law of storms.

Observations have been received from 135 places scattered over Europe, from the Mediterranean to Archangel in the north of Russia, and from the extreme west to the Ural Mountains. All parts of Europe are pretty well represented except Central Russia, the south-east of Austria, and Turkey. The following are the sources from which the observations have been obtained:—The places in Scotland have been selected from the stations of the Scottish Meteorological Society; and most of the places in England and Ireland from Admiral FITZROY's Tables, published daily in "The Times,"—the omitted observations on Sundays having been, to some extent, supplied by the observers themselves. Most of the continental stations have been taken from the lists given in LE VERRIER's bulletins of the weather, published daily in Paris, and from the "Meteorologische Jaarboek" of Dr BUYS BALLOT of Utrecht. I am further indebted to Dr BALLOT for his valued assistance in supplying me with additional observations to those printed in the Jaarboek. The observations from Russia were kindly furnished by M. FERDINAND MÜLLER, assistant in the Physical Central Observatory of Russia; those from Sweden, by M. BONNIER, Stockholm; those from Norway, by M. C. FIARNLEY, director of the Observatory, Christiania; and those from Denmark and Greenland, by Professor HOLTEN, Copenhagen. Rev. FRANCIS REDFORD supplied the observations from Silloth; Mr E. J. LOWE, those from Nottingham; Mr W. C. BURDER, those from Clifton; Mr HENRY DENNY, those from Leeds; Mr WILLIAM JOHNSTON, those from Banbury; Captain WILLIAMSON, those from Dublin; and Mr A. DICKEY, Queen's College, those from Belfast. The importance of observations from Iceland and Faroe was not overlooked, but we regret to say that no observations were made in those places during the period. I beg also to return my most grateful thanks to the MARQUIS OF TWEEDDALE and BARON BRUNOW for the interest they took in this inquiry in procuring some of the most valuable of the observations, especially those from the north of Europe.

Construction of the Tables and Maps.

The observations at the different places were made at 8 A.M. At the few places where they were made at a different hour, such as $9\frac{1}{2}$ A.M. at Dublin, a slight correction was adopted to bring them into accordance with the others. The amount of this correction was deduced from the preceding and succeeding observations at the place, modified by the apparent course and rate of motion of the storm, as suggested by the observations of neighbouring stations. It not being necessary for this inquiry to descend to the thousandth of an inch of barometric pressure, or parts of a degree of temperature, the tables and maps may be accepted as representing the pressure and temperature of the air over Europe, with scarcely any deviation from the truth

The *barometric* observations (Table I.) were brought to English inches, and then reduced to 32° and sea-level. Each observation, so reduced, was entered in its place on the map, and lines were then drawn through all those places where the pressure was equal. These isobarometric lines are given for every two-tenths in the difference of the pressure,—for 30·5, 30·3, 30·1, 29·9, &c., inches.

The lines of *temperature* have been laid down on a different principle. For lines exhibiting the actual temperature would fail to show, in a sufficiently clear manner, the real bearing of this important element, since the isothermals of October, November, and December run in a very irregular manner over the continent of Europe. Hence not the actual temperature, but the difference between the actual temperature and the mean temperature of each day at the several stations is traced on the maps.

Dr BUYS BALLOT has calculated the mean temperature of many places in Europe for every alternate day of the year, and for a few other places ten-day means. The results were published in 1861, in "*La Marche Annuelle du Thermomètre et du Baromètre en Neerlande et en Divers Lieux de l'Europe.*" In the observations of temperature in the "Jaarboek," 1863, not the actual temperature, but the deviations from the mean temperature of each day, are alone given. These I have adopted *simpliciter*. Of the other stations, I have calculated the mean temperature of each day, using for this purpose Dr BALLOT's tables, Professor DOVE's mean temperatures, as given in "*Darstellung der Wärmeerscheinungen durch Fünftägige Mittel,*" 1863; the same author's "*Monats-und-Jahresisothermen,*" 1864; and the data in the Scottish Meteorological Society's "Proceedings" bearing on the subject. The differences between these daily means and the daily observed temperatures are entered in Table III., in which the minus sign shows that the temperature was under the mean; and if no sign is used it was above the mean. The stations are pretty well distributed over Europe, and are sufficiently numerous to show the changes of temperature which occurred near the earth's surface before, during, and after the successive storms. They were entered on the maps, and then, as in the case of the barometer, lines were drawn through those places where the differences were equal. They show where the temperature was the average 0°, and then in succession where it was 4°, 8°, 12°, &c., above the average or below it.

The temperatures as actually observed are given in Table II.

The state of the sky with respect to *rain*, *cloud*, and *fog* is indicated in Table II. by means of letters—R showing that it was raining at the time of observation; C, that at least three-fourths of the sky was covered with clouds; B, that the sky was either quite clear, or not so much as three-fourths covered; and F, that fog prevailed. When r is attached to any of the above letters, rain fell at that station sometime during the previous twenty-four hours, but was not falling at the time of observation. The state of the sky at several places was not known. These places are marked with an asterisk, and R in such cases means that it rained sometime during the previous twenty-four hours; but whether it was raining at the time of observation or not cannot be learned from the returns.

The direction of the wind (Table V.) is indicated on the maps by arrows represented flying with the wind. The force of the wind is shown (1.) by plain arrows →, which represent light air to a moderate breeze; (2.) by arrows feathered on one side only ↣, which represent a fresh breeze to a fresh gale; and (3.) by arrows feathered on both sides ⤜, which represent a strong gale, storm, tempest, or hurricane. A calm is shown by ⊙. In the tables the force of the wind is shown by the different types employed, as there explained.

The observations comprehend four periods, viz., (1.) from the 26th October to the 12th November; (2.) from the 20th to the 26th November; (3.) from the 30th November to the 5th December; and (4.) from the 14th to the 18th December —in all thirty six days. Maps were constructed, as described above, for each of these days. A selection from these accompanies this paper Plates XIII. to XVIII. give the barometric pressure, the temperature, the state of the sky, and the winds, as observed on the mornings of the 30th and 31st October, and the 1st, 2d, 10th, and 11th November. Plates XIX. to XXI. give only the observations of the barometer and winds on the mornings of the 28th and 29th October, the 3d, 4th, and 12th November, and the 1st, 2d, 3d, 4th, 5th, 16th, and 17th December.

Observations of the Barometer.

The observations of the barometer are the most important of all the observations, since it is within the area where the barometer falls to some extent below the average that storms occur. Speaking roughly, the mean atmospheric pressure for these months is 29·9 inches. Therefore, the space comprehended within the isobarometric line 29·7, and the other lines showing a less pressure, may be called, for convenience' sake, *the area of low barometer*. Hence, while we trace the progress of these low pressures over Europe from day to day, we trace at the same time the progress of the storms.

A brief account of these lines is desirable, to give some idea of the extent and course of the storms. An area of low barometer occupied the greater portion of the northern half of Europe, from the 28th October to the 9th November, during which time its eastern limit advanced slowly and steadily eastwards from Norway to the Ural Mountains; while its southern limit, having first oscillated backwards and forwards over the space lying between Spain and Ireland, ultimately moved northward, and left Europe by the North Cape. During this time four storms passed across this disturbed area, which was generally about 1900 miles in length by 1400 in breadth.

Storm I. on the 28th October (Plate XIX.), embraced the British Islands and the west of Norway, having its centre at Elgin, where the pressure was 29·41. On the 29th (Plate XIX.), the area of low barometer now included the north-west of France, the north of Germany, and the whole of Denmark and Scandinavia. The centre of the storm was near Christiansund, in Norway, and on the following day had passed out of the map by the North Cape.

Storm II. had advanced so far by the morning of the 29th October (Plate XIX.) that its centre had all but approached the west of Ireland, where the pressure was 28·56, being nearly an inch lower than the depression which accompanied the previous storm. On the 30th (Plate XIII.), it had arrived at Shetland, the lowest pressure being 29·44. At that moment the whole atmospheric system of Europe, to use a familiar illustration, appeared to be swinging round Warsaw as a centre, in the direction of the motion of the hands of a watch, so that in the south and south-west barometers were everywhere rising, whilst in the north and north-east they were falling. On the 31st (Plate XIV.), it had advanced eastward to Christiania. At the same time the isobarometric 28·9 had greatly extended its area, and a new depression (II. *b*) had been formed in its western part, due west of the former, and contiguous to it. On the 1st November (Plate XV.), the isobarometric 28·9 had contracted to one-half of its former dimensions, the two depressions in the centre had united and advanced a considerable way to the north-east; and on the 2d (Plate XVI.), it may be observed further to the north-east, and to be now leaving Europe by way of Lapland.

Storm III. had its centre, on the 1st November (Plate XV.), apparently within a hundred miles of the west of Ireland, where the pressure was 28·9. On the 2d (Plate XVI.), it had travelled east to Liverpool. On the 3d (Plate XIX.), it had continued its eastern course, and was now on the west of Jutland. At the same time the area of the storm had contracted to a fourth part of its former diameter; and the lowest pressure of the centre, instead of 28·9, was only 29·3. It was thus giving unmistakable signs of wasting away, and next morning (Plate XIX.), it had quite disappeared—a wide space between the barometric lines at the entrance of the Gulf of Finland being all that remained to show where it had died out.

The general features of the other storms were similar to those already described. The storm of the 10th and 11th November, and the storm in the beginning of December, had, however, certain peculiar features of their own to which I shall briefly advert.

The chart for the 11th November (Plate XVIII.) is the most remarkable of the charts, and the more so if compared with that of the 10th (Plate XVII.). Though the barometer fell a little to the N.E. at the head of the Gulf of Bothnia, yet the great fall took place to the S.W. To so great an extent did this occur, that the whole atmospheric system of Western Europe must be considered as having retreated on its course, and to have travelled from the N.E. to the S.W. As the translation proceeded, the depression widened and deepened, and a new depression was formed. The depression near Shetland, circumscribed by 29·3 on the 10th, and measuring 410 by 250 miles, increased to 1100 by 550 miles on the 11th, and the pressure in the centre was two-tenths of an inch greater. The isobarometric 29·5 had changed its position in a most remarkable manner. Its distance from 29·3 was greatly increased in Great Britain, and a new storm (VI.) was formed in the interval, 300 miles in diameter, having its centre near Plymouth. A little to the north of this, a space of about the same extent was noted for its high temperature on the 10th. The isobarometric 29·7 had also changed to a position equally remarkable, leaving a large space between it and 29·5 from Sardinia northwards; and in the interval a depression was formed round Genoa. On the 12th the two northern depressions had coalesced, and the isobarometric 29·3 contracted to a fourth part of what it was on the 11th, and the whole driven backward toward the N.E. The southern depression had travelled to the N.W., tripled its area, and was one-tenth of an inch lower.

The storm in the beginning of December will be afterwards described under the head of the "Direction of Storms."

Form of Storm Areas.

The forms of forty-two different areas circumscribed by the isobarometric lines admitted of examination. Of these, thirty were either circular or slightly elliptical. In ten cases, the major axis of the ellipse was nearly double the length of the minor axis, and in one case it was three times the length. In two instances, 29·5 on the 11th November, and 29·3 on the 3d December, the outline of the areas was very irregular, owing to the occurrence of two central depressions in one case, and three in another, within it. It follows from this, that the storms most commonly assumed a circular or oval form, and that the ellipses were seldom much elongated.

The area over which the storms spread themselves was very variable in size, being seldom less than 600 miles across, but often two or three times that amount. This area was not constant, even as regarded the same storm from day to day, but varied in size, sometimes contracting and sometimes expanding. If it contracted, the central depression at the same time gave signs of filling up, and the storm of dying out. On the other hand, if the area widened, the central depression generally became deeper, and occasionally was broken up into two or more separate depressions, which appeared to become separate storms with the wind circling round each, as shown in the maps for 11th November and 3d and

17th December. These different depressions, however, soon reunited, and the storm proceeded as before.

Direction in which the Major Axis of the Storm Area lay.

The direction in which the major axis of the storm lay, could be determined on twenty-eight occasions. On seven occasions it pointed to the N.E.; on six to the E.; on five to the N.N.E.; on four to the S.E.; on three to the E.N.E.; and on three to the N. In most cases the major axis was coincident, or nearly coincident, with the direction in which the storm happened to be moving at the time. These two features of storms have important bearings on the prediction of storms, and on the direction and veering of the wind.

It has been sometimes affirmed of the European storms that they are constantly marked by a barometric depression stretching in a north and south direction over Europe; but the analyses of these storms given above show that this assertion, in these cases at least, receives no support from fact.

Direction in which the Storms advanced over Europe.

The direction in which the storms advanced from the position they occupied on one day to the position they occupied on the next day, could be ascertained in twenty-four cases. In eleven of these, the progressive movement was to the N.E.; in four to the E.; in four to the S.E.; in two to the E.S.E.; and one to the E.N.E., S.S.E., and S.W. Thus, twenty-two travelled towards some point in the quadrant from N.E. to S.E., and only one took a westerly direction. Hence, these storms travelled as often toward the N.E. as toward all other points of the compass put together, and almost every one toward some point between N.E. and S.E.

The storms seldom proceeded in the same uniform direction from day to day. Though generally the change was not great, yet occasionally it was so. Thus of the many interesting features which marked the storm of the beginning of December, none were more remarkable than the sudden changes of its progressive movement. I have added in the Appendix observations relative to this storm at shorter intervals of time than 24 hours. From these it appears that the centre of the storm on the 2d was near Liverpool at 9 A.M. (Plate XX.); Worcester at noon; Oxford at 3 P.M.; Cherbourg at 6 P.M.; and Oxford at 9 P.M. The greater number of observations at 9 P.M. show three depressions:—1*st*, at Shetland, 28·88; 2*d*, at Oxford, 28·89; and, 3*d*, in Holland, 29·22. It is very probable that the storm had separated into two parts near Liverpool, one of which took a north-easterly course toward Shetland, and the other a south-easterly course toward Cherbourg. At 9 A.M. of the following morning (Plate XX.), the first had advanced to the N.E. to Christiansund; the second had advanced northward to Shields; and the third had advanced eastward to Denmark. At 9 P.M. of the 3d, the first was no longer visible, having probably left Europe by the North Cape, while the second had advanced to the north of Holland. At 9 A.M. of the 4th (Plate XXI.), it had advanced to Copenhagen, had greatly diminished in area, and the central depression was five-tenths less than on the previous morning; and at 9 P.M., the observations at Christiania, Kiel, and Konigsberg show that the atmospheric equilibrium was restored, and the storm consequently had died out.

Most of the storms left Europe by the North Cape or the north-east of Russia; but two of them (Storms III. and X.) wasted away and died out before reaching Russia.

Rate at which the Storms travelled.

The distance between the points indicating the centre of the barometric depression, or the centre of the storm on two consecutive days, could be determined on twenty-one occasions, which are given in the following table:—

No. of Storm.	For Twenty-four Hours, ending 8 A.M.	Distance travelled by Storm in Eng. miles in one day.	Rate per Hour.
Storm I. . . .	Oct. 29	580	24
" II. . . .	" 30	600	24
" II. . . .	" 31	420	18
" II. . . .	Nov. 1	400	17
" II. . . .	" 2	420	18
" III. . . .	" 2	400	17
" III. . . .	" 3	430	18
" III. . . .	" 4	510	21
" IV. . . .	" 5	700	29
" IV. . . .	" 6	470	20
" IV. . . .	" 7	360	15
" IV. . . .	" 8	470	20
" V. . . .	" 11	260	11
" V. . . .	" 12	390	16
" VII. . . .	" 22	460	19
" VII. . . .	" 23	560	23
" IX. . . .	Dec. 2	500	21
" IX. . . .	" 3	390	16
" IX. . . .	" 4	425	18
" XI. . . .	" 17	485	20
" XI. . . .	" 18	460	19
Means, .	...	461	19

Hence, the mean distance the storms travelled each day was 460 miles, being at the rate of 19 miles an hour. The least distance was from the 10th to the 11th, being only 260 miles, or 11 miles an hour. It was on this occasion that Storm V. retrograded toward the S.W., and the distance given is in all likelihood too small, it being probable that it did not begin its retrograde motion till sometime *after* 8 A.M. of the 10th. Hence, 15 miles per hour may be accepted as the minimum rate per hour travelled by any of these storms. The greatest distance travelled on any day was from the 4th to the 5th, being 700 miles, or 29 miles per hour.

If Storm VI. really overtook Storm V. by the morning of the 12th, its progressive motion must have far exceeded any of those in the table, since it must have travelled, in twenty-four hours, at least 1050 miles, or 46 miles an hour. The following observations, in addition to those given in the table, bear on this interesting point:—

TABLE SHOWING THE BAROMETRIC PRESSURE AND DIRECTION OF THE WIND AT TWENTY-FOUR PLACES, AT 8–9 A.M., 2 P.M., AND 8–9 P.M. OF THE 11TH NOV. 1863.

11th November 1863.		E. Yell.	Sandwick.	Stornoway.	Portree.	Dunrobin.	New Pitsligo.	Greenock.	South Cairn.	Drumlanrig.	Barry.	Milne-Graden.	Liverpool.
8-9 A.M.	Barometer,	29·09	29·14	29·29	29·14	29·23	29·22	29·36	29·46	29·34	29·30	29·39	29·38
	Wind, . .	WNW	W	W	W	W	W	NW	E	W	Calm	...	S by W
2 P.M.	Barometer,	...	...	...	...	...	...	...	...	...	...	...	29·44
	Wind, . .	...	...	...	...	...	...	...	...	...	...	...	NW
8-9 P.M.	Barometer,	29·30	29·40	29·53	29·47	29·38	29·36	29·53	29·55	29·52	29·46	29·51	29·59
	Wind, . .	NWW	N	W	W	NW	W	NW	N	N	Calm	W	WNW

11th November 1863.		Nottingham.	Oxford.	Greenwich.	Flushing.	Helder.	Groningen.	Utrecht.	Luxemburg.	Paderborn.	Kiel.	Christiania.	Königsberg.
8-9 A.M.	Barometer,	29·45	29·38	29·39	29·50	29·41	29·43	29·47	29·68	29·53	29·45	29·35	29·67
	Wind, . .	...	NNE	E	SW	W	S	SW	NW	SE	SW	SSW	SE
2 P.M.	Barometer,	...	29·39	29·38	29·42	29·43	29·43	29·39	...	...	...	...	...
	Wind, . .	...	N by W	NE	S	SW	S	SW	...	...	...	...	...
8-9 P.M.	Barometer,	29·61	29·60	29·56	29·48	29·46	29·47	29·46	29·43	...	...	29·24	29·59
	Wind, . .	NNW	WNW	W	S	SE	S	S	SW	SE	SW	SE	S

At 12 noon, the pressure at Paris was 29·39, and wind S.S.E.; and at Luxemburg 29·52, and wind S.W. From these observations, it is probable that this storm continued to advance southwards for an hour or two after the morning observation; it then turned to the N.E., and in the evening advanced over the North Sea nearly to the south of Norway, as shown by the pressure and direction of the winds at that time, and soon after became absorbed in Storm V.

As the position of the centres of Storms VIII. and X. could be ascertained only on one day, the rate of their motion cannot be determined.

Since storms generally travel to the N.E. at an average speed of about twenty miles an hour, and since the distance of the S.W. of Ireland from any British port does not exceed 500 miles, it follows that these storms might have been predicted at least twenty-four hours before their occurrence at the eastern seaports of Great Britain; and as their approach could have been foreseen some hours before they burst upon the west of Ireland, they might have been predicted from thirty-six to forty-eight hours beforehand.

Comparison of the Barometric and Thermometric Lines.

The observations of the thermometer do not equal in importance those of the barometer, for this among other reasons, that while the barometer measures the weight of the whole atmosphere pressing on it, the thermometer gives only the temperature of that portion of the air which is in immediate contact with the earth. There appears to be little apparent connection between these lines at first sight; for while the barometric lines approach more or less closely the curves of the circle or the ellipse, the lines of equal thermometric disturbance present the greatest possible irregularity of form. When, however, the attention is confined to the region of greatest barometric disturbance, a remarkable connection is at once observed. It will be seen that in all cases the temperature rose a few degrees over the space toward which and over which the front part of the storm was advancing, and fell at those places over which the front part of the storm had already passed. In other words, the temperature rose as the barometer fell, and fell as the barometer rose. Generally, the temperature in advance of the storm was above the average, and in the rear of the storm below it. But if it was considerably above the average in advance of the storm, it was still above the average when the storm had passed, though lower than it was before.

In one or two cases the temperature, after falling a little, rose in what appeared to be the wake of a storm; but in these cases the observations of the following day showed that another storm was advancing close upon the one already past. The high temperature thus indicated the approach of the second storm, and properly belonged to it.

Observations of Rain and Cloud.

We learn from the observations that as long as the barometer did not fall below the mean, there was no continuous rain anywhere, but blue sky prevailed, varied with partially clouded sky or with fog. But when the barometer fell, the sky began to be obscured, and rain to fall at intervals; and as the central depression advanced, the rain became more general, heavy, and continuous. After the centre of the storm had passed, or when the barometer had begun to rise, the rain generally became less heavy, falling more in showers than continuously; the clouds began to break up, and fine weather, ushered in with cold breezes, ultimately prevailed.

In order to show where the greatest amount of rain fell, Table IV. has been prepared, giving the rainfall at those places over which, or near which, the storms passed. It is necessary to explain that, as far as known, the rain given in the table fell during the twenty-four hours preceding the date of each entry. Thus, as the centre of Storm II. was west of Ireland on the 1st November, and in the centre of England on the 2d, the rainfall of the 2d took place as the storm travelled between these places. The rainfall at the Irish, English, and French stations, over which the front part of the storm had passed, was excessive—two inches having fallen at Brest; one inch at Liverpool and Dover; and about three-quarters of an inch at L'Orient, Galway, &c. On the same day the rainfall in Scotland was everywhere small, none falling at many places, and the largest fall being about one-sixth of an inch at Portree, in Skye. In Scotland, where the fall of rain was small, the wind was feeble, in no case blowing a gale; and the barometer, though low, had varied little during the twenty-four hours, and a pressure almost equally low prevailed for a considerable distance round. On the other hand, where the rainfall was in excess, violent gales prevailed, the fluctuations of the barometer had been great, and the isobarometric lines were much crowded together in the vicinity. On the following day the storm had passed eastward to Denmark. The rain over the west of England and of France had diminished, but increased over the *east* of England, and in Belgium and the Netherlands, over which the storm had travelled on its way to Denmark. The Irish rainfall was small, but not so small as would have been, but for the advance of Storm III., whose rainfall swelled the amount; and the same cause increased the Scottish rainfall.

All the other storms showed similar relations to the rainfall. The amount precipitated was greatest during the time the front part of the storm passed any place, and appeared to be in a great measure proportioned to the atmospheric disturbance experienced during the twenty-four hours, and the violence of the wind occasioned by that disturbance. In the wake of storms, though the atmospheric disturbance was equally great, and the violence of the wind as great, or even greater, the rainfall was very much less, except when the advance of another storm increased the amount.

Observations of the Wind.

Every one of the storms on each day presented the winds under the same conditions, viz., whirling round the area of low barometer in a circular manner, in a direction contrary to the motion of the hands of a watch, with a constant tendency to turn inwards towards the centre of lowest barometer. The wind in storms neither blows round the centre of least pressure in circles (or as tangents to the concentric barometric curves), nor does it blow directly towards that centre. It takes a direction nearly intermediate, approaching, however, more nearly the direction and course of the circular curves than of the radii to the centre. To this general rule none of the eleven storms any day offered an exception. When the centre of the storm was in a situation where observations were made all round it, the following were the general directions as observed:—

At places	S.	of the centre of least pressure,	the wind was	generally	S.W.
,,	S.E.	,,	,,	,,	S.
,,	E.	,,	,,	,,	S.E.
,,	N.E.	,,	,,	,,	E.
,,	N.	,,	,,	,,	N.E.
,,	N.W.	,,	,,	,,	N.
,,	W.	,,	,,	,,	N.W.
,,	S.W.	,,	,,	,,	W.

The greater the force of the wind at any place, the more nearly did it approximate to the directions here indicated. On those occasions when no observations were obtained from one or more of the sides of the storm, such observations as were obtained followed the same rule. Hence the atmosphere on every occasion rotated round the centre of the storm; and it should be kept in mind that this is no theoretical statement, but the result of observations faithfully put down on maps.

It will follow from this, that as the storms advanced to the eastward the general veering of the wind at places lying north of the central path of the storm would be from the N.E. by N. to W.; and at places to the south of the centre, from the N.E. by E. and S. to N.W.

On referring to the chart of the 2d November (Plate XVI.), it will be seen that the violence of the wind was greatest in the north of France, and south of England and Ireland, where there were great differences in the pressure, as shown by the crowding together of the isobarometric lines. On the other hand, it will be seen that the wind was nowhere blowing a gale in North Britain, where the pressure varied little for a great distance all round, as shown by the distance between the isobarometric lines, even though the pressure there was absolutely low. Again, on the 11th November (Plate XVIII.), the isobarometric lines were far apart in Storm V. in the north, and the wind was nowhere strong within that disturbed area; whereas the lines were much crowded in Storm VI. round Plymouth, and the wind was blowing strongly all round. This blowing of the wind from a high to a low barometer, and with a force generally proportioned to the differences of the pressure, would appear from these storms to be the most important law concerned in regulating the movement of the wind. As the wind approached the centre of least pressure, its violence gradually abated, till, on reaching the centre, a lull or calm prevailed.

Calms and light winds also prevailed along the ridge of highest barometer, or the region where the pressure was greatest, and on receding from which, on each side, the pressure diminished. It may not unaptly be compared to the watershed in physical geography, since from it the winds flowed away towards those places where the pressure was less. It sometimes extended over the Continent from N. to S., sometimes from E. to W., and sometimes in other directions; frequently it curved through Europe in a very irregular manner, forming the boundary line between a disturbed area in the north and another in the south; occasionally it was broken up into different parts; and more rarely it was concentrated in one locality, forming an area of high barometer approaching a circular form. In this last case, which happened on the 5th December in western Europe (Plate XXI.), and on several other occasions, the wind was always observed gently *whirling out of the area of high barometer*, in the direction of the motion of the hands of a watch—being the opposite direction to that assumed by the wind when it blows round and in towards an area of low pressure.

Storms of the Mediterranean.

The observations from Austria, Turkey, Greece, Russia, and Syria are too scanty to enable us to trace satisfactorily any of the storms which occurred there during the period. There is enough, however, to show that the conclusions which may be drawn from those storms which passed over northern and western Europe cannot safely be applied to the storms of the Mediterranean, as regards their form, the direction from which they come, and the course generally pursued by them.

App.

APPENDIX.

Additional observations showing the progress of Storm IX. from the 2d to the 4th December 1863, referred to at p. 197.

TABLE I.—Showing Comparative Readings of the Barometer at Oxford and Greenwich, from December 1d 22h to December 3d 22h 1863. (*From Proceedings of the British Meteorological Society, Vol. II. pp.* 48–59.)

1863. Month, Day, and Hour.	Barometric Readings.		1863. Month, Day, and Hour.	Barometric Readings.		1863. Month, Day, and Hour.	Barometric Readings.		1863. Month, Day, and Hour.	Barometric Readings.	
	Oxford.	Greenwich.		Oxford.	Greenwich.		Oxford.	Greenwich.		Oxford.	Greenwich.
d. h.	in.	in.	d. h.	in.	in.	d. h.	in.	in.	d. h.	in.	in.
Dec. 1 22	28·77	28·82	Dec. 2 10	29·31	29·38	Dec. 2 21	28·89	28·90	Dec. 3 10	29·76	29·73
23	78	81	11	30	38	22	97	97	11	84	80
Dec. 2 0	81	84	11½	28	37	23	29·04	29·03	12	89	84
1	84	84	12	27	36	Dec. 3 0	09	09	13	94	89
2	95	87	13	24	35	1	13	14	14	98	92
3	29·04	95	14	19	32	2	17	18	15	30·01	96
4	10	29·06	15	04	28	4	23	23	16	04	30·01
5	18	14	16	28·93	14	3	29	27	17	07	05
6	24	23	17	80	06	5	37	33	18	11	09
7	26	27	18	73	28·92	6	44	40	19	12	13
8	28	30	18¾	68	85	7	53	48	20	14	15
9	31	34	19	74	83	8	62	57	21	16	18
Dec. 2 9½	31	38	Dec. 2 20	80	79	Dec. 3 9	70	66	Dec. 3 22	18	20

Dec. 1d 22h minimum reading at Oxford; 1d 23h minimum reading at Greenwich.
2d 8h 40m to 10h 40m stationary at maximum value at Oxford.
2 9 30 to 11 20 stationary at maximum value at Greenwich.
2 18 30 to 18 45 minimum value at Oxford.
2 19 30 to 20 12 minimum value at Greenwich.

TABLE II.—OF BAROMETRIC OBSERVATIONS IN DIFFERENT PARTS OF EUROPE FROM 9 A.M. OF THE 2D TO 9 P.M. OF THE 4TH DECEMBER 1863.

	2d December 1863.						3d December 1863.						4th December 1863.				
	9 A.M.	Noon.	3 P.M.	6 P.M.	9 P.M.	Mid-night.	9 A.M.	Noon.	3 P.M.	6 P.M.	9 P.M.	Mid-night.	9 A.M.	Noon.	3 P.M.	6 P.M.	9 P.M.
East Yell, ...	29·84	...	...	...	28·88	...	28·97	...	...	...	29·23	...	29·62	...	...	...	29·40
Stornoway, . . .	08	...	...	...	29·08	...	99	...	...	...	28	...	66	...	...	...	48
New Pitsligo, . .	17	...	...	...	05	...	94	...	...	...	52	...	82	...	...	...	64
Barry, ...	18	...	...	...	17	...	90	...	...	...	59	...	95	...	...	...	79
Cargen, ...	09	...	...	...	26	...	76	...	...	...	70	...	30·08	...	...	...	87
Liverpool, . . .	04	29·05	29·24	29·34	40	29·37	91	29·10	29·30	29·58	84	30·02	24	30·26	30·24	30·20	30·15
Nottingham, . .	29·00	28·94	10	41	...	41	83	00	20	48	...	29·90	25	30	25	22	...
Belvoir Castle, . .	...	...	06	...	...	...	...	...	12	...	...	...	...	...	20	...	...
Oxford, ...	31	29·27	04	28·73	28·89	09	29·70	89	30·01	30·11	30·16	...	...	...	...	...	...
Greenwich, . . .	34	36	28	92	90	09	66	84	29·96	09	18	...	...	...	...	...	...
Clifton, ...	28·97	...	43	...	...	...	25	...	63	...	...	...	38	...	45	...	...
Paris, ...	29·30	22	29	29·53	29·74	82	45	45	65	29·83	29·96	30·10	30·44	48	49	52	52
Luxemburg, . .	80	65	...	...	58	...	55	54	...	...	91	...	32	36	...	...	55
Flushing, . . .	34	...	13	...	33	...	25	...	25	...	42	...	26	...	40	...	38
Utrecht, ...	43	...	20	...	25	...	24	...	10	...	34	...	13	...	22	...	26
Leeuwarden, . .	54	...	26	...	24	...	34	...	28·85	...	28·77	...	29·96	...	15	...	20
Helvoetsluis, . .	44	...	13	...	23	...	28	...	29·09	...	...	...	30·22	...	34	...	34
Paderborn, . . .	77	...	...	...	33	...	54	...	...	...	29·15	...	29·73	...	...	...	30
Kiel, ...	80	...	...	...	37	...	30	...	...	...	17	...	18	...	...	...	10
Christiania, . . .	88	...	...	...	54	...	37	...	...	...	15	...	53	...	...	...	29·88
Konigsberg, . . .	30·20	...	...	...	96	...	85	...	...	...	65	...	71	...	...	...	30·13
Breslau, ...	08	...	...	...	74	...	83	...	...	...	55	...	69	...	...	...	09
Kremsmunster, .	10	...	...	...	82	...	30·03	...	...	...	84	...	91	...	...	...	39

XVII.—*On the Celtic Topography of Scotland, and the Dialectic Differences indicated by it.* By W. F. Skene, Esq.

(Read 17th April 1865.)

The etymology of the names of places in a country is either a very important element in fixing the ethnology of its inhabitants, or it is a snare and a delusion, just according as the subject is treated. When such names are analysed according to fixed laws, based upon sound philological principles, and a comprehensive observation of facts, they afford results both important and trustworthy; but if treated empirically, and based upon resemblance of sounds alone, they become a mere field for wild conjectures and fanciful etymologies, leading to no certain results. The latter is the ordinary process to which they are subjected. The natural tendency of the human mind is to a mere phonetic etymology of names, both of persons and of places. It is this tendency which has given rise to what may be called punning etymologies, in which the King of Scotland plays so facetious a part, when the first Guthrie had that name fixed upon by the king, from his proposing when asked, how many fish should be prepared, to *gut three;* and when Rosemarkie received its name because the king, on asking what land he neared, was answered, *Ross mark ye.* This illustrates the natural tendency to suggest a mere phonetic etymology, in which the sounds of the name of the place appear to resemble the sounds in certain words of a certain language, the language from which the etymology is derived being selected upon no sound philological grounds, but from arbitrary considerations merely.

Unhappily, an etymology founded upon mere resemblance of sounds has hitherto characterised all systematic attempts to analyse the topography of Scotland, and to deduce ethnologic results from it. Prior to the publication of the "Statistical Account of Scotland" in 1792, it may be said that no general attempt had been made to explain the meaning of the names of places in Scotland, or to indicate the language from which they were derived. We find occasionally, in old lives of the saints, and in charters connected with church lands, that names of places occurring in them are explained; and these interpretations are very valuable, as indicating what may be termed the common tradition of their meaning and derivation at an early period. Of very different value are a few similar derivations in the fabulous histories of Boece, Buchanan, and John Major, which are usually mere fanciful conjectures of pedantry.

The first impetus to anything like a general etymologising of Scottish topography was given when Sir John Sinclair projected the "Statistical Account of

Scotland." In the schedule of questions which he issued in 1790 to the clergy of the Church of Scotland, the first two questions were as follows:—

1. What is the ancient and modern name of the parish?
2. What is the origin and etymology of the name?

This set every minister thinking what was the meaning of the name of his parish. The publication of the "Poems of Ossian," and the controversy which followed, had tended greatly to identify national feeling and the history of the country with Gaelic literature and language, and, with few exceptions, the etymology was sought for in that language. The usual formula of reply was, "the name of this parish is derived from the Gaelic," and then followed a Gaelic sentence resembling in sound the name of the parish, and supposed admirably to express its characteristics, though the unfortunate minister is often obliged to confess that the parish is remarkably free from the characteristics expressed by the Gaelic derivation of its name. These etymologies are usually suggested irrespective entirely of any known facts as to the history or population of the parish, and are purely phonetic.

Thus the writer of the account of Elie, in the New Statistical, observes:— "The writer of the former Statistical Account has, according to the fashion which seems to have prevailed in his day, as well as now, had recourse to Gaelic, the mother as it should seem of languages, and tells us that the parish received its name from '*A Liche*,' signifying 'out of the sea.' We are disposed to doubt its soundness, for the village is not further out of the sea than any other part of the coast, nay, it extends further into it. We should rather be inclined to consider Elie as having sprung from the Greek word *elos*, a marsh."

Both etymologies are entirely irrespective of the fact, that the old form of the word was "chellin."

After the publication of the Statistical Account, Gaelic was in the ascendant as the source of all Scottish etymologies, till the publication of CHALMERS' "Caledonia" in 1807. JOHN PINKERTON had indeed tried to direct the current of popular etymology into a Teutonic channel, but his attempts to find a meaning in Gothic dialects for words plainly Celtic were so unsuccessful, that he failed even to gain a hearing. CHALMERS was more fortunate. His theory was, that a large proportion of the names of places in Scotland are to be derived from the Welsh, and indicate an original Welsh population. And this he has worked out with much labour and pains. In doing so, he was the first to attempt to show evidence of the dialectic difference between Welsh and Gaelic pervading the names of places, and to discriminate between them; but for almost all the names of places in the Lowlands of Scotland he furnishes a Welsh etymology, which, like his predecessors the Scottish clergy, he supposes to be expressive of the characteristics of the locality. His theory has, in the main, commanded the assent of subsequent writers, and is usually assumed to be, on the whole, a correct representation of the state of the fact. Yet his system was as purely one of a phonetic etymology, founded upon mere resemblance of sounds, as those of his predecessors. The MSS. left by GEORGE CHALMERS show how he set about preparing his etymologies, and we now know the process he went through. He had himself no knowledge of either branch of the Celtic language, but he sent his list of names to Dr OWEN PUGHE; and that most ingenious of all Welsh lexicographers, who was capable of reducing every word in every known language in the world to a Welsh original, sent him a list of Welsh renderings of each word, varying from twelve to eighteen in number, out of which CHALMERS selected the one which seemed to him most promising.

As an instance, we may refer to a pet etymology of CHALMERS, on which he has built as historical fact, and which has been followed by all subsequent writers. He interprets Kilspindy, the name of a place in Aberlady Bay, which belonged to the bishop of Dunkeld, as signifying in Welsh *Cill ys pendu*, which he renders "the Cell of the Black Heads," and supposed that it indicated a settlement of the Culdees. We have no reason to suppose that the Culdees were distinguished by having black head-dresses; but the etymology is philologically false, for *Cill* is Gaelic and not Welsh. *Ys* is no known form of the article in Welsh, and *pen du* means black head in the singular. In the plural, it would be *penau duon*. The old form of the word puts the etymology to rout, for it was originally written "Kinespinedin." His other etymologies are equally founded on a mere resemblance of sounds between the modern form of the word and the modern Welsh, as those of the clergy in the Statistical Account were between the modern form of the word and the modern Gaelic.

That system of interpreting the names of places, which I have called phonetic etymology, is, however, utterly unsound. It can lead only to fanciful renderings, and is incapable of yielding any results that are either certain or important.

Names of places are, in fact, sentences or combinations of words originally expressive of the characteristics of the place named, and applied to it by the people who then occupied the country, in the language spoken by them at the time, and are necessarily subject to the same philological laws which governed that spoken language. The same rules must be applied in interpreting a local name as in rendering a sentence of the language.

That system, therefore, of phonetic etymology which seeks for the interpretation of a name in mere resemblance of sound to words in an existing language, overlooks entirely the fact that such names were fixed to certain localities at a much earlier period, when the language spoken by those who applied the name must have differed greatly from any spoken language of the present day.

Since the local names were deposited in the country, the language itself from which they were derived has gone through a process of change, corruption, and decay. Words have altered their forms—sounds have varied—forms have become obsolete, and new forms have arisen—and the language in its present state no longer represents that form of it which existed when the local nomenclature was formed. The topographical expressions, too, go through a process of change and corruption till they diverge still further from the spoken form of the language as it now exists.

This process of change and corruption in the local names varies according to the change in the population. When the population has remained unchanged, and the language in which the names were applied is still the spoken language of the district, the names either remain in their original shape, in which case they represent an older form of the same language, or else they undergo a change analogous to that of the spoken language. Obsolete names disappear as obsolete words drop out of the language, and are replaced by more modern vocables. Where there has been a change in the population, and the older race are replaced by a people speaking a kindred dialect, the names of places are subjected to the dialectic change which characterises the language. There are some striking instances of this where a British form has been superseded by a Gaelic form, as, for instance, Kirkintulloch, the old name of which, NENNIUS informs us, was Cærpentaloch, *kin* being the Gaelic equivalent of the Welsh *pen;* Penicuik, the old name of which was Penjacop; Kincaid, the old name of which was Pencoed.

When, however, the new language introduced by the change of population is one of a different family entirely, then the old name is stereotyped in the shape in which it was when the one language superseded the other, becomes unintelligible to the people, and undergoes a process of change and corruption of a purely phonetic character, which often entirely alters the aspect of the name. In the former cases it is chiefly necessary to apply the philologic laws of the language to its analysis. In the latter, which is the case with the Celtic topography of the low country, it is necessary, before attempting to analyse the name, to ascertain its most ancient form, which often differs greatly from its more modern aspect.

It is with this class of names we have mainly to do, as presenting the phenomena I am anxious to investigate.

When the topography of a country is examined, its local names will be found, as a general rule, to consist of what may be called generic terms and specific terms. What I mean by generic terms are those parts of the name which are common to a large number of them, and are descriptive of the general character of the place named; and by specific terms, those other parts of the name which have been added to distinguish one place from another. The generic terms are usually general words for river, mountain, valley, plain, &c.; the specific terms, those words added to distinguish one river or mountain from another. Thus, in the Gaelic name Glenmore, *glen* is the generic term, and is found in a numerous class of words—*more*, great, the specific, a distinguishing term, to distinguish it from another called Glenbeg. In the Saxon term Oakfield, field is the generic term, and oak the specific, to distinguish it from Broomfield, &c.

When the names of places are applied to purely natural objects, such as rivers, mountains, &c., which remain unchanged by the hand of man, the names applied by the original inhabitants are usually adopted by their successors, though speaking a different language; but the generic term frequently undergoes a phonetic corruption, as in the Lowlands, where Aber has in many cases become Ar in Arbroath, Arbuthnot; Ballin has become Ban, as in Bandoch; Pettin has become Pen as Pendriech; Pol has become Pow; and Traver has become Tar and Tra.

On the other hand, where the districts have been occupied by different branches of the same race, speaking different dialects, the generic terms exhibit the dialectic differences when the sounds of the word are such as to require the dialectic change; thus in Welsh and Gaelic:—

Pen and Kin—a head,
Gwyn and Fionn—white,

shows the phonetic difference between these dialects.

The comparison of the generic terms which pervade the topography of a country affords a very important means of indicating the race of its early inhabitants, and discriminating between the different branches of the race to which the respective portions of it belong.

Between the Celtic and Teutonic races the generic terms afford this great leading distinction, that in Celtic names they are invariably found at the beginning of the word; in Teutonic names, at the end of the word. Thus, Glenesk in Celtic is Eskdale in Teutonic; Dunedin is Edinburgh; Auchindarroch is Oakfield, and so forth. In the one, the generic term, at the beginning of the word; in the other, at the end.

It was early observed that there existed in the Celtic generic terms a difference which seemed to indicate dialectic distinction. Even in the Old Statistical Account, the minister of the parish of Kirkcaldy remarks,—"To the Gaelic language a great proportion of the names of places in the neighbourhood, and indeed through the whole of Fife, may unquestionably be traced. All names of places beginning with Bal, Col or Cul, Dal, Drum, Dun, Inch, Inver, Auchter, Kil, Kin, Glen, Mon, and Strath, are of Gaelic origin. Those beginning with Aber and Pit are supposed to be Pictish names, and do not occur beyond the territory which the Picts are thought to have inhabited."

Chalmers states it still more broadly and minutely. He says,—"Of those words which form the chief compounds in many of the Celtic names of places in the Lowlands, some are exclusively British, as Aber, Llan, Caer, Pen, Cors, and others; some are common to both British and Irish, as Carn, Craig, Crom, Bre, Dal, Eaglis, Glas, Inis, Rinn, Ros, Strath, Tor, Tom, Glen; and many more are significant only in the Scoto-Irish or Gaelic, as Ach, Ald, Ard, Aird, Auchter, Bar, Blair, Ben, Bog, Clach, Corry, Cul, Dun, Drum, Fin, Glac, Inver, Kin, Kil, Knoc, Larg, Lurg, Lag, Logie, Lead, Letter, Lon, Loch, Meal, Pit, Pol, Stron, Tullach, Tullie, and others."

This attempt at classification is, however, exceedingly inaccurate. Two of the words in the first class, Llan and Caer, are common to both British and Irish;

and a large portion of the third class are significant in pure Irish, as well as in the Scoto-Irish or Gaelic. No attempt is made to show, by the geographical distribution of these words, in what parts of the country the respective elements prevail.

In a recent work, however, of some pretension, by an eminent Gaelic scholar, this attempt is made; and I refer to it to show how very loosely popular ideas on this subject are taken up. He says, "The Blackadder and Whiteadder contain distinctly the British Dwfr or Dwr, water." The two names are Teutonic, and have obviously no Celtic form. "In East Lothian, Yester is the old British word Ystrad, a valley." This is correct, but it is on British ground. "Tranent and Traquair have the British Tre, a town." The old form is Travernent and Traverquair, and Traver is unknown in Welsh topography. "On crossing the Forth, British names still appear nowhere more clearly than in the name of the Ochil Hills, where the British Uchel (high), cannot be mistaken." This is phonetic etymology, and, as we shall see, it has been mistaken. "In Fife we find several Abers, Pits, and Pittens, indicating the existence of a British population; and again the Pits and Pittens of Forfarshire are numerous." Of the Abers we shall talk presently; but if the Pits and Pittens indicate a British population, how comes it that they are unknown in Wales, and are not to be found in Welsh topography. "We have," says he, "Pens and Abers and Pits in abundance on through Kincardine and Aberdeenshire." Abers and Pits certainly, but no Pens except one solitary instance, which is doubtful. I need not proceed. The statement goes on in the same strain, at equal variance with topographical and philological facts.

The most popular view of the subject, and that which has recently been most insisted in, is the line of demarcation between a Kymric and a Gaelic population, supposed to be indicated by the occurrence of the words Aber and Inver.

This view has been urged with great force by KEMBLE, in his Anglo-Saxons; but I may quote the recent work by Mr ISAAC TAYLOR, on words and places, as containing a fair statement of the popular view of the subject:—

"To establish the point that the Picts or the nation, whatever was its name, that held central Scotland, was Cymric, not Gaelic, we may refer to the distinction already mentioned between *Ben* and *Pen*. *Ben* is confined to the west and north; *Pen* to the east and south. *Inver* and *Aber* are also useful test words in discriminating between the two branches of the Celts. The difference between the two words is dialectic only; the etymology and the meaning is the same—a confluence of waters, either of two rivers or of a river with the sea. Aber occurs repeatedly in Brittany, and is found in about fifty Welsh names, as Aberdare, Abergavenny, Abergele, Aberystwith, and Barmouth, a corruption of Abermaw. In England we find *Aber*ford in Yorkshire, and *Ber*wick in Northumberland and Sussex; and it has been thought that the name of the Humber is a corruption of the same root. *Inver*, the Erse and Gaelic forms, is common in Ireland, where *Aber* is unknown. Thus, we find places called Inver in Antrim, Donegal, Mayo, and Invermore in Galway and in Mayo. In Scotland the *Invers* and *Abers* are distributed in a curious and instructive manner. If we draw a line across the map from a point a little south of Inveraray to one a little north of Aberdeen, we shall find that (with very few exceptions) the *Invers* lie to the north of the line and the *Abers* to the south of it. This line nearly coincides with the present southern limit of the Gaelic tongue, and probably also with the ancient division between the Picts and the Scots."

Nothing can be more inaccurate than this statement. Ben is by no means confined to the west and north; and as examples of Pen, he refers, among others, to the Pentland Hills, Pentland being a Saxon word, and corrupted from Pectland; and to Pendriech in Perthshire, which is a corruption from Pettindriech.

So far from Inver being common in Ireland, it is very rare. The *Index locorum* of the Annals of the Four Masters shows only six instances. On the other hand, Aber is not unknown in Ireland. It certainly existed formerly, to some extent, in the north of Ireland; and Dr Reeves produces four instances near Ballyshannon.

The statement with regard to the distribution of Aber and Inver in Scotland here is, that there is a line of demarcation which separates the two words—that, with few exceptions, there is nothing but Invers on one side of this line, nothing but Abers on the other; and that this line extends from a point a little south of Inveraray to a point a little north of Aberdeen. This is the mode in which the distribution of these two words is usually represented; but nothing can be more perfectly at variance with the real state of the case. South of this line there are as many Invers as Abers. In Perthshire, south of the Highland line, there are nine Abers and eight Invers; in Fifeshire, four Abers and nine Invers; in Forfar, eight Abers and eight Invers; in Aberdeenshire, thirteen Abers and twenty-six Invers. Again, on the north side of this supposed line of demarcation, where it is said that Invers alone should be found, there are twelve Abers, extending across to the west coast, till they terminate with Abercrossan, now Applecross, in Ross-shire. In Argyleshire alone there are no Abers. The true picture of the distribution of these two words is—in Argyllshire, Invers alone; in Inverness and Ross shires, Invers and Abers in the proportion of three to one and two to one; and on the south side of the supposed line, Abers and Invers in about equal proportions.

Again he says, quoting Chalmers, "The process of change is shown by an old charter, in which King David grants to the monks of May, 'Inverin qui fuit Aberin.' So Abernethy became Invernethy, although the old name is now restored." In order to produce the antithesis of Inverin and Aberin, one letter in this charter has been altered. The charter is a grant of "Petneweme et Inverin quæ fuit Averin;" and I have the authority of the first charter antiquary in Scotland for saying that this construction is impossible; quæ fuit does not, in charter Latin, mean "which was," but "which belonged to," and Averin was the name of the previous proprietor of the lands. Abernethy and Invernethy are not the same place, and the former never lost its name. Invernethy is at the junction of the Nethy with the Earn, and Abernethy is a mile further up the river.

When we examine these Abers and Invers more closely, we find that in some parts of the country they appear to alternate, as in Fife—Inverkeithing, Aberdour, Inveryne, Abercrombie, Inverlevin, and so forth. *2d*, That some of the Invers and Abers have the same specific terms attached to them, as Abernethy and Invernethy, Aberuchill and Inveruchil, Abercrumbye and Invercrumbye, Abergeldie and Invergeldy; and, *3d*, That the Invers are always at the mouth of the river, close to its junction with another river, or with the sea; and the Abers usually a little distance up the river where there is a ford. Thus, Invernethy is at the mouth of the Nethy; Abernethy a mile or two above. These and other facts lead to the conclusion that they are part of the same nomenclature, and belong to the same period and to the same people.

When we look to the south of the Forth, however, we find this remarkable circumstance, that in Ayrshire, Renfrew, and Lanarkshire, which formed the possessions of the Strathclyde Britons, and was occupied by a British people till as late a period as the more northern districts were occupied by the Picts, there are no Abers at all.

What we have, therefore, is the Scots of Argyle with nothing but Invers, the Picts with Abers and Invers together, and the Strathclyde Britons with no Abers. As a mark of discrimination between races this criterion plainly breaks down, and the words themselves contain no sounds which, from the different phonetic laws of the languages, could afford an indication of a dialectic difference. The truth is, that there were three words expressive of the junction of one stream with another, and all formed from an old Celtic word, Ber, signifying water. These were Aber, Inver, and Conber (pronounced in Welsh *cummer*, in Gaelic *cumber*.) These three words were originally common to both branches of the Celtic as derivations from one common word. In old Welsh poems we find not only Aber as a living word in Welsh, but Ynver likewise,* and Dr Reeves notices an Irish document in which Applecross or Appurcrossan is called Conber Crossan. Ynver, however, became obsolete in Welsh, just as Cummer or Cumber and Aber became obsolete in Irish; but we have no reason to know that it did so in Pictish. In the Pictish districts, therefore, the Abers and Invers were deposited when both were living words in the language. When the Scots settled in Argyle, Aber had become obsolete in their language, and Inver was alone deposited, and in Strathclyde both words seem to have gone into desuetude.

In the same manner Dwfr or Dwr is quoted as a word for water, peculiar to the Welsh form of Celtic, and an invariable mark of the presence of a

* *Ynver* occurs twice in the Book of Taliessin.

British people, but the old form of this word in Scotland was Doboir, as appears from the Book of Deer, where Aberdour is written Abber-doboir, and in CORMAC's Glossary of the old Irish, Doboir is given as an old Irish word for water. In another old Irish glossary we have this couplet :—

> "Bior and An and Dobar,
> The three names of the water of the world."

These words, therefore, form no criterion of difference of race, and to judge by them is to fall into the mistake of the phonetic etymologists, viz., to apply to old names, as the key, the present spoken language, which does not contain words which yet existed in it in its older form.

In order to make generic terms a test of dialect they must be words which contain sounds affected differently by the different phonetic laws of such dialects, —such as Pen, Gwastad, Gwern, and Gwydd, which all enter copiously into Welsh topography, and the equivalents of which in the Gaelic dialects are Ken, Fearn, and Fiodh—Gwastad having no equivalent.

Such generic terms afford a test by which we can at once determine whether the Celtic topography of a country partakes most of the Kymric or the Gaelic character. The earliest collection of names in North Britain is to be found in Ptolemy's Geography in the second century, but we know too little of the origin of his names, whether they were native terms, or names applied by the invaders, to obtain from them any certain result. After Ptolemy, the largest collection of names in Great Britain is in the work of the anonymous geographer of Ravenna, a work of the seventh century. The exact localities are not given, but the names are grouped according to the part of Britain to which they belong. Those which commence the topography of Scotland are placed under this title :— "Iterum sunt civitates in ipsa Britannia quæ recto tramite de una parte in alia id est de oceano in oceano existunt, ac dividunt in tertia portione ipsam Britanniam." They commence with the stations on the Roman wall between the Tyne and the Solway, and then proceed northwards. Among these we find two names together, Tadoriton and Maporiton, and as Tad and Map are Kymric forms for father and son, we have no doubt that here we are on the traces of a Kymric population. The next group is arranged under this head :—"Iterum sunt civitates in ipsa Britannia, recto tramite una alteri conexæ, ubi et ipsa Britannia plus angustissima, de oceano in oceano esse dinoscitur." This part of Britain, which is *plus angustissima*, is the isthmus between the Forth and the Clyde, and in proceeding with the names northwards we come to one called Cindocellum. The Ocelli Montes were the Ochills, and here the Gaelic form of Kin is equally unmistakeable.

In the twelfth century, the Chartularies have preserved charters which contain the names of places, accompanied by an interpretation of the meaning of them. One bears upon the topography of Moray. It is a charter by Alexander II. to the monks of Kinloss of the lands of Burgyn, now Burgy, and has attached to it an old interpretation. Rune Pictorum is glossed the Pechts' fields, and Raoin is Gaelic for field. Tuber na crumkel, ane well with ane thrawn mouth—Tobar is well in Gaelic; Crom, crooked; and Beul, for which Kell is probably written by mistake, is mouth. Tuber na fein—of the Grett or Kempis men called Fenis, ane well.

In a perambulation of the marches of Monymusk by Malcolm IV., we have several such interpretations. Coritobrich is glossed Vallis fontis—Corre is Gaelic for valley, and Tobar, well. Scleuemingorn, Mora caprarum—Sliabh, Gaelic for moor; and Gabhar, goat. Alde clothi, rivulus petrosus—Ault, Gaelic for a stream; Clachach, stony. Breacachach, campus distinctis coloribus—Breacach, striped; Ach, field.

In a perambulation of the marches of Kingoldrum in 1256, we have names which are also glossed in a subsequent charter. Invercrumbyn is said to be the Concursus duorum amnium, Melgour et Crumbyn. Monybrech, Murrais of the quhilk runs ane strype—Monadh, a moss; Breac, striped. Pool of Monbuy, yellow pool—Buidh, yellow. Athyncroich, Gallow burne, from Ald, burn; Croich, gallows.

Thus on three points in the north-eastern lowlands, in Morayshire, in Aberdeenshire, and in Forfarshire, we find, as early as the thirteenth century, the local names interpreted in Gaelic. The names themselves are, too, in the Scotch Gaelic, not in the Irish form, and in most cases we find the dental substituted for the guttural, as *clothi* for *clachach*. When we apply to the present topography the testing words Pen, Gwynn, and Gwydd, the Gaelic equivalents of which are Kin, Fearn, and Fiodh, we find that with one exception, Pen, though frequent south of the Forth, where there was a British population, does not occur north of the Forth, while it is full of Kins, and Gwern and Gwydd occur only in their Gaelic equivalents.

Such then being the aspect in which the question really presents itself, it becomes important, with a view to ethnological results, to ascertain more closely the geographical distribution of the generic terms over Scotland, and in order to show this I have prepared a table of such distribution. The generic terms are taken from the index to the Record of Retours; and as this record relates to properties, and not to mere natural objects, the generic terms they contain are to a great extent confined to names of places connected with their possession by man, and more readily affected by changes in the population. For the purposes of comparison I have framed a list of generic terms contained in Irish topography from the index to the Annals of the Four Masters, and of those in Welsh topography from a list in the Cambrian Register. I have divided Scotland into thirteen districts, so as to show the local character of the topography of each part of Scotland, and opposite each generic term in Scotch topography is marked, 1*st*, if it occurs in Ireland, and how often; 2*d*, if it occurs in Wales; and 3*d*, I have marked the number of times it occurs in each district of Scotland from the Index of Retours.

On examining this table it will be seen that there are five terms peculiar to the districts occupied by the Picts. These are Auchter, Pit, Pitten, For, and Fin. Now none of these five terms are to be found in Welsh topography at all, and For and Fin are obviously Gaelic forms.

It is necessary, however, in examining these terms, which may be called Pictish, to ascertain their old form. Auchter appears to be the Gaelic Uachter, upper; and as such we have it in Ireland, and in the same form, as in Scotland Ochtertire, in Ireland Uachtertire. It does not occur in Wales.

The old form of Pit and Pitten, as appears from the Book of Deer, is Pette, and it seems to mean a portion of land, as it is conjoined with proper names, as Pette MacGarnait, Pette Malduib. But it also appears connected with Gaelic specific terms, as Pette an Mulenn, the Pette of the Mill, and in a charter of the Chartulary of St Andrews, of the church of Migvie, the terra ecclesiæ is said to be vocatus Pettentaggart—"an tagart" being the Gaelic form of the expression "of the priest."

The old forms of For and Fin are Fothuir and Fothen. The old form of Forteviot is Fothuir-tabaicht, and of Finhaven is Fothen-evin.

The first of these words, however, discloses a very remarkable dialectic difference. Fothuir becomes For, as Fothuir-tabacht is Forteviot; Fothuirduin is Fordun, but Fothuir likewise passes into Fetter, as Fothuiresach becomes Fetteresso; and these two forms are found side by side, Fordun and Fetteresso being adjacent parishes. The form of For extends from the Forth to the Moray Firth—that of Fetter from the Esk, which separates Forfar and Kincardine, to the Moray Firth.

An examination of some other generic terms will disclose a perfectly analogous process of change. The name for a river is Amhuin. The word is the same as the Latin Amnis. The old Gaelic form is Amuin, and the *m*, by aspiration, becomes *mh*, whence Amhuin, pronounced Avon. In the oldest forms of the language the consonants are not aspirated, but we have these two forms, both the old unaspirated form and the more recent aspirated form, in our topography, lying side by side in the two parallel rivers which bound Linlithgowshire—the Amond and the Avon. There is also the Amond in Perthshire. We know from the Pictish Chronicle that the old name was Aman, and the Avon, with its aspirated *m*, is mentioned in the Saxon Chronicle. It is a further proof that Inver is as old as Aber in the eastern districts, that we find Aman in its old form conjoined with Inver in the Pictish Chronicle in the name Inveraman.

In Dumbartonshire we find the names Lomond and Leven together. We have Loch Lomond and Ben Lomond, with the river Leven flowing out of the loch through Strathleven; but we have the same names in connection in Fifeshire, where we have Loch Leven with the two Lomonds on the side of it, and the river Leven flowing from it through Strathleven. This recurrence of the same words in connection would be unaccountable were it not an example of the same thing. Leven comes from the Gaelic Leamhan, signifying an elm tree, but the old form is Leoman, and the *m* becomes aspirated in a later stage of the language and forms Leamhan, pronounced Leven. Here the old form adheres to the mountain, while the river adopts the more modern.

A curious illustration of two different terms lying side by side, which are derived from the same word undergoing different changes, will be found in Forfarshire, where the term Llan for a church appears, as in Lantrethin. It is a phonetic law between Latin and Celtic, that words beginning in the former with *pl* are in the latter *ll*. The word Planum, in Latin signifying any cultivated spot, in contradistinction from a desert spot, and which, according to Ducange, came to signify Cimiterium, becomes in Celtic Llan, the old meaning of which was a fertile spot, as well as a church. In the inquisition, in the reign of David I., into the possessions of the See of Glasgow, we find the word in its oldest form in the name Planmichael, now Carmichael; and as we find Ballin corrupted into Ban, as Ballindoch becomes Bandoch, so Plan becomes corrupted into Pan, and we find it in this form likewise, in Forfarshire, in Panmure and Panbride. In the Lothians and the Merse this word has become Long, as in Longnewton and Longniddrie.

The Celtic topography of Scotland thus resembles a palimpsest, in which an older form is found behind the more modern writing. I shall not detain the Society further by going through other examples. The existence of the phenomenon is sufficiently indicated by those I have brought forward, and I shall conclude by stating shortly the results of this investigation.

1*st*, In order to draw a correct inference from the names of places as to the ethnological character of the people who imposed them, it is necessary to obtain the old form of the name before it became corrupted, and to analyse it according to the philological laws of the language to which it belongs.

2*d*, A comparison of the generic terms affords the best test for discriminating between the different dialects to which they belong, and for this comparison it is necessary to have a correct table of their geographical distribution.

3*d*, Difference between the generic terms in different parts of the country may arise from their belonging to a different stage of the same language, or from a capricious selection of different synonyms by different tribes.

4*th*, In order to afford a test for discriminating between dialects, the generic terms must contain within them those sounds which are differently affected by the phonetic laws of each dialect.

5*th*, Applying this test, the generic terms do not show the existence of a Kymric language north of the Forth.

6th, We find in the topography of the north-east of Scotland traces of an older and of a more recent form of Gaelic—the one preferring labials and dentals, and the other gutturals; the one hardening the consonants into tenues—the other softening them by aspiration; the one having Abers and Invers—and the other having Invers alone; the one a low Gaelic dialect—the other a high Gaelic dialect; the one I conceive the language of the Picts—the other that of the Scots.

Generic Terms.	Ireland.	Wales.	Scotland.												
			Angli.		Britones.				Picti.						Scoti.
			Berwick, Roxburgh, Haddington.	Mid-Lothian, Linlithgow.	Selkirk, Peebles.	Dumfries.	Ayr, Renfrew, Lanark.	Stirling, Dumbarton.	Perth.	Fife, Kinross.	Forfar.	Kincardine, Aberdeen, Banff.	Elgin and Nairn, Inverness, Ross, and Sutherland.	Kirkcudbright, Wigton.	Argyle, Bute.
Aber, . .	...	W	3	3	...	4	...	...	12	4	7	18	6	...	...
Ard, . .	66	...	...	...	...	...	...	16	34	6	14	66	51	5	93
Arn, . .	...	...	...	...	...	...	...	4	15	5	...	...	...	...	...
Ar, . . .	...	W	...	...	...	...	...	...	...	...	...	...	...	15	...
Auch, . .	25	...	...	...	...	...	25	...	24	6	27	162	153	12	107
Auchin, .	..	...	...	4	...	23	88	34	30	...	...	22	8	25	...
Auchter, .	...	...	...	...	...	...	...	...	6	10	6	12	4	...	...
Auld, . .	...	...	...	...	...	...	...	...	...	...	...	33	9	...	...
Bal, . .	...	...	...	...	...	...	36	63	90	88	127	67	59	56	39
Balna, . .	...	...	...	...	...	...	...	...	...	...	...	10	...	...	...
Ballie, . .	104	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Ballin, . .	...	...	3	...	...	...	...	...	...	...	...	3	...	...	...
Belloch, .	36	W	...	...	...	...	9	...	...	...	...	...	...	...	...
Bellie, . .	...	...	...	...	...	...	...	...	...	...	...	...	...	...	14
Ban, . .	...	...	...	...	...	...	...	...	...	16	...	...	...	...	...
Bar, . .	...	...	...	...	...	27	66	6	...	...	11	...	...	90	19
Barn, . .	...	...	...	...	...	...	...	...	...	...	...	...	...	6	...
Blair, . .	...	...	...	...	...	...	16	51	29	8	...	11	8	...	...
Bo, . . .	...	...	...	...	...	...	...	...	5	...	...	...	10	...	...
Carn, . .	28	...	...	...	...	...	11	...	13	8	8	54	15	4	...
Car, . . .	...	W	8	6	...	12	36	12	7	18	10	18	5	15	...
Col, . . .	...	...	...	...	...	...	...	...	7	...	...	17	...	...	...
Corrie, . .	...	...	...	...	...	...	...	...	9	...	...	...	8	...	...
Cambus, .	...	...	...	...	...	...	...	...	12	...	...	...	...	...	...
Clon, . .	93	...	...	...	...	8	13	...	...	...	...	...	...	7	...
Craig, . .	16	W	...	19	...	21	42	21	43	25	12	46	8	31	19
Cors, . .	...	W	...	...	...	14	...	...	...	...	...	...	...	9	...
Cul, . . .	39	...	...	...	...	...	47	...	25	11	...	22	22	7	...
Cumber, .	...	...	...	...	...	6	...	4	...	...	...	...	...	...	...
Cult, . .	...	...	...	...	...	...	...	...	10	...	...	...	...	...	...
Dal, . .	10	W	...	...	...	20	82	8	52	...	...	...	...	24	...
Drum, . .	64	...	...	4	...	30	50	26	51	33	25	56	36	57	25
Dun, . .	95	...	3	6	...	14	16	17	26	11	17	...	20	...	14
Fetter, . .	...	...	...	...	...	...	...	...	...	...	...	4	...	...	...
For, . .	...	...	...	...	...	...	...	...	13	9	11	22	...	...	...

Generic Terms.	Ireland.	Wales.	Scotland. Angli: Berwick, Roxburgh, Haddington.	Mid-Lothian, Linlithgow.	Britones: Selkirk, Peebles.	Dumfries.	Ayr, Renfrew, Lanark.	Stirling, Dumbarton.	Picti: Perth.	Fife, Kinross.	Forfar.	Kincardine, Aberdeen, Banff.	Elgin and Nairn, Inverness, Ross, and Sutherland.	Kirkcudbright, Wigton.	Scoti: Argyle, Bute.
Fin,	...	...	...	...	...	...	...	...	14	6	4	...	3	...	...
Glen,	35	W	5	...	17	42	44	...	56	...	23	52	54	61	52
Gar,	...	...	...	...	...	...	34	...	...	...	...	17	...	23	...
Garth,	...	W	...	...	...	...	10	23	13	...	...	...	...	...	10
Inch,	90	...	...	...	...	...	...	18	30	25	10	17	31	...	...
Iron,	...	...	...	...	...	...	...	...	...	...	...	...	...	15	...
Inver,	6	...	...	2	...	...	...	5	32	10	16	37	69	...	24
Kin,	30	...	...	3	...	...	...	6	43	34	52	88	57	...	7
Knock,	29	...	...	...	...	5	64	...	6	...	...	32	30	37	31
Larg,	...	...	...	...	...	...	...	...	...	...	...	...	...	13	...
Lin,	...	W	...	...	...	...	...	...	...	5	8	...	...	...	...
Lan,	3	W	...	...	...	...	...	...	...	6	...	...	...	...	6
Lath,	...	...	...	...	...	...	...	...	...	9	...	...	...	...	...
Loch,	100	...	...	7	...	14	34	16	30	18	...	15	19	26	...
Locher,	...	...	...	...	...	5	...	...	...	...	...	...	...	...	...
Led,	...	...	...	...	...	...	...	6	6	...	...	...	...	...	...
Mon,	14	...	...	...	...	7	...	...	11	13	13	31	...	...	...
Mul,	15	...	...	...	...	...	...	...	...	...	...	...	8	...	...
Pen,	...	W	9	3	...	5	7	...	...	...	...	...	...	2	...
Penny,	...	...	...	...	...	...	7	...	...	...	...	...	...	...	11
Pet,	...	...	...	...	...	...	...	...	...	...	...	30	5	...	...
Pit,	...	...	...	...	...	...	...	...	75	52	38	69	30	...	...
Pitten,	...	...	...	...	...	...	...	...	7	9	...	...	...	...	...
Pol,	...	...	...	...	...	13	7	6	9	...	...	...	17	9	...
Port,	22	...	...	...	...	...	...	3	...	...	...	...	...	8	...
Ra,	63	...	...	...	...	...	...	...	17	6	...	...	...	...	...
Strath,	...	...	...	...	...	...	...	...	19	13	...	27	35	...	13
Stron,	...	...	...	...	...	...	...	...	...	...	...	...	...	...	17
Stuck,	...	...	...	...	...	...	...	8	...	...	...	...	...	...	6
Tar,	...	...	...	...	...	...	...	...	...	...	...	14	...	...	...
Tra,	...	...	...	...	...	...	...	...	...	...	...	...	...	5	...
Tom,	...	...	...	...	...	...	...	11	...	...	...	...	...	...	...
Tor,	...	...	...	11	...	11	...	...	...	9	...	21	22	10	19
Tullie,	...	...	...	...	...	...	...	...	...	...	...	38	...	...	...
Tulli,	...	...	...	...	...	...	...	7	25	...	11	42	7	...	...
Tulloch,	17	...	...	...	...	...	...	5	...	...	...	10	...	...	...

Fig. 1.

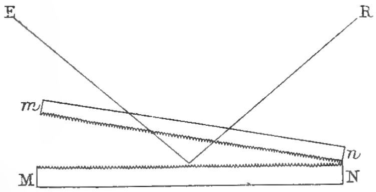


Fig. 2

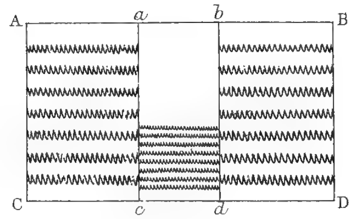


Fig. 3

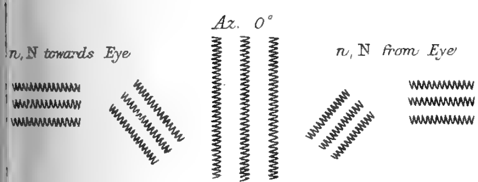


Fig. 4

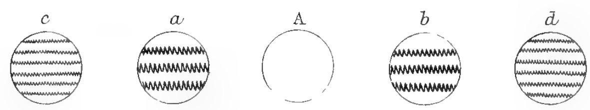


Fig. 5

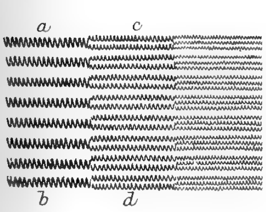
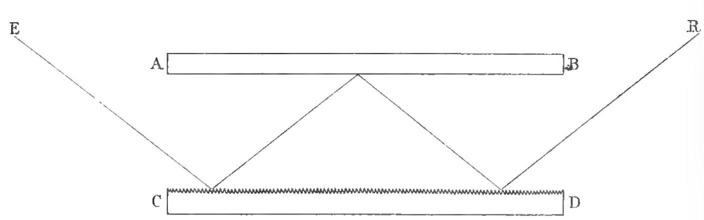


Fig. 6



XVIII.—*On the Bands formed by the Superposition of Paragenic Spectra produced by the Grooved Surfaces of Glass and Steel.** PART I. By Sir DAVID BREWSTER, K.H., F.R.S. Lond. and Edin. (Plate XXII.)

(Read 7th March 1864.)

In examining the colours produced by thin laminæ of the crystalline lens of fishes, I observed a series of rectilineal serrated fringes perpendicular to the direction of the fibres, and produced by inclining the laminæ in a plane cutting these fibres at right angles. I was thus led to imitate these fringes or bands by combining grooves or striæ cut upon glass or steel surfaces, or grooves taken from these surfaces upon isinglass or gums.

In my first experiments I combined a system of grooves on glass, executed for me by Mr DOLLOND, with a similar system on steel executed by Sir JOHN BARTON, both of them containing 2000 divisions in an inch. The plate of glass was placed above the plate of steel, and slightly inclined to it, as shown in Plate XXII. figs. 1 and 2. The glass plate ABCD, fig. 2, was covered with grooves, but the steel plate below it was grooved only on the shaded portion *abcd*, the parts A*a*C*c*, B*bd*D being polished so as to reflect to the eye at E (fig. 1), the grooves on the glass when illuminated by rays, R*r*, proceeding from the first pair of the paragenic spectra produced by the grooves.

When the direction of the grooves *ac* is nearly parallel to the plane of reflexion, and to one another, a series of minute serrated bands is seen on the space *abcd*, where the light has been transmitted twice through the grooves on glass, and reflected once from those on steel; but no bands are seen upon A*ac*C, B*bd*D, where the steel was only polished.

When the grooves were slightly inclined to the plane of reflexion, large serrated bands appeared upon the spaces A*ac*C, B*bd*D, and when this inclination was increased, these large bands became smaller and more numerous, crowding towards C*c* and *d*D. On the other hand, they become larger and larger as the direction of the grooves returned into the plane of reflexion. In the azimuth of 0° they become straight, and by increasing the azimuth, they pass, as it were, to the right hand, as shown in fig. 3.

When the direction of the grooves is inclined to the plane of reflexion, the minute serrated bands upon *abcd* become smaller and less serrated.

* In a very interesting paper on the Spectra produced by Gratings or Grooved Surfaces, M. BABINET has given them the appropriate name of *Paragenic*, in order to distinguish the Spectra produced by refraction from those produced by the lateral propagation of light. "*Sur la Paragenie ou propagation laterale de la lumiere.*" Paris, 1864. Extrait du COSMOS.

When the inclination *mn*NM of the grooved plates is increased, the large bands become smaller and smaller, and when it is diminished, they become larger and larger, getting inclined as in fig. 3, and becoming parallel at 0° of inclination.

Having been provided, by the kindness of Sir John Barton, with two grooved plates of glass containing 500 divisions in an inch, I was enabled to examine the fringes on the paragenic spectra under different circumstances.

When the grooved surfaces of the plates were placed in contact, and the grooves formed a small angle with one another, the middle or principal image, A (fig. 4), when observed with a lens whose anterior focus coincided with the grooves, had no bands, but the paragenic spectra *a*, *c*, *b*, *d*, on each side had numerous serrated bands or fringes perpendicular to the direction of the grooves, the number on the first spectra *a*, *b*, being at the rate of 19 in an inch of the luminous disc, and increasing in arithmetical progression.

When the luminous object is rectangular, and the rectangular paragenic spectra are brought nearly into contact, as at *ab* and *cd* (fig. 5), the bands, as seen at nearly a perpendicular incidence, are shown in this figure.

When the incident light is inclined to the direction of the grooves, the bands suffer no change, and appear immoveable on the surface of the glass plates.

When the ray of light is perpendicular to the direction of the grooves, and the surface of the glass on which they are cut is inclined to the ray of light, the bands all descend from *a* to *b* (fig. 5), moving off, as it were, at *b*, and *d*, and succeeded by others when the angle of incidence increases, while they ascend from *b* to *a*, and from *d* to *c*; moving off at *a* and *c*, when the angle of incidence diminishes. In this case, the grooves of the plate next the eye are turned to the left, the opposite motions taking place when they are turned to the right.*

The bands correspond to the intersection of the one set of grooves with the other set, and consequently they diminish in number, and recede from one another when the inclination of the one set of grooves to the other diminishes, becoming parallel to the grooves when the grooves on both plates are parallel.

Interference bands, parallel to the grooves, may be seen by transmitted light upon the paragenic spectra, when two systems of grooves are placed parallel to each other, and when the grooves in the one system are parallel to those in the other. They are seen both at a perpendicular incidence and when the plates are inclined in a plane parallel to the grooves.

These bands become narrow as the distance of the two grooved surfaces is increased, and they are seen at all angles of incidence, and in all planes of reflexion from the grooved surfaces.

I have observed those bands, which are generally more or less serrated, in com-

* This motion of the bands is not seen when the grooved surfaces are perfectly parallel.

binations of 1000 with 1000, 1000 with 2000, 1000 with 500, 2000 with 500, and in the combination of four surfaces of 2000, 1000, 100, and 500.

In the combination of 1000 and 500, and in no other, a very peculiar system of bands is seen with a lens. They are not serrated, and not perpendicular to the grooves. The system consists of two sets equally inclined to the direction of the grooves, when the grooves in one plate are slightly inclined to those in the other. By diminishing the inclination of the grooves, the inclination of the bands to the direction of the grooves diminishes, and when the grooves become parallel, the bands become parallel and disappear.

These bands must have a different origin from those previously described, as they are similar in number upon all the prismatic images.

In these experiments the duplication of the bands on the second spectrum, and their increase in arithmetical progression on the other spectra, is a remarkable fact which it is difficult to explain. The second spectrum differs only from the first, and the third from the second, only in their length; and we can hardly suppose that they have a property in a direction perpendicular to their length, or to FRAUNHOFER's lines, which would increase the number of their bands.

The bands which we have described are more distinct when the spectra are pure or formed from a narrow line or bar of light; but when we wish to see the bands on the bar of light or the central image O (fig. 4), the spectra must be formed from wide spaces which gave impure spectra.

In order to examine the interference bands under different conditions, I placed (as in fig. 6) a plate of polished steel at different distances from another plate of steel, containing six systems of grooves executed by Sir JOHN BARTON, varying from 312·5 divisions in an inch to 10,000. When the light was reflected twice from the grooved surface and once from the plain steel surface, the bands which covered the colourless image and the paragenic spectra were splendid beyond description, and unlike anything of the kind that I had previously seen.

1. The bands were parallel to the grooves, or to the lines in the spectra.

2. They are smaller and more numerous when the grooves are wider or fewer in an inch.

3. They become smaller and more numerous when the distance of the plates is increased.

4. They are smaller and more numerous when the angle of incidence is increased.

5. They become more numerous by increasing the number of reflexions.

6. They appear like minute black lines upon the colourless image, but when their magnitude is increased, they appear like blue or pink bands on a ground of a different colour, which is generally white or whitish blue.

These bands were visible on the systems of grooves, 312·5, 625, 1250, and 2500 in an inch, but not on the systems of 5000 or 10,000 in an inch.

When the spectra had suffered three, four, five, and six reflexions, the central and other images were covered with the same number of bands, as with two reflexions from the grooved steel; but another series of wider bands was superposed.

The following results were obtained with grooved surfaces having 1250 divisions in an inch :—

Distance of plates,	0·11 inch.	Angular breadth of each,	7° 50′
Distance of circular disc,	115·5 inches.	Distance of plates,	0·22 inch.
Diameter of disc,	1·317 inch.	Angle of incidence,	63° 30′
Angle of incidence,	63° 30′	Number of fringes on the disc, and on the first spectrum,	10
Angular diameter of disc,	39° 30′	Angular breadth of each,	3° 55′
Number of fringes on disc, and on the first spectrum,	5		

In order to observe the effect produced by varying the angle of incidence, I placed a luminous disc three inches and six-tenths in diameter* at the distance of nine feet six inches from the grating, and obtained the following results :—

Angle of Incidence.	No. of Bands on the Disc.	Angle of Incidence.	No. of Bands on the Disc.
70	29	50	17
60	21	40	14

The bands were seen at an incidence of $87\frac{1}{4}°$, when the plates were nearly in contact.

The following were the colours seen on the two spectra on one side of the colourless image; but I have not measured the precise angle of incidence at which they were seen, nor mentioned in my journal whether they were seen with the 625 or the 1250 grating :—

	First Spectrum.		Second Spectrum.
Great Incidences	White.	Great Incidences	Blue.
	Pale Red.		Bluish.
	Red.		Less Blue.
	Purple.		Bluish White.
	Blue.		White.
	Bluish.		Pale Red.
	Less Blue.		Red.
Lesser Incidences	White.	Lesser Incidences	Purple. Blue.

At small angles of incidence, about 42°, the bands become less distinct, and paler in colour, the white becoming yellow and the blue brownish.

In the systems of grooves, whether on glass or on steel, employed in the preceding experiments, the part of the original surface not removed by the grooves bears a very considerable proportion to the part removed; but when the grooves occupy a large part of the surface, and the intermediate parts a very small one, a new set of phenomena are produced, which must change in a remarkable manner all the bands of interference. The execution, however, of such systems

* This disc included part of the spectrum on each side of the bright image.

of grooves is very difficult. Sir JOHN BARTON, with all his experience, failed in producing good specimens ; but even with those which he executed for me, phenomena of a remarkable kind were exhibited, not only on the middle or colourless image, but upon all the paragenic spectra, varying with the number of grooves, but still more remarkably with the angle of incidence.*

P.S.—The preceding experiments were made in 1823 and 1827, and those described in p. 223, were repeated in 1838. Having lost or mislaid the glass gratings which I then employed, I am not able to compare the bands which they produced with a more remarkable series which I have recently obtained with new gratings, and which will be the subject of another communication.

* See Phil. Trans. 1829, p. 301.

Fig. 1

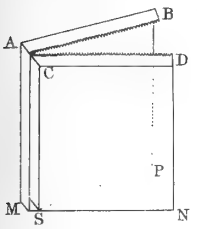


Fig. 2

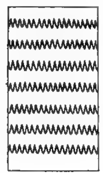


Fig. 3

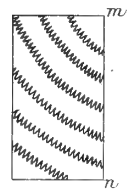


Fig. 4

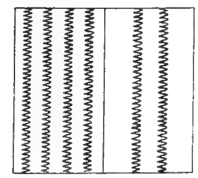


Fig. 5.

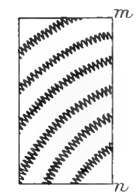


Fig. 6

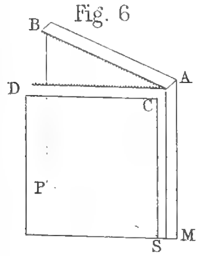


Fig. 8

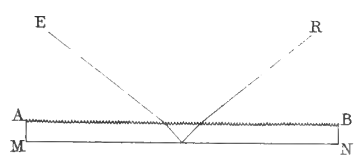


Fig. 7

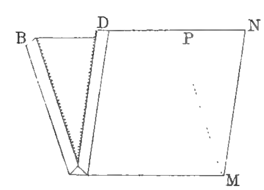


Fig. 9

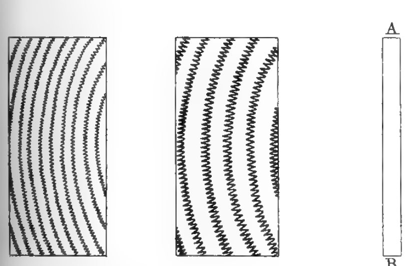


Fig. 10

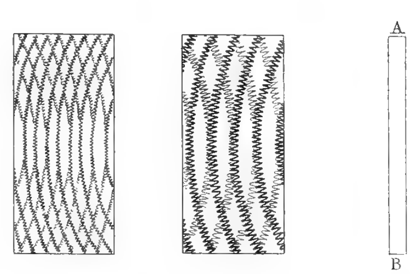


Fig. 11

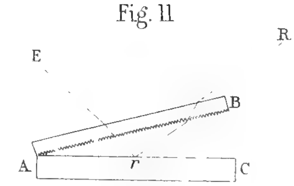


Fig. 12

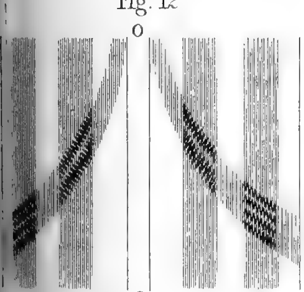


Fig. 13

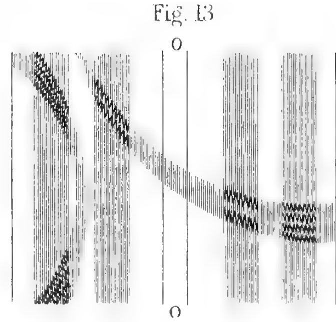


Fig. 14

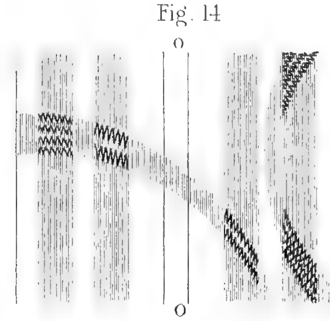


Fig. 16

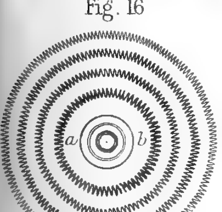


Fig. 17

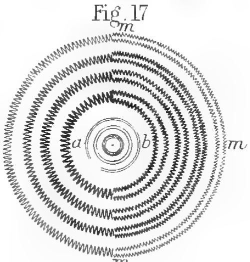


Fig. 18

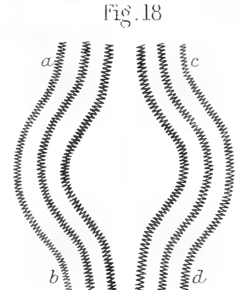


Fig. 19

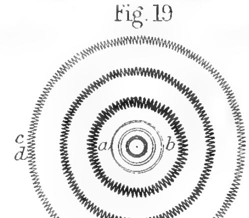


Fig. 21

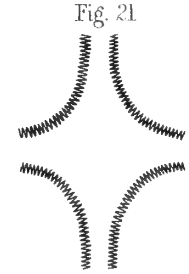


Fig. 15

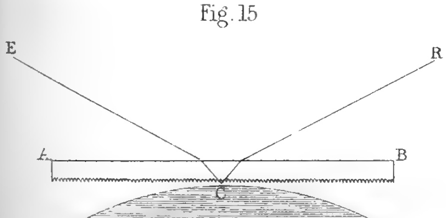
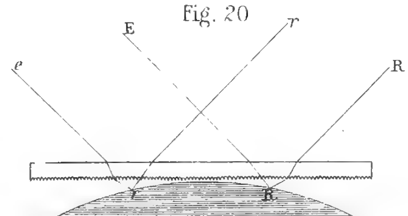


Fig. 20



XIX.—*On the Bands formed by the Superposition of Paragenic Spectra produced by the Grooved Surfaces of Glass and Steel.* Part II. By Sir DAVID BREWSTER, K.H., F.R.S. Lond. and Edin. (Plate XXIII.)

(Read 17th April 1865.)

In the preceding paper I have described the bands produced by gratings or grooved surfaces with 500 divisions in an inch, when the two grooved surfaces are in contact, and the grooves in the one slightly inclined to those in the other.

The following results were obtained with two gratings, one of which had 2000 and the other 1000 divisions in an inch.

1. When the surfaces are in perfect contact, and the grooves parallel, very irregular bands are seen on the united surfaces, either with a lens or by ordinary vision, and are parallel to the grooves. They are seen only on the 2d, 4th, 6th, &c., spectra on each side of the luminous bar or disc.

By turning the nearest grating slightly to the right from the azimuth 0°, the bands fall back to the left, increasing in number, and descending with their concave sides downwards into distinct serrated black and white bands, nearly perpendicular to the grooves. When the nearest grating is turned to the left, the bands descend towards the right, with their concave sides upwards, till they become nearly perpendicular to the grooves. In all these positions, the bands are twice as numerous on the fourth spectrum as on the second, and thrice as numerous on the sixth as on the second; and when the grooved surfaces are perfectly parallel, the bands are immoveable on the grooved surfaces at all angles of incidence.

2. When the grooved surfaces are separated by the thickness of one or both of the plates of glass, the bands are very indistinctly seen, and they seem to diminish in size with the distance of the grooved surfaces; but this is not certain, owing to the difficulty of fixing the plates with the grooves at the same inclination to each other.

Similar bands were seen on the united surfaces of gratings of 2000 and 2000, 1000 and 1000, 500 and 500, 1000 and 500, and 2000 and 500 divisions in an inch, but always less distinctly when the grooved surfaces are separated by the thickness of one or both of the plates.

The beauty and distinctness of these bands depend upon the skill with which the gratings are ruled. In several of the gratings which I possess, the phenomena I have described can hardly be recognised.

When the combined gratings have the same number of divisions, such as 1000 and 1000, the bands are seen upon all the spectra, and sometimes very faintly on the luminous disc, but when one of the gratings has twice the number of divisions as the other, such as 2000 and 1000, the bands appear as already mentioned, only on the 2d, 4th, 6th, &c., spectra. In such combinations, the 1st, 3d, 5th, 7th, &c., spectra of the 1000 grating have no corresponding spectra in the 2000 grating, with which they can interfere, whereas, when the divisions in both are the same, all the spectra of the one are superposed upon all the spectra of the other and, therefore, bands are produced upon each of them.

In like manner, if the number of divisions in the one grating is to those on the other as n to 1, n being a whole number, the bands will appear only on the spectra n, $2n$, $3n$, $4n$, &c.

When a grating of 1000 is placed above one of 2000, I have observed faint bands upon the spectra, 1, 3, 5, &c., of the 1000 grating, though none of the spectra of the 2000 grating could interfere with them. These bands are more numerous than those between which they lie, and are doubtless produced by the interference of spectra reflected from the plane surfaces of the glass plates with those seen by transmitted light.

When the gratings of 1000 and 2000 are placed at a small angle, as in Plate XXIII., fig. 1, the grooves being parallel to AM, and the light incident perpendicularly, the bands on the left-hand spectra are parallel and rectilineal, and highly *purple* and *green*, as in fig. 2.

By turning the gratings round AM as an axis, in the direction from D to B, the bands descend from m, as in fig. 3, till they become parallel vertical lines, increasing in number and less coloured, as in fig. 4, the number of bands on the second left-hand spectrum being double those on the first.

When the rotation is in the opposite direction from B to D, fig. 1, the bands rise from n, fig. 5, till they become parallel and vertical as before.

The opposite effects take place when the gratings are placed as in fig. 6, AM and CS being coincident, and when we observe the spectra on the right hand of the luminous disc. The bands now descend and ascend from the same points m, n, now on the outer side of the spectra.

When the two edges, AM, CS of the gratings are not parallel, as they are in fig. 1, but inclined at a small angle, AMSC, fig. 6, then if, when the fringes are parallel at a perpendicular incidence, we turn the gratings round AM as an axis from B to D, the fringes descend from m, becoming smaller and smaller, till they are parallel and vertical, but when the gratings revolve from D to B, the fringes become larger and larger, less numerous, and more coloured, till they are finally parallel to AM, the fringes being twice as numerous on the second spectrum as on the first.

When the grooves are perpendicular to AM, as in fig. 7, the bands are faint and indistinct. The light being incident perpendicularly, and the gratings turned round AM on a plane perpendicular to AM, the fringes do not increase in number or greatly change, if the motion is accurately in a plane perpendicular to AM.

When the gratings are turned in the plane of the horizon passing through AM, the side NM approaching the eye, the fringes on the left-hand spectra descend, increasing rapidly in number, and when the side MN recedes from the eye, the fringes ascend, increasing in magnitude and diminishing in number, and are highly coloured. At a certain angle, they become parallel to the grooves, when by continuing the rotation they move downwards increasing in number, and becoming parallel to the grooves.

In the preceding experiments, the bands are seen on the surface of the gratings, but when the grooved surfaces are in contact, and the grooves parallel, bands of an entirely different kind are seen, not on the surface of the gratings, but by rays diverging from the luminous disc. If we use a long and narrow bar of light, such as the opening between the window-shutters, then, when the grooves are parallel to the bar, and the grooved surfaces perpendicular to the plane of incidence, the bands are parallel to the bar and its spectra. By inclining the grooves to the luminous bar, the bands are inclined to the spectra, dividing each of them into a great number of spectra, and at an azimuth of 45° the bands become perpendicular to the spectra. At all these inclinations the bands on the second spectrum are double those on the first, the number increasing in arithmetical progression on succeeding spectra.

When the angle of incidence is increased, the bands increase in number, but very slightly with gratings of 1250 divisions in an inch.

By increasing the distance between the gratings, the bands also increase in number.

Bands similar to those now described are produced with interesting phenomena by a single grating placed as in fig. 8, so that the image of the grooved surface AB, reflected from MN, the lower surface of the glass is superposed as it were upon the grooved surface itself.

1. When the plane of reflexion is perpendicular to the grooved surface, and the grooves in the same plane, the bands on the spectra are parallel to the bar of light AB, those on the second spectrum being double those on the first. They are seen at all angles of incidence, and are larger and more distinct at small angles.

When the grating is turned round in its own plane, at any angle of incidence, so that the grooves form different angles with the bar of light, the bands cross the spectra and become perpendicular to them in the azimuth of 45°. The paragenic spectra are thus divided into a great number of spectra, the number increasing as formerly on each succeeding spectrum.

2. When the grooves are parallel to the bar of light, and the plane of reflexion perpendicular to the grooves, the bands are apparently segments of concentric

circles at great angles of incidence, the radius of which increases as the angle of incidence diminishes, so that they become straight lines at a perpendicular incidence. The bands are smaller at their upper and lower ends, and those on the second spectrum are, as before, double those on the first, as shown in fig. 9.

In the spectra on the left hand of the bar of light, the concave side of the circular bands is towards the bar; and in the spectra on the right hand of the bar of light the convex side of the circular bands is toward the bar. The bands on the right-hand spectra are smaller and more numerous than those on the left-hand spectra; and yet, by increasing the angle of incidence, the bands on all the spectra increase in size and diminish in number.

If at any particular incidence we turn the grating in its own plane, the bands cross the spectra at angles increasing with the degree of rotation, and becoming smaller and more numerous. When the end of the grating nearest the eye (A, fig. 8) ascends, the fringes, great and small, diminish and become more distinct, and the centres of the circles descend. When the grating is turned in the opposite direction, the centres of the circles ascend.

In the principal gratings which I possess, when upon thin glass,* including those of 1000 and 2000 in an inch, these circular bands are accompanied by another system of circular bands, convex to the luminous bar when seen on the left-hand spectra, and concave to it when seen on the right-hand spectra; but, what is remarkable, they are smaller and more numerous on the first spectrum than on the second, as shown in fig. 10. They are best seen when the principal circular bands cross the spectra obliquely.

In the preceding experiments with one grating, the grooves of the reflected image are necessarily parallel to those of the real grating, owing to the parallelism of the surfaces of the plate of glass, and therefore they cannot exhibit the result of superposing two systems of grooves inclined to each other. This condition, however, may be obtained by drawing the grooves on the faces of a prism with a small angle, or by placing a fluid prism between an ordinary grating and a plate of thin parallel glass, which would enable us to vary the inclination of the two sets of grooves. A better arrangement, however, is to place the grating AB (fig. 11) upon a polished metallic surface, MN. A ray from the luminous bar at R, incident on AC at r, reaches the eye at E, after reflexion from the steel surface MN, so that the reflected image of the grating, AB, is superposed as it were on the direct image.

When the grating, AB, of 1000 grooves in an inch is laid upon a steel surface, MN, and the grooves are in the plane of incidence, the paragenic spectra of a luminous bar are covered with bands, not serrated, parallel to the spectra, exhibiting all the phenomena already described as seen by reflexion from a single grating.

* These bands are not seen on a beautiful Munich grating, kindly lent me by Professor STOKES, having 3750 divisions in an inch. As the bands become smaller with the thickness of the glass, their absence in this grating arises doubtless from its great thickness, which is 0·158 of an inch, the thickness of the gratings upon which they appear being about 0·04.

The bands are of the same size as with a single grating when the grooved surface is uppermost, but they are very much larger when the grooved surface is in contact with the steel.

When the grooved surface is slightly inclined to the steel surface, as in fig. 11, and the grooves parallel to the plane of reflexion, a double system of hyperbolic bands is seen, as in fig. 12, having one asymptote coincident with the bar of light and the other at right angles to it. One of the systems of hyperbolas is on one side of the bar and the other system on the other side, the number of bands on the second spectrum being double those on the first.

When the grooves are inclined to the plane of reflexion by turning them to the left or to the right, the double system of hyperbolas moves to the left or to the right, the curves of each system crossing the spectra, as in fig. 13, and being, as before, twice as numerous on the second as on the first spectrum on both sides of the bar. By increasing the inclination of the grooves to the plane of incidence, the system of hyperbolas moves farther to the left or to the right.

When the bar of light is placed at E and the eye at R, fig. 11, the system of hyperbolas is inverted, as in fig. 14.

It is curious to observe the passage of the parallel rectilineal bands into hyperbolas, when the inclination of the grooved to the steel surface commences. The parallel bands open at their lower end, as in fig. 13, or at their upper end, as in fig. 14, and change into hyperbolas. When the light was strong, I observed a second but fainter system of hyperbolas lying between the principal system and the luminous bar, and caused probably by reflexion from the second surface of the grating. The effect produced by the crossing of the bands arising from these two systems of hyperbolas was remarkable, and similar to what I had observed in combining two gratings of 500 divisions in an inch. This second system of hyperbolas was most distinct when the plane of reflexion from the surface of the steel was coincident with the plane of reflexion from the glass; and the double system was seen with grooved surfaces of 500, 1000, and 2000 divisions in an inch.

In using accidentally a steel surface that was not perfectly flat, I was surprised to observe that the bands were not hyperbolas, but circular rings varying in form and size with the angle which the grooves formed with the plane of reflexion. In order to examine this new and beautiful phenomenon, I placed the grooved surface of the grating, AB, upon a convex surface of steel, MN, as in fig. 15, so that the rays from the luminous body might reach the eye at E, after reflexion from the convex surface, MN. The reflected image of the grating is thus superposed upon the direct image, and two systems of concentric rings are seen upon the surface of the grating. At the point of contact, C, and around it, are seen the rings of thin plates described by NEWTON, and increasing in size with the radius of the surface MN. Around and concentric with these as shown at *ab*, fig. 16, is seen a beautiful system of serrated rings formed upon the paragenic spectra, as in fig. 16, the number of rings upon the second spectrum being double those on the first, as before, and becoming narrower and closer as they recede from the centre. When the first and second spectra are close to one another, as in fig. 17, the rings upon entering the second spectrum are doubled, as shown at *mmm*. These rings are seen only when the grooves are inclined to the plane of reflexion. By increasing the inclination, they become smaller and more distinct, their size being a minimum, and their distinctness a maximum, when the azimuth of the grooves is 90°. When the azimuth is 0°, or when the grooves are turned into the plane of reflexion, the rings open, as at fig. 18, and when turned into azimuth 1° or 2°, those on the side *ab*, fig. 18, go back to the left, and those on the side *cd* bend into a ring, as shown in fig. 19. When the rings are again formed, they increase as the angle of incidence diminishes.

When the rings are increasing or diminishing, or passing from one spectrum to another, their centres are sometimes white, and at other times so black as to eclipse the rings of NEWTON. Their colour is very variable, sometimes black, with colourless intervals, and sometimes richly coloured with the tints of the spectra on which they are seen. When the grating is pressed upon the convex surface, or raised slightly from it, the rings exhibit the same phenomena as those of thin plates.

When the ray RR′ (fig. 19) from the bar of light, reaches the eye at E, the grooves being slightly inclined to the plane of reflexion, the hyperbolic bands are seen, as in fig. 12, and when the ray *rr′* reaches the eye at *e*, the hyperbolic bands are seen as shown in fig. 13, and when the eye receives all the rays between R′ and *r′*, the direct and inverted systems of hyperbolas are seen, as in fig. 20. If, when these are seen, we look at the surface of the grating, we shall see the system of concentric rings produced by the union of the two systems of hyperbolas.

XX.—*On the Influence of the Doubly Refracting Force of Calcareous Spar on the Polarisation, the Intensity, and the Colour of the Light which it Reflects.* By Sir David Brewster, K.H., F.R.S.

(Read 15th February 1864.)

It was the opinion of Malus, and adopted by Arago, Biot, and other philosophers, that the surfaces of regularly crystallised bodies acted upon light in the very same manner as the surfaces of ordinary bodies, whether solid or fluid; or, in other words, that the reflecting forces extended beyond the limits of the forces that produced double refraction and polarisation. Having been led to question this opinion, I undertook an extensive series of experiments on crystalline reflexion, as exhibited in calcareous spar, a crystal peculiarly fitted for this purpose, from its perfect transparency and great double refraction; and I published the results of these experiments in the Philosophical Transactions for 1819.

In these experiments, the peculiar action of crystalline surfaces which I had expected was placed beyond a doubt. The angle of complete polarisation on the surface of the primitive rhomb was found to vary with the inclination of the plane of reflexion to the principal section of the crystal; and with different surfaces the variation of that angle depended on the inclination of the surface to the axis of the rhomb.

As the doubly refracting force thus modified the polarising angle produced by superficial reflexion, it became probable that the polarised ray might suffer some change from the same cause; but, after the most careful observation, I could not discover the slightest indication of such an effect. Conceiving, however, that the change which I expected might be masked by the powerful action of the ordinary reflecting force, I thought of reducing it till it was overpowered by the doubly refracting force. With this view, I introduced a film of oil of cassia between the larger surface of a rectangular prism of plate glass and the surface of the spar, and by inclining the prism at a small angle, as in the Lithoscope, I was able to separate the image of the sun, or any other light formed by the common surface of the prism and the oil, from the image formed by the common surface of the spar and the oil, and to examine the properties of the last of these images.

In this way I found that the ordinary reflecting force of the spar was nearly reduced to nothing, and was almost entirely under the dominion of the force which emanated from the crystal. Light incident on the crystalline surface was no longer polarised in the plane of reflexion, but in planes inclined to the principal section of the crystal, the rotation or deviation of the plane increasing with the angle which the plane of reflexion formed with the principal section, and was so related to the angle which the incident ray formed with the axis of the crystal, that the Sine of half the rotation, or deviation, was equal to the square root of the Sine of the incident ray to the axis.

The bearing of these results, as published in the memoir already referred to, upon the theory of Light, directed the attention of mathematicians to this subject, and I was thus induced to resume the inquiry, by investigating the action of surfaces variously inclined to the axis of calcareous spar,—to study the effect of fluids of different refractive powers, in reducing the action of the reflecting force, and to ascertain the influence of the surfaces thus modified upon light polarised in planes differently inclined to the principal section of the crystal.

Some of the results thus obtained were communicated at different times to the British Association, and were found by the late Professor MACCULLAGH of Trinity College, Dublin, to be deducible from the Undulatory Theory; but other results, in which the phenomena were asymetrical with respect to the principal section of the crystal were less accordant with theory. Professor MACCULLAGH was therefore desirous to observe the phenomena himself; and having resolved to have an apparatus constructed more complex and perfect than the one I used, I willingly left the subject in the hands of my distinguished friend. What experiments he made, or whether he made any, before the sad and sudden close of his life in 1847, I have not learned. The subject has therefore again come into my hands; and having been encouraged by Professor STOKES of Cambridge to publish my experiments, as having an important bearing on the theory of light, I now submit them, incomplete and imperfect as they are, to the consideration of the Society.

The experiments which I published in 1819 were made on the cleavage planes of the primitive rhomb of calcareous spar, but those which I am about to describe were made on artificial faces, carefully prepared for me by the late Mr WILLIAM NICOL, the ingenious inventor of the polarising prism which bears his name.

I could have wished to repeat some of these experiments with a better apparatus, and with freshly polished surfaces of calcareous spar, but the sharp vision and the sensitive retina of early or middle life are necessary for the observation of delicate and almost evanescent phenomena.

In the following Table I have given the observed polarising angles of the surfaces employed, and of their inclination to the axis of the rhomb:—

	Inclination to Axis.			Observed Polarising Angle.*
A	0°	...	...	54° 3′
A1	4 40′	...	...	54 16
D	22 30	...	...	55 22
B	45 23	...	...	57 12
B1	50 51	...	...	...
B2	57 31	...	...	...
E	67 30	...	...	59 16
E1	84 30	...	...	59 38
C	90 0	...	...	59 59

B. Surface of Rhomb, Inclination to Axis 45° 23′.

1. *With Oil of Cassia, on an Artificial Face.*

Azimuth 90°. Obtuse angle to the right.

Light polarised 0° and 90°. E′ and O′† vanish simultaneously $42\frac{3}{4}$° to the left of the plane of incidence, E′ being *reddish* and O′ *yellowish.*

Light polarised + 45° and — 45°. E′ is polarised $45\frac{1}{2}$° to the left. O′ vanishes, but a little *blue* light is left, which disappears along with E and O. O′ vanished more completely by turning the rhomb that gave the images E, O, 9° or 10° farther, from 45° to 55°.

Azimuth 270°. Obtuse angle to left.

Light polarised 0° and 90°. E′ and O′ vanish simultaneously $33\frac{1}{2}$° to the right of the plane of incidence. A little *blue* light remains in E′ and O′ at the point of evanescence.

Light polarised + 45° — 45° O′ is polarised 35° to the right. E′ vanished completely.

2. *With Oil of Cassia, on a Natural Face of Cleavage.*

Azimuth 90°. Light polarised 0° and 90°. E′ and O′ vanish simultaneously $47\frac{1}{2}$° to the left.

Light polarised + 45° — 45°. E′ is polarised 46° to the left. O′ is scarcely visible.

Azimuth 270°. Light polarised 0° and 90°. E′ and O′ vanish simultaneously $42\frac{1}{2}$° to the right.

Light polarised + 45° — 45°. O′ is polarised $44\frac{1}{2}$° to the right. E′ is nearly invisible.

Common light is polarised 44° to the right.

N.B.—The evanescence is not complete either on the glass or on the spar surface; but more complete on the natural than on the artificial face of the spar.

Azimuth 38°. Obtuse angle to right. In common sun's light, E′ and O′ vanish simultaneously at 45° of incidence, and in a plane $82\frac{1}{2}$° to the right of the plane

* In the plane of the principal section. See Phil. Trans., 1819, p. 158.

† E′ and O′ are the extraordinary and ordinary images from the spar, and E and O the same from the prism surface.

of incidence. The vanishing image is at its *minimum* when crossed half with *blue* and half with *red* light.

Light polarised $+45°-45°$. E′ and O′ polarised simultaneously 82°½ to the plane of incidence. Both have the same intensity, and are crossed at their minimum with *red* and *blue* light.

Light polarised 0° and 90°. Although E does not suffer reflexion from the glass surface, yet E′ is visible, and vanishes, along with O′, 82½° to the right of the plane of incidence.

Azimuth 218°, Obtuse angle to the Left. Common sun's light is completely polarised 8° to the right of the plane of incidence. The evanescence is complete at the polarising angle of O, but not at greater angles.

Light polarised 0° and 90°. E′ and O′ are both polarised 8° to the right. E′ is bright *yellow* and O′ bright *pink*.

Light polarised $+46°-45°$. E′ and O′ are polarised 8° to the right, E′ being *yellow* and O′ *blue*.

Azimuth 15° to the left. Light polarised 0° and 90°. E′ and O′ vanish together, and O′ is polarised about 40° to the right.

Light polarised $+45°-45°$. Obtuse angle from the eye E′ and O′ are polarised 55° to the right.

Azimuth 45°. Light polarised $+45°-45°$. E′ and O′ vanish completely at polarising angle 92° to the right. The deviation is increased by increasing the incidence.

On the Intensity of the Reflected Pencil.

Common Light, Azimuth 0° and 180°. The spar and oil image S is equal to about ⅓ of the prism image P.

Azimuth 45° and 215°. $S=\frac{2}{3}$ P.

Azimuth 90° and 270°. S = P, P a little brighter.

Light polarised $+45°-45°$.

Azimuth 0° and 180°. O′ gradually diminishes and vanishes at 90°, while E′ increases and is a maximum at 9°.

Azimuth 90° and 270°. E′ is a maximum and nearly equal to O′, which almost vanishes.

Beyond 90° and 270°, O′ gradually increases while E′ diminishes, and they become equal at 180°.

Light polarised 0° and 90°. Azimuth 0° and 180°. O′ almost vanishes, but E″ is bright, though only equal to ⅓ P. O′ increases gradually to azimuth 90°, where it is equal to O.

Azimuth 90° and 270°. E′ = O′, both pretty bright, but less so than P.

Azimuth 123°. E′ vanishes. O′ a little less than P.

Azimuth 180°. O′ vanishes. $E'=\frac{1}{3}$ P.

B *With Oil of Anise Seeds.*

AZIMUTH 0° and 180°. E′ and O′ vanish together, E′ at all angles less than 45°. O and a great part of E, viz. the *blue* light and more is polarised at angles above 45°, a small portion of *red* light remaining. O = E, O *bluish* and E *yellowish*.

AZIMUTH 90° and 270°. Change of polarisation 94° to left. Maximum polarising angle for E′ at angles much less than 45°, polarisation nearly complete. No appearance of polarisation at great incidences. O′ = E′.

AZIMUTH 39° and 321°. Change of polarisation 50° to left. The maximum polarising angles commence at the angle of polarisation for O, and the polarisation is complete at angles both above and below 45°.

AZIMUTH 219°. Change of polarisation 0°, O′ and E′ vanishing at the same time at about 45°. Above 45°, E′ is nearly wholly polarised 15° to the right, while below 45°, it is polarised 0°, 5°, 10°, &c. to the left as the incidence diminishes.

With sunlight complete polarisation took place much lower than 45°, and *blue* light was less in this light.

B. *With Oil of Sassafras.*

AZIMUTH 0° and 180°. No change in the plane of polarisation. O′ is bluish and = 4 to 5 E′.

AZIMUTH 39°. Change of polarisation about 30°.

AZIMUTH 219°. Change of polarisation 0°.

AZIMUTH about 60°. Change of polarisation 45°, the polarisation increasing at great incidences.

AZIMUTH about 70°. Change of polarisation about 50°. Maximum polarisation about 48° of incidence.

AZIMUTH 90°. No light seems to be polarised at or above 45°.

AZIMUTH about 240°. Change of polarisation from 15° at great incidences to about 25° at small incidences. Light polarised in plane of reflexion is treated nearly as common light, but light polarised 90° out of that plane has a change of polarisation a few degrees greater, owing to its being incident at a less angle; the one image from the polarising rhomb being higher than the other.

B. *With Castor Oil.*

AZIMUTH 0° and 180. O′ and E′ completely polarised without any change of plane.

AZIMUTH 39°. Change of polarisation about 10° to left.

AZIMUTH 90°. Change about 20° to left at moderate incidences.

AZIMUTH 141°. Change 0°. Polarisation complete.

AZIMUTH 219°. Change 0°. Polarisation complete.

Azimuth 270°. Change about 10° to right at considerable incidences.
Azimuth 321°. Change 10° to right, and polarisation complete.

B. *Oil of Cajeput.*

Azimuth 39°. Change about 5° to left, and polarisation complete.
Azimuth 90°. Change 7° to left at moderate incidences.
Azimuth 141°. Change 0°. Polarisation complete.
Azimuth 219°. Change 0°. Polarisation complete.
Azimuth 270°. Change 5° to right at considerable incidences.
Azimuth 321°. Change 5° to right. Polarisation complete.

B. *With Olive Oil.*

Azimuth 39°. Change about 5° to left.
Azimuth 90°. Change about 100° to left.
Azimuth 141°. Change 0°. Polarisation complete.
Azimuth 219°. Change 0°. Polarisation complete.
Azimuth 270°, Change 8° to right.
Azimuth 321°. Change 5° to right. Polarisation complete.

B. *With Alcohol highly rectified.*

Azimuth 39°. Change about 1° or 2° to left. The two images vanish at different incidences, S at greater than P.

Azimuth 90°. Change about 1° or 2° to left, spar pencil at angle greater than prism pencil.

Azimuth 141°. Change 0°.
Azimuth 270°. Change about 1° or 2° to left.
Azimuth 280°. Change 0°. Polarisation complete.
Azimuth 321°. Change at 1° or 2° to right.

A. The Face parallel to the Axis.

With Oil of Cassia.

The following observations were made with a plate of glass placed on the surface of the spar, the plate being inclined about 5° in the Azimuth 0° parallel to the axis.

Azimuths.	Change of Polarisation.	Azimuths.	Change of Polarisation.
0°	93° *	50°	47°
10	85	60	37
20	80	67½	33
22½	74	70	32
30	55	80	20
40	59	90	0
45	55		

* The change was 90° at the polarising angle of the spar and oil surface.

These observations were made at angles of incidence considerably greater than 45°, the polarising angle.

The following experiments were made with polarised light.

Inclination of Plane of Polarisation to Plane of Incidence.	Inclination of New Plane to Plane of Incidence.
0°	0°
22½	30
45	67½
67½	79
90	90

When E′ was polarised 90° and O′ 0°, O′ was nearly thrice as faint as E′, and much *redder*.

When the spar and oil image vanishes, *red* light is seen on one side, and *blue* on the other side of the vanishing point, as in elliptical polarisation, the rotation being different for different colours.

The following experiments were made with an equilateral prism of glass.

In AZIMUTH 0° and 180°. Light polarised + 45° — 45° is polarised + 67°—67°, the change of polarisation being 22°.

AZIMUTH about 9°. Light polarised 0° and 90°. E′ is polarised 87° to the left, and O′ and O vanish together. Common light is polarised 87° to left.

Light + 45°—45°. E′ is polarised 59½° to left, and O′ 71½° to right. E′ and O′ are equally bright.

AZIMUTH about 17°. Light + 45°—45°. E′ is polarised 27½° to left, and O′ 80½° to right. E′ is very faint and *red*.

AZIMUTH about 40°. Light + 45°—45°. Both O′ and E′ polarised 25° to left. O′ is brighter than E′, which is faint and *bluish*.

AZIMUTH ABOUT 60°. Light + 45 — 45°. O′ and E′ vanish together a few degrees to the left.

AZIMUTH 90°. Light + 45°—45°, and light 0° and 90°, are treated exactly as by common surfaces; the prism and spar images undergoing the same changes.

As the plane of light polarised + °45 — 45°, becomes light + 0°—0°, or is all polarised in the plane of reflexion, as the azimuths change from 0° to 90°, the inclination of their planes must diminish from 90° to 0°, or from 135° to 0°; that is, from + 67½° — 67½°, to + 0° — °0.

From the observations with common light, it appears that at angles of incidence considerably above 45°, it is polarised a few degrees beyond the azimuth of 90°, and we have no doubt, that at 45°, it is polarised in that azimuth. Hence, it follows, that the change of polarisation is equal to the complement of the azimuth, or 90 — A.

A. *With Oil of Anise Seeds.*

Azimuth 0° and 180°. Scarcely any effect is produced upon common light at any incidence! The spar pencil is brighter than the prism pencil, and *yellow.*

Azimuth between 0° and 90°, and 0° and 270°, the pencil is almost wholly polarised at great incidences, and about 90° to the left.

Azimuth 0° and 180°. Light $+45° - 45°$. E′ was polarised $30\frac{1}{2}°$ to left, and 36° to right. Instead of widening the planes of E′ and O′ into $+67\frac{1}{2} - 67\frac{1}{2}$, the anise seeds oil has brought them nearer into $+30\frac{1}{2}° - 36°$. Conceiving, therefore, that an oil of intermediate refractive power might produce little or no change upon the light $+45° - 45°$, I mixed 2 drops of oil of cassia with 1 drop of oil of anise, and obtained the following results.

A. *Oil of Cassia and Oil of Anise Seeds.*

Azimuth 0° and 180°. Light $+45° - 45°$. E′ is polarised 44° to the *left,* and 0° 44° to the right; that is, almost no change is produced.

With common light there is not a trace of polarisation, and yet reflexion from a transparent surface!

Light 0° and 90°. O′ vanishes with prism image, and A′ polarised about 90°.

Azimuth 90° and 270°. With common light, the spar and prism image vanish together.

Between Azimuth 0° and 90°. With common light, the polarisation increases to about 45° of azimuth, when it is complete, and then gradually returns into common light at 90°.

C. *Face perpendicular to Axis.*

On the natural surface of the *Chaux Carbonatè Basé.*

Oil of Cassia.

With common light. the spar image E is polarised 90° out of the plane of reflexion, and is then *orange,* showing that the light polarised 90° is *blue.* It becomes whiter at great incidences, and redder at small ones. The prism image O is $=2E$ at 45°. At greater incidences E increases faster than O, and becomes nearly equal to it. At small incidences, E decreases much faster than O, so that it follows a different law of reflexion.

At the greatest incidences which the prism allows, the spar pencil is completely polarised.

Light polarised $+45° - 45°$. E′ is polarised 67° to right, and O′ 67° to left.

Light polarised 0° and 90°. O′ vanishes with O, and E is polarised 90° to left.

Oil of Anise Seeds.

In all azimuths about $\frac{1}{2}$ or $\frac{2}{3}$ of E is polarised 90° out of the plane of reflexion at great incidences. At the polarising angle, the light polarised 90° is scarcely perceptible. The residual light is *reddish*, and it is obviously *blue* light that is polarised 90°.

Light polarised +45—45°, becomes polarised +36—36.

With Alcohol.

The prism image O is completely polarised, and the spar image E equally so in the same plane, but at an incidence 3° or 4° greater.

With oil of sassafras, cassada balsam and water, there is no change in the plane of polarisation.

D. *Face inclined* $22\frac{1}{2}°$ *to Axis.*

Azimuth 0° and 180°. In common light, the change of polarisation is 90° to the left. It decreases to 20° in azimuth 90°, becomes 0° in azimuth 113°, and in

Azimuth 180°, it again becomes 90°, decreasing to 0° in azimuth 247°, becoming 20 in azimuth in 270°, and increasing to 90 in azimuth 360°.

Azimuth 0° and 180°. Light + 45 —43°. E′ is polarised $45\frac{1}{3}$ to left and O′ $62\frac{2}{3}°$ to right.

Azimuth about 15°. Light +45°—45°. O′ is polarised $85\frac{1}{2}°$ to left, E vanishing with O and E the prism images. When O′ vanishes E′ has reappeared, and is *red*.

Azimuth 38°. Light +55° — 45°. E is polarised 50° to the right, and O′ $77\frac{1}{2}°$ to the left.

Azimuth 20°. E′ is polarised 17° to left, and O′ 83° to right.

Azimuth 113. Light +45°—45′. No change, E′ and O′ being both polarised in the plane of reflexion. E′ nearly vanishes when O′ vanishes; but by increasing the incidence it vanishes completely, showing the usual red and blue at the point of evanescence.

Azimuth 135°. Light +45°—45°. E′ is polarised 22° to right, O′=O and vanishing with O.

Azimuth 142°. Light +45°— 45°. E is polarised 31′ to the right, O′ vanishing with O.

Azimuth 160°. Light +45°—45°. E′ is polarised 66° to right, and O′ 22° to right.

Azimuth 218°. Light +45°—45°. E′ is polarised 11° to left, and O′ 77° to left.

Azimuth 90°. Light +45°—45°. E′ and O′ are both polarised 28° to the left.

Azimuth 270°. Light +45° = 45°. E′ and O′ polarised 18′ to right.

AZIMUTH 315°. Light +45°—45°. E′ is polarised 59° to right, and O′ 69° to the left.

AZIMUTH 322°. Light +45°—45°. E′ polarised 61° to right, and O′ 83° to right.

AZIMUTH 180°. Light +45° — 45°. E′ is polarised $50\frac{1}{2}$° to left, and O′ 58° to right.

AZIMUTH 0°. Light 0° and 90°. E′ is polarised 90° out of the plane of reflexion, O′ vanishing with O.

AZIMUTH 180°. Light 0° and 90°. E′ is polarised to the left, about 86°, O′ vanishing with O.

AZIMUTH 270°. Light 0° and 90°. E′ and O′ polarised 21° to the right.

AZIMUTH less than 270°. Light 0° and 90°. E′ and O′ polarised 8° to the right.

With Oil of Anise Seeds.

AZIMUTH 0° and 180°. When the prism pencil with common light vanishes, the spar pencil is very bright, and a great deal of light, namely, reddish light is polarised in the plane of reflexion.

AZIMUTH 90° and 270°. The spar pencil is completely polarised about 7° to the left.

In the azimuth, between 90° and 180° perpendicular to the edge beside the obtuse angle, change of polarisation varies from 45° of incidence where it is 0°, up to great incidences, where it is about 45° or 50° to the left, the pencil being there completely polarised. Between azimuths 270° and 360°, the change varies from 125° to 100° at great incidences, but the pencil is nowhere completely polarised.

With Alcohol.

At AZIMUTHS 0° and 180°, there is no change of polarisation, but the spar image is polarised at a much less angle of incidence than the prism image, whereas at azimuth 90°, they are polarised at the same incidence.

E. *Face inclined* 67° 30′ *to the Axis.*

In AZIMUTH 0° and 180°, the change of polarisation is 90°, the polarisation being more complete, by considerably increasing the incidence.

The change of polarisation increases from azimuth 0° to azimuth 90°, diminishes from 90° to 270°, and increases from 270° to 369°.

In AZIMUTH 0° and 180°. At the polarising angle only half the pencil is polarised 90° out of the plane of reflexion, the other half appearing to be polarised in another plane.

AZIMUTH 0°. Light +45° — 45°. E′ is polarised 45° to the right, and O′ 61° to the left.

AZIMUTH 9°. Light + 45° = 45°. E′ is polarised 67° to the right, and O′ 55° to the left.

AZIMUTH about 16°. Light + 45° — 45°. E is poarlised 71° to right, and 51° to left.

AZIMUTH 45°. Light + 45° — 45°. E′ is polarised 90° out of the plane of reflexion, and O′ a few degrees to the left of O and E.

AZIMUTH about 60°. E′ is polarised 76° to left. O′ vanishing with O and E.

AZIMUTH 90°. Light + 45° — 45°. E′ is polarised 70° to left, and O′ 53°. O′ is very faint when the prism images vanish.

AZIMUTH 180°. Light + 45° — 45°. E′ is polarised 68° to the left, and O′ 63° to the right.

AZIMUTH 0°. Light polarised 0° and 90°. E′ is polarised 90° to the left. O′ which is reddish, vanishes with O.

AZIMUTH 180°. Light polarised 0° and 90°. E′ is polarised 85° to the right. O′ vanishes with O.

With Oil of Anise Seeds.

AZIMUTH 0° and 180°. There is no polarisation at great incidences. One-half the spar pencil seems polarised 90° to the left, red light being polarised in the plane of reflexion.

AZIMUTH 90°. Light is almost wholly polarised about 100° to the left at the greatest incidences. At less incidences it is half polarised, red light being left.

With Alcohol.

In AZIMUTH 0°, 90°, and 180°, there is no change of polarisation. The spar image is polarised at a less angle of incidence than the prism image.

In order to observe the changes of polarisation in passing from one plane to another, I had three artificial faces made, slightly inclined to the three principal planes C, B, and E.

E 1. *Face inclined 5° ½ to* **C.**

This face was used a few minutes after it was polished.

Polarising angle in principal section, 59° 38′.

Polarising angle perpendicular to it 58° 25′, much unpolarised light being left.

With Oil of Cassia.

AZIMUTH 0°. Light + 45° — 45°. E′ is polarised 61° to right, and O′ 73° to left.

AZIMUTH 180°. Light + 45° — 45°. E′ is polarised 67° to right, and O′ 76° to left.

In both these azimuths E is $\frac{1}{2}^{\circ}$ O with common light, and is yellowish red.

Azimuth 0°. Light polarised 0° and 90°. E′ is polarised 84° to the right. O′ vanishes with O′, and is fainter and redder than E′.

Azimuth 180°. Light polarised 0° and 90°. E′ is polarised 90° to right. O vanishes with O, and is fainter and redder than E′.

B 1. *Face inclined* 5° 28′ *to* **B.**

In Azimuth 0°. With bright sun-light the spar pencil E is distinctly polarised about 14° to the right.

Azimuth 0°. Light +45° − 45°. E′ and O′ polarised about 13° to left.

Azimuth 0°. Light polarised 0° and 90°. E is polarised about 50° to the right, and O′ vanishes with O.

B 2. *Face inclined* 12° 8′ *to* **B.**

With common light, a small quantity is polarised in the plane of reflexion.

As the azimuth approaches to 90° on either side of the principal section, the light is polarised about 90° out of the plane of reflexion, much *bright blue* light being left. At small incidences the *blue* becomes brighter and purer. The light is orange when the principal section of the analysing rhomb is in the plane of reflexion, as if red light was polarised 90° out of the plane of reflexion, and blue light in that plane.

The experiments described in the preceding pages form but a small portion of those which I have made, both with artificial and solar light on the action of the surfaces of calcareous spar on common and polarised light. In submitting them to the Society, it is proper that I should mention the great difficulty of obtaining precise results in such observations. The extreme faintness of the reflected light; its imperfect polarisation in many cases at the angle of maximum polarisation; and the loss of one-half of the light in the analysing prism, render it very difficult to determine the deviation of the reflected pencil, especially when it is partially polarised, or unequally double; and I have been surprised at the great difference in the results obtained at different times with the same surfaces, when the observations were in both cases recorded as satisfactory.

The following general results, however, are sufficient to show the importance of this class of researches, in reference to certain questions in the undulatory theory which have not yet been solved, and perhaps to guide the mathematician to their solution.

1. In the reflexion of light, the surfaces of calcareous spar in contact with fluids, act in some cases as ordinary uncrystallised surfaces in the polarisation of light.

2. In reflecting common light, they polarise it out of the plane of reflexion,

the deviation from that plane varying with light of different colours, and with the angle of incidence.

3. In reflecting polarised light, they change its plane of polarisation, sometimes as in refraction, and sometimes as in reflexion.

4. In certain azimuths, and at certain incidences when the pencil is not completely polarised, much light, apparently unpolarised light, is left; and in many cases, upon all the surface, A, B, C, D and E, the reflected pencil consists of two oppositely polarised pencils, as in the reflexion of common light from the surface of murexide, chrysammate of potash, and a few other bodies.

5. The changes in the planes of polarisation, both of common and polarised light, are related to the axis of double refraction, that is, to the short diagonal of the primitive rhomb of calcareous spar.

XXI.—*Additional Observations on the Polarisation of the Atmosphere, made at St Andrews in* 1841, 1842, 1843, 1844, and 1845. By Sir David Brewster, K.H., D.C.L., F.R.S., &c.

(Read 2d January 1866.)

Since the publication of my "Observations on the Polarisation of the Atmosphere," a long and elaborate Memoir on the same subject, by Dr R. Rubenson, has appeared in the Acts of the Royal Society of Sciences of Upsal.* The observations which it contains were made with the finest instruments, and with a degree of accuracy which had not been attempted by previous observers. They were begun at Upsal in 1859, and carried on at Rome between the 6th of June and the 5th of August 1861, at Segni in the Campagna, between the 6th and the 27th of August 1861, and at Rome from the 5th of October 1861 to the 27th of July 1862.

Although Dr Rubenson has devoted a section of his work to ascertain the cause of atmospherical polarisation, another section to the determination of the place of maximum polarisation, and a third to the causes which disturb the polarisation of the atmosphere, yet the chief object of his labours was to study the daily variation of the polarisation of the maximum point; and so fully has he treated this important branch of his subject, that the description of his polarimeter, of his method of using it, and the discussion of his observations, with the observations themselves, occupy three-fourths of his Memoir.

In his section on the Cause of Atmospherical Polarisation, Dr Rubenson is led to the same conclusion which I had deduced from my earliest observations, namely, that the light of the blue sky is polarised by reflexion from the molecules of air, and not from vesicles of water with parallel sides, as maintained by Clausius, nor, as conjectured by others, from extremely minute drops of water, nor from molecules of aqueous vapour in an intermediate state between that of gas and that of vesicles.

According to Arago, the distance of the place of maximum polarisation from the sun was 89° 6′, the mean of six observations. I found 89° to be the mean of a great number of observations, but, like Arago, I considered 90° to be the nearest approximation to the place of maximum polarisation. Dr Rubenson found it to undergo, as I did, great variations, chiefly from 88° to 92°, the general mean of

* Series 3d, tom. v. This Memoir has been published as a separate work in 4to, pp. 238. Upsal, 1864.

which, from his observations, was 90° 2′, half of which is so near to the polarising angle of air, which is 45° 0′ 32″, as to place it beyond a doubt that the light of the blue sky is polarised by reflexion from its particles.

In his section on the Causes which Disturb the Polarisation of the Atmosphere, Dr RUBENSON found, as I did, that clouds and fogs and smoke were the cause of the greatest perturbations; and he also found, as I had done,* that the intensity of the polarisation was reduced by the crystals of ice floating in the atmosphere which form the halo of 23°.

Dr RUBENSON has not observed the secondary neutral point which I found sometimes accompanying the neutral point of ARAGO, when it rises above the horizon, or is setting beneath it, and he has never been able to see, even under the fine sky of Italy, the neutral point which I discovered under the sun, and which, I believe, has not been seen by any other observer than M. BABINET.

In 1854, M. FELIX BERNARD† made several observations at Bordeaux, in order to determine the intensity of the maximum polarisation at different hours of the day. Though made only on four days of the month of October (from the 16th to the 19th inclusive), he found "that in proportion as the sun approaches the meridian the value of the maximum polarisation diminishes; that this value increases, on the contrary, in a continuous manner as the sun recedes from the meridian, and that it reaches its maximum when the sun is very near the horizon, the amplitude of this variation being about 0·09."

On the 16th October 1854, the maximum polarisation increased gradually after mid-day from 25° to 0° of the sun's altitude, from 0·6236 to 0·7051; and on the 19th October, from 5° to 35° of the sun's altitude, it diminished from 0·7083 to 0·6106. On these two days the maximum polarisation, at an altitude of 20°, was 0·6582, and 0·6464 respectively, the mean of which is 0·6523, differing only 0·12 from 0·64, as computed from FRESNEL's formula by M. BERNARD, from my observation in 1842, that when the sun's altitude was 20°, the intensity of the maximum polarisation at 90° from the sun was equivalent to that which would be produced by reflexion from the surface of glass, whose index of refraction was 1·486, at an angle of 65° 30′‡.

Before he became acquainted with the Memoir of M. BERNARD, Dr RUBENSON had completed his observations on the same subject; and, though they lead to a similar result, yet they possess a peculiar value from their having been made with the finest instruments, in different localities,—in almost all the seasons of the year, and under various states of the atmosphere.

From a careful examination of his observations, Dr RUBENSON arrives at the

* Treatise on Optics, p. 394, and Edin. Trans., vol. xxiii. p. 226.

† *Comptes Rendus*, tom. xxxix. p. 775, October 1854.

‡ Johnston's Physical Atlas—*Meteorology*, p. 10; or Phil. Mag., series 3d, vol. xxiv. p. 453, December 1847.

general conclusion "that *the atmospheric polarisation is subject to a diminution during the morning, and to an increase during the evening,* without one's being able to assign with certainty the precise hour of the minimum polarisation." These changes Dr RUBENSON found to be often influenced by perturbations commonly of short duration, and taking place indifferently at all hours of the day. They frequently arise from clouds or smoke, and probably often from *cirrus* too faint to be seen. According to Dr RUBENSON, the blue colour of the sky, in a normal state of the atmosphere, and 90° from the sun, is feeble at sunrise, increases rapidly in intensity, and attains to its maximum some hours before noon, the number of hours being different at different seasons. The intensity of the colour diminishes towards noon. It then increases, reaches a second maximum after some hours, and then diminishes quickly towards sunset. The relation between the blue colour of the sky and the intensity of its polarisation, is a problem which remains to be solved.

In 1859, M. LIAIS made observations on the polarisation of the atmosphere during his voyage from France to Brazil, and at San Domingo in the bay of Rio Janeiro. His observations were made at the beginning of dawn and at the end of twilight, with the view of determining the height of the atmosphere. From the observations made at sea he obtained 320, and from the observations made on land 340 kilometers, or 212 miles, as the height of the atmosphere.*

The most recent observations on the polarisation of the atmosphere were made by M. ANDRÈS POEY, between 1862 and 1864, under the tropical sky of the Havannah. The observations themselves have not been published; but he states, as one of the most important of their results, that "at sunrise and sunset the system of atmospherical polarisation ought necessarily to present two planes of rectangular polarisation, one vertical, passing through the eye of the observer and the sun, and the other horizontal, with four inversions of the signs, and four neutral points 90° from each other."

M. POEY adopts my theory of atmospherical polarisation, and the analogy which I pointed out between the lines of equal polarisation and the isochromatic lines of biaxal crystals, and between the same lines and those of uniaxal crystals when the sun is in the zenith,—the neutral points now meeting in the sun.†

It will be seen, from the preceding details, that the subject of atmospherical polarisation has become one of the most important branches of optical meteorology. It has already thrown much light on the constitution of the atmosphere; and when it has been studied in different climates, and at different altitudes above the sea, by Alpine travellers and scientific aeronauts, it will doubtless have still more valuable applications.

* *Comptes Rendus,* &c. tom. xlviii. pp. 109–112.

† See *Comptes Rendus,* &c. tom. lx. p. 781, Avril 17, 1865.

Under this impression I have been induced to submit to the Society the rest of four years' observations which I made at St Andrews, and which, along with those already published, will exhibit the optical condition of the atmosphere on many days during every month of the year.

1841, April 28.—Wind west; fine day.

Mean Time.
3^h P.M. Polarisation a maximum in the plane passing through the sun and the zenith, and at 88° 16′ from the sun.

When the sun, or the antisolar point, rose or set, the neutral line of the polariscope bands, held and moved vertically, was a hyperbola, as shown in fig. 1.

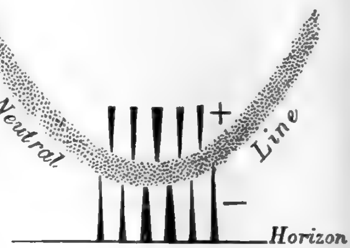


Fig. 1.

1841, April 30.

Mean Time.
2^h 5^m Polarisation a maximum in plane of zenith and sun, and at 78° 25′ from sun.

1841, May 6.

Mean Time.
3^h 30^m Polarisation, when a maximum, greater in plane of zenith and sun than in any other plane. At sunset the difference small. The polarisation was greater in the S. horizon than at the same point in the N. horizon, probably from the sky being there freer from haze.

1841, May 8.

Mean Time.
10^h 10^m Polarisation, or R, $= 25\frac{1}{2}°$, and a maximum in plane of zenith and sun. In the N.E., at an altitude of 40°, $R = 14\frac{1}{2}°$, and also much less in S.W. horizon.

1841, May 9.

Mean Time.
12^h NOON. Sky greenish-blue. In plane of zenith and sun $R = 13\frac{3}{4}°$. At 4^h $R = 24\frac{1}{2}°$ and $22\frac{1}{2}°$ in different places, and *always greatest where the sky was bluest.*

1841, May 11.

Mean Time.
3^h 45^m P.M. $R = 24\frac{1}{2}°$, and a maximum in plane of zenith and sun. In other planes, $R = 22\frac{1}{2}°$.

1841, May 12.

Mean Time.
10^h 15^m A.M. The sky blue and unusually clear throughout the day. Barom. 30·1; Therm., 9^h P.M. 48°.
$R = 26\frac{1}{2}°$ in plane of zenith and sun and a maximum. In other planes, $22\frac{1}{2}°$.
11 40 $R = 28\frac{1}{2}°$ in plane of zenith and sun.
$21\frac{1}{2}°$ in lower planes.

White clouds; cumuli in motion.

Mean Time.	
$12^h\ 0^m$	$R=27\frac{1}{2}°$ in plane of zenith and sun.
	$R=20\frac{1}{2}$ near horizon.
	$R=25\frac{1}{2}$ at intermediate points.
1 20	$R=26\frac{1}{2}$ in plane of zenith and sun.
	$R=21\frac{1}{2}$ near E. horizon.

Near the large white cumuli R diminishes.

Mean Time.	
$4^h\ 30^m$	$R=30\frac{1}{2}°$ in plane of zenith and sun.
	$R=25\frac{1}{2}$ in horizon.
6 0	$R=27\frac{1}{2}$ in plane of zenith and sun.
	$R=24\frac{1}{2}$ in horizon.
7 10	$R=30\frac{1}{2}$ in plane of zenith and sun.
	$R=28\frac{1}{2}$ in horizon.
7 35	$R=30\frac{1}{2}$ in plane of zenith and sun.
	$R=29\frac{1}{2}$ in horizon.
7 45	$R=28\frac{1}{2}$ in plane of zenith and sun.
	$R=28\frac{1}{2}$ in horizon.

See Edin. Trans. vol. xxiii. pp. 213–223, for the places of the neutral points on this day.

1841, May 14.—The sky in the forenoon has very little blue in it, being in its colour a French grey. R less than $14\frac{1}{2}°$.

Mean Time.	
$3^h\ 30^m$	$R=14\frac{1}{2}°$, and
	$R=18\frac{1}{2}$ in a bluer part of the sky.

According as the thin white haze which masked the blue colour of the sky was removed or returned, the place of the neutral points constantly varied in their position.

In the evening the sky became clear, and R became $24\frac{1}{2}°$ and $26\frac{1}{2}°$.

1841, May 16.—See "Edinburgh Transactions," vol. xxiii. p. 223.

1841, May 16.—Barom. 29·4. Windy. Considerably above the horizon R varied from $17\frac{1}{2}°$ to $14\frac{1}{2}°$, as the blue sky was more or less distant from the white moving clouds.

At 7^h, when the blue was purer, $R = 22\frac{1}{2}°$ at 45° of altitude in the S.

At $7^h\ 42^m$ $R = 24\frac{1}{2}°$ at 20° altitude in the N.

1841, May 17.—Barom. 29·5.

Mean Time.	
$1^h\ 20^m$	$R=17\frac{1}{2}°$, the maximum polarisation at 99° from sun in the plane of zenith and sun.
2 0	$R=17\frac{1}{2}°$ and $15\frac{1}{2}°$ at lower altitudes.

The following observations, from May 24 to June 3, were made in Edinburgh:—

1841, May 24.

Mean Time.	
$11^h\ 10^m$	$R=17\frac{1}{2}°$ maximum in plane of zenith and sun.
	$R=14\frac{1}{2}$ in horizon.

After a cloud had passed the polarisation was diminished.

Mean Time.		Height.
$3^h\ 15^m$	R=22½° maximum in plane of zenith and sun.	
	R=14½ elsewhere.	
5 0	R=26½ maximum in plane of zenith and sun.	
	R=24½ in horizon.	
	BABINET's neutral point near the sun.	
7 0	R=22½° in zenith and horizon.	
7 0	ARAGO's neutral point.*	22° 5′
8 0	Do. do.	17 48
9 0	Do. do.	15 50
	R=20½° in zenith.	
	R=22½ in horizon.	

1841, May 25.

Mean Time.	
$6^h\ 0^m$	ARAGO's neutral point in horizon, and the hyperbolic neutral line distinct.

1841, May 27.—Slightly hazy.

Mean Time.	
$4^h\ 45^m$	R=20½° in zenith.
	R=19½ in horizon.
	BABINET's neutral point not seen.

1841, May 28.

Mean Time.	
$11^h\ 0^m$	R=15½°. Hazy bands, ill-defined and *ragged*.

Observations resumed at St Andrews.

1841, June 3.—In the morning, R = 14½° and 18½°.

Mean Time.		
$6^h\ 0^m$ P.M.	R=25½° in zenith and horizon.	
6 27	ARAGO's neutral point not above horizon.	
6 36	Do. do.	very near the horizon.
6 43	Do. do.	above and close to horizon.

See Edin. Trans. vol. xxiii. p. 214, for the height of ARAGO's neutral point.

1841, June 6.—Barom. 29·9.

Mean Time.	
$4^h\ 45^m$ P.M.	R=14½° through zenith.
	R=23½ 45′ above S. horizon.
	R=22½ in S. horizon.

In and near the horizon, the white bands of the polariscope are *bluish* on the side of the neutral line from the sun. Maximum polarisation more than 90° from sun, and diminished by clouds coming on.

Mean Time.		Arago.†
$6^h\ 0^m$	Sky very clear.	18° 36′
8 20		19 28
8 30		21 20

* The height of ARAGO's neutral point is to be understood as above the antisolar point, and that of BABINET as above the sun.

† The numbers under ARAGO and BABINET are the heights of their neutral points above the antisolar point and the sun.

Mean Time.		Arago.
8h 36m	Sun set. Haze in S.E.	
8 45	R=28½° through zenith, and 26½° in horizon.	
9 10	R=28½ through zenith, and in S. and N. horizon.	17° 20′
9 17		21 39
9 30	Haze continued.	22 12

1841, June 8.—Barom. 30. Fine day.

Mean Time.	
11h 40m	R=21½° through zenith. R=19° in W. horizon.
1 5	R=29½ through zenith.
4 10	R=26½ through zenith. R=27° 30′ from N.E. horizon.
4 30	R=28½ 45° above N.E. horizon.
4 45	Babinet's neutral point and the neutral hyperbolic line clearly seen.
5 50*	R=20° to 24° as the sky was more or less clear.

At this hour the curious phenomenon shown in the annexed figure was seen, two hyperbolic neutral lines meeting in the sun.

Mean Time.	Babinet's N. Point above sun.
6h 42m	13° 24′
7 50	26 37
8 28	15 16
9 10	20 24
9 30	15 0

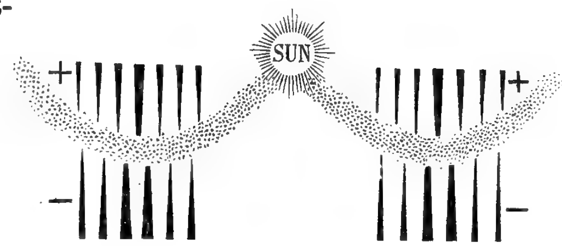


Fig. 2.

Mean Time.		Arago.
6h 45m		
7 30	R=27½° maximum polarisation.	19° 50′
7 35		20 25
8 20	R=24½° in S.W. horizon.	
8 23		21 10
9 10	Faint clouds near.	22 6
9 30		23 41

1841, June 9.—Barom. 20·9. Fine day; mackerel-sky occasionally.

Mean Time.		Babinet.
3h 55m	Fine pure sky. R=25½° in N. horizon.	5° 0′
4 30		12 0
6 30	R=26½°+in zenith and horizon.	17 15
7 7†		13 15
7 30	Fine cirri above the sun.	14 30
7 37	R=28½° in zenith and horizon.	16 7
8 0		17 20
8 40		18 5

Mean Time.		Arago.
7h 45m	Antisolar point below horizon.	21° 0°
7 54		20 45
8 21		20 35
10 7		24 45

At 8h 40m clouds suddenly covered the whole horizon.

* See Edin. Trans. vol. xxiii. p. 221.

† During the preceding quarter of an hour a stratum of cirri surrounded the neutral point, and was just absorbed, when the observation was made.

1841, June 10.—See "Edinburgh Transactions," vol. xxiii. pp. 214 and 223.

1841, June 11.—Barom. 29·8. Wind north-east.

Mean Time.		Arago.
4h 45m	R=26½° in zenith, and 24½° in horizon.	
6 38	R=27° in zenith and horizon.	
7 34		19° 37′
7 42		19 3
8 5		19 0
8 18		18 52
8 36		18 50
8 52		18 32
9 3		19 12
9 15		19 17
9 25		19 20
9 33		21 37
9 45		23 39
9 55		25 22
10 0		25 13

Mean of observations within less than 4° of the horizon, 18° 40′. The evening was not so fine as yesterday.

1841, June 12.—Barom. 29·85. Wind north-east.

Mean Time.	
9h 0m A.M.	R=24½° in horizon, 26½° in zenith, and R=25½ about 20° alt. S.W. and S.E.
11 22	R=24½ in zenith.
1 0 P.M.	R=23½ in zenith. Sky fine.

The day became cloudy, but suddenly cleared up at 8h P.M., when R=28½° in zenith and horizon.

Mean Time.		Arago.
8h 18m		17° 22′
8 36		17 15
8 52	Therm. 50° R=28½° in horizon.	18 8
9 3		17 30
9 15		18 42
9 25		18 50
9 33		20 52
9 45		21 54
10 0		24 43

Mean of observations within less than 4° of the horizon, 17° 39′.

Mean Time.	Babinet.
8h 25m	17° 13′
8 54	17 12
9 8	17 20

1841, June 14.—Barom. 29·6.

Mean Time.		Arago.
7h 45m		18° 35′
7 51	R=26½° in N.E.	19 15
8 5		18 53
8 18		18 58
8 36		

1841, June 15.—Barom. 30·0 at 9h, and 29·8 at 10h A.M.

Mean Time.		Arago.
8h 36m		18° 45′
8 52	R=25½° at 40° alt. N.E.	18 32
9 3		18 9
9 15		18 7
9 40		21 16
9 52	R=28½° at 40° alt. N.E.	21 52

1841, June 21.—Barom. 29·4.

Mean Time.		Babinet.
7h 42m	R=24° at 40° alt., S. 21½° in zenith.	21° 30′
7 52	Clouds passing over the neutral point.	20 44
8 0		20 0
9 15		20 28

1841, June 2.—Barom. 29·68.

Mean Time		Arago.
3h 0m P.M.	R=14½°. A faint whiteness over blue sky.	
8 31		17° 37′
8 47	R=14½° in horizon, and 25½° in zenith, and 24½° in alt. 40° S.W.	19 58
9 0		18 45
9 15	Sky not pure this evening.	18 5
9 30		18 0

1841, June 23.—Barom. 29·72. Sky impure.

Mean Time.		Arago.
8h 5m	Clouds below the neutral point. Observations not satisfactory.	21° 30′
8 23		17 25
8 45		20 35
8 52		20 48

1841, June 27.—After three days of eastern haur and rain.

Mean Time.		Arago.
10h 0m A.M.	R=19½° in zenith plane, the sky being cloudy.	
1 0 P.M.	R=14½° at 20° alt. in E. Barom. 29·6 and rising.	
7 30		21° 21′
7 45	R=22½° in zenith plane, and 21½° at 30 alt.	22 5
8 0		21 42
8 18		20 12

When light clouds covered the sky round and over the neutral point, the polarisation was + or vertical from the zenith to the horizon.

1841, June 28.—After a bad rainy day and the wind east, the sky cleared up in the evening and the wind became west.

Mean Time.	Arago.
8h 50m P.M.	18° 26′
9 13	18 43

1841, July 17.—See "Edinburgh Transactions," vol. xxiii. p. 214.

1841, July 24.

Mean Time.		Arago.
6^h 40^m	$R=14\frac{1}{2}°$; cloudy sky.	25° 55′

1841, July 28.—Barom. 29·37. A clear blue sky; cloudy.

Mean Time.		Arago.
7^h 10^m	$R=26\frac{1}{2}°$ to $28\frac{1}{2}°$	18° 12′
7 40		18 43
7 57		17 53
8 10		18 5

1841, July 31.—Cloudy. Neutral point covered with minute cirri.

Mean Time.	Arago.
8^h 0^m	16° 45′
8 14	17 25

1841, August 6.—After two days of rain.

Mean Time.		Arago.
8^h 5^m P.M.	$R=29\frac{1}{2}°$. A cloud had passed.	16° 28′

1841, August 8.—Morning rainy; splendid evening.

Mean Time.		Arago.
6^h 50^m	$R=29\frac{1}{2}°$	16° 17′
7 5	$R=28\frac{1}{2}$	18 20
7 35	Clouds came on.	17 53

1841, August 10.—After rain.

Mean Time.	Arago.
7^h 45^m	18° 15′

1841, August 17.—Clear and windy.

Mean Time.	
9^h 0^m A.M.	$R=21\frac{1}{2}°$ in zenith, and $15\frac{1}{2}°$ near horizon.

The blue of the sky, though very clear, was whitish, which always reduces the polarisation. Same day at Perth.

Mean Time.		Arago.
7^h 15^m	$R=24\frac{1}{2}°$ in zenith, and $20\frac{1}{2}°$ in horizon.	20° 38′
7 30		19 24

During the whole day the blue sky became whiter and whiter, and the polarisation fell below $14\frac{1}{2}°$ out of the scale of the Polarimeter. Small black clouds appeared upon the white sky.

The observations, from the 6th to the 17th August, were made at the Bridge of Earn.

1841, August 31, September 6th and 12th. See "Edinburgh Transactions," vol. xxiii. p. 214.

1841, September 6.—See "Edinburgh Transactions," vol. xxiii. pp. 215, 223.
September 12. ,, ,, ,, pp. 215, 223.

1841, September 13.

Mean Time.			Arago.
$5^h\ 55^m$	A.M.	R=$25\frac{1}{2}°$ zenith; $23\frac{1}{2}°$ horizon.	19° 10′
5 58			20 0
6 32	P.M.	R=$25\frac{1}{2}°$ zenith.	18 16
6 38			18 0
6 42			17 27

1841, September 15.—Barom. 29·61. Splendid day.

Mean Time.			Arago.
$10^h\ 0^m$	A.M.	R=$27\frac{1}{2}°$ in zenith plane, and $25\frac{1}{2}°$ in horizon.	
10 18		R=$28\frac{1}{2}°$. Maximum 88° from horizon.	
6 0	P.M.		19° 54′
6 8			18 18
6 10		Sky not altogether pure.	18 47
6 28		R=$27\frac{1}{2}°$ in zenith plane.	18 5

1841, September 21.—Barom. 29·95. Sky not pure; an eastern haur for three preceding days.

Mean Time.		Arago.
$4^h\ 50^m$	R=$15\frac{1}{2}°$ in zenith, and 14° 30′ above S. horizon.	
4 55	Sky whiter in zenith than in horizon.	21° 59′

1841, September 23.—Barom. 29·47. Air damp, but no rain.

Mean Time.		Arago.
$6^h\ 0^m$ P.M.	R=$26\frac{1}{2}°$ in zenith, and $17\frac{1}{2}°$ 15° alt. S.	13° 30′

The sky impure. Neutral point covered with small black clouds; an eastern haur supervened.

1841, September 26.—Barom. 29·25. Day showery.

Mean Time.		Arago.
$5^h\ 49^m$ P.M.	R=$22\frac{1}{2}°$ zenith; $26\frac{1}{2}°$ in horizon.	22° 2′
6 10	Not free of clouds about neutral point.	20 2
6 15	The sky purer.	21 50

1841, September 29.—See "Edinburgh Transactions," vol. xxiii pp. 215, 223.

1841, September 30.—Barom. 29·03.

Mean Time.		Arago.
$4^h\ 27^m$	R=26° in zenith plane, and 25° 10° above S. horizon.	
4 37	Sky whitish.	20° 20′
4 43	Black cloud passed over neutral point.	19 49
4 49		22 29

1841, October 3.—Barom. 29.8. Wind north-east; cold.

Mean Time.		Arago.
$4^h\ 1^m$	R=$27\frac{1}{2}°$ in zenith plane, and $26\frac{1}{2}°$ above horizon.	
4 44		21° 15′

A cloud passed, and the neutral point descended.

1841, October 12.—Barom. 29·1. Beautiful morning; the sun rose free of clouds.

Mean Time.		Arago.
6^h 17^m	A.M. R=$28\frac{1}{2}°$ in zenith, and $26\frac{1}{2}°$ in horizon.	21° 25′
6 24		18 20
4 54	P.M. R=25° in zenith; $27\frac{1}{2}°$ near horizon.	18 1
5 10		19 57

1841, October 18.—Barom. 29·5. Wind west; a very fine day.

Mean Time.		Arago.
3^h 0^m	R=$29\frac{1}{2}°$ in zenith, and $26\frac{1}{2}°$ near horizon.	
4 42		20° 5′

1841, October 23.—See "Edinburgh Transactions," vol. xxiii. pp. 215 and 224.

1841, October 25.—A cold day with a little rain. Wind north, and came round to the east at 4^h.

Mean Time.		Arago.
3^h 57^m	R=29° in zenith plane.	19° 10′
4 13	R=$29\frac{1}{2}°$ in zenith plane.	20 20
4 17	Slight clouds.	20 31
4 23	R=$28\frac{3}{4}°$ in zenith plane; $27\frac{1}{2}°$ 8° above S. horizon.	20 1

1841, October 26.—Barom. 29·6. Fine day and cold.

Mean Time.		Arago.
2^h 0^m	R=$25\frac{1}{2}°$ in zenith, sun shining; $27\frac{1}{2}°$ sun not shining.	
2 29	Arago's neutral point rose in the N.E. above sea.	
2 29	R=$25\frac{1}{2}°$ in zenith; $28\frac{1}{2}°$ in horizon.	14° 55′
4 22	R=29° in zenith; $29\frac{1}{2}°$ in horizon.	22 17
4 35		20 40
4 47	R=28° in zenith; $27\frac{1}{2}°$ in horizon.	20 35
4 53		22 39
		Babinet.
4 24		15° 5′
4 37		15 55
4 50		16 55
4 55		15 36

1841, October 28.—Barom. 29·9. Fine day.

Mean Time.		Arago.
3^h 15^m	A cloud had just left the neutral point.	16° 48′
8 39		19 40
4 23	A little cloud left about the neutral point.	20 2
4 39		21 50
4 50		21 28
		Babinet.
3 19	R=$29\frac{1}{2}°$ in zenith; $28\frac{1}{2}°$ 10° altitude N. and S.	12° 15′
3 35		11 50
4 15	Maximum polarisation more than 90° from sun.	13 28
4 25		14 27
4 42	R=$26\frac{1}{2}°$ in zenith, and at 10° above N. horizon.	14 5
4 47		15 4

1841, November 2.—See "Edinburgh Transactions," vol. xxiii. pp. 215 and 224.

November 4. " " "

November 15.—Barom. 29·5; therm. 35. Haze and clouds.

Apparent Time.		
2^h 0^m	R=27½° in zenith and horizon.	
		Arago.
2 15		13° 20′
3 56		18 10
4 1		17 40
4 5		19 50
		Babinet.
3 41		16° 31′
3 54		17 55
4 3		16 36
4 7		16 0

1841, November 16.—R=30° in zenith; 26½° near horizon.

Apparent Time.		Arago.
3^h 45^m	Sky whitish blue.	17° 58′
3 47		17 10

1841, November 17.—Barom. 29·43. Frost. See "Edinburgh Transactions," vol. xxiii. p. 228.

Apparent Time.		Arago.
11^h 35^m A.M.	R=26½° in zenith; 14½° near horizon.	14° 24′
12 0	Polarisation between the sun and horizon.	
		Babinet.
2 1		11° 30′
2 25		11 50
2 40		11 30
3 15		13 15
3 23		14 0
3 54		17 6
		Arago.
2 30	R=29½° in zenith; 26½° near horizon.	22° 31′
2 48	Fine day and fine sky.	23 34
3 19		21 30
3 51	R=29½° in zenith; 26½° near horizon.	19 0

1841, November 23.—Barom. 29·3. Cold, damp day; wind west.

Apparent Time.	Arago.
3^h 57^m	18° 51′
	Babinet.
4 0	15° 24′

1841, November 25.—See "Edinburgh Transactions," vol. xxiii. pp. 216, 224.

1841, Dec. 1.

Apparent Time.		Arago.
4^h 3^m		16° 41′
4 14	R=28½°; 30° above S. horizon.	17 51
		Babinet.
4 5		15° 34′
4 10		14 52

1841, December 5.

Apparent Time.	
11^h 0^m A.M.	R$=25\frac{1}{2}°$ in zenith plane.

1841, December 7.—Neutral line convex towards the sun.

Apparent Time.	Babinet.
8^h 51^m A.M.	15° 4′

1841, December 11.—Barom. 29·4; therm. 41.

Apparent Time.		Arago.
3^h 46^m	R$=28\frac{1}{2}$ in zenith, and in 20° above horizon.	18° 27′
3 51		16 16

Although the sky appeared free of clouds, yet, upon close examination, extremely faint and transparent clouds reflecting no light, but rather darker than the sky, covered the whole heavens.

1841, December 17.—Beautiful morning.

Apparent Time.		Babinet.
9^h 7^m A.M.	R$=27\frac{1}{2}°$ in S.W. horizon.	
9 13	Neutral line concave to sun.	13° 10′
10 30	R$=27\frac{1}{2}°$ in zenith plane; $25\frac{1}{2}°$, 30° above N.E. horizon.	

1841, December 18.—Splendid sky; without a cloud.

Apparent Time.	Babinet.
8^h 53^m A.M.	15° 55′
	Arago.
9 8	19° 52′

At 9^h the maximum polarisation R$=28°$ in zenith plane, and far beyond 90° from the sun.

Apparent Time.		Arago.
3^h 17^m P.M.		19° 5′
3 32		17 25
		Babinet.
3 22	R$=28°$ in zenith; $27\frac{1}{2}°$ in horizon.	17° 20′
3 36		18 25

1841, December 22.—Fine frosty day; clear sky.

Apparent Time.	Babinet.
9^h 23^m	14° 10′
	Arago.
9 33	20° 10′

R$=27\frac{1}{2}°$ in zenith plane; $24\frac{1}{2}°$ in S.W. horizon, and $26\frac{1}{2}°$, 10° above.

1842, January 6.—Fine day.

Apparent Time.	Arago.
2^h 34^m	19° 6′
	Babinet.
2 38	16° 18′

R$=2\frac{1}{2}°$ in zenith plane; $28\frac{1}{2}°$, 30° above E. horizon.

1842, January 7.—Very fine day, with haze.

Apparent Time.		Babinet.
9^h 18^m A.M.		15° 51′
12 0		13 53
		Arago.
11 52		20° 34′
12 4 P.M.		22 0
12 9	R=22½° in zenith.	22 37
3 25		20 40
4 17		19 49

At 9^h 18^m. R=24°, a maximum in horizon. The neutral line was *convex* to the sun. Sky clear, without clouds.

1842, January 16.—Ground everywhere covered with snow.

Apparent Time.	Babinet.
4^h 12^m	19° 25′

1842, January 17.—Fine clear day; therm. 36.

Apparent Time.		Babinet.
8^h 37^m A.M.	R=24½° at 35° alt. 20½° in horizon.	16° 55′
3 47 P.M.	R=25° in zenith; 24½° in horizon. Neutral line concave towards the sun.	

1842, January 21.—Barom. 29.77; dry, frosty day.

Apparent Time.		Arago.
3^h 34^m	R=27½° maximum polarisation in zenith plane, and 24½° near horizon.	19° 40′
		Babinet.
3 31		19° 3′

1842, January 25.—Snow covers the ground. Barom. 29·4.

Apparent Time.		Arago.
1^h 12^m P.M.	ARAGO's neutral point in horizon. R=17½° in zenith plane, and + 15° in horizon.	
2 15		16° 45′
2 12	R=18½° in zenith, and 15° in horizon.	19 21
3 40	R=18½° in zenith, and at 50° altitude, 15°.16	20
		Babinet.
2 15		17° 20′
3 36		18 30

1842, January 27.—Barom. 29·2; therm. 36° at 8^h 30^m.

Apparent Time.		Arago.
8^h 34^m A.M.	R=24½° near horizon; neutral line concave to sun.	
2 20 P.M.		11° 50′
4 37		20 40
2 32	R=25° in zenith plane and S. horizon.	
		Babinet.
4 35	Thin clouds, almost invisible everywhere but above the sun	18° 40′

1842, January 28.—Barom. 29·6. Fine day; fresh.

Apparent Time.		Arago.
2^h 7^m P.M.	R=$22\frac{1}{2}°$ in zenith; $25\frac{1}{2}°$ in E. horizon.	13° 5′
3 23	R=$26\frac{1}{2}°$ in zenith; $27\frac{1}{2}°$ in S.E., and $26\frac{1}{2}°$ in S.W. horizon.	21 15
3 43		20 25
4 5		19 41
		Babinet.
3 21		18° 48′
3 47		18 10
4 2		16 52

1842, January 29.—See "Edinburgh Transactions," vol. xxiii. pp. 216, 224.

1842, February 2.—Very fine day, and clear sky till 2^h.

Apparent Time.		
11^h 30^m A.M.	Arago's neutral point not risen. R=25° in zenith plane, $20\frac{1}{2}°$ in horizon.	
		Arago.
2 0 P.M.	A thick, impure sky.	16° 50′

1842, February 3.—Fine day.

Apparent Time.	Arago.
4^h 43^m	20° 20′
	Babinet.
4 45	22° 1′

1842, February 4.—Barom. 30·15. Fine day; cloudy.

Apparent Time.		Arago.
3^h 14^m	R=$20\frac{1}{2}°$ in zenith, $24\frac{1}{2}°$, 50° alt. W. horizon.	18° 40′
4 0	R=25° in zenith, thin whitish clouds, $24\frac{1}{2}°$ in E. horizon, 10° alt., $22\frac{1}{2}°$ in alt., 20° W. horizon.	20 37
4 44		20 14

1842, February 5.—Barom. 30·05. Fine day; sky perfectly clear.

Apparent Time.		Arago.
1^h 51^m		17° 25′
1 44		23 10
3 48		22 15
4 9		20 50
4 31		20 30
3 51	R=$28\frac{1}{2}°$ in zenith, $26\frac{1}{2}°$ to $18\frac{1}{2}°$ on E. horizon, neutral line convex to sun.	
		Babinet.
4 7		17° 20′
4 33	R=$27\frac{1}{2}°$ in zenith.	19 42
9 5 A.M.	R=$22\frac{1}{2}°$ 30° alt. S.W. horizon.	20 38
12 34 P.M.	R=$27\frac{1}{2}°$ in zenith, $20\frac{1}{2}°$ in horizon. Dark haze in horizon from E. to W. by N.	
2 6	R=$27\frac{1}{2}°$ in zenith to $14\frac{1}{2}°$ in horizon.	

1842, February 10.—Fine day, but cloudy.

Apparent Time.		Arago.
2^h 57^m	R=$24\frac{1}{2}°$, 25° above W. horizon.	16° 45′
3 58		19 20
4 45	R=$26\frac{1}{2}°$ in north.	17 43
		Babinet.
3 59	R=29° in zenith, and at alt. 30° E. horizon.	18° 38′
4 5		19 4
4 41		21 50

Clouds came on, followed by great rain and wind, at 10^h P.M.

1842, February 11.—Rain in the forenoon till 2^h 30^m.

Apparent Time.		Arago.
3^h 19^m	R = $24\frac{1}{2}°$ 30° above S.E. horizon.	17° 33′
3 55	R = $29\frac{1}{2}$ in zenith. Sky quite clear.	21 8
4 20	R = $29\frac{1}{2}$ in zenith. Sky quite clear.	19 45
4 38		20 5
		Babinet.
3 22	R = $28\frac{1}{2}°$ in zenith, and in N. horizon.	
3 53		16° 19′
4 23		17 0
4 36		19 34
4 5	R = $29\frac{1}{2}°$ in zenith. Neutral line convex to sun. R = $28\frac{1}{2}°$ 15° alt.	18 36

1842, February 12.—Barom. 29·3. Rainy, with wind. Cleared up at 4^h.

Apparent Time.		Arago.
4^h 18^m	Clouds passed away.	17° 28′
		Babinet.
4 20		17° 12′
3 55	R = $24\frac{1}{2}°$, but reduced to $20\frac{1}{2}°$ when watery clouds passed over the sky from W. to S.	

1842, February 15.—Rain in morning, then fine day. Wind west.*

Apparent Time.		Babinet.
4^h 25^m	R = $27\frac{1}{2}°$ in zenith to $22\frac{1}{2}°$ in S.E. horizon.	21° 58′
4 44		20 24
4 55	Barom. 30·05, therm. 43°; wind west.	20 30

Clouds came into S. horizon at 4^h 55^m, and the whole of the N. and N.E. horizon, especially above the sea, was covered 6° or 8° high with a dark band of distant haze.

N.B.—At 3^h 48^m, when the neutral point was 1° 57′ high, there was just above the sea horizon, H H, a portion *m n* of + bands, a continuation of those on the sea, so that there were two neutral points here. These were more fully developed at 3^h 58^m, as shown in a former paper.†

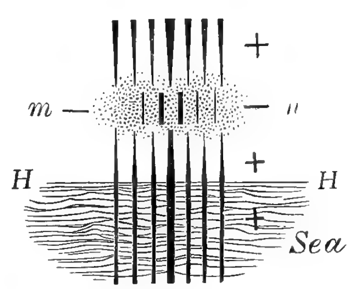


Fig. 3.

1842, February 16.—Barom. 30·16.‡

At noon, sun's alt. 21°, there is clearly a faint neutral point a little above the horizon, and 19° *below the sun*.

At 2^h 48^m, though the bands at ARAGO's neutral point are all +, as in fig. 4, they are most weakened at *m n*, which is the effect of the secondary cause. R = $19\frac{1}{2}°$ in zenith, and in both horizons at 25° alt.

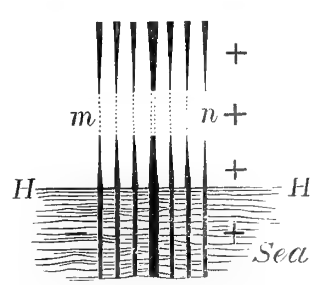


Fig. 4.

* See Edin. Trans., vol. xxiii. pp. 216, 224.
† *Ibid.*, vol. xxiii. p. 222.
‡ *Ibid.*, vol. xxiii. pp. 217, 228.

At $3^h\ 10^m$ the weak polarisation at $m\,n$ now extends down to H H. R = $22\frac{1}{2}$ in zenith, and in horizon at 25° alt.

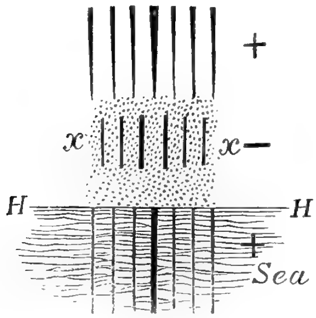


Fig. 5.

At $3^h\ 44^m$ the two neutral points are developed, as in the annexed figure, the — bands $x\,x$ being just distinctly visible.

1842, February 18.—See "Edinburgh Transactions," vol. xxiii. pp. 217, 229.

1842, February 19.—Fine day, with wind.

Apparent Time.		Arago.
$2^h\ 58^m$	R = $26\frac{1}{2}$° in S.E. horizon, alt. 20°.	
3 7	Neutral point 2° alt.	13° 38′
3 20	Secondary neutral point seen.	14 42
4 15	Fleecy clouds over neut. point. R = $24\frac{1}{2}$° S. hor.	20 0
4 40	R = $24\frac{1}{2}$° in zenith plane.	19 28

1842, February 21.—From $4^h\ 52^m$ to $4^h\ 57^m$, a secondary neutral point to that of Arago was gradually but imperfectly developed.*

1842, February 22.—Dull, cold morning, which cleared up about $1^h\ 25^m$, when R = $21\frac{1}{2}$° in clear sky, from which clouds had passed.

Apparent Time.		Arago.
$3^h\ 24^m$	R = $25\frac{3}{4}$° in zenith, $22\frac{1}{2}$° near W. and E. horizon.	15° 40′
3 38		17 29
3 44	Negative bands do not touch the sea horizon. Secondary neutral point in the horizon.	20 26
4 2		20 50
4 28		23 8
5 8		19 2
		Babinet.
4 6	Pol. of moon, R = 3°; R = $27\frac{1}{2}$° in zenith.	17° 15′
4 24		16 19
5 6		19 5

1842, February 24.—Barom. 29·0.

Apparent Time.		Arago.
$3^h\ 47^m$		17° 20′
2 32	R = $26\frac{1}{2}$° in zenith, $27\frac{1}{2}$° in S.E. horizon.	
3 51	R = $23\frac{1}{2}$ in zenith, and $25\frac{1}{2}$° at 35° alt. N.W. horizon.	

1842, February 25.—Dull day; frost in morning; cleared up at $4^h\ 2^m$.

Apparent Time.		Babinet.
$5^h\ 9^m$	R = 28° in zenith, $26\frac{1}{2}$° in E. and W. horizon.	17° 50′
		Arago.
5 12		18° 5′

* See Edin. Trans., vol. xxiii. pp. 217, 224, 228.

1842, March 2.—A wet day, the place of the sun being seen as a white spot.

Apparent Time.

2^h 20^m The polarisation everywhere extremely feeble. BABINET's neutral point was nearly 75° above the horizon, or about 54° above the sea! See March 16.

1842, March 4.—Cloudy and sunshine.

Apparent Time.			
1^h	18^m	$R=25\frac{1}{2}°$ in zenith, and $22\frac{1}{2}°$ at 30° alt. W. horizon.	
1	48	One plate of glass at 60° incidence compensates the polarisation on the sea horizon opposite the sun.	
			Arago.
3^h	53^m		
3	58		11° 13′
5	23		12 22
5	26		17 55
			Babinet.
5	38	$R=27\frac{1}{2}°$ in zenith, and $22\frac{1}{2}°$ in horizon.	17° 22′

1842, March 7.—11^h 30^m $R=28\frac{1}{2}°$ in zenith, to $18\frac{1}{2}°$ in horizon, but at 1^h 30^m, after showers of hail and rain, $R=22\frac{1}{2}°$ in zenith.

1842. March 10.—Sky clear, and wind in west.

Apparent Time			
11^h	0^m	$R=27\frac{1}{2}°$ in zenith.	
11	15	Neutral point seen below sun.	
3	14	$R=28°$ in zenith, $24\frac{1}{2}°$ in horizon.	
4	0	ARAGO's neutral point not risen.	
4	10	The + bands scarcely seen in horizon.	
4	13	$R=28°$ in zenith.	
			Arago.
4	15		12° 55′
4	20		13 56
4	27	The secondary neutral point just touching the sea horizon.	14 54

1842. March 13.—12^h 36^m $R=26\frac{1}{2}°$ in zenith. Sky clear.

Apparent Time.			Arago.
4^h	27^m	The breach in the + bands not completed.	12° 15′
4	39	But at 4^h 39^m the neutral point is formed.	12 6

Both on the 10th and 13th ARAGO's neutral point is above the horizon, though masked by the cause which produces the secondary neutral point. Over a space of $3\frac{1}{2}°$ above the sea horizon, the + bands almost wholly disappear before the — ones are perceptible, and the neutral point is distinct on the sea horizon.

1842, March 16.—Barom. 29·96, the sun occasionally shining through a thickish haze in a sky without blue. Wind slight in south-west.

Apparent Time.

10^h 45^m Polarisation the same as on March 2; BABINET's neutral point 30° above the sun, or more than 60° above the horizon!

1842, March 17.—Barom. 29·77. Much rain last night. Wind west; white clouds flying.

Apparent Time.		
10^h	20^m	$R=26\frac{1}{2}°$ in zenith plane, and soon after $24\frac{1}{2}°$.
10	50	$R=20\frac{1}{2}$ in zenith plane, and in a clear sky, over which clouds have passed.

1842, March 18.—Barom. 29·09. Wind and rain, day cold, and wind in west.

Apparent Time.			Arago.
3^h	27^m	R=25°, diminishing to 20°.	
5	28		15° 48′
5	55		18 10
6	12		17 48
			Babinet.
5	30	R=30° in zenith plane.	19° 10′
5	57		17 40
6	10		20 12

1842, March 19.—Barom. 29·0.

Apparent Time.			Arago.
3^h	54^m	Polarisation of moon, $20\frac{1}{2}$°; R=23° in zenith.	
4	39	R=24° in zenith plane; sky very clear.	10° 37′
5	44		19 45
6	14	Polarisation of moon, $20\frac{1}{2}$°; R=$22\frac{1}{2}$° in zenith.	18 47
6	19		18 45
			Babinet.
5	48	R=$26\frac{1}{2}$° in zenith plane.	19° 30′
6	12		19 45
6	21		17 26

1842, March 24.—Cleared up at 1^h. Fine day.

Apparent Time,
2^h 0^m R=$22\frac{1}{2}$° 50° above the horizon.
4 0 R=$20\frac{1}{2}$° in zenith plane.

1842, March 26.—Barom. 29·3. Cold wind from point north of west.

Apparent Time.			Arago.
5^h	49^m		18° 32′
6	16		19 25
6	31		18 4
			Babinet.
5	52	R=$28\frac{1}{2}$° in zenith plane.	19° 20′
6	34		19 40

1842, March 28.—Sky clear.

Apparent Time.
4^h 20^m R=$26\frac{1}{2}$° in zenith plane, and 18° and 20° in horizon.

1842, March 29^h.

Apparent Time.		Arago.
6^h	20^m	17° 55′
6	36	17 50
		Babinet.
6	18	18° 40′

1842, March 30.—Wind; flying clouds.

Apparent Time.			Arago.
6^h	27^m	R=$28\frac{1}{2}$° in zenith, to $23\frac{1}{2}$° in horizon.	18° 13′
6	45		17 55
6	52		19 37
6	24		
			Babinet.
6	48		20° 40′
3	10	R=$21\frac{1}{2}$° in zenith plane.	19 54

1842, April 2.

Apparent Time.	Arago.
6h 5m	19° 15′
7 4	19 42
	Babinet.
6 36	18° 5′
6 38	19 8
7 4	20 18

1842, April 3.—Fine clear sky; hail in the afternoon.

Apparent Time.

11h 45m { BREWSTER's neutral point most distinctly seen. Distance from sun, } 13° 0′

1842, April 5, 6, 8.—See "Edinburgh Transactions," vol. xxiii. pp. 217, 225, and 229.

1842, April 9.—Barom. 30·16. Wind east; bitterly cold.

Apparent Time.		Arago.
3h 25m	R=22½° in zenith plane. The sky clear.	
5 46		22° 7′
6 55	Effect of fog.	15 20
7 0		16 2
		Babinet.
5 49	R=15½° in horizon.	17° 10′
6 57		19 30

Between 5h 49m and 6h 57m a fog came on, and there were no neutral points.

1842, April 10.

Apparent Time.

4h 5m R=18½° in zenith, and 14½° in horizon.

4 22 R=27 in zenith, and 14½ in horizon.

BABINET's neutral point very near the sun, and no neutral point seen below the sun.

1842, April 13.—Barom. 30·12. Fine day.*

Apparent Time		Arago.
5h 48m		16° 20′
6 20		17 55
6 54		19 40
7 10		19 45
7 19	R=30½° in zenith.	19 4
7 29	R=32½° in zenith.	22 10

R increased from 25° at 4h to 32½° at 7h 29m.

1842, April 15.—See "Edinburgh Transactions," vol. xxiii. p. 230. Haze from west.

Apparent Time.		Arago.
5h 40		16° 25′
5 48		18 40
5 57	R=14½°, in zenith 18½°, 20° above N. horizon.	18 21
6 28	White nebulosity.	18 50
7 0		
		Babinet.
5 50		28° 20′

* See Edin. Trans., vol. xxiii. p. 225.

1842, April 16.

Apparent Time.		Arago.
5^h 54^m		17° 25′
6 31		22 22
6 59	R=$27\frac{1}{2}$° in zenith, $22\frac{1}{2}$° in N. and $18\frac{1}{2}$° in S. horizon.	19 50
7 21		18 55
7 35	R=$18\frac{1}{2}$° at moon.	0 45
7 46		23 50
		Babinet.
5 58	R=$23\frac{1}{2}$° in zenith, much less in horizon.	25° 40′
6 33	R=23° in zenith, 21° in W. and $18\frac{1}{2}$° in E. horizon.	19 8
6 57		20 3
7 23	R=$29\frac{1}{2}$° in zenith, and $24\frac{1}{2}$° in both horizons.	17 45
7 37	R=31° in zenith, and $29\frac{1}{2}$° in horizon.	17 35

1842, April 17.—Slight haze.

Apparent Time.		
3^h 30^m	BREWSTER's neutral point distinctly seen, R=$19\frac{1}{2}$° in zenith, and 12° in S. horizon.	
		Arago.
5 34	R=$17\frac{1}{2}$° in zenith, $22\frac{1}{2}$° in horizon ; clear sky.	20° 10′
		Babinet.
6 3	BABINET's neutral point just risen.	19° 30′
7 35	R=$28\frac{1}{2}$ in zenith plane.	

1842, April 18.—Barom. 30·0.

Apparent Time.		Babinet.
7^h 14^m	Pol. moon=$27\frac{1}{2}$°, $29\frac{1}{2}$° in zenith.	18° 45′
		Arago.
7 23	Pol. moon=$28\frac{1}{2}$°, $30\frac{1}{2}$° in zenith.	17° 45′

1842, April 19.

Apparent Time.		Arago
7^h 17^m		16° 35′
		Babinet.
7 20	Pol. moon $14\frac{1}{2}$° and a maximum.	18° 15′
7 37	$22\frac{1}{2}$°, and maximum $26\frac{1}{2}$°.	19 10

1842, April 20.—See "Edinburgh Transactions," vol. xxiii. pp. 218, 230.

1842, April 21.— " " " p. 222.

Apparent Time.		
11^h 30^m	R=$23\frac{1}{2}$°, maximum at 90° from sun.	
1 10	R=$25\frac{1}{2}$°, maximum at 90° from sun.	
		Arago.
7 20		17° 55′
7 48	A fog came on.	14 24
		Babinet.
7 22		20 15

1842, April 22.—Fine day.

Apparent Time.			
11^h	10^m A.M.	R=$17\frac{1}{2}$°; maximum polarisation in zenith 90° from sun.	
			Brewster.
2	0	R=27° at 90° from sun in zenith plane.	12° 10′
3	0	R=29 at 88 from sun in zenith plane.	11 10
4	10	R=$29\frac{1}{2}$° at 90° from sun.	
			Arago.
6	55		20° 25′
			Babinet.
6	58	Thin clouds.	22° 15′

1842, April 24.—Barom. 29·84, rising.

Apparent Time.			
3^h	0^m	A haze. BABINET's neutral point 70° high; BREWSTER's not yet risen. After the haze had increased the sky cleared.	
6	21	Altitude of ARAGO's neutral point above horizon.	8° 40′
6	10	,, ,, ,,	13 0
6	10	Secondary neutral point exactly in horizon.	

1842, April 25, 26, 27, 28, 29.—See "Edinburgh Transactions," vol. xxiii. pp. 218, 230.

1842, May 2.—Barom. 29·93; wind east.

Apparent Time.			Arago.
6^h	18^m		23° 45′
7	1	R=$22\frac{1}{2}$° zenith to $18\frac{1}{2}$° in horizon.	24 45
7	45		20 15
			Babinet.
6	21	R=$28\frac{1}{2}$° maximum in zenith plane.	16° 30′

1842, May 3.*—China-ink sky; wind east.

Apparent Time.			
9^h	0^m A.M.	R=$19\frac{1}{2}$° maximum polarisation in zenith to $14\frac{1}{2}$° in horizon.	
11	0	R=$19\frac{1}{2}$° maximum polarisation in zenith to $14\frac{1}{2}$° in horizon; bands ragged.	
			Arago.
11	29	R=20°; maximum at 89° from sun.	10° 15′
12	15	R=20°.	9 25

1842, May 4.

Apparent Time.			Arago.
5^h	56^m		
6	2	The — bands of *secondary* neutral point distinct, and this point commenced.	17° 34′
6	13		20 46
6	16	The secondary neutral point distinctly formed.	21 1

* See Edin. Trans., vol. xxiii. p. 230.

1842, May 9.—Barom. 29·83.

Apparent Time.		Arago.
5^h 42^m	$R=22\frac{1}{2}°$ in zenith plane.	
6 7	Bands all positive.	19° 15′
6 18	Positive bands still in horizon.	
6 39		22 47′
7 28		23 21
		Babinet.
6 45		15° 0′
7 30	$R=19\frac{1}{2}°$ in zenith plane.	16 5

1842, May 15, 16, 17.—See "Edinburgh Transactions," vol. xxiii. pp. 218, 230, 231.

1842, July 16.—At Lacock Abbey, Wiltshire.

$R=23°$ in a singularly fine day, this low polarisation indicating nebulosity, which collected and produced rain.

1842, August 2.—At St Andrews; Barom. 30·0.

Apparent Time.		Arago.
5^h 9^m	$R=20\frac{1}{2}°$ in zenith, and $14\frac{1}{2}°$ in horizon.	
5 27	The bands opposite the sun begin to weaken, and there is a second neutral point.	
6 15	Negative bands distinctly seen.	
6 0		20° 36′
6 26		25 1!
7 29		23 1
7 54		22 41
9 13		33 40!
		Babinet.
6 32		13° 40′
6 39	ARAGO's secondary neutral point distinctly formed. A dark band along the horizon, below ARAGO's neutral point.	

1842, August 4.—Slight rain in morning; Barom. 29·5; wind west.

Apparent Time.		
5^h 54^m	$R=25\frac{1}{2}°$ near horizon.	
6 41	ARAGO's secondary neutral point in horizon.	
7 6	$R=29\frac{1}{2}°$ in clear blue sky.	
7 9	Altitude of ARAGO's neutral point above horizon,	15° 40′

1842, August 5.—Rain in forenoon. I observed a singular sky in the west, to the north of the sun and below him. The whole sky, from A A to the horizon H H, was clear, but the part A A was darker than B B, and of a deep China-ink blue, while B B was much paler. But, what was singular, these differently coloured spaces were separated by an irregular line *m m m*, showing that the whole space *m m m* H H was a thin sheet of cloud or vapour, terminating abruptly at *m m m*.

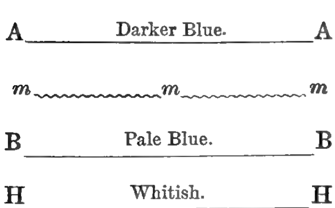


Fig. 6.

Apparent Time.		Arago.
5^h 40^m	$R=17\frac{1}{2}°$, maximum polarisation at alt. 40°.	
7 21	$R=27\frac{1}{2}°$ in zenith plane, and $26\frac{1}{2}°$ at alt. 40°.	17° 10′
7 25	A cloud approaching the neutral point.	16 35
7 40	$R=28\frac{1}{2}°$, maximum in zenith plane.	19 30

1842, August 6.—Barom. 29·6; rain at 5^h P.M.

Apparent Time.		
7^h 50^m	R=28½° max. in zenith plane to 24½° in horizon.	
7 56	Altitude above horizon of ARAGO's neutral point.	19° 10′

Clouds around the blue space.

1842, August 11.—Barom. 29·62; rain in morning.

Apparent Time.		
5^h 30^m	R=23½° in zenith plane, and 20½° in S. horizon.	
5 51	+ Bands opposite sun almost gone at 1¾° above hor.	
		Arago.
6 14		6° 40′
6 46		14 35
7 40		20 35
8 4	Altitude of the two neutral points above the horizon.	22 20
		Babinet.
7 43		18° 25′
8 10	R=29° in zenith.	12 0

1842, August 17.—See "Edinburgh Transactions," vol. xxiii. p. 231.

1842, August 22.—Warm; fine day.

Apparent Time.		
2^h 20^m	R=24½° maximum polarisation in zenith plane to 19½° in horizon.	
		Arago.
5 52		18° 36
6 37	R=27½° in zenith plane.	21 16
7 26	R=28½ in zenith plane.	19 56
7 49		22 8
		Babinet.
7 52		13° 8′

1842, August 28.—See "Edinburgh Transactions," vol. xxiii. p. 231.

1842, September 9.—Barom. 29·1, after rain.

Apparent Time.		
5^h 53^m	R=26½° maximum polarisation in zenith, to 25½° in horizon.	
		Arago.
6 38		18° 57′

1842, September 13.—Barom. 29·93. Fine day.

Apparent Time.		
4^h 39^m	R=28° maximum polarisation in zenith plane.	
		Arago.
4 50	R=27½° maximum polarisation in 30° alt. N.W. horizon, to 24½° in horizon. In S. horizon, alt. 30° 26½° to 22½° in horizon.	15° 35′
5 29		18 35
5 58	R=29½° maximum polarisation. Pol. of moon 19½°.	20 17
6 36		20 0
7 2		16 24
		Babinet.
5 31	R=29° maximum polarisation in zenith.	14° 25′
5 56		15 40
6 38	R=29½° maximum polarisation in zenith.	16 6
7 6		19 8

1842, September 17.—Barom. 29·45, after a rainy day.

Apparent Time.		Arago.
$6^h\ 48^m$ P.M.		17° 20′
		Babinet.
6 53	R=28½° maximum polarisation in zenith.	16° 25′

1842, September 18.—Fine blue sky. Barom. 29·57.

Apparent Time.		
$3^h\ 0^m$	R=21½° in zenith plane to 19½° in horizon.	
3 45	R=22½ in zenith plane to 20½ in horizon.	
4 20	R=25° in zenith plane to 23° in horizon.	
4 46	Fringes all + opposite sun and in horizon.	
4 47		Arago.
5 6		15° 28′
5 36		16 36
5 56		20 16
5 21	R=27½° max. polarisation in zenith to 24½° in hor.	19 40

1842, September 28.—Fine day.

Apparent Time.		Arago.
$4^h\ 32^m$	R=28° maximum polarisation in zenith plane.	
4 58		18° 36′

1842, September 29.—Fine day; cold; wind east.

Apparent Time.		Arago.
$4^h\ 37^m$	R=30½° maximum polarisation in zenith.	
5 11		17° 34′

1842, September 30.—Fine day.

Apparent Time.		
$4^h\ 18^m$	R=29½° max. polarisation in zenith to 26½° in hor.	
4 24	Neutral point not risen. Bands +.	
		Arago.
4 55		16° 11′
5 47		18 18
		Babinet.
4 58		15° 23′
5 44	R=29½° maximum polarisation in zenith plane.	17 28

1842, October 15.—Fine day. Barom. 30·0, rising.

Apparent Time.		Arago.
$4^h\ 32^m$	R=29½° maximum polarisation in zen. to 26½° in hor.	18° 0′
5 11		18 11
5 42		25 30 !
		Babinet.
4 34		15° 23′
5 9		17 26
5 46		20 0

1842, October 19.—Barom. 29·3. Cold.

Apparent Time.		Arago.
$4^h\ 56^m$	R=27½° maximum polarisation in zenith plane.	19° 11′
5 5		20 9
		Babinet.
5 8		16° 39′

1842, October 20.—Barom. 29·62. Fine day; cold.

Apparent Time.		Arago.
4^h 15^m	R=$26\frac{1}{2}$° near S. horizon. Clouds in zenith.	14° 46′

1842, October 21.

Apparent Time.		Arago.
4^h 49^m	Neutral point above a cloud.	18° 13′

1842, October 24.—Barom. 29·33. Rain in the morning; cold.

Apparent Time.		
4^h 28^m		Arago. 18° 46′
4 32	R=27° maximum polarisation in zenith plane.	Babinet. 21° 59′

1842, November 9.—Barom. 29·06, after a storm of wind and rain.

Apparent Time.		Arago.
4^h 41^m	Polarisation of moon in S. horizon $25\frac{1}{2}$°.	17° 20′

1842, November 14.—See "Edinburgh Transactions," vol. xxiii. pp. 218, 226.

1842, November 15.—Barom. 29·72. Hard frost in morning.

Apparent Time.		Arago.
2^h 12^m	Secondary neutral point in horizon.	17° 50′

1842, November 20, 21.—See "Edinburgh Transactions," vol. xxiii. pp. 219, 226.

1842, November 27.—Barom. 29·15, rising. A dark band along the horizon.

Apparent Time.	
3^h 57^m	R=27° in zenith, and $18\frac{1}{2}$° in horizon. Arago's secondary neutral point in horizon, and primary one considerably up.

1842, December 3.—Barom. 29·95. Fine day.

Apparent Time.		
12^h 46^m	Arago's neutral point not risen.	
12 46	R=$27\frac{1}{2}$° maximum polarisation in zenith, $20\frac{1}{2}$° in hor.	
2 16	R=$28\frac{1}{2}$° maximum polarisation in zenith, $18\frac{1}{2}$° in hor.	Arago. 19° 20′
4 23		24 22
4 28		Babinet. 20° 15′

1842, December 17.—Barom. 29·58, rising after rain.

Apparent Time.		
3^h 16^m		Arago. 18° 5′
3 19	R=29° maximum polarisation in zenith.	Babinet. 17° 50′

1842, December 18.—Barom. 29·79. Raining occasionally.

Apparent Time.	Arago.
11^h 13^m	13° 45′
12 23	14 42

1842, December 22.—Barom. 29·38, falling. Rain.

Apparent Time.		Arago.
2ʰ 43ᵐ		17° 50′
		Babinet.
2 46	R=29° maximum polarisation in zenith plane.	16° 10′

1842, December 23.—Barom. 29·01, after rain.

Apparent Time.		Arago.
3ʰ 0ᵐ		18° 16′
		Babinet.
3 3	R=30° maximum polarisation in zenith plane.	19° 4′

1842, December 24.—Barom. 29·33.

Apparent Time.		Arago.
12ʰ 3ᵐ	R=26½° maximum polarisation in zenith plane.	15° 28′
12 44	R=28° " "	15 5
1 32	R=27 " "	17 30
2 24	R=29 " "	18 40
3 8	R=27½° " "	17 52
3 37	R=29½ " "	19 14
		Babinet.
2 27	The point of maximum polarisation rising as the sun's altitude diminishes.	18° 28′
3 5		20 6
3 39		17 55 !

1842, December 26.—Barom. 28·88.

Apparent Time.		Arago.
1ʰ 3ᵐ	R=26½° maximum polarisation, alt. 30°.	16° 25′
3 27		17 15
		Babinet.
3 30	R=28° maximum polarisation in zenith plane.	17° 40′

1842, December 27.—Barom. 29·35. Sun shining through a dry haze, which continued all day. The lines in the sun's spectrum singularly sharp.

Apparent Time.		Arago.
11ʰ 48ᵐ	R=26½° maximum polarisation in zenith plane.	15° 50′
12 4	R=23½ maximum polarisation in zenith plane, 14½° in horizon.	17 15
1 45	R=24° maximum polarisation in zenith plane.	20 25
2 35	R=27½° " "	21 30
3 15	R=29½ " "	19 0
		Babinet.
12 9		12° 25′
1 50		14 40
2 38		16 5

1842, December 28.—See "Edinburgh Transactions," vol. xxiii. pp. 219, 226.

1842, December 29.—Barom. 29·50. Clear in north.

Apparent Time.		Arago.
11ʰ 40ᵐ		15° 25′
12 0		15 29
		Babinet.
11 44	R=26½° maximum polarisation in zenith plane. Slight clouds. R=28½° alt. 50°. Sky clearer.	17° 49′

1842, December 30.—Boisterous day; thin white clouds.

Apparent Time.		Arago.
2h 0m	R=22½° maximum polarisation in zenith plane.	
2 24		15° 5′

1842, December 31.—Windy, and sky cloudy.

Apparent Time.		Arago.
11h 53m	R=27½° maximum polarisation in zen. Sky impure.	16° 40′

1843, January 4.—Fine day.

Apparent Time.		Arago.
11h 32m	R=27½° maximum polarisation in zenith.	14° 41′
11 43		14 30
		Babinet.
11 35	R=23½° to 18°. Haze coming on.	15° 47′
1 51	Altitude of ARAGO's neutral point above hor. 4° 30′. R=24½° to 18½°. Haze to east and south.	

1843, January 5.—At Rankeilour M'Gill; clear and cold.

Apparent Time.		Arago.
11h 27m	R=28½° maximum polarisation, 18½° in horizon. Neutral line convex to sun.	14° 44′

1843, January 10.—Barom. 28·5; very cold.

Apparent Time.		Arago.
2h 3m	R=29° maximum polarisation in zenith plane.	14° 25′
2 41	Polarisation of moon 21½°, 24½° in horizon.	

1843, January 11.—Barom. 28·72. Fine day.

Apparent Time.		Arago.
12h 35m	R=26½° maximum polarisation. Nebulosity in zenith.	15° 15′
1 47	R=29½° maximum polarisation in zenith. Clear sky.	
2 55	R=28° maximum polarisation. Slight nebulosity.	19 50
3 10	Polarisation of moon 15°.	

1843, January 21.—A misty day.

Apparent Time.		Arago.
2h 46m	R=19½° maximum polarisationin zenith 30° at 14° alt.	16° 40′
3 18	Misty.	10 6

The mist increased, and the maximum polarisation everywhere reduced to 14½°, and the two neutral points descended to the horizon several degrees.

1843, January 28.—Barom. 29·45. Very windy.

Apparent Time.		Arago.
12h 28m	Neutral point below horizon.	14° 10′
12 47	„ „	14 0
1 57	„ „	11 5
3 51	Misty.	22 41!
		Babinet.
3 54	R=28½° maximum polarisation to 20½° in horizon.	
12 28	R=26½ „ „ to 18½ „	14° 45′

1843, January 30.—Barom. 29·50. Windy.

Apparent Time.			Arago.
12^h	18^m	Neut. point below horizon, R=21°, maximum clear.	15° 40′
1	47	Neut. point below horizon, R=24½° in zenith to 14½° in horizon.	12 25
2	28		11 29
3	52	R=28½° maximum polarisation in zenith, to 21½° in horizon.	16 29
4	2		18 32
			Babinet.
3	55		19° 0′
3	10	R=27½° maximum polarisation in zenith, to 21½° in horizon.	18 25

1843, February 15.—Very cold day; clear only in north.

Apparent Time.		Arago.
2^h	57^m	16° 5′

1843, February 16.—Barom. 29·18, rising. Fine sky.

Apparent Time.			Babinet.
11^h	56^m	R=25° maximum polarisation, 20° in horizon.	
12	57		9° 40′
			Brewster.
12	57		11° 25′
			Arago.
3	28	R=29½° maximum polarisation in zenith, 24½° in horizon.	12° 52′
4	3	R=29½° maximum polarisation in zenith, 26½° in horizon.	18 40
5	0	R=30° ,, ,,	17 52
4	7		14 52
5	4		17 26

1843, February 17.—Barom. 29·6. Sky not clear in N. horizon.

Apparent Time.		Arago.
3^h	6^m	11° 28′
3	30	14 14

1843, March 4.—Barom. 30·08.

Apparent Time.			Arago.
3^h	35^m	R=14½° maximum polarisation in zenith plane.	16° 45′
3	46		17 30
3	54	Secondary neut. point 50′ high. Hazy in horizon.	17 57
4	6		20 30

1843, March 7.

Apparent Time.			Arago.
4^h	40^m	R=24½° maximum polarisation in zenith.	17° 0′

1843, March 8.—Barom. 30·13. Wind east; hazy.

Apparent Time.			Arago.
4^h	14^m	R=24½° maximum polarisation.	17° 47′

1843, March 12.—Barom. 29·34, after rain.

Apparent Time.		Arago.
$4^h\ 2^m$	R=25½° maximum polarisation. Polarisation of moon 7°.	13° 15′

1843, March 25.—See "Edinburgh Transactions," vol. xxiii pp. 220, 226.

1843, March 28.—Barom. 29·84. Wind east; dry.

Apparent Time.		Arago.
$6^h\ 2^m$		16° 45′

1843, March 29.—Barom. 29·88. Fine day, cold; wind east; dry.

Apparent Time.		Arago.
$3^h\ 51^m$	R=28½°, 25½° in S. and 26½° in W. horizon.	
5 58		18° 35′
6 26		19 33
		Babinet.
6 1	R=29½° maximum polarisation in zenith, 28° in hor.	18° 22′
6 24	R=29½° ,, ,, ,,	18 27

1843, April 7.

Apparent Time.		Arago.
$6^h\ 54^m$		19° 0′
		Babinet.
6 48		16° 29′

1843, April 10.—Barom. 29·74. Very cold wind, north-west.

Apparent Time.		Arago.
$6^h\ 23^m$		20° 42′
		Babinet.
6 25		22° 23′

1843, April 11.—Barom. 29·80. Very cold.

Apparent Time.		Arago.
$6^h\ 43^m$	R=28½° maximum polarisation. Sky whitish blue.	17° 30′
6 48		17 45

1843, April 12.—Barom. 29·77.

Apparent Time.		Babinet.
$4^h\ 35^m$		41° 35′ !

The sky was covered with a thick haze; the sun barely seen through it, and showers of hail falling occasionally. The bands a maximum above the sun, but disappeared 9° above horizon. A neutral point was seen at 25° alt. opposite the sun, but the bands below it seemed + ! though extremely faint.

1843, April 17.—See "Edinburgh Transactions," vol. xxiii. p. 226.

Apparent Time.		Arago.
$6^h\ 30^m$	R=25½° maximum polarisation in zenith, 20½° in hor.	22° 32′
7 4	R=29 ,, ,, ,,	20 44
7 30	R=29½ ,, ,, ,,	20 10

1843, April 19.—Nebulosity in zenith. Clouds round horizon.

Apparent Time.		Arago.
7^h 30	R = $25\frac{1}{2}°$ maximum polarisation in zenith plane.	18° 35′
		Babinet.
7 33		19° 0′

1843, April 28.—Barom. 29·44, after rain. Clear sky after clouds had cleared away.

Apparent Time.		Arago.
7^h 9^m		20° 23′
7 40		19 37
		Babinet.
7 3	R = 29° maximum polarisation in zenith plane.	16° 46′
7 44	R = 29 maximum polarisation in zenith, $20\frac{1}{2}°$ in hor.	18 0

1843, April 29.—See "Edinburgh Transactions," vol. xxiii. pp. 226–27.

1843, April 30.—Barom. 30·07. Morning, and rising. Not a cloud.

Apparent Time.		Arago.
1^h 0^m	R = $26\frac{1}{2}°$ maximum polarisation in zenith, $22\frac{1}{2}°$ in horizon.	
4 15	R = $26\frac{1}{2}$ " " "	
5 43	Secondary neutral point forming.	
		Arago.
6 4		11° 20′
6 52	R = 29°. maximum polarisation in zenith, $22\frac{1}{2}°$ in hor.	19 20
7 33	R = $29\frac{1}{2}$ maximum polarisation in zenith, $22\frac{1}{2}$ in hor.	19 35
7 58		19 3

A brown haze rising up upon the blue sky. See "Edinburgh Transactions," vol. xxiii. p. 231.

1843, May 2.—Barom. 30·07. Wind east; no sun.

BABINET'S neutral point near zenith, and polarised bands scarcely seen, excepting at 90° from sun.

1843, May 3.—See "Edinburgh Transactions," vol. xxiii. p. 227.

Apparent Time.		Arago.
6^h 49^m		18° 26′
		Babinet.
6 53	R = 30° maximum polarisation in zenith, 27° in hor.	14° 20′

Very clear in zenith, with a whitish sky.

1843, May 6.—Wind east; uniform China-ink clouds over the sky, through which the sun shone brightly, but ill-defined.

4^h 43^m.—Polarised bands distinct over the face of the sun and above him, but exceedingly feeble opposite the sun.

1843, May 11.—Barom. 30·0. Fine day.

Apparent Time.		Arago.
2^h 30^m	R = $20\frac{1}{2}°$ maximum polarisation in zenith plane.	
6 0	Neutral point not up.	14° 34′
6 12	R = $23\frac{1}{2}°$ maximum polarisation in zenith.	18 15

Whitish-blue sky. White clouds in horizon.

1843, June 13.—Barom. 30. Wind east.

Apparent Time.		Arago.
6^h 56^m		19° 15′
9 10		20 25
		Babinet.
7 0	R=27½° maximum polarisation in zenith plane.	25° 10′
9 13	R=29½ in zenith, horizon clear.	15 30

1843, June 14.—Barom. 30·07. Splendid day; wind east.

Apparent Time.		Babinet.
6^h 12^m	R=29½° maximum polarisation in zenith, 21½° in hor.	13° 30′
7 59	R=29½° ,, ,, 22½ in hor.	15 30
		Arago.
7 0	R=29½° maximum polarisation in zenith, 22½° in horizon. Neutral point in horizon.	11° 5′
7 56		18 20

1843, June 15.—See "Edinburgh Transactions," vol. xxiii. p. 231.

1843, June 16.—Barom. 30·0. Sky covered with white nebulosity.

Apparent Time.		
12^h 20^m	R=25½° maximum polarisation in zenith plane.	Arago.
7 0		10° 45′
7 38		17 50
		Babinet.
7 41	R=29½° maximum polarisation in zenith plane.	15° 55′

1843, June 21.—See "Edinburgh Transactions," vol. xxiii. pp. 220, 227,

1843, June 22.—Barom. 29·90. Wind east; fine day.

Apparent Time.		Arago.
8^h 42^m	Antisolar point in horizon. Clouds in zenith.	19° 15′

1843, June 23.—Barom. 29·92. Fine day.

Apparent Time.		Arago.
7^h 17^m	R=25° maximum polarisation in zenith, 18½° in hor.	15° 35
7 26		17 30
8 58		18 30
		Babinet.
7 30		15 10
9 0	R=27½° maximum polarisation in zenith, 22° in hor.	16° 50′

1843, June 24.—Barom. 29·92.

Apparent Time.	Arago.
7^h 8^m	17° 47

1843, June 26.

Apparent Time.		Arago.
7^h 25^m		21° 6′
8 52	Neutral point in the middle of a bright orange cloud.	17 33

1843, July 3.—Barom. 29·57, rising.

Apparent Time.		Arago.
7^h 29^m	R=26° maximum polarisation in zenith, 20½° in S. hor.	16° 28′

1843, July 6.

Apparent Time.		Arago.
6^h 50^m	R=28½° maximum polarisation in zenith, 22½° in hor.	11° 45′
7 1		14 15
7 21	R=18½° polarisation of moon.	17 32
8 13	R=29° maximum polarisation in zenith plane.	19 30
8 55		18 15
		Babinet.
7 25		13 40
8 15	R=28½° polarisation of moon.	16 3
8 52		17 11

1843, July 11.—Barom. 30·0, rising.

Apparent Time.		Arago.
6^h 55^m	R=29½° maximum polarisation in zenith; clear sky.	12° 28′
7 51	White clouds forming in many places.	21 11 !
		Babinet.
7 54		19° 10′

1843, July 21.—Barom. 29·5; no rain.

Apparent Time.		Arago.
7^h 34^m	Thin clouds in zenith and near sun.	20° 4′

1843, July 24.—Barom. 29·87, rising; no rain.

Apparent Time.	Arago.
6^h 58^m	17° 52′

1843, August 6.—Barom. 29·77, after a wet day.

Apparent Time.		Arago.
7^h 27^m	R=29½° maximum polarisation in zenith; sky clear.	18° 49′
		Babinet.
7 49		17° 39′

1843, August 9.—Fine day; rain yesterday.

Apparent Time.	Arago.
7^h 37^m	19° 34′
8 3	18 17

1843, August 10.—Splendid day; haze in zenith; slight white clouds. Barom. 29·97; hot.

Apparent Time.	
11^h 38^m	R=24° maximum polarisation in zenith plane.

1843, August 19.—Barom. 29·6, falling; haze all forenoon.

Apparent Time.		Arago.
7^h 7^m		18° 53′
7 51		18 30
		Babinet.
7 23	R=27½° maximum polarisation.	17° 50′

1843, September 6.—Barom. 30·05; splendid, hot day.

Apparent Time.		Arago.
7^h 11^m		23° 10′
		Babinet.
7 18		16° 32′

1843, September 9.—Barom. 30·05; fine day.

Apparent Time.		Arago.
6^h 55^m		19° 25′
		Babinet.
7 0		17° 23′

1843, September 13.—Barom. 29·98; fine day.

Apparent Time.		Arago.
5^h 6^m	Neutral point in horizon; R=25° maximum polarisation in S. horizon.	10° 55′
6 48		Babinet.
6 53	R=30° maximum polarisation in zenith, 23½° in S. hor.	16° 26′

1843, September 20.—Barom. 29·80; no rain.

Apparent Time.		Arago.
6^h 14^m	Clouds W., clear E.; R=23½° zenith, 24½° horizon.	17° 42′

1843, September 21.—Barom. 30·0, rising.

Apparent Time.		Arago.
5^h 46^m	R=30° maximum polarisation in zenith, 25½° in horizon; not a cloud in the sky.	18° 59′
6 3		
6 28		17 55
		Babinet.
5 51		18 54
6 31		18 43

1843, September 22.—Barom. 30·28, rising; thick in horizon, with a brownish red light.

Apparent Time.		Arago.
6^h 1^m		18° 22′
6 7	R=29½° maximum polarisation in zenith, 22½° in hor.	19 58

1843, October 31.—Barom. 29·39; fine sunny day.

Apparent Time.		Arago.
4^h 9^m		15° 40′
4 22		17 56
4 59		20 49
		Babinet.
4 24	R=29½° maximum polarisation in zenith, 25½° in hor.	17° 8′
4 45	Polarisation of moon 24½°.	

1843, November 7.—Barom. 29·30, rising; fine day.

Apparent Time.		Arago.
4^h 29^m	R = $29\frac{1}{2}°$ maximum polarisation in zenith, $26\frac{1}{2}°$ in S. hor.	18° 22′

1843, November 14.—Barom. 30·13, rising; fine day; wind north-west by west.

Apparent Time.		
4^h 12^m		Arago. 18° 33′
4 17	R = 28° maximum polarisation in zenith plane.	Babinet. 18° 45′

1843, November 20.—Barom. 29·27, rising; cold day.

Apparent Time.	
4^h 36^m	Arago. 21° 32′
4 37	Babinet. 18° 13′

1843, November 29.—Fine day.

Apparent Time.	Arago.
3^h 31^m	14° 14′

1843, December 1.—Barom. 29·94; fine day.

Apparent Time.	
4^h 10^m	Arago. 18° 8′
4 12	Babinet. 19° 26′

1843, December 6.—Fine day; wind west; clear sky everywhere.

Apparent Time.		
3^h 3^m		Arago. 16° 58′
3 7	R = $28\frac{1}{2}°$, 90° from sun.	Babinet. 18° 11′

1844, January 23.—Barom. 27·8, rising; fine day.

Apparent Time.		
3^h 34^m		Arago. 18° 51′
3 37	R = 27° maximum polarisation in zenith plane.	Babinet. 17° 31′

1844, January 25.—Barom. 29·84; rain till 2^h P.M.

Apparent Time.		
4^h 20^m		Arago. 18° 16′
4 26	R = 28° maximum polarisation in zenith.	
4 38	Polarisation of moon $17\frac{1}{2}°$.	Babinet. 19° 0′

1844, February 3.—See "Edinburgh Transactions," vol. xxiii. pp. 220, 227.

1844, February 6.—Snow on ground.

Apparent Time.			Arago.
3^h	13^m		19° 1′
			Babinet.
3	19	R=25° maximum polarisation in zenith plane.	17° 58′
4	2	R=27 maximum polarisation in zenith, 19½° in hor.	

1844, February 7.—Clouds over zenith; snow.

Apparent Time.		Arago.
2^h	18^m	18° 13

1844, February 16.—Barom. 29·74; wind west; fine day.

Apparent Time.			
12^h	14^m	R=26½° maximum polarisation, to 19½° in horizon.	
1	58	R=26½ „ „ 20½ „	
3	1	R=23½ „ „ 18½ „	
			Arago.
3	14	Neutral point in horizon.	
3	20		10° 14′
3	24	Secondary neutral point in horizon.	14 33
3	33	Sky clouded.	15 31
4	6	Secondary neutral point disappears.	
			Babinet.
4	32		19° 49′

1844, February 21.—See "Edinburgh Transactions," vol. xxiii. p. 220.

1844, February 27.—Barom. 29·12., rising. Thaw.

Apparent Time.			Arago.
3^h	16^m	Polarisation of moon 17½°.	
3	44		14° 10′

1844, March 7.—Barom. 30·03, rising. Fine day.

Apparent Time.			Arago.
4^h	48^m	White nebulosity in zenith; R=13° maximum polarisation.	19° 20′

1844, March 27.—Barom. 29·70, rising.

Apparent Time.			Arago.
5^h	40^m	Polarisation of moon 22½°.	20 40′

1844, April 11.—Fine day and fine evening; wind west.

Apparent Time.			Arago.
6^h	23^m	R=27° maximum polarisation, 22½° in horizon.	18° 23′
7	1		18 37
			Babinet.
6	24		20° 50′
7	3		19 19

1844, April 22.—Fine day. Barom. 29·80.

Apparent Time.		Arago.
6^h 29^m	R=$27\frac{1}{2}$° maximum polarisation, $22\frac{1}{2}$° in S. horizon.	19° 31′
7 15		20 34
		Babinet.
6 37		21° 32′
7 18	R=$28\frac{1}{2}$° maximum polarisation, $22\frac{1}{2}$° in S. horizon.	17 28

1844, April 24.—Barom. 29·87. Fine day; windy.

Apparent Time.	Arago.
6^h 1^m	16° 55
6 38	20 30
	Babinet.
6 3	15° 58′

1844, April 26.—Barom. 29·84, rising.

Apparent Time.	Arago.
6^h 24^m	19° 20′

1844, April 27.—Barom. 30·07, rising.

Apparent Time.		Arago.
7^h 1^m	R=14°	20° 50′
		Babinet.
7 2	A great whiteness in the sky.	23° 22′

1844, May 2.—Barom. 30·25.

Apparent Time.		Arago.
5^h 52^m		21° 5′
6 0	R=$15\frac{1}{2}$° maximum polarisation in zenith, $14\frac{1}{2}$° in hor.	22 25
6 33	R=17 „ „ 14 „	23 20
7 22	R=20 „ „ $14\frac{1}{2}$ „	22 25
		Babinet.
6 5		13° 40′

1844, May 3.—See "Edinburgh Transactions," vol. xxiii p. 231.

1844, May 7.—Barom. 29·80. Whitish sky.

Apparent Time.		Arago.
7^h 3^m	R=$26\frac{1}{2}$° maximum polarisation, $20\frac{1}{2}$° in horizon.	20° 2′
		Babinet.
7 5		16° 30′

1844, May 15.—Barom. 30·0, falling. Fine day.

Apparent Time.	Arago.
6^h 36^m	16° 50′
6 52	20 5
7 46	21 40
	Babinet.
6 50	16° 10′
7 51	21 15

1844, June 3.—Barom. 29·86. Wind west.

Apparent Time.		Arago.
8^h 46^m		20° 58′
		Babinet.
8 52		20° 35′

1844, June 10.—See "Edinburgh Transactions," vol. xxiii, pp. 220, 227.

1844, June 13.— ,, ,, ,, ,,

1844, August 20.—Barom. 29·77, rising. Fine day.

Apparent Time.		Arago.
6^h 57^m	R=23° maximum polarisation, 27° in horizon.	17° 1′
7 38	R=27½ ,, ,,	20 40
8 8		21 25
8 46		19 0
		Babinet.
8 50	R=29°	17° 15′

1844, August 26.—Barom. 29·77, rising. Fine day.

Apparent Time.		Arago.
6^h 18^m	Neutral point among small mottled clouds.	29° 0′ !

1844, August 29.—Barom. 29·92, falling. Fine day. A China-ink sky. BREWSTER's neutral point distinctly seen.

Apparent Time.	
2^h 20^m	R=19½° maximum polarisation in zenith plane.

1844, September 21.—Barom. 30·1; cold and clear sky.

Apparent Time.		Arago.
6^h 16^m	R=28½° maximum polarisation in zenith, 25½° in hor.	18° 55′
		Babinet.
6 18	Polarisation of moon 17½°.	17° 11′

1845, January 11.—Fine day, cold; wind west.

Apparent Time.		Arago.
2^h 10^m	R=28½° maximum polarisation in zenith.	8° 15′

1845, January 20.—Barom. 29·48. Fine day; frosty.

Mean Time.		
3^h 10^m	R=29° maximum polarisation in zenith.	Above Horizon.
3 15	Alt. of ARAGO's neutral point,	12° 56′
3 17	Alt. of BABINET's neutral point; clouds came on.	22 0

1845, January 24.—Barom. 29·55, rising.

Mean Time.		Above Horizon.
3^h 37^m	Alt. of ARAGO's neutral point,	11° 0′
3 59	Alt. of ARAGO's neutral point,	17 30
4 4	Alt. of BABINET's neutral point,	19 35
4 39	Alt. of ARAGO's neutral point,	13 35
4 41	Alt. of BABINET's neutral point,	24 0

1845, January 31.—Therm. 18° at 10^h P.M. of the 30th; 8^h A.M., therm. 12°. Barom 29·54. rising. Fine frosty, clear day; ground covered with snow.

Mean Time.	
12^h 55^m	R=26½° in zenith and in horizon.
12 55	ARAGO's neutral point in N, hor.; sun's alt. then 14° 45′.
1 40	ARAGO's neutral point still in horizon.
1 40	R=26½° in zenith.

1845, February 1.—Barom. 29·80. Frosty day; cloudy till 3^h.

Mean Time.		Above Horizon.
3^h 30^m	Alt. ARAGO's neutral point,	8° 15′
	R=20½° maximum polarisation in zenith to 14½° in hor.	
3 45	Alt. ARAGO's neutral point,	14 30
	R=22½° maximum polarisation.	

1845, April 8.—Barom. 29·06.

Mean Time.		Above Horizon.
5^h 35^m	Alt. ARAGO's neutral point,	9° 10′
6 1	" " "	20 20
5 39	Alt. BABINET's neutral point,	25 10
6 4	" " "	15 25

1845, April 15.—See "Edinburgh Transactions," vol. xxiii. p. 220.

1845, July 14.—On the top of Scuirmore, near Glenquoich.

Mean Time.		Above Horizon.
5^h 24^m	Altitude of ARAGO's neutral point,	6° 40′
	R=23½° maximum polarisation in zenith plane.	

1845, September 6.—Barom. 30·10. Fine day; a milky sky.

Mean Time.		Above Horizon.
1^h 10^m	R=17° maximum polarisation in zenith plane.	
5 31	R=23° ARAGO's neutral point.	11° 0′
5 41	ARAGO's secondary neutral point seen in horizon.	13 24
6 36	ARAGO's neutral point,	19 10
5 43	BABINET's R=28½° maximum polarisation, 24° in horizon.	24 0
6 28		20 24

1850, July 1, 15, 29.—See "Edinburgh Transactions," vol. xxiii. p. 237.

1850, July 9.—Barom. 29·79, rising. Fine clear sky.

Mean Time.	
6^h 56^m	Bands just visible at the land horizon.
7 5	Bands invisible close to land horizon.
7 21	During the previous 21 minutes no trace of the + bands was seen. At 7^h 21^m they were seen, and became rapidly brighter.

The positive action which here produced the secondary neutral point was not strong enough to produce it by exhibiting the + bands counteracting the — ones, at some height above the horizon; but it was strong enough to neutralise them for 21 minutes, and to weaken them greatly when they did appear.

XXII.—*On the Laws of the Fertility of Women.* By J. Matthews Duncan, M.D.

(Read 5th February 1866.)

In a former paper* I described the variation of the fecundity of women according to age, and arrived at the conclusion that the climax of fecundity in women was at or near the age of 25 years. Researches, completed since that paper was read, regarding the variations of length and weight of children according to the mother's age, and regarding the mortality of childbed as influenced by the mother's age, have been published in the "Edinburgh Medical Journal." The results of these investigations seem to illustrate and confirm the statement made as to the age of the climax of fecundity, for I have found that, about that age, women produce the bulkiest children, as measured by length and weight; and about the same age of the mother there is the smallest mortality in childbed. As still further adorning the age of 25, I may add, that several sets of observations, including some made in St George's-in-the-East, London, and published in the eleventh volume of the "Journal of the Statistical Society," show a greater amount of survival and rearing among children born of women about that age than at any other; and recently Dr Arthur Mitchell has published a collection of cases of idiocy, with the respective ages of the mothers at the time of the idiots' births, and these also show a smaller proportion born of women about the age of 25 years than at greater and lesser ages.†

In the last portion of the paper first alluded to, having described initial fecundity, the age at which women are most likely to beget children soon after marriage, I said that I could not advance further without encroaching on another topic, viz., the fertility of marriage; or, as marriage is scarcely admissible as a term in physiology, the subject may be designated "sustained fecundity," or the laws of the fertility of women cohabiting with men during the child-bearing period of life. It is this subject which I propose here to enter upon. So far as I know, very little is ascertained or known in this department of physiology. The writings upon it are for the most part to be found in the works of political economists, and are chiefly confined to the single question of the rate of increase of a population under varying circumstances. To illustrate this topic, which is one of little interest to the physiologist, data are numerous and abundant. But when the writers referred to attempt to go deeper into the fundamental laws of the fertility of women, having very scanty materials and using them without care, they arrive at scanty results, which are either positively erroneous or of little value.

* Trans. Roy. Soc. of Edinburgh, vol. xxiii. p. 475, &c.
† Edinburgh Medical Journal, January 1866.

"The statistics," says Mr GRAHAM, registrar-general for England, "of a country in which the age of a mother at marriage, and at the birth of her children, is not recorded, must always remain imperfect, and leave us without the means of solving some of the most important social questions."* These data were secured for the first year of the registrations in Scotland. The results to be now described are derived from a study of a part of these registers, namely, those of Edinburgh and Glasgow for 1855, and are founded on an analysis of 16,301 families of wives.

CHAPTER I.—*The Fertility of the whole Marriages in a Population.*

On this subject much has been written, in latter times chiefly by Malthusians and anti-Malthusians, to whose works I refer generally. Elaborate comparisons are made between the fertilities of marriage in different countries; and there are exhibited variations to so great an extent, that they appear themselves to show the worthlessness of the data and of the comparisons instituted, at least in a physiological point of view. In illustration, I may refer to the variations described by M. BENOISTON DE CHATEAUNEUF,† in a paper on the intensity of fecundity in Europe at the commencement of the nineteenth century. The highest figure is derived from some villages in Scotland, where there are asserted to be six or seven children to a marriage, while his lowest figure is 2·44, the alleged productiveness of marriages in Paris.

We shall restrict our view to Great Britain, and we find the method generally followed of estimating the fertility of marriage to be the very old and simple one of dividing the number of legitimate births in any year by the number of marriages. "In 1861," says Dr STARK,‡ "for every marriage which occurred in Scotland there were born 4·64 legitimate children; that is to say, 464 legitimate children were born to every 100 marriages. During the same year, in England, only 3·89 legitimate children were born to every marriage, or 389 legitimate children to every 100 marriages." This is an exemplification of the ordinary method of calculating, and it is evident that the result derived is of not the slightest value as a contribution to the science of fertility. For, besides including marriages of all durations and at every fecund age, also second and third marriages, it includes many marriages at ages when fertility has entirely disappeared. It is impossible, indeed, to state what is the exact relation between the number of marriages in a population in any year and the number of legitimate children born in the same year, with a view to any physiological result. This aspect of the statement is, however, well worthy of being pointed out, because authors of respectability, whom it is needless to name, refer to and use these figures as exhibiting the fer-

* Registrar-General's Report for 1845, p. 14. (England).

† Annales des Sciences Naturelles, tome ix. 1826.

‡ Seventh Detailed Annual Report for 1861, published in 1865, p. xviii. (Scotland).

tility of continued married life in England and Scotland. MALTHUS was well aware of the real meaning of these figures,—of the fact that they merely show the relative frequency of marriage ceremonies and births in a population. "The rule," he says,* "which has been here laid down, attempts to estimate the prolificness of marriages, taken as they occur; but this prolificness should be carefully distinguished from the prolificness of first marriages andof married women, and still more from the natural prolificness of women in general, taken at the most favourable age. It is probable," he adds, "that the natural prolificness of women is nearly the same in most parts of the world; but the prolificness of marriages is liable to be affected by a variety of circumstances peculiar to each country, and particularly by the number of late marriages."

As a corollary from the preceding data, of value only in proportion to their value, it may be stated that the average duration of fertility in married women (including those who do not bear children) is about $7\frac{1}{2}$ years. For, as the intervals between marriage and the birth of a child, and between the births of successive children, is, on an average, 20 months, and as there are about $4\frac{1}{2}$ children to each marriage, we have about $7\frac{1}{2}$ years, counting from marriage, spent in producing that number.

British authors, as GRAUNT, SHORT, MALTHUS, SADLER, SENIOR, and those of later date, name 4, $4\frac{1}{2}$, or 5, as the fertility of marriage. MALTHUS, founding on such data, gives a wife eight years of fecundity to produce four children, a statement which cannot be passed over without the obvious remark that MALTHUS, so calculating, utterly neglects the force of the wise words which we have just quoted from his work.

I have nothing satisfactory to offer as to prolific marriages, to contrast with the statements given concerning all marriages. Dr LEVER† says, that "the average number of children consequent upon a prolific (not every) marriage is shown to be rather more than $5\frac{3}{4}$, but not amounting to 6." This is given without any authority stated or evidence detailed, and I know not what value to ascribe to it. In a physiological point of view, its value must be scarcely appreciable; for no allowance is made for the duration of the marriage, nor for the age of the woman at the time of the ceremony.

In St George's-in-the-East, London, the average number of children consequent on the prolific marriages was 5 to each marriage.‡ That is, 5 is the average number of children that has been born in all the families in a place at a given time. It tells nothing concerning the average number in completed families, or in still-growing families.§

* Essay on the Principle of Population, vol. ii. p. 6.
† On Organic Diseases of the Uterus, p. 5.
‡ Quarterly Journal of the Statistical Society of London, vol. xi., 1848.
§ Some interesting facts regarding the fertility of Esquimaux women are to be found in Roberton's "Essays and Notes on Physiology and Diseases of Women," p. 53.

Franklin says, that the females in America have, "one with another, eight children to a marriage;"* almost certainly a great exaggeration, especially as he does not even state, as a condition, that the marriages were prolific.

Chapter II.—*Annual Fertility of the Married Women of Child-bearing Age in a Population.*

Seeing the inexactness of the statements of which those just given are an example, Dr Stark has adopted another method of arriving at the comparative prolificness of marriages in England and Scotland. "In 1861," says he, "when the census was taken in England, the number of wives at the child-bearing ages, viz., 15 to 45, was 2,319,649; and as the number of legitimate children born during the year amounted to 652,249, this gives the proportion of one legitimate child for every 3·55 wives at the ages 15 to 45 in the population; or, in other words, every 355 wives in England, at these ages, gave birth to 100 children during the year. In Scotland, during the same year, there were 305,524 wives between the ages of 15 and 45 years; and as 97,080 legitimate children were born during the year, this gives the proportion of one legitimate child for every 3·14 wives at these ages in the population; or, in other words, every 314 wives in the population of Scotland, at these ages, gave birth to 100 legitimate children during the year."

While for every marriage in 1861 there were born in the same year in Scotland 4·64 legitimate children; every 3·15 wives between 15 and 45 in Scotland in the same year produced one legitimate child. Of 54,408 wives in Edinburgh and Glasgow in 1855 between 15 and 44 years of age, inclusive, 16,290 bore children fit for registering; or, 1 child was born to every 3·3 wives aged from 15 to 44.

If we adopt these latter statements, we must take care to note that they do not give the fertility of the whole marriages in a population, as the older and former statements in chapter first do. These latter give the annual productiveness of a mass of married women in our populations. The results of the two methods of computing the fertility of marriage cannot be contrasted, for each is concerned with an entirely different topic from the other.

Chapter III.—*The Fertility of the whole Marriages in a Population that are Fertile at a given time.*

In Edinburgh and Glasgow in 1855 there were 16,393 wives who bore first or subsequent children. Of these the necessary data are given in 16,301 cases. These 16,301 mothers had produced 60,381 children; or 3·7 children constituted

* Sadler. Law of Population, vol. ii. p. 495.

the average production of each mother. In other words, excluding the large class of wives sterile in 1855, we have 3·7 as the average number of children (surviving or not surviving) in each family that increased in 1855.

To compare with the above result, we may observe 16,414 women delivered in the Dublin Lying-in Hospital during Dr COLLINS' mastership, who had borne 53,458 children, whose families, on an average, numbered 3·25; also 6634 women delivered in the same hospital during the period reported on by Drs M'CLINTOCK and HARDY, who had born 20,680 children; whose families, on an average, numbered 3·12.

As there can be no doubt that these 16,301 families are a fair sample of all the growing families in Edinburgh and Glasgow, it appears that the average size of growing families existing at a particular time in our population is between 3 and 4; and, if it be true that, on an average, children are born with an interval not exceeding twenty months, then all mothers child-bearing at any particular time have been on average less than seven years fertile. It is to be remarked, that this statement concerns only the families of wives mothers child-bearing at a particular time (*i.e.* in 1855), and is not to be compared with the corollary to Chapter I., which includes all families, and especially the mass of completed families.

The accompanying Table (I.) shows the data upon which these statements are founded. It, in addition, gives the percentage of children (surviving or not) in families of different numbers, that increased in 1855.

TABLE I.—SHOWING THE NUMBER AND PERCENTAGE OF MOTHERS BEARING RESPECTIVELY 1ST, 2D, AND 3D CHILDREN, AND SO ON; ALSO PERCENTAGE OF CHILDREN IN STILL-GROWING FAMILIES OF DIFFERENT NUMBERS.

Number of Child.	Number of Wives Mothers.	Percentage of Wives Mothers.	Percentage of Children.	Number of Child.	Number of Wives Mothers.	Percentage of Wives Mothers.	Percentage of Children.
1	3,722	22·83	6·16	11	152	·93	2·77
2	2,893	17·74	9·58	12	61	·37	1·21
3	2,534	15·54	12·59	13	34	·20	·732
4	1,982	12·16	13·13	14	11	·06	·255
5	1,543	9·46	12·77	15	6	·03	·149
6	1,221	7·49	12·13	16	2	·01	·053
7	848	5·20	9·83	17	2	·01	·056
8	641	3·93	8·49	18	1	·006	·029
9	425	2·60	6·33	19	1	·006	·031
10	222	1·36	3·67				

Chapter IV.—*The Fertility of Fertile Marriages lasting during the whole Child-bearing Period of Life.*

This subject may be stated in the form of a question. How many children does a fertile woman produce, living in wedlock from 15 to 45 years of age? The only collection of data known to me, which can throw light on this point, is that published in the "Report to the Council of the Statistical Society of London, from a Committee of its fellows, appointed to make an investigation into the state of the poorer classes in St George's-in-the-East."* In that district there were found 80 mothers married at ages varying from 15 to 19, and who had lived in wedlock at least 31 years. These fertile wives having lived nearly all the child-bearing period of life in wedlock, had borne on an average 9·12 children.

There are evident sources of inexactness in the above very limited data, which tend to diminish the average fertility; and it will be as near the truth to state 10 as the average fertility of fertile marriages lasting during the whole child-bearing period of life.

The conclusions given in further parts of this paper will show that the figure of 10 children, for 30 years of child-bearing life, is not indicative of each mother having borne a child every third year. The fertility, while it lasts, will be shown to be much intenser than this. The average interval between births of living children is hereafter shown to be 20 months, which gives about 17 years as the average duration of fecundity in a fertile woman living in the married state all the child-bearing period of life.

In his work on Abortion and Sterility, Dr Whitehead gives no data which I can properly collate with those just given. After stating his belief that the actual duration of the child-bearing period in the female of this climate is about 20 years, he adds, that a woman, under favourable circumstances, has in that period 12 children. But as this includes abortions and premature deliveries, which he estimates at $1\frac{1}{2}$ for each individual, the figure 12 has to undergo that reduction for comparison with 10, and the approximation is very close.

Sadler states as a fact, "that marriages, on the average, are only fruitful for about a third part of the term of possible fecundity.†" But he nowhere, so far as I know, affords any evidence of this statement, and I therefore attach to it no importance.

* Quarterly Journal of the Statistical Society, August 1848, vol. xi.
† Law of Population, vol. ii. p. 276.

CHAPTER V.—*The Fertility of Persistently Fertile Marriages lasting during the whole Child-bearing Period of Life.*

This subject may also be conveniently stated in the form of a question. How many children does a fertile woman produce, living in wedlock from 15 to 45 years of age, and bearing children periodically up to the end of that time?

To this question I cannot give at once an answer founded on sufficient data; and I shall invert my usual mode of proceeding, stating the conclusion, namely, that 15 at least is the average number of children borne by a persistently fertile female in 30 years, before giving the reasons for it. These are as follows:—A persistently fertile woman, at all ages, is found to have borne one child about every 2 years; the average fertility of 15 mothers who have had each 26 years of persistently fertile life is 13. The fourth Table, to be hereafter given, showing an excess of fertility on the part of those long persistently fertile, or bearing children in the year of counting, would give 16 as the proportional fertility of 30 years of persistently fertile marriage, calculating from the actual values given for the other results in the Table. The deficiency of actual facts for settling this point is to be seen in the next Table (II.), where the number of women bearing children when above 26 years married, is only 7.

On this subject ALLEN THOMSON makes the following statement, which is remarkably accurate, seeing that it is apparently not founded on any analysis of documents. "A healthy woman," says he,* "bearing during the whole time, and with the common duration of interval, may have in all from 12 to 16 children, but some have as many as 18 or 20."

CHAPTER VI.—*Fertility of Persistently Fertile Wives at different Years of Married Life.*

The following Table (II.), from the 1855 Edinburgh and Glasgow data, gives at a glance the rate of yearly increasing production of wives mothers who are still fertile—that is, who produced a living child in the year of our census or counting. It is framed by adding together the whole children born of mothers having different durations of marriage, and dividing the sum by the number of mothers corresponding to each duration of marriage. The results will be found, on the whole, to tally pretty closely with those given in Table VI. It is easy to account for the differences between the two Tables. In the latter Table the wives arrived at different numbers of progeny are collated and compared, while in the former the wives arrived at different durations of marriage are collated and compared. The Table requires no further explanation; it is easily read.

* Todd's Cyclopædia of Anatomy and Physiology, vol. ii. p. 478.

TABLE II.—Showing the Average Number of Children that have been Born at the Completion of each Year of Persistently Fertile Marriage.

Duration of Marriage.	Number of Wives Mothers.	Number of Children.	Average to each Mother.
	16,301	60,381	3·70
1 year married and under,	3,172	3,336	1·06
2 years ,,	1,223	2,090	1·70
3 ,, ,,	1,540	3,195	2·07
4 ,, ,,	1,248	3,229	2·58
5 ,, ,,	1,193	3,645	3·05
6 ,, ,,	1,122	3,959	3·53
7 ,, ,,	870	3,414	3·92
8 ,, ,,	733	3,225	4·40
9 ,, ,,	719	3,447	4·79
10 ,, ,,	761	4,021	5·28
11 ,, ,,	624	3,502	5·61
12 ,, ,,	520	3,134	6·03
13 ,, ,,	441	2,878	6·53
14 ,, ,,	393	2,698	6·86
15 ,, ,,	372	2,659	7·15
16 ,, ,,	293	2,248	7·67
17 ,, ,,	240	1,918	7·99
18 ,, ,,	198	1,647	8·32
19 ,, ,,	177	1,541	8·71
20 ,, ,,	142	1,303	9·17
21 ,, ,,	115	1,116	9·70
22 ,, ,,	80	790	9·87
23 ,, ,,	56	557	9·95
24 ,, ,,	39	415	10·64
25 ,, ,,	8	95	11·87
26 ,, ,,	15	195	13·00
27 ,, ,,	2	25	12·50
28 ,, ,,	3	42	14·00
29 ,, ,,	1	14	14·00
30 ,, ,,	1	13	13·00

Chapter VII.—*Fertility of Fertile Wives at Different Periods of Married Life.*

With a view to comparison with the results given in Table II., I have prepared the following Table (III.), from the data of St George's-in-the-East, already referred to. The circumstances in which these data were collected, and their paucity, do not justify me in ascribing to them a value equal to those given in Table II., nor do I think they are well adapted for the purpose of the comparison for which they are adduced. But I know no other to refer to.

As in the Report of the Committee of the Statistical Society, the periods are counted from the birth of the first child, I have added to them 17 months ($1\frac{5}{12}$ths year), the average interval between marriage and birth of a first child, with a view to make the Table more easily contrasted with Table II.

TABLE III.—SHOWING, FROM THE DATA OF ST GEORGE'S-IN-THE-EAST, THE FERTILITY OF FERTILE WIVES AGED FROM 15 TO 45 YEARS.

Years Married.	Mothers.	Children.	Average of each Mother.	Years Married.	Mothers.	Children.	Average of each Mother.
$2\frac{5}{12}$	56	59	1·05	$8\frac{5}{12}$	76	269	3·54
$3\frac{5}{12}$	60	88	1·46	$11\frac{5}{12}$	254	1,178	4·64
$4\frac{5}{12}$	54	99	1·83	$16\frac{5}{12}$	215	1,319	6·13
$5\frac{5}{12}$	66	184	2·79	$21\frac{5}{12}$	148	1,075	7·26
$6\frac{5}{12}$	57	163	2·86	$26\frac{5}{12}$	44	353	8·02
$7\frac{5}{12}$	60	196	3·26				

The direct results of this Table are given in the figures, and require no statement. But comparing it with the preceding Table, we observe that, as is easily understood, the differences between the fertile and the persistently fertile increase as the duration of marriage increases; and that, while the numbers of the children of fertile women is about a third of the years of duration of marriage, the numbers of the children of persistently fertile women is about a half of the years of duration of marriage. In other words, if these Tables are at all trustworthy, we may guess that the number (surviving or not) of a fertile married woman's family is about a third of the number of years since her marriage. But if, in addition to knowing that the married woman has a family, we know that she has just had an addition to her family, then we may guess that the number of her family is about a half of the number of years since her marriage.

From the same London data I have also framed the following Table, without doing any apparent violence to them, and with a result that is extremely interesting. The student will observe, that beside the data from St George's-in-the-East I have placed corresponding data extracted from the Edinburgh and Glasgow registers of 1855. The comparison of the fertility of a set of fertile wives—that is, all wives who have borne children some time during their still-continuing married lives—with that of a set of persistently fertile wives—that is, exclusively, of wives bearing at the ends of the periods under consideration (that is, in this Table, the end of their child-bearing lives)—is, as already said, marred, and loses value on account of the two sets being of very different numbers, different localities, and different populations. Taking it as it stands, we find that fertile women generally, living with husbands for 16 years before the conclusion of child-bearing life, have an average family of about $4\frac{1}{2}$; while persistently fertile wives—that is, wives bearing children at the end of their child-bearing lives—have an average family of $11\frac{1}{2}$. While fertile wives, married 21 years, before and up to the age of

45, have an average family of about 6; persistently fertile wives have an average family of $10\frac{1}{2}$. While fertile wives married for 26 years, before and up to the age of 45, have an average family of 8; persistently fertile wives, in the same circumstances, have an average family of about 14. While fertile wives, married for 31 years, before and up to the age of 45 years, have an average family of 9; persistently fertile wives, in the same circumstances, have an average family which may be estimated at 16.

TABLE IV.—Showing a Comparison of the Fertility of Mothers and of Persistently Fertile Mothers.

		(St George's-in-the-East.) Wives Mothers.			(Edinburgh and Glasgow in 1855.) Wives Mothers bearing Children at the end of Child-bearing life.		
Age at Marriage.	Duration of Marriage.	Number of Mothers.	Number of Children.	Average fertility of each Mother.	Number of Mothers.	Number of Children.	Average fertility of each Mother.
15–19	At least 31 yrs.	80	730	9·12	...	...	16
20–24	At least 26 yrs.	179	1418	7·92	6	83	13·83
25–29	At least 21 yrs.	100	630	6·30	7	74	10·57
30–34	At least 16 yrs.	25	115	4·60	4	46	11·50

In this Table (IV.) it will be observed that the differences between the fertile and the persistently fertile are much greater than in the former (II. and III.), a circumstance which is easily explained. For, in the latter, all the women have been long married, and the persistently fertile have had time to far outrun the average fertility of all the fertile. It must also be noted, that all the women in the Table are fertile at or near the end of the child-bearing period, a time at which, it will be hereafter shown, the intensity of fertility is greater than at any other.

Chapter VIII.—*Degrees of Fertility of Wives Mothers of Families of different Numbers.*

Under this head, the first question that raises itself relates to the interval between marriage and the birth of the first child. In Table V. this question is found fully answered. In fertile marriages generally, there intervene about 17 months (1·38 year) between the ceremony and the birth of the first child. But in women of all ages this interval is far from being identical. As age increases above 25 years, the interval increases; the hope of the female is longer of being realised. The Table does not confirm this statement for wives married at 40 and upwards; but this is almost certainly a mere result of the paucity of the data at these ages. The whole tenor of the Table confirms the law of greatest fecundity according to age, meaning by fecundity, likelihood of having children. For it is observed, that not only are wives most fecund from 20 to 24, but also that they begin the career of fertility sooner than their younger or elder sisters.

TABLE V.—Showing the Interval between Marriage and the Birth of a First Child in Wives Married at Different Ages.

		Mother's Age at Marriage.							Total.
		15–19.	20–24.	25–29.	30–34.	35–39.	40–44.	45–49.	
Years Married.	Less	94	325	126	44	15	4	...	608
	1	409	1,259	533	135	49	3	2	2,390
	2	83	202	88	45	17	2	...	437
	3	25	50	35	12	10	1	...	133
	4	8	31	13	8	1	...	...	61
	5	13	10	3	3	3	...	...	32
	6	5	14	6	1	1	...	...	27
	7	5	3	1	3	...	...	...	12
	8	1	3	1	...	...	...	...	5
	9	2	3	...	...	...	...	...	5
	10	...	1	...	...	...	...	...	1
	11	...	1	2	...	...	...	...	3
	12	2	1	1	...	...	...	...	4
	13	1	1	...	...	...	...	...	2
	14	...	...	...	...	...	...	...	...
	15	1	...	...	...	...	...	...	1
	16	...	...	...	...	...	...	...	...
	17	...	...	...	...	...	...	...	...
	18	...	1	...	...	...	...	...	1
	Total	649	1,905	809	251	96	10	2	3,722
Average interval between Marriage and Birth of first Child.	Year.	1·516 or	1·329 or	1·350 or	1·510 or	1·594 or	1·400 or	1·000 or	1·385 or
	Months.	18·2	15·9	16·2	18·1	19·1	16·8	12·0	16·6

It is noteworthy, that while the average interval between marriage and the birth of the first child is 17 months, the average interval between the births of successive children, however numerous, is a little under 20 months; the two intervals approximating one another so closely as to destroy all probability of the truth of the explanations usually offered for the delay of impregnation after a recent childbirth, and of the efficacy of continued lactation in retarding the occurrence of a new conception. And we shall soon see, in a quotation from Sadler, that he finds that women who do not suckle their offspring have as long an interval between conceptions as others. But, while Sadler by this demonstration destroys the only foundation for his invective against the rich who do not suckle, he nevertheless proceeds enthusiastically, as if the dictum of physiologists were valid, even after their argument was ruined.

Speaking of the interval between marriage and a first birth, SADLER* gives the following indefinite statement:—"Married females do not become fruitful, on the average, during the first year of their nuptials, but nearly so. A great number of cases which I have collected, with a view of determining this point, give three-fourths of them as producing their first child at the average of one year after marriage."

WHITEHEAD,† founding on the observation of 541 married women, of the average age of 22 years, makes out the average interval between marriage and the birth of a first child, to be $11\frac{1}{2}$ months.

QUETELET‡ admits, with sufficient probability, as an average term, that the birth of the first-born takes place within the first year which follows marriage. His error, as those of the others, depends on the acknowledged want of documents.

It next comes to be inquired at what rate children succeed each other in families. This interesting topic is developed from the data given in Table VI. It is formed by dividing the whole years of duration of sets of marriages, of different durations, by the number of children born in the corresponding marriages; and it must be remembered, that as our data all spring from women who were fertile on the year of our census or counting, no women are included who, although fertile formerly, have now ceased to be so; and it is evident that, for the purposes of our argument, this is just.

TABLE VI.—SHOWING THE AVERAGE DURATION OF MARRIAGE AT BIRTH OF EACH SUCCESSIVE CHILD; AND THE AVERAGE INTERVAL BETWEEN THE BIRTHS OF THE SUCCESSIVE CHILDREN.§

Number of Child.	Number of Mothers.	Duration of Marriage in Months.	Average interval between successive Births.	Number of Child.	Number of Mothers.	Duration of Marriage in Months.	Average interval between successive Births.
1	3,722	17	17·0	11	152	235	21·4
2	2,893	38	19·0	12	61	246	20·5
3	2,534	64	21·3	13	34	263	20·2
4	1,982	90	22·5	14	11	281	20·1
5	1,543	115	23·0	15	6	280	18·7
6	1,221	137	22·8	16	2	336	21·0
7	848	162	23·1	17	2	252	14·8
8	641	181	22·6	18	1	252	14·0
9	425	203	22·5	19	1	204	10·7
10	222	225	22·5				
						Average	19·9

* The Law of Population, vol. ii. p. 30.

† On Abortion and Sterility, p. 242.

‡ Treatise on Man, p. 15.

§ This is not a correct statement of the contents of this Table. The last column does not directly give the average interval between the births of successive children, but the average interval between marriage and the birth of the child, divided by the number of the children born. For brevity's sake, the title is left as it stands.

The first conclusions deducible from the data are :—

1. That the mass of early or first children, up to the third or fourth, come into the world in more quick succession than those that immediately follow.

2. That a mass of children, numbering from the fourth or fifth on to the tenth, succeed one another more slowly than those of the first category, and of the third.

3. That a mass of children, following the tenth, come into the world hurrying after one another with a gradually increasing rapidity, which excels that of all their predecessors (a circumstance which may, in part at least, account for the great mortality of women bearing children after the ninth.)*

While all these propositions are true of a large number of children, it must not be supposed that they directly indicate laws regulating the fertility of women. But the Table bears important information relative to this last topic. And it appears to me that the first of the three conclusions given above can be explained only by supposing what may therefore be held as equally well demonstrated :—

1. That wives bearing their early children up to the third or fourth, breed more rapidly than they subsequently do.

For the average fertility of all wives is at least 4 children; and the great mass of fertile wives is therefore included in the calculation. All the wives destined to bear large families, and furnish data for the second and third conclusions, are included in the data for first 4 children. The mass of children born in families numbering 10 and more, is not large enough to have great influence on the data, should it be the case that they are proportionately very quick breeders from the first.

If we now regard the mothers whose children have afforded the data for the second conclusion as to the rapidity of the succession of a mass of children, we shall have, I think, no difficulty in accepting the proposition,—

2. That wives produce their children, numbering from the third or fourth on to the tenth, at greater intervals than their earlier progeny.

For, in the calculations, the earlier and more rapidly succeeding progeny are included, and have their full influence, and diminish the periods given in the Table opposite children numbering from 4 to 10, reducing them below what they would be were pregnancies from 4 to 10 alone counted, exclusive of those from 1 to 4.

Regarding, now, the mothers of families numbering 11 or more, it is evident that their paucity, though not such as to destroy all their value, is such as to prevent their having a paramount influence upon the figures of the two preceding categories. It might therefore appear necessary to leave undecided whether their specially rapid bearing were a consequence of their great fertility, and therefore an acquired or secondary rapidity, or were an original condition true of even

* Edinburgh Medical Journal, September 1865, p. 209.

their earlier pregnancies. That the latter is to be accepted to the exclusion of the former supposition is evident, if we observe that the married life of the women with families above 10 is not long enough to admit of their having gone through the series of lengths of pregnancies given in the Table opposite each successive child. It is thus shown,—

3. That wives bearing more than 10 children, or wives bearing very large families, breed more rapidly than others during their whole child-bearing lives.

Wives, therefore, who bear numerous progeny, do so in virtue of two differences from other married women. They bear their children more rapidly, and they continue fertile longer than their neighbours.

Were the third conclusion just given not before us, it might be supposed that the rapid bearing of earlier children was a result of youth and vigour. This supposition is not only inconsistent with the third conclusion, but with the law to be hereafter demonstrated, that the oldest women, who are continuedly fertile, bear children more rapidly than any other.

The average length of interval between all successive children is (19·9), nearly 20 months.

I have frequently heard it said, that a fertile woman bears a child every 2 years. Some authors have made careful statements on this point. WHITEHEAD* says, that fertile women produce children every 20 months; but "this includes abortions, false conceptions, so-called premature deliveries, and all having an unsuccessful issue, the average amount of which will be rather more than one-and-a-half for each individual." Sir WILLIAM PETTY long ago laid it down, that "every teeming woman can bear a child once in 2 years." MALTHUS† adopts the same period, and refers to the Statistical Account of Scotland as confirming it. The number and exactness, however, of the data here adduced, and the circumstance that they include only children born alive (excluding still-born and abortions), leave no room for doubt that all the authors referred to under-estimate the rate at which married women bring children into the world.‡

On this point SADLER is so full and distinct that I quote his words. "The interval of time," says he, "at which the fruitful couples produce their children, calculated from the period of their marriage to the birth of their last child, including the greater prolificness of the first year, exceeds 2 years. It extends to between $2\frac{1}{4}$ and $2\frac{1}{2}$ years, if calculated from the first birth."§ In this calculation, as in that of the interval between marriage and the birth of a first child, SADLER evidently errs, making the former too long, and the latter too short. For both he gives no data; yet, in regard to the interval between the births of

* On Abortion and Sterility, p. 245.

† An Essay on the Principle of Population, vol. ii. p. 3.

‡ See also Roberton's Essays and Notes on the Physiology and Diseases of Women, p. 185

§ Vol. ii. p. 30.

successive children he says:—"All the Tables are constructed upon the presumption of its certainty, and, happily, it is one which, on this very debatable question, has never been made the subject of controversy, and which does not admit of it. Nothing," he continues, "is more certain, or better ascertained, than the average period at which the human female, in a state of prolificness, reproduces. Were we, indeed, to form our general rules from particular exceptions, we should in this, as in all other cases, be grievously misled; we might conclude, for instance, that she would continue to multiply within the year; but general computations will rectify any such error, and conduct us to conclusions which are not only reconcilable with philosophy and truth, but resolvable into the ordinations of a merciful Providence. The human mother has to feed her infant for a period pretty nearly corresponding in length to that of gestation (I speak now as regards the necessity of the great mass of the community, with whom the question evidently rests); nature, therefore, has kindly ordained, as a general rule, that the period of impregnation shall be postponed till that essential duty is discharged, and for a period somewhat beyond it; and he must be ignorant indeed, who does not see most clearly that the health, and, indeed, frequently the existence, both of mother and offspring, are secured by this physical regulation of the common parent of mankind. The human being, in reference to the term of existence, multiplies later, and at longer intervals, and ceases to be prolific sooner, than any other animated being with whom we are acquainted; hence we find, on the average, that, in the maternal state, during its period of fruitfulness, the births are not so frequent as once in 2 years. Even in the rank of society which is absolved from the necessity (though not from the duty) of fulfilling one of the most important of the maternal offices, that of feeding, from their own bosoms, their infant offspring, and who too often avail themselves of that unnatural immunity, consequently removing what our physiologists regard as one of the physical impediments to an accelerated prolificness,*—even in this rank, I find the births are at intervals of about, but rather exceeding, 2 years; that period, therefore, as it respects the mass of the community, who are differently circumstanced in this respect, cannot be shorter. But arguments and proofs on this point are unnecessary, no writer having ever ventured upon supposing a shorter period than 2 years possible; and even Sir William Petty, when labouring to prove the possibility of a doubling every 10 years for a century after the flood, amongst his other suppositions, so extravagant if applied to the present era, only lays it down, that every teeming woman can bear a child 'once in two years.'"

* On this subject the work of Roberton already cited may be consulted; also a paper by Professor Laycock, quoted by Roberton.

CHAPTER IX.—*Fertility of Wives Mothers Married at Different Ages.*

Before discussing this and the next topics, it is necessary to remark that fertility may be maintained in degree in two ways—either by long-continuance or by intensity while it lasts. At present I omit entirely the consideration of intensity of fertility while it lasts, taking up this in the next part. But I shall show that, of a mass of fertile women, the younger are, on the whole, more fertile than the older. To demonstrate this I first adduce a Table (VII.) drawn from the data of St George's-in-the-East. It is evident here that the younger women 11 years married, and also those 21 years married, have, on an average, larger families than the elder, of whatever respective ages. It must be observed that the Table includes all wives, who, in a small selected population, have shown any fertility. And it must be added that the Committee of the Statistical Society have enunciated the same conclusion. I quote their own words :—"The following abstract will show the average number of children to each marriage, at the respective periods of 10, 20, 30, and 40 years after the birth of the first child, for each class of marriages formed at the four different quinquennial periods of life.

TABLE VII.—SHOWING THE FERTILITY OF WIVES MOTHERS MARRIED AT DIFFERENT AGES, FROM THE DATA OF ST GEORGE'S-IN-THE-EAST.

Mother's Age at Marriage.	11$\frac{5}{12}$ Years Married. Average Number of Children.	21$\frac{5}{12}$ Years Married. Average Number of Children.
15–19	5·0	7·7
20–24	4·5	7·0
25–29	4·4	6·4
30–34	3·4	3·0

"TABLE VIII.

Years elapsed since Birth of First Child.	Average number of Children to each Marriage formed at Ages			
	16–20.	21–25.	26–30.	31–35.
10	5·05	4·51	4·42	3·44
20	7·68	7·01	6·43	3·00
30	8·41	7·89	6·80	7·00
40	10·85	8·24	5·00	4·00

"It is thus obvious that marriages formed under the age of 25 are more prolific than those formed after that age, and that those formed between 16 and 20 years of age are still more so than those at any of the superior ages."*

As the doctrine generally taught, so far as I know, is exactly the opposite of that here sustained, it is important to establish the latter, if possible, by further proof. At another place I shall show the erroneous interpretation of the data which have been adduced in support of the opposite doctrine—namely, that marriages formed late in life are more prolific than those formed earlier.

The figures now to be adduced not only confirm the doctrine that early marriages are more fruitful than late marriages; they also explain it, showing that the younger married have a longer continuance of fertility than the older married, allowing to both the same duration of marriage, and all within the child-bearing period of life. So far as the demonstration has hitherto gone, we have shown that the younger are more fertile than the elder; that, excluding those who have no children, the younger will bear larger families than the older. We have not shown which bear their children most rapidly—that is, which have the greatest intensity of fertility while it lasts—leaving this topic for another chapter. We now proceed to show that, among the fertile, the younger have a longer continuance of fertility than the elder. It is this last circumstance which accounts for the greater fertility of the marriages of the younger. The following Table demonstrates this. It needs no explanation. The details are given in the footnote.†

TABLE IX.—SHOWING THE AMOUNT OF CONTINUANCE IN FERTILITY OF WIVES MARRIED AT VARIOUS AGES (AS SHOWN WITHIN TWELVE MONTHS).

Age of Mother at Marriage, ...	15–19	20–24	25–29	30–34	35–39	Total.
The number Child-bearing in the 5th year of Married Life is 1 in . . .	2·6	2·7	4·1	4·9	10·5	3 2
The number Child-bearing in the 10th year of Married Life is 1 in . . .	3 2	4·0	5·9	8·7	...	4 4
The number Child-bearing in the 15th year of Married Life is 1 in . . .	4·6	6·8	18·2	37·4	...	8·0
The number Child-bearing in the 20th year of Married Life is 1 in . . .	8·5	14·6	129·8	...	...	16·3
The number Child-bearing in the 25th year of Married Life is 1 in . . .	68·0	480 5	...	...	...	171·0

* Journal of the Statistical Society of London, vol. xi. p. 223.

† The Table IX. may be easily seen to be made up from the following five Tables, X., XI., XII., XIII., XIV. In these five Tables of the fertility of married life at different epochs, the number of

In order to derive from Table IX. more information as to the relative numerical value of the fertility of a mass of wives in the fifth, tenth, and fifteenth years of married life, and so on, I have framed the following Table (XV.) I have freely pointed out the sources of error in the fundamental figures of Table IX.; and after all I flatter myself that in these fundamental figures there is an approach to truth such as to justify the further deduction of Table XV.

wives mothers at the respective epochs is the actual registered number in Edinburgh and Glasgow in 1855. The number of wives of different ages is got by estimating, and the Carlisle Table of Mortality is used. The estimate is not made in the exactest way, but the errors will not injure the comparison of the figures with one another, as the same (perhaps unavoidable) error is introduced into all. The results probably give a near approach to the true degrees of fertility; for while among the child-bearing there are some omitted, there are probably fewer marriages omitted, and the numbers of wives as estimated would be too large were not a very high percentage taken off (1 in 100) for the special mortality of first confinements. (See Edinburgh Medical and Surgical Journal for October 1865, and Dr Stark's Report in the Seventh Annual Report of the Registrar-General for Scotland, p. xxxii.)

To find how many women, 5, 10, and 15 years married, are alive and not widowed in 1855, it would strictly be necessary to have the numbers married in 1850, 1845, and 1840, from which the estimates should be made. Instead of doing this, I have estimated from the number married in 1855. As the population is increasing not greatly, this error thus introduced will not be great.

It is partly with a view to correct this error that I have taken off an extravagantly high percentage for the mortality of first labours.

In making the estimate I have doubled the mortality, in order to exclude the widowed.

TABLE X.—Fertility of Wives in the Fifth Year of Married Life.

Ages at Child-bearing, ...	20–24	25–29	30–34	35–39	40–44	Total.
Number of Wives, ...	644	1686	1008	358	179	3875
Number of Wives Mothers, . . .	247	611	244	72	17	1191
Number Child-bearing, 1 in . .	2·6	2·7	4·1	4·9	10·5	3·2

TABLE XI.—Fertility of Wives in the Tenth Year of Married Life.

Ages at Child-bearing, ...	25–29	30–34	35–39	40–44	Total.
Number of Wives, ...	594	1528	902	313	3337
Number of Wives Mothers, . . .	186	381	153	36	756
Number Child-bearing, 1 in . .	3·2	4·0	5·9	8·7	4·4

TABLE XII.—Fertility of Wives in the Fifteenth Year of Married Life.

Ages at Child-bearing, ...	30–34	35–39	40–44	45–49	Total.
Number of Wives, ...	532	1360	782	262	2936
Number of Wives Mothers, . . .	116	200	43	7	366
Number Child-bearing, 1 in . .	4·6	6·8	18·2	37·4	8·0

only it is necessary to mention, that in this Table there are no actual values to keep it close to the truth. Taking, then, Table IX. as giving actual values, we have the fertilities for 1855; or for twelve months. But as 20 months has been shown to be the average time-unit of fertility, the fertilities of 1855 must be increased in like proportion; for as 12 is to 20, so are the fertilities given in Table IX. to the real fertilities. All the fertile women cannot be presumed to have shown that quality in 12 months, but all may be presumed to have shown it in 20 months. In this way, the following Table (XV.) may be held as an estimate of the comparative amount of fertility in living children, shown by wives at different epochs of married life.

The Table shows a gradually diminishing amount of perseverance in fertility as age advances. In illustration of the mode of reading it, I may state that about a half of all wives are fertile at the fifth year of married life; more than a third are fertile at the tenth year of married life; and only a fifth part of the whole wives arrived at the fifteenth year of married life are fertile, and so on.

Another interesting result is got from this Table (XV.), by comparing the different horizontal columns with one another. Reading the figures of adjacent columns obliquely from below upwards, we have a comparison of the fertility of a mass of wives of the same age, but of quinquennial differences of duration of marriage. And it is very interesting to observe that the younger married closely approach in fertility those married five years later in life, both being arrived at the same year of life at the time of the comparison.

Short and Sussmilch maintain that early marriages are not favourable to the population. But, so far as I know, they adduce no satisfactory evidence whatever for their belief. Yet they have considerable authority on their side, including the redoubtable Sadler, who arrays in his support the venerable names of Aristotle, of Plato, of Virgil, and of Plutarch.

TABLE XIII.—Fertility of Wives in the Twentieth Year of Married Life.

Ages at Child-bearing, ...	35–39	40–44	45–49	Total.
Number of Wives, ...	477	1171	649	2297
Number of Wives Mothers, . . .	56	80	5	141
Number Child bearing, 1 in . .	8·5	14·6	129·8	16·3

TABLE XIV.—Fertility of Wives in the Twenty-Fifth Year of Married Life.

Ages at Child-bearing, ...	40–44	45–49	Total.
Number of Wives, ...	408	961	1396
Number of Wives Mothers, . . .	6	2	8
Number of Child-bearing, 1 in . .	68·0	480·5	171·0

TABLE XV.—Showing the Probable Amount of Continuance in Fertility, at Different Epochs, of Wives Married at Various Ages.

Age of Mother at Marriage, ...	15–19	20–24	25–29	30–34	35–39	Total.
The proportion Child-bearing about the 5th year of Married Life is 1 in . .	1·56	1·62	2·46	2·94	6·30	1·92
or a Percentage of	64·1	61·7	40·6	34·0	15·9	52·1
The proportion Child-bearing about the 10th year of Married Life is 1 in .	1·92	2·40	3·54	5·22	...	2·64
or a Percentage of	52·1	41·7	28·2	19·2	...	37·9
The proportion Child-bearing about the 15th year of Married Life is 1 in . .	2·76	4·08	10·92	22·44	...	4·80
or a Percentage of	36·2	24·5	9·1	4·5	...	20·8
The proportion Child-bearing about the 20th year of Married Life is about 1 in	5·10	8·76	77·88	...	...	9·78
or a Percentage of	19·6	11·4	1·3	...	...	10·2
The proportion Child-bearing about the 25th year of Married Life is about 1 in	40·80	288·3	...	...	...	102·6
or a Percentage of	2·4	·35	...	...	...	·97

It is to be remarked that I only object to this statement of these authors so far as the increase of the population is concerned, and I do not consider the diminished chances of survival which children of very early marriages are believed to have. There can be, in my opinion, no doubt that early marriages are most favourable to the population; and, as I have already shown that wives under 20 are less fecund than those from 20 on to at least 24 years of age,* the fertility of the younger as a mass is the more striking. But although most highly fertile as a mass, the number of sterile among those married under 20 years of age is not inconsiderable, and it is probably this amount of sterility which, while satisfactory statistical evidence was deficient, has given rise to the error now commented upon. The authors referred to give no definition of what they mean by early marriage. Whatever they may mean, they have no good evidence for their doctrine.

Quetelet† enunciates on this topic the following doctrine, as a natural consequence from his data and reasonings. A marriage, says he, if it be not barren, produces the same number of births at whatever period it takes place, provided the age of the woman does not exceed 26 years. After this age the number of children, he adds, diminishes. Not only do I, of course, think Quetelet wrong in

* Trans. Royal Society, 1864.

† Treatise on Man, p. 15.

his conclusions, but I cannot in his work discover any satisfactory grounds for them.

Before passing from the perseverance in fertility of the early married, I will point out a difficulty of which it gives the solution. In my former paper, read to this Society, I showed that initial fecundity in wives from 15 to 19 years of age, is far less than at any age from 20 to 34; that is, of the young women very much fewer have children within two years; at the same time, I showed that the fecundity of the mass of wives in our population is greatest at the commencement of the child-bearing period of life, and after that epoch gradually diminishes; that is, those not the most fecund do, as a mass, produce most children. These two propositions are, at first sight, difficult to reconcile; and it is accordingly satisfactory to be able to show that the greater continuance in fertility of the mass of younger wives is the explanation of the apparent anomaly. To illustrate how the Tables read, in affording this explanation, I may state, that while I formerly showed that the wives from 15 to 19 years of age are not so fecund as those from 20 to 24 years of age, the Tables last adduced show that at the 5th year of marriage, the youngest married—that is, at ages from 15 to 19—already surpass all others in fertility, 1 in 2·6 bearing; that at the 10th year of marriage they still further surpass in fertility all others, 1 in 3·2 bearing; and that at the 15th year of marriage, they in a still higher degree surpass all others, 1 in every 4·6 bearing children, within a year.

Finally, under this head, I notice an important element of the inexactness that enters into the data here used, namely, the occurrence of second and third marriages. But the influence of this element is almost certainly inconsiderable for the following reasons:—In cases of second and subsequent marriages, the data used are exclusively those of the last marriage; as far as is known, a woman's previous marriage does not interfere with her subsequent fertility; it is shown in this paper, that a woman's previous fertility tends to ensure continuance in fertility; it will be shown that a woman's previous fertility tends to diminish the intensity of her subsequent fertility, when that is compared with the fertility of women late in being married and having family; and the admixture of second and subsequent marriages in the data which include only the last marriage, would tend to diminish the force of the results, as bearing out these conclusions. They are therefore all the more secure, from the fact of the intermingling of some data which would diminish their apparent influence.

Another inconsiderable element of inexactness I shall only mention, the occurrence of twins, and both being counted in the figures.

CHAPTER X.—*Fertility of Persistently Fertile Wives of different Ages.*

I may here repeat that, by persistently fertile, I mean fertile up till the time of the collection of the data. And I adduce a Table which clearly shows, so far as the mass of figures can be relied on, that the fertility of the elder is greater than of the younger, while it lasts; or, in other words, the fertility of the elder is the more intense.

TABLE XVI.—Showing the Intensity of Fertility in Wives Mothers of Different Ages.*

Duration of Marriage.	Mother's Age.						
	15–19.	20–24.	25–29.	30–34.	35–39.	40–44.	45–49.
Under 5 years, . . .	1·128	1·519	1·825	1·844	1·827	1·698	1·200
5 years and under 10,	2·500	3·190	3·750	4·048	4·085	3·792	4·000
10 years and under 15,	...	5·333	5·453	5·903	6·197	5·964	6·500
15 years and under 20,	...	...	6·000	7·379	7·914	7·993	8·435
20 years and under 25,	...	...	...	7·000	9·396	9·718	10·528
25 years and under 30,	...	...	...	...	...	12·368	13·600
30 years, ...	...	...	...	...	...	...	13·000

The conclusion here arrived at is founded upon lengths of married life. Were the figures such as to give, instead of lengths of married life, lengths of intervals between the births of first and last children, the results would be still more striking; for I have already shown that in the case of the elder, fertility is later in beginning to show itself than in the younger.

If, as I have shown, the younger are more prolific than the elder, and if, as also I have shown, the elder are more intensely fertile, while their fertility lasts, than the younger in the same time; then, it necessarily follows, as a corollary, that the fertile women married younger have a longer continued fertility than the fertile women married older. In no other way can the younger surpass the

* The following Tables give all the details and calculations from which Table XVI. is constructed :—

TABLE XVII.—Of Women under 5 Years Married.

	No. of Mothers.	No. of Children.	Average to each Mother.
	7,183	11,880	1·654
Mother's age—16 to 19 years, . . .	374	422	1·128
„ 20 to 24 „ . .	3,180	4,829	1·519
„ 25 to 29 „ . .	2,460	4,489	1·825
„ 30 to 34 „ . .	833	1,536	1·844
„ 35 to 39 „ . .	277	506	1·827
„ 40 to 44 „ . .	53	90	1·698
„ 45 to 49 „ . .	5	6	1·200

elder in their whole fertility; a conclusion which has already been otherwise demonstrated.

It may also be here pointed out, that the figures of Table VI. make it probable that elderly women when fertile, are more intensely so than younger, when their fertility has already resulted in a large family, for that Table shows that the children in large families are born very quickly one after another.

In his work on "the Law of Population," Mr SADLER enters upon this subject of the varying fertility of women according to age. Seeking arguments wherewith to overturn the teaching of MALTHUS, whose principles he hated as well as

TABLE XVIII.—OF WOMEN 5 YEARS MARRIED AND LESS THAN 10.

	No. of Mothers.	No. of Children.	Average to each Mother.
	4,637	17,690	3·815
Mother's age—16 to 19 years, . .	2	5	2·500
„ 20 to 24 „ . .	499	1,592	3·190
„ 25 to 29 „ . .	2,155	8,082	3·750
„ 30 to 34 „ . .	1,418	5,740	4·048
„ 35 to 39 „ . .	461	1,883	4·085
„ 40 to 44 „ . .	96	364	3·792
„ 45 to 49 „ . .	5	20	4·000

TABLE XIX.—OF WOMEN 10 YEARS MARRIED AND LESS THAN 15.

	No. of Mothers.	No. of Children.	Average to each Mother.
	2,739	16,233	5·930
Mother's age—20 to 24 years, . .	9	48	5·333
„ 25 to 29 „ . .	415	2,263	5·453
„ 30 to 34 „ . .	1,345	7,939	5·903
„ 35 to 39 „ . .	814	5,044	6·197
„ 40 to 44 „ . .	140	835	5·964
„ 45 to 49 „ . .	16	104	6·500

TABLE XX.—OF WOMEN 15 YEARS MARRIED AND LESS THAN 20.

	No. of Mothers.	No. of Children.	Average to each Mother.
	1,280	10,013	7·823
Mother's age—25 to 29 years, . .	7	42	6·000
„ 30 to 34 „ . .	253	1,867	7·379
„ 35 to 39 „ . .	721	5,706	7·914
„ 40 to 44 „ . .	273	2,182	7·993
„ 45 to 49 „ . .	23	194	8·435

opposed, he found data which at first sight appear to support his doctrine "that marriages are more prolific the longer they are deferred." Were this true doctrine, it would certainly go far to overturn the Malthusian system, and Mr SADLER might be justly proud of the demonstration. The facts which he adduces may, without cavil, be allowed to be, as he says, indisputable. It is his illogical use of the facts which has to be pointed out. Without pretending to enter on the defence of Malthusian notions, we accept Mr SADLER's challenge "to evade the demonstration," which the aforesaid facts afford. And it is of importance to do so, because, down to the latest authors, SADLER's facts and supposed demonstrations are generally quoted with unsuspicious approval.*

The first data afforded by SADLER are derived from the records of Dr GRANVILLE's experience as physician to the Benevolent Lying-in-Institution and the Westminster Dispensary, the calculations having been made by Mr FINLAYSON.

TABLE XXI.—OF WOMEN 20 YEARS MARRIED AND LESS THAN 25.

	No. of Mothers.	No. of Children.	Average to each Mother.
	432	4,181	9 678
Mother's age—30 to 34 years, . .	1	7	7·000
„ 35 to 39 „ . .	134	1,259	9·396
„ 40 to 44 „ . .	259	2,517	9·718
„ 45 to 49 „ . .	36	379	10·528

TABLE XXII.—OF WOMEN 25 YEARS MARRIED AND LESS THAN 30.

	No. of Mothers.	No. of Children.	Average to each Mother.
	29	371	12·793
Mother's age—40 to 44 years, . .	19	235	12·368
„ 45 to 49 „ . .	10	136	13·600

TABLE XXIII.—OF WOMEN 30 YEARS MARRIED.

	No. of Mothers.	No. of Children.	Average to each Mother.
	1	13	13·000
Mother's age—45 to 49 years, . .	1	13	13·000

See Boudin, Traité de Géographie et de Statistique Médicales, &c., tome ii. p. 59.

TABLE XXIV.—SHOWING THE EFFECT THE POSTPONEMENT OF THE MARRIAGES OF FEMALES HAS UPON THEIR ANNUAL PROLIFICNESS. (SADLER.)

Ages when Married.	Average number of Births for each year of Marriage.	Ages when Married.	Average number of Births for each year of Marriage.
From 13 to 16, . .	·456706	From 29 to 32, . .	·589811
„ 16 to 20, . .	·503610	„ 33 to 36, . .	·776866
„ 21 to 24, . .	·520227	„ 37 to 39, . .	1·125000
„ 25 to 28, . .	·545163		

Now this Table is made from the data of lying-in charities. It is therefore not a Table of fertile women, but of persistently fertile women; for every woman was entered in the records only when she came to have attendance in her confinement. All that the Table offers is corroboration of the law enunciated in this chapter, that elderly women are more fertile than younger, so long as their fertility endures.

It is almost incredible that so acute a reasoner, as Mr SADLER is, could be so deceived by appearances, as to suppose his figures showed that marriages at 39 years of age are as fruitful as marriages of any inferior age, down to 13. Yet, for aught he says, he appears so to believe.

SADLER did, indeed, get the length of seeing that the Table just given was somewhat deficient. " It may," he says,* " perhaps be objected to the whole of the foregoing proofs, that they are derived from a register which cannot profess to give the whole number of children which the marriages it records shall produce, from their commencement to their termination, but only those which have been born to each up to a period within these limits, all the facts which it can record being necessarily retrospective ones. I shall, therefore," he continues, " proceed to another series of proofs of the same principle, which will at once silence every such exception, and afford a strong additional demonstration of its truth. These are derived from the registers of the peerage, which, as I have observed elsewhere, I have gone through in order to collect a body of authentic facts illustrative of many of the principles advanced in these volumes. As far as they relate to the subject before us, those facts are as follows:"—

* Law of Population, vol. ii p. 279.

TABLE XXV.—Showing the Effect of the Postponement of the Marriages of the Peeresses on their Prolificness. (Sadler.)

Period of Marriage.	Number of Marriages.	Number of Children.	Births to each Marriage.
From 12 to 15, . . .	32	141	4·40
„ 16 to 19, . . .	172	797	4·63
„ 20 to 23, . . .	198	1033	5·21
„ 24 to 27, . . .	86	467	5·43

To this Table of Sadler's many objections may be made, such as the paucity and insecurity of the data, as also their deficiency, the highest age of marriage included in them being only 27, and all notice of the important element of the duration of marriage being omitted.

Sadler not only erred in supposing he had demonstrated that late marriages are more prolific than early. He was ignorant also that a larger proportion of the elder than of the younger wives bears no children at all, and that an elderly woman continues fertile a shorter time than a younger, counting, in both cases, only up to periods within the child-bearing portion of life.

It is a natural, and I believe a true, notion, that twin-bearing should be a sign of intense fertility in woman, as the number of a litter certainly is in bitches, and other inferior animals. In confirmation of this notion, and of the law of intensity of fertility now demonstrated, we find that women are more likely to bear twins the older they are. This subject is capable of some interesting developments; but, as I have already elsewhere* entered upon them, I shall add no more in this place, merely remarking, that they were completed at a time when the law of intensity of fertility was only guessed at, and, therefore, when the explanation of the great twin-bearing of old women was not known to me.

In like manner, it is natural to suppose that the length and weight of children should go with intensity of fertility. But my researches† seem to show that this is not the case, but that length and weight of children go with the intensity of fecundity, or likelihood of bearing children, according to age. Professor Hecker, of Munich, has, however, elaborately shown that my conclusions on this head do not agree with those derived from his larger data.‡ Mine are based on 2087 observations only, and I am willing, in the meantime, to hold it as *sub judice*, whether his or my conclusions are to be received. His do appear to me the more

* Edinburgh Medical Journal for March and April 1865. † *Ibid.*, December 1864.

‡ Monatsschrift für Geburtskunde und Frauenkrankheiten, November 1865.

probable, because they bring the laws of length and weight of children, according to the mother's age, into agreement with the law of intensity of fertility here demonstrated.

Chapter XI.—*The Fertility of Elderly Women.*

So ardently did Sadler desire the triumph of his attack on Malthus, that he adopted the dream of Mason Good, who says, "that the usual term (of cessation of the menses) is between 40 and 50, except where women marry late in life, in which case, from the postponement of the generative orgasm, they will occasionally breed beyond their fiftieth year"!!* Mason Good refers to some extraordinary cases of pregnancy in old women, curiosities in physiology, but he adduces no good evidence in favour of the doctrine he here propounds. An opposite doctrine is taught by Burns, an author equally celebrated, and much more worthy of confidence in a question of the kind now before us. "It is well known," says the Glasgow Professor,† "that women can only bear children until a certain age, after which the uterus is no longer capable of performing the action of gestation, or of performing it properly. Now it is observable, that this incapability or imperfection takes place sooner in those who are advanced in life before they marry, than in those who have married and begun to bear children earlier. Thus we find, that a woman who marries at forty shall be very apt to miscarry, whereas had she married at thirty, she might have born children when older than forty; from which it may be inferred, that the organs of generation lose their power of acting properly sooner, if not employed, than in the connubial state. The same cause which tends to induce abortion at a certain age, in those who have remained until that time single, will also, at a period somewhat later, induce it in those who have been younger married; for in them we find that, after bearing several children, it is not uncommon to conclude with an abortion; or sometimes after this incomplete action, the uterus, in a considerable time, recruits, as it were, and the woman carries a child to the full time, after which she ceases to conceive." My own opinion has always coincided with that so well expressed by Burns; and I may add, that the curious observations regarding abortion at the close of the fertile period of life has its analogue in the lower animals. Several times I have been told by men of experience, that an old bitch often ends her career of breeding by a dead and premature pup. Whitehead also‡ regards those pregnancies which occur near the termination of the fruitful period in women as being among the most commonly unsuccessful.

In Edinburgh and Glasgow in 1855, 53 women above the age of 45 bore living

* The Study of Medicine. 1822. Vol. iv. p. 63.
† Principles of Midwifery. Tenth Edition, p. 309.
‡ On Abortion and Sterility, p. 247.

children. Among these 53, only one was primiparous—her age was 49, and she had only been one year married; 2 bore second children,—1 was aged 46 years, and had been four years married—the other was aged 52 years, and had been three years married; 4 bore fourth children; 4 bore fifth children; 3 bore sixth children; 3 bore seventh children; 6 bore eighth children; 8 bore ninth children; 7 bore tenth children; 4 bore eleventh children; 1 bore a twelfth child; 4 bore thirteenth children; 2 bore fourteenth children; 1 bore a fifteenth child; 2 bore sixteenth children; 1 bore a nineteenth child. In short, the great majority of women child-bearing late in life are mothers of considerable families, not women for whom a postponement of the generative orgasm has to be imagined, a circumstance which destroys all shadow of ground for MASON GOOD's supposition. *

This completes my remarks on the fertility of married women. But the subject is susceptible of further interesting developments, by an inverted method of proceeding, which I hope to carry out.

It is evident that the conclusions arrived at in this paper, or others still more definite, can alone form a sure basis for speculation in the great questions in political economy regarding population, and the various means of increasing it, or of retarding its excessive growth. And it is to be hoped that the promoters of that science will avail themselves of information which MALTHUS, SADLER, and their followers, evidently desired ardently to possess. In default of this information, they have fallen into many manifest errors in their groping after truth.

But it is not to the political economist alone that such information is valuable. It will form an element in the guidance of social life, and will certainly greatly contribute to the wisdom in council of the well-informed medical practitioner.

* For other corroborative evidence, see Roberton, Physiology and Diseases of Women, p. 184.

XXIII.—*On some Laws of the Sterility of Women.* By J. Matthews Duncan, M.D.

(Read 19th February 1866.)

Before commencing the discussion of the subject, it is necessary to make some definitions, with a view to avoiding the confusion which extensively prevails, from the neglect of the all-important definition of terms. I might be even more exact than I shall be, and excuse myself from adopting such a seeming improvement, on the ground that further refinement of definition would itself cause confusion in the present stage of advancement of our knowledge.

Absolute sterility, I shall hold to mean the condition of a woman who, under ordinary favourable cirumstances for breeding, produces no living or dead child, nor any kind of abortion.

Sterility, I shall hold to mean the condition of a woman who, under ordinary favourable circumstances for breeding, adds not even one to the population, or produces no living and viable child.

Relative sterility, I shall hold to mean the condition of a woman who, while she may or may not be absolutely sterile, while she may or may not be sterile, is, under ordinary favourable circumstances for breeding, sterile in relation to the circumstance of time; or, in other words, in relation to her age, and the duration of her married life.

Chapter I.—*Sterility of Marriages in our Population.*

Under this head, the age at marriage, and the duration of it, are not regarded. We simply compare the number of people living in the married state, without and with living children. The only information Ihave on this point is derived from the writings of Dr Stark.* "It is a pity," says he, "that when the census was taken up, a query had not been put to every married woman, whether she had borne children. We have at present no means of ascertaining what proportion of the marriages proves unfruitful; and it is no criterion to ascertain the number of married persons who had children living with them on the night of the census. Married persons who had a numerous family, may have none with them, because they are grown up, or are absent at schools or trades. We know, however, from other sources, that a considerable proportion of marriages proves unfruitful; and as it was shown that the married women of Scotland produce

* Census of Scotland, 1861. Population Tables and Report, vol. ii. p. xxxvi.

more children in proportion to their number, than the married women of England, it would have been extremely interesting to have ascertained whether that depended on more of the Scottish married women being fruitful."

On this point I may here interpolate the observation, that, in my opinion, it is highly improbable that there is any essential difference in the fecundity of women in England and in Scotland. The researches now published make it necessary, with a view to settling the question raised by Dr STARK, to look into the comparative ages at marriage of the women in England and Scotland; a difference in that respect alone may prove sufficient to afford the solution of the whole matter. And like remarks are applicable to the supposed great fertility of Irish women.

"As it may," continues Dr STARK, "however, give a distant approximation, it may be stated, that taking two of the largest registration districts of Glasgow, it was found, that of 14,523 married persons living together, 11,718 had children living with them; while 2805 had no children with them. This would yield the proportion of 80·686 per cent. with children, and 19·314 per cent. without children; or, without the decimals, that in every 100 married couples, 81 had children, while 19 had none. These numbers may be safely taken as the proportion in the town populations, seeing that for each district the proportions came out within a very small decimal fraction of one another; also from the circumstance, that in other tables which have been published in the Registrar-General's second detailed Annual Report, relative to the proportions of children born by mothers at different ages in Edinburgh and in Glasgow, the results of the one town almost exactly corresponded with those of the other."

CHAPTER II.—*Sterility of Wives.*

The wives who do not increase the population, may be called sterile. But a wife who has one or several abortions, or who bears one or several dead children, or to whom both of these events happen, adds not a unit to the population; yet such a wife cannot be said to be absolutely sterile. In order to discover the amount of sterility of married women, I proceed on the following plan. I take the registers of Edinburgh and Glasgow for 1855, and find what is the number of first children produced in that year. With this I compare the number of marriages in that year. It is evident that the first children only should be counted, for they indicate all the wives who are not sterile. If one living child is born to a marriage, that marriage is not sterile. Further, it is evident that, although the first births in 1855 will not all pertain to the women married in that year, it may be assumed that, if the marriages be nearly the same in number for a few contiguous years, the first births in one year will give the fertility very accurately of any of the contiguous years. From this fertility, the sterility can be easily computed.

Now, in 1855, there were, in Edinburgh and Glasgow, 4447 marriages, and 3722 first deliveries of living children, leaving 725 marriges sterile, or 1 in 6·1. But in these figures are included 75 marriages which did not take place till after the women had passed 44 years of age, and these will damage the physiological value of the statement, as these 75 women could not be expected to be prolific.

Of women between the ages of 15 and 44 inclusive, there were married 4372; among wives of the same ages, 3710 had first children, leaving 654 marriages sterile, or 1 in 6·6. In other words, 15 per cent. of all the marriages between 15 and 44 years of age, as they occur in our population, are sterile.

The statement of the amount of sterility just given appears to me, from the largeness of the figures used, to be far more valuable than any other I know of. But on account of their great interest, I shall quote the statements of two authors.* "In the Dictionnaire des Sciences Médicales (vol. vi. p. 245; see also Neue Abhandlungen der Schwedischen Akademie der Wissenschaften, vol. xi. p. 70), it is stated," says Sir James Y. Simpson,† "that Hedin, a Swedish minister, had noticed that in his parish, composed of 800 souls, one barren woman is not met with for ten fertile. It is further stated, that Frank asserted, but from what data is not mentioned, that it would be found on investigation, that in most communities containing 300 to 400 couples, at least 6 or 7 would be sterile, without anything in their physical condition to explain the fact. It seems to have been from this assertion of Frank's, that Burdach, who is almost the only author who even alludes to the matter, has given the general statement, that one marriage only in 50 is unproductive (Dr Allen Thomson's excellent essay on Generation, in Todd's Cyclopædia, vol ii. p. 478, foot note).

"For the purpose of ascertaining the point by numerical data, I had a census taken of two villages of considerable size, viz., Grangemouth in Stirlingshire, and Bathgate in West Lothian,—the one consisting principally of a seafaring population, and the other of persons engaged in agriculture and manufacture.

"The following form the results in these two places:—Of 210 marriages in Grangemouth, 182 had offspring; 27 had none; or about one marriage in 10 was without issue. Of the 27 unproductive marriages, all the subjects had lived in wedlock upwards of five years, and in all, the female had been married that period before she reached the age of 45. Again, of 402 marriages in Bathgate, 365 had offspring; 37 had none; or about one marriage in 11 was unproductive. There were at the same time living in the village 122 relicts of marriages, and of

* Lever's statement I here quote, but I cannot ascribe much value to it, because no evidence is adduced, and because there is an evident numerical error in some part of the passage. He says, "It is found that $\frac{1}{20}$th, or 5 per cent. of married women are wholly unprolific."—*Organic Diseases of Uterus*, p. 5.

† Obstetric Works, vol. i. p. 323.

these 102 were mothers; 20 were not mothers; or about 1 in 6 had no family. In all, of 467 wives and widows, 410 had offspring; 57 had none; or about one marriage in 8 was unproductive. Of these last 57, six had not been 5 years married, and there were other six above the age of 45 when married. If we subtract these 12, we have, of 455 marriages, 410 productive; 45 unproductive; or 1 in $10\frac{1}{9}$th without issue.

"Returns such as I have just now adduced are exceedingly difficult to obtain, in consequence of no registers being anywhere kept, so far as I know, that could be brought to bear upon the question. If it had been otherwise, I would here, if possible, have gladly appealed to a larger body of statistical facts, in order to arrive at a more certain and determinate average of the proportion of unproductive marriages in the general community. For the purpose, however, of extending this basis of data, I have analysed, with some care and trouble, the history of 503 marriages, detailed by SHARPE, in his work on the 'British Peerage,' for 1833. Among British peers, there were 401 marriages with issue; 102 without issue; or of 503 existing marriages among British peers in 1833, 74 were without issue, after a period of five years. Of those who had not yet lived in the married state for five years, 28 were still without family; and in BURKE's 'Peerage,' for 1842, there still remained among these 28 marriages, 7 without issue, making 81 as the total number of unproductive marriages among the original 503; or the proportion of the unproductive to the productive marriages among this number is, as nearly as possible, 1 in $6\frac{2}{7}$. In the above calculation, I have excluded 8 unproductive marriages, in which the age of the husband at the date of marriage exceeded 56. These 8, however, ought to be deducted from the original sum of total marriages that were included; or, in other words, the 503 should be reduced to 495, and then the whole result would stand thus:—among 495 marriages in the British Peerage, 81 were unproductive, or 1 in $6\frac{1}{9}$ were without any family." The proportion of unproductive marriages in Grangemouth, Bathgate, and the British Peerage, all taken together, was found by SIMPSON to be 1 in$8\frac{4}{7}$.

Dr WEST* states, that he found the general average of sterile marriages, among his patients at St Bartholomew's Hospital, to be 1 sterile marriage in every 8·5.†

CHAPTER III.—*Absolute Sterility of Wives.*

In order to arrive at the absolute sterility of the wives in Edinburgh and Glasgow, it is necessary to add to the number of wives bearing first living children, the number of those who bear only dead children or abortions.

* Diseases of Women, 3d edition, p. 3.

† A statement of the sterility of Esquimaux women is given by ROBERTON, Essays and Notes on the Physiology and Diseases of Women, p. 53.

The number of abortions has been variously estimated by GRAUNT, SHORT, WHITEHEAD, and others. The number of children born dead has been the subject of much investigation, among others by JACQUEMIER, BOUDIN, and LEGOYT. But were our information on these points very exact, it would not help us in this inquiry. For our purpose, the desideratum is not the number of abortions in a number of pregnancies, nor the number of children born dead in a number of births, but the proportional number of married women who produce nought else than abortions or dead children; who, while not absolutely sterile, yet add none to the population. Of this class of wives I know of no estimate.* I believe they are few, and I leave the statement of the sterile as a near approximation to a correct statement of the absolutely sterile.

CHAPTER IV.—*Sterility according to the Ages of Wives.*

To illustrate the variations of sterility according to age, I bring forward the accompanying Table (I.).

TABLE I.—SHOWING THE VARIATIONS OF STERILITY ACCORDING TO THE AGES OF THE WIVES.

Ages of Wives at Marriage, ...	15–19	20–24	25–29	30–34	35–39	40–44	45–49	50, &c.	Total.
Number of Wives, . .	700	1835	1120	402	205	110	46	29	4447
First Children, . . .	649	1905	809	251	96	10	2	...	3722
Sterile Wives, . . .	51	...	311	151	109	100	44	29	725
Percentage Sterile, . .	7·3	0	27·7	37·5	53·2	90·9	95·6	100	16·3
Proportion Sterile, 1 in	13·72	0	3·60	2·66	1·88	1·10	1·05	1·00	6·13

With the numbers of marriages taking place in Edinburgh and Glasgow in 1855, at different ages of the wives, are compared the numbers of first children born in the same year to wives married at the same ages in that year or previously. The number of sterile wives is got by subtracting the latter figures from the former, and the percentage of sterile marriages is given in the penultimate horizontal line.

So far as the numbers are to be relied upon, we have from this Table the interesting results, that about 7 per cent. of all the marriages between 15 and 19 years of age inclusive, and as they occur in our population, are without offspring; that those married at ages from 20 to 24 inclusive, are almost all fertile; and

* The following extract from the work of Dr WEST, on Diseases of Women (3d edit., p. 367), may be of some value. It refers to the histories of a set of poor women labouring under uterine cancer. "There were but two out of the whole 150 women, whose pregnancy had issued merely in abortion."

that, after that age, sterility gradually increases according to the greater age at the time of marriage.

CHAPTER V.—*Expectation of Sterility.*

The main element in the expectation of sterility is the age of the woman at marriage. This has just been described. But, besides this, our statistics suggest to us other laws as to the expectation of sterility. Of these the first is :—

That the probability of a woman's being sterile is decided in 3 years of married life. For while a large number are fertile in each of the first three years of married life, only 7 per cent. of the fertile bear after 3 years of marriage, or about 1 in 13.

TABLE II.—SHOWING THE FERTILITY OF MOTHERS, OF DIFFERENT AGES AT MARRIAGE, COMMENCING AFTER THREE YEARS OF MARRIED LIFE.

Mother's Age at Marriage, .	15–19	20–24	25–29	30–34	35–39	40–44	45–49	Total.
Number of Fertile, . . .	649	1905	809	251	96	10	2	3722
Number commencing Fertility after being 3 years married,	63	119	62	27	15	1	...	287
Percentage commencing Fertility after being 3 years married, ...	9·7	6·2	7·7	10·7	15·6	10·0	...	7·7
or 1 in	10·3	16·0	13·0	9·3	6·4	10·0	...	13·0

This same Table affords us a second law of expectation of sterility :—

That when the expectation of fertility is greatest, the probability of sterility is soonest decided, and vice versa. For our Tables show that of the wives married from 20 to 24 who are all fertile, only 6·2 per cent. begin to breed after three years of marriage; while at the other ages, with less fecundity, a greater percentage commences after the completion of the third year of marriage.

CHAPTER VI.—*Relative Sterility.*

Here I take into consideration only those who have borne children, only those who are not sterile. Of course all these wives, if they survive in wedlock, will sooner or later become relatively sterile. Now, in a paper lately read to this Society, I showed that the prolongation of fertility was greater according as the age at marriage was less. From this conclusion it is easy to derive one in regard to relative sterility, to the effect that :—

Relative sterility will sooner arrive according as the age at marriage is greater.

The demonstration of these proportions is arrived at by showing the proportional numbers bearing at different years of married life, according to age at marriage. This is an indirect way of proceeding, but it is the only one I can find available, while I have no documents giving the ages of mothers at marriage, and their ages at birth of last children, the mothers continuing to live in wedlock.

TABLE III.—SHOWING THE RELATIVE STERILITY OF A MASS OF WIVES MARRIED AT DIFFERENT AGES AT SUCCEEDING EPOCHS IN MARRIED LIFE.

Age of Mother at Marriage,	15–19	20–24	25–29	30–34	35–39	Total.
Proportion Sterile about the 5th Year of Married Life is about 1 in	2·78	2·61	1·68	1·51	1·19	2·09
or a percentage of	35·9	38·3	59·4	66·0	84·1	47·9
Proportion Sterile about the 10th Year of Married Life is about 1 in	2·09	1·71	1·39	1·24	...	1·61
or a percentage of	47·9	58·3	71·8	80·8	...	62·1
Proportion Sterile about the 15th Year of Married Life is about 1 in	1·57	1·32	1·10	1·05	...	1·26
or a percentage of	63·8	75·5	90·9	95·5	...	79·2
Proportion Sterile about the 20th Year of Married Life is about 1 in	1·24	1·13	1·01	...	...	1·11
or a percentage of	80·4	88·6	98·7	...	...	89·8
Proportion Sterile about the 25th Year of Married Life is about 1 in	1·02	1·00	...	...	...	1·01
or a percentage of	97·6	99·65	...	...	...	99 03

Table III. gives the calculated amounts of sterility at different periods of married life in women married at different ages. It is needless to enter on the method of construction of this Table. It is merely the complement of Table XV., given in my former paper, where full details are stated. I shall only state, that this Table is all calculated for 20 months, with a view to giving the nearest accurate estimate, 20 months being what I have called the time-unit of fertility, the shortest time within which all women may be expected to show fertility if they possess it.

CHAPTER VII.—*Expectation of Relative Sterility.*

As a sort of appendix to this paper, I produce five Tables, giving all the details of the expectation of continued fertility; and conversely, of relative sterility. These Tables not only give data for calculating the chances of relative sterility, but also for calculating the probable number of the family produced in women at different ages becoming relatively sterile. To enter further upon these considerations would be merely to give in writing what is more succinctly stated in the Tables themselves.

TABLE IV.—FIFTH YEAR OF MARRIED LIFE.

Number of Child, ...	1st	2d	3rd	4th	5th	6th	7th	8th	9th	Total.
Wives Mothers, of Ages 20–24, .	13	39	160	31	4	...	...	...	...	247
Proportion of above to 644 Wives Married at from 15–19 is 1 in	49·5	16·5	4·0	20·8	161·0	...	..	...	...	2·6
Wives Mothers, of Ages 25–29, .	10	82	398	106	13	2	...	...	...	611
Proportion of above to 1686 Wives Married at from 20–24 is 1 in	168·6	20·5	4·2	15·9	13·0	843·0	...	...	...	2·7
Wives Mothers, of Ages 30–34, .	3	31	147	52	8	2	1	...	...	244
Proportion of above to 1008 Wives Married at from 25–29 is 1 in	336·0	32·5	6·8	19·4	126·0	504·0	1008	...	...	4·1
Wives Mothers, of Ages 35–39, .	3	12	37	14	1	1	2	1	1	72
Proportion of above to 358 Wives Married at from 30–34 is 1 in	119·3	29·8	9·7	25·6	358·	358·	179·	358·	358·	4·9
Wives Mothers, of Ages 40–44, .	3	2	11	1	...	...	...	...	...	17
Proportion of above to 179 Wives Married at from 35–39 is 1 in	59·6	89·5	16·3	179·	...	...	...	...	...	10·5
Total Wives Mothers, of Ages 20–44,	32	166	753	204	26	5	3	1	1	1191
Proportion of above to 3875 Wives Married at from 15–39 is 1 in	121·1	23·3	5·1	19·0	149·	775·	1291·6	3875·	3875·	3·2

Lastly, I state a law of relative sterility for which I do not here adduce the numerical proofs, these having already been given in my paper lately read to the Society. This law is, that:—

A wife who having had children has ceased for three years to exhibit fertility, has probably become relatively sterile; that is, will probably bear no more children; and the probability increases as time elapses. For the probability of sterility only commences after three years of sterile marriage. Further, the data given in Table II. of the paper just referred to, show that fertile women bear a child, on an average, about every two years, so long as they remain fecund. The data given in Table VI. show that successive children in a family succeed one another with an average interval of about 20 months. To these propositions I have to add the general consent, shown in the same paper, that fertile wives breed generally every two years; consequently, that no class breeds, though individuals do, at shorter intervals; and no class breeds at longer intervals, though individuals do so. Considering these different statements, it is apparent to the student, that there is no room left for any but a very inconsiderable number of women to breed at longer intervals than two years. For were there any considerable number of wives breeding at longer intervals, the averages just given would be far overpassed. And some of these averages are, as already shown, considerably less than were believed to be the true averages by writers who were not thinking of the law now demonstrated, but of the ordinary rate of time-fertility of married women.

Besides, being of evident intrinsic value, the conclusions arrived at in this paper will afford to medical men, means of estimating the utility of the many vaunted methods of curing sterility which are now much in vogue, and which, considering the nature of the condition to be cured, justly excite anxiety for the honour of the profession in the minds of its best friends.

TABLE V.—Tenth Year of Married Life.

Number of Child,	1st	2d	3d	4th	5th	6th	7th	8th	9th	10th	11th	Total.
Wives Mothers, of Ages 25–29,	...	1	14	30	78	51	8	4	...	...	...	186
Proportion of above to 594 Wives Married at from 15–19 is 1 in	...	594·	42·4	19·8	7·6	11·6	74·2	148·5	...	...	...	3·2
Wives Mothers, of Ages 30–34,	1	4	17	55	148	105	34	11	3	1	2	381
Proportion of above to 1528 Wives Married at from 20–24 is 1 in	1528·	382·	89·9	27·8	10·3	14·5	44·9	138·9	509·3	1528·	764·	4·0
Wives Mothers, of Ages 35–39,	...	2	4	19	60	48	13	5	2	...	...	153
Proportion of above to 902 Wives Married at from 25–29 is 1 in	...	451·	225·5	47·5	15·	18·8	69·4	180·4	451·	...	...	5·9
Wives Mothers, of Ages 40–44,	...	...	5	11	10	6	2	1	...	1	...	36
Proportion of above to 313 Wives Married at from 30–34 is 1 in	...	...	62·6	28·5	31·3	52·2	156·5	313·	...	313·	...	8·7
Total Wives Mothers, of Ages 25–44, .	1	7	40	115	296	210	57	21	5	2	2	756
Proportion of above to 3337 Wives Married at from 15–34 is 1 in	3337·	476·7	83·4	29·0	11·3	15·9	58·5	158·9	667·4	1668·5	1668·5	4·4

TABLE VI.—FIFTEENTH YEAR OF MARRIED LIFE.

Number of Child, . .	1st	2d	3d	4th	5th	6th	7th	8th	9th	10th	11th	12th	13th	Total
Wives Mothers, of Ages 30–34, . .	1	3	2	6	11	18	24	28	18	2	1	1	1	116
Proportion of above to 532 Wives Married at from 15–19 is 1 in	532·	177·3	266·	88·7	48·4	29·6	22·2	19·0	29·6	266·	532·	532·	532·	4·6
Wives Mothers, of Ages 35–39, . .	...	...	5	4	18	32	53	41	29	14	2	1	1	200
Proportion of above to 1360 Wives Married at from 20–24 is 1 in	...	...	272·	340·	75·5	42·5	25·6	33·2	46·9	97·1	680·	1360·	1360·	6·8
Wives Mothers, of Ages 40–44, . .	...	...	1	2	4	7	12	14	2	1	...	...	...	43
Proportion of above to 782 Wives Married at from 25–29 is 1 in	...	...	782·	391·	195·5	111·7	65·2	55·9	391·	782·	...	...	...	18·2
Wives Mothers, of Ages 45–49, . .	...	...	...	1	1	1	...	1	2	...	1	...	...	7
Proportion of above to 262 Wives Married at from 30–34 is 1 in	...	...	...	262·	262·	262·	...	262·	131·	...	262·	...	...	37·4
Total Wives Mothers, of Ages 30–49, . .	1	3	8	13	34	58	89	84	51	17	4	2	2	366
Proportion of above to 2936 Wives Married at from 15–34 is 1 in	2936·	978·6	367·	225·8	86·3	50·6	33·0	34·9	57·5	172·7	734·	1468·	1468·	8·0

TABLE VII.—TWENTIETH YEAR OF MARRIED LIFE.

Number of Child, . .	4th	5th	6th	7th	8th	9th	10th	11th	12th	13th	Total.
Wives Mothers, of Ages 35–39, ...	...	...	2	5	5	17	15	9	3	...	56
Proportion of above to 477 Wives Married at from 15–19 is 1 in	...	...	238·5	95·4	95·4	28·0	31·8	53·	159·	...	8·5
Wives Mothers, of Ages 40–44, ...	1	1	2	9	14	28	8	13	2	2	80
Proportion of above to 1171 Wives Married at from 20–24 is 1 in	1171·	1171·	585·5	130·	83·6	41·8	146·4	90·1	585·5	585·5	14·6
Wives Mothers, of Ages 45–49, ...	...	...	...	...	...	3	...	1	1	...	5
Proportion of above to 649 Wives Married at from 25–29 is 1 in	...	...	...	...	...	216·3	...	649	649	...	129·8
Total Wives Mothers, of Ages 35–49, .	1	1	4	14	19	48	23	23	6	2	141
Proportion of above to 2297 Wives Married at from 15–29 is 1 in	2297·	2297·	574·2	164·	121·	47·8	99·9	99·9	383·	1148·	16·3

TABLE VIII.—TWENTY-FIFTH YEAR OF MARRIED LIFE.

No. of Child, ...	10th	11th	12th	13th	14th	15th	16th	17th	Total.
Wives Mothers, of Ages 40–44,	2	2	1	1	...	...	...	...	6
Proportion of above to 408 Wives Married at from 15–19,	204·	204·	408·	408·	...	...	...	...	68·
Wives Mothers, of Ages 45–49,	...	1	...	...	...	...	...	1	2
Proportion of above to 961 Wives Married at from 20–24,	...	961·	...	...	...	...	...	961·	480·5
Total Wives Mothers, of Ages 40–49, ...	2	3	1	1	...	...	...	1	8
Proportion of above to 1369 Wives Married at from 15–24,	684·	456·	1369·	1369·	...	...	...	1369·	171·

[illegible]

XXIV.—*On a New Property of the Retina.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., &c.

(Read 19th February 1866.)

In a paper on Hemiopsy, published in the present volume of the Transactions (p. 15), I have mentioned the remarkable fact, that the parts of the retina which are insensible to visual, are sensible to luminous impressions, the light being occasioned by irradiation from the adjacent parts of the retina. The parts thus affected in hemiopsy extend irregularly from the *foramen centrale* to the margin of the retina; but the space which they occupy is so small, their distribution so irregular, and the time of their continuance so short, that it is difficult to make such observations upon them as would establish a general property of the retina.

Mr AIRY, our distinguished Astronomer-Royal, who has had more than twenty attacks of hemiopsy, has been induced, by the perusal of my paper, to describe their character, and delineate the form of the parts insensible to visual impressions.* The hemiopsy, in his case, commences at the *foramen centrale* c, Fig. 1, and extends outwards in a zig-zag curve line, the curve "being small at first, and gradually increasing in dimensions," as shown in the figure. It is accompanied with "tremor and boiling so oppressive, that if produced only in one eye, they may nearly extinguish the corresponding vision in the other," and it lasts from twenty to thirty minutes. It occurs sometimes on one side, and sometimes on the other side, of the foramen; and Mr AIRY has "never been able to decide with certainty whether the disease really affects both eyes." On one occasion, when under its influence, he lost "his usual command of speech, and his memory failed so much that he did not know what he had said, or had attempted to say, and that he might be talking incoherently." He, therefore, entertained "no doubt that the seat of the disease was in the brain; that the disease is a species of paralysis; and that the ocular affection is only a secondary symptom."

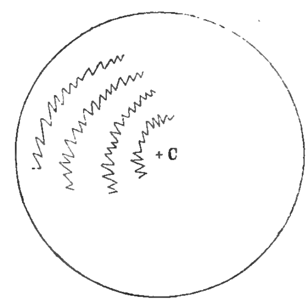


Fig. 1.

From these important facts, it will be seen that Mr AIRY's case differs essentially from mine, in which the locality of the indistinctness occurs in irregular

* Philosophical Magazine, July 1865, vol. xxx. p. 19.

zig-zag lines proceeding, as in Fig. 2, from the foramen outwards, and not in a circular arch, as shown in Fig 1. The "general obscuration," mentioned by Mr Airy, shows that the luminous impression on the affected parts is not so strong in his case as in mine, and that the retina is still sensible to light derived from the surrounding parts by irradiation. The severity of the affection in Mr Airy's case is remarkable. In mine the attack is little more than disagreeable, and I have never experienced the slightest effect either upon the speech or the memory. I have given this brief abstract of Mr Airy's interesting paper from the relation of hemiopsy to the permanent affection of the retina which I am about to describe.

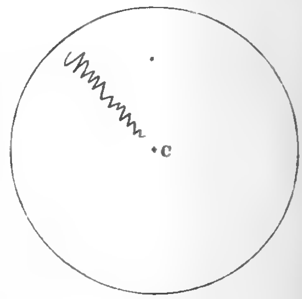


Fig. 2.

When without the hope of obtaining any precise information respecting the irradiation into the parts of the retina affected with hemiopsy, an accidental observation revealed to me the disagreeable fact that a considerable portion of the retina of my right eye was absolutely blind, or insensible to visual impressions; and I have thus been enabled, from the extent and permanence of the affection, to make whatever observations were necessary to ascertain the true character of the phenomenon.

The portion of the retina thus affected with what may be called *local amaurosis* is situated, in the field of vision, about 15° from the foramen, in a line to the left inclined 45° to the horizon. Its angular magnitude is about 6° in its greatest breadth, which corresponds to a space about the twenty-eighth of an inch on the retina.

When the image of a bright object covers the whole, or any part of this spot, it is invisible. If the image is the flame of a candle, or of the moon, or of the sun near the horizon, it is wholly invisible. The eye is therefore at this part of it absolutely insensible to light falling upon it from without. If we now direct the eye to the sky, to the white ceiling of a room, or to any extended white surface, no dark spot, even of the slightest shade, is seen in the field of vision. The portion of the retina, therefore, insensible to light incident upon it directly, or from without, has been illuminated by irradiation from the surrounding parts. But for this wise provision, an eye affected with local amaurosis would carry about with it a black spot, disfiguring the aspects of nature, and ever reminding the patient of his misfortune.

How long this condition of my retina has existed, I cannot discover. It may have existed for half a century, or more; and, but for a casual observation, its existence might never have been discovered. Whether it came on gradually, or was produced in some of the experiments in which the eye was exposed to the light of the sun, I have no means of ascertaining. If from the first of these causes, it is likely to extend itself; if from the second, it may remain as it is. Having observed it only for a year without noticing any enlargement, it is probable that it was produced by the strong action of light.

Owing to the compound structure of the retina, consisting of different layers, and these layers composed of bodies of different shapes, it is very difficult to discover the part which each of them performs in the act of vision; but considering each element of the retina as a *rod*, the end of which next the vitreous humour is an expansion of the optic nerve, we know that distinct vision of external objects arises from the law of visible direction, by which every ray of light, at whatever angle it may fall, gives us vision of the point from which it proceeds, in a direction perpendicular to the part of the membrane on which it is incident. When this outer layer of the retina is insensible to the light of external objects, its luminosity, or the light which it exhibits, may be received from the surrounding parts of the expanded nerve by irradiation, or from the parts of the elemental rods behind it, if they were not paralysed, or if they are, by the action of the unparalysed rods around them.

Although in hemiopsy, and in the case of local amaurosis which I have described, the paralysed parts are still luminous, yet there are cases in which these parts are absolutely black, and into which no light is introduced by irradiation. An example of this fact presented itself to me in the morning of the 16th October 1837, and is represented in Fig. 3, where two black curved lines proceeded from the *foramen centrale* of the retina of the right eye. These lines were so black that, in the memorandum which I made at the time, I state that they were blacker than the black ink lines upon the paper. The lines continued only about *ten* minutes, and were probably produced by the pressure of blood-vessels, as I had, the day before, been subject to much giddiness. In this case, the elementary rods of the retina beneath these lines must have been paralysed throughout their length; and, therefore, it is probable that in the cases of hemiopsy and local amaurosis, the paralysis affects only the end of the rods in contact with the vitreous humour, and formed by the expansion of the optic nerve.

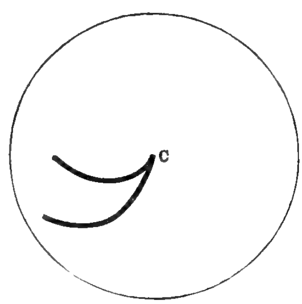


Fig. 3.

In concluding this notice, I would suggest to philosophers and medical practitioners the importance of studying the manner in which sight and hearing are, in their own case, gradually impaired, for it is in the decay or decomposition of organic structures, as well as in their origin and growth, that valuable results may be presented to the physiologist; and facts of this kind have a peculiar value when the patient is himself a practised observer.

XXV.—*On the Classification of Chemical Substances, by means of Generic Radicals.* By Alexander Crum Brown, M.D., D.Sc.

(Read 5th February 1866.)

The idea of chemical structure, as founded on that of atomicity (or the equivalence of atoms), enables us to divide any molecule, whose chemical structure is known, into radicals. The number of ways in which this may be done increases with the complexity of the molecule. Each of these modes of division corresponds to a series of conceivable reactions, some of which have been observed. Any one of these series may be made the basis of classification; but it is obviously most convenient to select for this purpose the most characteristic reactions, and those which are common to such substances as form natural groups. In studying these, we find that each series implies the presence of a particular radical, within which the reactions in question take place. We may call such series of reactions the Generic reactions, and the corresponding radicals Generic radicals. These are sometimes residues of double decomposition, but very frequently this is not the case, and this may account for the fact, that the importance of these generic radicals has been very much overlooked.

I shall consider some of the cases in which this principle of classification is already, to some extent, recognised, before proceeding to apply it generally, and examine first those examples furnished by groups of bodies which are referred to the types H_2O and NH_3.

I. A large number of the substances referred to the type H_2O, are formed by the replacement of *one* atom of H in each molecule of H_2O, by a radical, as,—

$$\left.\begin{matrix}K\\H\end{matrix}\right\}O,\quad \left.\begin{matrix}C_2H_5\\H\end{matrix}\right\}O,\quad \left.\begin{matrix}C_2H_3O\\H\end{matrix}\right\}O,\quad \begin{matrix}H\\C_2H_4\\H\end{matrix}\begin{matrix}\}O\\ \\ \}O,\end{matrix}\quad \begin{matrix}H\\SO_2\\H\end{matrix}\begin{matrix}\}O\\ \\ \}O,\end{matrix}\quad \&c.$$

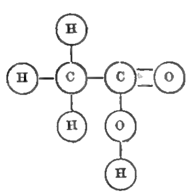
The part common to all such bodies is the radical $(HO)'$ or $\left.\begin{matrix}\\ \dot{H}\end{matrix}\right\}O$, and the reactions common to the group affect this radical alone. These reactions are—1*st*, The replacement of $(HO)'$; 2*d*, The replacement of H in $(HO)'$; and 3*d*, The replacement of O in $(HO)'$. Thus (HO) may be replaced by Cl, Br, &c., or $(HO)_2$ by O; $(HO)'$ may become $(KO)'$, $(C_2H_5O)'$, &c. by the replacement of H, or (HS), $(HSO_4)'$, &c. by replacement of O. $(HO)'$ is therefore the generic radical of this large genus.

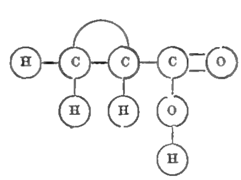
II. An extensive series of substances referrible to the type NH_3 have only *one* atom of H in each molecule of ammonia replaced.

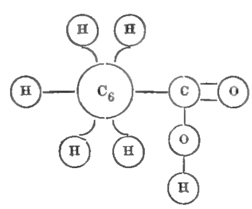
As $\left.\begin{matrix}C_2H_5\\ H_2\end{matrix}\right\}N$, $\left.\begin{matrix}C_2H_3O\\ H_2\end{matrix}\right\}N$, $\left.\begin{matrix}C_2H_3O_2\\ H_2\end{matrix}\right\}N$, $\begin{matrix}H_2\\ C_2H_4\\ H_2\end{matrix}\begin{matrix}\}N\\ \\ \}N\end{matrix}$ $\begin{matrix}H_2\\ C_2O_2\\ H_2\end{matrix}\begin{matrix}\}N,\\ \\ \}N,\end{matrix}$ &c.

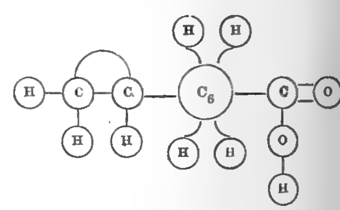
In these we have $(NH_2)'$, or $\left.\begin{matrix}\cdot\\ H_2\end{matrix}\right\}N$ common to the group. The generic reactions are—1*st*, Replacement of one or both atoms of H in NH_2 by radicals. 2*d*, The addition of two monatomic atoms to each NH_2, N''' becoming N^v. 3*d*, The replacement of N''' by O'' and $(HO)'$ (by the action of nitrous acid), or what comes to the same thing, the replacement of NH_2 by HO. All these affect the radical NH_2 alone, which is therefore the generic radical. In the same way $(NH)''$ is the generic radical of the substances derived from ammonia, by the replacement of two atoms of H.

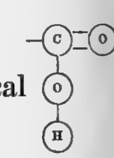
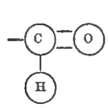
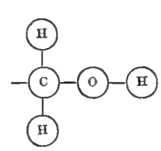
The best way of extending this method of classification generally, is to examine the chemical structure of those bodies which form well-marked genera, and see what group of atoms they have in common, and whether the common group (or radical) is that part of the molecule, in which the reactions characteristic of the genus take place. One very well-marked genus is that of the monobasic acids, which have aldehydes and alcohols corresponding to them (KOLBE's "Monocarbonsäuren"). As examples, we may take acetic acid, acrylic acid, benzoic acid, and cinnamic acid. The chemical structure of these substances is represented, as far as known, by the following graphic formulæ:—

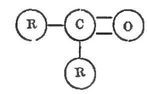

Acetic Acid.

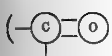
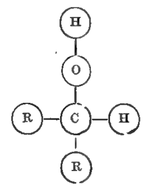

Acrylic Acid.


Benzoic Acid.


Cinnamic Acid.

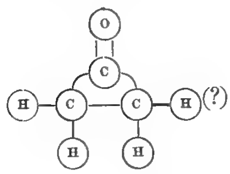
The part common to all these acids is obviously the monatomic radical  and the generic reactions take place within it; the salts are formed by the replacement of the H by metal,—chlorides, bromides, aldehydes, &c., by the replacement of the $(HO)'$, by Cl, Br, H, &c.; alcohols from the aldehydes, by the addition of H_2  becoming ; amides, by the replacement of $(HO)'$ by NH_2; nitriles, by the replacement of O'' and $(HO)'$ by N'''; acetones, from two molecules, by the loss of CO(HO) in one, and the substitution of the radical thus produced for HO in the other. $\{(CO(HO)\}'$ is, then, the generic radical of these acids. As this radical is of very great importance, it is advisible to indicate it by a single symbol, and I shall use the Greek letter Ξ for this purpose.* The relation of Ξ to CN, or of the acid to the nitrile, is seen not only in the monobasic acids (monocarbonsäuren), but also in the di- and tri-basic acids (di- and tricarbonsäuren), so that these contain the radical Ξ, two and three times respectively.

The investigation sketched above also shows that COH and $CH_2(HO)$, are the radicals of the aldehydes and the "true" alcohols. Pursuing this method further, we arrive at a system of classification for the various groups of pseudo-alcohols, the number of which has recently increased so much. One of these groups is formed by the hydrogenation of the acetones. The acetones have the general formula COR_2 (in which R_2 may represent either two atoms of the same or of two different radicals); taking the graphic formula, we have 

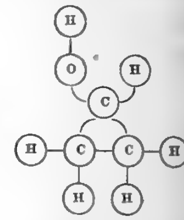
( being the generic radical), by the addition of H_2, we get  the reaction being similar to that by which the aldehydes are converted into true alcohols, one of the two pairs of equivalents by which the O atom is united to the C, being separated, and hydrogen added to each of the equivalents (one of O and one of C), thus rendered free. The generic radical here is obviously $\{(CH(HO)\}''$, and the subgenera and individual substances are determined by the radicals saturating the two free equivalents of this generic radical. This genus, besides the universal character of the alcohol family (the formation of ethers) has the property of forming aldehydic bodies (acetones) by the loss of two atoms of H, $\{CH(HO)\}''$ becoming $(CO)''$; and in that subgenus which contains the radical $\{CH_2(HO)\}'$ (or in which one of the R's is H), this aldehydic body is a true aldehyde, capable of forming an acid by further oxidation. When none of the equivalents of the carbon atom in the generic radical are directly saturated with H, the alcohol is incapable of producing an aldehyde or acetone; and, in this case, we have the characteristic radical reduced to $\{C(HO)\}'''$, as in BUTLEROW's trimethyl alcohol. We thus see that the most general form of alcohol is $C(HO)R_3$ (where R_3 represents one triatomic, or one diatomic and one monatomic, or three monatomic radicals); and the genera, sub-genera, and individuals of this family are determined by the nature of the radical or radicals, combined with the family radical $\{C(HO)\}'''$. For convenience let us, in the meantime, represent this radical by the symbol Φ'''. The different subdivisions of the family will then be $R'''\Phi$, $R''R'\Phi$, and $R'_3\Phi$. If

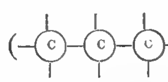
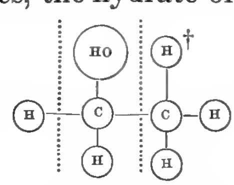
* I had proposed to express the radical $(COHO)'$ by the symbol Ξ, before I was aware that BUTLEROW had already used the symbol A to represent the same radical. While fully acknowledging the priority of BUTLEROW's recognition of this radical, I prefer to retain the symbol Ξ. By using such of the Greek capitals as differ from the Roman in form, to represent generic radicals, we avoid the danger of confounding them with elementary atoms.

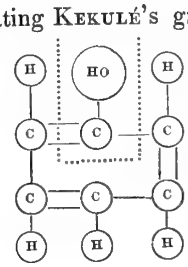
the original and ingenious speculations of KEKULÉ on the constitution of the aromatic bodies should be experimentally confirmed, phenylic alcohol would be an example of the first form $(C_5H_5)'''\Phi$.* The second and third form may be subdivided into genera, having the generic radicals $(R'\Phi)''$. As we have seen, by far the most important of these is that containing $H\Phi$, and it may be convenient to express this by a separate symbol, say Θ''. Under this genus we have two forms, $R''\Theta$ and $R'_2\Theta$. The so-called ketones of the dibasic carbon acids (dicarbonsäuren) may be regarded as the aldehydes of unknown alcohols of the first form; thus succinone

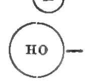
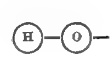


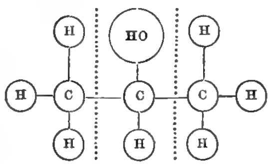
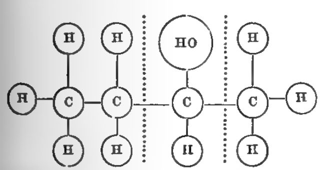
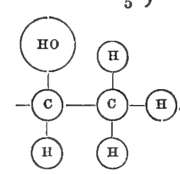
may be considered as the aldehyde of the unknown alcohol, ...

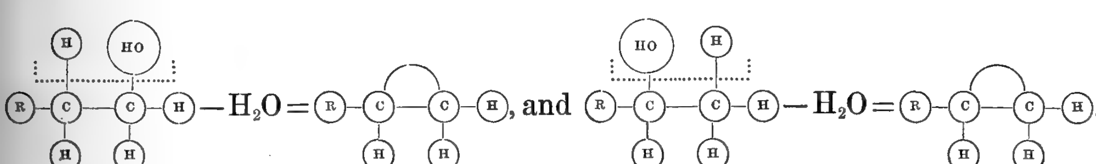


To the second form $R'_2\Theta$, belong the greater number of known alcohols, and a considerable number of bodies possessing alcoholic properties, though not generally classified with the alcohols. They form aldehydes or acetones by oxidation; and this reaction is not confined to those substances to which the name alcohol is commonly applied: for instance, mesoxalic acid is the aldehyde (or acetone) of tartronic acid, alloxan that of dialuric acid, glyoxylic acid that of glycollic acid. Confining our attention to those substances of the form $R'_2\Theta$, in which R is of the form $(C_nH_{2n+1})'$, and has its C atoms arranged in the simplest way (, &c.), we may form the following series of sub-generic radicals $H\Theta$, $CH_3\Theta$, $C_2H_5\Theta$, &c. The first of these we have seen to be the radical of the "true" alcohols, and it seems probable that the second is that of the hydrates of the olefines. The arguments in favour of this view (which undoubtedly requires and admits of further experimental research) may be stated thus:—The first member of the series, the hydrate of ethylene, is identical with common alcohol, and has the formula  † or, $\left.\begin{matrix}CH_3\\H\end{matrix}\right\}\Theta$. The

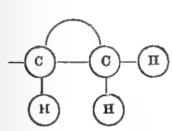
* Translating KEKULÉ's graphic formula for phenylic alcohol into the system used in this paper, we have  in which we have the triatomic radical C(HO) united to the triatomic radical C_5H_5.

† Using the symbol  as a contraction for .

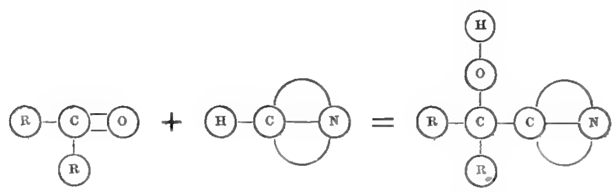
hydrate of propylene is almost certainly identical with the alcohol derived from acetone, which is  or $\left.\begin{matrix}CH_3\\CH_3\end{matrix}\right\}\Theta$. The constitution of hydrate of butylene has not been directly ascertained, but as it is obviously *meta*meric with butylic alcohol, and as it is in the highest degree improbable that its C atoms are arranged in a different way, we may safely assume that its formula is $\left.\begin{matrix}CH_3\\C\ H_5\end{matrix}\right\}\Theta$, . In all these bodies we have the radical , $\left.\begin{matrix}CH_3\\ \cdot\end{matrix}\right\}\Theta$, and from the similarity of the method by which the others are produced, it is reasonable to infer that they are similarly constituted. If so, while the "true" alcohols have the water residue united to what we may call a *terminal* C atom, the olefine hydrates have the water residue united, not to this, but to the C atom next to it. As both alcohols give the olefine by dehydration, the carbon equivalents deprived of HO and H, must be those which are combined with HO, the one in the one, and the other in the other. Thus:—

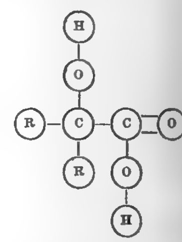


True Alcohol. Olefine. Olefine Hydrate. Olefine.

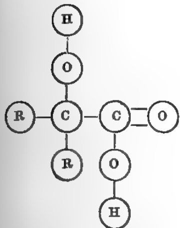
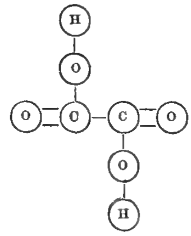
So that  $(C_2H_3)'$ must be the generic radical of the olefines. As the true alcohols are formed from the aldehydes, or, as they may be called, the formo-ketones (as produced by distilling a mixture of formiate of lime with another lime salt), so we may presume that the alcohols $\left.\begin{matrix}CH_3\\R\end{matrix}\right\}\Theta$, may be produced from the aceto-ketones (the ketones formed by distilling acetate of lime with other lime salts), and the same is no doubt the case with all the other genera of pseudo-alcohols of the form $R'_2\Theta$. $(HCO)'$, $(CH_3CO)'$, $(C_2H_5CO)'$, &c., becoming $(H\Theta')$, $(CH_3\Theta)'$, $(C_2H_5\Theta)'$, &c. The same considerations would, of course, apply to other series besides that of completely saturated bodies, but it is unnecessary to do more than mention this as LINNEMANN's benzhydrol $(C_6H_5)_2\Theta$ stands, as yet, alone as a pseudo-alcohol of this class among non-saturated bodies.

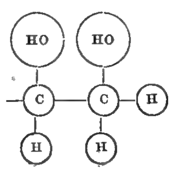
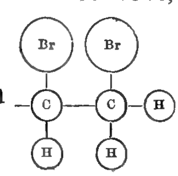
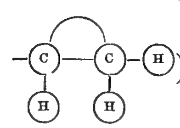
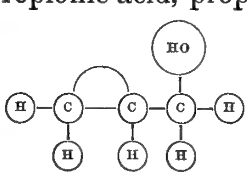
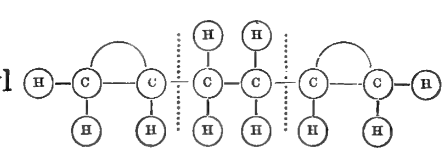
Derived from the aldehydes and acetones, we have another series of substances, having well-marked reactions in common, and forming what we may call a subgenus of the genus containing the radical Ξ. These are the acids obtained by the action of HCl and HCN, on the aldehydes and acetones. The mode of formation of these acids appears to be the following:—To the aldehyde or ketone HCN is first added in the same way as H_2 is added to form the alcohol, a body which may be called an *oxynitrile* being then produced.



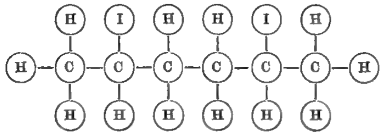
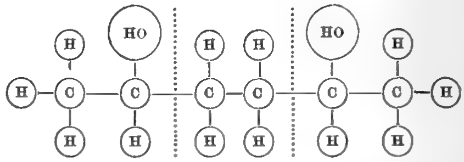
(this stage of the reaction can be traced in the case of bitter almond-oil; the so-called hydrocyanate of bitter almond-oil being, no doubt, the nitrile of mandelic acid). In the second stage of the reaction this nitrile is decomposed (like other nitriles) by the HCl and water, yielding NH_4Cl, and the acid 

by this reaction it will be seen that the radical $(CO)''$ has been transformed into $(C(HO)\Xi)''$ or $(\Phi\Xi)''$, which is therefore the generic radical of this series of acids. This genus may (like the acetones) be subdivided still further, the aldehydes giving rise to acids containing $(\Phi H\Xi)'$ (or $\Theta\Xi$), the acetoketones to those containing $(\Phi CH_3\Xi)'$, &c. The best known of these subdivisions is that containing $(\Theta\Xi)'$. As indicated by the generic radical, these bodies have the properties both of acids and alcohols, giving rise to salts by the replacement of the hydrogen in the Ξ by metals, and to ethers by the replacement of the (HO) in the Θ by salt radicals (acids minus H). Some of them, at least, seem capable of forming aldehydes; for, as DEBUS has pointed out, glyoxylic acid is the aldehyde of glycollic acid. (It is worthy of note, that glycollic acid is the only member of the series which is a "true" alcohol, containing the radical $(H\Theta)'$, and giving rise, by oxidation, to an aldehyde and an acid—glyoxylic and oxalic acids). The typical formulæ of FRANKLAND and DUPPA indicate in a different way the same constitution as that expressed by the radical formulæ above; $C_2\left\{\begin{matrix}O''\\ HO\\ \overline{HO}\\ R_2\end{matrix}\right.$ being obviously identical with

 and if we write oxalic acid $(\Xi)_2$, thus  it is plain that the acids of the lactic acid series may be derived from it by replacing O″ by R_2.

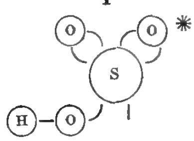
Kolbe long ago suggested, and Maxwell Simpson has since proved, that the dibasic and tribasic carbon acids are related to two and three molecules respective of carbonic acid, in the same way as the monobasic acids are to one molecule of the same substance. They therefore contain the radical Ξ two and three times respectively. Similarly, we might expect to find bodies containing the derived radicals (COH′), (CH_2HO), &c., two or three times, forming thus diatomic and triatomic aldehydes, alcohols, &c. We only know one diatomic aldehyde, glyoxal $(COH)_2$, and with certainty, only one diatomic *true* alcohol, glycol $(CH_2HO)_2$. From the way in which the other glycols are formed, it will be seen, that if the view of the structure of the olefines suggested above be correct, they contain the radical  (formed by replacing Br by HO in  which again is formed by the direct addition of Br_2, to ), and it is only when this radical is united to H that the glycol can be a *true* alcohol *on both sides.* Taking this view, propylenic glycol is a compound of the radicals (H⊖)′, and (CH_3⊖)′; amylenic glycol of (H⊖)′ and (C_3H_7⊖)′, and so of the others, one of the atoms of water residue being in the position of the HO in a true alcohol, the other in that of an olefine-hydrate. Of course, it is quite conceivable, and indeed very likely, that there are bodies which contain the radical (CH_2HO) twice or oftner, and are thus polyatomic *true* alcohols; but, with the exception of ethylenic glycol, they are as yet unknown. In treating of diatomic alcohols, we cannot pass over the curious body obtained by Wurtz, by the addition of water to allyl. A consideration of the chemical relations of acrylic acid, acrolein and allylic alcohol to propionic acid, propionic aldehyde and propylic alcohol indicates for allylic alcohol the formula  and for allyl 

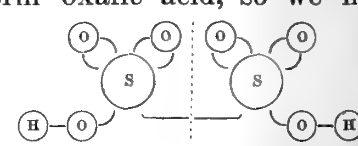
It is therefore a diatomic olefine, containing the radical $(C_2H_3)'$ twice, and if its reactions correspond to those of the monatomic olefines the formulæ of WURTZ's, dihydriodate and dihydrate will be,—

 and 

the latter will thus contain the radical $(CH_3\Theta)'$ twice, and be a diatomic olefine-hydrate.

The organic acids derived from sulphuric acid, and which are formed by the addition of SO_3 to organic substances containing hydrogen, stand in the same relation to sulphuric acid as the carbon acids (carbonsäuren) do to carbonic acid; in these we have an organic radical replacing one HO in $SO_2(HO)_2$. They therefore contain the radical $SO_2(HO)$; which, assuming the hexatomic character of the sulphur atom in sulphuric acid, may be represented by the graphic formula,

 * As we have numerous generic radicals derived from $\{(CO)\ HO\}'$ such as COH, $CH_2(HO)$, we might expect to find similar derived radicals from $SO_2(HO)$, and there can be little doubt that substances containing such radicals exist; but as yet very few of them are known. Thus we have only one or two substances corresponding to the acetones, such as sulpho-benzid, $(C_6H_5)_2SO_2$; a few chlorides containing SO_2Cl, but no bodies corresponding to the aldehydes or alcohols.† The remarkable substance discovered by V. OEFELE, and named by him "Triæthyl sulphin oxydhydrat," and which has the formula $S^{iv}(C_2H_5)_3(HO)$, is a proof of the possibility of such bodies. This substance has the same relation to sulphurous acid that BUTLEROW's trimethyl alcohol has to carbonic acid.

As the radical COHO unites with itself to form oxalic acid, so we have SO_2HO, forming hyposulphuric acid $(SO_2HO)_2$ .

The examples of generic radicals might be considerably increased in number, but as the purpose of this paper is not so much to tabulate known substances, as to show how this may be done, those given above may suffice. It will be seen that by this method, bodies having strongly marked chemical reactions in common are placed together. The relations between different genera are

* As this radical occurs very frequently, it may be advantageous to have a single symbol for it, and I have been in the habit of using the Greek letter Σ for this purpose. Thus we have $CH_3\Xi$, acetic acid; $CH_3\Sigma$ methylsulphuric acid; $CH_2\Xi\Sigma$ sulphacetic acid, &c.

† See KOLBÉ, Lehrbuch der Organischen Chemie, Bd. ii. s. 742.

prominently brought forward, and the vacancies, not yet filled up by experiment, in the list of conceivable compounds, distinctly pointed out.

The division of molecules into two parts, one more readily undergoing chemical change than the other, presents certain analogies to the "Theory of Copulæ," proposed by BERZELIUS. This is most marked in the case of the acids containing $(COHO)'$ and $(SO_2HO)'$. On the theory of copulæ, these acids contain oxalic acid C_2O_3, H_2O, or hyposulphuric acid S_2O_5, H_2O, and a copula. Thus acetic acid was considered as C_2H_6, C_2O_3, H_2O, sulpho-benzolic acid as $C_{12}H_{10}$, S_2O_5, H_2O, &c.; according to the view which is taken in this paper, acetic acid contains the *half* of C_2H_6 (methyl), and the *half* of oxalic acid; sulpho-benzolic acid, the *half* of $C_{12}H_{10}$ (phenyl), and the *half* of hyposulphuric acid. In the same way, the older chemists regarded hydrochloric acid as H_2Cl_2, a compound of the *molecule* H_2, with the *molecule* Cl_2, while we now consider it as containing the *halves* of these molecules. It is therefore as correct to say, that acetic acid is a compound of methyl and oxalic acid, as that hydrochloric acid is a compound of hydrogen and chlorine. To prevent confusion, it is however better, whenever it can be done, to have separate names for the *radical* and the *substance*. Thus we may call $(COHO)'$ *Carboxyl* (as proposed by BAYER), and distinguish it from $(COHO)_2$ oxalic acid, $(SO_2HO)'$ might be called *Sulphoxyl*, and thus be distinguished from $(SO_2HO)_2$ hyposulphuric acid. Even in the case of those radicals whose names are the same as the isomeric substances, we may, in some instances, make the distinction; thus $(CH_3)'$ is methyl, $(CH_3)_2$ methyl gas, $(CN)'$ cyanogen, $(CN)_2$ cyanogen gas, Cl' chlorine, Cl_2 chlorine gas.

In conclusion, it may be interesting to enumerate some of those substances which consist *entirely* of generic radicals—all of whose reactions are therefore generic reactions—thus we have cyanogen gas $(CN)_2$, consisting of two atoms of the generic radical of the nitriles united together; glyoxal $(COH)_2$; glycol $(CH_2HO)_2$, or $(\Theta H)_2$; oxalic acid $(COHO)_2$, or Ξ_2; hyposulphuric acid $(SO_2HO)_2$, or Σ_2,; tartaric acid (probably) $(CH(HO))_2\,(COHO)_2$, or $(\Theta\Xi)_2$; glycolic acid (CH_2HO) $(COHO)$, or $(\Theta H)\Xi$; glyoxylic acid $(COH)\,(COHO)$, or $(COH)\Xi$; tartronic acid $CH(HO)\,(COHO)_2$, or $\Theta\Xi_2$; glycerine (probably) $(CH_2HO)_2(CH(HO))$, or $\Theta\,(\Theta H)_2$; glyceric acid $(CH_2HO)\,(CH(HO))\,(COHO)$, or $(\Theta H)\,(\Xi\Theta)$; mesoxalic acid (CO) $(COHO)_2$, or $CO\Xi_2$.

XXVI.—*Some Observations on Incubation.* By John Davy, M.D., F.R.S., Lond. and Edin.

(Read April 16, 1866.)

The observations which I have now the honour to submit to the Society were made chiefly with the intent to endeavour to ascertain whether, in the instance of the egg of the common fowl, that which may be presumed to be vital action can for a while be arrested, and yet be capable of renewal. Whilst this was the main object kept in view in conducting the trials, a secondary one was to observe, however cursorily, the changes which take place in the contents of the egg when vital development has been prevented.

Of the many experiments I have made, during a period of more than two years that my attention has been directed to the inquiry, I shall select those, the results of which were best defined, or were least ambiguous. Considering the obscurity of the subject, it seems best to give the particulars of each of the selected trials, though, in so doing, I have to fear that the details may prove tedious.

In all the trials, newly or recently laid eggs were put under the hen for incubation, with those which were the special subject of experiment.

I. *Of Unimpregnated Eggs.*

The trial with these was made as a preparatory measure in relation to those which were to follow.

Four eggs were selected, obtained from a hen that had been kept apart after her last sitting. Of these, three in their fresh state were put under a hen with eleven ordinary eggs; the fourth was left exposed to the air in a room, the temperature of which varied from about 60° to 65° Fahr. on the 15th June, the day they were placed for incubation.

No. 1 weighed	727·5 grs.		No. 3 weighed	801·6 grs.	
2 ...	851·5 „		4 ...	843·4 „	

On the 9th of July the eleven impregnated eggs were hatched, producing healthy chickens. The three unimpregnated were found to be little altered. Again weighed, the loss of each, per cent., was as follows:—

No. 1, ...	14·6 grs.	No. 3, ...	12·3 grs.
2, ...	13·7 „	4, ...	2·7 „

The three from under the hen sank in water. Each broken under water, yielded a little air; that from No. 1 was found to consist of 20 of oxygen, 80 of azote; that from No. 2 was of like composition. In neither could any carbonic acid be detected by milk of lime.

The appearance and qualities of the contents of both these eggs were similar. The white and yolk were distinctly apart, each confined in its proper membrane. There was no unpleasant smell from either. The white was quite transparent, with a slight yellowish tinge; the yolks were of their natural colour, with here and there spots of a lighter colour on their surface. The chalazæ were readily detached. Neither white nor yolk, when triturated with hydrate of lime, gave off the slighest ammoniacal odour, nor showed more than a faint fume when brought near a rod dipped in hydrochloric acid, a fume but little stronger than when water was substituted. Under the microscope, the appearance of the yolk differed but little from that of the yolk of an ordinary egg fresh and impregnated.

The egg No. 3 was not now examined; it was kept exposed to the air, with No. 4. These, on the 16th of October, were found, on weighing, to have sustained an additional loss—No. 3 of 9·6 per cent., No. 4 of 8·7; and on the 23d of November, when again weighed, of a further loss—No. 3 of 2·5 per cent., No 4 of 3·4.

Broken under water, No. 3 gave off a good deal of air of an offensive smell, that of sulphuretted hydrogen predominating. A portion collected was found to consist of 15 per cent., absorbable by milk of lime, chiefly carbonic acid, and of 85 not diminished by phosphorus, chiefly, if not entirely, azote. The contents of this egg were much changed: there was no distinct yolk or white, but a semifluid mixture of a whitish milky hue, partly curdled, with which were intermixed gelatiniform greenish masses, which, under the microscope, appeared to consist of cells and granules, the former suggestive of a kind of mucedo.

No. 4, broken under water, yielded a good deal of air, which had no odour, and which, on examination, was found to consist of about 20 per cent. oxygen and 80 azote, without any appreciable quantity of carbonic acid. The contents of the egg were hardly perceptibly altered; as of Nos. 1 and 2 the white and yolk were distinct, each in its proper membrane; the white showed an alkaline reaction; the yolk neither an acid nor alkaline; mixed with hydrate of lime, neither afforded a perceptible smell of ammonia.

A second trial with unimpregnated eggs, conducted in the same manner as the preceding, was made with some from a Bantam hen that had been kept secluded. Five of her eggs a few days old (the oldest ten days) were put under a hen on the 21st November, with two ordinary eggs, which in due time were hatched, producing healthy chickens. On examination, after twenty-one days, two of the Bantam eggs were found broken. The remaining three were undergoing putrefaction; their contents were very offensive, a fluid, of a greenish hue, containing scattered through it small masses of a dark green colour, almost black. Under the microscope, its lighter portion exhibited granules and oil globules, its darker matter that of a hyaloid transparent substance without cells. The fluid was coagulated by heat. The air collected from one of them consisted of 40 per cent. carbonic acid, 2 oxygen, 58 azote.

No contrast could be greater than the appearance and state of the unimpregnated eggs in these two trials; in the first, with the exception of No. 3, so little changed, in the last so much changed, so much so as to be suggestive of the death of the eggs, the formation of mucedo, and of its death and decomposition, the colouring matter remaining.

II. *Of Eggs kept at a Temperature of about* 32° *Fahr.*

On the 24th of June, four newly laid eggs were put into an ice-house, where they were left until the 19th of July. Cracks were found in two of them when taken out, but without any exudation of contents. These cracks might denote the freezing of the eggs. Put under a hen on the evening of the 17th, with nine fresh eggs, the latter were hatched on the 10th of August, as was also one of the former; the two cracked eggs were crushed. The fourth from the ice-house, when broken was found to contain a mixture of yolk and white in the form of emulsion, with some unmixed yolk in a thickened state. The contents had an unpleasant smell, as if from incipient putrefaction.

III. *Of Eggs subjected to the Air-Pump.*

1. On the 14th of April six newly laid eggs were thus treated until the 23d. The air-pump was in good order, and it was worked twice or thrice daily. On the 23d, these eggs were put under a hen with seven newly laid ones. The hatching began on the 13th of May; on the 14th all were hatched with the exception of one,—one of those subjected to the air-pump: this egg swam in water; had, when broken, an unpleasant smell, denoting incipient putrefaction; and the white and part of the yolk were mixed, forming a yellow opaque emulsion. There was an obscure appearance of an embryo in that portion of the yolk which was retained in its membrane. The newly laid eggs were hatched a few hours earlier than those acted on by the air-pump.

2. Three newly laid eggs, on the 28th of May, weighed as follows:—

No. 1 weighed	874·8 grs.	No. 3 weighed	953·0 grs.
2 ...	912·0 „		

They were subjected to the air-pump until the 2d of June, when, on weighing, they were found to have sustained the following loss:—

No. 1, ...	4·8 grs.	No. 3, ...	5·1 grs.
2, ...	4·0 „		

They were put into water, and again subjected to the air-pump, which was worked twice or thrice daily. Taken out on the 18th of June, they were found to have gained as follows :—

No. 1,	...	8.2 grs.	No. 3,	...	11·3 grs.
2,	...	8·3 „			

They were now put under a hen, with seven newly laid eggs. On the 9th of June all the seven were hatched, but neither of the three.

No. 1, when weighed, was found to have lost 30·8 grs. It sank in water. No traces could be detected in it of an embryo. It contained a bright yellow emulsion, a mixture of yolk and white, free from any unpleasant smell, of specific gravity 1036; it was neutral to test papers. The chalazæ and membranes were shrunk together.

No. 2 had sustained a loss of 81 grs. Air procured from it was found to consist of about 18 per cent. carbonic acid, 82 azote. Its contents were very offensive and putrid, liquid and greenish, with a dark green clotted sediment. This under the microscope exhibited corpuscles, varying in diameter from 1500 of an inch to 1000; they were nearly circular, and contained greenish nucleoli.

No. 3 had sustained a loss equal to 81·6 grs. It contained a yellowish emulsion of specific gravity 1035. Some of its white still remained in its membrane in a thickened state. The fluid part had a putrid smell, but less offensive than the preceding.

3. On the 23d of June four newly laid eggs weighed as follows:—

No. 1	weighed	966·1 grs.	No. 3	weighed	886·8 grs.
2	...	786·4 „	4	...	830·4 „

They were put into water the same day—water deprived of air by the air-pump—and were subjected to the action of the pump. No air came from the water, but a good deal from the eggs. The pump was worked daily. On the 25th of June a little air still continued to be given off from the eggs. On the 27th there was a cessation; nor until the 19th of July did any more appear; then two or three bubbles were seen to rise from one of the eggs. Now taken out and weighed, they were found to have gained as follows:—

No. 1,	...	7·3 grs.	No. 3,	...	7·6 grs.
2,	...	7·0 „	4,	...	8·2 „

No. 1 was broken for examination. Its white appeared rather more liquid than common. Its specific gravity was 1033. The cicatricula seemed somewhat enlarged. The yolk was not apparently altered. There was no unpleasant smell either from the white or the yolk. On the exterior of its shell, and of the shell of the other three, there were minute opaque white spots, with a central aperture, distinguishable by the naked eye; the spots were a little depressed. The appearance was suggestive of solution by a current of air (carbonic acid?) from the egg under the action of the air-pump. They were mostly in the big end, but they were not confined to that end.

The remaining three eggs were put under a hen with ten recently laid. The latter were hatched on the 11th of August. Of the eggs subjected to the air-pump, No. 4 only was hatched, and only about four or six hours later than the ten. The chick was healthy. The two aborted eggs were found to have lost as follows:—

No. 2,	... 53·9 grs.	No. 3,	... 32·16 grs.

Both sank in water. Of No. 2 the yolk and white were in part mixed; a portion of the white was free and thickened. The contents had no unpleasant smell; no embryo could be found. Of No. 3 the yolk and white were found distinct, each in its proper membrane. In neither of them was there any apparent change, except that the white seemed more liquid than usual. Neither had any offensive smell, merely that of a stale egg.

IV. *Of Eggs kept in Lime Water.*

1. On the 17th of July three newly laid eggs were put into lime water, in which there was a great excess of lime. They weighed as follows:—

No. 1 weighed 1024·5 grs.		No. 3 weighed 937·1 grs.
2 ... 900·5 „		

The vessel used, which was of glass, held little more than a pint; it was full nearly to the mouth, and the mouth was only just large enough to admit the eggs. It was closed by a cork, and placed in a dark cupboard, where the temperature was about 63°, and subject to little variation. Taken out on the 17th of September (the water was covered with a crust of carbonate of lime), they were found to have gained as follows:—

No. 1,	... 2·3 grs.	No. 3,	... 4·0 grs.
2,	... ·6 „		

On the same day they were put under a hen with seven fresh eggs. Of the latter all but one were hatched on the 11th of October. This one, on receiving a blow, broke explosively, scattering wide its yellow, offensive contents; the explosion was nearly as loud as that of a pistol, showing how much the air it contained was compressed. Each of the eggs from lime water was unproductive. They were found to have lost as follows:—

No. 1,	... 25·7 grs.	No. 3,	... 10·9 grs.
2,	... 11·1 „		

All three sank in water. No. 1, broken under water, gave off two or three bubbles of air; the quantity was too small for analysis. The yolk and white were distinct, but the former seemed unduly thin, as if from the admixture of some of the white. Both showed an alkaline reaction, but the white the strongest. The contents of No. 2 were similar. No. 3 was not examined.

2. On the 26th of March thirteen eggs, which, when newly laid, had been placed in lime water,—some in the last week of February, some a few days later, but with less precaution than in the preceding trial,—were put under a hen. Of these one only was hatched, and on the 17th of April. The chick was healthy; the rest all aborted. In five of them embryos were found more or less advanced; in the other four no traces of an embryo could be detected; their contents varied much in quality. Of one the yolk and white were distinct, each in its membrane, and so little altered, that the yolk retained its natural acid reaction, as well as the white its alkaline. The contents of the others were free from any marked putridity.

V. *Of Eggs in the Ordinary Process of Incubation.*

For the sake of comparison, I shall now notice briefly the results obtained in ordinary instances of incubation with eggs presumed to be impregnated, and which had in no way been interfered with.

1. On the 27th of June thirteen eggs were put under a hen. Of these six were newly laid; of the other seven, reckoning from the time of laying,

No. 1 had been kept	36 days.		No. 5 had been kept	31 days.	
2 ...	35 „		6 ...	29 „	
3 ...	34 „		7 ...	25 „	
4 ...	33 „				

Of the six newly laid, three were hatched, three aborted. All three just swam in water; one, opened under water, afforded a little air, which was found to consist of 20·6 oxygen, 79·4 azote. Each of them contained a well-advanced fœtus.

Of the seven, the numbers of which have been given according to the time of keeping or age, all but the first three were hatched, giving birth to healthy chickens. Of the unproductive three neither contained an embryo, or showed any signs of development.

No. 1 contained a pale, thick, yellowish matter; it had a smell like that of sour milk; had an acid reaction; was of the consistence of soft curd, and had much the same appearance.

Of No. 2 the contents were liquid, with little viscidity; of a richer yellow than the preceding; had an alkaline reaction, and was of the specific gravity 1032. Distinct from the yellow liquid there was a small portion of glairy white. The contents of No. 3 showed no well-marked difference.

The hatching in this instance was unusually prolonged; the first chick appeared in the night of the 16th, the last of the seven in the night of the 18th. After that the hen deserted her nest, and may have been the occasion of the death of the advanced fœtuses; neither of them showed any signs of putridity.

2. Of sixteen duck's eggs put under two hens in May, all but four were hatched. Of these four, two were found to contain each a fœtus well advanced. In the other two there were no traces of development. Of one of these the white and yolk were distinct, and little altered; of the other the white and yolk were commingled, very liquid, and had a slight disagreeable smell. It is worthy of remark, that there was a thick layer of mucor (*M. mucedo*) on the air vesicle of each of the eggs containing a fœtus; it was nearly black; in a less degree it was found on the lining membrane of the shell of all three. Air from one of these eggs, equal in volume to ·67 cubic inch, consisted of about 20 oxygen and 80 of azote.

3. Ten eggs were put under a hen on the 24th of February. Three only were hatched. The seven unproductive eggs swam in water. In the first examined a fœtus was found well advanced; the fluid brownish and offensive. In the second the yolk and white were mixed, of a yellow colour, curdly, and offensive. In the third the contents were similar, but only slightly offensive. In the fourth they were more liquid, not curdly, very slightly offensive. In the fifth the yolk and white were only partially mixed, of a bright yellow, and not offensive. In the sixth there was a fœtus about one quarter the size of that of the first; the yolk and white mixed, of a dirty yellow, and offensive. In the seventh there was also a fœtus; it was less advanced than that of the first, but more than that of the last; the yolk was brownish-yellow, the white gelatinous and transparent; both offensive.

VI. *Conclusions.*

What are the conclusions to be drawn from the foregoing results? The changes experienced in the egg, as described in the several experiments, are so many and various, and the difficulty of referring them to their causes is so great, that I have much hesitation in drawing any decided inferences from them, especially as regards suspension of vital action, in the trials, whether with the air-pump, lime-water, or ice-house, in which incubation was afterwards successful.

In the various experiments, it may be said that the whole of the oxygen was not withdrawn from the eggs, that a minute portion remained sufficient to maintain a very low degree of vitality, enough, at least, to place in equilibrium for a time the antagonistic agencies—those administering to life and death. I shall relate one experiment which seems favourable to this view. On the 13th of May three newly laid eggs were put into water sufficient to cover them, and, with a piece of phosphorus placed by the side of the containing vessel, were subjected to the air-pump until the 28th. After the greater portion of the air had been extracted from the water and the eggs, the phosphorus ceased to shine until the instant that the pump was worked (it was worked twice or thrice daily, and was in good order); then there proceeded from it flashes of light, lighting up the interior of the bell-glass, suggestive of its vapour being diffused through the aqueous vapour filling the receiver, and of the disengagement of a little air from the eggs, the cause of the combustion or luminous appearance. Not until the last night was there a cessation of the phenomenon. On the following day, the 28th, the eggs, with a certain number of fresh eggs, were put under a hen; owing to an accident, the hatching process was interrupted. After an incubation extended to the 20th of June, one of the three eggs was found to contain a fœtus; the other two, in an unknown manner, had been taken from the nest. That in this instance a very minute portion of oxygen might have remained in the egg —a portion not exhausted by the air-pump—seems not improbable. Thus much granted, there seems little difficulty in admitting the persistence of a feeble action in the egg in question, and this a vital action, similar to that which, it may be inferred, is in progress in the ordinary egg when in a fit state for hatching—a condition limited as to time, and in the common fowl seldom exceeding thirty days.

If considerations of this kind render the results obtained from the vacuum eggs inferentially questionable, they are not less applicable to the results of the trials of the eggs kept in lime water and the ice-house. Under lime water access of air only is excluded. The little air in the egg may suffice for sustaining a very feeble action, sufficient for the preservation of life for a limited time. In an ice-house, at a temperature of 32°, or lower, if not low enough to freeze the egg, action may be diminished seemingly, but not be really arrested. The ova of the salmon, it has recently been ascertained, are capable of being hatched after having been kept in ice-water one hundred and twenty days, and thus conveyed to Australia. Whether there can be life without action, or its equivalent change, is a problem which I hardly venture to approach. In the seeds of certain vegetables, which, circumstances not favouring, remain without germinating months and years, there seems to be during the period an arrest of vital force or action; and yet, may it not be more apparent than real? When we reflect that each kind of seed, like each kind of egg, has its term of retension of vitality—the longest, in the instance of the seed, little exceeding thirty years—we may be allowed to have our doubts on the subject. It is possible that during the whole period, however long, there may be a very feeble action, though imperceptible, sustaining life. It may be well to reflect on the coarseness of our measures of time, and that great cosmic changes, which require hundreds and thousands of years to become conspicuous, are produced by causes in continued operation, which are absolutely inappreciable in their momentary effects. An instance of this is afforded by the worn-foot of the bronze statue in St Peter's, so worn by the kisses of devotees during hundreds of years. What we witness in certain hibernating animals seems to favour our doubts. In the instance of the dormouse, in the depth of winter, there are no distinct indications observable of life; its temperature is about that of the air; no arterial action is perceptible; yet it would appear that the heart's action, and the action of the secreting organs, is not absolutely suspended. Even congelation itself, it may be conjectured, may be compatible with the retention of a low vital force, more or less morbid or deranged; at least, congelation, I have found, does not entirely arrest action in the blood, ammonia being formed in it, and evolved from it when frozen.*

Relative to the varied changes witnessed in the aborted or unproductive eggs—some amounting to putrefactive decomposition, some indicative of the formation of new compounds, some so inconsiderable as to be only just appreciable—it is difficult to offer any satisfactory explanation, especially as, in every instance of incubation, all have been apparently very similarly acted on. Mr HUNTER has expressed the opinion that eggs which have "not hatched become putrid in nearly the same time with any other dead animal matter.† This statement is not supported by the preceding results. At one time I was disposed to infer, from various experiments I had made, some of which are to be found in the last volume of my "Physiological Researches," that the circumstance which most favours the putrefactive change in the egg is the commingling of the white and yolk. But from later experiments, especially one recently made, my confidence in this opinion has been shaken. The experiment was the following:—Eight newly laid eggs were wrapped in paper, placed in a basket, and covered with paper, in a room, the temperature of which ranged from about 50° at night to 55° by day. The placing them was begun on the 27th of November, and continued as follows, each egg being weighed at the time:—

No. 1,	November 27,	weighed	831·3 grs.	No. 5,	December 5,	weighed	890·5 grs.
2,	... 28,	...	991·0 ,,	6,	... 8,	...	862·4 ,,
3,	... 30,	...	842·5 ,,	7,	... 10,	...	869·0 ,,
4,	December 2,	...	893·4 ,,	8,	... 13,	...	1012·5 ,,

They were left undisturbed until the 30th of January, when they were taken out and again weighed.

No. 1	was found to have lost		2·4 per cent.	No. 5	was found to have lost		2·9 per cent.
2	...	...	3·2 ,,	6	...	...	3·0 ,,
3	...	...	3·5 ,,	7	...	...	3·0 ,,
4	...	...	3·3 ,,	8	...	...	2·2 ,,

These eggs were now put under a hen with five fresh ones. On the 28th of February the latter were all hatched; the former were found to have failed.

No. 1	had sustained a further loss of		15·8 per cent.	No. 5	had sustained a further loss of		14·7 per cent.
2	...	...	14·4 ,,	6	...	...	12·2 ,,
3	...	...	13·8 ,,	7	...	...	12·1 ,,
4	...	...	16·6 ,,	8	...	...	5·8 ,,

This trial was made on the idea that, by checking evaporation, and by perfect

* See Transactions Roy. Soc. of Edin. vol. xxiv. p. 26.

† Philosph. Trans. for 1778, p. 29.

rest, a retardation might be effected of the changes unfavourable to life, and that it might be indicated by some traces of vital development in the eggs. But the results were all of the contrary kind; in no one of the eggs were there any marks of development. Two of them, No. 4 and No. 8—the one which had sustained the greatest loss during incubation, the other which had sustained the least—were opened under water. The air from No. 4, a little more than half a cubic inch, was found to consist of 1 per cent. carbonic acid, 19 oxygen, 80 azote; whilst that of No. 8, somewhat less in quantity, consisted of 2·5 carbonic acid, 5 oxygen, and 92·5 azote. The contents of the two differed considerably. Those of No. 4 were a mixture of yolk and white, forming a yellow fluid, of little viscidity, of no unpleasant smell, of faint alkaline reaction, and giving off with quicklime a slight smell of ammonia. The contents of No. 8 had an offensive smell, approaching the putrid, a duller colour, a more distinct alkaline reaction, and mixed with lime, a stronger ammoniacal odour. The contents of the other eggs, with the exception of No. 7, were found to resemble very much No. 4. They had no unpleasant smell, and, if anything, they were of a brighter yellow, and of feebler alkaline reaction. No. 7 had undergone a greater change; its contents were of a greenish-mottled hue, nowise viscid, of unequal consistence, partially curdled, of a very offensive putrid smell, strong alkaline reaction, and with lime emitting a strong smell of ammonia. Under the microscope it was seen to consist of very fine granules and of globules or cells, like those of a mucedo, in which, it may be inferred, that the colouring matter existed. Now, as in all these eggs, excepting one, putridity had not taken place, though the yolk and white had become intimately mixed, and were exposed to a temperature favourable to the change, it seems pretty evident that a mere admixture of the two is not adequate to excite the putrid fermentation, and that something else is essential. But what that something is, I cannot at present venture to conjecture. It seems to me that the putrefaction of the egg, as regards its *vera causa*, is as yet nearly as much unsolved as that of the coagulation of the blood, and, like it, may perhaps be considered as belonging to the great mystery of life and death.

PLATE XXIV.

Mean Daily Curves of the six Summer and the six Winter Months of 1826 & 1827.

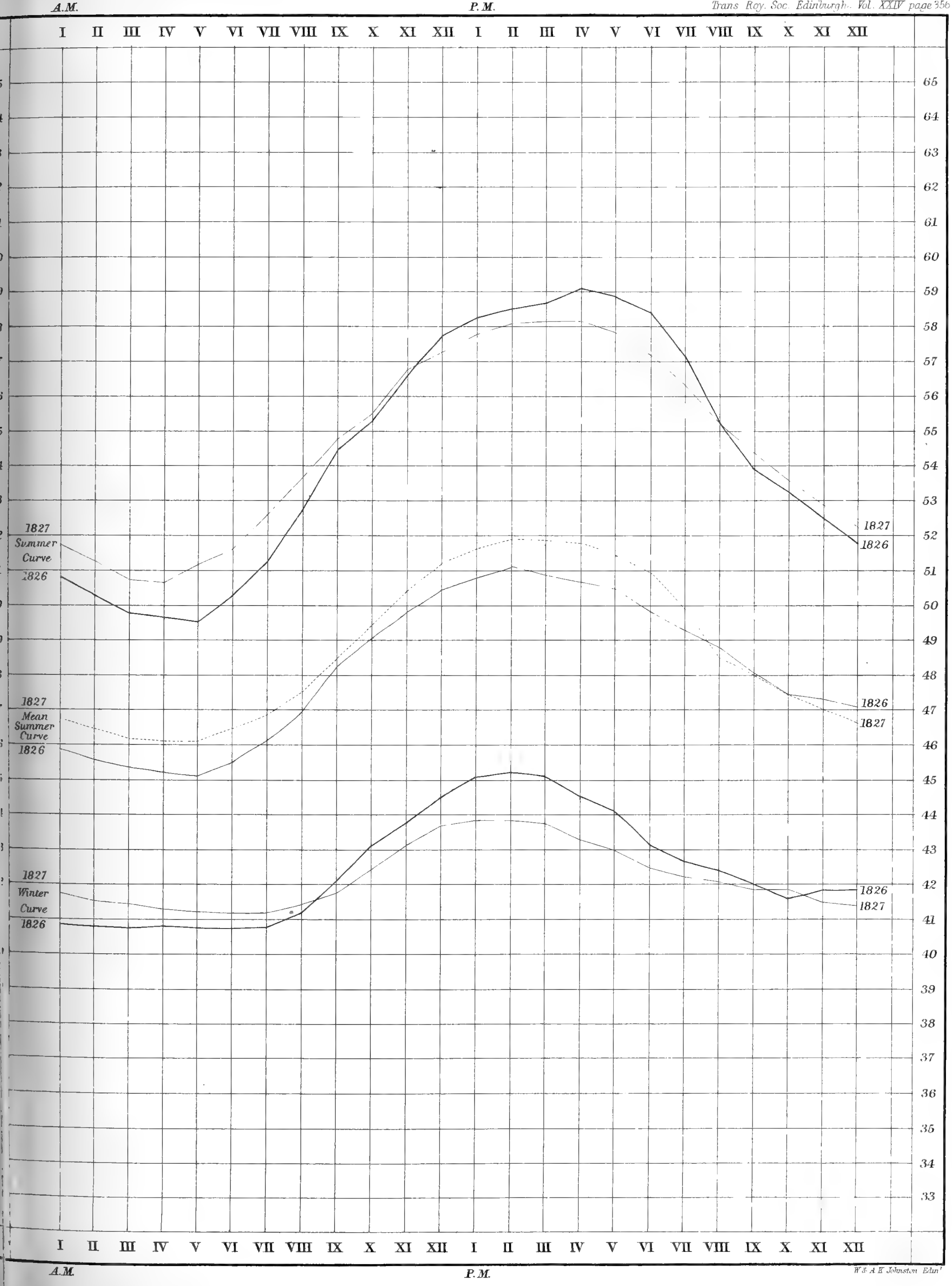


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PLATE XXV-1

Mean Daily Curves for each Month of 1826.

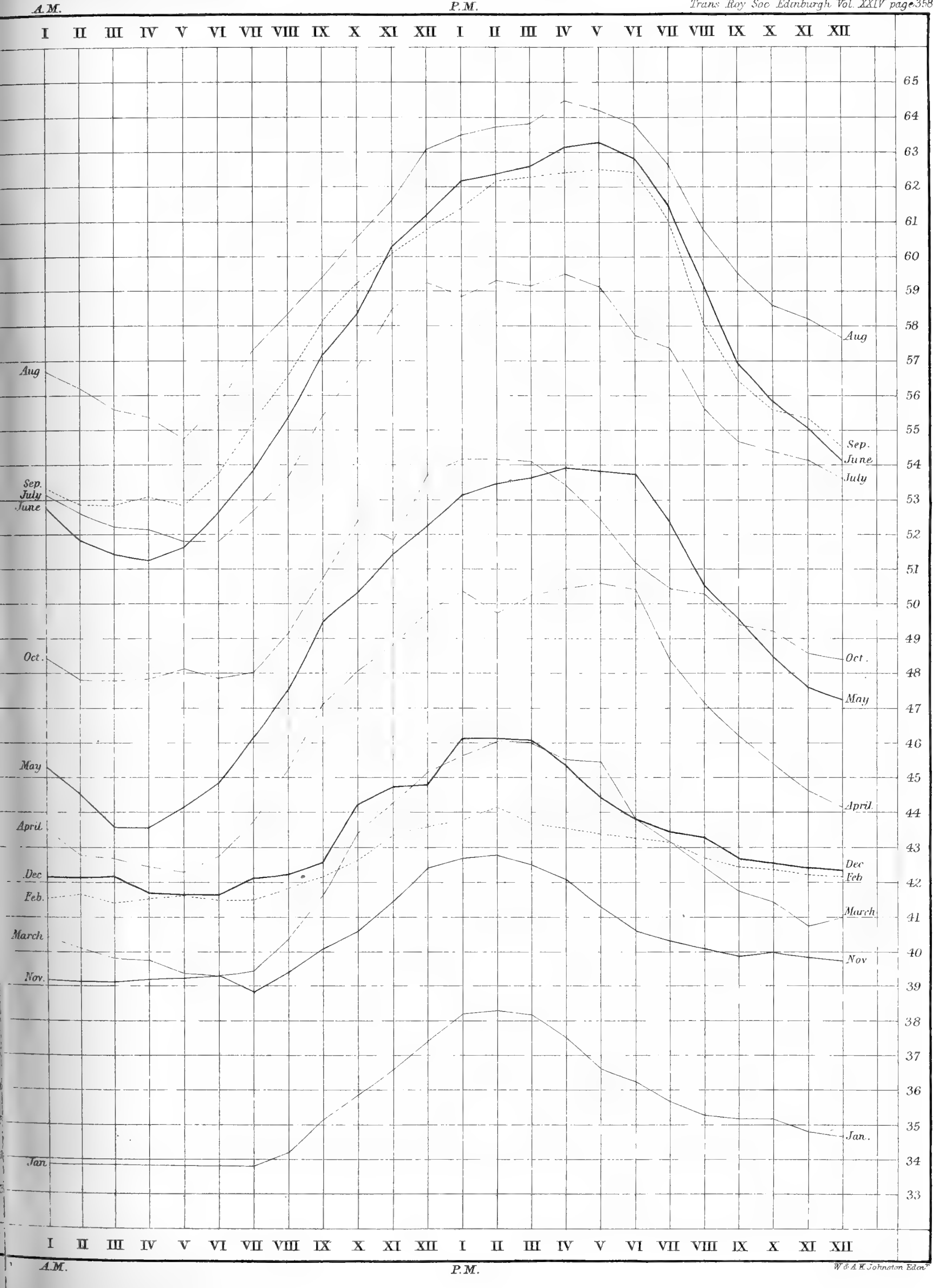

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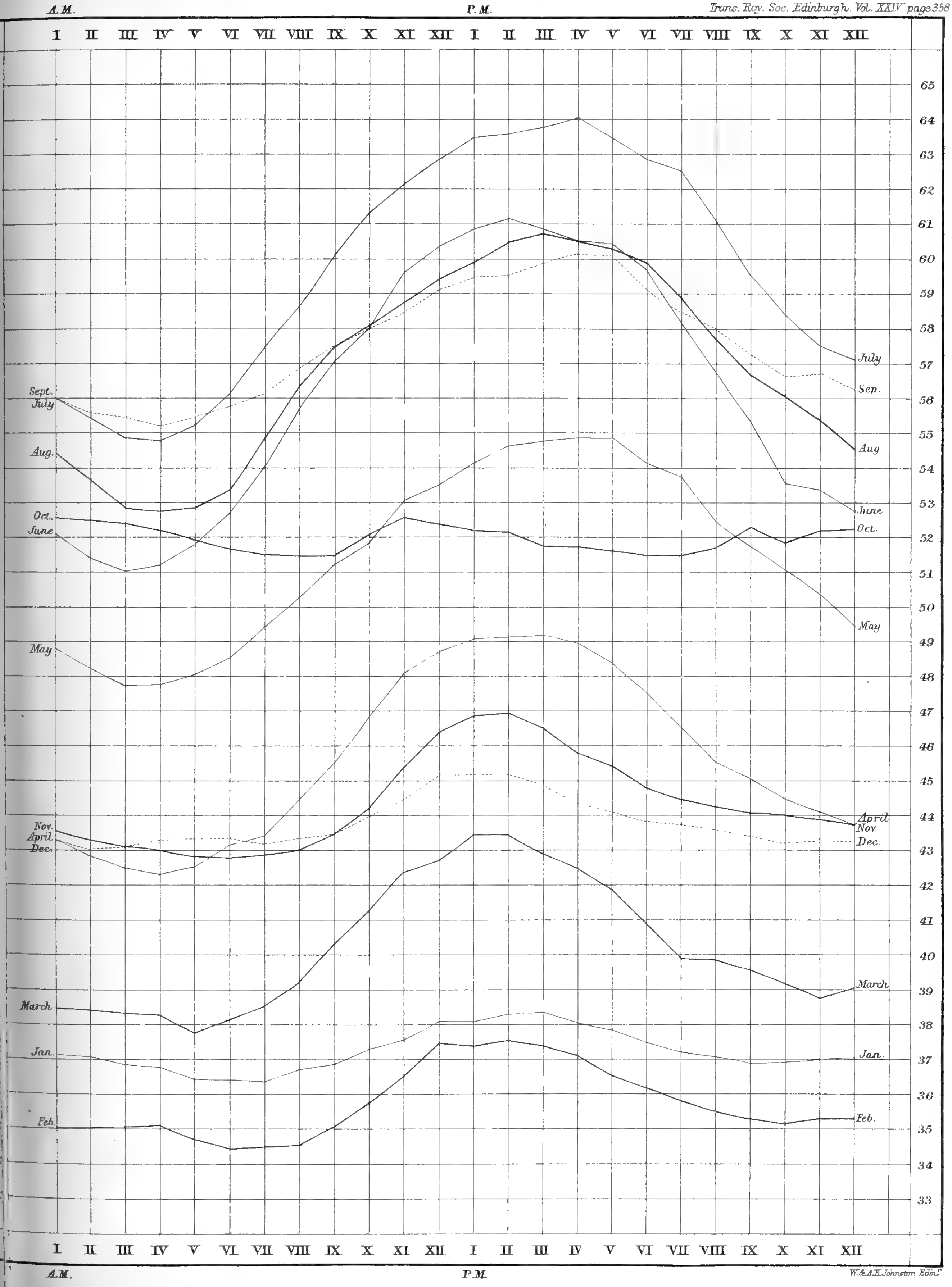
PLATE XXV-2

Mean Daily Curves for each Month of 1827.

Trans. Roy. Soc. Edinburgh. Vol. XXIV page 358



W. & A. K. Johnston Edin.r

XXVII.—*Report on the Hourly Meteorological Register kept at Leith Fort in the Years* 1826 *and* 1827. By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S. (Plates XXIV. and XXV.)

(Read 19th February 1866.)

Having already published in the Transactions* a detailed report on the Hourly Meteorological Register kept at Leith Fort, at the expense of the Society, during the years 1824 and 1825, it is unnecessary to enter into any recapitulation respecting the origin and history of this class of observations.

The singular and unexpected results obtained from these Registers, and the rapid approximation to general laws which some of these results exhibited, attached a great interest to the observations of future years; and it is satisfactory to find that the results for the two following years of 1826 and 1827 are almost perfectly coincident with those for 1824 and 1825, not only in their general relations, but even in their numerical laws.

The following are the Mean Temperatures of the four years during which the hourly observations were made at Leith Fort:—

Mean Temperature of 1824,	47°·588
" " of 1825,	48·911
" " of 1826,	48·436
" " of 1827,	48·407
Average Mean Temperature of four years,	48·335

The following Tables contain the mean temperatures for every day of the year, and for every hour of the day for 1826 and 1827:—

* Vol. X. p. 362.

MEAN RESULTS OF THE HOURLY REGISTER FOR 1826.

The Mean Temperature of the Winter Months, viz. Dec. Jan. Feb. is	40°·556
„ „ of the Spring Months, viz. March, April, May, . . .	46·135
„ „ of the Summer Months, viz. June, July, August, . .	58·263
„ „ of the Autumn Months, viz. Sept. Oct. Nov. . . .	48·818

The Mean Temperature of the Year 1826, from 8760 observations, is . . . . 48·436

TABLE I.—Containing the Daily and Monthly Mean Temperatures for 1826.

Day.	January.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1	32·94	40·40	45·49	46·48	44·65	54·09	62·04	56·77	56·46	56·44	43·54	39·29
2	37·77	43·82	41·40	46·55	49·03	49·88	61·66	52·96	58·57	52·69	43·21	40·38
3	39·42	47·92	42·28	51·29	42·57	51·33	61·96	56·50	58·35	51·78	41·67	36·38
4	37·34	43·26	42·38	49·65	43·09	52·84	60·47	56·58	57·90	47·55	43·62	35·00
5	36·54	43·21	39·15	50·21	45·75	52·69	60·94	56·20	53·25	45·06	46·41	31·51
6	36·21	47·54	41·59	50·40	43·00	58 88	62·50	56·00	53·58	47·73	39·08	32·75
7	36·16	42·77	43·22	49·33	45·55	53·97	60·58	59·97	56·82	55·53	39·08	41·99
8	30·69	46·84	44·08	53·87	44·38	54·92	62·26	58·42	54·04	48·02	39·09	44·09
9	24·36	47·34	54·57	50·27	44·26	49·00	56·31	54·86	52·68	45·96	38·37	44·31
10	30·98	43·55	59·20	46·01	48·40	50·80	57·47	54·25	54·20	48·24	44·22	46 92
11	31·26	39·08	50·57	48·17	47·49	54·33	57·77	55·86	55·15	51·27	50·29	50·01
12	29·45	43·75	43·92	47·50	48·16	60·35	55·46	56·15	56·50	51·16	41·80	48·91
13	25·49	45·60	41·26	46·31	51·72	62·35	56·47	60·40	59·02	50·41	39·11	46·76
14	20·94	43·31	39·65	49·72	53·86	57·45	52·38	62·17	54·36	49·15	37·71	44·25
15	22·03	46·26	41·18	46·97	54·38	54·88	53·85	61·96	50·82	56·05	38·72	45·15
16	24·01	46·79	39·06	46·10	49·90	51·51	55·36	60·51	56·55	54·85	35·42	44·42
17	40·79	42·04	38·88	46·99	54·78	55·14	54·42	61·58	60·25	52·37	39·15	44·05
18	43·75	37·00	43·48	47·65	57·67	56·99	56·60	69·75	53·07	48·26	37·65	43·16
19	34·94	38·75	42·36	50·67	52·54	55·34	54·34	68·23	55·01	48·55	41·09	38·86
20	40·15	39·17	44·24	53·73	49·32	57·47	51·44	62·44	54·09	52·76	39·00	39·99
21	43·30	41·55	43·22	49·80	52·27	54·63	50·42	59·72	51·99	54·71	39·70	37·46
22	39·21	48·06	39·65	49·27	53·71	52·27	50·14	57·94	52·16	54·21	42·97	45·29
23	40·32	39·65	39·44	44·45	48·90	56·52	50·16	63·74	51·21	56·11	45·38	47·66
24	37·76	43·15	39·22	43·16	49·75	65·11	53·94	63·53	52·87	56·05	38·94	47·60
25	40·95	43·67	36·92	41·17	51·31	68·79	57·60	63·28	53·90	50·91	35·87	44·96
26	38·15	41·78	36·71	42·11	46·32	69·34	58·01	60·04	60·08	45·09	34·52	40·12
27	38·37	41·35	40·60	35·91	50·28	64·11	61·80	60·26	59·05	48·12	31·48	39·17
28	40·09	47·78	44·91	35·30	51·47	67·01	64·20	59·87	55·00	47·86	38·99	42·65
29	41·16	...	37·22	37·85	50·48	64·31	62·09	64·15	58·57	47·55	42·72	45·12
30	43·72	...	38·26	41·44	50·79	64·66	62·46	61 94	61·31	45·87	43·88	47·67
31	44·42	...	41·46	...	51·13	...	61·69	57·67	...	44·26	...	47·58
Mean Temp. of each Month.	35·570	43·407	42·438	46·611	49·269	57·365	57·633	59·792	55·561	50·470	40·423	42·692

The Mean Temperature of 1826 is, by this Table, 48°·436.

TABLE II.—Showing the Mean Temperature of each Hour for each Month in 1826, and for the whole Year.

Hour.	January.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean Temp. of each Hour for the whole Year.
1 A.M.	33·822	41·723	40·492	43·650	45·234	52·833	53·274	56·790	53·125	48·363	39·242	42·193	45·916
2 ...	33·927	41·812	40·089	42·975	44·645	51·975	52·927	56·274	52·767	47·976	39·100	42·089	45·565
3 ...	33·926	41·562	39·960	42·700	43·822	51·367	52·871	55·645	52·267	47·863	39·125	42·145	45·289
4 ...	33·880	41·822	39·855	42·433	43·710	51·225	53 008	55·427	52·242	47·960	39·142	41·976	45·240
5 ...	33·890	41·750	39·331	42·358	44·153	51·700	52·742	54·863	51 808	48·057	39·283	41·742	45·154
6 ...	33·890	41·732	39·355	42·925	44·968	52·725	53·863	55·847	51·833	47·960	39·383	41·605	45·525
7 ...	33·863	42 009	39·516	43·958	46·145	53 917	55·250	57·307	52·750	48·000	38·992	41·653	46·133
8 ...	34·169	42·281	40·468	45·358	47·661	55·358	56·685	58·307	53·750	49·129	39·550	41·935	47·075
9 ...	35·024	43·527	41·806	47·075	49·572	57·075	58·161	59·363	55·325	50·807	40·008	42·145	48·347
0 ...	35·952	44·277	43·331	48·125	50·322	58·400	59·282	60·597	56·917	52·379	40·685	42·693	49·438
1 ...	36·661	44·795	44·258	48·958	51·500	60·150	60·073	61·766	58·400	51·984	41·640	43·395	50·407
2 ..	37·557	44·973	45·290	49·985	52·274	61·150	60·806	63·057	59·167	53·863	42·433	43·670	51·214
1 P.M.	38·210	46·134	45·758	50·400	53·065	62·192	61·427	63·347	58·973	54·153	42·790	43·943	51·724
2 ...	38·274	46·295	46·169	49·917	53·581	62·425	62·185	63·726	59·333	54·162	42·907	44·161	51·958
3 ...	38·200	46·170	46·081	50·208	53·613	62·650	62·282	63·920	59·300	54·065	42·617	43·853	51·941
4 ...	37·677	45·402	45·645	50·650	53·992	63·092	62·540	64·468	59·557	53·516	42·017	43·605	51·879
5 ...	36·758	44·661	45·476	50·692	53·903	63·367	62·653	64·137	59·092	52·411	41·433	43·415	51·533
6 ...	36·202	43·920	43·935	50·475	53·879	62·858	62·428	63·912	57·807	51·121	40·717	43·307	50·915
7 ...	35·798	43·553	43·072	48·433	52·306	61·417	61·000	62·645	56·358	50·540	40·342	43·057	49·910
8 ...	35·476	43·286	42·516	47·108	50·693	59·025	58·057	60·813	55·683	50·307	40·067	42·847	48·851
9 ...	35·363	42·857	41·984	46·283	49·548	56·975	56·589	59·476	54·792	49·508	39·873	42·532	48·008
0 ...	35·226	42·678	41·540	45·675	48·540	55·908	55·726	58·670	54·323	49·097	40·000	42·379	47·504
1 ...	34·903	42·420	40·911	44·800	47·701	55·183	55·387	58·210	54·067	48·677	39·773	42·137	47·036
2 ...	34·702	42·312	41·000	44·017	47·298	54·033	54·492	57·670	53·633	48·484	39·692	42·121	46·670

The Mean Temperature obtained from the last column in the above Table is 48°·468.

It occurred at $9^h\ 7^m$ A.M. and $8^h\ 27^m$ P.M.

HOURLY REGISTER FOR 1827.

The Mean Temperature of the Winter Months, viz. Dec. Jan. Feb. is . . . . 38°·945
" " of the Spring Months, viz. March, April, May, ... 45·817
" " of the Summer Months, viz. June, July, August, ... 57·612
" " of the Autumn Months, viz. Sept. Oct. Nov. . . . . . 51·255

The Mean Temperature of the Year 1827, is . . . . 48·407

TABLE III.—Containing the Daily and Monthly Mean Temperatures for 1827.

Day.	January.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1	43·68	37·64	41·70	48·79	47·61	54·50	56·92	61·67	56·09	56·48	41·71	45·29
2	28·22	35·60	34·63	50·99	51·25	52·36	55·58	62·69	59·35	54·43	48·31	43·25
3	20·44	33·29	33·34	50·36	53·23	49 80	57·59	61·83	57·73	55·75	49·38	46·67
4	28·26	36 27	32·86	48·53	54·84	52·34	59·47	56·80	55·79	54·71	48·30	53·14
5	27·85	41·22	33·77	51·13	52·11	50·71	59·99	59·05	56·69	52·57	48·62	50·49
6	44·88	40·05	38·50	48·34	46·39	51·91	61·62	55·41	55·51	56·58	43·60	40·73
7	48·97	37·90	32·76	46·85	44·83	53·15	59·41	58·82	56·72	54·06	43·00	45·38
8	48·95	35·98	30·34	47·87	47·14	57·37	62·71	59·31	56·50	52·51	44·27	42·48
9	38·66	35·45	32·96	49·05	47·95	61·23	59·37	61·67	58·69	54·08	50·18	43·95
10	38·75	38·72	36·46	46·12	43·42	61·23	57·75	57·43	62·89	48·11	49·26	49·56
11	32·31	37·93	41·84	44·39	45·95	61·34	56·58	55·92	63·56	50·65	43·89	46·69
12	29·38	36·26	41·99	47·58	50·93	60·87	57·17	55·14	57·01	50·52	49·25	40·91
13	40·48	35·13	40·67	47·31	54·18	57·56	57·61	56·95	55·75	48·35	55·23	40·75
14	40·97	36·13	39·58	48·53	49·19	57·63	57·09	53·76	58·62	48·37	53·33	44·96
15	35·25	32·41	37·10	47·81	48·72	56·12	60·30	52·54	62·62	56·58	43·13	45·12
16	38·92	30·36	40·27	45·74	48·58	61·45	65·01	51·35	63·91	58·01	46·29	41·85
17	35·46	31·24	42·78	44·53	51·66	57·98	65·37	53·86	62·68	55·53	44·10	45·43
18	37·21	28·35	38·72	42·08	48·53	56·66	59·78	53·38	55·61	51·98	36·44	43·19
19	37·44	28·69	47·42	42·93	54·46	54·33	56·59	55·12	49·98	53·98	38·44	46·59
20	35·32	31·58	47·15	43·01	53·37	50·88	55·46	54·42	56·53	54·04	44·42	41·98
21	35·96	35·09	47·65	42·38	59·12	52·59	56·10	57·86	56·66	53·24	33·48	43·08
22	35·37	33·30	50·71	40·69	56·89	53·23	58·33	55·87	57·46	50·05	31·58	41·34
23	33·13	35·92	51·91	35·54	54·75	53·77	59·58	58·55	57·50	50·15	33·08	40·41
24	34·01	36·21	46·14	34·65	56·76	54·95	64·58	57·57	56·22	53·26	32·35	46·47
25	36·11	35·85	38·69	37·53	50·46	57·63	61·54	55·58	57·56	54·02	38·83	45·97
26	35·77	46·05	40·17	41·39	51·03	58·16	56·64	54.87	56·50	55·12	42·71	50·88
27	30·96	40·41	45·12	45·75	51·21	58·60	59·73	59·81	55·42	52·51	47·98	45·66
28	45·73	37·36	40·61	47·49	54·66	58·53	62·72	56·96	54·95	43·96	49·98	40·72
29	48·19	...	38·12	50·25	56·35	58·02	63·53	56·21	55·86	43·46	48·45	32·67
30	45·26	...	40·55	51·25	57·02	57·98	62·67	59·61	55·61	46·69	47·33	30·62
31	43·52	...	42·15	...	57·26	...	63·97	58·15	...	42·65	...	42·69
Mean Temp. of each Month.	37·271	35·728	40·215	45·628	51·608	56·096	59·702	57·037	57·532	52·013	44·221	43·836

TABLE IV.—Showing the Average Mean Temperature of each Hour for each Month in 1827, and for the whole Year.

Hour.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	Mean Temp. of each Hour for the whole Year.
1 A.M	37·121	34·982	38·508	43·275	48·718	52·091	56·056	54·379	56·050	52·597	43·550	43·282	46·717
2 ...	37·097	34·955	38·395	42·925	48·161	51·491	55·492	53·685	55·608	52·589	43·368	43·129	46·408
3 ...	36·935	35·080	38·290	42·500	47·750	51·016	54·863	52·935	55·433	52·403	43·116	43·185	46·125
4 ...	36·734	35·160	38·169	42·356	47·790	51·258	54·677	52·693	55·241	52·153	43·000	43·290	46·043
5 ...	36·508	34·768	37·855	42·508	48·072	51·850	55·201	52·976	55·550	51·903	42·808	43·282	46·107
6 ...	36·532	34·553	38·185	43·083	48·645	52·866	56·145	53·306	55·800	51·763	42·733	43·322	46·411
7 ...	36·419	34·643	38·516	43·291	49·492	54·066	57·605	54·911	56·091	51·677	42·841	43 242	46·899
8 ...	36·701	34·652	39·185	44·475	50·234	55·716	58·725	56·330	56·850	51·556	43·008	43·411	47·570
9 ...	36·911	35·134	40·379	45·608	51·218	57·075	60·105	57·355	57·516	51·580	43·491	43·548	48·327
10 ...	37·250	35·768	41·201	46·800	51·895	58·121	61·306	58·182	58·075	52·057	44·183	44·008	49·070
11 ...	37·605	36·518	42·387	48·041	53·145	58·783	62·153	59·435	58·650	52·476	45·375	44·556	49·927
12 ...	38·072	37·375	42·750	48·658	53·492	59·400	62·951	60·290	59·233	52·355	46·400	45·153	50·511
1 P.M.	38·129	37·259	43·395	49·016	54·185	59·975	63·589	60·871	59·641	52·193	46·908	45·201	50·863
2 ...	38·362	37·625	43·371	49·066	54·734	60·541	63·516	61·161	59·607	52·258	46·991	45·209	51·037
3 ...	38·282	37·419	42·927	49·183	54·879	60·725	63·806	60·814	59·966	51·968	46·600	44·943	50 959
4 ...	38·064	37·089	42·645	48·950	54·943	60·633	64·056	60·669	60·233	51·888	45·800	44·339	50·776
5 ...	37·911	36·616	41·935	48·300	54·943	60·400	63·363	60·323	60·283	51·637	45·458	44·242	50·451
6 ...	37·564	36·202	40·935	47·566	54·137	59·750	62·951	59·863	59·075	51·532	44·866	43·830	49·856
7 ...	37·218	35·785	39·958	46·541	53·709	58·191	62·387	58·967	58·558	51·516	44·616	43·766	49·268
8 ...	37·153	35·563	39·896	45·658	52·355	56·841	61·185	57·669	58·000	51·847	44·275	43·637	48·673
9 ...	36·968	35·330	39·605	45·041	51·750	55·341	59·571	56·750	57·300	52·161	44·075	43·540	48·119
10 ...	36·927	35·259	39·193	44·391	51·097	53·625	58·274	56·048	56·841	51·944	44·008	43·242	47·571
11 ...	37·008	35·321	38·887	44·050	50·314	53·541	57·532	55·427	56·891	52·201	43·900	43·484	47·379
12 ...	36·959	35·357	39·024	43·691	49·629	52·733	57·209	54·597	56·300	52·330	43·850	43·589	47·105

The Mean Temperature obtained from the last column in the above Table is $48°·423$.

The Mean Temperature of $48°·423$ occurs at $9^h\ 12^m$ A.M. and $8^h\ 23^m$ P.M.

The general results which may be deduced from the preceding Tables relate—

1. To the form and character of the annual and monthly daily curves, or the daily progression of temperature.

2. To the arrangement of the monthly curves in separate groups.

3. To the determination of the times of the day when the mean temperature occurs.

4. To the relation between the mean temperature of the day, and that of any single hour or pair of similar or *homonymous* hours.

5. To the parabolic form of the four branches of the annual daily curve.

I. *On the Form and Character of the Annual and Monthly Daily Curves, or the Daily Progression of Temperature.*

The mean temperature of the year 1826 was 48°·436, and that of 1827 48°·407, both of them intermediate between that of the two preceding years; but though in its average character the temperature of 1826 was moderate, yet it differed from both of them in a remarkable manner. Though the mean annual curves of 1824 and 1825 differ from one another, from the former representing a *cold* and the latter a *warm* year, yet they are perfectly parallel, indicating the same vicissitudes of climate. The curve of 1826, however, exhibits the character of an American climate, descending almost as low as that of 1824 in the morning branch, and rising nearly as high as 1825 in the warm period of the day.

The curve for 1827 differs remarkably from that of 1826, keeping above it from 1 o'clock in the morning till 8 o'clock in the evening, but almost touching it at the morning and evening hours of mean temperature. See Plates XXIV. and XXV.

In all the curves for these four years, the lowest temperature took place at 5 o'clock in the morning. The temperature increased, with great regularity, till 3 o'clock in the afternoon, when it descended to its minimum. The period, therefore, of its ascending is *ten* hours, and that of its descending motion *fourteen* hours.

By comparing the summer and winter curves or the mean temperatures of the six summer months, from April to September inclusive, with those of the six winter months, from October to March inclusive, as exhibited in the annexed Table, we are enabled to discover whether or not the peculiar character of 1826 is derived from the warm or the cold season.

TABLE, SHOWING THE MEAN TEMPERATURE OF EACH HOUR FOR THE SIX SUMMER MONTHS, FROM APRIL TO SEPTEMBER INCLUSIVE, AND FOR THE SIX WINTER MONTHS, FROM OCTOBER TO MARCH INCLUSIVE, FOR 1826 AND 1827.

	1826.			1827.	
Hours.	Six Summer Months.	Six Winter Months.	Hours.	Six Summer Months.	Six Winter Months.
	°	°		°	°
1 A.M.	50·818	40·972	1 A.M.	51·761	41·667
2	50·260	40·832	2	51·227	41·589
3	50·112	40·763	3	50·749	41·501
4	49·667	40·772	4	50·669	41·418
5	49·604	40·675	5	51·026	41·187
6	50·366	40·654	6	51·641	41·181
7	51·554	40·672	7	52·576	41·223
8	52·853	41·255	8	53·721	41·419
9	54·428	42·219	9	54·813	41·900
10	55·604	43·219	10	55·396	42·411
11	56·808	45·123	11	56·701	43·153
12	57·740	44·590	12	57·337	43·684
1 P.M.	58·234	45·165	1 P.M.	57·846	43·848
2	58·528	45·271	2	58·104	43·969
3	58·662	45·104	3	58·229	43·689
4	56·731	44·647	4	58·247	43·304
5	59·333	44·026	5	57·935	42·966
6	58·726	43·200	6	57·224	42·488
7	57·026	42·727	7	56·292	42·143
8	55·229	42·416	8	55·285	42·062
9	53·943	42·019	9	54·292	41·946
10	53·141	41·820	10	53·379	41·766
11	52·561	41·470	11	52·959	41·800
12	51·857	41·385	12	52·359	41·851
Mean,	54·324	42·541	Mean,	54·574	42·257

The summer curve of 1826 retains the same intermediate position between those of 1824 and 1825 that it did in the annual curve; but in the morning hours it descends nearly to the curve of the cold year of 1824, while in the afternoon hours it rises towards the curve of the warm year 1825, thus displaying, in the summer season, the character of an American climate.

The summer curve of 1826 bears the same relation to that of 1827, keeping below it in the morning till about 11 o'clock, when it rises high above it till 8 o'clock, when it descends till midnight.

In the winter curves, that of 1826 keeps between those of 1824 and 1825 from 1 o'clock A.M. till 8 o'clock. It then rises above that of 1825, and keeps above it till 6 o'clock in the evening, when it again meets that of 1825, coinciding with it till about 3 o'clock in the morning. Hence it follows that the peculiar character of 1826 appears still more strikingly in the winter than it does in the summer season.

The winter curve of 1826 bears a different relation to that of 1827 in its morning branch, but a similar relation to it in its evening branch.

II. *On the Arrangement of the Monthly Curves into three separate Groups.*

By examining the daily curve for each month, it will be seen that it preserves the general form of the daily annual curve, occasionally deviating into salient and re-entering portions; but were we to delineate the individual daily curves, we should, in most cases, find the very form of a curve obliterated, and a capricious succession of elevations and depressions substituted in its place.

The most remarkable result, however, is the distribution of the monthly curves into three separate groups, namely, curves of high temperature, such as those of *June*, *July*, *August*, and *September;* curves of low temperature, such as those of *November*, *December*, *January*, *February*, and *March;* and curves of moderate temperature, such as those of *April*, *May*, and *October*.

This distinct separation of the monthly group is well seen in the Plates XV. and XVI. of Volume X., which represents them as in 1824 and 1825. In that for 1824 there is a very slight encroachment of the April curve upon that of January, but in that for 1825 the separation is complete.

In 1826 and 1827 (See Plates XXIV. and XXV.) these curves are grouped, though less distinctly, according to the same law; but, what is very remarkable, the curve for January 1826 is entirely thrown out of the cold group, and in consequence of the extraordinary cold which prevailed in that month, its curve is as far separated from those of the winter group, as any one of the groups are separated from each other.*

III. *On the Determination of the Two Hours of the Day when the Mean Temperature occurs.*

Previous to the establishment of the hourly Register at Leith Fort, nothing was known respecting the times of the day when the mean annual temperature occurs. It was generally supposed to be about 8 o'clock in the morning, and Professor PLAYFAIR adopted this as the most probable result. With regard to the time when the annual mean occurred in the evening, I am not aware that even a conjecture had been formed.

* The extraordinary character of the October curve in 1827 requires to be explained. When the daily schedules for that month were sent to me from Leith Fort, I was surprised to find *two* for the *same day* of October with very different numbers. Upon inquiring into the cause, I found that some of the non-commissioned officers who had voluntarily undertaken the duty of observing the thermometer, and for doing which they were liberally paid, had neglected to make the observations, and had filled up the daily schedules with false numbers. It is obvious from the curve that this fraud was committed by the person who made the afternoon and evening observations.

It is interesting to observe how little effect these erroneous observations have had upon the general results for 1827, when compared with those of other years.

	Hours of Morning and Evening Mean Temperature.	Critical Interval.
In 1824 it occurred at	$9^h\ 13^m$ A.M. / 8 26 P.M.	$11^h\ 13^m$
In 1825, ...	9 13 A.M. / 8 28 P.M.	11 15
In 1826, ...	9 7 A.M. / 8 27 P.M.	11 20
In 1827, ...	9 12 A.M. / 8 23 P.M.	11 11
	Mean,	$11^h\ 15^m$

The mean of which is

$9^h\ 11^m$ A.M. and $8^h\ 26^m$ P.M.

The interval between the morning and evening mean temperature has been called the *critical interval*, which at Leith Fort is $11^h\ 15^m$, and which, there is reason to believe, is a fixed quantity. The equality of these numbers in four different years is very remarkable, the deviation of each from the mean not exceeding 4^m.

Although the hours of mean temperature vary in different latitudes, and at different heights above the sea, yet the critical interval seems to be a fixed quantity everywhere, as appears from the following table:—

At Padua, . .	$11^h\ 14^m$	At Philadelphia, .	$11^h\ 20^m$
At Appenrade, .	11 14	At Belleville, .	11 14
At Inverness, .	11 13	At Trincomalee, .	11 5
At Tweedsmuir, .	11 15	At Kingussie, .	10 44

The mean of which is $11^h\ 10^m$, differing only 4^m from the Leith result.

The determination of the times of mean annual temperature gives us the two best hours for recording the indications of the thermometer, namely, $9^h\ 11^m$ A.M. and $8^h\ 26^m$ P.M.; for if any of the observations is accidentally omitted at one of the hours, the mean of the remainder will approach nearer to the mean temperature of the year than if any other pair of hours had been taken and similar omissions made.

Another advantage of this determination is, that the mean temperature of the year may be obtained with great accuracy from a single observation made every day at one of the hours of mean temperature.

If we examine the annual, or even the monthly, curves, it will be seen that the ascending, or morning branch, is more regular in its progression than the descending, or evening branch, and therefore a single observation made at the time of the morning mean is preferable to one made at the time of the evening mean.

This regularity in the morning curve has been observed in other phenomena, but especially in atmospherical polarisation, and the cause of it has been explained by DOVE and RUBENSON.*

* *Memoire sur la Polarisation de la Lumiere Atmospherique*, p. 86, note.

The hours of mean temperature have a considerable range in the monthly curves, varying in the morning from half-past 8 to half-past 10, and in the evening from 7 o'clock to 9.

IV. *On the relation between the Mean Temperature of the Day, and that of any single Hour, or pair of similar or homonymous Hours.*

It was long the practice of meteorologists to observe the thermometer at *two* convenient hours, so that if the one gave a temperature greater than the mean, the other might give a temperature as much less, and in this way several registers were kept with considerable accuracy. The hours of 10^h A.M. and 10^h P.M., suggested by the Rev. Dr GORDON, were frequently used, and gave a result nearer to the mean of the *maximum* and *minimum* than any other pair of convenient hours.

Upon computing the mean temperature of every pair of similar or homonymous hours, I found, as shown in the following Table, that they differed very little from the mean temperature of the 24 hours :—

Hours of Observation.		Diff. from Mean Temp. of Day in Thousandths of a Degree. Leith.	Inverness.
5^h A.M. and	5^h P.M.	−0·134	−0·434
6	6	−0·281	−0·543
7	7	−0·372	−0·552
8	8	−0·421	−0·396
9	9	−0·285	−0·113
10	10	−0·086	+0·174
11	11	+0·176	+0·374
12	12	+0·374	+0·555
1	1	+0·367	+0·550
2	2	+0·366	+0·389
3	3	+0·252	+0·173
4	4	+0·059	−0·175

Hence it appears that the defect or excess of the mean temperature of any pair of similar hours, when compared with that of the 24 hours, is always in the Leith observations less than *half a degree*. It appears, also, that the mean of 4^h and 4^h approaches nearest to the daily mean, and 10^h and 10^h next to it.

I have added to the above Table the results of the Inverness hourly observations. The deviations are very slightly greater, but the law is the same; and it is interesting to observe the interchange of the signs at 10^h and 10^h, and 4^h and 4^h, a proof of the singular equality between the mean temperature of the day, and half the sum of the mean temperature of these hours.

In speaking of this law, as given in the Report upon the Registers for 1824 and 1825, HUMBOLDT says,—

"We are surprised, at the first glance, by the generality of this law. The *homonymous* hours are very inequally distant from the hour of the *maximum* of the daily temperature. . . . . It is a thing truly remarkable, that from the mean of two ordinates, we may deduce the mean temperature of the whole year; that is, the mean of all the horary ordinates."

As meteorological registers have sometimes been kept only once a day, it is desirable to ascertain the relation of the mean temperature of each hour to that of the day. In the following Table, I have given the results for 1826 and 1827, and also for 4 years, from 1824 to 1827 inclusive :—

Hour.	1826.	1827.	Mean of Four Years. 1824–1827.
1 A.M.	−2°·552	−1°·706	−2°·131
2	−2·903	−2·015	−2·396
3	−3·179	−2·298	−2·658
4	−3·228	−2·308	−2·793
5	−3·314	−2·316	−2·844
6	−2·943	−2·011	−2·545
7	−2·335	−1·524	−1·956
8	−1·393	−0·853	−1·180
9	−0·121	−0·096	−0·760
10	+0·970	+0·647	+0·777
11	+1·939	+1·504	+1·702
12	+2·746	+2·088	+2·463
1 P.M.	+3·256	+2·440	+2·865
2	+3·490	+2·614	+3·125
3	+3·473	+2·536	+3·135
4	+3·411	+2·353	+2·927
5	+3·065	+2·028	+2·576
6	+2·447	+1·433	+1·984
7	+1·442	+0·845	+1·211
8	+0·383	+0·350	+0·362
9	−0·460	−0·304	−0·410
10	−0·964	−0·852	−0·949
11	−1·432	−1·044	−1·351
12	−1·798	−1·318	−1·713

From this Table it appears, that the mean annual temperature of any hour never differs more than $3\frac{1}{2}°$ from the mean temperature of the day for the whole year. The very same result was obtained from the Register of 1824 and 1825.*

V.—*On the Parabolic form of the Four Branches of the Annual Daily Curve.*

In the report upon the Register for 1824 and 1825, I have shown that the four branches of the annual daily curve approach so nearly to Parabolas, that the greatest difference between the observed and calculated temperatures is only *a quarter of a degree of* Fahrenheit. The following Table contains the calculated temperatures for 1826 and 1827, and the difference between them and the observed temperatures :—

* Edinburgh Transactions, vol. x. p. 387, 388.

	1826.	Difference.	1827.	Difference.
Mean	48·468	...	48·423	...
	48·055	0·047	48·056	−0·063
	47·375	−0·129	47·522	−0·049
	46·786	−0·250	47·070	−0·309
	46·287	−0·383	46·700	−0·405
	45·879	−0·037	46·413	−0·304
	45·562	−0·003	46·207	−0·201
	45·335	+0·046	46·084	−0·041
	45·199	−0·041	46·043	0·000
Min.	45·154	−0·000	46·131	−0·024
	45·345	−0·180	46·396	−0·015
	45·918	−0·215	46·836	−0·063
	46·874	−0·201	47·453	−0·117
	48·214	−0·133	48·247	−0 080
Mean	48·468	0·000	48·423	0·000
	49·616	+0·178	49·220	+0·150
	50·641	+0·234	50·015	+0·088
	51·373	+0·159	50·583	+0·072
	51·812	+0·088	50·923	+0·066
Max.	51·958	0·000	51·037	0·000
	51·874	−0·067	50·973	+0·014
	51·623	−0·256	50·780	+0·004
	51·203	−0·330	50·459	+0·008
	50·576	−0·339	50·010	+0·154
	49·861	−0·049	49·431	+0 163
	48·938	+0·087	48·725	+0·052
Mean	48·468	0·000	48·423	0·000

From this Table it appears, that the difference between the observed and the calculated temperatures for 1826 and 1827, is only four-tenths of a degree of Fahrenheit, a very little more than in 1824 and 1825.

I cannot conclude these observations, without directing the attention of the Society to the singular fact, that laws so regular as those we have been contemplating should have shown themselves after only four years of hourly observations. When we consider by how many disturbing causes the temperature at any particular instant is affected—by the winds which blow over surfaces differently heated,—by the showers which instantly cool the air,—by the interposition of clouds, now screening the sun, and now giving a free passage to his rays, and by many other causes, as capricious in their origin as they are irregular in their influence, it cannot but appear wonderful that all these effects should be so nicely balanced, as to produce a perfect compensation at every point of the annual daily curve. In virtue of this compensation, we may consider the mean annual daily curve as representing the mean daily progression of the solar heat, whether received directly from the sun, or returned into the atmosphere, by terrestrial radiation.

XXVIII. *On the Buried Forests and Peat Mosses of Scotland, and the Changes of Climate which they indicate.* By James Geikie, Esq., of the Geological Survey of Great Britain. Communicated by Archibald Geikie, Esq., F.R.S.

(Read 19th March 1866.)

The following communication is an attempt to eliminate the geological history of our Scottish Peat Mosses. So much, however, has already been done in this matter, that the reconsideration of phenomena, for the most part well known, may appear almost a superfluous task. But, notwithstanding the essays of Walker, Rennie, Anderson, and others, in this department of geological inquiry, there is still probably much to be gathered from the same source, which shall greatly increase our knowledge of the condition of these latitudes in the ages that followed upon the close of the glacial epoch. At present, I mean to give only an outline of the subject, reserving for some future occasion a fuller statement of the facts on which the conclusions arrived at are based.

Our peat mosses appear to contain the record of certain changes of climate, which have not hitherto fully engaged attention. The evidence furnished by the buried timber has indeed been frequently considered, but not so the proofs of altered conditions which the peat itself supplies. These last, more especially, form the subject of this memoir. But any paper treating of the origin and history of our peat mosses would be incomplete, without reference to the ancient forests which they cover, and the evidence on this head has therefore been recapitulated.

It is proper to state here, that many observations on the present aspect of the peat of our hills and valleys were made conjointly by my colleague Dr Young and myself, during our survey of a large portion of the Peeblesshire hills. The subject of this communication was partially sketched out by us some time ago, but the pressure of other matters latterly deprived me of my colleague's co-operation.

The phenomena revealed by our peat mosses are three-fold:—

1*st*, The buried trees, and the condition of this country at the period of their growth.

2*d*, The causes which led to the destruction of those trees.

3*d*, The present aspect of the peat mosses.

I. *Trees in Peat,—Condition of the Country at the Period of their Growth.*

It is well known, that below many peat mosses of this and other countries, the roots and trunks and branches of forest trees, and the remains of shrubs, are of common occurrence. Our Scottish mosses have yielded the oak, the pine, the birch, the hazel, the alder, the willow, the ash, the juniper, &c.; but a greater number of species are dug from the peat of more southern latitudes. No one now doubts, that the vast majority of those trees and shrubs have grown *in situ*. And as there are few parts of the country where buried trees have not been disinterred from peat or alluvium, the conclusion is forced upon us, that at some period in the past our island must have been exceedingly well wooded. Even the remote islands of the Hebrides appear to have had their groves of oak and pine. Throughout the bleak Orcades and sterile Zetland, large trees have at one time found a congenial habitat. Of the main-land it is difficult to say what district has not supported its great forests. The bare flats of Caithness, the storm-swept valleys of the Western Highlands, the desolate moory tracts of Perthshire and the north-eastern counties, the peaty uplands of Peeblesshire and the Borders, and the wilds of Carrick and Galloway, have each treasured up some relics of a bygone age of forests.

It is much to be regretted, that in noting the occurrence of the various trees which our peat mosses have yielded, so little attention should have been paid to the relative elevation of the species above the sea-level. Enough is known, however, to assure us, that the pine and its congeners enjoyed a greater range in former times than at the present day. Mr WATSON gives "600 yards and upwards" as the elevation now reached by the Scotch pine. But he "has seen also small scattered examples at 800 and even 850 yards of elevation." These last, however, he thinks, had probably been planted. "But that the pine," he continues "has grown naturally on the Grampians, at an equal elevation in former ages, is rendered certain by the roots still remaining in the peat mosses of the elevated table lands of Forfar and Aberdeen, at 800 yards and upwards."* Again, in Glenavon, Banffshire, there are peat mosses, nearly 1000 yards above the sea, which contain abundant roots of the pine.† In the north of England, at the same height, "roots and trunks of very large pines are still seen protruding from the black peat."‡ Mr WATSON says the Scotch pine now ranges from Perthshire into Sutherland, within latitudes 56–59°.§ But in ancient times, it must have grown indiscriminately throughout the length and breadth of Britain, as we meet with it in many of the English mosses,—those of southern as well as of northern regions.

The common oak has a similar wide diffusion in our peat mosses. According

* Cybele Britannica, vol. ii. p. 409. † SINCLAIR's Stat. Acc. of Scotland, vol. xii. p. 451.
‡ Mr WINCH, quoted in Cybele Britannica, *loc. cit.* § Cybele Britannica, *loc. cit.*

to Mr WATSON, it is now restricted to latitudes 50–58°, finding its northern limits in Ross, Aberdeenshire, and western Inverness-shire.* But the peat mosses of more northern regions exhibit its decaying roots and branches; and nothing is more common than to meet with trunks of oak, of very large dimensions, in situations now in the highest degree unfavourable to the growth of that tree. Similar remarks apply to other species. But not only do the buried trees reach elevations unattained now by the same natural wood, they are also constantly dug out of peat mosses close to the sea-shore, of a size which rivals, or more frequently surpasses, that of their present representatives in Scotland, even when these are placed in situations most favourable to their growth.†

Submerged Trees and Peat.—At various points along the sea-coast, observers have noted the occurrence below high-water mark of tree-roots fixed in a soil, and frequently covered over with peat moss. The shores of the Orkney and Shetland Islands,‡ and the Inner and Outer Hebrides,§ furnish many interesting examples of these phenomena; and along the coasts of the mainland ‖ they are equally abundant. The "Submarine Forests" of England have long attracted attention. Few of the maritime counties do not exhibit them.¶ Around all the shores of Ireland drowned peat is of common occurrence. "At numerous points along the south and west coast it is a common practice for country people to go to the sandy bogs at dead low-water of spring tides, and dig turf from underneath the sand; and it has been equally noted in similar situations along the western and northern coasts. The stumps and roots of trees in the position of growth are found in this peat."**

Again, on the further side of the English Channel, sunk forests abound along the coasts of Brittany, Normandy, and the Channel Islands. In those regions, trees have been observed at a depth which "could not have been less than 60 feet below high-water."†† The peat mosses of Holland, with their buried trees, are constantly continued outwards, so as to extend below the level of the sea. Thus, both on the east and the west shores of the German Ocean, we meet with the

* Cybele Britannica, *loc. cit.*

† Edin. Phil. Jour. vol. xvii. p. 53. See also Phil. Trans. vol. xxii. p. 980; and the Old and New Stat. Accs. Scot. *passim.*

‡ Edin. Phil. Jour. vol. iii. p. 100; SINCLAIR's Stat. Acc. vol. vii. p. 451; BARRY's Orkney Islands; New Stat. Acc., *Orkneys, Sandwick.*

§ SINCLAIR's Stat. Acc. vol. x. p. 373; vol. xiii. p. 321; Edin. Phil. Jour. vol. vii. p. 125.

‖ The Caithness coast shows submerged peat with trees, at Lybster and Reiss (from information by my colleague Mr B. N. PEACH); for notices of submarine forests and peat, see a Practical Treatise on Peat Moss, p. 150; New Stat. Acc. vol. i. pp. 16 and 243; SINCLAIR's Stat. Acc. vol. xvi. p. 556; Trans. Royal Soc. Edin. vol. ix. p. 419. Along the shores of the Firth of Forth drowned peat occurs, as at Largo; also at several points on the Solway coast.

¶ Phil. Trans. vol. xxii. p. 980; vol. l. p. 51; vol. lxxxix. p. 145; Jour. Geol. Soc. vol. vi. p. 96; Phil. Jour. April 1828.

** JUKES' Manual of Geology, 2d edit., p. 686.

†† Jour. Geol. Soc., vol. iii. p. 238.

same appearances as are found to characterise the margins of the English Channel, and the western and northern coasts of the British Islands.

Tree-bearing Peat of Maritime Regions.—These facts, taken alone, prove a general loss of land. But, even without the evidence of the sunk forests, we should arrive at the same conclusion, after considering the nature of those trees entombed in mosses that occur close to our sea-coasts. The great size of the oak, and the dimensions attained by the pine, convince us that, during the period of their growth, those trees were far enough removed from the sea to escape its blighting influence; in other words, that the land formerly extended farther seawards. When we turn to the trees of the submerged forests, we find them in like manner characterised by their large size. Hence, we are compelled to grant a still wider area for the old sunk country.

No island of the Orkney or Shetland groups, can boast the presence of any natural trees deserving of the name. Cultivated saplings are protected by walls, but they cannot raise their tops above the level of the copestones. And yet the mosses and sunk forests of those regions abound with fallen trees, many of which equal in thickness the body of a man. When these buried trees decked the now bleak islands with their greenery, the land stood at a higher level, and the neighbouring ocean at a greater distance. A study of similar appearances in the Inner and Outer Hebrides will induce us to form a like opinion of the changes which they indicate. The broad barren flats of Caithness were also in ancient times overspread with a thick growth of large-sized natural wood, the peat mosses containing which pass below the sea. To have permitted this strong forest growth, we are again compelled to admit a former elevation of the land and a corresponding retreat of the ocean. And so on of all the maritime regions of Scotland.

The same inferences may be drawn from the facts disclosed by the mosses of Ireland and England. On the coasts of France and Holland, as I have said, peat dips underneath the sea, and along those bleak maritime regions of Norway, where now-a-days the pine tree will hardly grow, we find peat mosses which contain the remains of full-grown trees, such as are only met with in districts much further removed from the influence of the sea.*

Continental Britain.—Thus, over a very large area, we have proofs of a process of submergence which, to say the least, has materially diminished the extent of dry land in the north-west of Europe. From other evidence, which it is unnecessary to recapitulate here, geologists have concluded that the area covered by the German Ocean, the English Channel, and the Irish Sea, has been at no distant date in the condition of dry land. After the deposition of the marine beds of the Drift Formation, a movement of elevation ensued, which resulted in the

* From information obtained in Norway, 1865.

union of the British Isles with the Continent. The surface of this new land (overspread with an undulating and profusely dimpled covering of drift deposits), abounded with lakes and pools. We have some grounds for believing, that at this period the climate was still cold enough to nourish glaciers in the higher valleys of our mountainous regions. FORBES has conjectured,* that at this early date our country may have been in the condition of the "barren grounds" of North America. But be that as it may, there can be little doubt, that at some time, during the latest great extension of the European Continent, the upraised beds of the Irish Sea, the English Channel, and the German Ocean, were included under the folds of that broad mantle of green forest, the relics of which are so conspicuous in our peat mosses.

It is certain, that at this time the oak and the Scotch pine were contemporaneous throughout the greater part of Scotland. In the high-level mosses, the latter occurs most abundantly, while the former predominates in the peat of the lowlands. The pine does not appear to have formed any extensive forests at the lower levels of the country, although its remains have been disinterred from many lowland peat mosses.† Its choice of the more elevated regions was influenced, no doubt, chiefly by atmospheric conditions, but also in no slight degree by the nature of the soil. For underneath some low-level mosses, where oak forms the bulk of the buried timber, occasional large-sized pine trees are, as already remarked, not uncommon; showing, that where the soil was favourable, the climate offered no great hindrance to their growth. It is in the hilly regions that the pine obtains that light gravelly soil which it prefers. At the lower levels, those drift clays and earths chiefly abound, which at a former period afforded a favourite habitat to the oak.

Upon the whole, it must be conceded, that north-western Europe possessed at this period a climate more nearly approaching perhaps to that of the wooded regions of Canada, than to the climate which characterises Germany at the present time. The tough resinous wood and thick bark of our bog-pines bear emphatic testimony to the rigour of the seasons that nourished them. The present range of the pine in this country, as contrasted with its former wide diffusion, is also very significant. How changed the conditions which at one time permitted the increase of great conifers in the south of England and Ireland, but which now restrict their native growth to a limited area in the north of Scotland!

II. *Causes of the Destruction of the Ancient Forests.*

Wind.—Some of the more apparent causes of the destruction of our ancient forests may now be considered. It is remarkable, that the trees below peat often

* Memoirs of Geol. Survey.

† *Vide* SINCLAIR's Stat. Acc., and the New Stat. Acc. *passim*, and notices in various county histories, &c.

all lie one way, as if overturned by some potent agency they had met their fate at one and the same time.* The direction taken by the fallen trunks corresponds in a notable manner with that of prevailing winds in the regions where they occur; and hence a large share in the destruction of our woodlands has been attributed to storms of wind. Doubtless, many acres of forest may have been overturned in this way. But we cannot suppose the peculiar position of the buried trunks to be in every instance the result of storms. Trees are usually bent over in the direction of prevailing winds; and when any cause shall lead to their overthrow, whether it be natural decay or otherwise, the position taken by the falling trunks will be determined by the overhanging weight of their tops.†

Lightning.—Again, in our own day, large tracts of forest land in the backwoods of America have been dismantled by fire, kindled during a thunder storm. And we may believe that the resinous conifers of the ancient Scottish woods may also have suffered from the same cause. The marks of fire are conspicuous on the trees of some of our peat mosses. These appearances are to be traced chiefly to the hand of man, but we cannot quite ignore the possible agency of lightning.

Ice.—It is not unlikely also that our ancient woods may have experienced what are known in America as *ice-storms.* In winter time the trees of the American forests sometimes become so heavily laden with snow and ice that they are borne to the ground by the pressure.

Man.—That man has largely aided in clearing the woods is indisputable. Besides the evidence of his hand afforded by the charred wood under peat, we sometimes come upon marks of adze and hatchet.

The earliest historical accounts of North Britain have afforded abundant food for controversy to antiquarians, but when the geologist has gleaned together the few descriptive remarks which occur here and there, in the pages of TACITUS, HERODIAN, and others, he will find that his knowledge of the physical aspect of Scotland does not amount to much that is very definite. He will learn, however, that Caledonia was notorious on account of its impenetrable forests and impassable morasses. But the precise extent of ground covered by these woods and marshes must always be matter of conjecture. The forest land known as *Sylva Caledoniæ* appears to have stretched north of the wall of SEVERUS, but south of that boundary large forests must have existed; indeed, down to much more recent times, many wide districts of Southern Scotland could still boast of their woodlands. Of the nature of those waste plains, described by the

* Highland Society's Prize Essays, vol. ii. p. 19 (Old Series); RENNIE's Essays, p. 31; SINCLAIR's Stat. Acc. vols. iv. p. 214; v. p. 131; and xv. p. 484; New Stat. Acc. *Paisley* and *Carluke*. *Vide* also for similar phenomena in English and Foreign peat mosses, Phil. Trans. vol. xxii. p. 980; RENNIE's Essays, *loc. cit.;* Degner de Turfis, p. 81.

† *Vide* Trans. Royal Soc. Edin. vol. iii. p. 269.

ancients as full of pools and marshes, we can have little doubt, although we cannot of course pretend to point out their particular site. Those who have traversed the central counties of Scotland, must have been struck with the numberless sheets of alluvium which everywhere meet the eye, betokening the presence, in former days, of so many little lakes. In BLEAU'S Atlas, many lochans appear in spots that have long ago come under the dominion of the plough. These, however, must form but a small proportion of the lakes which have been drained since the time of the Romans. Such inconsiderable peaty lochans were not likely to merit particular mention by the Roman legionary who had gazed on the Alpine lakes, save as they became vexatious interruptions to his progress through the country; and surrounded, as many of them in all probability were, with treacherous morasses, the words of the old historians appear to have been descriptive enough of certain ample areas in the Scottish lowlands.

It seems to have been the common practice of the Romans to cut down the trees for some distance on either side a "way," to prevent surprise by the enemy. Several old "ways" have been discovered on the clearing away of mosses, and in their neighbourhood lie many trunks of trees bearing evidence of having fallen by the hand of man. The presence of Roman axes and coins leaves us in no doubt as to who the destroyers were. RENNIE has remarked,* that "of all the antiques found in mosses, by far the greatest part are Roman. No coins nor utensils of any other nation, so far as I know, at least none that would lead us back to a more remote period than the Roman invasion, have ever been discovered." He is therefore disposed to limit the origin of much, if not the greater part, of our peat to the era of the Roman occupation. It need scarcely be added, that since RENNIE wrote, many relics of human art have been disinterred from the peat mosses of Scotland and other countries, which archæologists agree in considering to be of much more ancient date than the Roman invasion. But it is quite evident, that such imbedded relics do not enable us to fix the age of a peat moss. They merely tell us, that the origin of the peat cannot date back beyond a certain period, but may be ascribed to any subsequent time.† Hence, it is impossible to say what amount of waste we are to set down to the credit of the Romans. Some authors have, perhaps, been too ready to exaggerate the damage done by the legions. The buried forests which can be proved to have fallen before Roman axe and firebrand are not many after all; but we may reasonably suppose that these form only a portion of the woods which were cleared at that time.

We have, however, what appears to be direct evidence, to show that some regions had been divested of their growing timber before the Roman period; for

* Essays, p. 69.

† It appears not unlikely that the fact of several mosses having yielded remains of undoubted Roman age, may not infrequently have weighed with local antiquarians in assigning to the same era certain relics of no marked character, which have occasionally been discovered under peat.

if SOLINUS may be trusted, the Orcades were, in the days of the Romans, bare and bleak as they have been ever since. He says, " Numero tres, vacant homine, non habent silvas, tantum junceis herbis inhorrescunt, cætera earum nudæ arenæ et rupes tenent." A patriotic Orcadian might insist that the statement "numero tres" renders what follows untrustworthy; and perhaps he might prefer the testimony of OSSIAN, who, in his poem of Carric-thura, says of some island in the group, " a rock bends along the coast with all its echoing wood." According to TORFAEUS (historiographer to the King of Denmark),* the condition of the Orcades in the year 890 agreed with the description given by SOLINUS.† For at that time EINAR conferred a great boon upon his countrymen by teaching them the use of peat for fuel, *enim in Orcadibus non erant sylvæ.* Yet it is well known that the peat mosses of the Orkneys, and even those of Zetland, contain the remains of considerable trees.

The limits of this communication will not permit me to consider in detail accounts of the condition of the Scottish forests in times subsequent to the Roman period. Any reference by the chroniclers to the state of our woodlands is only incidental, and perhaps not always to be relied upon. It is interesting, however, to learn from BOETHIUS, that the *horrida Sylva Caledoniæ* had in his time become mere matter of history.‡ He further tells us, that Fifeshire had formerly been well wooded (in the times of some of his early Scottish kings); but "it is now," says his old translator, " bair of woddis; for the thevis were sometime sa frequent in the samin that they micht na way be dantit, quhill the woddis war bet down."§ Again, BOETHIUS describes the island of Isla (whose peat mosses contain roots and trunks of trees) to be an island rich in metals, which could not be wrought on account of the want of wood.‖

After the period to which BOECE refers, any allusions to the aspect of the country are best sought for in cartularies and such records. For the rights acquired by monasteries over various forests throughout the country, these cartularies afford abundant evidence. CHALMERS¶ has enumerated many instances of special grants by kings and barons " of particular forests in pasturage and panage, and for cutting wood for building, burning, and all other purposes;" and Mr TYTLER** has added to the list. It need hardly be remarked, that the greater part of these woodlands has long disappeared. And yet, according to CHALMERS,

* TORFAEUS wrote about 1690. He was a native of Iceland, and died in 1720.

† SOLINUS is supposed to have written about A.D. 240.

‡ If this had not been the case, he would surely have quoted a less ancient authority than PTOLEMY for the site of the ancient forest. *Vide* Cosmographie and Description of Albion.

§ Croniklis of Scotland, chap. xi.

‖ BELLENDEN's version of the passage is characteristic. He says, Isla is " full of metallis, gif thair wer ony crafty and industrius peple to win the samin;" but he quietly drops all allusion to the want of wood in the island.

¶ Caledonia, vol. i. p. 792, &c.

** Hist. of Scot. vol. ii. chap. ii., third edition, and the authorities there quoted and referred to.

the old cartularies "abound with notices of forests in every shire during the Scoto-Saxon period." I have not hesitated to quote the authority of those records, and the opinions of two such learned and correct writers as CHALMERS and TYTLER. No one can deny that the evidence of the cartularies is in favour of a better wooded condition for the country than now obtains. But we must guard against the mistake of supposing that all the area embraced under the designation of a "forest" was covered with forest trees. And there can be little doubt that both CHALMERS and TYTLER read the cartularies in the light of the facts which are disclosed by our peat mosses. The trunks of pine, oak, ash, and other hard timber dug out of the mosses, were regarded as proofs that the regions indicated by the cartularies were in reality the sites of great forests at the time to which those records refer. But it is probable, nay, in many cases quite certain, that much of this buried timber belongs to a more remote period. But even with this reservation, Scotland, down to the fourteenth century, would appear scarcely to have merited the description given by ÆNEAS SILVIUS at a later date. During the civil commotions of the country, and the long wars with England, much wood seems to have been destroyed, and the gradual progress of cultivation also began to encroach upon the forest lands. Another cause which aided in clearing away the woods from some portions of the maritime districts, is to be found in the great number of salt-pans that were early established in Scotland, and the right which the proprietors usually obtained to cut the requisite firewood from the forests of the country. But although wood appears to have been the fuel commonly employed in the manufacture of salt, yet it is not unlikely that peats may also have been burned in some cases. It is certain, at least, that peat was a common enough fuel in David I.'s reign, and that, as CHALMERS says,* "petaries became frequent objects of grant to the abbots and convents during the Scoto-Saxon period." This fact ought perhaps to be looked upon as a further proof of the increasing decay of the forests.

But by far the most remarkable testimony to the bare condition of the country is furnished by the Acts of the Scottish Parliament. From the times of the First James, stringent acts were adopted by successive Parliaments,† having for their

* Caledonia, vol. i. p. 793.

† *Vide* Acts of Scottish Parliament. The more interesting acts referring to the state of the woods were passed as follows:—James I., Second Parliament, A.D. 1424; James II., Fourteenth Parliament, A.D. 1457; James IV., Sext Parliament, A.D. 1503; James V., Fourth Parliament, A.D. 1535; Mary, Sext Parliament, 1555; James VI., First Parliament, 1567, Sixth Parliament, 1579, Eleventh Parliament, 1587. It is curious to notice how, from the time of James I. the penalties imposed upon the destroyers of wood increase in severity. Pecuniary fines are succeeded in time by stocks, prison, or irons; the culprit is to be fed on bread and water during confinement, and to be scourged before parting from his jailers. The climax is reached in the following act, which became law in 1587:—"Whatsoever persone or persones wilfully destroyis and cuttis growand trees and cornes, sall be called therefore before the Justice or his deputes, at Justice Airs, or particular diettes, and punished therefore to the death, as thieves."

object the preservation of the woods. Æneas Silvius (afterwards Pope Pius II.), who visited this country about the middle of the fifteenth century, relates, "Pauperes pene nudos ad templa mendicantes, acceptis lapidibus eleemosynæ gratia datis, laetos abiisse conspeximus. Id genus lapidis sive sulphurea sive alia pingui materia praeditum, *pro ligno, quo regio nuda est*, comburitur."* Such a statement regarding the bare condition of the country might have been thought somewhat exaggerated, for it is the testimony of a visitor from more favoured climates; but its truth is curiously illustrated by the wording of an Act of Scottish Parliament, passed in the reign of James IV.:—"Anent the artikle of greenewood, because that the Wood of Scotland *is utterly destroyed*, the unlaw theirof beand sa little: Therefore," &c.†

There are, of course, numerous traditions regarding the former wooded condition of various districts from which the trees have long since been stripped. Many of these refer to some of those woods which I have already mentioned, as being frequently named in the cartularies and similar records.

Another line of evidence is supplied by local names; but into this subject I do not enter here.

The short outline of historical facts given above seems to prove—

1*st*. That when the Romans entered Britain they found the surface of the country to some extent covered with forests, but diversified in many places with bogs and marshes.

2*d*. That to this period we must refer the destruction of some portions of the ancient forests, whose remains are dug out of our peat mosses; but what amount of damage the woods then sustained we have no means of ascertaining.

3*d*. That from the time which elapsed after the departure of the Romans, down to the eleventh century, we have no certain records referring to the state of the preservation of any part of the Scottish woods, if we except the statement of Boethius, who tells us that Fife had in great measure been divested of its forests by some of his early Scottish kings.

4*th*. That from the eleventh to the thirteenth century, and down even to later times, there appear to have been still considerable areas of forest land, the rights to which were frequently granted to ecclesiastical communities and others.

5*th*. That during these centuries much forest was thus cleared and brought under cultivation; that at the same time woods were exhausted by building and burning, more especially as fuel for the salt-works; while extensive tracts were displenished and laid waste during times of war and civil strife.

6*th*. That from the time of James I. there appears to have been a progressive decay of the remainder of the Scottish woods.

* De Europa, c. 46.

† Sext Parliament, A.D. 1503.

Having alluded to some of the more obvious causes which have aided in the overthrow of our ancient forests, I shall now proceed to discuss what I consider to have been the chief agents in the work of destruction. For this purpose it is necessary that the more striking peculiarities exhibited by a section of a workable peat moss should be here borne in mind.

The best *peats* are "cast" towards the bottom of a peat moss. They show a somewhat close and compact texture, so much so as occasionally to resemble coal. Above this the peat begins to lose its more compact structure, and vegetable fibres may be detected, which on a closer inspection are recognisable as those of a moss. Towards the upper portions of the section this appearance becomes still more conspicuous, and the peat seems to consist almost entirely of mossy fibres. Throughout the section long grasses may be seen, sparingly in the lower portions, but becoming more abundant as we near the top, where twigs of heather begin to mingle with them. The upper surface or crust of the peat moss (a foot more or less in thickness), seems to be made up chiefly of heather and grasses, and such plants as Polytrichum. When peat moss wants this crust, it generally shows a treacherous surface covered with moss, into which the unwary pedestrian may sink deeper than he might have expected. Small areas of this nature are not uncommon, but they may be considered as exceptional cases. Most peat mosses are provided with a crust of heath and grass. This *crust* is termed "heather-," and sometimes "hill-peat," from its common occurrence on the slopes and summits of hills, where it does not necessarily overlie true moss peat. It seldom exceeds a foot or two in thickness, and ought properly to be considered as *turf* rather than *peat*.

The compact nature of bottom peats is due to mineralisation. But another variety of peat bog appears to show that vegetable matter, the tissue of which has been nearly lost, may be deposited in a peculiar manner under water. The mosses in which this process takes place are termed in Scotland "flows," a kind of bog characteristic of the peat of the low grounds.* The surface of a flow moss is usually flat, or nearly so, frequently showing dark lochans or tarns, the appearance of which gives us the key to the history of the flow. Their examination is attended with some inconvenience, owing to the instability of the peat, which, when ventured upon, will sometimes rise and fall with a disagreeable undulating motion. The peat, in short, is a mere pan or crust spreading over and concealing

* I have, however, observed flow-mosses on the flat *col* between two hills, and they are also common enough in some valleys, where we have no reason to suppose that they mark the site of lakes. The origin of "flows" in such situations is due to the presence of springs. "Grass and weed," says Dr King, "grow rapidly at the outburst of these. In winter, these springs swell and loosen all the earth about them; the sward, consisting of the roots of grasses, is thus lifted up by the water. The sward grows thicker and thicker, till at last it forms a quaking bog." In the same manner, "flows" are often extended beyond the limits of the lakes which they cover, by the outwelling of the imprisoned water during wet seasons.

a sheet of water, portions of which are seen in the dark lochans referred to. Many flows, however, do not exhibit tarns, and these, during wet seasons, when the underlying reservoir has received the surplus drainage of the moors and mosses, are liable to swell up and burst. It is not difficult to see how the subjacent lake has acquired its covering of peat; for in the gaps of this covering we can watch the process of bridging-over the dark inky water in full operation. Creeping out from the edges of the peat, a thick growth of Sphagnum and various aquatic plants gradually encroaches upon the limits of the tarns. As this outgrowth becomes denser, rusty grasses begin to steal over its surface, and bright tufts of Polytrichum also find there a congenial soil. So the process goes on until a crust, firm enough to support such plants as cranberry, bog myrtle, and heather, eventually makes its appearance; but while the upper surface of this crust or cake of peat thus solidifies and thickens, its under portion rots and falls down as a black vegetable sediment upon the lake bottom, where it slowly accumulates, until in time the depression occupied by the lake may come to be filled up. Many of our deeper peat mosses appear to have had such an origin.

There are thus two kinds of peat—1*st*, That which is due to the continuous upgrowth from the soil of Sphagnum and its allies; 2*d*, Flow-moss peat. The mode of formation of a "flow" is sufficiently evident, but the origin of the other peat mosses cannot always be so readily made out. It is from these last that the buried trees have been dug, and hence it has commonly been thought that the fall of the trunks, by obstructing the drainage, allowed moisture to collect and form a marsh, in which bog-mosses sprung up. The overturning of the timber is thus considered to have been the proximate cause of the formation of our peat mosses. That much peat may owe its origin to such a process is certain, and several cases are on record where the changes referred to have been observed in progress. But this does not seem to have been the exclusive, or even the most frequent, cause of its formation. There are many peat mosses in which no decayed ligneous matter whatever can be detected. In the hilly districts of Southern Scotland, peat, made up almost entirely of mosses, with the usual capping or crust of heather peat, is of common occurrence, even on considerable hill slopes. While in the majority of cases, where the peat of the higher flat-topped hills in the same region is found to contain ligneous remains, the roots and branches are often of small size, indicating the presence in former times of a scraggy brushwood. Now, if bog mosses could of themselves find sufficient moisture to enable them to form peat on a slope of 25°, it is unnecessary to suppose that they must have awaited the overthrow of mere brushwood before they could begin to grow on a flat hill top. It must therefore be allowed that some peat mosses at least have originated without the aid of fallen timber to collect moisture for their support.

Those peat mosses, however, which exhibit the trunks of large trees, bearing marks of fire, or adze and hatchet, may be considered to have had their growth materially aided, if not always primarily caused, by the obstruction of the prostrate trees. But the peat bogs which have supplied such proofs of human agency bear only a small proportion to the mosses where no such traces have been detected. In most cases, both roots and trunks tell distinctly their story of natural decay. In the many mosses which I have visited, the trees were invariably found in such a state as plainly showed that natural decay had preceded their overthrow.

Are we to suppose that the peat only began to grow after those trees had thus yielded to decay? Did the fall of the trees, by choking the drainage, only then bring about the requisite conditions for the increase of Sphagnum and its allies?

In a favourable climate, trees, which have given way before tempest or old age, are quickly replaced by seedlings, and thus the gap caused by their overthrow is gradually filled up Why then, we may ask, was not this the case with the ancient forests of Scotland? It seems strange that the death of the trees, instead of being succeeded by the appearance of another generation, should invariably give rise to a peat moss. The explanation of this anomaly ought to be attributed to a change of climate. From some cause or other, the conditions requisite for the continuous growth and succession of forest trees no longer existed to the same extent. The nature of the great trees embedded in many of our peat mosses points, as already remarked, to the former prevalence, over these regions, of a somewhat excessive or continental climate; and (following other authors) I have sought to connect this period with the continental condition of Britain that followed upon the close of the glacial epoch. Ere long certain changes ensued, with a marked effect upon the vegetation. The succession of trees revealed by several peat mosses seems to warrant us in concluding that the severity of the climate which had nourished the hardy Scottish pines began at length to give way. The most obvious cause of this change must be referred to the new geographical position of the country. The gradual separation of these lands from the continent must have been followed by as gradual an amelioration of climate. Whether, apart from the changes arising from oscillations of level, there may not have co-existed some cosmical cause sufficient of itself to have brought about an alteration of climate, can only be conjectured in the present state of the evidence.*

It is worth noting that the succession of trees revealed by some English peat

* I have confined my remarks on this subject to the peat mosses of our own country, where the appearances presented may be explained, as stated above, by a change from continental to insular conditions. If, however, the alteration of climate referred to was also in great measure due to cosmical causes, the proofs are no doubt to be found in continental peat mosses. As I do not know these turbaries from personal observation, I am unable to say whether the greater proportion of their buried timber has fallen from natural causes or otherwise. It may be surmised, however, that marks of natural decay will probably occur most abundantly in the maritime regions.

mosses does not quite tally with that which is said to characterise the peat of Denmark. In the Danish peat mosses the pine lies at the bottom, and is succeeded in ascending order by the oak and beech. But in the bogs of England, oak and pine occur, as in Scotland, on the same horizon, and in such a way as to show that they must have grown contemporaneously in the same forest, the pine occupying the higher levels and more gravelly soil. Above the oak and pine we often find a *second* stratum of timber, consisting chiefly of birch and hazel. When, over this, we come upon a *third* layer, its prevailing wood is generally alder*. Occasionally, however, pine trees are met with in peat mosses at a lower level than oak.† While it is not denied that in this succession of trees we may have evidence of certain changes of climate, we ought to be careful that we do not attach too much importance to what may in many cases be only a local accident. The succession of trees may sometimes be explained by a change in the nature of the soil alone. Thus, in some of those clayey depressions in the drift, which have been occupied at one time by the oak, we have evidence of a subsequent irruption of fresh water converting the grove into a marsh. When the oaks had succumbed to these changed conditions, we find them succeeded in place by some other species, such as the alder or willow: from which it does not seem necessary to infer more than a mere local change of circumstances. This peculiar succession of trees, however, appears to have so frequently recurred in the peat mosses of England (if not in those of Scotland), that we are forced to conclude that phenomena so general in their appearance must be due to some common and widely acting cause.

But, apart from the evidence supplied by a succession of trees, the geological history of the peat mosses themselves is conclusive upon this point. The phenomena they present indicates the former prevalence of an extremely humid climate. During the continental period the atmosphere must have been moist from excess of vegetation, but in the succeeding or insular condition this humidity appears to have greatly increased. At what time such a climate first began to characterise these regions, it is of course impossible to say; but probably long before the complete submergence of the area now covered by the German Ocean, those changes had already been set in progress, which, in the course of ages, were to result in the formation of many, if not by far the greater portion, of our peat mosses.

The most continuous sheets of peat occur on the west side of our island, and this fact is to be connected with the greater rainfall of the west as compared with that of the east coast. It was over this rainy region that peat would first begin

* *Vide* Timber Trees, *Society for Diffusion of Useful Knowledge*, p. 32.

† Mr SAINTER, in a letter to my colleague, Mr A. H. GREEN, describes the Danes' Moss, a large peat bog near Macclesfield. In this moss, he says, "the Scotch fir is found at a depth of about 20 or 25 feet. A few feet above this lies the larch, and then in ascending order come the oak (*Quercus Robur*), birch, hazel, alder, and willow." This moss occupies a depression.

to spread itself. In the lakes and pools of the country, *Sphagnum* and other aquatics had luxuriated from an early period, covering the surface of the water with an unstable crust, which often-times gave way beneath the weight of large quadrupeds, such as the Irish deer. Many old lake hollows, long since filled up with peat, teem with the relics of this and other animals of the period; but the process of filling up these ancient basins is still in many cases incomplete. Flow-mosses and quaking bogs have yet to pass into the condition of solid peat. With the increasing humidity of the atmosphere, bog mosses were no longer to be restricted to lakes and pools. As the forest trees decayed along the exposed sea-coasts, the mosses crept over their prostrate trunks, and, spreading inland, began to invest those trees that had not yet succumbed to the inclemency of the climate. On the moist hill-tops the same increase of mosses went on, as throughout the country generally there were doubtless many other spots where the same conditions were followed by like effects. The stems, invested by the wet mosses in their upward growth, gradually rotted away, and were thus ready to yield to the first strong wind. So the destruction proceeded—the mosses ever widening their area, creeping outwards and downwards from the misty hills, and inland from the storm-swept coasts.

This mode of accounting for the decay of the trees seems to be warranted by the state in which they have been preserved. RENNIE has remarked, and the truth of the statement is easily confirmed, that the "upper side or surface of trees found in moss is uniformly most consumed; an oak may be often seen where the upper half is so consumed that only the semi-diameter of the tree remains." He subsequently says that when the bark has been preserved (which is not often), it usually adheres to the under portion of the prostrate stems. Thus, at the time these trees fell, a certain thickness of mosses had formed, which received and protected from decay the under portions of the stems, but the upper semi-diameters had rotted from exposure before the advancing mosses could reach them. Dr RENNIE, however, was of opinion that this partial preservation of the buried trees "is a proof that the tree when it fell on the spot had been half-immersed in the mass of ruins, and that half," he adds, "has been thereby preserved entire." Again he says: "Some trees in every forest decay through age; it is probable whole forests may have suffered this fate, especially where a subsoil of *moss* had been formed around the roots of the trees during the period of their growth. Chilled by this means, not only the bark but the white wood would crumble away before the trees were finally overthrown." By the expression *subsoil of moss*, Dr RENNIE means the vegetable mould resulting from the decomposition of fallen leaves, fir cones, and branches, which in another place he shows us might oftentimes form a considerable thickness of peat. Now, it is quite true that at the bottom of many peat mosses we have a certain thickness of what has been called *stratified peat*—the vegetable mould that collected underneath the ancient forests. It is remarkable, however, that this kind of peaty matter is in general so much decomposed that frequently merely the roots of the old trees can be detected. Leaves, twigs, branches, and trunks have usually mouldered away; it is chiefly in its upper portions that this stratified peat yields such remains. And their preservation is due to the protecting properties of the overlying true moss peat. It may be doubted, therefore, whether this vegetable mould has in itself any antiseptic qualities. When trees are overturned in a forest, it usually happens that the portions next the soil are the first to decay. Dr WALKER has described* the destruction of Drumlanrig Wood in the year 1756. The overturned trees, he tells us, being allowed to rot on the ground, ripened into what he has termed "peat-earth." The same author has given an account of the moss of Strathcluony,† as showing distinctly, first, at the bottom, roots of fir fixed in a subsoil. Above these came three feet of "peat," then another set of fir roots, covered over by yet other three or four feet of "moss." Again, atop of "these last roots, which were situated three or four feet deep in the moss, an aged fir was growing on the surface." By *peat* and *moss* Dr WALKER gives us to understand that he means the vegetable mould resulting from the growth and decay of the trunks and branches. These, it would seem, had *entirely decomposed.*

With regard to the chilling effect of this vegetable mould upon growing trees, it may be questioned whether it is likely to produce the effects which Dr RENNIE attributes to it. In the American forests it often attains a considerable thickness, yet the trees of those regions show no signs of decay in consequence. Moreover, the succession of fir stumps with intervening peaty matter, at Strathcluony and other places, shows that the mould could not have the destructive effect supposed. For if it had brought about the death of the first tier of trees, it would not surely permit the growth of a second, and even of a third, generation.

The peat which encloses the fallen trunks of our bogs will be found, when carefully examined, to show fibres of moss, and sometimes to resemble closely the overlying true moss peat. In its upper portions it is certainly not always a heap of decaying leaves and ligneous matter. We must not forget that the weight of a large tree would crush down the few feet of soft pulpy moss on which the trunk fell. Twigs and branches would soon crumble away, and the mould resulting from the partial waste of the stem itself would mingle with the surrounding peat moss; so that it might well appear, when the tree came to be dug up, as if the peaty matter on which it rested, and by which it was partially enveloped, were composed of its own decomposed substance, differing, as in some degree it must, from the overlying pure mossy peat.

* Essay on Peat, Highland Society's Prize Essays, vol. ii. p. 19 (Old Series).

† *Idem*, p. 20. *Vide* also, for examples of the same phenomena, Irish Bog Reports, vol. ii. p. 61; Transactions of the Geological Society (Second Series), vol. ii. p. 140.

In hill mosses, where the buried trees are of small size, the bark, when preserved, not infrequently adheres all round the stems, as if the mosses had been enabled to cover over the prostrate trees before decomposition had well begun.* Again, both in high and low-level mosses, the stools of trees have not uncommonly retained their bark, even when the trunks and branches in their neighbourhood have greatly decayed. The wet plants seem thus to have protected the under portion of the stem, while at the same time they killed the tree. When the main mass of the trunk eventually gave way, it fell to the ground stripped of its bark. But upon this last appearance much stress cannot be laid, because we should expect that when old age shall overtake a tree, its lower portions will be the last to go; and consequently, long after the other parts of the trees have mouldered down, the stools might still resist decay, and by and by come to be buried in a good state of preservation.

It is well known that the stools or stumps in many mosses are frequently all of one height. When the overthrow of the trees has evidently been the result of natural causes, this appearance is inexplicable unless we call in the aid of the bog mosses in the manner stated. The old statist of the parish of Kilbarchan, in Renfrew, tells us† of the buried trees in the mosses of his district, that "the stumps are standing in their original position. The trees are all lying from south-west to north-east. "How an oak tree," he says, "could break over at that particular place I never could understand. But we may be allowed to form a conjecture, that before the tree fell, the moss had advanced along its stem, and rotted it there."

In those mosses where the rooted stumps are of unequal height, we may account for their overthrow in the same manner. In the case of the Kilbarchan trees, *wind* may have been the immediate cause of their downfall. But when, left to themselves, the doomed trunks were no longer able to support their own weight, and began to give way, they would not all do so at the same time. Some species might die and fall to ruins, while others still lived on, and the more massive trees would long tower over their less sturdy brethren. As the mosses were all the while adding to their thickness, the trunks, as they successively yielded, would break off at different levels, while their fallen portions would be buried at slightly different depths, the pressure of the fallen trees tending to squeeze down the soft underlying moss.

Nay, it has even happened that mosses have overtopped the trees which they have killed. In the moss of Curragh, Isle of Man, large trees have been met with standing erect, with as much as 20 feet of peat above them. The same pheno-

* It must be remarked, however, that a large proportion of the trees here referred to consists of birch, the bark of which is usually obtained in a good state of preservation, although the wood itself may have entirely decayed.

† Sinclair's Stat. Acc. vol. xv. p. 484.

mena are said to characterise "many of the deepest mosses of the continent." Thus the chief cause of the destruction of our ancient forests appears to have been a change of climate. The altered atmospheric conditions were not only directly unfavourable to the propagation of forest trees over certain large districts, they also brought about a vast increase of marsh plants. To the chilling effect of the wet bog mosses in their upward growth must be attributed the overthrow of by far the greater portion of the buried timber in our peat bogs.

As the origin of this change carries us back far beyond the earliest dawn of history, it follows, that much of the peat and buried timber of our country may be of great antiquity. And, indeed, in the case of many mosses, we seem more likely to err in ascribing too recent than too early a date to the period of their formation. We cannot estimate the time which has gone by since our western islands supported those timber trees, the remains of which are dug out of the mosses. It is highly probable that at this early period those islands were joined with the mainland, and shared a continental climate. To the same date we may refer much of the buried timber of the Orkney and Shetland Islands. Again, the more elevated peat mosses of our country must have been among the first to be formed; for, as already remarked, any change from a continental to an insular state, would tell first upon the trees that grow along the sea-board, and at the higher elevations of the land. It seems very reasonable therefore to conclude that, long before the Romans set foot in Britain, the growth of peat moss and the overthrow of timber had made considerable progress; that, in short, the *Sylva Caledoniæ* was but the relics of that great forest which in former ages had spread all over the area of these islands and the German Ocean.

I cannot enter into the vexed question of the probable area of wooded land in Scotland at the time of the Roman invasion, but there can be little doubt that had our peat mosses never yielded any remains of former forests, very little could now be said for the wooded condition of Scotland in the days of the Cæsars. The buried trees are of no avail as evidence, unless we can prove them to have been overturned since the times of which TACITUS writes; and it is surprising how few tree-bearing or other peat mosses can be shown to be of this or later dates. By far the greater number belong to much earlier times. In the few historical notices given above of the state of the Scottish woods in ages subsequent to the departure of the Romans, no small share in the work of destruction has been assigned to man himself. How far his efforts were seconded by an adverse climate it is impossible even to conjecture. But that to some extent they may have been is not unlikely, from the fact that some of the buried trees, belonging to peat of Scoto-Saxon and still more recent dates, have evidently fallen from natural decay; while, in many districts, which record and tradition allow to have been at one time well wooded, the indigenous wood of the country either refuses to grow, or at best attains to but a sorry size.

III.—*The Present Aspect of the Peat Mosses.*

A glance at the present aspect of our peat mosses will convince any geologist that this formation has not only ceased to spread, but is in most cases rapidly disappearing. The moisture which in former times afforded it nourishment and support, has now become its chief enemy. Every shower of rain, every frost, gives fresh impetus to the decay; and leaving altogether out of account the operations of agriculture, we can yet have no doubt that natural causes alone would, in time, suffice to strip the last vestige of black peat from hill and valley. These remarks might be illustrated by examples drawn from any peaty district of Scotland, did space permit, but a general description of the appearances presented by dead peat is all that can be attempted.

The surface of peat which has ceased to grow is usually covered with short scrubby heath and rusty grasses, but frequently in so sparse a manner, that every here and there the black peaty mould peers through. Nay, in many cases the decomposing peat lies exposed and bare, with not a tuft of heath or blade of grass to be seen. Peat mosses of this description are not confined to any particular locality or situation. They occur generally throughout the country, and may be found on hill tops, on hill sides, and in valleys.

The peat of a hill-top usually shows a most ragged and wasted aspect.* Innumerable winding gutters of variable breadth intersect it in all directions. These widen as they approach the circumference of the peat towards the brow of the hill, where, at their outlets, they are often yards across. When the hill-slopes have retained any of their peaty covering, it is usually still more cut up than that on the hill top. These natural channels or drains are due to the wasting action of running water, assisted by winter's frosts. The peat is dead, and, like the upraised coral reefs of the South Seas, wears rapidly away at the touch of the atmospheric agents. Year by year, the little channels are eating their way back into the heart of the peat, and the process of destruction has often been carried so far as to have left merely a few irregular-shaped segments of peat scattered here and there over the top of the hill.

* Few regions exhibit the decay of hill-peat to better advantage than the Moorfoots and Peeblesshire hills. The summits of the Moorfoots may be well described as a wide platform or table-land, out of which valleys have been scooped by rivulets and streams. Standing on the shoulders of one of the hills at the head of the Leithan Water, we see stretching out before us what appears to be a wide-spread and undulating peaty plain. To the wanderer across these hill-tops, the deception is often for some time complete, till of a sudden he finds himself on the brink of a green grassy bank, which slopes steeply down to a brawling stream, and again rises to a corresponding height on the other side. The peat stopping thus abruptly where the ground begins to descend, has the appearance from below of a black wall running continuously along the brow of the hill. This is especially conspicuous in the valleys of the Leithan and its tributaries, and also at the heads of the Luggate, the Heriot, and the Gladhouse Waters. The higher grounds of the Border counties also show peat in every stage of decay. The same appearances characterise the flat-topped hills of Carrick, which overlook the broad undulating moors of Wigton and Kirkcudbright.

The peat of the lower grounds attains to a much greater depth, and has less of a denuded and wasted aspect than that of the hilly regions. In many spots, indeed, Sphagnum and other peat-forming mosses seem to thrive well. But wherever the slope of the ground suffices to create a drainage, the peat crumbles away. Even where the peat occupies a hollow or plain, where the drainage must be weak, the decay of the moss has often been observed making rapid progress. There are, perhaps, but few bogs in Scotland which do not exhibit what are known as *moss-hags*—holes usually filled either with soft black mud or dry peaty mould. These moss-hags are the result of denudation, and increase in width by the gradual mouldering away of the surrounding peat. Sometimes a system of natural drains or gutters connects the different holes; but many of these show no apparent outlet, and the water collected in them escapes, by slowly soaking through the moss, or by creeping outwards between the peat and the soil.

The peat mosses of Scotland are thus only a wreck of what they have once been. The out-growth of peat has ceased to be general. Here and there mosses continue to increase in sufficient abundance to form that substance; but this increase, such as it is, is far exceeded by the general rate of decay. The peaty covering is almost everywhere full of holes and winding channels, a sure sign that the bogs have ceased to combat against the denuding powers of rain and frost. Their upper surfaces are no longer overspread with Sphagnum—a hard crust of heath and grasses caps them instead. All this points to a decrease in the humidity of our climate. It would be interesting to ascertain at what time a diminished rain-fall first began to tell upon the growth of peat. Dr ANDERSON has remarked: "It is well known that mosses, large ones especially, are universally bare on the surface (I mind not trifling exceptions), or covered with a few cows of heather only; and it is as well known, that they have been in the same bare and unproductive state since the earliest accounts of them have been preserved." But the early accounts to which that author refers do not enable us to form a definite notion of the nature of the surface of our peat mosses in olden times. We can only gather from them that moss-troopers enjoyed a certain immunity from the visits of troublesome strangers who, not knowing the secret paths across the mosses, dared not trust themselves to the treacherous surface. Some of the bogs alluded to were, no doubt, flow-mosses, which are equally impassable at the present day by any body of men, either on foot or horseback. It seems impossible, however, to learn from historical records at what time those peat mosses, which cover over the ancient woods, first received their crust of heath and grass, and then began to break up into hags and channels. We know that the Covenanters of the seventeenth century employed the holes in the mosses as hiding-places. Many traditions to this effect are floating about the moory districts of Carrick; and the cairns and memorial-stones, marking out the spots where those unfortunate dissenters were discovered and shot, confirm the truth of the traditions.

The decrease of moisture which induced the death and waste of the mosses must have been very gradual. Year by year its influence would begin to be felt,—first, in those districts which have the smallest rain-fall,—last, in hilly regions, and other places, where the rain-fall is great. Hence it is that we have still the largest display of peat along our western and south-western borders, and upon the moist hill tops and elevated valleys of the north and south of Scotland. The same may be said of England; and Ireland (which drinks most deeply of the Atlantic rains) can still show a goodly quantity of growing peat.

It has been said that the Scottish peat mosses have originally covered a much wider area than they do now. In the east, as well as in the west, great mosses once existed, which have now either disappeared or are fast disappearing. The peat on the tops of the Lammermuirs, the Moorfoots, the Peeblesshire hills and other hilly districts in the same region, is evidently the remains of a broad peaty mantle, which at one time stretched down many of the hill sides, and was continuous with the mosses of lower levels. Segments of this old covering may still be seen lying on some hill sides, and looking as if they had broken off and slipped down from the wasting cap of peat on the top of the hill. The alluvium of the same regions often shows alternations of sand and clay with peaty matter, borne into its present position from the hill sides and upper parts of the valleys.

I have often had occasion to notice, that of a peat-covered hill the least ragged portion of the mantle will invariably face the direction whence come the rainy winds. This is very well seen in the hilly districts of South Carrick and Galloway. In that region, the hill-slopes looking to the south-west may have a thick, continuous coating of peat, while the peat on the opposite side is seamed with innumerable winding channels, as if it had been violently rent in pieces.

All these appearances convince us that the climate of the country has become less humid. We must look to the drainage works of the agriculturist, and consider whether these are sufficient to account for this change. But after allowing that much may be done by a good and extensive system of drainage to affect a climate, we may, perhaps, doubt whether the changes which have rendered our atmosphere less humid can be assigned exclusively to this cause. Such considerations are not, strictly speaking, within the geologist's province. I may, however, state in conclusion some of the reasons which have led me to suspect the existence of some cosmical cause, apart altogether from the agency of man.

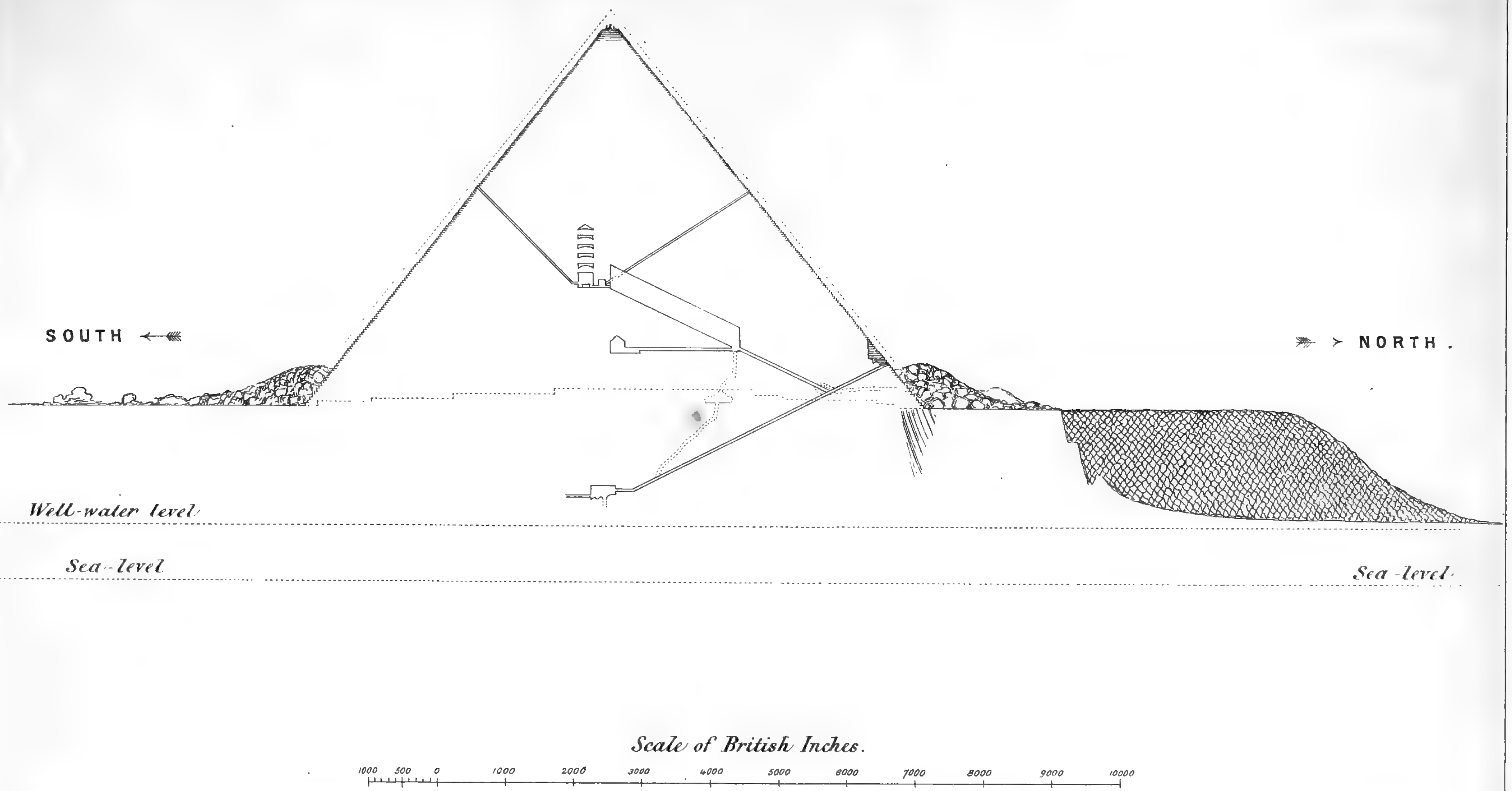
Drainage operations, although carried on very generally throughout the country, are yet not everywhere effective. Some districts are much better drained than others; and there are still portions of the country to which this important work has not as yet been extended. It is only within recent years, indeed, that the drainage of peat lands has met with the attention it merits. And yet almost everywhere in Scotland death and waste have assailed the peat mosses, showing that the decrease of moisture which has brought about this result does not arise from any mere local cause, but is characteristic of the general climate of the country. To this we must add the fact, that our peat mosses had begun to moulder away long before anything like the present systems of drainage had been adopted.

As we proceed from north to south, we find that the peat has not only suffered a larger amount of denudation, but its substance has been pulverised or "consumed" in a greater degree. Thus, the peat of England, especially in the southern districts, is more consumed or decomposed than that of the Scottish mosses. In other words, a longer time has elapsed since the English peat ceased to grow, so that having been exposed during this period to the power of the atmosphere, it exhibits stronger marks of waste than the peat of Scotland. The French peat is said to be still more consumed than that of England; and indeed, it may be remarked generally of the peat of southern latitudes, that it has crumbled away to a much greater extent than that of more northern countries.

The change of climate indicated by the wasted aspect of peat moss thus appears to have shown itself first along the southern limits of that formation in Europe. It then slowly extended its influence in a northward direction, meeting in its course with many modifications, such as must arise from local circumstances. Chief among these was the configuration of the land—the peat of low-lying districts dying out more quickly than the mosses of higher levels, where any diminution of moisture is last to be appreciated. In the same manner, the track of the rainy winds on the west and south-west coasts have also marked out a region where we now meet with less waste among the mosses than in those of other districts. But as the effects of such a cosmical change must be so blended with the results brought about by the progress of cultivation, we can do little more than suggest the extreme probability of its existence. As it can be shown that the destruction of our ancient forests has not been primarily due to man, although in the later stages of the process he certainly played an important part, so we may suspect that the change from a humid to a drier climate has also been effected by natural causes,—but man, eagerly following nature, has outstripped her in her work, and so identified this with his own, that it now becomes hardly possible to distinguish the one from the other.

VERTICAL SECTION OF GREAT-PYRAMID OF JEEZEH,

IN THE PLANE OF THE PASSAGES.



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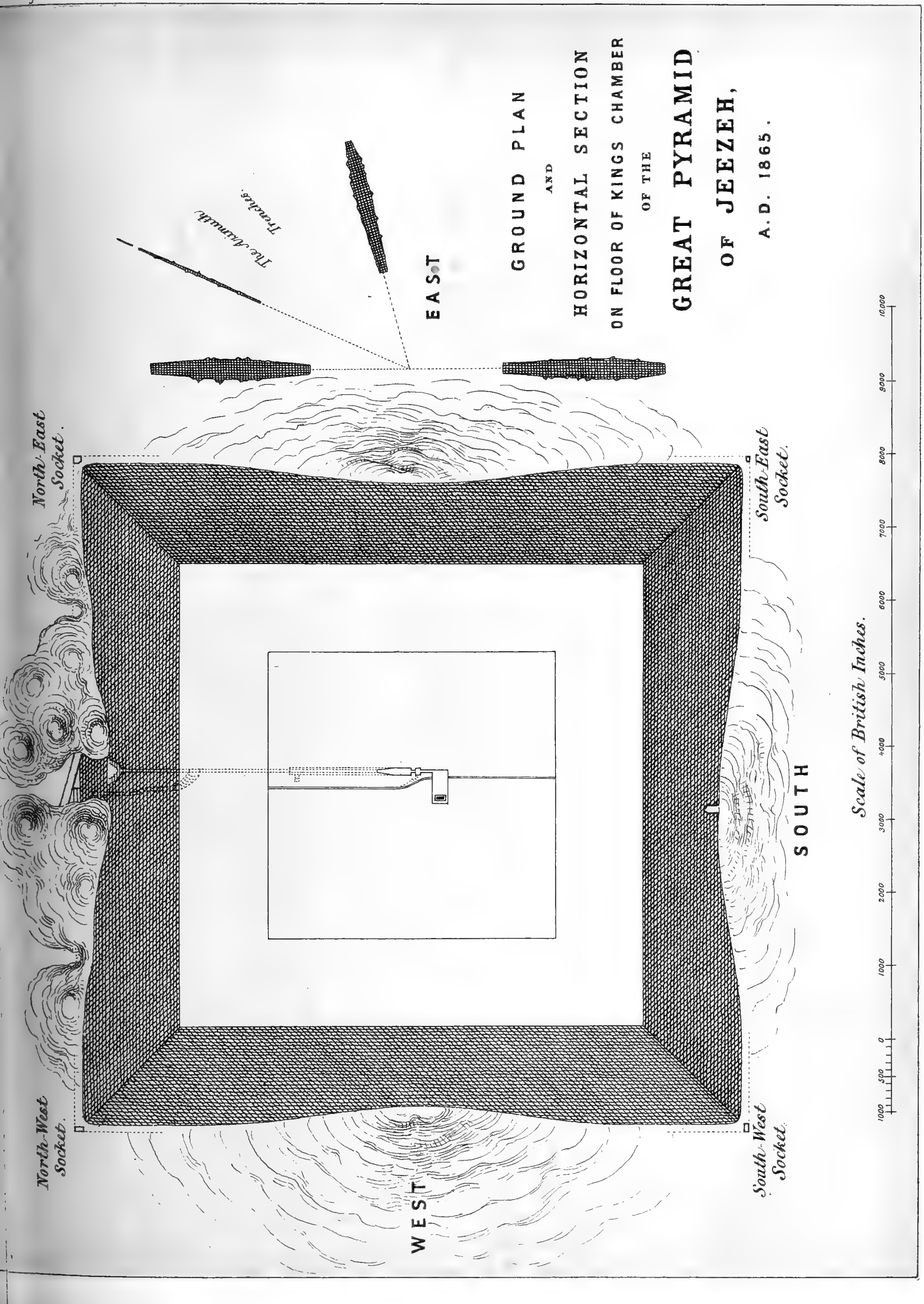
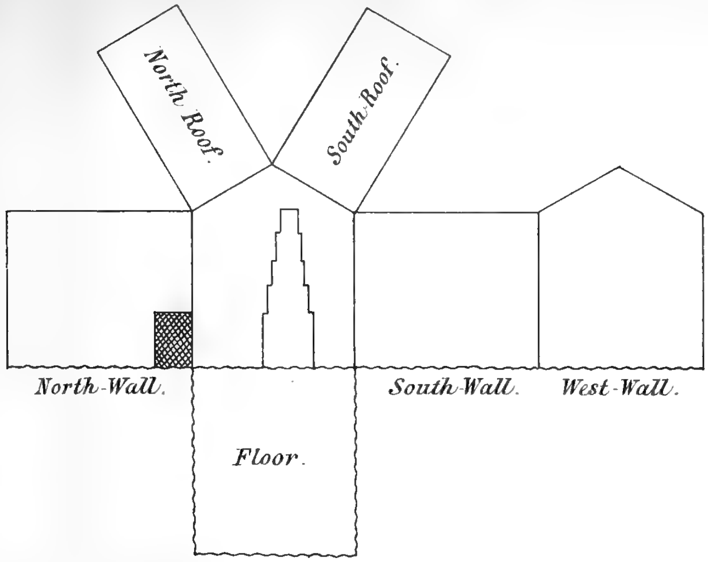
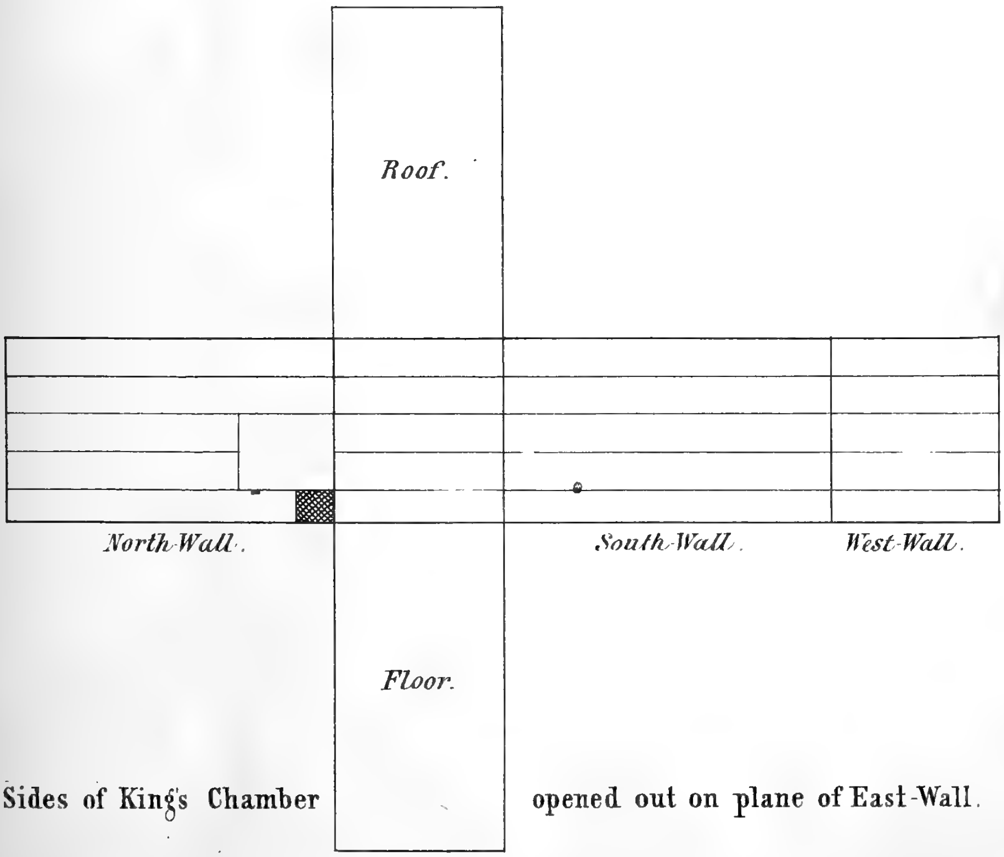

GROUND PLAN
AND
HORIZONTAL SECTION
ON FLOOR OF KINGS CHAMBER
OF THE
GREAT PYRAMID
OF JEEZEH,
A.D. 1865.
EAST
WEST
SOUTH
The Azimuth Trenches.
North-East Socket.
South-East Socket.
North-West Socket.
South-West Socket.
Scale of British Inches.
1000 500 0 1000 2000 3000 4000 5000 6000 7000 8000 9000 10,000
W H Mc Farlane, Lithr Edinr

THE CHAMBER OF SEVEN.



Sides of Queen's Chamber opened out on plane of East-Wall.

THE CHAMBER OF FIVE.



Sides of King's Chamber opened out on plane of East-Wall.

W H MFarlane, Lith. Edin.

XXIX.—*A Notice of Recent Measures at the Great Pyramid, and some Deductions flowing therefrom.* An Address delivered to the Royal Society, Edinburgh, at the request of the Council, by Professor C. PIAZZI SMYTH, Astronomer Royal for Scotland. (Plates XXVI., XXVII., and XXVIII.)

(2d April 1866.)*

MR PRESIDENT AND GENTLEMEN,—I beg to thank you for the favourable opportunity which you have kindly afforded, and the facilities you have granted, for enabling me now to try to lay before you some short and simple account of my chief employments last winter at the Great Pyramid of Jeezeh.

My object in going there, was not to excavate, nor to collect antiquities, but merely to inquire instrumentally, and by my own individual labour, into the very discrepant, and sometimes mutually contradictory, accounts which have been published of the form and detail of that ancient monument by writers of almost all nations; and as one chief source of their unfortunate variations seems to have been the usually short and hurried character of their visits, my first care was to apply for leave, from His Highness the Viceroy of Egypt, to occupy the ground at the Pyramid with a permanent establishment, and to stay there as long as might be necessary for the work in hand. His Highness, as I am extremely happy to confess, and with the best of my thanks to acknowledge, was most liberal in his condescensions; conveyed our party, at his own expense to the Pyramids; lent us tents for the period of our stay; and sent a force of twenty men for a month to clean out the interior of the Great Pyramid, or otherwise prepare its more than classic walls for the examination to be made, and which lasted from that time uninterruptedly for a period of four months.

Of preliminaries to the instrumental inquiry, I would beg to mention, first of all,—but in a general way only as to the locality, and in reference to the fossils now on the table,—that the hill on which the several Pyramids stand, with much of the Libyan desert behind, or westward of it, is composed of limestone of the earlier tertiary formations, arranged in extensive and massive sheets of tough strata, all of them dipping a few degrees to the south-east, and richly charged with nummulites, echini, and various other fossils. From that stand-point looking northwards, you may see older strata, such as the chalk of the secondary rocks, cropping out on the distant edge of the tableland; and southwards, you

* ERRATA.

Page 394, line 2, *for* measure, *read* measures.
,, 395, last line, *for* This last important element, *read* This important density element.
,, 397, line 26, *for* it is, *read* such pound is.
,, 400, ,, 15, *for* in ten, *read* in terms of ten.
,, 401, ,, 4 ab imo, *for* been marked, *read* been passage-marked.
,, 402, ,, 11, *after* the Pleiades, *insert* or their *lucida* η Tauri,.
,, 402, ,, 22, *after* than, *insert*, but equally primeval with,.

come upon newer formations, probably the latest of the pleistocene, where the hill surface is, in places, richer in well-preserved and almost fresh-looking cardiums and other shells than any modern sea-beach.

Throughout all these various ages of hills, however, it is important for our present purposes to know, especially as confuting *some* theories of the Pyramids, that there is no trace of igneous or metamorphic action of any kind or degree; and though large quantities of granite, greenstone, basalt, and diorite are constantly found lying about (and of which the table holds a few specimens), they can all be traced up to various monuments in the neighbourhood, whereto they were brought by ancient industry from distances of many hundreds of miles.

Secondly, and in a similarly general manner only, I would mention,—that it was easy to perceive that the Pyramids, with all their mysterious grandeur of a prehistoric age, are surrounded by tombs, often fully as old as themselves; the whole region is, in fact, an enormous burying-ground, used over and over again by several of the earlier dynasties of Egyptian polity; and since their time plundered, emptied, and ransacked to the last degree by later-empire Egyptians, Persians, Greeks, Romans, Arabs, and at last by modern Europeans, causing all one's steps occasionally to be amongst smashings of coffins and bones of men.

Yet though found among them, and often used for the same purposes, the Pyramids, as a class, are distinguished from tombs proper; for while the interiors of these are covered with writing, carving, and painting, replete with description and story upon nearly every particle of their surface, there is nothing of that kind, excepting of course the quarry marks on concealed fragments, to be met with inside the Pyramids. In the interior of the Great Pyramid, indeed, which differs even more than the others, and in a most pointed and peculiar manner from any known sepulchral arrangements, *its own* grand and solemn walls either speak not at all, or in the pure and highly-fraught language of proportions of geometrical surfaces and mathematical angles.

In the interpretation of *such* signs, hierology and literary Egyptology give us no assistance; classical authors are entirely misleading, and there is nothing for it in our case and times, but to attack with the instruments and methods of modern science, and as a scientific problem, the great practical work which bridges, at least, the last four thousand years of the world; and brings us in these latter days, face to face with men who thought great thoughts and acted nobly, more than a thousand years before Homer wrote or Agamemnon ruled.

In this view, Sir, my examinations and proceedings took the form, first, of measures of length, by means of the various rods and scales now exhibited on the table and about the room.—Second, of measures of angle, by means of the sextant apparatus, circular clinometer, and alt-azimuth instrument before you.—And, third, of measures of heat, by means of a variety of thermometers.

The individual observations made with all these instruments are numerous enough to fill several manuscript books, and will be accessible when required; but at present I will only ask your attention to a few of the results; and these results should begin by bearing on some of the very simplest propositions that have ever been ventured concerning the Pyramid; for we cannot at first starting be too sceptical of anything like mere assertion, and must try to prove our way as we proceed.

Proposition I.

Let this model, if you please, on the table, represent the figure of the Great Pyramid; drawing No. 1 (Plate XXVI.), a vertical section of the same through its central axis; and drawing No. 2 (Plate XXVII.), a ground plan; then, many theorists and travellers have assumed as exact, what is evidently true approximately; viz., that the base of the Great Pyramid is a *square.*

But they have only assumed, asserted, or concluded, not measured it; and meanwhile, a Dr VANSLEB has declared in his published Travels, that he could see very easily that *all* the Pyramids had invariably two out of their four base-sides much longer than the other two; and he tried to measure the amount of such difference at the Great Pyramid with a long rope. But the outside of the monument was so ruinous, and its base so encumbered with heaps, nay positive hills, of broken stones, that his efforts at measure were defeated; and yet he leaves his readers under the impression that the anomaly he had previously indicated, must have been at least a sixth or so of one of the sides.

To such a statement, then, my labours answer thus,—first, having measured again and again about the huge monument, correcting for errors of dilapidation and encumbrance of the ground as well as I could, there was shown to be no error in the length of any of the four base-sides, equal to the 80th part of one of them.

Second, during the last fortnight of our party being at the Pyramid, there came there with a following from the works of the Suez Canal, Mr WILLIAM AITON, the well-known and most energetic railway contractor of Glasgow; and he set his people digging at all the four corners of the Pyramid, until they had not only rediscovered the two corner sockets, marking the original size of the northern side of the monument, and which had been first discovered by the French *savants* in 1799, but until they had also come upon the other two corner sockets, which had never been seen by mortal eye for the last thousand years at least.

Then, all four corners of the ancient base of the Pyramid were visible at once, and were found to be clearly cut in the solid and monolithic rock of the hill; and Mr AITON's chief assistant, Mr INGLIS, having measured every one of the four base-sides from socket to socket, but still under excessive difficulties from the encumbrance of the intervening ground, found the greatest difference of any two sides, including error of observation, not more than $\frac{1}{508}$th.

And, thirdly, I was enabled, though in a rather round-about way by reference to the Pole star, but with the magnificently powerful alt-azimuth circle before you, to measure the angle contained between two adjacent base-sides of the Pyramid, as defined by their sockets; and found such angle to be equal to 90°, all but the small quantity of 44″; so small a quantity, that if produced by an error in the length of the opposite side, such error could be only the $\frac{1}{3600}$th part of the whole side.

Proposition II.

The next assumption has usually been, that the four sides or slopes of the Great Pyramid incline towards the central vertical axis at *equal* angles. But against such a belief, Dr PERRY has recorded in his Travels, that every one can see that each of the sides of the Pyramid inclines at a different angle, one 3°, another 4°, and another even 8°, from its fellow. There was a memorandum also handed to me before going out to the Pyramid, by a member of the Egyptian Institute in Cairo, making, from recent observations, differences of as much as 4°.

These differences, no doubt, arise to some extent from the present dilapidated and worn state of the surface of the monument, all the ancient exterior of which has been long since stripped off, and only the ruined courses of the mere internal bulk of masonry remain. Yet, nevertheless, when I tried, after a familiarity of some months with features and accidents of the Pyramid, to choose out stones above and below, very little weathered, and measure between them, with better instruments too than my predecessors had used, the difference of any two sides was reduced to 18′.

In the second place, the angles of elevation at the corners of the Pyramid being measured from the exact stand-points given by the sockets cut in the rock, the greatest difference of any one angle from another was found to be, on reducing them all to one level, only 3′.

And, in the third place, by looking about among the heaps of broken stones on every side of the foot of the Pyramid, innumerable fragments of the ancient casing stones were picked up; and all which admitted of measure, proved to have practically the same angle on whatever side of the Pyramid they were found.

The nature of the casing stones of the Pyramid may be easily gathered from the accompanying model. The great bulk of the Pyramid is constructed of coarse masonry in rectangular steps, like these which form its present exterior; but outside them a casing for the whole Pyramid was anciently laid on, thus,—first, one or two layers of smooth, and comparatively small-sized, but well-cut rectangular white stones; and, finally, a layer, rectangular within, but beveled off outside, at the general angle of the whole Pyramid.

These stones were always laid in accurately horizontal courses; and having had a number of their veritable fragments recently inserted into a horizontal section of this carefully constructed model of the theoretical Pyramid (App. II.), no difference in the outer slope of each of the stones can be detected, as dependent on the side of the original Pyramid, from which the specimen came.

Proposition III.

Our third proposition is to the effect that the angle of elevation of any one of the sides of the Great Pyramid is 51° 51′ 14″ 3. This quantity was affirmed by the late venerable JOHN TAYLOR of London, founding on Colonel HOWARD VYSE'S measures of his two colossal casing stones *in situ.* Most important too is JOHN TAYLOR'S statement, if proved to be real; for on possessing that precise angle, and that only amongst any number of possible angles, depends the power of the Pyramid to symbolize by its height and length of perimeter of base, one of the most radical and important truths in both pure and applied science, viz., the proportion of radius to circumference in a circle.

But that angle for the Pyramid, though derived from good measures, has by no means been allowed to stand unchallenged either by observers, or theorists; one of the very greatest of whom, the late eminent Baron BUNSEN, insists on 51° 20′ 25″ being the true quantity, because it results from a favourite hypothesis about the employment of the profane Egyptian cubit.

I beg therefore to report from my practical proceedings,—

First, That the mean of many measures on all the present sides of the Pyramid, dilapidated though they be, amounts to 51° 49′ nearly.

Second, The mean of the angles of nineteen fragments of casing stones, many of which you see before you, gave, on being carefully measured, 51° 54′.

Third, The angles of the corner lines of the Pyramid being measured from the sockets, and reduced to the equivalent angle of the sides, give, with an assumed probable thickness of the ancient casing near the top of the Pyramid,—the measures too having been taken with the large PLAYFAIR altitude azimuth instrument,—give 51° 51′.

And, *fourth*, There is a completely different and a new testimony to be referred to, the azimuth trenches. (See Plate XXVII.)

These are long and deep pits cut in the solid limestone of the hill, with hammer and chisel, on the eastern side of the Great Pyramid; and there is nothing analogous to them about any other of its sides, or indeed any side of any other Pyramid.

Speculation has been various on these trenches; men of architectural turn of mind have pronounced them the pits in which the mortar was mixed at the building of the Pyramid, and others of Egyptological pursuits have declared them tombs. To myself, however, they gave so eminently the impression of a laying out of angles before the erection of the Pyramid, that I set to work with the powerful discrimination of the large altitude azimuth circle to ascertain what the angles were.

Placing the instrument therefore at the very remarkable general converging point, it was first ascertained, and through agency of the Pole star again, that the north and south trenches were all but exactly parallel to the north-east and south-east sockets of the Pyramid's base; and then, that the grand diverging trench towards the east went off from them at an angle of 76° 18′ 38″; or the angle very nearly of the top of the Pyramid, according to Mr TAYLOR's hypothesis.

From the angle at the summit we can, of course, deduce trigonometrically the angle at the foot of the Pyramid; but we have that given also by these trenches in another and directer manner, viz., by the amount cut off the large angle by the small subsidiary trench; and if we take the mean of that and the former result, the angle comes out 51° 51′ 35″, nearly.

Now it may be said, that this close agreement with Mr TAYLOR's number is only an accident; but then *what* an accident! Tradition and subsequent history have indicated that the other Pyramids of Jeezeh were built in imitation of the Great Pyramid, and that their builders were well content with the success of their imitation; how close then to their grand exemplar did they come in this ethereal matter of angle?

The second Pyramid has an angle of 52° 50′; the third of 51° 0′; and the others are near upon 52° 12′: *i.e*, not one of them comes within a quarter of a degree.

Now I would not attempt to maintain against the architects, that these trenches were *not* used at the building of the Pyramid for mixing mortar in; nor would I argue against the Egyptologists, that they were *not* used for tombs, when the building was finished, and the mortar mixing over,—but I would only point out that these purposes in themselves cannot explain the angles at which the axes of the trenches are actually situated; and the cutting of them at these angles must have preceded the two subsequent utilitarian purposes the long cuts were eventually put to.

Hence opinion would seem to be driven to either one or other alternative; viz., if the Pyramid architects laid out these angles intentionally, they knew something of their scientific importance; but if they did not lay them out intentionally, and attained the precise angles without effort on their part, then, what some men call accident, others may prefer to look on as Providence overruling the fitful works of men for grander ultimate ends than they had any idea of at the time. But let us proceed further in our safe practical examination.

Proposition IV.

This proposition refers to the vertical angle of the long inclined passages in the interior of the Pyramid; they have been described by various travellers at anywhere between 25° and 27°, and even 30°; but Mr TAYLOR's theory extended says they should be at 26° 18′ 10″, and there are three different passages each to be measured. (See Plate XXVI.)

The first and second, viz., the entrance passage, and the first ascending passage, are indeed both small and of inferior workmanship, and were found by my measures to be, one at 26° 27′, and the other at 26° 6′, or one too much and the other too little. But the grand gallery is a vastly superior passage, in height, breadth, length, and its whole architectural character, besides being a perfectly unique feature in the Great Pyramid over all the Pyramids of Egypt.

With the extremest anxiety therefore to be accurate, did I begin to measure the angle of the grand gallery by a clinometrical method. The clinometer itself, one of the most accurate instruments of the kind ever made in Europe, was one of the two separate instruments which my friend, Mr ANDREW COVENTRY of this Society, voluntarily presented to me before going to Egypt, as an indication of his earnest desire to assist in having the Great Pyramid, if possible, better measured last winter in some of its crucial features, than it ever had been before.

In use, the fine part of the clinometer was fixed on a long and stout beam of mahogany duly prepared and furnished; and in that state the apparatus was made to step up the whole distance of one side of the grand gallery from north to south, and then to step down the other side from south to north, when the mean of all the observations in one day came out 26° 17′ 4″. Other observations were taken subsequently, and the mean of the whole was finally left at 26° 17′ 34″.

Proposition V.

The fifth proposition depends on a further development of the theory, and demands a latitude for the Great Pyramid close upon 30°.

The large alt-azimuth instrument was well adapted to this question, and from observations taken with it on the summit as well as at the foot of the Pyramid, the monument was found *not* to be in the required latitude, but at a distance of 21″ south of it.

The error is small enough, no doubt, and the Great Pyramid is moreover visibly nearer to the theoretical position, than any other Pyramid round about it: yet there are indications that the builders knew of the residual error, and tried to make it as small as possible. On no other hypothesis for instance can it be explained why, with the whole of the hill top to choose upon for mere foundation, though not for latitude, the builders placed the Great Pyramid so very close to the northern side of the hill; even dangerously close to its actual northern cliff; for slickenside-scratches there prove large portions of the rocky edge already to have slided off, and there is a deep cleft preparing for another such rock-slip, which passes under the very north foot of the Pyramid. The builders knew it too, for they not only filled up the cleft with masonry, and covered it over; but they banked up a whole hill of rubbish against the north face of the cliff outside, as though to keep it up. And, hitherto, it has kept up; but meanwhile the Pyramid is realising much more nearly than most persons are aware of, the favourite quotation of many travellers as to its standing "on the utmost bound of the everlasting hills." (See Plate XXVII.)

Proposition VI.

When inquiring very closely into the ancient Pyramid's angles by means of *modern* observation, it becomes important, after eliminating errors of observation, dilapidation, and of workmen, to ascertain whether any slow geological changes of the earth's crust, accumulating during the 4000 years or more that the Pyramid has been standing, have introduced by this time any sensible alteration of the angles in the building as originally constructed.

Now without alluding to some indications of a shifting of the earth's crust over the place of the poles of rotation, a variety of our measures in the interior of the Pyramid do show a slight tilt or dip of the monument towards the south; and this is further testified to by the differences of the observed corner angles of the Pyramid outside, with the addition that there is a dip eastward also. The two directions are moreover the very ones in which all the component strata of the hill, and indeed all this part of the country do largely, though somewhat variously, dip, or have been so tipped by geological forces beginning from ages long anterior to the Pyramid, and now apparently continued to some extent within its history. The quantity of this post-pyramid tilt, in the Meridian direction, as given by the corner observations appears to be about 37″; it is therefore a rather small, and perhaps doubtful quantity; but yet we should note that if it be applied as a correction, with its proper sign to the previously observed angle in the grand gallery, it makes that important feature of the symbolization of this building come, within 1″, exactly the same as the theoretical quantity already given.

And now, Sir, I crave your pardon for having so exceedingly hurried over the numerous practical details of all these observations, and having told you little more than their final results,—but the truth is, they could not be completely set before you, except in a long series of discourses; and meanwhile, there are many other topics not less important which require to be touched on this evening.

Yet allow me, Sir, also to say, that at the stage whereto we have reached—I shall have altogether failed in my duty, if Members do not now begin to see that the builders of the Great Pyramid had a remarkable appreciation of certain definite angles, and realised them on an enormous scale, and with astonishing success; wherefore arises the very natural question,—What was all this labour for, and what object was sufficient to justify a cost so huge?

If the full explanation of these angles, and of certain other existing mechanical facts and features, is to accompany the true theory, then we must plainly wade through the mere burial hypothesis of the middle ages of the world, as wel as of the modern hierologists, and try what virtue there may be in that which is at once the oldest Eastern tradition, and the youngest Western theory, viz., the metrological; implying in other words, that the Great Pyramid was primarily,—whatever it may have been made into, or used for subsequently, and which we need not, therefore, now inquire into further,—a monument devoted to *weights and measures;* not so much as a place of frequent reference for them, but one where the original standards were to be preserved for some thousands of years, safe from the vicissitudes of empires, and the decay of nations.

To this *theory,* however, we will show no other favour, than to test it on every hand with the most extraordinary severity; and, let it come out from such an ordeal brighter, if it can.

(A.) *Standards of Size.*

In the ancient Coptic language, and still in the memory of some of the people of Egypt, the name "Pyramid" is derived from *Pyr* division, and *met* ten, or a division into ten; while again, in actual fact, the Pyramid with its five corner stones, is considered by many as symbolical of five; wherefore, if a system of weights and measures be appropriately included in a Coptic Pyramidal building, we might expect to find the numbers *ten* and *five* very frequently made use of.

Again, if the system be a complete one, that leading department or *linear* measure, will of course be represented. Under such a belief, very many writers have joined in looking for the grand standard of length in one side of the base of the Great Pyramid. What that length was precisely, no man knew, even approximately, until the corner sockets were recently uncovered; and even now that they have all four been seen, the best measures vary between 9102 and 9168 inches, the mean of all being 9142 British inches. Accepting these numbers for the time, what does such a length mean as a standard of linear measure?

First, have rushed forward to answer, the hierologist scholars with the cubit of the nilometer in their hands, well preserved from the most remote ages, and they declare *a priori,* that 400 times that cubit, exact and even, was the measure of the side of the base of the Great Pyramid; but on measuring the cubit, 400 times its length is found equal only to 8280 inches.

Next, have come forward various celebrated geodesists, declaring that the side of the Great Pyramid was made exactly equal to $\frac{1}{500}$ of a degree of the meridian previously measured for the purpose, such degree being $\frac{1}{360}$ of a circle.

But they had no proof that in the Pyramid-building day the circle was divided into 360 degrees; and even if it had been, $\frac{1}{500}$ of such a degree is equal only to 8750 inches

Lastly came Mr Taylor, teaching us to look not to anypart of the surface of the earth, but to its internal axis of rotation, as the only fit reference for the highest class of linear measure, and to divide it decimally: that is, for *distance*-measuring to take $\frac{1}{10}$millionth of the *radius* of rotation for the standard; expressing that in units, whereof the peculiarly Pyramid number of five hundred millions measure the whole axis of rotation.

Such a small standard then possesses 5×5 or 25 of the units; which being longer than British inches by only ·001 of an inch, we may call Pyramid inches; while the whole 25 of them make up a length which is almost identically that of the ancient sacred cubit of the Israelites as determined by Sir Isaac Newton; and very notably different from that other cubit which, according to him, they used for ordinary purposes, and called the "profane cubit," because it was the same as that in vogue among the Egyptians, Phœnicians, and other Gentile nations. We shall allude, therefore, to a standard composed of 25 of these Pyramid inches, as the cubit of the Pyramid; and may have to note again and again the curious triple parallelism which prevails between the inner Pyramid, sacred Hebrew, and ancient Saxon measures.

But how is that 25 inch cubit to be identified with the 9142 inch base side of the Great Pyramid?

In this simple and suggestive manner;—there are in that base side as many lengths of that cubit as there are days in a year; *i.e.*, $365·25 \times 25$, and reduced from Pyramid to British inches, the units in terms of which all our Pyramid measures are expressed, amount to 9140.

(B.) *Standards of Weight.*

If weight measure is to be anything more than accidental in its origin, it must be connected with the previously established linear standard, through the medium of capacity measures connected with the doctrine of specific gravity.

On this well known principle, several authors have decided on the granite coffer, a hollow, lidless, rectangular vessel, cut with great skill out of a huge mass of dark red granite (porphyry according to some), and carefully preserved in the secluded King's chamber near the centre of the Great Pyramid, being, in its hollow contents, the capacity standard, and in the weight of that content of water, the weight standard of the Great Pyramid. Foremost amongst these authorities is again the late JOHN TAYLOR, who has further shown that the coffer is alike the parent of the Hebrew chomer, and also of the old Saxon quarter, as capacity measures for corn, each of these being the $\frac{1}{4}$ part of the coffer.

This inference hinges entirely on what the measured contents of the coffer may be, and though the results given by some travellers are in Mr TAYLOR's favour, the statements of others are not a little contradictory. With eager zeal, therefore, I went to the coffer one morning armed with many measuring rods, and hoping to be able, in so small and simple a matter, to see immediately who was right and who was wrong,—when lo! there was an anomaly that *no one*, out of all the various authors, had ever noticed.

Here is a carefully constructed model of the coffer as it now stands, of $\frac{1}{10}$ the linear size. Chipped along all the edges and grievously broken at one corner, as I was prepared to find it; but with also, a totally unexpected ledge cut inside the top and all across the western side, to a depth of 1·72 inches; and this ledge not only takes away near 4000 inches from the cubic capacity of the vessel, but enables all men to say—"so! this coffer, then, this lidless box of history, this "granite chest without a top, this porphyry vase of measure we have heard so "much about, is nothing but a sarcophagus after all:" for the ledge is so very similar to that by which the lids of sarcophagi are slided into their places.

The case was really most egregious in one way or another; and every student of Egyptian history, on first hearing of so unexpected a discovery, cannot fail to be beset according to his theoretical prepossessions, either with intoxicating triumph, or the confusion of dire defeat.

"Why were we not informed of all this by earlier travellers," some will exclaim! And others in their agony will turn again to the magnificent engravings of the so called immortal French national work on Egypt, and say, "Look at the "regular and equal sides of the coffer in these exquisite pictures; is it not *impos-* "*sible* that there could have been a ledge cut into them when their portraits were "taken at the end of last century?" But then, alas! who will be answerable for the perfect accuracy of even French academicians?

In short, we must investigate the matter *de novo*, and by measures of line and angle.

My first step, therefore, was to study acknowledged sarcophagi and their lids; and here is a model of the sarcophagus of the second Pyramid and its lid. They are both well preserved; and by an ingenious arrangement of the angles of the sliding grooves, combined with certain little fixing pins, they form a system which locks itself when the lid is pushed into its place.

Let us now make a similarly proportioned lid for a restored model of the great Pyramid coffer and its ledge, attending though to its peculiar angles; and then we find, after pushing that lid into its place, it has, and can have, no locking power whatever. Now there is a piece of clumsiness in contrivance, or inferiority in execution, which we should be careful how we charge on the architects of the Great Pyramid, where every other part is planned so much more skilfully than in any other Pyramid; and we may easily see, too, that the ledge is actually nothing more than a portion taken out of, and away from, an *originally* unledged and merely box-sided vessel.

Then if that be the case, we have nothing to do with the vessel's capacity in the ledged state, only with what it measured in its original unledged state; and then, did it or did it not coincide with one, precise, invariable theoretical quantity, depending on the cube of $\frac{1}{10}$millionth of the earth's axis of rotation or 50 inches, and the mean density or specific gravity of the earth as a whole, divided by ten?

This last important element is not known even to modern science quite so accurately as it should be, but produces, with its best known determination in the above expression, close upon 71,250 inches.

Now, that is not a likely quantity for an Egyptian sarcophagus to measure inside, for all that I examined were very much less, many of them only half or at most $\frac{2}{3}$, even when treated as unledged vessels; but when the Pyramid coffer came to be measured, and with extreme care both to make the most of all original traces and to correct for small errors of figure, the result then came out 71,316 inches. So exactly, too, is the vessel constructed for purposes, or suitably with the requirements, of commensurability, that the capacity of the outside divided by two = 71,160; and the mean of both = 71,238. Hence it would appear to be the right vessel after all; and the parent, as to size, of both the sacred Hebrew, and ancient Saxon capacity measures. But it receives some further corroboration from the place in which it stands—the so-called King's chamber.

Over the doorway outside this room is an eminent symbol of five; and again, on entering the chamber, an attentive observer finds himself in the midst of a still grander masonic symbol of five; in that the granite walls are formed of *five* even courses (see Plate XXVIII.), running round and round the room at the same precise height, and in a manner quite unique in the general Pyramid masonry.* Each of these five courses, too, is of exactly the same thickness as every other, excepting only the lowest one, and that is five inches less than the rest. Not in reality, indeed, for it is an appearance produced to observation by the manner in which the granite flooring is introduced *within* the granite walls.

Still, why was it introduced in that manner, breaking in upon the admirable equality of all the other steps of this symbol—of five, pervading the whole chamber?

* As an illustration of the necessity of the present remeasurement, the following contradictory accounts by modern travellers, of the wall-courses of this room, the King's Chamber in the Great Pyramid, may be cited:—

George Sandys, A.D. 1610.—" Eight stones flagge the ends, and sixteen the sides."

Professor Greaves, A.D. 1639.—" From the top of it descending to the bottom, there are but " *six* ranges of stone, all of which, being respectively sized to an equal height, very gracefully in one " and the same altitude run round the room."

Lord Egmont, A.D. 1709.—" The walls were composed of *five* ranges of stone."

Dr Shaw, A.D. 1721.—" Height (of *five* equal stones) = 16 feet."

Dr Pococke, A.D. 1743.—" *Six* tiers of stones, of equal breadth, compose the sides."

M. Fourmont, A.D. 1755.—" The walls were composed of *six* equal ranges."

Dr Clark, A.D. 1801.—" There are only *six* ranges of stone from the floor to the roof."

Dr Richardson, A.D. 1817.—" Lined all round with broad flat stones of large red-grained " granite, smooth, highly polished, *each* stone ascending from the floor to the ceiling."

Lord Lindsay, A.D. 1838.—" A noble apartment, cased with enormous slabs of granite *twenty* " *feet high*."

W. R. Wilde, M.R.I.A., A.D. 1838.—" An oblong apartment, the sides of which are formed of " enormous blocks of granite reaching *from the floor to the ceiling*."

Mr E. W. Lane and Mrs Poole, A.D. 1843.—" Number of courses in walls of King's " Chamber, *six*."

Sir Robert Ainslie, in 1804, copied by J. Taylor in 1859.—An engraving, showing *six* courses between floor and ceiling.

This may have been one element connected with the reason; that lowest course of the room forms in fact a sort of larger coffer, or a tank containing within itself the smaller coffer; and the capacity of that course of the room, being computed carefully, subject to the very peculiar and hitherto unnoticed correction given to it by the original builders, is equal to *fifty* times the capacity of the coffer, or comes out actually in numbers so divided, 71,292.

There is even more testimony still, as to the distinguishing number 50, and proof which I had certainly never expected to obtain, until it showed itself on quite a different inquiry. The bulk of the mass of the Pyramid, as already mentioned, is composed of rectangular courses of masonry, rude but strong, and though excessively irregular in their consecutive heights, still every course preserves its own height admirably round and through the Pyramid, and keeps a good horizontal level. I had measured the individual heights of all the several courses with some accuracy, in going up and down the Pyramid, merely because they were the literal stepping-stones towards obtaining the whole height; and have only recently ascertained that the *fiftieth* course from the ground has a horizontal plane, which coincides with the floor on which the coffer stands in its tank of *fifty*, in its chamber of *five;* or, in other words, and looking to what went before, the coffer is not only the right vessel, but it is still standing in the right chamber; and all the substance of the Pyramid was built in deference to, and unison with that chamber and its internal symbolizations.

It only remains, therefore, now to state the neat, and, so to speak pyramidal form, in which a small standard of Pyramid weight may be most scientifically described, and in noble reference to universal weight and capacity measure; for if the whole coffer weight be divided by 50 times 50, yielding what may be called a pound-weight of the Pyramid, it is representable anywhere, with the utmost exactness, as *five cubic inches of the earth's mean density.*

(C.) *Standards of Heat.*

Of heat I shall merely attempt to say, on the present occasion, that the theoretical idea is, that the temperature of that remarkable chamber of *five* in the Pyramid, was intended to be a temperature of $\frac{1}{5}$; *i.e.*, $\frac{1}{5}$ the distance from freezing up towards boiling of water, amounting in Fahr. to 68°.

But the moment the measure is taken at the Pyramid, it is found to be about 74° or 75°. The present circumstances of the room, however, are entirely abnormal and in a way that should raise its temperature. Falling back therefore on the mean temperature of the air taken night and day for the first four months of the year at the foot of the hill, and also on the temperature of wells there, these are both found 64°·0 nearly; increasing which by several degrees to reduce the mean of these four months to the mean of the whole twelve, and deducting a smaller correction for elevation to the level of the Pyramid, we attain a temperature for it under natural circumstances, which if not 68° exactly, is as close to it, as the mean temperature of any one year is usually found to the mean of a number.

(D.) *Standards of Angle.*

Measures of angle present themselves shortly at the Pyramid in the following manner:—several modern philosophers have already stated their beliefs elsewhere that the builders of the Pyramid must have been too barbarous and primitive to have any idea of angle, as angular measure; and that although the interior passages are set at certain angles, that arose only as a consequence of specified linear proportions being adopted in the building,—the proportion of two horizontal to one vertical being a very favourite instance and giving an angle of 26° 34.′ Against this statement the answer seems to be, 1*st*, That every one of the inclined passages points to a sensibly smaller angle than 26° 34′. 2*d*, That the manner of showing the angles in the azimuth trenches, is incompatible with linear, and only adapted to angular, measure. And 3*d*, The angles of the sides of the Pyramid, as well as those of the passages and some other features too, are strongly indicative of angular measure having been highly appreciated in some of its crucial refinements. That such Pyramid angular measure too, if existing at all, was represented in the favourite Pyramid numbers, would seem to follow on the rule applied to obtain the dominant standards elsewhere; for if we assume a quadrant = 250°, and the circle 1000°, then the two dominant angles of the Pyramid, the 51° 51′ 14″ ($\frac{0}{360}$) of the sides, and the 26° 18′ 10″ ($\frac{0}{360}$) of the passages are representable by even numbers of the *Pyramid* degrees, to an accuracy of less than a tenth of one of those degrees; as thus, 144·0 ($\frac{0}{1000}$) and 73·0 ($\frac{0}{1000}$).

(E.) *Standards of Time.*

At first sight we might consider that measures of time could have no place in a pyramidally arranged building, because the utter incommensurability of the day and year prevent any satisfactory introduction there of fives and tens.

Yet time evidently *was* included in the idea of the Great Pyramid, when the linear standard, as we showed some time since, was expressed in terms of time. Moreover some persons may argue that though the day and year may be difficult to settle pyramidally,—the day and week are easy enough; for what is to prevent men having weeks of five days or ten days each! And in fact some of the hieroglyphic scholars maintain that the Egyptians of and about the Pyramid age actually did employ weeks of ten days each. May we expect then to find such weeks enshrined in the Great Pyramid?

Not necessarily, seeing that the several departments of measure hitherto recognised in the monument, have by no means proved to be those in use among the Egyptians, but rather to point to a different origin altogether. Let us therefore simply try to see what there is in the Pyramid, hoping only that the indications may be sure and unmistakable.

The grand gallery has long since been quoted as the part of the Pyramid, where time-measures may be expected to appear,—and men have also frequently remarked on the "seven overlappings" which run from end to end of the gallery, and form its chief architectural feature; but the most precise testimony expected by the theory was, that the whole height of the grand gallery might be found seven times that of a small passage; the height of such small passage being supposed to represent a unit-day.

Examination at the place soon showed that the small passages are not of invariable height. No objection need be taken in principle to that, for men must not come to this part of the Pyramid seeking standards of *linear* measure, these things being provided elsewhere with greater accuracy than would be possible here. We take the mean therefore of the vertical heights first of the passage entering the north end, and second, of that leaving the south end of the grand gallery, and find such quantity to be 48·4 inches.

Now, what is the vertical height of the grand gallery? This has been extremely variously estimated, and with great difficulty by different travellers; and to put me into a more favourable position than my predecessors was the object of the second instrument, with which the free and spontaneous liberality of my friend Mr COVENTRY supplied me, viz., the long measuring rod which you see stretched along both chimney-pieces. This being duly employed at many places in the length of the gallery, gave as a mean for the vertical height 339·5 inches, whose $\frac{1}{7}$th is 48·5 inches; *i.e.*, 48·4 is much rather the $\frac{1}{7}$th than the $\frac{1}{6}$th, $\frac{1}{8}$th or $\frac{1}{9}$th of the grand gallery.

This close approach then is the full amount of testimony that had been expected to be found, or had been asked for. But the building affords more.

Under the grand gallery, southward runs a small horizontal passage, from whose total length a part equal to $\frac{1}{7}$th of the whole is conspicuously cut off; and this passage leads into a chamber, the mean breadth of whose floor is equal to this $\frac{1}{7}$th of its entrance passage.

The object of this chamber, called without any reason the Queen's chamber, has been a serious puzzle to the learned.

Hierologists have considered it was intended for the burial of some one, but who was never brought to it; and architects have considered it was merely a space for storing, during the progress of the Pyramid's building, the large blocks of stone which were afterwards employed in filling up the inclined entrance passage from the inside.

But if constructed merely for such a purpose, would the whitest and fairest stone combined with the most exquisite workmanship in the whole Pyramid have been spent on the walls of this room? There is now a coating of salt over most of the stone; but wherever the original surface or the joints can be met with, they are precisely as I have described; fair white stone, and joints conscientiously cemented yet microscopically fine.

This is a fact; so is it also that the room, so remarkably approached, has seven sides, on account of the peculiar double form of the ceiling, as shown in the diagram. (See Plate XXVIII.) These sides too are not very far from equal, especially when we supply, as we are bound to do, on the rough unfinished floor some fine lining blocks, similar to those of the walls.

But there is still something wanting to measured symmetry; and we notice in the east wall of the room, the large, well-wrought, deep-reaching niche which is without a parallel through the whole of the Great Pyramid. The mechanical effect of that niche on a computation of the bulk of the room, is plainly equivalent to the eastern wall being pushed outwards somewhat; not to the full depth evidently of the niche, but probably somewhere between twenty and thirty inches; I tried twenty-five in the computation and then found that, expressed in ten thousand square inches, six of the walls each measure accurately three, and the seventh measures five.

Now we had been previously led to look on this room as a masonried representation of the week of seven days; and here we have that realised with the addition, that six of them are ordinary days followed by, or founded on, one of greater importance; which is, in number, not only the definition precisely of the sacred Hebrew week of Genesis and Sinai, but is entirely peculiar to that one, and ancient arrangement of days. The full precision, however, of this result depends on the somewhat arbitrary employment of twenty-five inches of increased length to the room; something of the sort should be done on account of the great niche-space; but what authority does the room afford for that particular length?

Look round about the room now (Plate XXVIII.) if you please, and if there be any anomaly at all, other than the existence of the niche, it is, that so admirably wrought an architectural decoration is not in the centre of its containing wall.

Why it should not have been constructed in the centre, I have never heard the remotest guess; but measure the distance of its vertical axis from the same feature of the room, and you will find it just this twenty-five inches, or the sacred cubit, first of the Pyramid, subsequently of the Israelites, and now, by the light of modern science, shown to be no accidental length, but the one ten-millionth of the earth's radius of rotation.

Such then is the standard under which this remarkable arrangement of the *chamber of seven* was constructed; and, from the manner of its construction, at no other time than that of the original building of the Pyramid; sealed up too from that, or near that, time to all mankind, until the accident which was more than an accident, and revealed this part of the interior of the Pyramid to Caliph ALMAMOUN only a few hundred years ago.

What was the date then of the construction of this chamber of seven?

Why, of course it was the same as that of the building of the Great Pyramid; and if you ask when that was built, the answer is strange for the exactness and precision of some literary inquiries. Nearly fifty years ago the best writers fixed the date at 1800 B.C.; but within twenty years since, 2400 B.C. was considered nearer the mark; still more recently the German scholars who are so highly thought of at present in most of our universities, extended the time to 3200 B.C., then to 3700 B.C., and at last M. RENAN in France, has just announced that the epoch cannot be less than 4500 or 4700 B.C.

In the midst of all these contradictions then, what does the Pyramid say for itself? If it deals in time measures at all, it ought to have some capable of measuring large, as well as small intervals of time. In short, what of the astronomy of the Pyramid?

Astronomy has been tried already to settle the age of the Great Pyramid, by several eminent men; there was the happy identification for instance in 1838 by Sir JOHN HERSCHEL of the direction of the entrance passage with the once position of α Draconis, giving a date of about 2160 B.C., which agreed perfectly with the literary authorities of the time. But when these had grown in twenty years to 3400 B.C., then came MAHMOUD BEY, the Egyptian astronomer, with a reference of the star Sirius, the ancient Soth, or Sothis of the Siriadic land, to the southern side of the Pyramid in a manner that brought out a date also of 3400 B.C.; and then too it was found that Sir JOHN HERSCHEL'S idea of α Draconis admitted of a second solution, which likewise gave 3400 B.C. nearly. But since then the literary dates have gone up so much higher as to throw all astronomy into discredit.

What then of the examination of the Pyramid last winter?

This shortly; *1st*, The Great Pyramid's astronomy is not observing astronomy; the Pyramid was not intended to be used as an astronomical observatory; it symbolizes only.

2d, MAHMOUD BEY'S idea of Sirius has nothing positive to connect it with the Pyramid, and has three out of four possible features absolutely against it.

3d, Sir JOHN HERSCHEL'S idea of the entrance passage and α Draconis is very good so far as it goes, but it was not conducted far enough to reach a positive conclusion.

As I may gratefully acknowledge to that eminent philosopher, he taught me years ago the importance in scientific investigation of attending excessively to all anomalies in results. And on that principle I was much troubled at the Pyramid to see, that if α Draconis had been made such important use of as the ancient Polar star, only one of its two daily meridian passages had been marked; and that one, the lower and less important.

Why such an anomaly? Convinced by much experience at the Pyramid that there is hardly anything there without a reason—I concluded,—" because, when that star was crossing the meridian *below* the Pole, a more important star was crossing *above* the Pole; and it is the manner of the Pyramid not to wear its most vital truths in prominent outside positions."

Out of that consideration seems to have come the full explanation, of which the lateness of the evening prevents my doing more than briefly noticing the end, —as thus—

The more important star or star-group was, the Pleiades.

The grand gallery with its seven overlappings and peculiar angular position, is commemorative, amongst other things, of the proverbial seven stars.

The Polar star, α Draconis, was first employed to regulate the meridian position of the Pyramid; and then, the Pleiades, at that date a nearly equatorial star, was observed when on the meridian and at a high altitude, to obtain an accurate instant of time.

The epoch, was the *last* occasion when α Draconis was $3^\circ 42'$ from the Pole or in 2170 B.C; and then, not only were the Pleiades opposite to α Draconis almost exactly in Right Ascension, or crossing the Meridian above the Pole, when α Draconis was crossing below the Pole; but they, the Pleiades, were in a most peculiar cosmical position, well worthy of being monumentally commemorated; for they were actually at the commencing point of all Right Ascension, or at the very beginning of running that grand round of stellar chronological mensuration which takes 25,868 years to return into itself again; and has been termed elsewhere, for reasons derived from far other studies than anything hitherto connected with the Great Pyramid, *the Great year of the Pleiades.*

If, moreover, we would desire to learn whether the founders of the Pyramid really had in that early age of the world a knowledge of the entire duration of so grand a cycle of time, we may seek the answer in the base of the entire monument; where the sum of the two diagonals, whose equality best proves our first proposition of all, or the squareness of the base, amounts, at the rate of an inch for a year, to 25,836 years.

Perhaps now some attempt to indicate who the men were who planned, or under what guidance it was that they were enabled to plan in the year 2170 B.C. such a monument as the Great Pyramid, should follow; but I rather imagine the inquiry ought from this point to pass into other and stronger hands than mine; and I will therefore this evening, with your leave, only make one farewell remark,—it is to request special attention to the radical difference between the known vulgar measures of Egypt, such as they have been through all periods of her history, either Ancient or Modern,—and Great Pyramid measures, as illustrated in that building's carefully deposited, but long concealed, metrological arrangements. Take for example the two terminations of either system.

At one end, as the linear standard of Egypt in all time past and present, you have the cubit of the Nilometer; an accidental length of trifling character, and incommensurable evenly with anything noble under the sun; but for the linear standard of the Great Pyramid, you have no less precise or suggestive a quantity than one ten-millionth of the earth's radius of rotation.

At the other end,—in the star Sirius, Soth or Sothis, the Great dog-star, which the Egyptians voluntarily, even obstinately selected out of all the host of heaven to identify the history of their country with,—modern astronomy shows, that what with its very large amount of proper motion, historical change of that proper motion, and orbital movements round some dark body unknown, they could not have picked out for long chronological referring purposes a more inaccurate or unsatisfactory star.

But in the star, or star-group of reference chosen for the Great Pyramid, viz., the Pleiades, though not the brightest and most glaring in the sky, yet is it a group, (as particularly shown by the extensive researches of Mr R. G. Haliburton of Halifax, Nova Scotia),* connected of old by all primeval nations and primitive tribes of men with "sweet influences," and with deeply wise traditions too, dating from as early and mysterious an origin, as human language itself; while by the latest advances of modern physical astronomy this favoured Pleiades group is supposed to contain the orb, which is the central orb, of all the sidereal globes.

APPENDIX I.

The numerical observations so very shortly alluded to in the foregoing Address, occupy in MS., as prepared for intended future publication, rather more than 300 pages, and are distributed in subject, thus:—

Part A.—*Linear Measures.*

	Pages.	
Introduction,	1 to	5
Entrance Passage of Great Pyramid,	6	39
First Ascending Passage,	40	51
Horizontal Passage and Queen's Chamber,	52	65
Grand Gallery,	66	94
Antechamber,	95	106
King's Chamber,	107	120
The Coffer,	121	131
Azimuth Trenches,	132	135
Height and Breadth of Base of Great Pyramid,	136	145

* "New Materials for the History of Man, derived from a Comparison of the Calendars and Festivals of Nations," by R. G. Haliburton, F.S.A., Halifax, Nova Scotia. 1863. Printed privately.

PART B.—*Angular Measures, Geometrical.*

PART C.—*Angular Measures, Astronomical.*

PART D.—*Heat Measures.*

PART E.—*Miscellaneous and Communicated Measures.*

APPENDIX II.

SPECIMENS FROM GREAT PYRAMID REGION, PRESENTED TO THE ROYAL SOCIETY, EDINBURGH, AND TO BE SEEN IN THEIR MUSEUM.

A.—*Examples of the Natural Rock.*

1. Large specimen of nummulitic rock from the neighbourhood of the Great Pyramid, and abounding in both large and small nummulites.
2. Smaller do do.
3. Do. do. do.
4. Hand specimen of rock, almost entirely a congeries of small nummulites.
5, 6, and 7. Portions of the lime-stone rock, with fossils, from the neighbourhood of the Second Pyramid.
8. Loose nummulites, large and small.
9. Echinus and other separate fossils of the nummulitic rock.
10. Sulphate of lime, found near the Third Pyramid.
11. Four specimens of fossil wood found on the desert surface West of the Pyramids: one of them recently sawn in Edinburgh.
12. Jasper and quartz pebbles, large and small, found similarly on the desert hill surface.
13. Two portions of an ancient rock-matrix holding said pebbles.
14. Specimens from a hill about three miles south of the Great Pyramid, all of more recent formation than the nummulite, and consisting of large echini out of "green-sand"(?), and numbers of well-preserved cardium shells, casts of volutes, and portions of rock made up almost exclusively of organic remains.
15. Drift-sand of the desert, from near the Sphinx.

B.—*Examples of Imported Materials.*

These were picked up on the open ground of the Pyramid hill, in so far like naturally placed specimens, but were undoubtedly brought to the Pyramid region by man, and for building purposes; in testimony whereof, portions of one or more *worked* surfaces are usually to be seen on every fragment; and if such specimens are now mixed up with the soil of the hill, and only to be obtained by digging,—that indicates the great length of time which must have elapsed since the monuments, which these stones once helped to form, were broken to pieces.

1. Large lump of dark red granite, similar to the material of the coffer.
2. Smaller lump of light red granite.
3. Smaller piece of light-red granite, one side polished.
4. Three varieties of coarse black basalt, each of them with worked surfaces.
5. Remarkably fine-grained basalt, excessively hard, and having a portion of a rectangular edge exquisitely worked, and left rather higher than the more central part of the surface, as if to insure the stone standing firmly by resting on its circumference when introduced into some construction.
6. Fragment of diorite, from the ancient rubbish-heap on the north flank of the Great Pyramid hill.
7. Three specimens of arragonite from the neighbourhood of Shafre's tomb and the temple on the east of the Second Pyramid.

C.—*Casing-Stone Fragments of Great Pyramid.*

These were picked up amongst the innumerable fragments of stone composing the small hills of modern rubbish which lean against the middle of the lower part of each side of the Great Pyramid. All the fragments nearly of those hills seem, from their composition, to have belonged to the casing-stones; but only a few are *provable*, by chancing to have a portion of the bevilled surface and an adjoining portion of one of the horizontal surfaces, or an arrangement capable of showing the angle of the ancient slope.

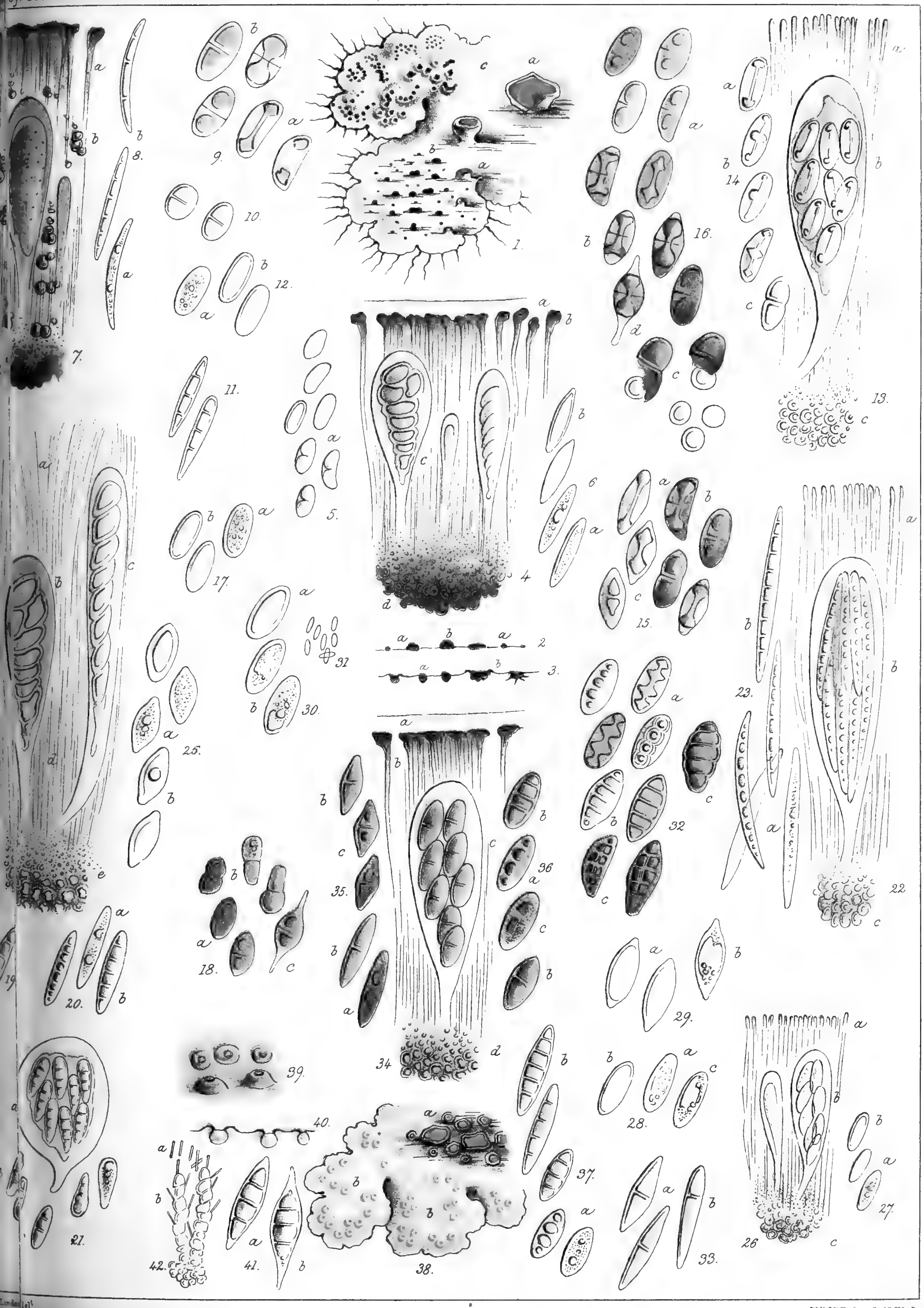
The sixteen specimens presented to the Royal Society, Edinburgh, are of this latter class, and are arranged as follows:—

Three specimens showing the upper angle of the casing-stone or 128° 9′, are inserted along the similar edge of a wooden model of a casing-stone 20 inches long and 7 inches high, and with the angle made true according to the theory.

One large specimen of the 128° 9′ angle, and another of the 51° 51′ angle standing upon it, are nserted in a model of a casing-stone 24 inches long and 5 inches high.

And eight specimens of the 128° 9′ angle, with three of the 51° 51′ angle are arranged partly in a horizontal section, and partly at the foot of a model of the whole Pyramid about 25 inches long in one side of its base, and with the angle of the sides true according to John Taylor's theory of them.

Finally one small fragment of a casing-stone recently cut and polished, to show the nature of the material.



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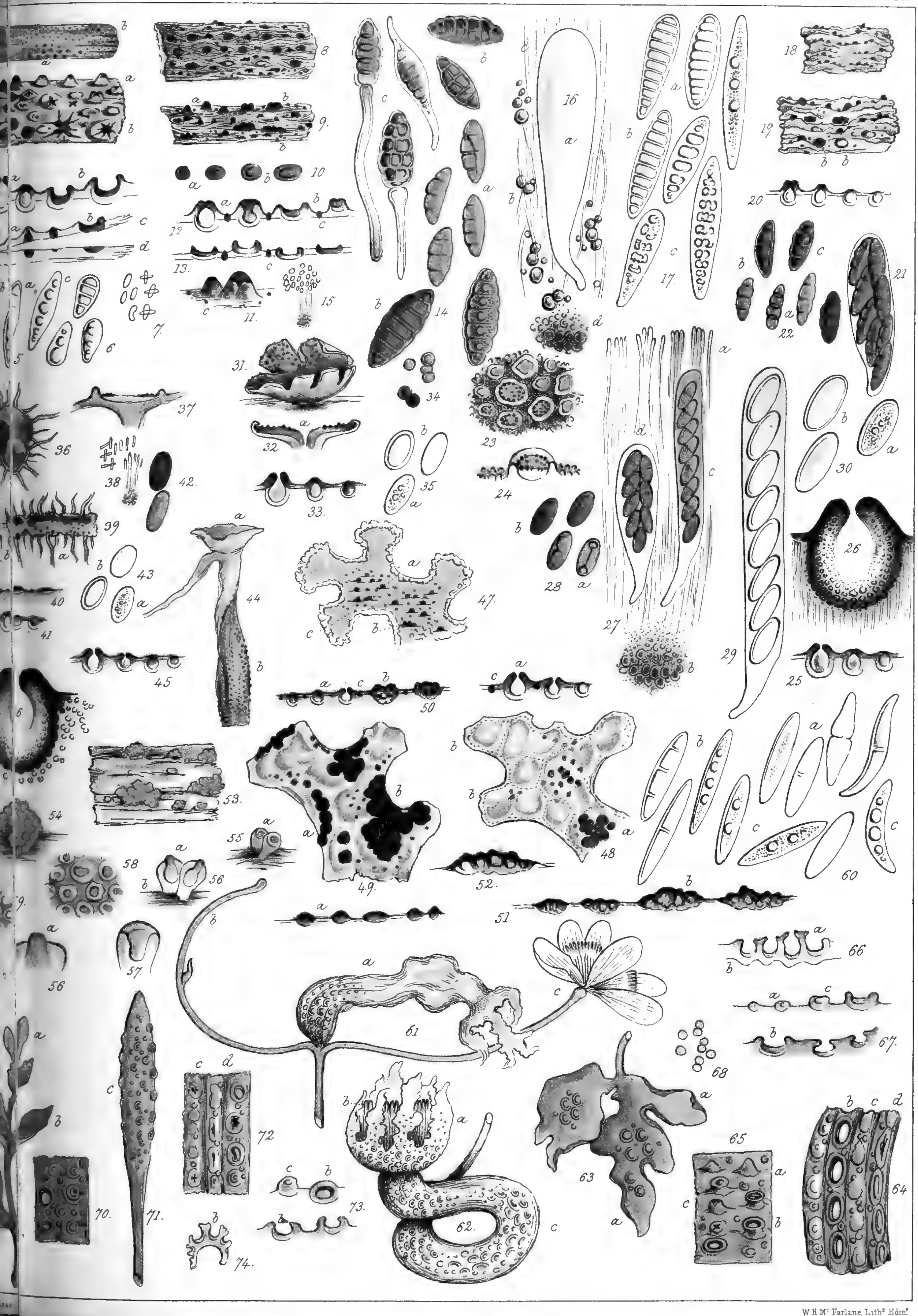

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XXX.—*Observations on New Lichens and Fungi collected in Otago, New Zealand.*

By W. Lauder Lindsay, M.D., F.L.S., Honorary Fellow of the Philosophical Institute of Canterbury, New Zealand. (Plates XXIX., XXX.)

(Read 2d January 1866.)

INTRODUCTION.

In 1861, in a part of the province of Otago, New Zealand, not previously botanically explored, I made, among other botanical collections,* one of Lichens and Fungi. The number of *new species* and varieties proved to be considerable, amounting to about 20 per cent. of the whole *lichens*, and 40 per cent. of the whole *fungi*, collected. Since my return home, I have submitted (with a view specially to the study of *the minute anatomy of the reproductive organs and their contents*) the new species (and varieties) in question—some of them repeatedly—to microscopical examination: the results whereof are contained in the notes which follow.

I hold that he only is fully competent to determine and describe species from *new countries*, who, in addition to the requisite analytical and descriptive power, has, on the one hand, constant access to, and an intimate knowledge of, the now overwhelming and ever-increasing mass of Botanical Literature in all the principal European languages; and, on the other, equally habitual access to Herbaria which contain the largest collections of specimens from all parts of the world, such, for instance, as those of Kew or Paris. By no other means does it appear possible now-a-days accurately to ascertain or *distinguish what is new from what is already known in the plant-world.* This virtually restricts systematic and descriptive botany to the Naturalists of London or Paris, or of similar centres of botanical knowledge; and as virtually excludes Provincial Botanists, who are isolated from the sources of the necessary fundamental information. It were easy for a collector or observer in a new field to name and describe, what to himself, according to his limited opportunities for judging, appears to be new. But if he do so, however otherwise qualified, without that knowledge, which can, generally speaking, only be acquired in the Botanical Libraries, and from the Herbaria, of the largest European cities, he cannot fail to add to the confusion of synonyms, and impede the true progress of botanical discovery and science, by

* *Vide* "Contributions to the Flora of Otago, New Zealand:" Transactions of Botanical Society of Edinburgh, vol. viii. p. 250: and "List of Lichens collected in Otago, New Zealand," *ibid.* p. 349.

publishing as new, and under new names, species, which a wider experience speedily proves to be identical with, or mere forms of, other plants already known as natives of other parts of the world. Collectors are, as a body and as a rule, naturally desirous of naming and describing their own collections; and, *in certain respects,* no other Botanists *can* be so well qualified to do so. Nor is it always possible to secure the co-operation of those overworked, eminent authorities, who have the largest knowledge of the special departments of botanical science which they respectively cultivate and adorn.

Holding such views, and in the absence, on my own part, of the necessary qualifications, advantages, or opportunities, I have gladly availed myself, *in the determination of species* (and to a certain extent also, in their description), of the valued assistance of my friends Dr NYLANDER of Paris (for Lichens), and FRED. CURREY, F.R.S., of London (for Fungi),—the one, the most eminent living authority in systematic and descriptive Lichenology; the other, one of our most accomplished British Fungologists.*

In reference to the following enumeration of Otago *Lichens*,—with one or two exceptions,—the *names* assigned are those of Dr NYLANDER,† who writes as follows regarding the Lichen collection:—

"Les votres sont d'un grande valeur pour la Flore Antarctique, surtout à cause des saxicoles et espèces d'un ordre inférieur qu'elle renferment et qui avant étaient trop imparfaitement représentés parmi les matériaux rapportés de ce bout du monde.‡ . . . Certainement les espèces . . . et les variétés sont *toutes nouvelles pour la Science.* La Flore de la Nouvelle Zélande a par vos découvertes fait des acquisitions importants dans le domaine lichénographique."§

In regard to the *Fungi*, in several cases I am indebted to Mr CURREY, not only for names, but also for specific diagnoses and notes on structure or affinities. In a few other cases (of fungi or fungo-lichenes), where complete materials do not exist in my collection for full description, the plants not being in a perfect state as to fruit or otherwise, I have assigned names with much diffidence, but not without due deliberation, in the belief that the subsequent researches (to which the names and notes now given may perhaps lead) of Local Botanists, who

* I use the term *Fungology* in preference to *Mycology* (referring to that department of botanical science which treats of *Fungi*), because, though less euphonious or elegant, it is also less open to misunderstanding; the term *Mycology* being equally applicable and applied to that department of *anatomical* science which treats of the *muscular* system in man and animals. I am borne out, in the preference of the term *Fungology*, by the recent and high authority of BERKELEY ("Outlines of British Fungology," 1860, p. 2).

† Since my "Observations" were committed to the printer, a paper by Dr NYLANDER, entitled "Lichenes Novæ Zelandiæ, quos ibi legit anno 1861 Dr LAUDER LINDSAY," has been published in the Journal of the Linnean Society: Botany, vol. ix. p. 244, which contains the specific diagnoses of the majority of the Lichens referred to in the following and aforesaid "Observations." Fortunately the paper has been issued in time to enable me to insert references thereto at their proper places in the present text.

‡ Letter, dated August 3, 1864.

§ Letter, dated August 22, 1865.

may have at command, for investigation, ample series of *living specimens*, in all their forms or conditions of growth, will prove the plants in question—what I here presume them to be—in reality new species.

I. LICHENES.—(PLATE XXIX.)

1. *Abrothallus Curreyi*, nov. sp. (figs. 1 to 5.)

Parasitic on the thallus of *Parmelia perforata*, Ach., (which is copiously covered with apothecia and spermogones): on the trunks and branches of dead trees,* Greenisland Bush.

In characters, this species is intermediate between *A. Smithii*, Tul., and *A. oxysporus*, Tul.† These species, when they occur (as they most frequently do) on the thallus of *Parmelia saxatilis*, Ach., are almost invariably found occupying special growths from, or anamorphoses of, its thallus. But *A. Curreyi* occurs directly on the ordinary thallus of *P. perforata* (fig. 1, *a b*), towards its periphery, in the position usually occupied by the spermogones of the *Parmelia* (*c*). In this respect it resembles other species of *Abrothallus*, which are parasitic on the thallus of other species of *Parmelia*, and on species of *Platysma* and *Stictina*.

The apothecia of *A. Curreyi* are less prominent and tuberculiform, and smaller than those of *A. Smithii;* more convex and protuberant than the discoid, flattened, sub-immersed ones of *A. oxysporus*. In *A. Curreyi*, the apothecia are typically minute, black, convex, and immarginate; partly immersed in the thallus, in whose superficial tissues they have been originally developed (figs. 1 *b*, 2 *a*). They vary, however, considerably in form and size, having a tendency on the one hand to become tuberculiform, and on the other, discoid. In the young and old states they are apt to be confounded with the *spermogones* of *P. perforata*, which are generally more or less abundant on adjoining lobes of the thallus. In the young state, the apothecia of *A. Curreyi* appear as very minute papillæ; in the old, when the tuberculiform hymenium has fallen away, it frequently leaves a black, stellate-fissured scar, resembling that characteristic of old emptied spermogones of the *Parmelia*. I have elsewhere‡ described the characters of these *spermogones* of the *Parmelia*, which are easily distinguishable from the apothecia of the *Abrothallus* on microscopical examination, though sometimes not otherwise. In my Otago specimens of *P. perforata*, I find its spermogones (figs. 1 *c*, 3) though generally sub-marginal, punctiform, and immersed, occasionally occupying also central positions on the

* Especially "Goai" (*Sophora tetraptera,* Aiton). *P. perforata* is equally abundant sometimes also on *living* trees in Saddlehill Bush, and other remnants of the primitive forest.

† "Monograph of the genus *Abrothallus;*" with two coloured plates.—*Quart. Journal of Microscopical Science.* January 1857.

‡ "Memoir on the Spermogones and Pycnides of the Higher Lichens."—*Trans. Royal Society Edinburgh,* vol. xxii. p. 211 (Plate II. figs. 4, 5).

thallus; large, prominent, and sub-papillate; seated on, or immersed in, minute thalline wartlets or elevations. They become, moreover, with age occasionally confluent and difform, frequently irregularly stellate or radiate (fig. 3 *b*). Generally speaking, corticolous forms of *P. perforata* are abundantly, while saxicolous ones in Otago are sparingly, spermogoniferous. In the former, spermogones sometimes abound to such an extent as to give the thallus, to the naked eye, a black-punctate character.

The constituents of the hymenium of *A. Curreyi* (fig. 4 *a d*), are somewhat indistinct, from their state of close aggregation. The tips of the paraphyses are dark-brown (*b*), very granular, and agglutinated; and they are covered by a colourless epithecial membrane (*a*). The hypothecial tissue is also very granular and dark (*d*). The *thecæ* (*c*) are of the typical form, ·0012″ long, and ·0006″ broad,* containing eight spores arranged in one or two rows. In the young state, and while crowded in the thecæ, these spores are generally sub-angular or otherwise difform from mutual pressure (*c*). The thecal lichenine gives a beautiful blue reaction with iodine; in this respect resembling *A. oxysporus.* The *spores* (fig. 5) are simple, colourless, sometimes showing a double contour, ·0003″ long, and ·00025″ broad; broadly ellipsoid or sub-oblong; sometimes slightly curved like those of the genus *Ramalina;* and also, like them, sometimes exhibiting a tendency to central division into two loculi (*a*), with occasionally a slight constriction opposite the septum. The spores thus resemble those of *A. oxysporus*, rather than those of *A. Smithii*, which are brown, solæform, and 1-septate. The tendency, however, to division and constriction is an approach to the characters of the latter. They are always much smaller, broader, more rounded at the ends, or more oblong, than those of *A. oxysporus.* With this species I have associated the name of my friend, the eminent Fungologist, FRED. CURREY, F.R.S.

2. *A. oxysporus*, Tul. (fig. 6),

also occurs in Otago, apparently identical in its characters with its Scotch prototype.† I found it parasitic on the larger-lobed forms of *Parmelia conspersa*, Ach., which grow plentifully on basalt, in the gullies or glens of the Greenisland hills (*e.g.*, near Greenisland church). The apothecia are typically flattish or discoid; in the young state, however, they are frequently tuberculiform or sub-papillæform; and under moisture, in the mature condition, they swell so as to become sub-convex, and to assume somewhat the characters of those of *A. Smithii* and *A. Curreyi.* In the old state, generally from the falling away of the hymenium, they leave a black urceola, which may become irregular in its outline, or stellate-

* The microscopical analyses were made with a Nachet's microscope: objective $\frac{1}{6}$″, ocular, No. 3—magnifying 425 diam.-linear; and the measurements here given are in decimal fractions of the English inch.

† "Monograph of Abrothallus," p. 80; "Memoir on Spermogones," p. 232–3.

fissured. As in *A. Curreyi*, the constituents of the hymenium are indistinct and closely united. The *paraphyses* contain much brown colouring matter about their tips, which are intimately agglutinated. The hymenial lichenine gives a blue reaction with iodine. The *spores* (fig. 6) are narrowly ellipsoid, almost fusiform; simple; sometimes with a double contour (*b*); colourless; ·00075″ long, and ·00025″ broad.

The genus *Abrothallus* (which is properly to be considered only a group of lower, parasitic, athalline *Lecideæ*), and the two species above recorded, are new to the New Zealand Flora.

From their minute size and inconspicuous character, the plants composing the genus in question are apt to be overlooked by all but the practised Lichenologist. From what I have myself seen, I have no reason to suppose them rarer in New Zealand than in Britain; and I have therefore to recommend their being carefully looked for by local Botanists on the thallus of the higher or foliaceous Lichens, especially in the genera *Parmelia* and *Stictina*.

3. *Melanospora Otagensis*, nov. sp.

(LINDSAY "On a New *Melanospora* from Otago, N.Z.," Trans. Bot. Soc. Edin., vol. viii. p. 426, Plate V. figs. 7–12.)

Thallus sub-determinate, tartareous, thick, of cretaceous texture and chalk-white colour, sub-farinose, smoothish, sub-areolate. *Apothecia* vary in form from lirellæform (Opegraphoid) to angulose-patellæform (Lecidine): most usually they are short, sub-oblong, broadish pseudo-lirellæ, generally straight, black, simple, solitary and scattered, sub-sessile, base only sub-immersed. *Epithecium* rimæform or exposed, flat or concave; margin distinct, thickish, generally entire, sometimes more or less involute on the disk. *Spores* abundant and distinct, brown, 1-septate; about ·0006″ long, and ·0003″ broad; oval-oblong, constricted or not at the septum, sometimes figure-8 shaped or solæform.

Habitat on columnar basalt, Greenisland Bluff; associated with a sterile condition of *Pertusaria velata*, Turn. This species (which has been described from an imperfect specimen—the only one in my herbarium) appears closely allied to the British *M. cerebrina*, DC. (Mudd, "Manual," 226, E. Bot., Pl. 2632, fig. 1), so far as I can judge from figures and descriptions only. The family or tribe to which the genus *Melanospora* (Mudd) belongs, viz., the *Xylographidei*, Nyl., as well as the genus itself, and *M. Otagensis*, are alike *new to the New Zealand Flora*.*

4. *Lecidea Otagensis*, Nyl., Lich. N.Z., 255 (figs. 7, 8).

On stockyard fences of "Goai" timber; in the Bush, ravines of the Chain Hills, Greenisland: associated with *Arthonia excedens*, Nyl.

* *Vide* "List of Otago Lichens," pp. 356–8.

The apothecia somewhat resemble externally those of *L. grossa*, Pers., and *L. pulverea*, Borr. They are sometimes angular, or sub-lirellæform; sometimes sub-pedicellate. The hymenial gelatine and the thecæ become beautifully blue under iodine (fig. 7 *c*). Both hymenium and thecæ also contain large quantities of oil globules (*b*). The *thecæ* (*c*) are somewhat small; ·0015″ long, and ·00045″ broad; 8-spored, and of the ordinary form. The *paraphyses* are indistinct, and the dark-brown clavate heads are closely agglutinated (*a*). The *spores* (fig. 8) are ·0012″ long, and ·00001″ broad; acicular or very narrowly fusiform; colourless; generally slightly curved; poly-septate (frequently 3 to 5 septa); like the majority of lichen-spores, granular in the old and young states (*a*), with no distinction of loculi or septa.

5. *L. flavido-atra*, Nyl., Lich. N.Z., 257 (fig. 9).

On stockyard palings of "Goai," Martin's Bush, Chain Hills.

Externally it differs from *L. grossa*, Pers., *L. marginiflexa*, Tayl., and other *Lecideæ*, only by the colour of its thallus—a lemon yellow.

The *paraphyses* are indistinctly seen, but are sub-discrete, delicate, filiform, sub-hyaline, colourless even at their tips, which are not knobbed or clavate. The *thecæ* are large and distinct; 8-spored; ·0036″ long, and ·0015″ broad; giving a beautiful blue with iodine. The *spores* (fig. 9) are broadly ellipsoid; 1-septate; colourless; ·0009″ to ·0012″ long, and ·0006″ broad. In the young state (*a*) they are polari-bilocular, and save as to colour resemble those of *Physcia pulverulenta*, Fr. In the young state (*a*) also, the septum is generally wanting, and the loculi have a pale lemon-yellow tint.

6. *L. melanotropa*, Nyl., Lich. N.Z., 255 (fig. 10).

On trees and shrubs, Stoneyhill Bush; and on the bark of dead trees, Saddlehill Bush; associated with *Arthonia excedens*, Nyl., and *Collema leucocarpum*, Tayl.

The apothecia in the young state are sometimes waxy or corneous, of a glaucous or olive hue, becoming, however, with age pitch-black, and then resembling those of *L. grossa*, Pers.

The constituents of the hymenium are most indistinct, and the spores (fig. 10) are with difficulty seen. The latter are globose or sub-globose; 1-septate, colourless, about ·0003″ in diameter. The hymenium and thecæ give a blue reaction with iodine.

7. *L. amphitropa*, Nyl., Lich. N.Z., 256 (fig. 11).

On rocks and the ground, Woodburn Ravine, Saddlehill.

The plant consists of a patch of white thallus, with a very few straggling black apothecia, resembling in general aspect our *L. epigæa*, Schær., or *L. Hookeri*, Schær.

The constituents of the hymenium are indistinct and closely aggregated. The *thecæ* are smallish, but give a distinct deep blue with iodine. The *spores* (fig. 11) are fusiform, colourless, 3-septate, ·0009″ long, and ·00015″ broad.

8. *L. leucothalamia*, Nyl., Lich. N.Z., 255.

On the bark of dead trees, Saddlehill Bush.

Though the disk of the apothecium is generally whitish, its colour is variable, being sometimes glaucous or brownish, with a corneous aspect. It resembles somewhat *L. melanotropa*, Nyl., and *L. pulverea*, Borr., from which the spores, however, at once distinguish it. A similar pallor of disk occasionally occurs in *L. marginiflexa*, Tayl., and other *Lecideæ*, whose apothecia are typically and usually pitch-black. The constituents of the hymenium are very indistinct and closely aggregated. The hymenial lichenine becomes blue with iodine. Both paraphyses and thecæ are shortish; I saw neither satisfactorily; nor did I see any spores.

Var. *melachroa*, Nyl., which occurs in the same locality, is simply a form with black apothecia, which then resemble those of *L. marginiflexa*, Tayl., *L. grossa*, Pers., and many other *Lecideæ*, with typically black apothecia.

9. *L. allotropa*, Nyl., Lich. N.Z., 254.

On Mica slate, Glen Martin, Chain Hill Range.

There was no result of microscopical examination worthy of record.

10. *L. coarctata*, Ach.; var. *exposita*, Nyl., Lich. N.Z., 254 (fig. 12).

On tertiary grits and conglomerates, base of Saddlehill.

The apothecia are mostly convex, resembling some forms of *L. parasema*, Ach. Sometimes they are sub-difform, or irregular in outline. At other times they exhibit remains of a coarctate or urceolate character, and they then resemble—as does the thallus—some forms of *Lecanora cinerea*, L.

The hymenium gives a very faint blue with iodine. The *paraphyses* are very indistinct and closely aggregated; they are obscured, especially about their tips, by much granular matter of a deep reddish-yellow colour. The *thecæ* approach in size and other characters those of *Urceolaria* and *Pertusaria;* they are ·0030″ to ·0036″ long, and ·0006″ to ·0009″ broad. The *spores* (fig. 12) are simple, colourless, ellipsoid-oblong, with double contour; frequently ·0006″ long, and ·00045″ broad.

11. *L. trachona*, Nyl.; var. *marginatula*, Nyl., Lich. N.Z., 254.

On columnar basalt, Greenisland Bluff.

It has a general resemblance to various small forms of *L. lenticularis*, Ach., *L. parasema*, Ach., and *L. contigua*, Fr.

The hymenium gives a deep blue with iodine; the thecæ and paraphyses are very small, and very closely aggregated. I did not succeed, under power 425 of my Nachet's microscope ($\frac{1}{6}$-inch objective), in distinguishing the spores, which are also very small.

12. *Lecanora homologa,* Nyl., Lich. N.Z., 251 (figs. 13, 14).

On the trunks of living trees, Greenisland Bush: associated with *Physcia plinthiza,* Nyl.

It has a general close resemblance to some forms of *L. subfusca,* Ach.; but its spores (fig. 14) at once distinguish it. These are broadly ellipsoid or oblong, colourless; sometimes convex on one side, and straight on the other (plano-convex); 1-septate, and constricted (*c*) or not at the septum (in the old state); ·0009″ long, and ·00045″ broad. In the young state they are occasionally polari-bilocular (*a*); or there are three loculi (*b*), (the central one by far the largest), round, oval. sub-angular or quadrilateral, united generally by a tubule running up the centre of the spore as a kind of longitudinal canal. Very commonly they are 3-locular, with loculi resembling those of the spores of *Verrucaria nitida,* Schrad.

The *thecæ* (fig. 13 *b*) are large, distinct, and ventricose; 8-spored; ·0036″ to ·0045″ long, and ·0012″ broad; beautifully blue under iodine, as are also the hymenial lichenine generally, and the tips of the paraphyses (*a*). The *paraphyses* are very delicate, filiform, colourless, indistinct, devoid of clavate or coloured heads.

13. *L. peloleuca,* Nyl., Lich. N.Z., 251 (fig. 15).

On columnar basalt, Greenisland Bluff.

The *paraphyses* are delicate and indistinct, without coloured, clavate heads. The *thecæ* are closely aggregated, of medium size, deep blue with iodine. The *spores* (fig. 15) are broadly ellipsoid or oval; of various shades of olive or brown, according to age; being deepest brown in the old state (*c*), palest olive in the young (*a*); ·00075″ long, and ·00045″ broad: in the young state polari-bilocular (*a*), the loculi sometimes connected by a median longitudinal tubule or canal: in the old state 1-septate (*b*), with a constriction or not opposite the septum, sometimes having the figure of 8-form of (*c*), and otherwise resembling, the spores of *Physcia pulverulenta,* Fr.

14. *L. thiomela,* Nyl., Lich. N.Z., 252 (fig. 16).

On basaltic porphyry, Forbury Heads, Dunedin.

The thecæ and spores are best seen in young apothecia, which have much the aspect of spermogones, being yellow papillæ, perforated apparently by a darker, brownish-yellow ostiole, which is, however, the unexposed and unexpanded disk. The *paraphyses* are delicate, filiform, indistinct, without coloured, clavate heads. The hymenial gelatine gives a beautiful dark-blue colour with iodine. The *thecæ* are broadly and irregularly obovate above, 8-spored; ·0045″ long, and ·0009″ to ·0012″ broad. The *spores* (fig. 16) are broadly ellipsoid or oval; ·0009″ to ·0012″ long, and ·00045″ to ·0006″ broad; 1-septate; olive or brown, according to age. In the young state, and in young apothecia, no septum is visible (*a*); the spores are polari-bilocular, as in *L. peloleuca;* the locules connected or not by a median canal; and there is almost no colour, or a very faint olive. In old spores there is generally a constriction opposite the septum (*c*), where they frequently split, giving exit to a globose nucleus (*c*), about ·00045″ in diameter, which is either colourless, or of a faint lemon-yellow tint. Sometimes the spores are seen in the process of elongation at both ends preparatory to germination (*d*).

15. *Placopsis perrugosa,* Nyl., Lich. N.Z., 250 (fig. 17).

On basaltic boulders, top of Kaikorai Hill (1092 feet); associated frequently with *P. gelida,* L., to which it has a close affinity, and for which it may readily be mistaken.

The *thallus* is greyish or brownish, thick, and crustaceous, consisting of, or divided into, a series of pulvinuli or isidiiform cushions, arranged, especially peripherally, in sublinear rows, having a general resemblance, in this respect, to the thallus of *Lecanora ventosa,* Ach. When sterile, it generally bears cephalodia resembling those of *P. gelida.* It is one of the saxicolous lichens, whose thallus is apt to occur sterile, and isidiiferous or sorediiferous.

The *apothecia* are closely crowded, assuming various angulose forms from mutual pressure. The disk is of a port-wine-red colour, assuming a more brilliant pink or crimson under moisture; and in this respect also—the characters of the apothecia—the plant resembles *L. ventosa.* Its *spores* (fig. 17), however, are very different. They are ellipsoid-oblong, simple, colourless, generally with double contour (*b*); ·0006″ long, and ·00045″ broad, arranged in a linear series, eight in each theca: in riband-shaped *thecæ,* 0045″ long, and ·00045″ broad, which give, as does the hymenial lichenine, a beautiful blue colour with iodine.

The disk of the apothecium is occasionally the seat of the parasitic *Microthelia perrugosaria,* nov. sp., described in the 3d section (Fungo-Lichenes).

16. *Opegrapha subeffigurans,* Nyl., Lich. N.Z., 258 (fig. 18).

On the bark of the "Totara" pine (*Podocarpus Totara,* A. Cunn.), Greenisland Bush; associated with *Arthonia platygraphella,* Nyl.

The *paraphyses,* as in all the New Zealand *Opegraphæ,* are delicate, filiform, indistinct, without coloured, clavate heads. The hymenial lichenine gives a yellow or pale wine-red tinge, with iodine; the *thecæ* a very pale blue. The latter are ventricose; ·0021″ to ·0030″ long, and ·0006″ to ·0009″ broad; 8-spored. The *spores* (fig. 18) are ellipsoid-oblong, dark-brown, 1-septate, ·0006″ to ·0008″ long, and ·00025″ to ·0003″ broad. In the old state, there is frequently a constriction opposite the septum, giving them a figure-of-8 form (*b*); occasionally also they are solæform, with one end or half broader, and generally also shorter and rounder, than the other (*b*.) In the latter case, they resemble the spores of *Abrothallus Smithii*, Tul.

17. *O. agelæoides*, Nyl., Lich. N.Z., 257 (fig. 19).

On the trunks of living trees; Greenisland Bush.

The thallus is whitish and thin, following the rugosities and furrows of the bark. The lirellæ have a general resemblance to those of our common *O. varia*, Pers., which occurs in the North Island (KNIGHT and MITTEN), or of *O. saxatilis*, DC.

The *thecæ* are ventricose, ·0021″ long, and ·0006″ broad, 8-spored, and give a wine-red reaction with iodine. The *spores* (fig. 19) are fusiform, colourless, polyseptate (frequently 5-septa); ·0009″ long, and ·00025″ broad.

18. *O. spodopolia*, Nyl., Lich. N.Z., 257 (fig. 20).

On basalt; Shaw's Bay, The Nuggets, mouth of the Clutha.

Its microscopical characters differ little from those of the preceding species. The *spores* (fig. 20) are of the same character and dimensions. The *thecæ* are somewhat longer and broader—·0030″ long, and ·0012″ broad—and are unaffected by iodine; while the hymenial gelatine assumes a very pale wine-red tinge.

Professor CHURCHILL BABINGTON, in the "Flora Novæ Zelandiæ" of Dr HOOKER, remarks on the absence of the large and widely-diffused genus *Opegrapha* from the Lichen-Flora of New Zealand as one of its marked peculiarities. This statement arose evidently from the circumstance that, at the period when he wrote (1855), no species of the genus had been collected in New Zealand, or had been sent home, so as to be accessible for examination in the Kew or other public Herbaria. But KNIGHT and MITTEN* have since described several species from the province of Auckland. My Dunedin herbarium contains the three species just mentioned; and, I doubt not, the further researches of Botanists, and especially of Lichenologists, accustomed to the detection of minute or microscopic, inconspicuous corticolous and saxicolous Lichens, will discover other species of this genus in all parts (of the lowlands at least) of New Zealand. Similar remarks might be made in regard to other Lichen-genera at present supposed to be altogether absent from the Lichen-Flora of New Zealand; and until, indeed, its Lichen-Flora has been *fully* investigated by competent *resident* Botanists, we must be cautious in asserting that any given families, genera, or species are absent,—unless we do so distinctly with the qualification, that our statement is simply *provisional*, and is intended to direct the attention of local Botanists to the supply of a desideratum in our knowledge of the New Zealand Lichens.

* "Contributions to the *Lichenographia* of New Zealand, being an account, with figures, of some new species of *Graphideæ* and allied Lichens." By Dr KNIGHT of Auckland, New Zealand, and W. MITTEN, of Hurst-Pierpoint, Sussex (the eminent Muscologist).—*Trans. Linnean Soc., London*, vol. xxiii. p. 101, plate xii.

19. *Arthonia platygraphella*, Nyl., Lich. N.Z., 258 (fig. 21).

On "Totara" bark, Greenisland Bush; associated with *Opegrapha subeffigurans*, Nyl.

The apothecia are frequently roundish and sub-convex, with a Lecideiform aspect, exhibiting a thin, white, obscure, thalline margin. Sometimes they are oblong and irregular, sometimes confluent.

The *thecæ* and *paraphyses* are unaffected by iodine; the former (*a*) are broadly saccate, as is the general character of thecæ in the genus *Arthonia*; 8-spored; ·0009″ long, and ·0006″ broad. The *spores* (*b*) are irregularly fusiform or obovate, colourless, 3-septate; ·00045″ long, and ·00025″ broad. They have frequently much the appearance of half-spores—halves of fusiform, and 1-septate spores—with minor nuclei, or granular contents.

20. *Platygrapha longifera*, Nyl., Lich. N.Z., 258 (figs. 22–3).

On the bark of dead trees; Saddlehill Bush.

The *paraphyses* (fig. 22 *a*) are subdiscrete, delicate, filiform, without clavate heads. The *thecæ* (*b*) are somewhat sac-shaped, 8-spored; ·0030″ to ·0036″ long, and ·0006″ broad; generally *un*tinged by iodine, but occasionally violet. The *spores* (fig. 23) are acicular or narrowly fusiform, sometimes slightly curved; poly-septate (generally about 10-septa). They vary considerably in dimensions, from ·0009″ to ·0018″ long, and ·00015″ to ·00025″ broad.

21. *Pertusaria perfida*, Nyl., Lich. N.Z., 253 (figs. 24, 25).

On tertiary grits and conglomerates, base of Saddlehill; on trappean rocks, Shaw's Bay, The Nuggets.

The *thallus* varies in colour from dark slate or lead-colour to whitish. The sterile portions are made up of a series of Isidia; and the plant, in this state, would have been described by the earlier Lichenologists under the genus *Isidium*. The fructiferous portions of thallus bear a great general resemblance to certain forms of *Lecanora cinerea*, L.

The *paraphyses* (fig. 24 *a*) constitute a network of very delicate, indistinct, hyaline filaments, without clavate, coloured heads; and are thus typical, or possess the ordinary characters of those of the genus *Pertusaria*. The *thecæ* (*bc*) vary in length from ·0069″ to ·0090″, and in breadth from ·0015″ to ·00075″, according as the 8-spores are arranged in one (*c*), or a double (*b*) series, being in the one case ribband-like, and in the other ventricose or obovate superiorly; they strike a beautiful blue with iodine (*b*), while the hymenial gelatine gives a violet. The *spores* (fig. 25) vary considerably in length, from ·0006″ to ·0015″, with a general breadth of ·0006″; they are broadly ellipsoid; simple, colourless, generally exhibiting a double contour (*b*); granular, or full of oil globules in the young state (*a*). They have the general characters of the spores of *Lecanora* and *Psoroma*, rather than of *Pertusaria*, especially as regards size.

22. *P. perrimosa*, Nyl., Lich. N.Z., 253 (figs. 26, 27).

On columnar basalt; Greenisland Bluff.

This is one of several local saxicolous Lichens, whose thallus is apt to occur abundantly sterile, and isidioid or variolarioid (sorediiferous). The plant differs greatly from the majority of our British types of *Pertusaria*, in the extreme smallness of its thecæ and spores. The paraphyses also are scarcely typical, inasmuch as they are colourless, delicate, and filiform, without coloured or clavate heads; they are, nevertheless, sub-discrete, regularly arranged linearly, with their apices readily seen (fig. 26 *a*). The *thecæ* (fig. 26 *b*) are unaffected, moreover, by iodine; are 8-spored; ·0021″ long, and ·0003″ to ·0006″ broad, according to age and maturity of contents. The spores (fig. 27) are simple, ellipsoid, colourless; ·0004″ long, and ·00015″ broad.

23. *Pannaria immixta*, Nyl., Lich. N.Z., 249 (fig. 28).

On the branches of trees, East Taeri Bush.

The thallus is sub-coralloid; the paraphyses very delicate, filiform, indistinct, without coloured, clavate heads; the thecæ indistinct, 8-spored; the hymenial gelatine pale blue with iodine. The *spores* (fig. 28) are ellipsoid, simple, colourless; 0006″ long, and ·00025″ broad.

24. *P. gymnocheila*, Nyl., Lich. N.Z., 250 (fig. 29).

On trees; Martin's Bush, Chain Hills; associated with *Collema leucocarpum*, Tayl.

The plant has a general resemblance to *Coccocarpia plumbea*, Lightf., or *C. molybdœa*, Pers.

The hymenium gives a beautiful blue with iodine. The *paraphyses* do not much exceed in length the thecæ; they are sub-discrete and filiform; their clavate heads (epithecium) are of a brownish or yellowish colour, and are intimately united and covered by a colourless, transparent membrane; the hypothecial tissue has also a brownish or yellowish tint. The *thecæ* are ·0036″ to ·0045″ long, and ·0006″ broad; 8-spored, with the spores arranged in one or two rows. The *spores* (fig. 29) are ·0009″ long, and ·0003″ broad; ellipsoid, simple, colourless; exhibiting generally, in the mature state, a double contour (*a*).

25. *Psoroma sphinctrinum*, Mnt. (figs. 30, 31).

On stockyard fences of "Goai" timber, Martin's Bush, Chain Hills; on *living* trees, East Taeri Bush; on the bark of *dead* trees, Saddlehill Bush.

An abundant corticolous species in Otago, fruiting freely. The colour of the thallus varies from pale grey to lurid (blackish-brown). The minutely squamulose character of the thallus gives the plant the aspect of a *Pannaria*. The hymenium and thecæ give a beautiful blue with iodine. The latter are 8-spored (with the spores generally arranged in a single series), ·0036″ long, and ·0006″ broad. The sub-discrete, filiform paraphyses are united by yellow clavate heads. The *spores* (fig. 30) are broadly ellipsoid, oval or oblong; simple, colourless; with double contour generally in the mature state (*a*); ·0006″ long, and ·00045″ broad. What appear to be *spermogones* are marginal, brown tubercles, resembling the spermogones of *Peltigera* and *Nephroma*, containing myriads of atomic or ellipsoid spermatia (fig. 31), about ·0001″ long, and ·00005″ broad; possessed of vivid Brownian movements, borne apparently on arthosterigmata, which are indistinct.

The species is confined to warm countries, such as South America and Australia. In specimens in the Hookerian Herbarium, from the Cape (Miller) and the Mauritius, I found the spores of the same character as those in my Otago plants, differing only as to size, being here ·00038″ long, and ·00020″ broad.

Var. *pholidotoide*, Nyl., Lich. N.Z., 250.

On living trees, East Taeri, and Saddlehill, Bush.

The *thallus* is rudimentary, consisting of a few minute, sparsely scattered scales, seated on the black hypothallus, the plant having thus the aspect of certain of the smaller *Endocarpa*. The *apothecia* are large, closely crowded in the centre of the thallus, almost constituting the plant; becoming, from mutual pressure, irregular and difform, sometimes sub-confluent. They have a thickish, rugose or crenulate thalline margin, and a disk of chestnut colour, without any tinge of black. This form frequently bears considerable resemblance to various *Pannariæ*, e.g., *P. brunnea*, Sw., or *P. nebulosa*, Hffm. Its thecæ and spores are exactly those of the type.

26. *Physcia plinthiza*, Nyl., Lich. N.Z., 249 (fig. 32).

On the trunks of living trees, Greenisland Bush; bearing both apothecia and spermogones.

The *thallus* is distinct and foliaceous; sometimes lobulate, of a slate-grey colour, apt to be obscured by overgrowing Jungermanniæ. The apothecia become with age convex, and have a widened, flattened exciple. The hymenium, thecæ, and tips of the paraphyses all assume a more or less deep and beautiful blue with iodine; this tinge is sometimes comparatively deep on the tips of the paraphyses, while it is very pale on the apices of the thecæ. The *thecæ* are 8-spored, ·0036″ long, and ·0009″ broad. The *spores* (fig. 32) vary considerably in their characters, combining the features of those of *Verrucaria nitida* and *Physcia pulverulenta*. Their usual length is from ·0009″ to ·00135″, breadth about ·00045″; their general form broadly ellipsoid or ellipsoid-oblong. Sometimes they are convex on the one side, and straight on the other (plano-convex) (*c*); sometimes where septa exist in the old state, there are constrictions opposite each of generally 3-septa. The colour varies from pale olive (*a*) to deep brown (*c*), according to age; darkness of tint, as is generally the case in the spores of lichens, being proportionate to age. For the most part, especially in the young spores, there are no distinct septa (*a*); but a number of loculi, varying from 4 to 8 (most frequently 4), which are sometimes globose, sometimes lenticular or quadrilateral, connected or separate, according apparently to age; sometimes becoming longitudinally divided, giving the spore a sub-muriform character (*c*).

27. *Ricasolia herbacea*, DN., var. *adscripta*, Nyl., Lich. N.Z., 248 (fig. 33).

On the bark of dead trees, Saddlehill Bush; bearing both apothecia and spermogones. There are also specimens from Tarndale, Nelson, *ex* Herb. Dr Sinclair, of Auckland; some bearing apothecia, others sterile.

The plant appears to me hardly to deserve a separate name as a variety; I do not see wherein it differs sufficiently from its type. The *paraphyses* are sub-discrete, filiform, united by yellow tuberculated tips. The *thecæ* are 8-spored, blue with iodine; ·0036″ long, and ·0009″ broad. The *spores* (fig. 33) are more or less fusiform, 1-septate; sometimes with the loculi of unequal size (*b*), colourless or pale yellow; ·0009″ long, and ·00025″ broad. The spermogones are those of the type, which I have elsewhere described.*

28. *Sticta subcoriacea*, Nyl., Lich. N.Z., 247 (figs. 34–36).

On trees, Saddlehill Bush; bearing both apothecia and spermogones; also from Wellington, sent me by Dr Müller of Melbourne (identical in external characters with my Otago specimens).

The plant has much of the aspect, and most of the general external characters of various *Ricasoliæ*, e.g., *R. herbacea*, DN., and *R. coriacea*, Hook. and Tayl. It seems indeed a connecting link between the genera *Sticta* and *Ricasolia*. The main difference consists in the presence in the *Sticta* of *cyphellæ;* but in some *Stictæ* these are absent, or they (or Pseudo-cyphellæ†) occur on the *upper* instead of *lower* surface of the thallus (*e.g.*, in the following species *S. episticta*, Nyl.) The mere presence or absence of *cyphellæ* is not, I think, a sufficient character for the separation as genera of *Ricasolia* and *Stictina* from *Sticta*. Not unfrequently in the same species of either of these genera, cyphellæ occur or not. In my Otago specimens of *Ricasolia coriacea* occur a few cyphellæ, which are exactly like those of *S. subcoriacea*. I do not know, indeed, in what essentials *S. subcoriacea*

* "Mem. Spermogones," p. 202, Plate X. figs. 6–11.

† I am not satisfied that there is any essential distinction (anatomical, morphological, or functional) between *Cyphellæ* and *Pseudo-cyphellæ*. Though the former are typically urceolate and smooth, they become pulverulent and shallow; and pass thus, by imperceptible gradations, into the latter. (*Vide* "History of British Lichens," 1856, pp. 42–336.)

differs from *Ricasolia coriacea*, save as to the greater size of thallus, and the more general presence of cyphellæ in the former. Both plants, moreover, grow in the same habitat, though the *Ricasolia* is more generally found fertile than the *Sticta*.

There is the same rigidity of thallus, the same white-pilose apothecia and thaline margins. Under water, the lower surface of the thallus of the *Sticta* exhibits well its beautiful velvety-pilose character, while the upper surface assumes a greenish tint.

In my Otago specimens the hymenium gives a beautiful blue with iodine. The *paraphyses* (fig. 34, *b*) are sub-discrete, united by yellowish tuberculated heads, which are covered by a colourless membrane (epithecium) (*a*). The *thecæ* (*c*) are 8-spored; like the hymenium, blue with iodine; from ·0024″ to ·0036″ long, and ·00045″ to 0009″ broad. The *spores* (fig. 35) are more or less ellipsoid, 1-septate; ·0006″ to ·0009″ long, and ·0003 broad; of olive (*b*) or brown (*a c*) colour, and otherwise resembling the spores of *S. fossulata*, Duf.

In Wellington specimens, the *thecæ* are ·0024″ to ·0030″ long, and ·0008″ broad; the *spores* (fig. 36) ·0010″ long, and ·00045″ broad; broadly ellipsoid or ellipsoid-oblong; 1 or 3-septate, according to age; brown. In young apothecia and within their thecæ, they are almost uniformly 1-septate, and more or less deep brown in colour; but in old apothecia and out of their thecæ, they are frequently, if not generally (typically), 4-locular or 3-septate (*b*). In other respects, Wellington and Otago specimens agree in internal as well as external characters.

29. *S. episticta*, Nyl., Lich. N.Z., 248.

On trees, Saddlehill Bush; on columnar basalt, Greenisland Bluff.

The saxicolous forms have much the aspect of thick, rigid, coriaceous states of *Parmelia saxatilis*, Ach.; while the corticolous ones resemble *Ricasolia herbacea*, DN.

In this species there is a reversal of the ordinary position of the *Pseudo-cyphellæ*, which are here on the *upper* surface of the thallus, and being whitish, are somewhat conspicuous on the buff-coloured epithallus (cortical tissue). Corresponding to the Pseudo-cyphellæ above, the under surface of the thallus is marked by a series of minute buttons or papillæ of the same pale reddish-brown colour as the under surface of the thallus, and nestling among the fine fibrillose tomentum, with which it is copiously covered. On the upper surface there occasionally occur small sub-globose isidia or *cephalodia*, of a bright gamboge-yellow colour.

All my specimens are sterile, so that I have had no opportunity of examining its apothecia or spermogones.

30. *S. filix*, Hffm., Nyl., Lich. N.Z., 246 (fig. 37). (LINDSAY, Spermog., p. 194, Plate X. fig. 28. *Stictina filicina*, Ach., Nyl., Synopsis, 349.)

On trees, East Taeri Bush; Signal Hill, Dunedin (*ex* Herb. Dr Sinclair).

One of the most beautiful of the New Zealand *Stictæ.* Some specimens show the tendency to multifid division of the edges of the laciniæ, so common among these *Stictæ*. Similar minute squamules or granules occur occasionally also on the margins of old apothecia, as well as on the surface of the thallus. These old apothecia frequently lose their disk, and assume the colour of the thallus, or a paler or whitish tint. The colour of the upper surface of the thallus in the Herbarium varies from buff to bright reddish-brown. The prominent ribs of the lower surface, which, in the Herbarium, have generally a dark-brown colour, give the plant somewhat the aspect of certain marine *Algæ* (*e.g.*, species of *Fucus*). These costæ have, moreover, frequently corresponding sulci on the upper surface of the thallus. The stout, rigid stem in the larger forms, frequently becomes sub-fistulose, from incurving of its margins.

In Signal Hill specimens the *thecæ* are ·0030″ to ·0036″ long, and ·0006″ broad; the *spores* (fig. 37 *a*) broadly ellipsoid, 3-septate, colourless or pale yellow; ·0009″ long, and ·0003″ broad.

Var. *parvula*, Nyl., Lich. N.Z., 247 (fig. 37).

Tarndale, Nelson (ex Herb. Sinclair); bearing both apothecia and spermogones.

One of the most elegant of the lesser *Stictæ* of New Zealand. The thallus is very smooth and glistening, divided into very narrow, minutely cut laciniæ. The whole plant resembles in size and shape (it is under an inch high) some forms of *Cladonia cervicornis*, Ach., and its allies.

The *paraphyses* are sub-discrete, with coloured and irregular, tuberculiform heads, closely united. The *thecæ* are 8-spored, blue with iodine; ·0036″ to ·0045″ long, and ·0006″ to ·0009″ broad. The *spores* (*b*) are narrowly ellipsoid, 3 to 5-septate, colourless; ·0015″ long, and 00025″ broad.

31. *S. damæcornis*, Ach., var. *subcaperata*, Nyl., Lich. N.Z., 247 (figs. 38–42).

On trees, East Taeri Bush; bearing both apothecia and spermogones.

In colour and other characters it resembles *Ricasolia herbacea.* The *paraphyses* are discrete, filiform, united by yellow clavate apices, which are covered by a yellow thin membrane. The hymenium gives a beautiful blue with iodine, and its whole constituents are very distinct. The *thecæ* are 8-spored, ·0045″ long, and ·0009″ broad. The *spores* (fig. 41) are broadly fusiform, colourless, 3-septate; ·0012″ to ·0015″ long, and ·00045″ broad. The *spermogones* (fig. 38 *b*) are those of the type, and have been described by me elsewhere.* They are sub-marginal, papillæform, with darker brown ostioles (fig. 39). The *spermatia* (fig. 42 *a*) are rod-shaped; ·00015″ long, and ·00005″ broad; borne on arthrosterigmata (*b*) about ·00015″ broad, and of varying lengths, generally about ·0015″ long.

II.—FUNGI (Plate XXX.)

Genus I. *Sphæria.*

Species 1. *S. Lindsayana*, Currey MSS. (figs. 1 to 7).

Diagnostic characters: Division, *Caulicolæ.*—" Perithecia very small, round; rupturing the epidermis by a circular, rimose, or radiate fissure. Sporidia 8, biseriate, colourless, irregularly cymbiform, 0·0014 to 0·002 inch long."

" The perithecia are so adherent to the epidermis, that it is impossible to make out the nature of the ostiolum. There is apparently no Rostellum; or if any, it is not visible above the epidermis. The plant, when dry, has somewhat the appearance of *Sphæria nebulosa*, Fr." (Currey MSS.)

Habitat.—Covers, in the form of very minute, point-like, black dots, the dry, incurled, yellowish, dead leaves of *Phormium tenax*, Forst. (fig. 1), (the familiar and abundant " New Zealand flax"); in the swamps of Glen Martin, Saddlehill.

I have little doubt it will be found, if looked for, in profusion on dead flax leaves throughout the New Zealand islands.

In the specimens, which I have myself examined under the lens and microscope, the plant has very much the aspect of certain minute, corticolous *Verrucariæ.* There is, perhaps, a distinction, however, in the non-action of tincture of iodine† on the hymenium of the *Sphæria*, and in the intimate adhesion of the perithecia of the latter to the epidermis of the *Phormium* leaf—an adhesion, which renders it difficult to manipulate them for microscopical examination, save after boiling and maceration.

In the mature state, the Fungus appears as a distinct, but minute, epidermal papilla; its circumference marked by a dark, well-defined ring; its apex pierced by a black, very small ostiole, normally punctiform, but becoming with age, compression, or other causes, rimose or irregularly radiate (fig. 2 *a*). In old and emptied perithecia, this ostiole widens variously, producing an *urceola* instead of

* "Mem. Spermogones," p. 194, Plate X. figs. 16–19.

† The reaction of this tincture with the hymenial " gelatine" (so-called, but which is really that modification of starch designated by chemists *Lichenine*), is too variable and uncertain to constitute a safe or good character for distinguishing *Lichens* from *Fungi.* Though this gelatine, and the thecæ specially, in the great majority of *Lichens*, give a reaction with tincture of iodine, which varies in colour, from beautiful Prussian *blue*, to an obscure port-wine *red*, of every intensity of shade, there is, in a minority of cases, no distinct coloration; while, on the other hand, the *blue* coloration, formerly supposed peculiar to *Lichens*, occurs, Mr Currey informs me (MSS. 1859), among indubitable *Fungi.* In other parts of this Paper I have shown that in the same species of *Abrothallus*, in which there is generally no coloration by iodine, it nevertheless sometimes occurs in foreign specimens.

a *papilla* (figs. 3, 4, *b*); or into a ragged, black, rent-like mouth; the apex of the epidermal papilla falling away, and the margins of the torn or fissured leaf-epidermis fringing the irregular remanent basilar cavity (fig. 2 *b*). The upper half of the perithecium is formed by the epidermis of the *Phormium*, which is gradually elevated, as the Fungus is developed and approaches maturity; and this epidermal papilla is perforated apicially by the ostiole of the *Sphæria*, the discharge outlet for its spores (fig. 3). The lower half of the perithecium is sub-epidermal, seated in the subjacent fibro-cellular tissues of the *Phormium*-leaf. Maceration causes the site of the perithecium to become more apparent; the epidermis of the *Phormium* becomes detached, as a somewhat translucent thin membrane (fig. 4 *c*), from the subjacent tissues, carrying with it the projecting papilla, which contains the ostiole and upper half of the perithecium; while the black, circumscribed base remains as an irregular mass, resembling somewhat the old Lichen-genus *Pyrenothea* (fig. 4 *b*, 2 *b d*). Moisture or maceration, moreover, alters the appearance of the epidermal papilla, which now becomes wholly black, instead of exhibiting a mere black ostiole and ring (fig. 2 *c*). The epidermal papilla frequently appears to fall off, leaving a black ring alone, as an indication of its former presence—of its medial circumference.

The perithecia vary in size, number, and closeness of aggregation. For the most part, they are, to the naked eye, punctiform, greatly crowded, and frequently confluent or sub-confluent. Normally, they are round, but from pressure on each other in a state of close aggregation, they become ellipsoid, or otherwise variously elongated. Sometimes they are flattened on the surface or apex, and they then have, under the lens, somewhat the aspect (save as to colour and size) of the wart-like apothecia of a *Pertusaria*, the epidermis of the *Phormium* appearing as if covered with a series of minute blisters.

Their position is equally on and between the rugæ and furrows, which mark the surface of the *Phormium* leaf. Occasionally they are disposed in rows, especially when they occur in the leaf-furrows. More generally, their distribution is quite irregular or scattered.

Notwithstanding many examinations, I have not succeeded in satisfying myself as to the form and size of the *thecæ*, which are extremely delicate and hyaline. But, in a few cases, the *spores* were abundant and distinct. Those I saw were normally ellipsoid, 6 to 8 locular; colourless; hyaline; with a very delicate envelope; ·0003″ to ·00025″ broad, and ·0016″ to ·0013″ long; resembling closely the septate spores of many Lichens (fig. 5). Occasionally (apparently only aged ones), I found them slightly curved, a condition which is likewise common in the old state of the fusiform and ellipsoid spores of Lichens; but this appeared to me to be an accidental condition, or one depending on age. Occasionally, intermixed with the normal fully developed spores, were half spores, apparently the result—if not, sometimes at least, of artificial division by friction of the glass slides during examination—rather of abortive development, than of the partition of the full-sized, mature spore (fig. 6). Sometimes these half spores occurred in considerable numbers.

In certain old perithecia, in their *Pyrenothea*-like bases seated below the epidermis of the flax leaf—and in the same perithecia, moreover, that had apparently contained thecæ and normal endothecal spores (fig. 4 *b d*)—I found myriads of extremely minute corpuscles, resembling in size and other features the spermatia of various Lichens,—differing, however, in their irregularity of form (fig. 7). They were ellipsoid, fusiform, or rod-shaped; occasionally slightly curved; about ·00016″ long, ·000083″ to ·000066″ broad, endowed with a vivid Brownian or molecular movement. Materials are probably imperfect for determining whether these corpuscles really belong to the *Sphæria*. If they do, they are probably to be considered its *stylospores*. In reference to these corpuscles, and to the observation connected with their discovery, Mr Currey writes—"It is not, I think, improbable that you may have found *spermatia* and *asci* within the same perithecium in *S. Lindsayana*, although such an occurrence is not common in the genus. Indeed, I do not know that it has ever been observed. *Conidia* (the bodies resembling *spermatia*, but which are larger, and *capable of germination*) have been noticed by Tulasne in the same perithecium with asci. You will find a figure of this in the second volume of his 'Selecta Fungorum Carpologia.'* . . . . . In *S. Lindsayana*, you have probably come upon perithecia in what is called a *Sphæropsoid* state, *i.e.*, having only very minute *stylospores and no asci*. These imperfect perithecia are found in some *Sphæriæ*, and are probably more common than has been supposed. But further observation alone can determine whether they are universal." †

Sp. 2. *S. Otagensis* (figs. 8 to 15).

Diagnostic char.: "Div. *Villosæ*.—Perithecia erumpent, or flattened, with a mammillate ostiolum, tomentose. *Sporidia* (? biseriate) brown, curved, 3–6 septate, constricted at the septa, variable in length; 0·0006 to 0·001 or more inch, sometimes with one (or more ?) longitudinal septa. Not in good condition." (Currey MSS.)

Hab. On a stockyard fence of old, weathered "Goai" timber (*Sophora tetraptera*, Aiton); farm of Fairfield, Saddlehill.

To the naked eye, the perithecia appear to be a series, closely aggregated, of very minute, irregular tubercles or buttons: but, when moistened and carefully examined under the lens, they resolve themselves into two main forms—the cone or papilla, and the disk. The former resemble the perithecia of various of the larger black *Verrucariæ;* the latter the young apothecia of various *Lecideæ*. The papil-

* Letter, March 22, 1865.

† Letter, November 6, 1865.

late condition is characteristic of the young state only. The cone is sometimes quite symmetrical and well-formed, and generally exhibits distinctly the apicial ostiole (figs. 11, 12 *a*). The discoid form is characteristic of maturity and age, and is more common than the conical. Here the papilla becomes flattened; the ostiole is so transformed that it loses all its ordinary characters, and is not recognisable as such; most frequently it occurs as a saucer-like cavity, surrounded by a thickened margin, resembling the apothecial exciple of Lichens (Lecideæ). Sometimes, the cavity becomes funnel-shaped, the perithecium having the form of an inverted cone (figs. 10, 12 *b*).

Frequently the perithecia are confluent, and then they lose their normal appearance, and become variously deformed (fig. 9). They are generally seated in the lacunæ or interstices of the woody fibres; and there, from pressure apparently by these fibres, they become variously elongated (fig. 8). Sometimes, from their state of close aggregation and form, they resemble our common *Dichœna rugosa*, Fr.

They are originally hypophlæod, developed in the superficial tissues of the bark or wood. Through these they gradually burst in process of development, until they become sub-sessile; their base only being seated in a hollow of the wood. Sometimes they are seated on pedicle-like projections of the superficial layers of the wood; the portions of the latter not protected by the growth of the parasite being eroded or worn away by weathering. Generally the fabricated timber on which this *Sphœria* grows is more or less bleached or whitened by weathering; and on this whitish base the pitch-black perithecia are prominently visible.

In the old state, the upper half, or three-fourths of the perithecium, fall away, leaving the base as a black, saucer-like hollow or scar in the wood; or as a saucer, sessile, or semi-sessile, adnate, not immersed (fig. 13).

The spores vary in length from ·0009″ to ·0006″, and in breadth from ·0002″ to ·0003″; they are septate (3 to 6, or more septa, though frequently 3); becoming sub-muriform from longitudinal division of the loculi; colour varying from olive green to dark brown: outline irregular from bulgings opposite the loculi (fig. 14). They exhibit a double contour (the cell-wall or general envelope being distinct from its loculi) usually in their mature state (*b*); but it is indistinct, or apparently absent, in the young (*a*). They have a close general resemblance to the spores of *Urceolaria scruposa*, L., and *Lecidea petrœa*, Wulf. They do not differ much, even in size, from those of my New Zealand specimens of the former Lichen. I saw them frequently germinating from one or both ends—the terminal cell becoming gradually discharged of its colouring matter, and sending forth from its distal end, or extending itself into, a tube about ·00015″ in diameter; with a double wall, hyaline, or of a brown tinge, especially towards its tip (*c*).

In very minute, black, punctiform conceptacles, semi-immersed in the wood, resembling in external character the spermogones of many Lichens, and apparently bearing a similar relation to the *Sphœria* (figs. 11, 12, 13 *c*), I found myriads of globular or oval corpuscles—spermatia—about ·00006″ to ·000075″ in diameter—given off apparently from the ends of extremely minute, simple sterigmata (fig. 15).

The *Sphœria* occurs on the same wood with *Patellaria atrata,* Fr., from which, however, its perithecia may be distinguished by their less size, greater thickness, more verrucarioid aspect, and greater closeness of aggregation. The disks of the *Patellaria* are largish, thin, very irregular in form, with incurved or corrugate margins; frequently angular or lirellæform, and resembling the difform apothecia of various *Umbilicariœ*, *Graphideœ*, *Lecanorœ*, or *Lecideœ.* The *Patellaria* is distinguished further by the microscopic characters of the hymenium. The *thecœ* (fig. 16) are large and distinct; ·0045″ to ·0030″ long, and ·0009″ broad, resembling those of Lichens. The *hymenium* contains a large quantity of oil globules (*b*). The *spores* (fig. 17) are large and very distinct, resembling those of certain *Arthoniœ* and *Verrucariœ.* They are, moreover, *colourless,* somewhat irregular in form; ellipsoid, obovate, or pyriform (broad at one end, and tapering at the other); ·0009″ to ·0012″ long, and ·0003″ broad; 8–10 septate. In the old and young states (*c*), their contents are a mass of granules instead of distinct loculi; and in the former state, also, they frequently become greatly elongated—narrowly fusiform; ·0018″ long; apparently preparing to germinate, and extend themselves into the tubular or mycelioid condition (*c*).

Sp. 3. *S. Martiniana* (figs. 18 to 22).

"Fruit 0·0007 inch, rather larger than in *S. pulvis-pyrius;* otherwise exactly like latter, but it can hardly be a rostrate form of that species." (Currey MSS.)

Hab. On trunks of living trees (apparently chiefly "Goai"); Greenisland Bush.

The bark, on which the perithecia occur, is very rugose and whitish; they are jet-black, and consequently very prominent, and easily seen under the lens. They are very minute and punctiform; but vary considerably, both in form, size, and surface, having greatly the external characters of the Lichen-genera *Pyrenothea* and *Microthelia.* They are generally granular or powdery on the surface, and on this account, as well as by reason of the minuteness of the perithecia themselves, their ostiole is seldom or never visible. The young perithecia exhibit only a punctiform, black papilla on the surface of the bark; while their large black body is sub-epidermal or immersed—requiring to be enucleated from the woody tissue for examination (fig. 20). In maturity and age, however, they become—gradually emerging from the woody tissue—sub-sessile,—the base only being immersed, or occupying a saucer-like hollow of the matrix. As in the case of

S. Otagensis, in age the upper portion of the perithecium frequently falls away, leaving only the saucer-like hollow in the matrix occupied by its base: this assuming the appearance of a black scar, or being whitish with a black ring, according as the base remains or disappears (fig. 19 *b*). The perithecia vary greatly also as to their closeness of aggregation, being isolated, closely aggregated, or confluent ;and in the latter case necessarily becoming difform.

The thecæ and paraphyses are very delicate, and are seldom, or with difficulty, distinctly seen. The *thecæ* are apparently 8-spored, and the *spores* biseriate, as in most Lichens. The thecal wall seems closely to envelope the spores like a sac, as is common in the *Arthoniæ* (fig. 21). The *spores* are 3-septate; brown; ·00045″ long, and ·00015″ to ·00025″ broad; irregularly ellipsoid; generally bulging more or less, according to age, opposite each locule; very seldom muriform, but having a tendency to longitudinal division of the loculi (*c*); sometimes slightly curved (fig. 22).

With this species I have associated the name of my friend WILLIAM MARTIN of Fairfield, Saddlehill, Otago, one of its few resident Botanists and pioneer settlers, to whom I was indebted for much assistance in my Otago excursions and collections, and on whose property in Greenisland a great portion of these collections was made.

I have no doubt that many new species of the large and ubiquitous genus *Sphæria*, and of allied spheriaceous Fungi, have yet to be added to the New Zealand Flora; but their discovery and determination will require all the care of experienced local Fungologists.

GENUS II. *Nectria*.

Sp. 1. *N. Otagensis** (*N. armeniaca*, Currey MSS.), (figs. 53 to 60).

Diagnostic characters.—"Perithecia cæspitose; pale-apricot coloured, with ostiolum darker; sporidia biseriate, colourless, about 0·0007 inch." (Currey MSS.)

Hab. On stockyard fences of old "Goai" timber, ravines of the Chain Hills.

A beautiful little Fungus of a pale orange-red colour, and waxy aspect; forming a bright contrast to the greenish bark on which it grows (figs. 53, 55). It generally nestles in the rough and deep furrows between the irregular rugæ of the very rugose bark of "Goai," and is best seen when the latter has a dark olive tint. The plant has much the appearance of masses of certain fish-roes;

* The name originally bestowed by CURREY was the very appropriate one, *armeniaca* (apricot-coloured); but, in the meantime, the same designation has been conferred by TULASNE (*Selecta Fungorum Carpologia*, vol. iii. p. 75, plate x.) on a French and very different species; and it appears easier and preferable to render the name of the Otago plant more distinctive, rather than to raise trivial questions of priority of nomenclature.

occurring generally in the form of irregular glomeruli (fig. 54), which are compound—made up of an aggregation of individual perithecia. These perithecia are found isolated only in the young state of the plant (figs. 55, 56). They are then seen to be isidiiform or sub-columnar warts, having a rounded apex, marked by a sub-papillate ostiole (*a*), of a darker red, which leads to a flask-shaped imbedded nucleus (*b*), of similar colour. Under moisture, the whole perithecium becomes more beautiful and distinct; it swells, and assumes a brighter or purer colour; the ostiole becomes quasi-pellucid or gelatiniform, and much more papillate and prominent; and the nucleus shows itself on section as a viscid or gelatiniform mass, of colour resembling that of the ostiole, having more of a brownish or reddish tinge than the exterior or envelope of the perithecium.

These perithecia coalesce or become aggregated in numbers of from two to twenty or upwards, to form the compound glomeruli. From mutual pressure, they undergo changes of shape, but they never entirely lose their individuality; for each perithecium in a glomerule may be distinguished by its red punctiform ostiole (figs. 54, 58), and on section, by the dissepiments between the separate nuclei.

The glomeruli vary necessarily greatly in size and form, according to the number and closeness of aggregation of the constituent perithecia (fig 53). They, as well as the ostioles, also vary much in deepness of colour, being sometimes comparatively pale, at other times darker, and more red or brown, than usual. The ostioles, moreover, vary greatly in size and form; sometimes they are mere points—in large glomeruli of closely aggregated perithecia; sometimes they are so large and distinct as to resemble the disks of *Lecanorine* apothecia (fig. 58). Their form is determined by that of the perithecia; which again is regulated by the amount and direction of mutual pressure in their condition of aggregation in the glomeruli. In the old state, the nucleus frequently falls out, leaving an inverted conical cavity of a much paler (whitish) colour than the exterior walls of the perithecium (figs. 57, 59). In this state, the perithecium bears some resemblance to certain species of the old Lichen-genus *Gyalecta*.

The *spores* (fig. 60) vary considerably in their characters. Their length is generally about ·0006″ to ·00075″; their breadth ·00015″ to ·00025″; they are colourless, and very delicate, resembling those of certain *Verrucariæ*. Their form is generally narrowly ellipsoid; but they may be convex on one side only, and straight on the other; or fusiform; or variously curved in the same or opposite directions. Sometimes they are simple, or occupied by two to four or more nuclei or granules, or with fine granular matter alone. There is, however, a tendency to division of the cell-contents into two or four loculi (*a.b*), producing the appearance of 1 to 3 septa; and sometimes also there is a tendency to constriction opposite the septum (*a*).

Genus III. *Æcidium.*

Sp. 1. *Æ. Otagense* (figs. 61 to 74).

1. Parasitic on the flowers, flower-petioles and leaves of *Clematis hexasepala*, DC. East Taeri Bush; Novem.; in abundant flower; a beautiful bush-climber, known to the Maoris as the "Pūawānanga," or "Pōanānga."

The flowers and flower-petioles are completely deformed by the growth of the parasite; their aspect is so entirely changed, that were the diagnosis of the plant dependent on specimens possessing these diseased organs, its species and genus would probably not be recognisable. The filiform or slender petioles, in particular, not only become twisted and curled variously, but are the seat of irregular, succulent, gouty swellings—of cucumber or cactus-like growths (figs. 61 *a*, 62 *c*), whose nature is rendered apparent by the beautiful buff-coloured Peridia, by which they are covered.

When the *flower* is the seat of the parasite, the sepals become thickened and somewhat coriaceous or fleshy, at least toward their bases or insertions. They also acquire adhesions to each other at their proximal ends, forming a sub-campanulate or sub-urceolate perianth (fig. 62 *a*). On the *outer* surface of the sepals the Peridia exhibit themselves as *Urceolæ* (fig. 66 *a*), resembling the cyphellæ of some *Stictæ;* while, on their *internal* surface, the position of each urceole is marked by a corresponding *papilla* (fig. 66 *b*). When the Fungus occurs on the *leaf* of the *Clematis* (fig. 63), it alters its texture and appearance to this extent, that the leaf becomes thickened and coriaceous; its margin notched, thickened, and curled up (*a*); and its colour assumes a russet-brown or faded autumnal tint.

In the young state, the Peridia appear as epidermal papillæ (figs. 65, 67 *a*), gradually pierced by an apicial ostiole, which rapidly expands into an urceola (figs. 66 *a*, 65 and 67 *b*), developing or exposing the concealed disk. Frequently, also, they are verrucæform or disk-like, having a more or less flat surface, with or without a thick, rounded, prominent rim (figs. 64, 65, 67 *c*). Normally, in the mature and old state, the Peridia are urceolate; their disk being sunk below the level of the epidermis of the *Clematis;* with raised margins projecting above the same level, varying much in thickness (fig. 67 *b*). Sometimes this margin is as thick as the apothecial exciple of many *Lecanoræ;* at other times it is thin and sub-membranous. Normally, the shape of the Peridia is round; but from pressure on each other, when more or less closely aggregated, they become variously ellipsoid or elongated (fig. 64 *d*). They vary greatly in their numbers and closeness of aggregation. Sometimes they are isolated, or occur in twos or threes (fig. 63), or in very small, scattered groups. This is the case on the *leaf* of *Clematis* in my specimens. At other times they are so closely crowded, that there is left between each Peridium no intervening tissue of the matrix (figs. 61, 62, 64).

This form is common on the *flower-petiole* of *Clematis*. Between the widely segregated and the closely aggregated forms there is every variety of distribution. It is in the former that the thick margin and verrucæform character generally occur; while in the latter we usually find the thin margin and regular urceola, with flattened disk (best seen on the flower petiole in my specimens).

In this country, similar deformities are produced on our common Nettle and Elder by *Æcidium Urticæ*, DC.* In the one case, the leaf-petioles and stem, and in the other, the stem, become twisted, curled, and swollen in a similar way. My friend M. C. Cooke, author of a recent "Synopsis of the British Æcidiacei"† (who was kind enough to examine my Otago species or forms), remarks of the one under description—"The *Æcidium* on *Clematis*, producing gouty swellings, &c., just corresponds with our *Æ. Ranunculacearum*, DC., which I have on *Clematis vitalba* from France and Germany." It does not, however, follow that it is the same species; indeed, its characters do not correspond with those of *Æ. Ranunculacearum*, as given in the "Flora Novæ Zelandiæ"‡ (in which work the said *Æcidium* is described as hitherto found only in the *North* Island, growing on *Ranunculus rivularis*, Banks and Sol.)

When exhibiting the monstrosities of *Clematis hexasepela* above described, and explaining or demonstrating their causes—the growth of the parasitic *Æcidium*—to the colonists of Otago, I was informed by them of the existence of similar deformities of parts or organs of a variety of trees and shrubs, which, from the descriptions given, may prove to be attributable to a similar cause—the development of a co-generic or co-specific parasitic Fungus. There is every reason to believe, therefore, that the study of *local vegetable teratology*—of the *diseases of local plants*—their causes and effects, offers a wide and novel field of research to the local Botanist.

2. Parasitic on the leaf (under surface) § of *Epilobium junceum*, Forst., on plants growing 8 to 15 inches high on the Chain hills and flanks of Saddlehill; December; in flower.

The leaves on which the parasitic Peridia are scattered, are generally somewhat altered in colour and texture (fig. 69). The colour becomes russet-brown; the leaf looks faded, and presents a premature appearance of age (for the plant, on which the parasite occurs in my Herbarium, is a young one, with unexpanded flower-buds) (*a*). Sometimes the margin of the leaf becomes puckered or curled

* Excellent coloured plates of this and other British species—including the deformities they produce—may be found in an admirable popular "Introduction to the Study of Microscopic Fungi," by M. C. Cooke. London, 1865.

† Journal of Botany, vol. ii. p. 33, with a plate.

‡ Of Dr Hooker, vol. ii.—Cryptogamia, 1853.

§ It is of interest to note that the *British Æ. Epilobii*, DC., occurs on the *under* (rarely upper) side of the leaves of *Epilobium montanum*, L.; *E. hirsutum*, L.; and *E. palustre*, L. (British species of *Æcidium*, Cooke's "Introduction," p. 190.)

inwards or upwards.* Its texture becomes thickened and sub-coriaceous; and this change is most evident, when leaves affected by the parasite are compared with those unaffected; which, in the latter case, are thin, sub-membranous and altogether of greener colour, with a healthier, fresher, younger appearance, and more flaccid consistence. The mal-conditions, or deformity here produced by the parasite, are, however, much less marked than in the case of *Clematis* or of *Microseris.*

The Peridia on the specimens examined by me were generally regularly round and deeply urceolate (fig. 70); the sunk disk was always of a lighter colour than the raised margin, both being, however, pale buff or brownish. Sometimes both disk and margin assumed the same colour as the faded-looking leaf—a russet brown.

"Possibly," says Mr Cooke, "your *Æcidium* on *Epilobium* is *Æ. Epilobii,* DC., which has some of the habits of the present species."

3. Parasitic on the leaves of *Microseris Forsteri*, Hook. fil.; growing in marshy places, Abbott's Creek, Greenisland; October to December; in flower.

Generally the Peridia occur as oblong or ellipsoid papillæ or tubercles, unmarked or unpierced by an ostiole in the young state, but exhibiting various forms of urceolæ—for the most part with irregular margins—in the old state (figs. 71, 72). Essentially they resemble in appearance and structure the Peridia which occur on *Clematis.* They also resemble, however, more than do the Peridia on *Clematis* or *Epilobium,* the wart-like apothecia of *Pertusaria*, including both the normal and variolarioid conditions of the latter.

Mr Cooke makes the following interesting remarks regarding this form of the parasite:—"I should say that your specimen on *Microseris* is closely allied to *Æ. Tragoponis*, Pers., and that it certainly is *not* the very common and variable *Æ. Compositarum,* Mart. All the marginal teeth are gone, and most of the spores. If it could be determined whether the Peridia were seated on any definite coloured spot (I think they were not), and what the colour of the spores in the fresh state, and whether that colour was fixed or mutable, we could say definitely whether it really is *Æ. Tragoponis.* My own impression is that it would prove to be *an undescribed species;* but there is not sufficient material to state for certain, or to describe it if new. The colour of the spores, when fresh, is of specific value—as at present acknowledged—whether rightly or wrongly. . . . . You will observe that . . . . the Peridia are scattered, and *not* collected in definite clusters. These are not so numerous or close as in *Æ. Tragoponis,* as you will see by reference to Plate I. fig. 1 of my 'Microscopic Fungi.' . . . . In *Æ. Tragoponis* the spores are at first yellow, then blackish. If, from a better specimen, you can determine that the spores are permanently yellow in your

* Similar thickening and involution of the edges of the leaves on which they grow are sometimes caused by British species, *e.g.*, *Æ. Asperiifolii,* Pers., and *Æ. Euphorbiæ*, Pers. (Cooke, "Introd.," p. 191–2.)

species, it might be regarded as *a distinct species of which I know of no description.* It is not the *Æ. australe* of BERKELEY, which is a *Rœstelia*, I think."

So far as my data enable me to judge, it appears to me to be the same Fungus, which is parasitic on the three Phænogams above mentioned; and it seems doubtful, so little do they vary in their characters, whether the three *forms* deserve separate consideration or classification as *varieties*. The Peridia occur—most generally on the *leaves* of the plants on which they grow—as a series of minute cups or saucers sunk in the tissues of the matrix; pale buff-coloured in the centre, with a darker projecting margin. In shape and colour, these have considerable resemblance to the urceolate apothecia of the old Lichen-genus *Gyalecta* (fig. 64 *b*). To the naked eye they generally appear to be a series of minute, round, buff-coloured spots, sometimes from their abundance giving the leaf on which they grow a yellowish or reddish tinge, with a dry, withered, or puckered character (fig. 69 *b*). For their proper examination, however, they require the aid of the microscope and lens; and the examination should be made *on fresh or living specimens*—for this is one of the many genera of Fungi, whose species can only in this way be accurately determined or fully described. In so far as my specimens were all examined in the dried state, in the Herbarium, my data for determination and description are confessedly imperfect.* It remains, therefore, for *local Botanists* to settle such questions in the natural history of the genus *Æcidium* and its species (as developed in New Zealand) as the following:—

1. Whether the genus is autonomous, and not a mere form or condition of other *Uredineæ*, as OERSTED and DE BARY suggest?

2. Whether, assuming that it *is* a good genus, it contains so many good species as is at present supposed? This is a subject regarding which the most eminent Fungologists are somewhat at issue. On the one hand, Mr CURREY writes me—"I have no faith in the *species* of *Æcidia*: I think that in all probability they are reducible to two or three, if not to *one, species*." On the other hand, Mr COOKE, though admitting, doubtless, their great variability and complexity, describes no less than *thirty-one British species alone.*

3. Whether *Æ. Otagense* differs so essentially from species already described as to deserve a permanent place as a separate species? It differs from the only two species recorded in the "Flora Novæ Zelandiæ," *Æ. Ranunculacearum*, DC., and *Æ. monocystis*, Berk. (which is apparently confined to New Zealand). Nor does it appear to agree in all particulars with any of the thirty-one British *Æcidia*. The

* In all three cases of the parasite on *Clematis*, *Epilobium*, and *Microseris*, the Fungus was determined to be a true *Æcidium* (as the genus is at present established), by the presence of its characteristic spores. "*Æcidia*," says Mr CURREY, in reference to some difficulties that occurred to me in my microscopical examination, "*never bear thecæ*. . . . . In the early state of *Æcidium*, the perithecia produce minute *spermatia;* but neither in that state, nor in the more advanced condition, have *asci* (thecæ) ever been observed. . . . . The spores are always produced in chains; and when they fall apart, after the opening of the cups, they produce the yellow dust (or white) with which the cups are filled."—*Letter*, March 22, 1865.

probability at present therefore is, that if it is proper to maintain upon botanical records the names of so many species as at present established, this merits at least one separate place among their number.

4. Whether the forms occurring on *Clematis*, *Microseris*, and *Epilobium* are *species, varieties, or conditions?*

It will be a further and interesting problem for the *local Botanist* to determine —Whether the same species or forms of *Æcidium* affect plants of different species, genera, and orders; or, on the other hand, whether certain species, genera, or orders of Phænogams are characterised by their specific parasitic *Æcidia*. The present probability is all in favour of the first supposition. The genus in all its forms is eminently deserving of study by resident Fungologists; because, in addition to the points of interest already enumerated, some at least of its species are most destructive to the flowering plants on which they are parasitic.

In all probability several, if not many, species or forms of this large genus *Æcidium* remain to be added to the Flora of New Zealand.

III. FUNGO-LICHENES. (Plate XXX.)

I have elsewhere* pointed out the desirability or propriety of instituting and maintaining an intermediate provisional class between Lichens and Fungi for the purpose of separately grouping a number of doubtful organisms—mostly parasites on the thallus or apothecia of Lichens—regarding which our knowledge cannot yet be said to be either complete or satisfactory, and which, so far as they have been at all specially studied, are the subject of most opposite opinions among Lichenologists and Fungologists. They are placed now among Lichens, now among Fungi, by different authors, whether Lichenologists or Fungologists. They partake of the characters of both these classes of Cryptogams. They are in great measure lost sight of in the ranks of either; while their interest as connecting-links between Lichens and Fungi is such as to render it most desirable to keep them prominently under the notice of Botanists till present conflicting views are reconciled, and their true place in classification is established. Some, if not most, of the parasites in question are to this day equally the puzzles of Lichenologists and Fungologists—the "opprobria," of Lichenology and Fungology. Lichenologists, regarding them as *Fungi*, give them no special examination; while Fungologists, considering them mere degenerations or imperfect conditions of Fungi (if Fungi at all), and with equal probability *Lichens*, also appear to give them no adequate attention. The consequence of this common neglect is, that there is perhaps no group either of Lichens or Fungi of which we really know so little of a precise character. The group of Fungo-Lichenes is incapable of precise scientific definition. It is a

* On *Arthonia melaspermella*, Nyl —*Journal of Linnean Society*, vol. ix. (Botany), p. 268.

heterogeneous assemblage of genera (such as *Microthelia*, *Celidium*, and *Phymatopsis*) * of very varied character.

Nor are these genera themselves satisfactorily or perfectly defined—consisting of an equally heterogeneous collection of species of diverse character—species which improvement in our knowledge regarding them will probably, in course of time, lead us to draft off for the most part to existing genera of Lichens or Fungi, though a minority may become the basis of really new, separate, and properly definable genera, whether of Lichens, Fungi, or Fungo-Lichenes. The genera *Microthelia*, *Celidium*, and *Phymatopsis* are here then regarded and adopted—as the group to which they have been here referred is also—simply as *provisional;* an adoption, however, which is convenient, if not necessary, for reference, and for the facilitation of their further study. The genera referred to agree with those lower Fungi, which are possessed of several forms of fructification, in rarely, if ever, exhibiting all these forms contemporaneously in the same specimen or locality, or it may be, country. I am not aware at present of any instance of complete or perfect fructification—that is, the co-existence of apothecia and spermogones or pycnides—in a given specimen in any given species. Usually fructification, where it occurs, is imperfect, only one form of the reproductive organs occurring (as the sporiferous perithecia in *Microthelia*). Frequently (in *Celidium* and *Phymatopsis*) there is no normal fructification at all; neither spores, spermatia, nor stylospores can be distinguished by repeated and careful examination—the plant existing only in a sterile or degenerate, rudimentary or protothalline condition—the dark-brown irregular and indistinct cellular tissue of the maculæ or wartlets furnishing no clue to the Order even to which the plant belongs. From these circumstances, it happens that an observer may examine—as I have done—in the long course of years, many thousand Lichens from every part of the known world, before he finds a fertile condition of some of these parasites, that is, their apothecia, spermogones, or pycnides; before he is in a position, therefore, to venture to assign the sterile Fungiform Maculæ, so familiar to him, to their proper Natural Order, family, genus, or species. In plants of such a character there cannot fail to be extreme difficulty—frequently for the time, and for years insuperable—in tracing the connection of the several forms of fructification, or their relation to a common species; and it is not surprising, considering the nature of the material, that errors should and must continue to occur in the assignation of names and affinities. Though the connecting links may appear to have been discovered, it may, and probably will, in a certain proportion of cases, prove, in course of time,

* The same genera are classed as "*Pseudo-Lichenes*" by KREMPELHUBER ("Die Lichenen-Flora Bayerns," 1859, p. 275); and by ANZI, *Celidium* and *Abrothallus* are included among "Genera inter Lichenes et Fungos incedentia;" while *Microthelia* is placed among the *Verrucariœ* ("Catalogus Lichenum quos in Provincia Sondriensi et circa Novum-Comum collegit" MARTIN ANZI, 1860; Como: Introduction, xvi.)

that certain apothecia, spermogones, or pycnides, which usually or always occur by themselves, unassociated with their complementary forms of fructification, are really properly referable to plants even of a different order or family. In the absence of *spores*, it is generally impossible or unsafe to determine either species or genus; but their presence does not always render diagnosis facile—for numbers of plants presently classed in the genus *Microthelia* possess essentially the same small, brown, solæform, 1-septate spores, while their habitats or other characters are diverse.

There are several parasites on Otago Lichens, which I prefer, for the reasons above assigned, to describe in a group as Fungo-Lichenes, rather than either as Lichens or Fungi.* In my Dunedin Herbarium, some of these exhibit only perithecia (*Microthelia*); others only spermogones (*Phymatopsis*); while a few occur only as sterile maculæ or warts, which are presumably rudimentary or degenerate perithecia or apothecia, or confluent clusters thereof (*Celidium*). In the two latter classes of cases my New Zealand specimens do not in themselves furnish sufficient means or characters for their determination. But on the same or similar Lichens from other parts of the world, I have found what seem to be links connecting the maculæ or spermogones in question with the relative apothecia, and which enable me—though doubtfully, and with reservation after the explanation already given—to refer them to apparently new forms of *Celidium* and *Phymatopsis*. The nature and relations of these Otago parasites cannot, however, possibly be understood (in so far, at least, as they are imperfect or infertile) without reference to the fertile condition of the most closely allied European plants; and I have the less hesitation in here recording the results of a comparative examination of the latter, that they themselves are little known to Lichenologists and Fungologists, and hence possess an inherent interest warranting their description.

GENUS I. *Microthelia*, Körb. Syst. Lich. Germ. 372 (Syn. *Verrucaria*, Pers.; *Pyrenula*, Ach.; *Endococcus*, Nyl.; *Tichothecium*, Mass.; *Phæospora*, Hepp; *Buellia*, De Not.; *Lecidea*, Ach.; *Abrothallus*, De Not. (*Lichens*), pr. p.: *Sphæria*, Hall (*Fungi*), pr. p.

Typical species have papillæform, superficial perithecia: whence the generic name (*μικρος*, small, and *θηλή*, a wart or papilla). In recedent or exceptional forms, the perithecia are punctiform and immersed, or semi-immersed. In all cases the perithecia are extremely minute or microscopic; and they resemble in size and external aspect the black papillæform or punctiform spermogones of many

* In the Society's "Proceedings," vol. v. p. 528, I have classed them provisionally as *Sphæriæ*, to which they have a more or less resemblance, and to which some at least may hereafter really prove to be referable. Nos. 4, 5, 7, and 8 are now described as *Microtheliæ*; No. 6 as a *Phymatopsis*; and No. 9 as a *Celidium*.

Lichens, especially the lower crustaceous groups. By KÖRBER, STIZENBERGER,* MUDD, and others, the genus is classed among the *Verrucariaceæ* (*Lichens*); † but while KÖRBER proposes enlarging his genus at the expense of the *Sphæriæ* (*Fungi*), MUDD ‡ is apparently disposed to hand over the genus itself, with all its species, to the *Fungi*. My own impression is, that in course of time the genus *Microthelia* will be partitioned mainly, if not entirely, between the *Verrucariaceæ* and *Sphæriacei*—between *Lichens* and *Fungi*.

Sp. 1. *M. perrugosaria*, nov. sp. (figs. 23 to 28).

Hab. Perithecia parasitic on the apothecia of *Placopsis perrugosa*, Nyl., which I found somewhat plentifully on basaltic boulders on the top of Kaikorai Hill (1092 feet).

The parasite is best seen by moistening the apothecia of the *Placopsis*. The disk or epithecium is naturally of a dark or dull port-wine red or crimson colour. Under moisture this colour becomes lighter, the epithecium swells and becomes waxy, and the black punctiform parasitic perithecia then become prominent (figs. 23, 24). They occur in considerable numbers on a single apothecium; generally discrete or isolated; sometimes confluent. The perithecium is found, on careful examination, to be, in its upper half, a papilla seated *on* the epithecium of the *Placopsis*, while the lower half is immersed *in* its tissues (figs. 25, 26). Its walls are formed of dark-brown, small, irregularly formed, and densely aggregated cells (figs. 26).

The *paraphyses* are delicate, filiform, indistinct; without clavate heads; coloured yellow by iodine (fig. 27 *a*). The *thecæ* are ·0021″ long, and ·00045″ broad; ribband-shaped or ventricose, according as the spores are arranged in one or two rows; 8-spored; coloured yellow by iodine (fig. 27 *c d*), and thereby distinguished from the thecæ of the *Placopsis*, which (with its hymenial gelatine), under the same reagent, assume a beautiful Prussian blue, and which are ·0045″ long, and ·00045″ broad (fig. 29). The *spores* are broadly ellipsoid or oval; olive or brown; 1-septate; seldom or never constricted at the septum; polari-bilocular in the young state, and otherwise resembling, on a small scale, the spores of *Physcia pulverulenta*, Schreb, and other Lichens; ·00045″ long, and ·00025″ broad (fig. 28). They are readily distinguished from the spores of the *Placopsis*, which are oblong-ellipsoid; with double contour; *simple; colourless;* and ·0006″ long, ·00045″ broad (fig. 30).

* "Beitrag zur Flechten-systematik," 1862, p. 147.

† The most recent arrangement of the *Verrucariæ* is by Prof. GAROVAGLIO of Pavia ("Tentamen Dispositionis Methodicæ Lichenum in Longobardio nascentium," with Plates and Dried Specimens, 1865), who includes in the comprehensive genus *Verrucaria*, no less than 35 Massalongina genera, and among these *Microthelia*, Körb., and its allies, *Thelidium*, Mass.; *Tichothecium*, Flot.; and *Thrombium*, Wallr.

‡ "Manual of British Lichens," 1861, p. 306.

The plant does not appear to differ materially, save in its site, from two British species, *M. gemmifera*, Tayl., and *M. rugulosa*, Borr. (Mudd, "Manual," p. 306). It has further the characters of various Lichenicolous *Sphæriæ*, as these are described, especially by MUDD. It is, however, most doubtful whether, on the one hand, many, at least, of the *Sphæriæ* so-called, should not properly, as KÖRBER thinks, be referred to his Lichen-genus *Microthelia;* and, on the other, whether the latter genus itself should not be wholly transferred to the *Fungi*. The whole question, as it at present stands, is beset with difficulty and confusion.

The *Sphæriæ* and allied Fungi, which occur parasitic on the apothecia and thallus of Lichens, are at once so little known and so interesting, that I append, for the sake of comparison, a few illustrative examples.

1. *Sphæria squamarioides,* Mudd, p. 130; parasitic on the thallus of *Placopsis gelida*, L.; a Lichen which occurs equally in Otago and Britain, and is so closely allied to, and so frequently associated with, *P. perrugosa*, Nyl., as to be apt to be confounded therewith. *Microthelia perrugosaria* has many of the characters of *S. squamarioides* as described by MUDD, though the habitat differs—being *thallus* in the latter, *apothecia* in the former.

2. *Sphæria gelidaria*, Mudd, p. 130; parasitic also on the thallus of *Placopsis gelida*, L. Its characters at once distinguish it from *M. perrugosaria.*

3. *Sphæria cerinaria*, Mudd, p. 136; parasitic on the apothecia of *Lecanora cerina*, Ach., in Britain.

4. *Sphæria leucomelaria*, Mudd, p. 105; parasitic on the thallus of *Physcia leucomela*, Mich., and *P. ciliaris*, L.

5. *Sphæria epicymatia*, Wallr. (Nyl., Prodr. 85; *S. lichenicola*, Sommrf; Dur. and Mont. Fl. D'Algér. p. 529, Pl. I. No. 130; probably also the *S. apotheciorum*, Mass. Rich. p. 26, fig. 41).

Parasitic on the epithecium (of the apothecia) of *Lecanora albella*, Pers., throughout Europe and Northern Africa; and also, according to TUL. (Mém. Lich. p. 126), on that of *Physcia parietina*, L. Interesting from the possession of spermogones and pycnides, in addition to perithecia.

6. *Sphæria homostegia*, Nyl. (Prodr. 56; *Dothidea*, Syn., 389).

Parasitic on the thallus of *Parmelia saxatilis*, Ach., and *P. Borreri*, Turn. Possesses spermogones.

7. *Sphæria urceolata*, Schær. (Linds. Mem. Spermog. p. 175, Plate IX. fig. 35; Mudd, Manual, p. 267; Hepp exs. 475; described by some authors as an *Endocarpon* or *Dacampia*).

Parasitic on the thallus of *Verrucaria psoromia*, Schær, and *Solorina saccata*, L.

8. *Sphæria Hookeri*, Nyl. (Prod. p. 175). Parasitic on the thallus of *Endocarpon rufescens*, Ach. It would appear to be the same parasite, which occurs so frequently on the thallus of *Lecidea Hookeri*, Schær. (Nyl. Prodr. 139); which has been confounded with that Lichen in our Scottish Alpine (Ben Lawers)

specimens: which is described by MUDD (Manual, 271) as *Verrucaria Hookeri* (Borr. E. Bot. Suppl. plate 2622; Leight. exs. 318): and which has been variously mentioned by other authors as a *Lecidea*, *Verrucaria*, *Endocarpon*, and *Dacampia*.

9. *Sphæria ventosaria*, nov. sp.

Parasitic on the thallus of *Lecanora ventosa*, Ach.—a Lichen which is closely allied, especially in its apothecia, to *Placopsis perrugosa*. In 1856, I collected this Sphæria and Lecanora largely in the Scottish Highlands; and in 1860, in some communications on the subject with Mr CURREY, I proposed for it, as a new British species, and in reference to its habitat, the name *S. ventosaria*.* I have not, however, up to this date, published a description of this parasite; and meanwhile, it appears to be the same plant, which is described by MUDD (Manual, p. 307), as *Microthelia ventosicola*, he arranging it, though expressing great doubt as to the true position of the genus *Microthelia* (p. 308), among *Lichens*. It is, however, says Mr CURREY (in MSS. 1861), "a *true Sphæria*, but the species is quite new to me. It presents a very interesting peculiarity of fructification. Most of the *Sphæriæ* have only 8 spores in each ascus. A very few have an unlimited number; but the spores in these cases are almost colourless, and always simple (*i.e.* not septate), and slightly curved. In your specimen the spores are brown, 1-septate, elliptical, and very numerous in each ascus." HEPP (exs. 644, sub nom. *L. ventosa* var. *spermogonifera*) figures and describes oblong-*ellipsoid* spermatia, which do not agree with the ordinary spermatia of the *Lecanora*, as observed by myself in 1856, or as described by MUDD (157); the latter spermatia being cylindrical or acicular—that is, *linear*.

There are many other Lichenicolous *Sphæriæ*, which, however, have never been properly studied, either by Lichenologists or Fungologists. Besides which, species affecting parasitically the thallus or apothecia of both higher and lower Lichens are to be found—and yet remain to be fully studied and accurately determined—in the following *Fungus*-genera, *inter alia*:—

Dothidea, *Fr.*
Stictis, *Pers.*
Spilomium, *Nyl.*
Sclerococcum, *Fr.*
Hymenobia, *Nyl.*
Epilithia, *Nyl.*
Gassicurtia, *Fée.*
Illosporium, *Mart.*
Peziza, *Link.*

Sp. 2. *M. Cargilliana*, nov. sp. (figs. 31 to 34).

Parasitic on the apothecia of *Parmelia perforata*, Ach., which latter grows

* Mr CURREY proposed for it the name *Sphæria lichenicola* (in letter, January 28, 1861). But this was unsuitable or inadmissible, not only because many other Sphæriæ are equally lichenicolous—parasites on the thallus or apothecia of Lichens; but the term itself had been previously applied by SOMMERFELT, and other of the earlier Lichenologists and Fungologists, *inter alia*, to various species of *Sphæria* (*S. epicymatia*, Wallr.), and *Microthelia* (*M. propinqua*, Körb., and *M. pygmæa*, Körb. Syst. Lich. Germ. 374).

plentifully on the branches and trunks of old or dead trees in Greenisland Bush.

In site and in general characters, this species resembles *M. perrugosaria*, from which, however, it differs in the character of its spores. The perithecia are black and papillæform ; the upper half seated *on* the epithecium, generally of old deformed apothecia, the lower immersed *in* its tissues ; generally isolated or discrete ; scattered in considerable numbers on a single apothecium (fig. 31). Like those of *M. perrugosaria*, they have externally the characters of a minute *Verrucaria*. No thecæ were seen, but the perithecia contained abundance of brown, round spores, about ·00015″ to ·00025″ in diameter ; simple, or with double contour (fig. 34). They cannot be confounded with the spores of the *Parmelia*, which are greatly larger, ·00045″ long, ·0003″ broad ; *colourless ;* and oblong or oval ; though they are also simple, with or without a double contour, according to age (fig. 35). The beautiful blue reaction with iodine of the thecæ and hymenial gelatine of the *Parmelia* further sufficiently distinguish it from the hymenium of the parasite. Nor can the *Microthelia* be confounded with the parasitic *Abrothallus Curreyi*,* which affects the *thallus* of the same *Parmelia*, and which may in some of its conditions or parts be mistaken for the spermogones of the *Parmelia*. The *Abrothallus* is distinguished as a *Lichen* by the blue reaction of its thecæ and hymenial gelatine with tincture of iodine, and by the presence of distinct paraphyses with very granular and dark brown tuberculated heads, as well as by its general analogies.

Materials scarcely exist for the accurate determination or full description of *M. Cargilliana*, whose name is in honour of my friend JOHN CARGILL, F.R.G.S., of Greenisland, Member of the Legislative Assembly of New Zealand, and one of the best known pioneer settlers of Otago, to whom I was indebted for many acts of kindness in the course of my Otago excursions.

Sp. 3. *M. Ramalinaria*, nov. sp. (figs. 44–6).

Parasitic on the thallus of *Ramalina calicaris*, Fr., as it grows on the branches of trees (especially dead "Goai") in Greenisland Bush.

It occurs as extremely minute (microscopic), black, punctiform perithecia, scattered in great numbers, and in a state of close aggregation, over the surface of some of the branchlets, to which they give a dirty or blackish aspect (fig. 44). They are distinct only under the lens. Their wall consists of closely aggregated, dark brown, very small cells (fig. 46) ; but they contained no spores ; and my present materials are, therefore, imperfect for full description or accurate determination. It appears, however, to be the same parasite, which occurs occasionally in this country on *R. scopulorum*, Ach. (Linds. Mem. Sperm. p. 130), and which I

* Described in the Section on *Lichens*, p. 409.

have found also on the apothecia of var. *fraxinea*, L. (of *R. calicaris*), rendering them black-punctate, on specimens in the Hookerian Herbarium, Kew, from Concepçion, Chili.

Sp. 4. *M. vermicularia*, Linds. (sub nom. *Lecidea*, Mem. Spermog. p. 143, plate v. figs. 19, 24, and 25).

Parasitic on the thallus of *Thamnolia vermicularis*, Schær. (specimens, as usual, sterile, bearing neither apothecia nor spermogones); Tarndale Mountains, Nelson; Dr Andrew Sinclair.

I did not find this Lichen in Otago; but I have no doubt it occurs in the central and western alps, which I had no opportunity of visiting.

In New Zealand specimens the parasite exists only in a degenerate or abortive state. It occurs as very minute microscopic), black, punctiform, immersed perithecia, which sometimes become elongated or variously difform; under moisture assuming a brown tint and sub-corneous aspect. The envelope or wall of the conceptacle consists of brown, minute, closely aggregated cells; as do the perithecia of all the allied *Microtheliæ* (figs. 26, 46); but it contains no spores. The plant is, however, so similar otherwise to the parasite, which occurs abundantly in a fertile or normal state on the same *Thamnolia*, in the Falkland Islands, that I have little difficulty as to its identification.

M. vermicularia diverges—as does also *M. Ramalinaria*—from ordinary types of the genus, in so far as the perithecia are not papillæform and epithalline, but wholly immersed. In other respects, their characters are those of *Microthelia*, though they have, I think, equal claims to rank as *Fungi*.

These two species, moreover, are of interest as types of a group of parasites on the thallus or apothecia of Lichens, whose true position or affinities are not at present thoroughly understood or recognised, because, as a group, it has never been made the subject, as it deserves, of special study. Hitherto these parasites have been by myself, as well as other Lichenologists, classed, though *provisionally*, with *Lichens;* but I feel, and have always felt, that they have probably at least equal claims to be considered *Fungi*, and that, as such, they should be made the subject of a mutual re-examination and arrangement by and between Lichenologists and Fungologists. I refer to such parasites as the following, described or mentioned in my "Memoir on Spermogones:" *—

1. *Lecidea obscuroides*, Linds. (Spermog. 247; plate xiii. figs. 36–8; Mudd, 212). Parasitic on the thallus of a leprose form of *Physcia obscura*, Ehrh.
2. *L. Alectoriæ*, Linds. (Sperm. p. 135; plate i. figs. 12–13). Parasitic on the

* Compare also the species of the genus *Microthelia*, Körb., and certain species of the genus *Thelidium*, Mass, as described by Mudd (Manual, pp. 306 and 298); as well as certain species of the genera *Tichothecium*, Fw., and *Endococcus*, Nyl.)

thallus, as well as on the back or lower surface of the apothecia, of *Neuropogon Taylori*, Hook. fil.

3. *L. Cladoniaria*, Linds. (Sperm. p. 163, plate vii. figs. 14–16; Nyl. Enum. Suppl. p. 359). Parasitic on the folioles of the horizontal thallus; on the Podetia, or on their scales; in *Cladonia uncialis*, Hffm., and *C. bellidiflora*, Ach.

4. *Microthelia alciornaria*, Linds. (Sperm. p. 161).

5. *M. prunastria*, Linds. (Sperm. p. 137).

6. *M. Solorinaria*, Linds. (Sperm. p. 175). Parasitic on the thallus of *Solorina crocea*, Ach.

7. *M. Collemaria*, Linds. (Sperm. p .272). Parasitic on the thallus of *Collema melœnum*, Ach.

Genus II. *Phymatopsis*, Tul. (*Abrothallus*, De Not.)

I adopt Tulasne's name for the genus, as being much more correct and appropriate than that of De Notaris, especially when applied to such forms or species as *P. dubia* and its allies. The original name, *Abrothallus* (ἁβρός, thin or delicate), was based on an entire misconception—all species of the genus being strictly athalline and parasitic on an alien thallus, as I have elsewhere pointed out.* But Tulasne's designation† (φῦμα, a tuber or excresence, and ὄψις, like) appropriately represents the external aspect of certain, at least, of its species. Like *Microthelia* and *Celidium*, I regard the genus as in the meantime simply a *provisional* one, made up of species of diverse character. The majority of modern Lichenologists class the genus with the *Lecideæ*, and include all its species, so far as at present known. Nylander, however, while he ranks *A. oxysporus* as a *Lecidea*, removing it altogether from *Abrothallus*, considers *A. Smithii* a Fungus.‡ I am at a loss to understand on what principle such a distinction is made. One of the principal supposed tests for distinguishing Lichens from Fungi —the blue coloration of the hymenial Lichenine of the former under iodine— cannot be depended on; for, on the one hand, it does not follow that a plant, whose hymenium does not give this reaction, is not a Lichen, or which does give it, is not a Fungus; while, on the other, I have sometimes met with blue coloration in foreign forms or specimens of the same plant, which in Europe does not usually yield any colorific reaction.

Sp. 1. *P. dubia* (figs. 36–42).

Parasitic on the apothecia of *Usnea barbata*, Fr., var. *florida*, L., as it grows plentifully on trees in the Bush, Pelichet Bay, Dunedin; a dwarf form of the *Usnea*, with articulate branches, and in abundant fructification. Some of the mature

* Monograph of *Abrothallus*, p. 7.

† Mém. Lich. p. 113.

‡ He also alters the name—both specific and generic—recording it as *Habrothallus parasiticus*, Prod. 55.

and normal apothecia are marked by irregular, flat, brown, superficial maculæ, two or three of which generally nearly cover the epithecium (fig. 36). They are scarcely raised above the surface of the epithecium, of which they appear simply a discoloration; but when moistened and closely examined under the lens, they are found to be the seat of numerous extremely minute (microscopic), brown papillæ, which are semi-immersed. These papillæform conceptacles contain, instead of spores, myriads of rod-shaped *spermatia*, varying in length from ·00010″ to ·00015,″ and about ·00007″ broad, borne on the apices of very short, simple, sterigmata (fig. 38). These conceptacles have externally, save as to colour, the aspect of certain *Microtheliæ* (e.g., *M. perrugosaria* and *M. Cargilliana*); but their contents show them to be *spermogones*, whose relative sporiferous perithecia remain yet to be discovered or determined. As regards their sterigmata and spermatia especially—as well as in other characters—these spermogones do not agree with those of the only species of *Abrothallus* in which they are yet known to occur.* The knowledge we possess of the genus is, however, both fragmentary and unsatisfactory. It does not follow, then, that a difference of character excludes them from the genus; while, on the other hand, it must be stated as a possibility that they may ultimately prove referable to some other genus or order.

The parasitic spermogones in question are, moreover, quite different in all their characters from the spermogones of the genus *Usnea* or any of its species.† The latter are minute wartlets, of the same colour as the thallus, scattered upon the tips of the ultimate ramuscles, as well as over the apothecial marginal cilia. The sterigmata are much larger, and are compound or articulated; the spermatia, though also linear, are longer.

Beneath the brown maculæ of the parasite the hymenium of the *Usnea* has its normal character, striking its usual blue colour with iodine, and the thecæ containing the ordinary spores, which are oblong-ellipsoid, or sub-globose; simple; colourless; ·0003″ long, and ·0002″ broad; with or without double contour, according to age (fig. 43).

On *U. barbata*, Fr., var. *ceratina*, Ach., growing on the branches of trees in Saddlehill Bush. I found both apothecia and thallus infested with various forms of a parasite.

The one form consists of very minute, black, punctiform, or difform, semi-immersed perithecia, abundantly scattered over the surface of the roughened branchlets (fig. 39 *a*). In or *on* some of these I found *brown*, simple, oblong-ellipsoid spores; ·0003″ long, and ·0002″ broad; but they are of the same size and form as the normal spores of *Usnea*, to which they probably really belong, having

* In *A. oxysporus*, Tul., as described by myself ("Monograph Abrothallus," p. 33). TULASNE (Mém. 113) described the spermogones as *un*known in the genus.

† Which are described and figured in my "Mem. Spermog." p. 121, plate i. figs. 1–8.

acquired *colour* only from the parasite, with which they have come into accidental contact (fig. 42).

The other form occurs as maculæ, more or less raised and difform, sometimes apparently destroying the superficial tissues of the apothecium or thallus—sometimes simply seated thereon (fig. 39 *b*). These maculæ have much the external characters of the *Celidia* of the *Stictæ*. They contained no spores, and were in an imperfect and unsatisfactory condition for determination. It remains for Local Botanists to determine whether the parasitic spermogones, perithecia, and maculæ above described are referable to the same plant, or to different species, genera, or orders.

The nature or relations of these isolated forms of fructification cannot be understood without comparison with a suite of European and foreign specimens of the same or allied species of *Usnea*, on which I have found at various times similar parasites in more perfect conditions. Of these it will be most instructive first to describe the highest forms of the most nearly allied parasites—those which exhibit sporiferous apothecia.

1. Sub. nom. *Abrothallus Usneæ*, Rabenhorst exs. ("Lichenes Europæi") No. 537; sent me some years ago by Dr Rabenhorst himself from Germany; a fine specimen, abounding in the parasitic apothecia in every stage of their growth; affecting var. *florida*, L., of *Usnea barbata*, Fr., in abundant fructification. The parasite, if all its diverse forms are referable to one species, equally affects the thallus from base to apex; the apothecia normal and degenerate—upper and under surface; and the cephalodia of the *Usnea*.

The latter, as they occur in the genus *Usnea*, are so little known, and their true nature so much misunderstood, that they demand some description before we proceed further. I have repeatedly met with them both in British and foreign forms of *U. barbata*, and in foreign allied species, and I was long puzzled with their true character. A careful examination of a series of specimens leads me to concur with Nylander (Syn. p. 15), in regarding them as warts or excrescences of the cortical layer or tissue of the thallus, having, however, a modified structure, consisting of a very dense, finely striated (filamentous) tissue, not unlike that of many hymenia, where the paraphyses are very closely aggregated; a circumstance, which, along with the frequently pale-bluish or violet coloration with iodine, gave certain grounds for the apparently common belief that the growths in question were really degenerate apothecia. This coloration with iodine is, however, a fallacious indication of hymenial structure; for, in the cortical tissue of the thallus of the *Usnea* on which the *Abrothallus* is parasitic—far removed from all forms, both of apothecia and cephalodia—towards the base of attachment of the plant, I sometimes met with a distinct, though pale, violet-blue colour under iodine. I doubt not that the thallus of other of the higher Lichens, containing Lichenine in their tissues, will be found to yield a similar reaction.

The *cephalodia* of *Usnea* are not mentioned by MUDD (Manual, 1861); but they appear to have been familiar to the earlier English Lichenologists, who contributed to the "English Botany" of Sir JAMES E. SMITH. In that work they are both figured and described, apparently as of a twofold character, viz., partly as "*orbillæ*," or abortive apothecia, partly as "warts" or excrescences (p. 71). The "orbillæ" are best marked and commonest in var. *florida*, L. (p. 71, plate 2250). They agree in aspect with the cephalodia of *U. longissima*, Ach., from Sikkim (Himalayas) in the Hookerian Herb., Kew. In var. *hirta*, Fr., they are described as "flesh-coloured, solid warts" (p. 72, plate 2252); and in the type *U. barbata*, Fr., as "fleshy tubercles or warts." The writer adds in regard to the latter (the type *U. barbata*) a reference to the "absence of 'orbillæ,' which have never yet been discovered upon it in Britain or elsewhere" (p. 72, plate 2253): thus drawing an unnecessary distinction between the "warts" and "orbillæ," which are evidently, nevertheless, of essentially the same nature.

RABENHORST appears to regard the cephalodia affected by the parasitic *Abrothallus* as a diseased condition of the apothecia of the *Usnea*. But the latter, in their normal condition, are large, handsome, flat, broad, peltate; with a disk of similar colour to the thallus, or pale flesh-coloured as in *Ramalina*, the margin fringed with cilia, having the characters of the ultimate thalline ramuscles. In exceptional cases (as in a specimen of var. *florida* lately sent me from Lagos, West Africa, E. L. SIMMONDS) the apothecium is Parmelioid; the disk red and sub-urceolate; the margin raised, and furnished with few and short cilia, or sub-simple. Here the apothecia are seated at angles of the thalline ramules, a circumstance which, though common or general in *cephalodia*, is rare in *apothecia*. Exceptionally cephalodia are apotheciiform—that is, they are flattish, with a raised, generally thickish margin, the parasite occupying the cavity resembling the disk. But I have never been able to trace any distinct transition between cephalodia and apothecia; hence I regard them as essentially differing in character and function. The cephalodia so common on the genus *Usnea* may be regarded as analogous to those of *Stereocaulon* (Nyl. Syn. 15), save that in *Usnea* they are solid, while in *Stereocaulon* they are hollow; in both cases, they must be considered morbid growths of and from the cortical layer of the thallus. In *Usnea*, they usually have the aspect of Cladonioid or Biatorine, compound or confluent, apothecia; being irregularly tuberculated, and more or less of a pale brownish or flesh-colour. Almost universally, where they occur, they produce angularity of the thalline ramules, so that they appear to have their normal seat at natural angles of the branchlets. The cephalodia are abundant all over the plant; so that in regard to their site, they are in marked contrast to the normal apothecia of *Usnea*. In Sikkim specimens of *U. longissima* I have found cephalodia terminal on two of the divergent cilia or fibrillæ of the thallus; in which case they had greatly the aspect of Cladonioid apothecia.

The apothecia of *Abrothallus Usneæ* in RABENHORST'S specimen are frequently, especially towards the base of the thallus, isolated, large, and epithalline; convex, Biatorine, immarginate; of a blackish-brown colour; and having all the aspect of the usual apothecia of *A. Smithii.* Frequently, also, they become aggregated, confluent, and difform, then assuming the aspect of species of *Phacopsis** (e.g., *P. vulpina*). Another—the most rudimentary, and a sterile, probably a protothalline, condition of the plant—occurs in the form of maculæ—most irregular in form, size, and position; sometimes raised or convex; occupying occasionally such exceptional sites as the under surface of the apothecia, or the tips of the ultimate thalline ramuscles; in the latter case, giving the appearance of Cetrarioid spermogones. Between the large, isolated, sporiferous, Biatorine tubercles, and the aggregated or confluent sterile maculæ, there is every gradation of character. Of the isolated fertile apothecia, forms occur, which, though generally rounded or sub-spherical, are sometimes conoid or flat and wart-like, sub-immersed and emergent; sometimes girt with a thick distinct thalline border. The aggregate or confluent conditions, which are less frequently found fertile, generally produce irregular one-sided swellings of the branchlets, or apothecial cilia, on which also they find a site. Very seldom do they, in any of their conditions, surround the branchlet or fibril, being almost invariably seated only on one side thereof, producing angularities, swellings, and other deformities. Occasionally they are sub-terminal clusters of tubercles; in this, and in all other cases, of a blackish-brown colour, which, under moisture, becomes a pure, though dark, *brown.* The flattened disk passes gradually into the macula, frequently becoming stellate or irregular in outline.

The structure and contents of the hymenium must be examined in the isolated, large, regular, Biatorine apothecia. The aggregated, confluent, and difform conditions are almost invariably degenerate and sterile. The whole hymenium is generally obscured by more or less abundance of brown granular colouring matter; sometimes, however, it is very free from this colouring matter, and in such cases the thecæ and spores are very distinct, though the paraphyses are always indistinct, delicate, and obscured by much dark brown granular colouring matter about their tips. The *thecæ* are generally indistinct; 8-spored; the spores generally arranged in two rows; ·0018″ long, ·00045″ broad; giving no reaction with iodine. The *spores* are plentiful and distinct; oblong-ellipsoid;

* Contrast with the genera *Phymatopsis* and *Celidium* this genus *Phacopsis*, Tul. "Mém. Lich." p. 124 (name from Φακὸς, a nævus or lentigo, a skin-wart); and especially the species *P. vulpina*, Tul. (p. 126), Linds. "Mem. Spermog." plate iv. fig. 22, p. 125; Hepp exs. 474. Than this species no *Lichen* could more resemble, in its external aspect and habit of growth, a *Fungus;* but no Lichen gives, at the same time, a more distinct and beautiful blue or violet colour (varying in shade, and sometimes very pale) with iodine (thecæ and sometimes hymenium). While in *Phymatopsis* (*Abrothallus*) and *Celidium* the apothecia or groups of perithecia are essentially *brown* (though frequently of a dark blackish-brown), in *Phacopsis* they are essentially *black* ab initio.

simple in outline or solæform;* generally 1-septate; sometimes no septum visible in young state; olive or brown according to age; ·0003″ to ·0004″ long, ·00015″ broad. Associated with the full-sized spores are numerous half spores; probably the result of the friction of the glass slides under examination; generally sub-oblong or sub-spherical. In one sub-degenerate and oldish apothecium—not apparently in any distinct or separate conceptacle—I met with multitudes of corpuscles, having the characters of *stylospores;* colourless, irregular (sub-spherical, oval, oblong, or pyriform); about ·00015″ long, and ·00009″ broad.

So far as regards the highest or most perfect condition of the apothecia, it will be observed that those of *A. Usneæ* agree with those of the microspermous varieties of *A. Smithii.* But the spores are greatly smaller than those of the commoner forms of *A. Smithii,* which are generally ·00066″ to ·00090″ long, and ·00030″ to ·00040″ broad; while the stylospores (should they really prove to be so) are also greatly inferior in size to those of *A. Smithii,* in which they are ·00040″ to ·00066″ long, and ·00033″ to ·00040″ broad. No spermogones and no separate or isolated pycnides were detected. There are two forms of *A. Smithii,* which it is instructive to compare with *A. Usneæ,* viz.:—

2. Sub nom. *A. microspermus,* Tul. (Hepp exs. 477. *Lecidea thallicola,* Mass.), on the thallus of *Parmelia caperata,* Ach. The mature apothecia are usually papillæform; but in the old state they become flattened and maculæform, resembling the genus *Celidium* in external aspect. This maculæform condition is much more unlike typical species of *Abrothallus* than are the majority of forms of *A. Usneæ.* The apothecia are accompanied with pycnides; and associated also are the spermogones of the *Parmelia,* which are apt to be confounded with the said pycnides; but which are immersed, black, and punctiform.

3. *A. Smithii,* Tul. var.; parasitic on the thallus of *Ricasolia pallida,* Nyl. (Syn. 372. *Sticta pallida,* Kunth; *S. Kunthii,* Del. Linds. Mem. Spermog. 205); Mexico; in Herb., Kew. Apothecia small, black, lecidiiform; rounded or flattened; epithalline or sub-immersed. Hymenium, *blue with iodine;* a reaction which is exceptional in this species, and which is noteworthy as connecting it with *A. oxysporus,* and retaining it within the category of the *Lichens* (from which NYLANDER dissociates it, regarding it as a *Fungus,* Prodrom. 55). Paraphyses with deep brown tips. *Thecæ* 8-spored; ·0013″ long, ·0006″ broad. *Spores* brown; 1-septate; figure-8 shaped, or without constriction opposite the septum; variable as to size—frequently elongated and narrow; ·0006″ long, ·00016″ broad.

4. Var. *ceratina,* Sch. (*U. barbata,* Fr.); Rio Janeiro, HENRY PAUL, 1846; in

* "Shoe-sole-shaped"—"Schuhsohlenförmig" (Körb. Syst. Lich. Germ. 373), a graphic and appropriate term in reference to spores, which are 2-locular, with one division (upper) broader and shorter, and the other (lower) narrower and longer (as in *Abrothallus Smithii,* Tul. Linds. Monog. Abroth. plate iv. fig. 12).

my Herb.; exhibits sporiferous perithecia, cephalodia, and difform sterile tubercles and maculæ. The perithecia are scattered among the spermogones of the *Usnea,* and may be confounded therewith, or overlooked among them; but they are at once distinguished by the brown, 1-septate, figure-8 spores, which agree with those of *A. Usneœ.* The cephalodia resemble those in specimen No. 1 (var. *florida*), being generally difform-biatorine, or irregularly tuberculated, producing angularities of the branchlets. They occur also, however, as minute isolated papillæ or warts; sometimes becoming difform or maculæform; scattered on the ramules from base to apex of the plant; sometimes sub-terminal; generally producing angularities or deformities, like the larger more common forms—always more or less dark brown; and frequently assuming the aspect of species of *Celidium* or *Phacopsis.* These cephalodia are equally abundant and variable also in specimens of var. *ceratina* from Tasmania (LAWRENCE), and in *U. longissma* from Sikkim (Himalayas), both in Herb., Kew.

5. Var. *plicata,* Fr., Mauritius; in Herb., Kew; exhibits sporiferous maculæ and cephalodia, as variable in character as those described under No. 4. The maculæ are brown; they contain spores irregular as to size and form; always brown; sometimes simple and oval; sometimes 1-septate and figure-8-shaped; ·0004″ to ·0008″ long, and ·0002″ broad. The cephalodia are abundant, generally small, wart-like, resembling the apothecia of *Alectoria jubata,* Ach., producing, as usual, angularities of the ramules; occasionally having on their surface brown spores of the character of those of the parasite just described, and evidently referable to it.

6. Var. *ceratina* (*U. barbata,* Fr.), Hepp exs. 561. Some of the branchlets exhibit tuberculate, irregular, brown gouty swellings, resembling those produced by the spermogones on *Neuropogon melaxanthus,* Ach. and *N. Taylori,* Hook. fil. (Mem. Spermog. plate iv. figs. 9, 10, and 13, 14.) The parasite wholly surrounds the branchlet; is distinctly limited above and below; its brown colour is much heightened by moisture; it has no connection with any forms of either cephalodia or apothecia, but it exhibits no reproductive structure. The whole aspect of the parasite or deformity is Fungoid. In a specimen of *U. angulata* from New South Wales (also in Hepp's exs. 561), I find simple brown maculæ, which sometimes envelope the branchlet, sometimes colour its tip; but exhibiting no reproductive structure.

GENUS III. *Celidium,* Tul. Mém. Lich. p. 120; plate xiv. figs. 9–13; *Delisea,* Fée; *Plectocarpon,* Fée; *Dothidea,* Smrf. pr. p.; *Sphœria,* De Not. pr. p.

The essential character of the genus is the close aggregation of the sporiferous perithecia in round sori or maculæ (whence the generic name κηλίδιον, a macula), with central spermogones, as in *Phacopsis.* These sori or maculæ are blackish-brown; less or more raised above the thallus on which they are parasitic;

more or less irregular in their surface, though generally with a defined roundish form; immarginate; epithalline (not hypophlœod). Sometimes all—sometimes only the central perithecia—coalesce or become confluent—their apices only appearing and remaining somewhat free and distinct—forming the projecting prominences, which roughen the surface of the maculæ. The spermogones are spherical, simple, seated in the centre of the cluster of perithecia; spermatia straight, linear, very delicate, imbedded in hyaline mucilage. Pycnides have not been discovered.

Its species form parasites affecting the thallus and apothecia of various of the higher (foliaceous) lichens. The genus is variously regarded as a *Fungus** and *Lichen*,† whether by Lichenologists or Fungologists. Those who regard it as a *Lichen* class it usually among the *Lecideæ*,‡ though it appears to me, if it *is* to be considered a Lichen, to have at least an equal affinity to certain genera of the *Graphideæ* and *Verrucariæ*.

Sp. 1. *C. dubium* (figs. 47 to 52).

Parasitic on the thallus of various species of *Sticta*.

1. On *S. granulata*, Bab. (sterile specimen), Signal Hill, Dunedin; Dr Sinclair. The parasite affects generally the sterile, larger, and more granulose forms of the thallus, and seems most abundant towards the ends of the laciniæ. In what appears to be the young state of the plant, the perithecia are isolated and papillæform, with visible ostiola (figs. 47, 50 *a*); they gradually, however, become confluent, forming difform groups (*b*); and lastly, maculæform, exhibiting no distinct ostiola. Both papillæ and maculæ vary greatly in size; in both, the colour varies from deep umber to black (in the dry state). Both are raised more or less above the thalline surface; both are only partly or apparently sub-immersed by their bases. Interspersed among both occur much more minute, punctiform conceptacles, which may (when their normal contents are discovered) prove to be spermogones or pycnides (figs. 47, 50 *c*).

2. On *S. fossulata* Duf. (fructiferous and sterile specimens); corticolous (on trees), Saddlehill Bush.

In regard to site and external aspect, the parasite here has the characters described in No. 1. It is abundant on some specimens of the *Sticta* towards the tips of the laciniæ, to which it sometimes gives, even to the naked eye, a black-mottled appearance (fig. 49). There is the same transition from papillæ to maculæ (figs. 51–2), the latter apparently being always due to the confluence of the former. Here, however, the papillæ are more irregular in form, and the ostiole is never distinct. The maculæ are generally more or less raised or convex on their surface; and they are sometimes regularly spherical (figs. 49, 51 *a*), in which case they closely resemble the apothecia of certain *Arthoniæ* (e.g., *A. lurida* Ach). More generally, from the coalescence of several maculæ, or difform sori

* Nylander, Prod. 52. † Stizenberger, 163. ‡ *Ib.* 163.

of confluent perithecia, the plant forms most irregular blotches of a dark-brown colour (figs. 49, 52 *b*). The parasite can scarcely be confounded with the spermogones of the *Sticta*. The latter are confined to the thalline rugæ* (fig. 48 *b*), while the parasite is irregularly and generally scattered over fossæ and rugæ alike (fig. 48 *a*). The spermogones are, moreover, paler brown, wholly immersed, punctiform, and greatly more minute than the smallest or papillæform condition of the parasite. The latter occurs on the larger (sterile) darker-coloured, and more fossulate forms of thallus; while the spermogones are most distinct on paler and fertile forms. I have found similar parasitic perithecia or papillæ, isolated, variable as to size, but containing neither spores, spermatia, nor stylospores, in Tasmanian (and fertile) forms of *S. fossulata*, sent me by my friend M. C. COOKE.

3. On *S. rubella*, Hook. and Tayl. (sterile specimens), on trunks of dead trees, Greenisland Bush.

In some specimens of the *Sticta* the thallus is covered with blackish or brown blotches, apparently allied to the larger maculæ described in No. 2. But here they are not distinctly defined—the blotch gradually merging or fading into the beautiful red colour of the thallus. Nor are they distinctly raised above the thalline surface. Under the microscope, all the structure these blotches exhibit consists of a brown or blackish-brown filamentous tissue, composed of jointed cells, thick-walled, and varying much in length and breadth; whereas the perithecia, composing the maculæ in *S. granulata* and *S. fossulata*, though they exhibit no spores, possess a distinct envelope, consisting of small, irregular, dark-brown, closely packed cells; a structure which resembles that of the conceptacle of the parasitic *Microtheliæ* (figs. 26, 46).

It remains for the Local Botanist to determine whether it is the same parasite, which affects *S. granulata*, *S. fossulata*, and *S. rubella*; whether all the forms of Perithecia, Sori, and Maculæ above described, are referable to a single plant.†

Inasmuch as the parasite in all my Otago specimens was in an imperfect (infertile) condition for description and determination, it is necessary, in order to an approximate understanding of the nature and relations of the perithecia and maculæ referred to, to append some description of authentic and fertile conditions of the nearest European species. Of these, the most closely allied which I have seen appears to be—

4. *Celidium Pelvetii*, Hepp exs. 372 and 589. Parasitic on the thallus of *Sticta aurata*, Ach. (*S. aurata* var. *abortiva*, Schær.); Brazil; excellent specimens. The maculæ are so large and conspicuous, as well as so abundant, that the thallus is prominently black-mottled to the naked eye. Typical (fertile) maculæ are regularly round; convex or sub-conoid; isolated; of a deep blackish-brown colour; resembling in general aspect the apothecia of *Arthonia lurida*. In

* Lindsay, Mem. Spermog. p. 198, plate x. figs. 26–7.

† Compare *Sphæria homostegia*, Nyl.

the rudimentary condition (protothalline), they are flat, dendritiform maculæ; while, in the old state, from coalescence of sori, they also assume the condition of difform, flattened maculæ or blotches. At no stage of growth did I observe that division into separate, though agglomerated perithecia, which are described by TULASNE as characteristic of the genus; the surface of the sori is smoothish and uniform as in the apothecia of *Arthonia.* The maculæform condition frequently resembles various species of *Dothidea* (Fungi). Sometimes the maculæ, which are usually distinctly raised and epithalline, are sub-saccate, like the apothecia of *Solorina saccata,* Ach.; seated in a depression or pit of the thallus, and surrounded by an indistinct, raised, spurious thalline border. The maculæ vary greatly in size—the sterile rudimentary or degenerate, especially compound or confluent, ones being the larger. The latter especially have a decided Fungoid aspect. Fertile and sterile forms generally do not occur on the same thallus; hence, while spores are common and distinct in HEPP's exs. 589, they are with difficulty discovered or seen in No. 372. The hymenium, as examined especially in No. 589, whose constituents are extremely indistinct in the absence of any reagent, gives no blue reaction with iodine; but the paraphyses, which are closely aggregated and indistinct, acquire at their tips, which are naturally pale brown, a deep brownish red (Port wine) colour, or a violet red—sometimes very intense—which extends frequently to the thecæ and throughout the hymenium. The *thecæ* are 8-spored; short; sub-saccate; ·0018″ long, ·00045″ to ·0006″ broad; their protoplasm in the young state is very granular. The *spores* are solæform (1-septate), as in *Abrothallus Usneæ;* ·00045″ long, ·00022″ broad; but they are colourless or pale yellow. Under iodine, the epispore becomes more easily distinguished from the contained loculi, which are most distinct without the addition of any reagent, and which resemble somewhat those of the spores of various forms of *Verrucaria epidermidis,* Ach. The spores must be looked for in the youngish, or mature, regularly formed, spherical, convex sori. The same *Sticta* (*S. aurata*) in SCHÆRER's exs. 558, and in my Herb. from Rio Janeiro, PAUL, shows on its thallus a few irregular flattish maculæ, devoid of reproductive structure, and probably in their rudimentary or protothalline condition; apparently referable to *Celidium Pelvetii.*

5. *Celidium Stictarum,* Tul.; Mém. Lich. p. 121, plate i. fig. 17 *c.* and plate 14, figs. 5–8 (var. *pleurocarpa,* Ach., of *Sticta pulmonacea,* Ach.: "apothecia Fungosa abnormia"—of *S. pulmonacea*—Wallr. and Fr.; *Dothidea Lichenum,* Smrf.; *Sphæria Stictarum,* De Not.) Affects the apothecia (disk or epithecium) and the apotheciiform cephalodia of *Sticta pulmonacea,* Ach. NYLANDER refers the parasite to the *Fungi;* but points out its affinity to certain species of *Arthonia* (e.g. *A. varians,* Dav., and *A. Abrothallina,* Nyl., Prodr. 52, Syn. 352). The parasite covers the apothecia with an irregular black crust, which consists of considerable numbers of closely aggregated perithecia, sub-confluent, having in

the centre of the group the spermogones, which are small, spherical, frequently confluent; the walls thick and black; the spermatia straight, very slender, and about ·00012″ long.

A. NYLANDER exs. Covering the disk of the apothecia (the larger and more mature) of *Sticta pulmonacea*, Ach. *Thecæ*, pale blue or dirty purple with iodine; a fact of some significance in relation to the question, whether the genus *Celidium* is a *Lichen* or a *Fungus;* 8-spored; ·0023″ long, ·00066″ broad. *Spores* fusiform or ellipsoid; somewhat variable as to form and size; colourless; 3-septate; sometimes not exhibiting the epispore in the young state; sometimes seen in process of germination; in the old state brown and obscurely granular; ·00066″ long, ·00025″ to ·00016″ broad. Tips of paraphyses deep brown.

B. SCHÆRER exs. 550, sub nom. *B. pleurocarpa*, Ach., of *S. pulmonacea*, Ach. Here the paraphyses are extremely indistinct; the *thecæ* are 8-spored, and ·0020″ long, ·00066″ broad. *Spores* ·00083″ to ·00066″ long, ·00025″ broad; 3-septate; pale yellow; ellipsoid, sometimes sub-pyriform; sometimes exhibiting bulgings of the epispore opposite the septa.

Scattered irregularly over the thallus are numerous maculæ or papillæ of variable size and appearance, wholly devoid of reproductive structure, and evidently partly rudimentary or protothalline, partly degenerate; but apparently referable to the parasitic *Celidium*, whose sporiferous perithecia (perfect or fertile condition) are to be found only on the apothecia of the *Sticta*. The papillæ resemble young lecidiiform apothecia, or *Verrucariæ*, or spermogones. The maculæ are sometimes sufficiently raised or convex, and regular in their outline (spherical), to approach in character some forms of *Abrothallus* or certain lecidiiform *Arthoniæ*. Both papillæ and maculæ are blackish brown of various shades, and are isolated and somewhat regular in form, or confluent and difform; in the latter case frequently with ragged margin. The flat, thin (rudimentary) maculæ are sometimes irregularly stellate and Arthonioid; sometimes sub-dendritic; seldom confluent; sometimes elongated, or sub-oblong, and with a general Fungoid aspect; generally, with the other forms, seated on the thalline rugæ.

In HEPP's exs. 590, the *Celidium* of the apothecia (of all sizes) of *S. pulmonacea* is accompanied with maculæ on the thallus, similar to those just described, and, as usual, without reproductive structure. Similar *Celidia*,* whether in their fertile and perfect—or, as is more generally the case, in their sterile and imperfect—state, occur on a large proportion of the *Stictæ;* sometimes on the apothecia or thallus, or both (as in *S. scrobiculata*, Ach., and *S. Freycinetii*, Del.); sometimes on the verrucose or tuberculose thalline cephalodia (as in *Ricasolia corrosa*, Ach. Nyl. Syn. 372): in which latter case the parasite is analogous to *Abrothallus*

* Yet apparently undescribed.

Usneæ on the cephalodia of *Usnea barbata*. In general terms it may be stated, that I have examined, with a view specially to the detection of this or other parasites, few species of the genera *Sticta*, *Stictina*, or *Ricasolia* (and especially belonging to the groups represented in this country by the types *Sticta pulmonacea*, *S. linita*, Ach., or *S. scrobiculata*, Ach., or in foreign countries by *S. fossulata*), without succeeding in finding some traces thereof.

The sterile or imperfect states of various *Celidia* are not distinguishable from each other, even on the most careful examination; without the *spores*, the species cannot be safely determined. The rudimentary or degenerate maculæ of *C. Stictarum* and *C. Pelvetii* are alike; but the spores are very different, regularly 3-septate and ellipsoid in the one, and equally regularly 1-septate and solæform in the other; while their site, also in the perfect or fertile condition, is generally diverse, being apothecia in the one case, and thallus in the other.

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*Explanation of Plates XXIX. XXX.**

Plate XXIX.

Figs. 1–5. *Abrothallus Curreyi*, Linds.
1. Portion of thallus of *Parmelia perforata*, Ach.; nat. size; bearing
   a. Apothecia (of *Parmelia*).
   b. *A. Curreyi*; parasitic apothecia.
   c. Spermogones (of *Parmelia*).

2, 3. Diagrammatic sections (variously magnified) of
2. *A. Curreyi*, apothecia.
   a. Young.
   b. Mature.
3. Spermogones of Parmelia.
   a. Young.
   b. Old.
4. Section of hymenium of *A. Curreyi*, mag. 425 diam. linear.
   a. Epithecial membrane.
   b. Paraphyses.
   c. Thecæ.
   d. Hypothecial tissue.
5. Spores of *A. Curreyi*.

Fig. 6. *Abrothallus oxysporus*, Tul.
Spores; *a*. Young.
b. Mature.

Figs. 7, 8. *Lecidea Otagensis*, Nyl.
7. Section of hymenium.
   a. Paraphyses.
   b. Oil globules.
   c. Thecæ.
   d. Hypothecium.

Fig. 8. Spores; *a*. Young.
b. Mature.

Fig. 9. *Lecidea flavido-atra*, Nyl.
Spores; *a*. Young.
b. Mature.

Fig. 10. *L. melanotropa*, Nyl.
Spores; Mature.

Fig. 11. *L. amphitropa*, Nyl.
Spores; Mature.

Fig. 12. *L. coarctata*, Ach.
var. *exposita*, Nyl.
Spores; *a*. Young.
b. Mature.

Figs. 13, 14. *Lecanora homologa*, Nyl.
13. Section of hymenium.
   a. Paraphyses.
   b. Theca.
   c. Hypothecium.
14. Spores; *a*. Young.
   b. Mature.
   c. Old.

Fig. 15. *Lecanora peloleuca*, Nyl.
Spores; *a*. Young.
b. Mature.
c. Old.

Fig. 16. *L. thiomela*, Nyl.
Spores; *a*. Young.
b. Mature.
c. Old.
d. Do. in process of germination.

Fig. 17. *Placopsis perrugosa*, Nyl.
Spores; *a*. Young.
b. Mature.

* From the *colours* being *printed*, and not done by hand, the tints, in many cases at least, are more approximative than exact copies from Nature.

Fig. 18. *Opegrapha subeffigurans*, Nyl.
Spores; *a*. Young.
b. Mature.
c. Old, in process of germination.
Fig. 19. *O. agelæoides*, Nyl.
Spore; Mature.
Fig. 20. *O. spodopolia*, Nyl.
Spores; *a*. Young.
b. Mature.
Fig. 21. *Arthonia platygraphella*, Nyl.
a. Theca.
b. Spores.
Figs. 22, 23. *Platygrapha longifera*, Nyl.
22. Section of hymenium.
a. Paraphyses.
b. Theca.
c. Hypothecium.
23. Spores; *a*. Young.
b. Mature.
Figs. 24, 25. *Pertusaria perfida*, Nyl.
24. Section of hymenium.
ad. Paraphyses.
bc. Thecæ; *b*. to which iodine has been applied.
e. Hypothecium.
25. Spores; *a*. Young.
b. Mature.
Figs. 26, 27. *P. perrimosa*. Nyl.
26. Section of hymenium.
a. Paraphyses.
b. Thecæ.
c. Hypothecium.
27. Spores; *a*. Young.
b. Mature.
Fig. 28. *Pannaria immixta*, Nyl.
Spores; *a*. Young.
b. Mature.
c. Old.
Fig. 29. *P. gymnocheila*, Nyl.
Spores; *a*. Mature.
b. Old.
Figs. 30, 31. *Psoroma sphinctrinum*, Mut.
30. Spores; *a*. Mature.
b. Old.
31. Spermatia.
Fig. 32. *Physcia plinthiza*, Nyl.
Spores; *a*. Young.
b. Mature.
c. Old.
Fig. 33. *Ricasolia herbacea*, DN.
var. *adscripta*, Nyl.
Spores; Mature.
Figs. 34–36. *Sticta subcoriacea*, Nyl.
34. Section of hymenium.
a. Epithecial membrane.
b. Paraphyses.
c. Theca.
d. Hypothecium.
35. Spores in *Otago* specimens.
a. Young.
b. Mature.
c. Old.
36. Spores in Wellington specimens.
a. Young.
b. Mature.
c. Old.
Fig. 37. *Sticta filix*. Hffm.
a. Spores; young and mature.
var. *parvula*, Nyl.
b. Spores; mature.
Figs. 38–42. *Sticta damæcornis*, Ach.
var. *sub-caperata*, Nyl.
38. Portion of thallus; nat. size.
a. Apothecia.
b. Spermogones.
39. Same spermogones, considerably magnified.
40. Diagrammatic section of ditto, considerably magnified.
41. Spores; *a*. Mature.
b. Old, beginning to germinate.
42. *a*. Spermatia.
b. Sterigmata.

Plate XXX.

Figs. 1–7. *Sphæria Lindsayana*, Curr.
1. Portion of leaf of *Phormium tenax*, Forst., bearing the perithecia of the *Sphæria*; nat. size.
a. Larger, segregated forms of perithecia.
b. Smaller, crowded forms.
2. Perithecia; greatly magnified.
3, 4. Diagrammatic sections of ditto, greatly magnified.
5. Normal spores, magnified 425 d. l.
a. Young.
b. Mature.
c. Old.
6. Abortive spores.
7. Stylospores.
Figs. 8–15. *Sphæria Otagensis*, Linds.
8. Perithecia and spermogones, slightly mag.
9, 10, 11. Do. greatly mag.

12, 13. Diagrammatic sections of ditto.
14. Spores; *a.* Young.
b. Mature.
c. Old, in process of germination.
15. Sterigmata and spermatia.

Figs. 16, 17. *Patellaria atrata*, Fr.
16. Section of hymenium.
a. Theca.
b. Oil globules.
c. Paraphyses.
d. Hypothecium.
17. Spores; *a.* Young.
b. Mature.
c. Old.

Figs. 18–22. *Sphæria Martiniana*, Linds.
18. Perithecia; nat. size.
19. Do. magnified.
20. Diagrammatic section of ditto, considerably magnified.
21. Theca, with its spores.
22. Spores; *a.* Young.
b. Mature.
c. Old.

Figs. 23–28. *Microthelia perrugosaria*, Linds.
23. Portion of thallus of *Placopsis perrugosa*, Nyl.; showing its apothecia bearing the perithecia of the *Microthelia;* somewhat magnified.
24. Section of one of same apothecia; considerably magnified.
25, 26. Diagrammatic sections of apothecium of *Placopsis* and perithecia of *Microthelia* variously (but greatly) magnified.
27. Section of hymenium of *Microthelia.*
a. Paraphyses—Coloured by iodine.
b. Hypothecium.
cd. Two forms of thecæ.
28. Spores; *a.* Young.
b. Mature.

Figs. 29, 30. *Placopsis perrugosa,* Nyl.
29. Theca, with its spores—under iodine.
30. Spores; *a.* Young.
b. Mature.

Figs. 31–34. *Microthelia Cargilliana*, Linds.
31, 32. Apothecium of *Parmelia perforata*, Ach.; bearing the perithecia of the *Microthelia*; nat. size.
32. Section; *a.* the central perforation.
33. Diagrammatic section of same apothecium and perithecia; greatly magnified.
34. Spores; mature.

Fig. 35. *Parmelia perforata*, Ach.
Spores; *a.* Young.
b. Mature.

Figs. 36–41. *Phymatopsis dubia*, Linds.
36. An apothecium of *Usnea barbata,* Fr. var. *florida,* L.; bearing the parasitic spermogones; somewhat magnified.
37. Section of same apothecium.
38. Sterigmata and spermatia.
39. Portion of a branchlet of *U. barbata,* Fr. var. *ceratina,* Ach.; bearing the parasitic perithecia and maculæ; somewhat magnified.
40, 41. Sections of same branchlet maculæ (40), and perithecia (41).

Fig. 42. *Usnea barbata*, Fr. var. *ceratina.* Ach.
Spores; discoloured and abnormal.

Fig. 43. *U. barbata*, Fr. var. *florida,* L.
Spores—normal; *a.* Young.
b. Mature.

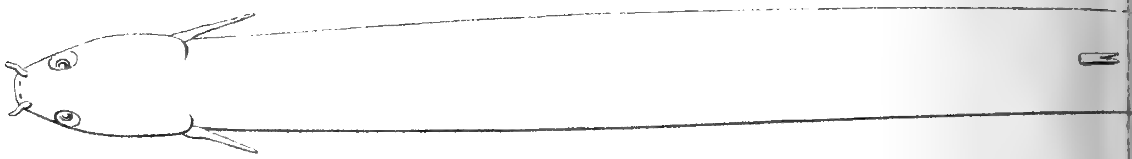
Figs. 44–46. *Microthelia Ramalinaria*, Linds.
44. Portion of a branchlet of *Ramalina calicaris*, Fr.; black-punctate, with the perithecia of the *Microthelia* (*b*); *a.* apothecium of the *Ramalina;* all somewhat magnified.
45. Section through the epidermis (cortical layer) of same branchlet, and through perithecia of the *Microthelia;* considerably magnified.
46. Section through one of same perithecia; greatly magnified.

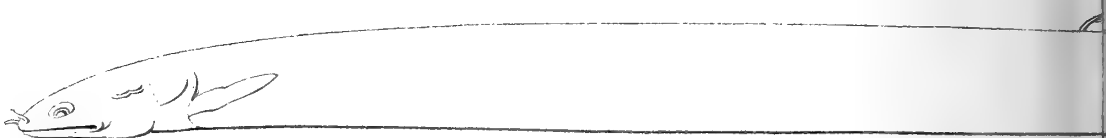
Figs. 47–52. *Celidium dubium*, Linds.
47–49. Portions of thallus (all somewhat magnified) of *Sticta granulata*, Bab. (47); *S. fossulata*, Duf. (48, 49).
48. More fossulate, brown form.
49. Smoother, green form.
50–52. Sections of thallus of the *Stictæ* and of the parasitic maculæ and perithecia; all considerably magnified.
50. *S. granulata*, Bab.
51, 52. *S. fossulata*, Duf.

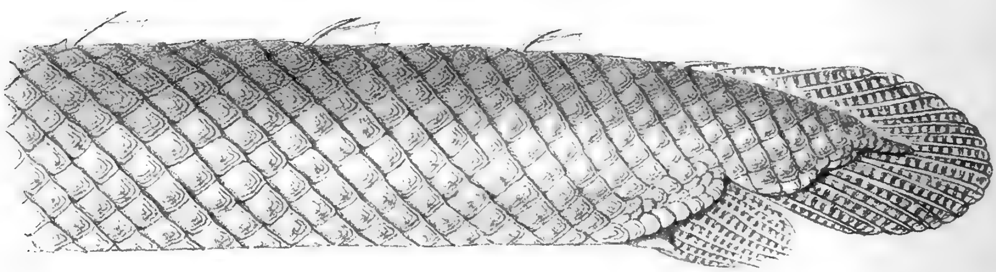
Figs. 53–60. *Nectria Otagensis*, Curr.
53. Agglomerated perithecia; nat. size.
54, 55. Do. more or less magnified.
56, 57. Sections of do., considerably magnified.
58. Perithecia; mature; viewed from above.
59. Do. old and empty; viewed from above.
60. Spores; *a.* Young.
b. Mature.
c. Old.

Figs. 61–74. *Æcidium Otagense*, Linds.
61–68. On *Clematis hexasepela*, DC.
69, 70. On *Epilobium junceum,* Forst.
71–74. On *Microseris Forsteri*, Hook. fil.

61. Less than nat. size.
  a. Flower and flower-stalk of *Clematis* affected by the parasitic *Æcidium.*
  bc. Normal flower and flower-stalks.
62. Nearly nat. size.
  a. Deformed perianth of *Clematis.*
  b Normal anthers and stamina.
  c. Deformed flower-stalk.
63. Leaflet of *Clematis;* about nat. size; Peridia of *Æcidium*, somewhat magnified.
64, 65. Portions of flower-stalk (64), and leaf (65), of *Clematis,* with the parasitic peridia; considerably magnified.
66, 67. Sections of peridia of *Æcidium* in different stages of development; considerably magnified.
68. Spores of *Æcidium;* mature.
69. *Epilobium junceum;* bearing the parasitic peridia on its leaves; about nat. size.
70. Portion of one of the leaves of the *Epilobium*, with the peridia considerably magnified.
71. Leaf of *Microseris Forsteri;* bearing the peridia of *Æcidium;* about nat. size.
72. Portion of same leaf and peridia; considerably magnified.
73, 74. Diagrammatic sections of same peridia; considerably magnified.







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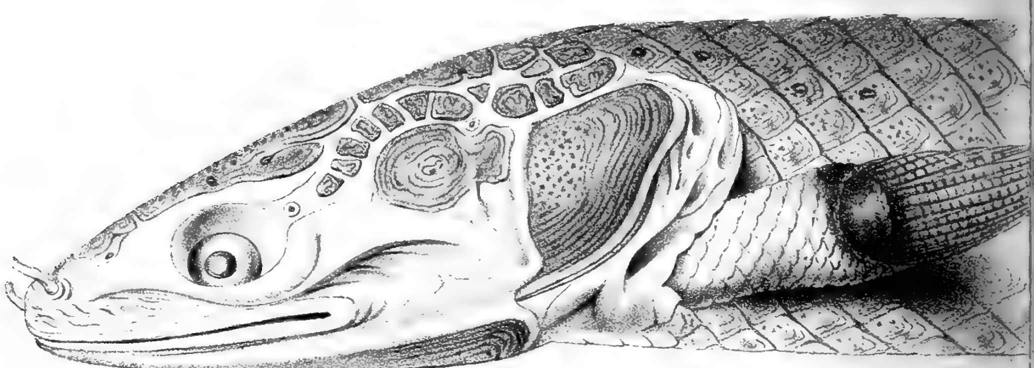

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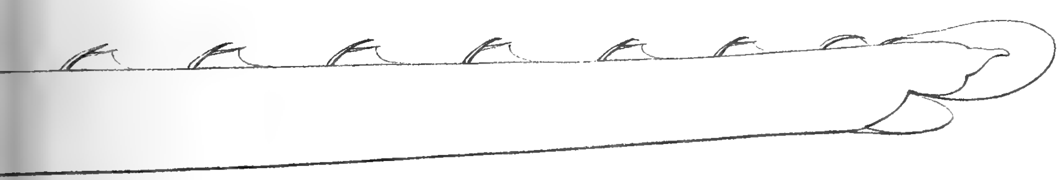

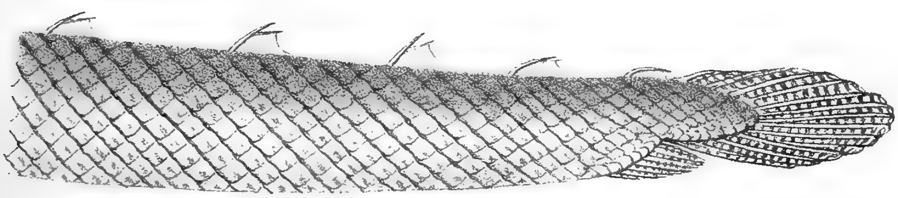

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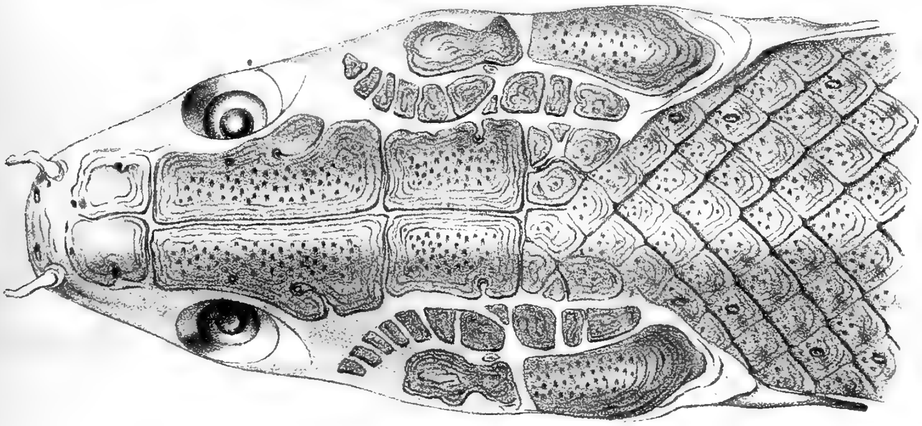

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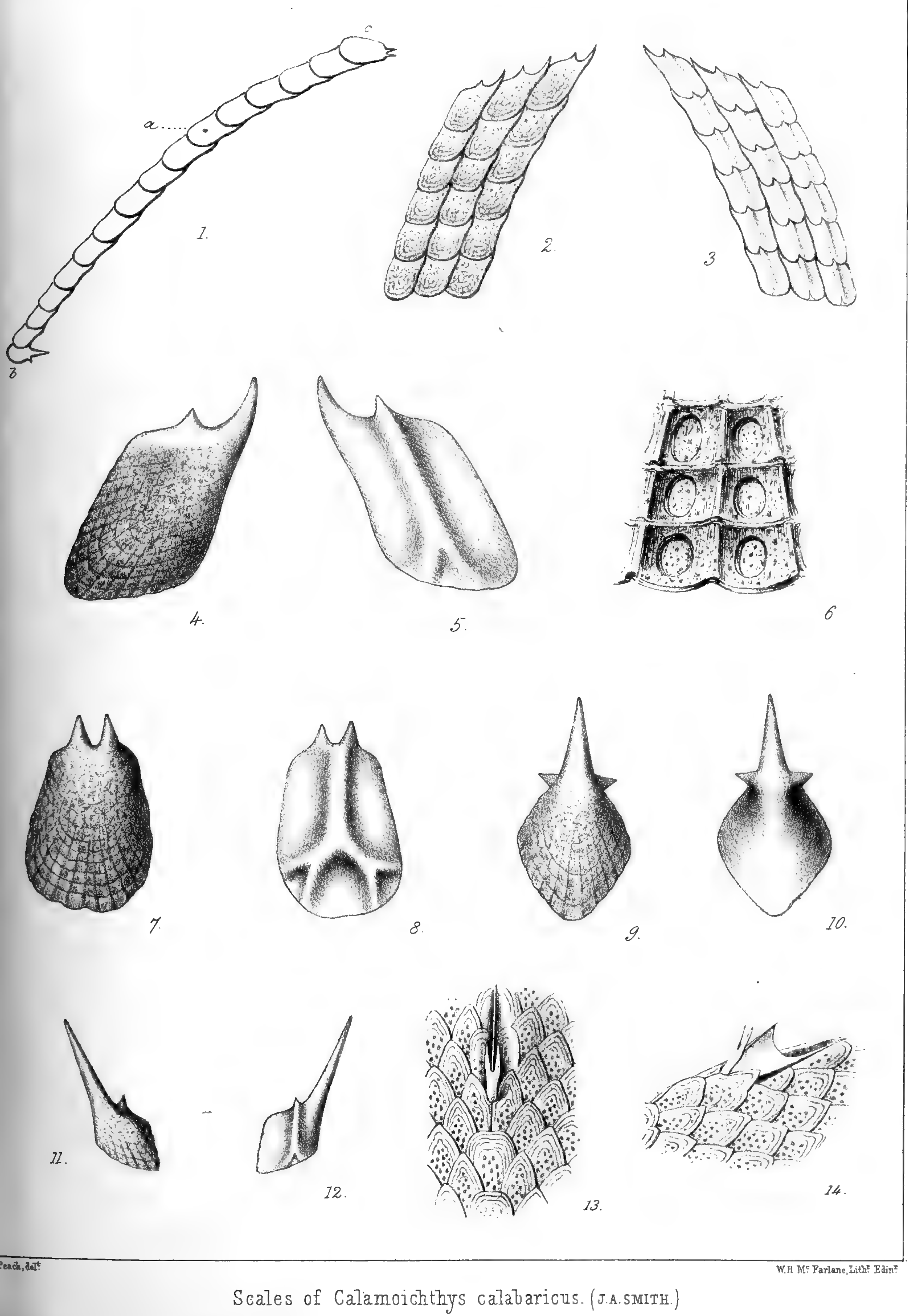
CUS (J. A. SMITH.)



C.W. Peach, del^t
W.H. M^c Farlane, Lith^r Edin^r

Scales of Calamoichthys calabaricus. (J.A. SMITH.)

ERRATUM.

Page 408, *Foot-note*,* *line* 3.

An erroneous definition of the word *Mycology* in M'Nicoll's "Dictionary of Natural History Terms," p. 317, wherein he confounds the older and better known term *Myology* with *Mycology*, has given rise to an inadvertent confusion of the two terms here. The use of the term *Fungology* would render such confusion of words so similar in their spelling impossible. The term *Myology* is properly defined in Noah Webster's, and other standard *English* Dictionaries; but M'Nicoll omits the word altogether, though he includes its definition under the term *Mycology;* an error both of omission and commission scarcely pardonable in a Dictionary exclusively of *scientific terms!*

XXXI.—*Description of* CALAMOICHTHYS, *a new Genus of Ganoid Fish from Old Calabar, Western Africa; forming an addition to the Family* POLYPTERINI. By JOHN ALEXANDER SMITH, M.D., F.R.C.P.E., F.R.S.E. (Plates XXXI. and XXXII.)

(Read 16th April 1866.)

In the beginning of the year 1865, I received from the Rev. ALEXANDER ROBB, one of the missionaries of the United Presbyterian Church, residing at Creek Town, Old Calabar, Western Africa, a package containing some zoological specimens; preserved in spirits; and in a letter dated 28th October 1864, referring to its contents, he states, there "are also two or three small eel-like fishes." Some time passed before I was able carefully to examine the package, and I was then at once attracted by these small fish, which I saw belonged to the very distinct and interesting Order of the Ganoid fishes, so abundant in a fossil state in the rocks of our earlier geological epochs, but of which so few representatives are now to be found as living inhabitants of the present waters of our globe.

The specimens of the fish sent were unfortunately very imperfect, the body of each having been torn across, apparently about the anal region, towards its caudal extremity, which was wanting; so that some of the characteristic details of the fish could not be ascertained.

The fish seemed to be closely allied to the genus *Polypterus*, first described in 1802 by Geoffroy-St-Hillaire. The genus *Polypterus* is believed to be the only living representative of a very ancient group of Ganoid fishes, included by Professor HUXLEY in his SUBORDER CROSSOPTERYGIDÆ, whose fossil remains are principally found in the Palæozoic rocks; and some of the Families of the group, to which, indeed, *Polypterus* seems most nearly allied, belong exclusively to rocks of the Old Red Sandstone or Devonian age.

I may remark in passing, that the true Ganoids (whose bodies are completely covered with Ganoid scales) of the present day, appear to be all inhabitants of the fresh waters of the globe. The various species of the genus *Polypterus* are found in the great rivers of the African Continent, and all the species of the other allied genera—those of *Lepidosteus* and *Amia*—inhabit the fresh waters of North and South America. While of the other Ganoids (whose bodies are only partially covered with ganoid scutes or plates), one genus, *Scaphirhynchus*, is found in the rivers of North America; and another, the genus *Accipenser*, the Sturgeon, inhabits the salt waters of the globe, and seems only at particular times to ascend the fresh waters or rivers. It would be an interesting subject of inquiry to learn whether the different Orders and Genera of the Ganoids of the ancient world, those found fossil in our rocks, and taken cognisance of by the geologist, bore any similar relation to the rivers and their estuaries, and to the seas or salt waters of the Pre-Adamic earth. Judging of the past by the analogy of the present, it might almost be expected that the ancient Ganoids would bear a somewhat corresponding relation to the fresh and salt waters of their day, as is known to be the case with their living representatives in ours. Accordingly, it does certainly appear, in this country more especially, that numerous fossil remains of Ganoid fish have been found in formations that seem to point to a fluviatile, or at least an estuarine character and origin; and should there be any truth in the analogy, the presence of these fish would form a valuable additional element towards determining the character of any rock formation.

Only some six species of *Polypterus* have as yet been observed, and of these four belong, it is said, to the river system of the Nile; another to Senegal and proceeding still farther south along the western coast of Africa, we find a sixth species, which has been discovered in the rivers of Cape Palmas.

The specimens of fish sent from Old Calabar seemed, however, to differ considerably from all the species of the genus *Polypterus*, in various important characters to be afterwards detailed, and especially in the very elongated form of the body, and in the apparent total absence of ventral fins, which, however, from the imperfect state of the specimens, could not be conclusively determined.

While waiting, therefore, for more perfect specimens to settle definitely the question of the absence of the ventral fins, I exhibited the mutilated fish at a meeting of the Royal Physical Society, held on the 22d March 1865, and stated that all the species of the genus *Polypterus* seemed to bear a close resemblance to each other, in the general form of their comparatively short and fish-like body, and in the presence of ventral fins, forming thus a very natural group or genus. While the new fish, with its much elongated and more cylindrical form of body, and apparently the entire absence of ventral fins, suggested at least the probability of the existence, nearer the equator, of another allied but distinct group of these African fish. I would therefore place this fish provisionally in a new genus, which, from its general reptile or serpent-like aspect and form, I would designate *Erpetoichthys** (Ερπετὸν Ιχθύς)—the reptile or serpent-fish; and following the example of those Naturalists who have given the name of the locality where the fish was taken, to some of the species of *Polypterus*, I gave to this new fish the specific name of *E. calabaricus*.

I wrote to the Rev. Mr Robb, informing him of the very interesting nature of

* I have since learned that this designation, or a closely allied one, has been already used in Ichthyology, and accordingly, on the recommendation of Dr Grunther of the British Museum, I have changed the name of the genus to Calamoichthys (Καλαμος a reed, and Ιχθύς, a fish), which will still bear a relation to the cylindrical character of the fish.—J. A. S.

the eel-like fish he had sent me, and requested him, if possible, to forward perfect specimens, so that its specific or generic differences might be determined and described; at the same time, I asked him to give me what information he might be able to gather as to its natural history. Since that time, accordingly, I have had letters from Old Calabar, and more recently, the perfect specimens of the fish now exhibited. Mr Robb writes from Creek Town, Old Calabar, that the fish is well known there, being occasionally sold in the markets, and eaten by the natives, though not by the most refined. It attains a considerable size, and is an inhabitant of the fresh waters of their brown, mud-laden rivers and creeks, through which, however, the tide flows for a great distance above Creek Town, where, indeed, at high water it rises to a height of seven feet, although Creek Town is some forty or fifty miles from the bar, at the mouth of the great river. The fish is essentially, Mr Robb says, a mud fish, and he considers it strange that he cannot get it in the dry season. It is caught during the rains, which last there from the beginning of June to the end of September. The fish is then to be found in the little fresh-water streamlets that run into the main rivers or creeks, and also in the pools in the marshy lands or mimbo swamps, as they are designated, from a kind of palm wine, or *mimbo*, made from some of the species of palm—the mimbo palms—which grow abundantly in such localities. The fish is most commonly found where the roots of the trees interlace together, and rise out of the water, leaving thus spaces below, through which the stream rises and falls with the ebb and flow of the tide. From the fact of the fish being only found at the season of the rains, it is not unlikely that they come at that time from the deeper parts of the river, to spawn in the shallows and streamlets; and this is also somewhat confirmed by the fact of the ova being abundant, and of considerable size, in some of the female specimens sent to me, which were opened for examination. The Rev. Mr Robb states that the fish sent are good specimens; two of them were brought to him alive in the fish-traps in which they were taken. This trap is a wicker basket, with an entrance made tapering inwards, like a funnel, so that if the fish once goes in, it cannot get out again. The traps are generally baited with a few palm nuts, and then sunk in the water. The natives have, however, many different ways of catching fish. Mr Robb kept the fish, thus caught, alive in water for several days, and noticed the snake-like agility and force of their movements.

Native name.—The fish is named by the natives *ú-nyang*, the literal meaning of which Mr Robb does not know, unless it may signify the struggler, or, to use a Scottish word as more descriptive of this supposed meaning—the "*wambler*." *Nyang-a* is a native verb signifying to struggle for a thing, as when two persons desire possession of one object; while *ú-nyang-a* is a scuffling or struggling for the possession of a thing. The name of the fish *ú-nyang* appears therefore to be a shortened form of the verbal noun; and was probably suggested by the twisting, wriggling, or struggling movements of the fish, as it propels itself through the water or mud of the river.*

On examining the specimens of these fish, sent to me from Old Calabar, it was at once evident, from the total absence of ventral fins, that they entirely confirmed the view I had previously taken of their being new in character, and of the propriety of forming them into a genus distinct from that of the genus *Polypterus*.

Male and female different in appearance.—It was, however, also evident that the fish could be divided into two apparently distinct varieties, suggesting at first the idea that they might possibly be two species belonging to the same genus.

One having the body nearly equal in depth in its whole length towards the anal fin, with a distinct and well-developed anal fin and caudal extremity. While the other had a more tapering form of body posteriorly, and a very small anal fin, so small indeed, that when closed, and lying in its scaly groove, it might almost escape notice altogether.

On making a more careful examination, however, it was found that all the fishes of the first variety were males, and those of the second were females. The fact was therefore forced upon me, that this apparently strange and considerable variety in form was entirely sexual in its character, although somewhat the reverse of what we find in fishes generally,—the females being commonly distinguished from the males by the fuller and deeper form of their bodies,—and, as in all other respects the characters of both fish seemed to agree, that the fish were simply the male and female of the same species.

I also exhibit a specimen of the female of this fish, of larger size than the others, which was, with some other kinds of fish, politely sent to me from Africa, a few months ago, by Mr W. G. MYLNE. These fish are stated to have been all collected by him in the district of the Great Camaroon mountains, on the coast of which the missionary station of Victoria has within the last few years been placed; this new ganoid, therefore, also inhabits the rivers of that still more southerly and little-known region, and, accordingly, this gives us an additional habitat for the fish.

I shall now give a detailed description of the fish, both of the male and female, and shall conclude with a summary of its characters, pointing out its general relations to, and differences from, the allied genus *Polypterus*.

* Mr ROBB has recently sent me the following additional remarks on the native name of this fish:—"Many of the natives call this fish *Nyang;* and this suggests another possible etymology. *Nyang* is the stipule of the leaf of the mimbo palm, which, being of a narrow ribbon shape, dries up and remains twisted round the tree, from year to year, as long as the tree exists. The colour of the dried stipule is like that of the fish; and very possibly either the fish was named from *Nyang* of the mimbo palm, or *Nyang* was named from the fish. In this latter case, which is not at all improbable, the etymology is left undecided, and remains a matter of conjecture."

I. *Description of a Male* Calamoichthys calabaricus :—

Male.—A specimen measuring nearly $12\frac{1}{2}$ inches in total length. (See Plate XXXI. figs. 1, 2, 3, 5, 6.)

Head.—The head is small, being less than one-twelfth of the total length of the body; depressed above, and rounded in front; it bulges out laterally, a little behind the orbits, and contracts again towards the back part, and the narrower neck of the fish. The front of the snout, sides of the head including the eye, and back to the opercular plate, are covered with a soft, smooth skin. On each side of the snout, in front, is a small projecting *tubular cirrus*, forming the *external nasal openings;* these cirri are about $\frac{1}{5}$ of an inch apart, and each measures $\frac{3}{20}$ of an inch in length; at the distance of $\frac{1}{5}$ of an inch behind them, and $\frac{1}{4}$ of an inch from the point of snout the *orbits* are situated, at the junction of the superior and lateral parts of the head. The orbital openings are small, measuring rather more than $\frac{1}{10}$ of an inch across; and from the back part of the orbit to the upper and posterior angle of the opercular plate, it measures $\frac{3}{5}$ of an inch in length.

A *series of bony plates* very similar in their arrangement to those of the genus *Polypterus*, commences a little behind the nostrils, and passing above the orbits, covers the whole upper portions of the head. These plates are arranged in pairs, one on each side of a nearly straight mesial line or suture; they are the exposed portions of the cranial bones, and are sculptured on their surface, like the scales of the body, and are also ganoid in their character. Beginning in front, a little behind the snout, there is, first, a pair of small rhomboidal-shaped plates, the *nasal bones ;* these are rounded in outline in front, lie immediately on each side of the mesial line, and pass backward to the front of the orbits, where they are separated by a transverse wavy suture from the pair of plates immediately above and beyond them. The next plates are the *frontal bones*, the largest of the cranial plates; rather narrow in front, they become gradually broader at the back part of the orbit, and contract again in breadth as they proceed backwards, to meet, by a transverse suture in front of the spiracular openings, the next pair of plates, the *parietal bones ;* which lie immediately behind them in the mesial line. The parietal bones or plates are somewhat quadrate in shape, but longer than broad; they are longer than the nasal plates, but are about a third shorter than the frontal bones, and they terminate posteriorly in a nearly straight transverse suture, which separates them from the small *supra occipital plates* or bones, two in number on each side of the mesial line, which cover the upper and back part of the head, at the nape of the neck. The *internal supra occipital plate*, that next the mesial line, is the longest and largest, being about a third longer than the external plate; it is somewhat rhomboidal in shape, and terminates in a rather rounded posterior extremity. The *external supra* *occipital plate* is smaller in size, and somewhat triangular in shape, the straight base being in front, and the lateral margins terminating in a pointed posterior extremity. Between these supra occipital plates, laterally, and the smaller ossicles, to be afterwards noticed, which run along the sides of the head, there are other two small bony plates, on each side of the nape. One in front, the smaller, triangular in form, is the *epiotic plate* of Professor HUXLEY; it lies to the outside of the external supra occipital plate, and behind it lies the second, the *supra scapular plate*, somewhat larger, and more rhomboidal in shape, its longest diameter being outwards and backwards, it fills up the angular space behind the internal supra occipital plate, and the series of lateral ossicles; completing thus the covering of the upper and back part of the cranium. (Plate XXXI. fig. 6.)

In the mesial line, immediately behind and between the two internal supra occipital plates, lies the first of the true scales of the body on the dorsal surface, from which a row of scales passes the posterior margin of the supra scapular plate, and runs diagonally backwards and downwards along each side of the body.

The bony plates which cover the lateral and back parts of the head, consist, first, of a series of small ossicles, which commence a little behind the orbits, and run in a curved direction upwards and backwards, along the external margins of the frontal and other cranial plates, which have just been described, to the back part of the head. These small plates vary in shape from somewhat quadrate to triangular forms, and they also vary in number on the different sides of the head, being in this specimen ten on the one side and eleven on the other. Two of these small bones—which lie alongside the parietal bones, and behind the extremities of the transverse suture between them and the frontal bones, on each side of the head —have their inner margins free or unattached to the adjoining bones. This enables them to be raised up and opened, like a valve, which communicates with the branchial cavity. They have accordingly been styled the *spiracular ossicles*, which may be a sufficient designation for the whole series—those in front, between them, and the orbit, numbering some four or five bones, have, however, been named the *supra temporal ossicles;* and the plates placed behind these spiracular ossicles, are three or four in number, they continue the series along the upper margin of the operculum, and may therefore be styled the *supra opercular ossicles.* Outside and immediately below the supra temporal and spiracular ossicles the front of this series of small bones, and at a little distance behind the orbit, there is a larger, somewhat oval-shaped plate, the *preoperculum* of AGASSIZ, which runs backwards nearly to the line of the posterior margin of the parietal bones; and immediately behind it lies the still larger and broader *operculum,* an irregularly four-sided plate of bone, its anterior and superior margins being nearly straight, its posterior by far the shortest, and its lower and external margin much rounded in its outline. Below these plates the sides of the head are simply covered with soft and smooth skin, which expands into a free margin behind the operculum, becoming much broader below, to join the back part of the jugular plates. (Plate XXXI. fig. 5.)

The branchiostegal membrane, between the rami of the lower jaw, is almost completely occupied by a pair of large flattened plates or rays—the *jugular plates*, as they have been named—the form of each being somewhat like a scalene triangle, with its acute point in front, and its longest side next the ramus of the jaw, the second lying parallel to the corresponding side of the other jugular plate, and the third or shortest and posterior side being somewhat rounded in its outline.

All the plates of the head, as well as the jugular plates, are sculptured, showing slight projections with rounded outlines, on their surface, which generally towards the margins of the plates assume an arrangement of concentric lines, and these lines commonly enclose a space in the centre, or towards one extremity of the plate, which is filled up with shorter and more irregularly arranged projections.

The *Mouth* is large. Its opening extends horizontally backwards beyond the back part of the orbit. The jaws are nearly equal in length, the upper projecting very slightly beyond the lower. The lips are full, and expand in a free membrane, from behind the nasal cirri, which becomes narrower towards the angle of the mouth; and again expands along the sides of the lower jaw. A series of strong, conical, and pointed *teeth*, sloping slightly backwards, extends along the edges of both upper and lower jaws; and immediately within this row, there is in the upper jaw a smooth furrow or groove, which seems to receive the teeth of the lower jaw when the mouth is closed. Beyond this groove in the upper jaw is a patch of villous-like teeth, which is broadest anteriorly ($\frac{1}{20}$ of an inch), and crescentic in shape, the horns projecting backwards; it covers the inner portion of the superior maxillary bones, continuing on to the palatines, the toothed surfaces becoming granular in character. The internal pterygoid plates, and the vomer in the centre of the roof of the mouth, are also all covered with granular teeth. Within the lower jaw anteriorly is a crescentic patch of granular teeth, which also measures $\frac{1}{20}$ of an inch across in front, and extends backwards in a line of teeth along the inside of the rami of the jaw; between this accessory group of teeth in front, there also runs a fainter smooth line or furrow, somewhat similar to that of the upper jaw.

The *Tongue* is full and rounded, and covered with papillæ, a deep fold or notch, on each side, partially divides it into two portions; and its base is covered with a patch of granular teeth.

Body.—The body is much elongated, its depth being about $\frac{1}{24}$ part of the total length of the fish; anguiform (cylindrical from behind the pectoral fins, to about the middle of its length a little behind the commencement of the dorsal finlets; it then becomes gradually more compressed in character (laterally), but remains of nearly the same depth (vertically), tapering very slightly towards the anal fin, behind which it diminishes in depth to its caudal extremity). The caudal extremity is short, and tapers rapidly to a thin and conical point, around which the caudal fin-rays spring.

Scales.—The scales are osseous, and ganoid in character, and generally rhomboidal in shape; they are arranged in a regular series of rows, which cover the whole body. On the back each row begins in the median line, and runs obliquely downwards and backwards along both sides, until it terminates in the median line of the abdomen, and thus completes the circuit of the body. These rows are formed throughout of a single series of scales; in this specimen, however, there is a solitary exception to the rule, which occurs opposite to the commencement of the anal region, where two smaller and separate rows on the back run about half way down each side of the body, and then join in a single row below; this is the only exception to the rule which I have yet noticed.

The scales behind the head are small in size, and they gradually become larger as they proceed backwards over the body (where the rows of scales measure above $\frac{1}{10}$ of an inch across); they again, however, diminish in size over the caudal extremity of the fish. The scales also vary somewhat in size and shape in different parts of the same series, being slightly larger towards the lateral line of the body.

The general form of each scale is rhomboidal; a small tooth-like process projects forwards from the middle of its upper or anterior margin, and a lengthened lateral tubercle also projects outwards and forwards, from the inner and anterior angle of the scale. The anterior or upper edge of a scale, and one of its sides, the inner, that from which the projecting tubercle springs, are gently sloped or beveled, so as to pass slightly under the adjoining scales; its outer or free margin is nearly straight, and its posterior or lower margin is gently rounded in its outline. The under surface of each scale has a strong bar running down its centre; it begins anteriorly in the pointed process, or tooth of the scale, and posteriorly it divides into two branches, to accommodate the tooth, or anterior process, of the adjoining scale, which is laid in this space or socket, left at its posterior termination; the larger lateral process at the same time turning up along the under surface of the posterior and internal angle of the scale in front. The mesial scale of each series, on the dorsal surface, up at least to the first dorsal finlet, is oval in shape, the narrower anterior extremity being forked, having two anterior teeth lying side by side, which fix it to the connecting tissue and scales in front; and the longitudinal central bar on its under surface divides into two branches posteriorly, which run out laterally to the posterior edges of the scale, and each of these lateral terminations of the bar receives the tooth of the anterior or upper margins of the two adjoining scales—the first of the lateral series, and their tubercles lie along, and below its surface. The scales next in order, on each side, have their anterior teeth laid in the forked terminations of the longitudinal bars of each of the scales in front and above them, their lateral tubercles being placed below the posterior and internal angles of these scales; and this arrangement is continued throughout the whole series, until we reach the mesial scale of the abdomen below, which is somewhat of a battledore shape, having a rounded body posteriorly, and a long projecting point, or handle, in front; about the junction of this handle to the body of the scale, are two small teeth-like processes projecting outwards and forwards, one on each side of the handle; and the scale is placed like a wedge between the adjoining scales, so that each tooth lies at the posterior extremity of the central bar of each of the scales in front of it; the series, or elongated loop of scales, which surrounds the body, being thus firmly and beautifully locked together and completed by the addition of the connecting tissue; so that it is no easy matter to separate one scale from another. The number of scales belonging to one of these loops or series is about thirty, or sixteen scales, including those of one side of the body only, and the median dorsal, and abdominal scales; the whole series, their free margins forming projecting lines, bend gently in an ogee-like manner, and run backwards and downwards across the sides of the fish, at an angle of about 45 degrees.

The scales generally are thin, especially towards their lateral and posterior margins, where they are almost semi-transparent or translucent, so that, when the specimen is moist in spirit, the toothed arrangement of the scales can be easily detected by the eye, and from the white colour of the fibrous attachments of the teeth and edges of the scales, a finely mottled character is given to the colour of the fish. These teeth-like processes appear to be present on most of the scales, until you reach the posterior parts of the body at its caudal extremity, where they seem to diminish considerably in size and in distinctness of character. (Plate XXXII. figs. 1–12.)

Beyond the origin of the first dorsal finlet, the oval-shaped scales in the dorsal mesial line are wanting, the bony spines of the finlets, as it were, taking their place; and the adjoining lateral scales on each side of the mesial line are laid together in a somewhat alternating manner; the sockets for the base of the osseous spines of each of the dorsal finlets being deeply cut out of the posterior margins of these scales, so as to give a shoulder or fixed support for the spine to lean against, when the finlet is raised up. (Figs. 13, 14.)

There are in all about 107 distinct rows of scales, between the nape of the neck and the extremity of the body; some 8 or 9 shorter series or rows being added in front, on the under surface of the body, to complete its scaly covering below, making altogether about 116 series of scales. Taking the number of the rows of scales (107) on the dorsal aspect; we find there are 50 series or rows between the nape of the neck and the scales forming the socket at the base of the spine of the first dorsal finlet; 8 series of scales lie between that and the second dorsal spine; 6 between the second and third dorsal spines; 5 between the third and fourth; 6 between the fourth and fifth; 6 between the fifth and sixth; 6 between the sixth and seventh; 5 between the seventh and eighth; 5 between the eighth and ninth; 4 between the ninth and tenth; and 6 shorter rows between the tenth dorsal spine and the extremity of the body.

Microscopic character of scales.—The upper surface of each scale is shining, and sculptured in a manner somewhat similar to the plates of the head. Under the microscope the sculpturing of the scales shows numerous flattened disks, or projections of rounded outline, which are irregularly disposed towards the centre of its surface, but become more regularly arranged in a concentric manner towards its free margins; the anterior and internal beveled margins of the scale being nearly smooth. With a stronger microscopic power, the surface exhibits the raised and rounded portions alternating towards the margins, with intervening narrow and cylindrical-like projections, radiating outwards. The whole surface of the scale is found to be also covered with minute perforations or pores, apparently communicating with the cellular interior of the scale; while interspersed among these pores, there occur a smaller number of perforations or pores, of a larger size. (Fig. 6.)

I am indebted to my friend Mr Charles W. Peach, the well-known naturalist, for the correct and careful drawings exhibited, which display these various details in the arrangement and structure of the scales. (See Plate XXXII.)

Lateral line.—The lateral line of the body is shown by a series of small mucous pores or openings, which appears in the scales (about the third, or fourth, from the mesial line of back), and runs in a line from behind the posterior angle of the operculum, to the middle of the caudal extremity. Occasionally, one or two scales in this line are apparently passed over, having no pores on their surface; and the series then goes on again after this interrupted manner; sometimes, however, the pores appear in the scales immediately over those in the line which appear to be passed over. Besides these pores, there are various others scattered apparently among the scales over the upper parts of the body of the fish, and occasionally even in the median scales of the back.

The fins.—The fins generally are small in proportion to the size of the fish.

The *Pectoral fins* arise immediately below the operculum, and are obtusely lobate in character, being connected to the body by a pedicle or arm-like process (as in *Polypterus*), which terminates in a gently curved distal extremity, from which the fin-rays spring, to form a rounded fin—the central rays being the longest. The base of the pedicle is covered on its external surface with small ganoid scales, apparently laid simply side by side, in rather irregular rows; these terminate in a nearly straight line across the middle of the pedicle, leaving an oval space bare, or free from scales, at the base of the fin-rays; the scales at the edges of the pedicle, however, pass backwards to the base of the fin-rays, and a few are scattered along the base of the fin-rays themselves. The pedicle is smooth, having no scales on its inner surface, next the body of the fish. There is a roundish, dark, or black spot on the distal extremity of the pedicle, and partly also on the base of the fin-rays. The fin measures $\frac{1}{2}$ of an inch from the base of the pedicle to the extremity of its central fin-rays; the pedicle measuring $\frac{1}{5}$ of an inch, and the longest fin-rays $\frac{3}{10}$ of an inch in length. The breadth across the pedicle is nearly $\frac{3}{20}$ of an inch. The fin-rays are seventeen in number, and soft in character; they are, however, covered with single rows of very minute ganoid scales, which are placed, side by side, along the rays, and extend nearly to the membranous extremities of the fin-rays.

The *Dorsal finlets* are ten in number, and resemble in general character those of *Polypterus*, but they are each quite distinct and separate from the one next it. They commence about the middle of the fish, or a little behind it. In this specimen the first finlet lies at the distance of about $5\frac{5}{8}$ inches from the front of the snout; and the succeeding finlets are placed at short intervals from each other, backwards to the caudal extremity of the fish, the last being attached to the commencement of the fin-rays at the base of the tail. The intervals between the different dorsal finlets generally diminish gradually in this specimen as they proceed backwards towards the caudal extremity: thus the distance between the first and second finlets is $\frac{9}{10}$ of an inch; between the fourth and fifth finlets $\frac{7}{10}$ of an inch; and between the ninth and tenth finlets only $\frac{3}{10}$ of an inch.

Each finlet lies in a lengthened, somewhat triangularly-shaped, pit or longitudinal depression, which occupies the median line of the adjoining rows o scales, including one or two rows, and sometimes passes on to the third row of scales; and only about half, or even less, of this depression is occupied by the bony spine of the fin. When the fin is raised, the front of it is seen to be supported by this strong, bony ganoid spine, granulated or sculptured on its upper or anterior surface in a somewhat corresponding manner to the scales of the body, it is more sculptured in front, and becomes smoother towards the forked posterior extremity of the spine; the sharp points of the forks are nearly equal in length, the forked part occupying about a fourth or so of its length. A single soft fin-ray, springing from the root of the forked part of the spine, passes backwards to support the upper margin of each soft membranous fin. The last spine is attached to the base of the tail, and the ray is hard in character, and resembles those of the tail. When a finlet is closed, the soft ray and membrane lie in the longitudinal depression, and are covered and protected by the hard osseous spine which lies over them; its upper and sculptured ganoid surface being on a level with the rest of the dorsal surface of the fish. The spines vary slightly in length, being longest about the middle of the series of the finlets, and diminishing gradually towards each extremity; they measure about $\frac{3}{20}$ of an inch in length, the soft ray about $\frac{1}{10}$ of an inch, and the membranous finlet along its base, including the thickness of the bony spine itself, about $\frac{3}{20}$ of an inch.

The *Ventral fins* are entirely wanting.

The *Anal fin* is placed immediately behind the anal opening, and close to the caudal extremity of the fish. Its base is marked by a loose flap or fold of the skin along each side of the body, which is covered by a distinct series of large oblong-shaped scales, some twelve or thirteen in number, laid side by side; and within this fold the closed fin lies as in a protecting case or sheath.

At the anterior part of the base of the fin there is a somewhat triangularly-shaped and thickened portion, the broad base of the triangle being next the body, and its point extending upwards in a fleshy or membranous skin, along the anterior margin of the fin; this triangular portion of the fin is covered by a group of scales, a patch of smaller ones in the middle, being surrounded by a row of larger scales outside.

The fin-rays are hard, and are thirteen in number; they consist of eight large, broad, and pointed rays, each closely covered with a row of ganoid scales; and of five smaller fin-rays posteriorly, more thinly covered with scales; and a membranous edge or border of the fin extends beyond the rows of small ganoid scales which cover each of the rays. You have, therefore, in this fin, first, the triangular base, with its extension alongside and in front of the fin-rays, then the group of eight large and strong fin-rays placed closely together, and completely covered with closely-set ganoid scales, the first ray being very large and broad; and next to this there is the group of slender, softer, and more separated or secondary rays, with their smaller rows of ganoid scales; the membranous extension of the rays forming a soft border to the fin all around. The length of the fin along its base is $\frac{2}{5}$ of an inch, and the length of its fifth or longest central fin-ray is also about $\frac{2}{5}$ of an inch; and its rays extend considerably beyond the base of the caudal fin-rays.

The *Caudal extremity* of the fish, from anus to extremity of caudal fin-rays, is $1\frac{1}{20}$ inch in length; it is therefore rather more than $\frac{1}{11}$ of the total length of the fish. It is much compressed laterally, and somewhat triangular in form; and the fin arising around its extremity is homocercal in character, at least in its general contour; the form of the tail being somewhat oval or rounded at its distal extremity; the fin-rays are large, strong, distinct, and covered with rows of ganoid scales; the anterior rays being shorter and somewhat fringe-like, especially above; those next the centre divide at about the middle of their length into two terminal rays, which again subdivide, and along which the ganoid scales are continued in a smaller series, to their distal extremities. The rays are all connected together by membrane, which is most distinct between the more separated rays at the commencement of the fin, on the dorsal surface of the caudal extremity. The fin-rays are thirteen or fourteen in number, the length of the central rays, which spring from the termination of the vertebral axis, is $\frac{2}{5}$ of an inch, and the greatest breadth across the fin-rays is $\frac{2}{5}$ of an inch.

A sharp pointed style, or free bony process, covered with a continuation of the scales of the body, projects outwards for $\frac{1}{5}$ of an inch to the left side of the centre of the fin-rays of the tail; seven rays rising above, and six rays below, the line of its projection. This appears to be the termination of the vertebral column prolonged beyond the body, and lying among the fin-rays of the tail. It reminds one of a somewhat analogous but much greater extension of the vertebral column, prolonged, indeed, through the caudal rays altogether, and even bearing additional fin-rays of its own, which exists in various extinct fossil fishes. (Plate XXXI. fig. 3.)

The *Colour* of the fish is a uniform purplish brown above, shading off on the sides, and below, to a yellowish white; and the fish, when preserved in spirits, shows a faintly mottled appearance over the body, due to the whitish edges or lines at the junction of the scales shining through the adjoining scales.

The fins are like the body in colour, the scaly fin-rays being also dark, but the margins of the fins are generally lighter; the base of the pedicle of the pectoral fin is light in colour like the belly of the fish, and there is a black spot on its distal part, and at the base of its fin-rays, which are of a brown colour.

The measurements of this male fish are as follows:—

Length, or Longitudinal Measurements.

Total length, from point of snout to the extremity of caudal fin-rays, rather more than $12\frac{1}{4}$ inches.

Do. to caudal extremity of body, $11\frac{18}{20}$ inches.

Of head, above, from snout to commencement of scales of body, $\frac{16}{20}$ of an inch.

Laterally, from tip of snout to superior and posterior angle of operculum, $\frac{19}{20}$ of an inch.

From point of snout to base of first dorsal finlet, $5\frac{5}{8}$ inches.

From base of first dorsal finlet to extremity of caudal fin-rays, $6\frac{7}{8}$ inches.

From point of snout to anal opening, immediately in front of base of anal fin, $11\frac{1}{5}$ inches.

From anal openings to extremity of caudal fin-rays, $1\frac{1}{20}$ inch.

Of central caudal fin-rays from extremity of body (not including style), $\frac{7}{20}$ of an inch.

Breadth, or Transverse Measurements.

Of head across the widest part, over opercular plates, rather more than $\frac{1}{2}$ of an inch.

Of body behind the origin of the pectoral fins, $\frac{2}{5}$ of an inch.

Of body at the distance of one inch behind the pectoral fins, $\frac{11}{20}$ of an inch.

N.B.—The body continues of the same breadth onwards to beyond the first dorsal finlet, after which it gradually diminishes towards the caudal extremity.

Of body at the second dorsal finlet, about $\frac{1}{2}$ of an inch.

,, at the sixth dorsal finlet, about $\frac{7}{20}$ of an inch.

,, at the ninth dorsal finlet, about $\frac{3}{20}$ of an inch.

Of caudal extremity, $\frac{1}{10}$ of an inch.

Depth, or Perpendicular Measurements.

Of head, at pre-operculum, $\frac{7}{20}$ of an inch.

Of body, at the origin of pectoral fins, $\frac{9}{20}$ of an inch.

,, at the distance of one inch behind the pectoral fin-rays, $\frac{1}{2}$ of an inch.

,, at first dorsal finlets, $\frac{1}{2}$ of an inch.

,, at second dorsal finlets, $\frac{1}{2}$ of an inch.

,, at sixth dorsal finlets, about $\frac{1}{2}$ of an inch.

,, at ninth dorsal finlets, $\frac{9}{20}$ of an inch.

,, at posterior margin of anal fin, $\frac{1}{4}$ of an inch.

Greatest breadth across caudal fin-rays, $\frac{2}{5}$ of an inch.

Additional Specimens of the Male Fish, and Varieties in Character.

Six other specimens of the male fish were examined; they varied in length from about 8 to 10 inches. They also varied in some of their minor characters.

Dorsal finlets.—The dorsal finlets ranged from nine to eleven in number, and this variety occurred independently of the length of the fish; the longest fish not having necessarily the greatest number of dorsal finlets. They also differed, in the relation of the last dorsal finlet, to the caudal fin-rays; in some cases it was attached by its finlet to the base of these fin-rays, and in others it was quite distinct from them; and this last dorsal finlet had in some cases a hard fin-ray, like those of the tail, and in other cases a soft one, like those of the other dorsal fins. The dorsal finlets in these specimens varied also in their distance from one another; in some the distance between the finlets gradually diminished from before backwards, and in other specimens, again, the finlets at each extremity of the body were closer to one another than those occupying the middle of the range. When the finlets are more numerous, they rise nearer to the head of the fish than in those specimens in which they are fewer in number; thus, in a fish measuring $9\frac{4}{10}$ inches in length, with eleven dorsal finlets, the first one is at the distance of $3\frac{8}{10}$ from the point of thesnout. Any specific character of this fish, derived from the dorsal finlets, will therefore require to allow a considerable variety both in their number, their relations to one another, and also to the caudal extremity.

Scales.—The scales on the body are arranged in a series of single rows regularly succeeding one another, over the whole body; the only exception I have noticed being in the large specimen already minutely described, and it is probably simply an individual peculiarity. In some specimens one or two of the mesial dorsal scales vary a little in form, being sometimes larger, and having a projection to the right, or to the left side, so as to include and cover the space commonly occupied by the first lateral scale. These occur irregularly along the mesial line, and sometimes one or two mesial scales are absent altogether.

Caudal fin.—The caudal fin also varied considerably in shape in different specimens, being much rounded at its extremity in some fishes, and in others rather acutely pointed. The fin-rays also appear to vary slightly in number.

No specimen, however, except the large one described, showed the projection of the termination of the caudal extremity of the vertebral column, in the form of a distinct style or process; but in some of the others there was noticed a fulness or thickness in the centre of the tail, where the prolongation may be covered and concealed by the scales at the base of the tail, and by the caudal fin-rays themselves.

Some of these peculiarities may be noticed a little more in detail in referring to these additional specimens of the fish :—

1. The first had only nine dorsal finlets, the last finlet with the hard fin-ray, as in the fish already described, being wanting.

2. The second, measuring $9\frac{1}{2}$ inches in length, the head $\frac{7}{8}$ of an inch, has the same peculiarity as the last, the finlets being nine in number.

3. The third has eleven dorsal finlets; the last is distinct, or not attached to the base of the caudal fin, and its small fin-ray is soft in character.

4. The fourth has also eleven dorsal finlets, the last finlet being also distinct, or separated from the caudal fin, and the fin-ray soft. It measures $9\frac{2}{5}$ inches in length, and the first dorsal finlet is $3\frac{4}{5}$ inches distant from the point of the snout.

5. The fifth has eleven dorsal finlets, the last is attached to the base of the caudal fin-rays, and its fin-ray is soft. It measures $9\frac{1}{4}$ inches in total length. The head measures laterally rather less than $\frac{3}{4}$ of an inch.

6. The sixth has ten dorsal finlets, the last being attached by its finlet to the base of the caudal fin-rays. It measures $8\frac{1}{4}$ inches in length, and the head laterally, rather less than $\frac{3}{4}$ of an inch.

7. The seventh has ten dorsal finlets, the last close to the base of the caudal fin-rays, and its ray soft. The tail is pointed.

8. The eighth has eleven dorsal finlets, the last being close to, but free from, the base of the caudal fin-rays, and the ray is soft. Tail rounded. It measures $9\frac{1}{2}$ inches in length.

9. The ninth has eleven dorsal finlets, the last attached to the base of the caudal fin-rays, and the ray hard. It measures $10\frac{1}{4}$ inches in length.

II. *Description of a Female* Calamoichthys calabaricus :—

Female.—A specimen, measuring $9\frac{1}{2}$ inches in total length (including caudal fin-rays).

Head, like the male.

Body, relatively more slender and pointed posteriorly, towards its caudal extremity.

Fins.—Fins small :—

The *Dorsal finlets* like the male, but eleven in number, the last being attached to the base of the caudal fin-rays; each finlet has a single soft ray, with the exception of the last, which is hard, like the caudal fin-rays. The interval between these different finlets is widest in the middle of the series of the finlets :— thus, between the first and second finlets, it is $\frac{1}{2}$ of an inch; between the fourth and fifth, rather more than $\frac{1}{2}$; and between the ninth and tenth, it is only $\frac{3}{10}$ of an inch. The osseous forked spines supporting the front of each finlet are about $\frac{3}{20}$ of an inch in length.

The *Pectoral fins* are obtusely lobate, fin-rays seventeen, soft; length of pedicle and fin-rays about $\frac{3}{10}$ of an inch, of which the central fin-rays measure $\frac{3}{20}$ of an inch. The breadth across the pedicle is $\frac{1}{10}$ of an inch.

The *Ventral fins* are wanting.

The *Anal fin* is much diminished in size, as compared with that of the male, and when closed and laid between its scaly longitudinal folds of abdominal skin, it is scarcely perceptible. It is relatively smaller, and also more pointed in form, than that of the male, described at length. The triangularly-shaped group of scales at its base anteriorly is very small. The fin-rays are only eight in number; five of them being larger and more distinct, and covered with rows of small ganoid scales, and three rays being small and less distinct. Its rays extend only to the base of the caudal fin-rays, below; the length of the fifth or longest ray being $\frac{1}{5}$ of an inch, and the breadth across the base of the fin is about $\frac{1}{5}$ of an inch.

The *Caudal* fin is more acute or pointed; and the fin-rays are thirteen in number, and hard; the rays towards the centre of the tail being divided, and subdivided, as in the male.

The *Lateral line*, like the male, occasionally interrupted in its course, one scale, and then two or three scales, being passed over or having no pores. Various scattered pores also occur over the sides of the back.

The *Colour*, like that of the male.

The measurements of this female fish are as follows :—

Length, or Longitudinal Measurements.

Total length (including caudal fin-rays), nearly $9\frac{1}{2}$ inches.

Do. of body—From point of snout to caudal extremity (not including fin-rays), $9\frac{1}{5}$ inches.

Of head, above, from snout to commencement of scales of body, $\frac{12}{20}$ of an inch.

Laterally, from snout to upper and posterior angle of operculum, a little more than $\frac{14}{20}$ of an inch.

Eyes, rather less than $\frac{1}{10}$ of an inch across.

From point of snout to base of spine of first dorsal finlet, $4\frac{1}{5}$ inches.

From spine of first dorsal finlet to extremity of caudal fin-rays, $5\frac{3}{10}$ inches.

From point of snout to anal openings, $8\frac{11}{20}$ inches.

From anal openings to extremity of body (not including caudal fin-rays), $\frac{12}{20}$ of an inch.

From anal openings to extremity of caudal fin-rays $\frac{18}{20}$ of an inch.

From extremity of body to extremity of caudal fin-rays, $\frac{3}{10}$ of an inch.

Breadth, or Transverse Measurements.

Of head, across operculum, about $\frac{2}{5}$ of an inch.

Of body, behind origin of pedicles of pectoral fin, $\frac{3}{10}$ of an inch; the body then increases rapidly in breadth, and at the distance of one inch behind the pectoral fins, measures about $\frac{2}{5}$ of an inch.

,, at the first dorsal finlet, $\frac{2}{5}$ of an inch.

,, at the second, $\frac{7}{20}$ of an inch.

,, at the sixth, $\frac{1}{4}$ of an inch.

,, at the ninth, $\frac{3}{20}$ of an inch.

,, at the caudal extremity of the body, immediately behind the anal fin, rather less than $\frac{1}{10}$ of an inch.

Depth, or Vertical Measurements.

Of head, over pre-operculum, $\frac{1}{4}$ of an inch.

Of body, opposite the origin of the pectoral fins, $\frac{3}{10}$ of an inch.

,, at the distance of one inch behind the pectoral fins, $\frac{7}{20}$ of an inch.

,, at the first dorsal finlet, $\frac{7}{20}$ of an inch.

,, at the second dorsal finlet, rather less than $\frac{7}{20}$ of an inch.

,, at the sixth, about $\frac{7}{20}$ of an inch.

,, at the tenth, rather less than $\frac{3}{10}$ of an inch.

,, at the caudal extremity of the body, immediately behind the anal fin, $\frac{1}{5}$ of an inch.

Additional Specimens of the Female Fish, and Varieties in Character.

Several additional specimens of the female fish were also examined; and similar varieties in the minuter details of their characters were found to exist among them, as in the males. Some small range was found to exist in the relative proportions of the head and the body.

Scales.—The series of scales over the fish were generally in regular single rows on all the specimens. Varieties in the mesial dorsal scales were noticed, similar to those in the males.

Dorsal finlets.—Varieties in the number of the dorsal finlets also occurred. These seemed to range from nine to eleven in number, and to be irrespective of the size or total length of the specimen, the longest and largest specimen having, indeed, the fewest dorsal finlets. The relation of the last finlet to the base of the tail and the caudal fin-rays seemed also to vary, as well as the softness or hardness of its accompanying fin-ray. The relative distance between the different individual dorsal finlets also varied in the different specimens.

Anal fin.—The anal fin in all the specimens was similar in character to the one described.

Caudal fin.—The caudal fin was rounded in its outline in some of the specimens, and more acute and pointed in others, so that in this also some range of shape must be allowed.

The *Colour*, or shade of colour, was like that of the male, but varied in the different specimens to a considerable degree, the darker colour of the back extending considerably over the sides of some of the specimens, and leaving only a small portion of the abdomen of a lighter or white colour; while in others, the colour was lighter on the middle of the sides of the body, and the abdomen of the fish was nearly white.

I add a few notes of some of these female specimens examined:—

1. The first had ten dorsal finlets, and the tenth had its fin-ray soft, and the fin attached to the base of the caudal fin-rays.

2. The second measured $8\frac{3}{4}$ inches in length, the head measuring laterally nearly $\frac{3}{4}$ of an inch; dorsal finlets ten in number, the tenth distinct and separate from the caudal fin-rays, and its fin-ray soft.

3. The third measured $8\frac{11}{16}$ inches in length; dorsal finlets ten in number, the last not connected to the caudal fin-rays; and the secondary ray of the last finlet soft.

4. The fourth was of a larger size than the others, measuring 11 inches in length. It was sent to me, as already stated, by Mr W. Grant Mylne, from the district of the Camaroons, Western Africa. Dorsal finlets only nine in number, the ninth being distinct from the caudal fin-rays, and its fin-ray soft. The first dorsal finlet is $5\frac{2}{5}$ inches distant from the point of the snout, and from the first dorsal fin it measures $5\frac{3}{5}$ inches to the extremity of the rather pointed caudal fin. (Plate XXXI. fig. 4.)

Food of the Calamoichthys calabaricus.

Termitidæ.—Various specimens of perfect insects and larvæ were taken from the stomach of one of the female fishes. These fish are, therefore, as might have been expected, carnivorous and insect-feeders; in anglers' phrase, they take the fly; and although some of them were caught in fish traps, which are stated to have been baited with palm nuts, it must have been simply from curiosity that the fish poked into them, and were taken; or perhaps the palm nuts might attract insects and their larvæ, in the water, and the fish, following in pursuit, be in this way decoyed into the trap, and captured.

The insects found in the stomach of the fish belonged, as far as was noticed —for the more perfectly preserved insects alone were examined—to the abundant African family of the *Termitidæ*, the Termites, or white ants, as they have been designated. These insects were found under all their stages or transformations.

1. Eighteen specimens of *labourers* or *larvæ* were collected. These measured each $\frac{1}{5}$ of an inch in length. The head is whitish, but the jaws are tipped with blackish or brown colour, and are horn-like in their appearance. The thorax and legs are white; the abdomen is rounded, the dark-coloured viscera or their contents shining through the white and semi-transparent skin. No eyes were visible.

2. One specimen only of a *soldier* Termite was seen; it was $\frac{3}{10}$ of an inch in length. The body is white, but the contained viscera show black, like the last, through its semi-transparent skin. The thorax is white. The head is large, square in front, and rounded behind; it is larger in size than the abdomen, and is reddish in colour especially in front, and horny-like in structure, with two black scimitar-like projecting jaws. No eyes were visible.

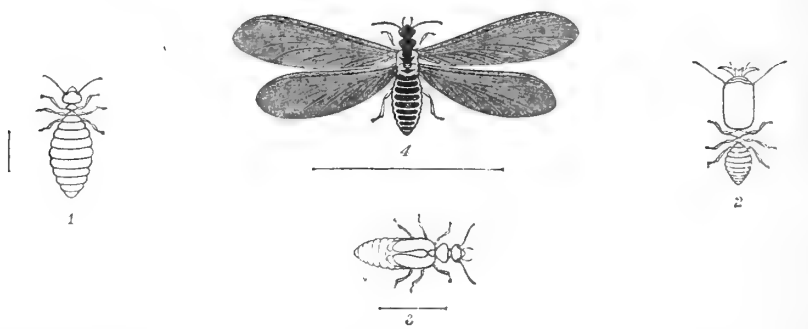
3. Six specimens of *pupæ* were also noticed; their bodies are entirely of a pale yellowish white colour, and seem to be of rather a soft consistence; the eyes, however, are now present in the form of minute black specks, distinctly visible. The abdomen is large and full. Four flattened lobes—the projecting wings—pass backwards from the thorax; they are very distinct, but do not extend backwards as far as the posterior extremity of the body, the wings being apparently folded upon themselves from below upwards, at about the middle of their length. The pupæ measure each about $\frac{7}{20}$ of an inch in length.

4. Five-winged and *perfect insects*, or *Termites*, were examined. Their bodies are generally dark-brown in colour, with four long narrow and blackish-coloured semi-transparent wings, nearly equal in length, with black nervures, the costal and sub-costal are robust and unbranched, the other nervures being finer, and branching over the rest of the wing. (In the specimen figured below, the nerve next the subcostal varies in the arrangement of its branches in each of the upper wings, probably an accidental peculiarity.) The wings extend considerably ($\frac{6}{20}$ of an inch) beyond the body, the total length of the insect to the extremities of the wings being $\frac{11}{20}$ of an inch, and that of the entire body $\frac{1}{4}$. The head and upper parts of the thorax are dark-brown, the legs lighter; the stemmata are immediately in front of, and slightly below, the eye on each side, and a little in front and above the origin of the antennæ, and no central point was observed; the abdomen on its upper surface has ten segments or plates of a dark colour

showing intervening spaces of white skin in some specimens, and these bands are separated on each side, from those of a similar character on the under surface of the abdomen, by a longitudinal space of white skin, which extends along the whole length of the abdomen. This arrangement allows for the wonderful expansion and development of the abdomen in the gravid female. The plates on the under surface of the abdomen are white in the middle of each plate, and become blackish only towards their extremities.

The antennæ in all these different creatures are moniliform and rather short, consisting apparently of fourteen small, rounded, dark-coloured, and hairy joints or divisions; they are paler in the soldier, and nearly white in the pupæ.

The presence of these various insects, in the different stages of their transformation, found in the stomach of the fish at the same time, is interesting as showing that numbers of them must undergo these changes, at different periods of time or in succession, in the same nest.



TERMITES from the stomach of the *Calamoichthys calabaricus*, Old Calabar River.—1. The Labourer; 2. The Soldier; 3. The Pupa; 4. The Perfect Insect.

The Termites, it has been said, furnish a regular supply of delicious food not only to other insects, to reptiles, to birds and beasts, and even to man himself, who eats the perfect insects at least, of a larger species than the one just described; but the finny tribes, as represented by our ganoid, seem also to come in for a share of these savoury and most abundant insects.

It is easy to understand how the winged flies should become the prey of fish, as vast quantities of them are said at certain seasons of the year, as at the beginning of the rains, to be spread over the whole face both of the land and water; but it is not quite so easy to see how the other wingless and less perfectly developed insects should come within the range of the feeding-grounds of the fish. Either their under-ground galleries, which spread out in every direction from their wonderfully-constructed castles, may have been accidentally broken open at the river's banks, and their teeming population poured forth; or more probably, the heavy tropical rains may have washed them from their broken galleries into the river's bed.

I took these specimens of Termites to the British Museum, but was not able to learn if the species was new, as the gentleman who has the charge of that particular department was unfortunately absent in bad health. They appeared, however, to resemble a species from Sierra Leone, unnamed in the collection; and the presence of the so-called pupæ, is probably somewhat of a rarity, at least none were noticed among the specimens exhibited in the cases.

I am indebted to Mr R. F. Logan, Duddingston, who fortunately combines in himself the entomologist and the artist, for the annexed very correct drawings of these insects, which will, I hope, enable those more intimately acquainted with the *Termitidæ* to determine their species.

A small and partially digested crustacean, of the prawn kind, was the only other creature observed in the stomachs of these fishes.

General Summary of the Genus Calamoichthys, *and its relation to the Genus* Polypterus.

GENUS CALAMOICHTHYS.—*Head*, small, depressed above, somewhat oval in shape (rounded and narrow in front, expands laterally behind orbits, and contracts again at the back part towards neck); sub-operculum wanting (no small plates below pre-operculum).

Body, much elongated, anguiform (cylindrical for about half its length, then becoming gradually more compressed laterally, and tapering very slightly towards its caudal extremity). *Caudal extremity*, short, tapering rapidly. *Caudal fin*, rounded; homocercal in its general contour; fin-rays, hard. (*Scales*, osseous, rhombic, sculptured; becoming somewhat larger in size as you proceed backwards along the body, and again smaller on the caudal extremity.)

Fins, small:—*Pectorals*, obtusely lobate; fin-rays, soft. *Dorsal finlets*, numerous, separate. *Anal*, in male, large; in female, small; fin-rays, hard. *Ventrals*, wanting.

The last character is rather an important one, as this fish thus appears to be the only living ganoid yet known which has no ventral fins. VAN DER HOEVEN, in his valuable "Handbook of Zoology," gives the presence of ventral fins as one of the characters of his great Section III. of the Class PISCES, the *Ganoidei*, or, as he terms them, the *Ganolepidoti*. He says they have "pectoral and ventral fins, the ventrals placed behind the pectorals." While older naturalists, as CUVIER, from the supposed constant presence of ventral fins, have placed the ganoids in the Order of MALACOPTERYGII ABDOMINALES. The discovery of this new fish will, therefore, necessitate a change in this character of the whole section.

In the GENUS POLYPTERUS (on the other hand), the *Head* is relatively larger (with apparently little or no lateral expansion, and subsequent contraction towards its neck); its gently swelling outlines gradually expand, and run backwards into the tapering outlines of the body. Sub-operculum present; several small plates below pre-operculum.

Body, relatively much shorter, generally tapering gradually from behind the region of pectoral fins, and becoming more compressed laterally, and tapering slightly towards its caudal extremity. *Caudal extremity*, longer. (*Scales* generally smooth [?])

Fins, larger:—*Pectorals*, fin-rays osseous. *Anal*, apparently alike in size in male and female. *Ventrals*, present.

This new ganoid fish, from the great elongation of its body and the shortness of its caudal extremity, must swim in a manner somewhat different from the fishes of the genus *Polypterus*, and still more from that of ordinary fishes, whose progression is due to alternate strokes or sweeps of the caudal extremity; whereas in this fish, assisted by its very large and long swimming or air bladders, it must be accomplished, as in eels and serpents, almost entirely by alternate and successive lateral undulations of the whole body; and the peculiar character of its scales, which are comparatively small anteriorly, and enlarge posteriorly, the posterior margin of each series of scales being free and projecting, would also seem to assist in giving greater flexibility and force to these movements.

The genus *Calamoichthys* agrees with the genus *Polypterus*, in the general character of its numerous dorsal finlets; lobate pectorals; two nasal cirri; a spiracle, or valve-like opening on each side of the head above, and a large flat branchiostegal ray, or jugular plate, on each side of the mesial line below; and also in the hard, osseous, rhomboidal-shaped ganoid scales, arranged in rows, running obliquely downwards and backwards; and in the tapering caudal extremity of the body.

It differs from *Polypterus*, however, in the various distinct peculiarities which I have already enumerated, and especially in the great elongation of its body and the total absence of ventral fins; and these appear to be fully sufficient to justify and confirm the propriety of its being placed in a distinct and separate genus, to which, from the general lengthened and cylindrical form of the fish, I have given the designation of *Calamoichthys*—(Καλαμος and ιχθύς). The new genus belongs, therefore, to the same family as *Polypterus*, and would accordingly fall to be placed next to the genus *Polypterus*, in the family of the *Polypterini*.

FAMILY POLYPTERINI.

I. Genus POLYPTERUS. II. Genus CALAMOICHTHYS.

1. Species *C. calabaricus*.

Habitat, Old Calabar river, and the Bimbia, Camaroons, Western Africa.

For the purpose of getting an anatomical description of this new fish, I placed specimens, both of the males and the females, in the hands of Dr Ramsay H. Traquair, Junior Demonstrator of Anatomy in the University, who has, accordingly, prepared a detailed account of its anatomy.*

* An abstract of Dr Traquair's paper is published in the Proceedings of the Royal Society of Edinburgh, vol. v. p. 657.

Description of Plates XXXI., XXXII.

Plate XXXI.

Calamoichthys calabaricus.

Fig. 1. Male, dorsal view (natural size).

Fig. 2. Do. lateral view „

Fig. 3. Caudal extremity of Male (double the size of specimen).

Fig. 4. Do. of Female „ „

Fig. 5. Head of Male, lateral view (four times the size of specimen).

Fig. 6. Do. dorsal view „ „

N.B.—Unfortunately the anal and caudal fins, &c., and their fin-rays, have not been drawn with a very minute attention to details.

Plate XXXII.

Scales of Calamoichthys calabaricus.

Fig. 1. A series of scales, from the lateral surface of the fish, including a mesial dorsal (*c*), and ventral scale (*b*). The mucous pore is shown at *a*.

Fig. 2. Portion of three adjoining rows of scales, external surface.

Fig. 3. Internal surface of the same scales, showing the manner in which they are locked together.

Fig. 4. A single scale from the middle part of the fish, showing the tooth and internal tubercle, and the sculpture on its external surface.

Fig. 5. Internal surface of the same scale, with the longitudinal bar along the back and the socket at its posterior termination, for receiving the tooth of the adjoining scale.

Fig. 6. Highly magnified view of part of the sculptured or external surface of the same scale.

Fig. 7. One of the median dorsal scales ; its outer or external surface.

Fig. 8. Lower or internal surface of ditto.

Fig. 9. One of the median ventral scales ; its outer surface.

Fig. 10. Inner surface of ditto.

Fig. 11. External surface of scale from posterior part of body, near the anal region, showing gradual alteration in form.

Fig. 12. Internal surface of ditto.

Fig. 13. First dorsal finlet, showing the forked spine of the finlet, and the general arrangement of the scales around it ; seen from above.

Fig. 14. Lateral view of ditto ; the spine of the first dorsal finlet, being partially raised, showing its secondary ray and finlet (from male fish described).

N.B.—All these figures are more or less magnified.

XXXII.—*Note on Formulæ representing the Fecundity and Fertility of Women.* By Professor TAIT.

(Received Oct. 1, 1866, and ordered by the Council to be inserted as an Appendix to Dr DUNCAN'S papers in Vol. XXIII., and in the present volume, of the Transactions.)

1. Dr MATTHEWS DUNCAN having requested me to point out to him some simple method of comparing the fertility of different races, I endeavoured, as a preliminary step, to represent by formulæ some of the chief results which he has obtained in his very lucid and elaborate papers recently read to this Society, and printed in their Transactions for 1863–4 and for the present session. Some of the formulæ which I have obtained are so simple, and accord so well with the tables, that I have thought them worth bringing before the Society. Of course it must be understood that I advocate no theory, and pretend to no physiological knowledge of the question. I merely try to represent, in a simple analytical form, the contents of some of Dr DUNCAN'S tables.

2. To prevent misconception, let us begin by defining the terms *fecundity* and *fertility* as they will be used in this note, unless qualified in some manner.

By *fecundity* at a given age we mean the probability that during the lapse of one year of married life, at that age, pregnancy, producing a living child, will ensue. This is, in all likelihood, modified in each individual woman by the previous duration of marriage (see § 10 below). But at present, in dealing with the mass of wives, we omit this consideration. We do not require, in our calculations, to consider any questions connected with the duration of life of husband and wife, of the length of time the child may live, &c., as the numbers in the tables are already influenced by such causes. The numbers in the tables do not usually denote the fecundity as above defined, but are quantities proportional to its values.

By *fertility*, at any age, we mean the number of children which a married woman of that age is likely to have during the rest of her life, or some numerical multiple of it.

The subject divides itself into three heads—(I.) The fertility and fecundity of the mass of wives; (II.) their value for the average individual; (III.) the *relative* fertility and fecundity of different races.

These we proceed to consider in order.

I. *Fertility and Fecundity of the Mass of Wives.*

3. If f_t represent the fecundity, and F_t the fertility at the age of t years,

the ordinary laws of probability, if applicable to this question, give us the expression—

$$F_t = f_t + f_{t+1} + \ . \ . \ . \ + f_{50} = \Sigma_t^{50} f_t$$

assuming that sterility arrives about the age of fifty.

Before going further, it may be well to verify this formula by comparison with the tables, so that we may be assured of the validity of our reasoning.

Dr DUNCAN gives (*Trans. R. S. E.* 1863–4, p. 358) the following numbers for the wives in Edinburgh and Glasgow, *taken as a whole* :—

Age	15–19	20–24	25–29	30–34	35–39	40–44	45–49
Fecundity	50	41·8	34·6	26·6	20·4	8	1·3

The two last numbers are probably not so accurate as the others—one from vague statements as to "forty years of age;" the other on account of some omissions noticed in a footnote to the table. As, unfortunately, we cannot get data for each year separately, we can only test the above formula for intervals of five years. The numbers just given may therefore be taken as *proportional* to $f_{17}, f_{22}, f_{27}, f_{32}, f_{37}, f_{42}$, and f_{47} respectively.

4. We may now construct the second line of the following table, according to the formula above, by adding to the number for any quinquennial period all those which follow it:—

Age	15–19	20–24	25–29	30–34	35–39	40–44	45–49
Fertility	182·7	132·7	90·9	56·3	29·7	9·3	1·3
F_t	12	8·9	6	3·7	2	0·6	0·1

The numbers in the last line are proportional to those in the second, on the assumption that a woman of 15–19 will have a family of twelve.

Dr DUNCAN quotes (*Trans. R. S. E.*, 1865–6, p. 302) from the Journal of the Statistical Society the following table of values of F_t for the mass of married women in the district of St George's-in-the-East. This is, unfortunately, not quite comparable with the last, as the quinquenniads differ by one year of age; and, besides, the ages at marriage differ in the different columns. But there seems to be no attainable table so nearly approaching what we require for comparison.

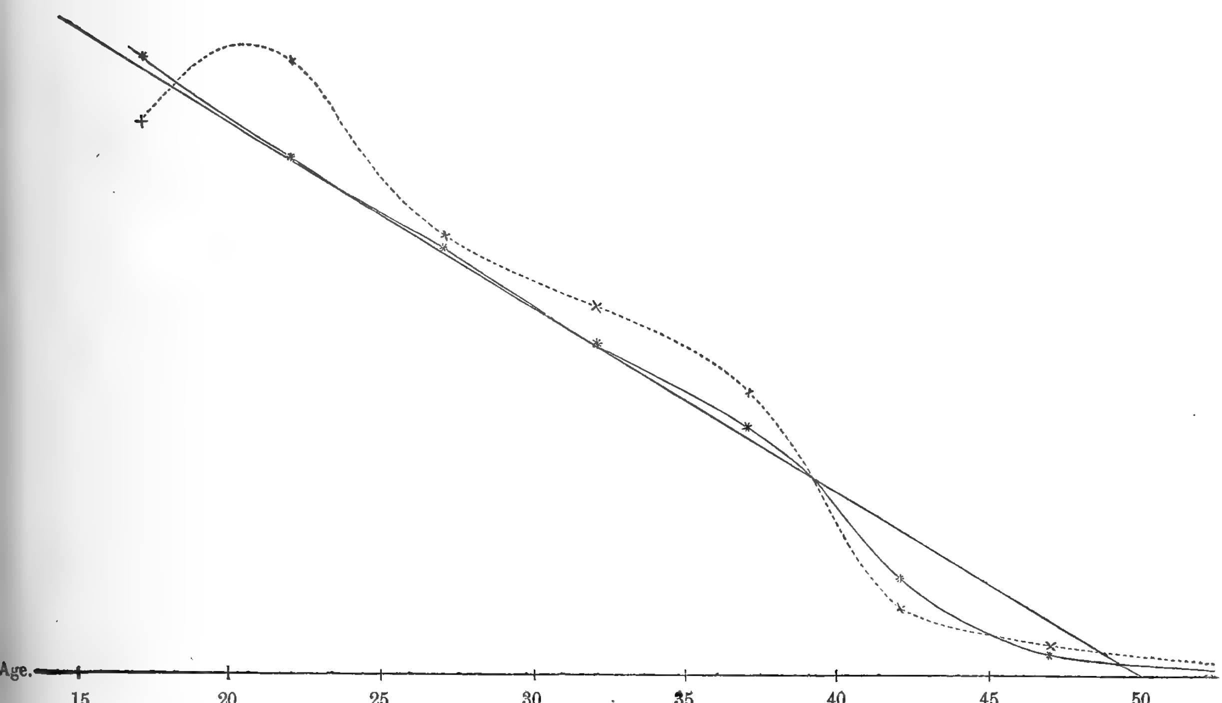
Age.	16–20	21–15	26–30	31–35
F_t	10·85	8·24	5·00	4·00

Neglecting the difference of the quinquenniads in the two tables, and taking 11 instead of 12, for the sake of direct comparison, as the value of F at 15–19 in the first, we have—

Calculated	11	8·16	5·5	3·4	1·9	0·55
Observed	10·85	8·24	5·0	4·0	...	...

These numbers agree as well as could possibly be expected.

5. If we project the numbers above given for $f_{17} \ldots f_{47}$, and try to represent the values of f for all ages by the ordinates of a curve, whose abscissæ denote the corresponding ages, we have the continuous curve of the following diagram :—



FECUNDITY AT DIFFERENT AGES.

The continuous curve is founded on Edinburgh and Glasgow Tables. The dotted curve on Tables of Sterility. The straight line from the formula $k\ (50-t)$.

The straight line, which almost coincides with the continuous curve—at least from the age of 17 to that of 40—and whose departure from it above that age must depend to some extent on the defects of the table pointed out in § 3, intersects the axis at 50. We may obviously assume it as very nearly representing the tables. And we can therefore express the value of f_t by a number proportional to $50-t$. Thus—

$$f_t = k(50 - t)$$

(where k is a *number*, whose value we can easily find), is a simple formula very closely representing the tabulated results.

6. But F_t can now be represented in a form almost as simple. For—

$$F_t = f_t + f_{t+1} + \ldots\ldots + f_{49}$$
$$= k\left\{(50-t) + (49-t) + \ldots\ldots + 1\right\} = \tfrac{1}{2}k(50-t)(51-t)$$
$$= \tfrac{1}{2}k(50-t)^2, \text{ nearly enough for our purpose.}$$

7. Thus it appears that—

Fecundity is proportional to the number of years a woman's age is under fifty; and

Fertility at that age is proportional to the square of the same number.

8. To show, numerically, how closely these formulæ represent the tables is of course easy.

Fecundity.

Age	15–19	20–24	25–29	30–34	35–39	40–44	45–49
Dr DUNCAN	50·0	41·8	34·6	26·6	20·4	8·0	1·3
$\frac{3}{2}(50-t)$	49·5	42·0	34·5	27·0	19·5	12·0	4·5

Fertility.

Age	15–19	20–24	25–29	30–34	35–39	40–44	45–49
Dr DUNCAN	10·85	8·24	5·00	4·00	...	...	...
Calculated from f_t as in (4)	11·0	8·16	5·5	3·4	1·9	0·55	0·09
$\frac{1}{10}(50-t)^2$	10·89	7·84	5·29	3·24	1·69	0·64	0·09

9. *Example.*—As an application of the formula, let us suppose a woman, who was married ten years ago at the age of twenty, to have now five children :—

At marriage	$F_{20} = \frac{1}{2}k(50-20)^2 = 450\,k$
At present	$F_{30} = \frac{1}{2}k(50-30)^2 = 200\,k$

But the difference $F_{20} - F_{30}$, or $250\,k$, represents five children. Hence F_{30}, or $200\,k$, represents four more. So that her family will probably amount to nine.

10. As illustrating the subject farther, I append portions of another of Dr DUNCAN's tables (*Trans. R. S. E.*, 1865–6, p. 306), with formulæ for comparison founded on the type $f_t = k\,(C - t)$.

Fecundity.

Age at Marriage.		Age 20–24	Age 25–29	Age 30–34	Age 35–39	Age 40–44	Age 45–49
15–19							
	Table . . .	64·1	52·1	36·2	19·6	2·4	...
	3 (43—age) .	63·0	48·0	33·0	18·0	3·0	...
20–24							
	Table . . .	...	61·7	41·7	24·5	11·4	0·3
	3 (46—age) .	...	57·0	42·0	27·0	12·0	0·0
25–29							
	Table . . .	...	...	40·6	28·2	9·1	1·3
	2·66 (47·5—age)	...	...	41·4	28·0	14·7	1·4
30–34							
	Table . . .	...	...	...	34·0	19·2	4·5
	3 (48·5—age) .	...	...	...	34·5	19·5	4·5

These formulæ seem to represent the tables pretty closely—with the exception of a solitary number for those married at 25–29—and if they may be trusted, indicate a very curious result. Of course, when the fecundity is given by an expression of the form k (C — t), C is the age at which sterility arrives.

Now, it appears that we have for wives married at

	Fecundity.
15–19	k (43—age)
20–24	k (46—age)
25–29	k (47·5—age)
30–34	k (48·5—age)

In words, *the advent of sterility is hastened by early marriage.*

Thus sterility occurs according to the following table:—

Age at Marriage.	Age of Sterility.
15–19	43
20–24	46
25–29	47·5
30–34	48·5

This is singular enough, and seems to be well borne out by the tables, since the age of sterility is *uniformly* later as the age of marriage is greater. But, of course, far more extensive observations must be made and discussed before such a point as this can be settled.

Accepting it, however, for the present, we may calculate from the last table, and the table of fecundity already given, the whole fertility as depending on the age at marriage. For, if t be the age at marriage, C_t the corresponding age of sterility—

$$\text{Whole fertility} = \tfrac{1}{2}\, k\, (C_t - t)^2.$$

In this formula k is to be found. But we have

$$\text{Fecundity at marriage} = f_t = k\, (C_t - t).$$
$$\text{Hence, whole fertility} = \tfrac{1}{2}\, f_t\, (C_t - t).$$

If we accept 10 children as representing the whole fertility at 15–19, which seems a reasonable assumption, we have

$$10 = \tfrac{1}{2} f_{17} (43 - 17) = 13 f_{17}.$$

Hence $f_{17} = \frac{10}{13}$, from which (by proportion) the other values of f are easily found. Hence—

Age at Marriage.	Whole Fertility.	Ft.
15–19	10·0	10·0
20–24	7·7	7·4
25–26	5·5	5·0
30–34	3·4	3·1

The last column has been added for comparison, so as to show how the later advent of sterility in the more advanced marriages increases the fertility.

It may be well to notice that the interpretation of the expression $f_{17} = \frac{10}{13}$ is, that *a wife of* 15–19 *will, on the average, become pregnant at* 1·3 *years after marriage*, that is, she will have a child within about two years of marriage. This limit of time depends, however, on our assumption of 10 children as the measure of the fertility at 15–19, and childless marriages are included in the data. Dr DUNCAN gives (*Trans. R. S. E.*, 1865–6, p. 297) 1·52 years as the average interval between marriage and the birth of the first child. The reason of the discrepancy is of course this, that in our calculation the mass of wives is considered, and in Dr DUNCAN'S only fruitful marriages are taken account of.

II. *Fecundity and Fertility of the Average Individual.*

11. If we endeavour to derive formulæ of a similar character from the tables of sterility, the results are not quite so simple. Thus we find (*Trans. R. S. E.*, 1865–6, p. 319)—

Age at Marriage	15–19	20–24	25–29	30–34	35–39	40–44	45–49
Percentage Sterile .	7·3	0	27·7	37·5	53·2	90·9	95·6
Percentage not Sterile	92·7	100	72·3	62·5	46·8	9·1	4·4

From the manner in which this table was formed, it would appear that we are justified in taking the numbers in the last line as proportional to the average fecundity at the respective ages. But the curve representing these numbers differs considerably more from a straight line than that derived from the other tables. It is the dotted curve in the figure. It is true that if we consider the loose way in which women from 30 to 40 call themselves 30, and those from

40 to 50 call themselves 40, we might expect the smaller ordinates belonging to higher ages to be pushed back, as it were, towards 30 and 40, thereby apparently accounting for the two depressions which appear in the curve about those ages. That this is no fancied explanation may be gathered from the following scandalous facts recorded in the Census Report of 1851, p. xxiv.:—

In 1841 the number of girls, of ages 10–15 was . . .	1,003,119
But in 1851 these had become young women aged 20–25, and numbered .	1,030,456

This number, when corrected from the tables of mortality, obviously includes about 140,000 women whose ages had increased by less than 10 in ten years!

Again, in 1841 the number of women aged 20–25 was . . .	973,696
But in 1851 those who had reached 30–35 were only . . .	768,711

indicating at first sight a fearful death-rate, but really showing how strong is the desire to be considered as remaining under the magic limit of thirty years of age.

To complete the examination, however, let us see how far these data from sterility agree with the formula deduced by the other processes.

Fecundity.

Age.	15–19	20–24	25–29	30–34	35–39	40–44	45–49
Percentage not Sterile,	92·7	100	72·3	62·5	46·8	9·1	4·4
$3\frac{3}{7}(50-\text{age})$	113·1	96	78·9	61·8	44·5	27·4	10·4

It is easy, of course, to construct a formula to represent any series of numbers, but unless it be simple it is of little use; and the approximation we have got seems close enough, if we remember the almost certain deficiencies in the numbers for the two highest ages, and the immaturity, &c., which may easily be supposed to account for that at 15–19. But there is another cause which may serve to account for part of the discrepancy, as in fact Dr DUNCAN's table shows. This is that plural births are not eliminated. In fact, at age 20–24 there are a good many more children per annum than mothers in his table, which thus virtually assumes that no woman of 20–24 is sterile. This accounts for the great rise in the (dotted) curve at the age of 22.

By the process of § 3 we form the first line of the following table. The second is formed on the type $F_t = \frac{1}{2} k\ (50-t)^2$.

Fertility.

Age.	15–19	20–24	25–29	30–34	35–39	40–44	45–49
F_t from f_t observed,	387·8	295·1	195·1	120·8	60·3	13·5	4·4
$\frac{19}{50}(50-t)^2$	414	298	202	123	64	24	4

This coincidence also is close enough, and would be still closer if we had the numbers for f_{50} and upwards, as the smaller numbers in the table, where the deficiency lies, would thus be increased proportionally much more than the larger ones.

III. *Relative Fertility of Different Races.*

12. We may apply the above results to compare the fertility of different races—a problem of considerable interest. We shall not attempt a rigorous solution, for the application of which, indeed, we have no sufficient data; but shall make one or two postulates, which will probably be easily admitted, and which will enable us to avoid complication.

13. Suppose that for ten or fifteen years we may consider the number of marriages at any given age to remain practically unaltered, we may then consider *the births in any one year as represented by the total fertility of those married in that year*. That is, the children born in that year of mothers married at 30–34, for instance, are due to those married last year, the year before last, and so on for fifteen years back, at the age of 30–34; and as the number is supposed nearly constant for some years, we have the fertility of *all* for one year (very nearly) by calculating the total fertility for the rest of their lives of those married in that year. As population, and with it the number of marriages, is generally increasing, this process will slightly exaggerate the numbers sought; but, in *comparing* two growing countries, such as England and Scotland, no perceptible error will be introduced.

14. We next assume that the *law of fertility as depending on age is the same in the two countries compared.* That is, we assume that

$$\frac{F'_t}{F_t} = \frac{F'_{t+1}}{F_{t+1}} = \frac{F'_{t+2}}{F_{t+2}} = \text{etc.,} = e,$$

where e is some definite number; and F_t, F'_t represent the fertility in the two races at age t.

This will evidently be the case if the fertility be really expressible, as above, in the form

$$F_t = \tfrac{1}{2}\, k\, (50 - t)^2,$$

for two such expressions can only differ through the number k; unless indeed the age at which sterility comes on, here represented by 50, should happen to be greatly different for different races. On this point we have no information.

15. Let, then, μ_t be the number of marriages of women at t years of age in any one year, β the number of legitimate births in a year, we have by the above postulates

$$\beta = \Sigma \mu F = \mu_{15} F_{15} + \mu_{16} F_{16} + \quad . \quad . \quad . \quad + \mu_{49} F_{49}.$$

For another country

$$\beta' = \Sigma\mu' \mathrm{F}' = e\,\Sigma\mu' \mathrm{F},$$

where e is the ratio of the fertility of the second race to that of the first. These equations give

$$e = \frac{\beta'}{\beta} \cdot \frac{\Sigma\mu \mathrm{F}}{\Sigma\mu' \mathrm{F}}$$

where the *absolute* fertility of either country is no longer involved, so that we may employ for the values of F the expressions in § 4, or those in § 8.

16. *Example.*—From the Registrar Generals' Reports for England and Scotland last published, we extract the following data :—

	England, 1864.	Scotland, 1862.
No. of Marriages, .	180,387	20,597
Legitimate Births, .	692,827	96,693

Percentage of marriages of women in 1862 at the ages—

	15–20	20–25	25–30	30–35	35–40	40–45	45–50
England, . .	13·60	49·74	19·55	7·28	3·89	2·67	1·57
Scotland, . .	13·07	46·28	24·13	8 54	4·36	2 03	0·95
And we assume, in accordance with §§ 4 and 8—							
F proportional to .	12·0	8·9	6·0	3·7	2·0	0·6	0.1

This gives $\Sigma\mu\mathrm{F} = \dfrac{20{,}597}{100}[12 \times 13{\cdot}07 + 8{\cdot}9 \times 46{\cdot}28 + \;.\;.\;.\;.]$

$= 20{,}597 \times 7{\cdot}55$ for Scotland.

Also $\quad \Sigma\mu'\mathrm{F} = 180{,}387 \times 7{\cdot}595$ for England.

Hence $\quad e = \dfrac{692{,}827}{96{,}693} \cdot \dfrac{20{,}597}{180{,}387} \cdot \dfrac{7{\cdot}55}{7{\cdot}595} = 0\;812$ nearly.

A more accurate comparison would be obtained by employing the average number of marriages at various ages for five or ten consecutive years, instead of those in any one year, as above, which are liable to considerable fluctuations. But we have not data enough. It would appear, then, that *the absolute fertility of the mass of married women in England is only about* 80 *per cent. of that in Scotland.*

That the fertility is less in England than in Scotland has been shown by the Registrar-General for Scotland (*Report* 1866). But he makes the ratio considerably greater than the preceding estimate.

It is to be observed that if the insinuations we sometimes hear about

Scottish marriages have any foundation in fact, their consideration would tend to make the difference in fertility between the two countries even greater than that just given; for legitimation *per subsequens matrimonium* does not put a child's name on the Registrar's books.

17. The fact that in England and Scotland the quantities $\Sigma\mu F'$ and $\Sigma\mu F$ are almost exactly proportional to the number of marriages in the two countries, shows that, although Scottish women, as a rule, marry later in life than English women, the long period (25–40) during which their marriage-rate exceeds that in England, as compared with the shorter period (20–25), during which it falls behind, almost makes up for the diminished fertility at the more advanced age.

18. It only remains to construct the values of the quantities F_t for each country, taken, of course, from the mass of the wives.

As before (§ 15), we should have had

$$\beta = \Sigma\mu F$$

if we had used proper absolute values of F. But we used the numbers 12, 8·9, 6, &c., which are obviously too large. Reducing them all in the ratio ϵ to 1, and substituting for β, &c., their values, we get

$$96{,}693 = 20{,}597 \times 7{\cdot}55\ \epsilon.$$

This gives

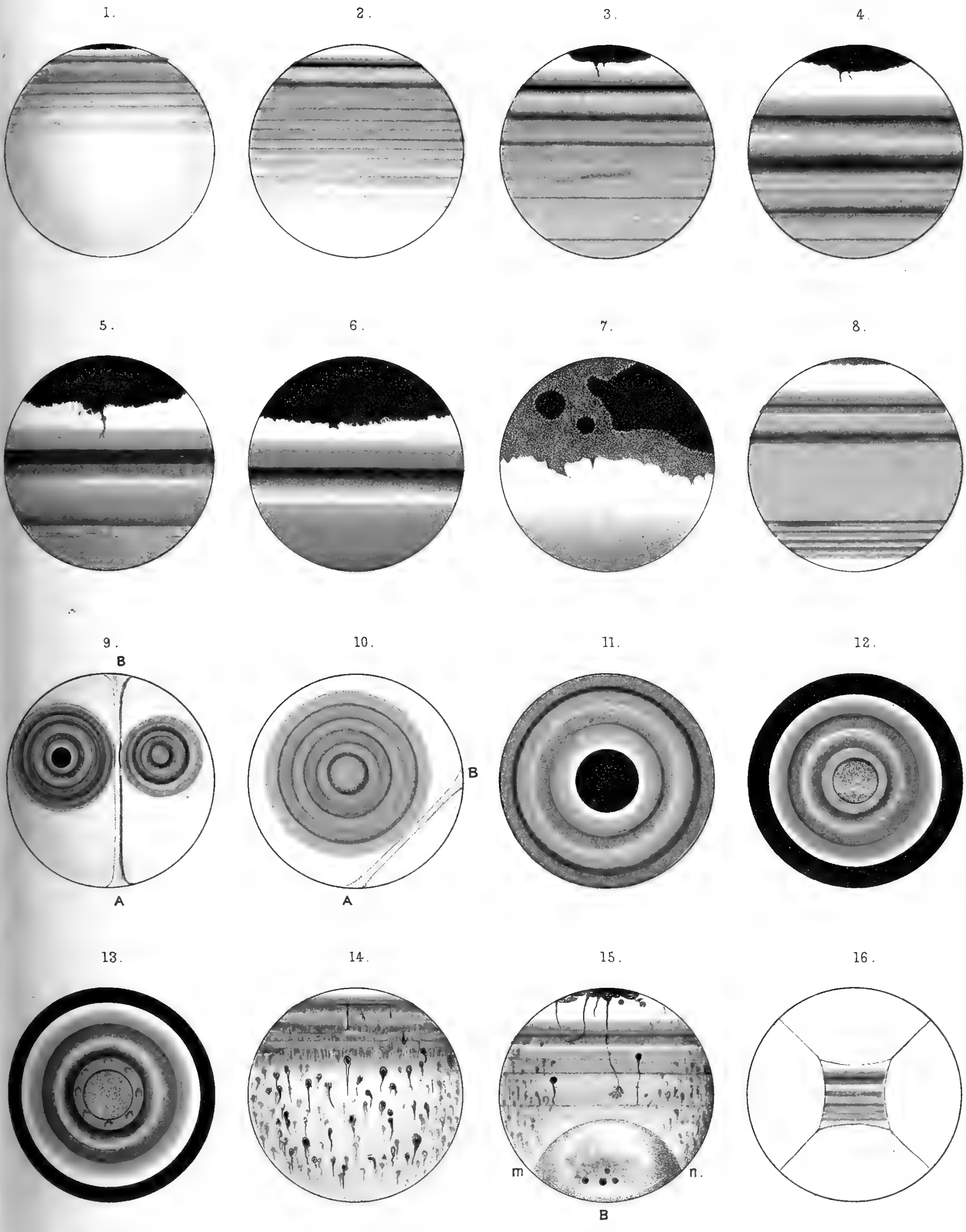
$$\epsilon = 0{\cdot}622.$$

from which we construct the following table of

Fertility of the Mass of Wives.

	15–19	20–24	25–29	30–34	35–39	40–44	45–49
Scotland, ...	7·44	5·54	3·77	2·30	1·24	0·37	0·06
England, ...	6·04	4·49	3·02	1 87	1·01	0·30	0·05

19. In conclusion, it may merely be repeated that we have attempted no elaborate or exact inquiry into this question; indeed the utter insufficiency of data would have rendered such a proceeding absurd; and we have, for the same reason, abstained from employing some of our own results, such as those of § 10, in modifying the earlier ones, by the help of which they have been arrived at. Thus, for instance, we should be led by the results of § 10 to use in the formulæ of §§ 5, 6, a number somewhat less than 50, as corresponding to the average age of sterility. As in all questions of average, the value of our deductions in this matter is mainly dependent on the extent and accuracy of our data, and it is sad to think that the enormous blue-books which load our shelves contain so much painfully-elaborated information which is of no use, and so little of those precious statistics which would at once be easy of acquirement and invaluable to physiologists.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XXXIII.—*On the Colours of the Soap-Bubble.* By Sir DAVID BREWSTER, K.H., F.R.S. (Plate XXXIII.)

(Read 21st January 1867.)

The phenomena and colours of the soap-bubble have been the subject of more frequent observation than any other facts in science. For nearly two centuries they have afforded amusement and instruction to the young, and have exercised the genius of the most distinguished philosophers. HOOK* and NEWTON† ascribed the beautiful colours of the soap-bubble to the different degrees of thickness of the attenuated film, and this opinion has been implicitly adopted by every optical writer down to the present day. In the latest and best Treatise on Light, Sir JOHN HERSCHEL‡ expresses the current theory more distinctly than others, when he says, "that the brilliant colours which appear on soap-bubbles consist of a regular succession of hues disposed in the same order, and determined, obviously not by any colour in the medium itself, in which they are formed, or on whose surfaces they appear, but solely by its greater or less thickness. . . . . It is at first uniformly white, but as it grows thinner and thinner, by the subsidence of its particles, colours appear to begin at its top, where thinnest, which grow more and more vivid, and arrange themselves in beautiful horizontal zones about the highest point, as a centre."

Of the correctness of this theory I never entertained a doubt, till I resumed the study of the subject, while repeating the beautiful experiments of Professor PLATEAU, "On the Figures of equilibrium of a liquid mass without gravity." The colours exhibited by plane, convex, and concave liquid films thus came under my notice, and led me to the true cause of the colours of the soap-bubble.

Dr THOMAS YOUNG§ is the only author, so far as I know, that has described these colours, as produced by a film stretched across the mouth of a wine-glass, and it is strange that he did not observe certain changes in the coloured bands produced by the simple motion of the glass, which might have led him to their true explanation. He contents himself with giving an incorrect coloured drawing of the bands in their first stage, and adopts the usual theory of their formation.||

* BIRCH's Hist. of the Royal Society, vol. iii. p. 29.

† Optics, 3d edition, p. 187.

‡ Treatise on Light, § 633, p. 462.

§ Elements of Natural Philosophy, vol. i. p. 469, and plate xxx. fig. 448. The dark band in this figure does not exist.

|| "When a film of soapy water," says Dr YOUNG, "is stretched over a wine-glass, and placed in a vertical position, its upper edge becomes extremely thin, and apparently black, while the parts below are divided by horizontal lines into a series of coloured bands."—*Id.* p. 458.

As the colours of the soap-bubble cannot be well studied on the bubble itself even when defended from the action of the air by "a clear glass," as NEWTON did, I have employed plane, convex, and concave films of all sizes, up to $3\frac{1}{4}$ inches in diameter, as obtained upon glasses, conical or cylindrical, or upon tubes of glass or metal closed at one end, or open at both. I have also employed wires of different metals bent into various shapes, rectangular, triangular, and curvilinear, but in the following experiments I have principally made use of a cylindrical wine-glass, which gives a film $1\frac{1}{4}$ inch in diameter, and unless otherwise stated, I have obtained the films from the common solution of soap and water, sufficiently strong to give an ordinary bubble.

I. *Phenomena of Colour in a Vertical Plane Film.*

1. When the film has been newly formed and held vertically for a second or two, it exhibits at its apex *six*, *seven*, or *eight* horizontal bands of colour, the uppermost white and orange-red, with a little black above them, being the first order of NEWTON'S scale. In a few seconds the different orders increase in width, descending, and becoming in succession, as shown in Figs. 1–6, eight, seven, six, four, three, &c., in number, the film when unprotected generally bursting when four or five bands cover the whole of it, as shown in Figs. 4 and 5. Previous to bursting, the black portion of the first order of colours has appeared at the apex, as shown in Fig. 3.

In order to observe the changes in the state of the bands, after their number is reduced to five or four, we must protect the film by a watch-glass, or a piece of plate glass. When this is done, the eighth, seventh, and sixth orders of colours gradually disappear, being succeeded by the fifth, fourth, third, and second orders, which, along with the first, cover the film. After a while the third and second orders disappear, and the film is covered, as in Fig. 7, with the *black* and *white* of the first order. In a few minutes the *white* of the first order disappears, and the black band covers the whole film. Before the *black* film is complete, it often advances with an irregular margin, and throws out filaments into the white band, as shown in Figs. 5 and 6. It very frequently includes also minute systems of rings of different colours, and portions of the *white* of the first order of all shapes, having within them, in constant motion, small portions of the black matter. These *white* portions gradually disappear, leaving behind them the most beautiful silvery dendritic forms, which move about till the film bursts.

When the black band is formed from a disturbed condition of the coloured bands, where a great number of separate black portions are slowly united, there may be seen a portion of the black space much darker than the rest, with a beautiful margin of white spots, and accompanied with one or two circular spaces of equal blackness, and surrounded also with white spots, so small as to require a lens to see them. This phenomenon is shown in Fig. 7. The deep black colouring matter sometimes occurs in small spots and in dendritic forms, moving over a lighter black portion.

The development of the black band is very remarkable. At first, a slightly dark shade appears at the apex of the film, increasing in darkness till a patch of deep black rushes in with a brilliant margin of white silvery spots, which adhere to it even when its lower edge becomes a straight line. These silvery spots, in a very minute state, and obviously of the first order, sometimes form a sort of network within the black space, and at other times dilute, as it were, the black with white specks, seen only with the microscope.

2. If, when the film has four or five bands upon it, as in Fig. 4, or indeed any number, we make the glass turn round its axis as quickly as possible, the bands will remain horizontal, as if under the influence of gravity, proving that they are not produced by the subsidence and consequent thinning of the film, for which there is not time, as the change of place in the film is almost instantaneous.

In confirmation of this result, place the film in a horizontal position. The bands, now bent or broken, will float in irregular shapes on its surface, the film itself, which is a fixture, adhering by capillary attraction to the margin of the wine-glass.

These phenomena are seen with interesting variations, when the film is at first placed at different angles with the horizon. When it is nearly horizontal, the changes take place slowly, and the movements of the separate portions of colouring matter, to which I have given the name of *tadpoles*, is very curious, the whole film being sometimes covered by them without the trace of a band. The black matter is produced very irregularly, and in detached portions, which ultimately unite, chasing away all the other colours at the lowest part of the film. While these changes are going on, there is a considerable deposition of aqueous vapour on the lower surface of the watch-glass which covers the film.

3. When the first, second, and third, &c., bands are floating about, like oil upon water, blow upon them with the mouth gently, till they are broken down, and thus cover the film with any one colour, suppose *green* of the third order. If the film is now placed vertically, the colouring matter will resume the form of bands, as in Fig. 8, the colour produced by blowing, namely, the *green* of the third order, taking its proper place on the film in a very broad band, having bands of inferior orders above it, and very narrow bands of higher orders below it.

The phenomena produced by this process are innumerable and highly beautiful, varying with the order of the number of tints originally on the film, and with the order of the tint produced by blowing, whether the blowing commences before any bands appear, or after most of them have quitted the film.

The special colour produced over the whole film by blowing is not composed wholly of colouring matter of one order, though that matter predominates. When examined by the microscope the film exhibits a most beautiful combination of tints, the colouring matter of various orders having collected themselves into innumerable small circular and irregularly shaped systems of rings, floating on the coloured film. Every successive blast upon the film in this state produces a new general tint, and new microscopic systems of rings.

All the preceding phenomena take place with convex and concave films, and may be observed in films one-fourth and one-half of an inch in diameter, produced upon test tubes.

II. *On the Production of Revolving Systems of Coloured Rings on the Soap-Film.*

If we place a coloured film horizontally when it is in any of the states described in the preceding experiments, and, through a narrow tube, one-eighth or one-tenth of an inch aperture, blow upon its surface in the direction of a diameter AB, Fig. 9, there will be produced two systems of rings, C, D, revolving rapidly round their centres, the system C revolving from *right* to *left*, and the system D from *left* to *right*. If we now blow in the direction AB, Fig. 10, there will be only one circular system revolving from *right* to *left*, and covering the whole surface of the film. If the film is square, or of any other form, the rings, when fully developed, will take the same shape. When there are two systems of rings, as in Fig. 9, the colours upon the rectilineal current AB are very remarkable.

These rings are produced whatever be the colours on the film, and however irregularly they may be distributed; and, in general, the two systems will be of different forms and of different colours. If, previous to blowing, however, the colours are in regular bands or in concentric circles, as they may be in films of all forms and in all positions, and if the line of the blast bisects them so that the same colours are on each side of it, then the revolving systems will be similar in form and colour.

The order of the colours in these systems is very curious. The colours of the first order, or rather the first colours of the first order, occupy the centre of the system, if they are upon the film, the *black* being in the centre, and the *white* next to it, and the successive orders in successive rings, the breadth of each ring being proportional to the quantity of colouring matter put in motion. The *black*, for example, is often a small central spot, and when that colour occupies one-half of the film, and the *white* the other half, which they often do, as in Fig. 7, the revolving portions may be wholly *black* on one side of the blast AB, and wholly *white* on the other. If there is no colour of the first order on the film previous to blowing, the first colour of the second order will occupy the centre, the largest particles of the colouring matter being carried by the centrifugal force to the outer rings of the system. If we continue the blast, the rings will gradually disappear, the colouring matter which formed them having been restored to its colourless state, and recombined with the original film. When this is effected, and the film placed in any position, the colour bands will be again formed exactly as they would have been in a fresh film similarly placed. The revolving rings may then be again produced, and the colouring matter again combined with the soapy film.

The colours which compose the two systems of rings may be exhibited by holding the film vertically, when the colouring matter will arrange itself in bands of the different orders to which it belongs. The bands thus exhibited are very beautiful, and of great variety.

The revolving systems of rings may be produced when the film is in any other position.

III. *On the Form and Movements of the Bands and Rings in Convex and Concave Films.*

1. When convex and concave films are held vertically, the bands are formed parallel to the horizon, exactly as in plane films, and the same phenomena take place upon turning the glass round its axis.

2. When a *convex* film is placed in a true horizontal position, and defended from currents of air, the colours begin to form at the apex or summit of the film; first, a faint *green* or *red* of a high order, followed by coloured rings of inferior orders, the first coloured rings descending till the film is covered with a regular system, having black in its centre, and *red* and *green* rings occupying the circumference of the film,* as in Fig. 11.

This regular circular system may be produced more rapidly by first placing the convex film vertically, and then, when the tints of several of the first orders are developed, turning it cautiously into a horizontal position. The horizontal bands will thus be converted, in a few seconds, into a regular concentric system. This change is most easily effected with small and very convex films at the mouth of closed tubes, about half-an-inch or three-quarters in diameter, in which case we can increase the convexity even to a hemisphere by heating the tube with the hand or otherwise.

3. When a *concave*† film is placed in a true horizontal position, and defended from the air, the rings commence at the circumference of the film, and gradually extend towards the centre, when they appear as in Fig. 12, in which there are three orders of colours, from black of the first order to the red of the third order, and within this is a lens having often a number of hardly visible rings within it. This central portion of the colouring matter is obviously lenticular, as the di-

* The order of these colours, as produced upon the upper hemisphere of the soap-bubble, is described by Sir Isaac Newton in his *Optics*, p. 188.

† It is difficult to obtain a good *concave* film by dipping the cylindrical wine-glass into the soap solution. During the experiments of a whole day I never failed to obtain one, but with the same glass and similar solutions I cannot now produce one. A certain mode of producing them will be found in the following paper.

minished image of a candle may be distinctly seen by reflection from its outer and inner surfaces. It is sometimes small, and sometimes the size of a sixpence, in which case it is not visibly lenticular. It sometimes increases by the appropriation of all or part of the colouring matter around it, and sometimes diminishes by its conversion into colouring matter, or partially recombining with the film.

When the circular system of rings, with the lens in its centre, is perfect, it decays in the following manner, as observed in three different films. When there is a *green* ring of the second order round the lens, eight or ten spots of *red* begin to form upon the *green*, as in Fig. 13, and sometimes shoot out into as many red spokes, like those of a wheel, joining the next red ring. The system of rings is now splendid, varying in size and colour. Outside of the spotted ring there are generally two orders of colours. The *black* is gradually enlarging, and the *white*, with its enclosed rings, diminishing. The eight or ten spots are now *green* upon a pale *greenish yellow* ground. In a minute or so they are *purple* upon a *green* ground, then *reddish*, with *white* in their centre, and moving about deforming the rings of the second order of colours. The *white* ring has now become *yellow*, the spots, with tails like tadpoles, and *green* inside, encroaching upon the second order of colours. The spots are now small, *brilliant red*, and one or two *blue* upon a *yellowish* ground. The *blue* of the first order is now being covered with *white* spots, and the *black*, with a sharp edge, constantly encroaching on the *white* and other colours. The *white* is now covered with *orange* tadpole spots, and the other spots changing their form, colour, and position. The film now burst.

In another concave film, which was very thin, there were six orders of colours, but no lens. The fifth and sixth orders disappeared by the advance of the others, and the third and fourth, occupying the centre, were broken up by numerous tadpole spots, some of them enclosing different tints. These tadpole spots are collecting, as if into a lens or large grey spot full of rings, surrounded by a *green* ring, round which is a *red* one. The film now burst from an accident.

In a third *concave* film a lens was beautifully formed within the third order of colours, and surrounded with a bright *red* ring of the third order, and ten bright *green* spots on a yellowish ground. The spots varied in colour and number till the film burst.

In a fourth concave film the changes continued till the whole film was *black*, the lens floating in the centre, and moving about with the motion of the glass.

When the concave film is thick, or the colourless fluid copious, the lens is sometimes the size of a shilling, but flat, with a few close and almost invisible rings at its margin. It then diminishes to the size of a sixpence, and takes longer time to pass throughthe changes above described.

IV. *Phenomena produced by different Solutions.*

When the films are produced from solutions of soap and water, they burst much sooner than those from glycerine solutions, and the changes in the coloured bands take place more quickly.

In a solution made by Mr Dewar of the University Laboratory, according to Plateau's receipt, with dry Oleate of Soda, the bands, in a vertical film, were produced more slowly, and the film lasted longer than when formed from the soap solution, neither of them being defended from currents of air. After standing half an hour, the third and fourth orders, and sometimes the second, were broken up by coloured tadpole portions streaming up from the lowest point of the film, as shown in Fig. 14.

With a solution which I made according to Plateau's receipt, but with humid Oleate of Soda, similar phenomena were more quickly produced. The second, third, and fourth orders were more easily and completely broken up, and the film burst sooner.

When the colours upon the film, from either of these solutions, were scattered by blowing, they reformed distinctly, and after being a second and a third time scattered by blowing, they recovered their original position and distinctness.

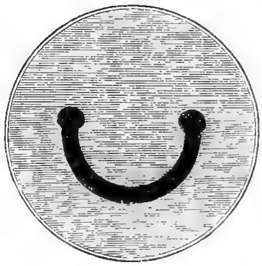
In a glycerine solution made by a London chemist, the phenomena were entirely different from those produced by other solutions. It had become so thick and ropy that it could not be poured out of the bottle. When a film of the usual size was produced from it, and placed vertically, it exhibited none of the phenomena we have described. It gave no bands, and when convex or concave, and placed horizontally, no rings or portions of rings were produced. The colouring matter, too thick to give colours, floated on the surface of the film in currents or little streams, and sometimes remained at rest in irregular patches. From these currents or fixed portions, streams of various and brilliant colours rose to the apex of the vertical film, with different velocities, from the bottom and sides of it, jostling one another, and, when crushed together by meeting a colourless portion at rest, losing their colour.

If, before any colour appears, we blow upon the film, it produces colourless eddies, like those shown in Figs. 9 and 10.

When the colours are produced on plane or curved films, the streams are singularly beautiful, assuming the most extraordinary shapes, and running from every part of the circumference. The coloured lines are sometimes serrated and sometimes mottled, black portions and portions of rectilineal bands occasionally appearing.

In some films *red* and *green* colours appear the instant they are made, and when this happens, the streams above described are more quickly formed.

When the solution that exhibits these phenomena is diluted with an equal quantity of water, the film which it gives is much smoother. Two or three serrated bands appear at its apex when it is placed vertically, and the streams of colour flow quickly from the circumference. In one of these films the singular black figure shown in the annexed diagram was produced. The film immediately burst before exhibiting any colours.



When the film from this solution is first made, it is almost always perfectly colourless, and remains so for a short time; but if we blow on it in this state, through a narrow tube, along a diameter or otherwise, the most brilliant systems of revolving rings are produced, as in Figs. 9 and 10, a result obtained from the soap-film only when the colours had previously existed upon it. The first rings that are seen in this experiment are colourless. The highest order of colours then appear in the centre of the system which is most influenced by the blast, and this is succeeded by the next lower order, till the centre becomes black, and the film bursts. When the blowing ceases, the colours often wholly disappear, the colouring matter being restored by a gradual rise of tint to its colourless state, as it was in the newly-made film. If we place the film vertically, after it has been blown upon, an approximation to mottled bands appears, the greater part of the film being colourless. If we blow upon the newly-made film with the mouth, instead of a tube, no colours are produced.

V. *On the Origin and Development of the Colours on the Soap-Bubble.*

It is impossible to witness the simplest of the preceding experiments without being convinced that the common theory of the colours of the soap-bubble is incorrect, and that they are not produced by different thicknesses of the elastic film which composes the bubble. A colouring matter of a very peculiar kind floats upon the bubble, or upon the film, as oil does upon water, or as the oil of laurel varnish of the late Mr Delarue does, in producing those magnificent colours which he succeeded in transferring to paper. In these cases, however, the colours are caused by the mere expansion of the oil or varnish into thin films, producing what Newton calls the colours of thin plates; but in the case of the soap-bubble the colours are formed by minute molecules, either of the soapy solution itself, or, what is more probable, by some of its ingrédients or elements expressed or secreted from it only when in the state of a film, forming, under the influence of gravity and their mutual attraction, the different orders of colours we have described.

This curious process, of which there is no other example in the production of colours, may be traced experimentally with the microscope from the formation of the elastic film till its explosion; but by what process the colourless secretion parts with the colorific molecules which compose it, and by what laws and affinities these molecules take their place in the different orders of colours, enter into those innumerable configurations which we have described, and finally return into their original colourless condition, are problems of great interest and difficulty.

When we examine the surface of a solution of soap, or of the glycerine mixture, in a vessel however shallow, no colour or colouring matter appears upon it. It reflects the images of objects, like the surface of water or glass; but as soon as it is blown into a bubble or formed into a film, its surface becomes for a time uneven, and reflects objects imperfectly, in consequence of its being covered with round-edged waves or tadpole portions of a colourless fluid moving from the margin of the film. When the film is held vertically, these portions rush chiefly to the circumference of the film, and, rising in streams of considerable velocity, give out molecules of different colours, and consequently of different sizes, to form the horizontal bands, carrying the smallest to the apex, to form the black of the first order, and those of a greater size to the bands of the higher orders.

When the colour molecules have thus arranged themselves, they may be scattered, as already mentioned, by blowing, or by pouring upon them some of the soapy solution, or even by brushing them from their place by a feather wet with the solution. Thus scattered over the film, they are singularly mixed together, so as to produce compound tints; but when the film rests in a vertical or horizontal position, they re-arrange themselves under the influence of gravity, taking their place in bands or circles as above described.

The mode in which these changes take place, and by which the horizontal bands are broken up, as shown in Fig. 14, is well seen when a portion of one or more bubbles adhere to the margin of the film, as represented in Fig. 15. When the bands of three or four orders are produced in this film, held vertically, the rush of coloured molecules, in the tadpole form, from the margin *mn* of the bubble B, disturbs, or effaces, as it were, the regular bands; and when the bubble is burst, or bursts spontaneously, it leaves a film behind it, enlarging the original film, and scattering all its colouring matter over the enlarged film. When there is a series of bubbles round the margin of the film, the effect of their bursting, or being burst, in succession upon the successively enlarged film is very beautiful. In one of these experiments the accidentally symmetrical position of four equal bubbles round a perfectly square central film, as shown in Fig. 16, was so remarkable as to deserve being noticed. On all these bubbles there were coloured rings, or rather curved bands, the colouring matter of which descended into the line of junction of the bubbles with the film, and rose to obliterate or disturb its horizontal bands.

In the preceding experiments the soap-film was fixed by capillary attraction to the rim of a wine-glass; but the general phenomena, and especially the formation and breaking up of the coloured bands, may be best seen upon plane films which are surrounded with other plane films, as in the figures of equilibrium discovered by Professor Plateau. When an elliptical plane film is formed between two wire rectangles crossed at right angles,* the four curved films in which it is encased pour upon it their colouring matter in the form of tadpoles, and speedily efface the horizontal bands which are formed upon it when held vertically. The streams of colouring matter which ascend to the apex of the film, along its delicate fluid rim, are seen to much greater advantage than in films adhering to a wine-glass or any other solid body. They are not impeded in their ascent by capillary attraction, and, in general, the currents are in succession of the same colour and order.

There seems to be no direct means of determining the nature of the colouring matter thus active upon the surface of the soap-film, but there is one fact which may prove useful in such an inquiry.

When a convex film was stretched across a conical wine-glass, and had stood ten minutes, it gave brilliant coloured rings of the first and second order. A lens, with its lower side concave, was placed above the film, and happened to be very near it. When the rings had increased to the third and fourth order, the film rose up, as if by attraction, or more likely from a slight increase in the temperature of the air within the glass, and broke upon the concave surface of the lens, leaving a ring of fluid 1·37 inches in diameter, and within it a distinct colouring matter, which continued visible for several hours.

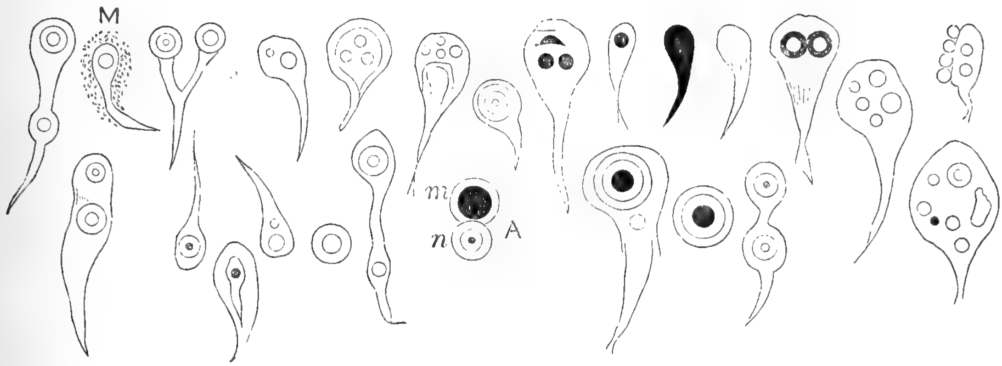
In order to see more distinctly the coloured rings thus embalmed, I covered the concave side of the lens with black wax, so as to reflect as little light as possible, and repeated the experiment. The film again rose and burst, and the colouring matter remained upon the wax *thirty-two hours.* Even when the wax is at some distance from the film, the experiment will always succeed by heating the air within the wine-glass, and increasing the convexity of the film till it touches the wax. The electrical excitation of the glass by friction will doubtless answer the same purpose. Whatever, therefore, be the nature of the colouring matter left on the wax, it cannot consist of a number of thin plates of soapy water.

These various phenomena may be finely seen and shown to others by placing the film in a cone of divergent light. The transmitted beam will exhibit the colourless fluid in the act of secretion from the film, rushing up in tadpole and other forms from its circumference to its apex, while groups of circular portions with dark edges and white centres descend from the upper part of the film. The horizontal colour bands while thus forming, and after being formed, will be seen magnified in the reflected beam, and may be farther magnified if we reflect them from a convex film.

In the formation and decay of the coloured bands, the portions of the coloured matter, which I have called tadpoles, perform a curious part. Their movements, whether we examine them singly, or when revolving in one or more masses, while other parts of the film are at rest, are very extraordinary; and their mutual actions, and apparent vitality, are so unlike anything that has been

* *See* the following paper.

hitherto described, that a more careful study of them may enable us to form some idea of their structure, and of the matter of which they are formed. A very few of their numerous and ever-changing forms are shown in the annexed diagram, and in Figs. 14 and 15. Their outline is generally well defined; and when they



are ascending through the colouring matter of any band they displace it, raising a little its tint at their margin, as at M. The breadth of their head seldom exceeds the thirtieth of an inch, and it increases with the size of the film. They carry in their heads, and also in their tails, colouring matter of various tints and orders; and when borne to the top of the films, as at A, by the lighter matter *m* of the first orders, they leave it in its place, and return with their heavier freight *n* to deposit it in the bands to which it belongs. Single tadpoles are often united together in the most capricious manner, both laterally and by their tails; and when the whole film is covered with them and without bands, they will lay themselves together, so that a band of a particular colour will be formed by their similarly-coloured tails, and another adjacent band by their similarly-coloured heads.

When a plane film is slightly inclined to the horizon, a number of colourless circular portions and colourless tadpoles will at first move over its surface, rising to its apex, chiefly round its circumference. These tadpoles gradually show colours of different orders, which are carried to their appropriate bands. A large portion of this colouring matter is frequently carried up the margin of the film to its apex, where the black band must be formed; but, as it cannot remain there, it moves slowly downward, reinforcing the bands to which it belongs, and leaving the apex for the formation of the black band.

When the black band is nearly as large as in Fig. 5, and there is colouring matter above it, it will cut its way through the black mass, dividing it into three or four portions, separated by narrow channels, through which the colouring matter moves down in small specks to join their proper bands. When this is done, the three or four portions of the black band unite, and the band expands itself over the whole film, pushing before it, in striped currents or in circular portions, the higher orders of colouring matter, which returns to its original colourless state, as shown in a former experiment, where, in a concave film, it was collected into a double convex lens, in the centre of the black film.

It is impossible to convey in language an adequate idea of the molecular movements and the brilliant chromatic phenomena, exhibited on the soap bubble; and it is equally impossible for art to delineate them. The visible secretion of a colourless fluid from a film less than the 12,000th of an inch in thickness—its separation into portions of every possible colour—the quick passage of these portions into bands of the different orders in NEWTON's scale—their ever-varying forms and hues, when the bands either break up spontaneously or are forcibly broken up—their conversion into revolving systems of coloured rings under the influence of a centrifugal force—their various motions when the film is at rest, and protected from aerial currents—their recombination into a colourless fluid, when driven to the centre or the margin of the concave or convex films—and their reabsorption by the film by means of mechanical diffusion—are facts constituting a system of visible molecular actions, of which we have no example, and nothing even approaching to it in physics.

The phenomena of colour, described in the preceding experiments, are more various and beautiful than any I have ever witnessed, whether caused by refraction, or by thin plates under the influence of common or polarised light. The compound tints produced by ever-varying changes in the combination of the differently-coloured molecules, have a brilliancy and a peculiarity of hue which I have never before observed; and I am persuaded that, if we could examine them prismatically under the microscope, we should obtain remarkably banded spectra.

Owing to the small size of the soapy film, it is not easy to show these colours to more than one individual, but it would not be difficult to exhibit them by the magic lantern to the largest audience.

V. *On the Mode of Producing Plane and Curved Films, and Examining the Phenomena they Exhibit.*

In conducting experiments on the colours of the soap-bubble, it is of great importance to be able to produce films perfectly plane, and films with various degrees of convexity and concavity, to protect the colouring matter upon them from currents of air, and get rid of the extraneous light by which the phenomena may be obscured.

1. *Plane Films.*—Plane films are often produced by dipping the rim of a cylindrical wine-glass in the soap solution; but they are frequently convex, and sometimes concave, results which I cannot explain.

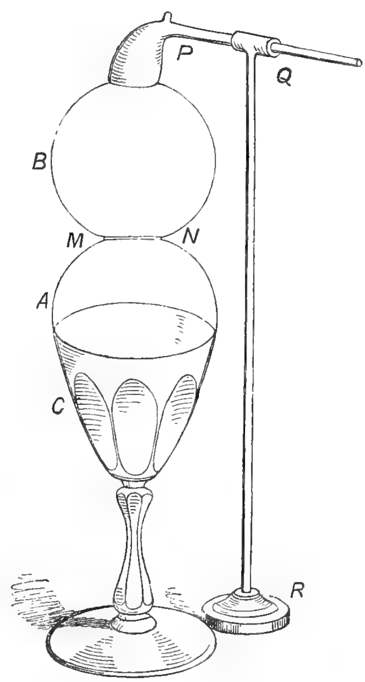
Surfaces almost perfectly plane are invariably produced by using cylinders of glass or metal, open at both ends. They are more perfect when the metallic rim is smooth and accurately circular.

A good plane film may be produced in a very singular way. If we deposit a bubble on the mouth of a cylindrical or conical wine-glass, a little less in diameter than that of the bubble, the bubble, according to its size, will leave on the glass one-third, or one-fourth, or one-fifth of itself as a plane film, and will stand above the film two-thirds, three-fourths, or four-fifths of a sphere.

If we deposit the same bubble upon a cylinder of glass or metal, open at both ends, it will deposit the lower portion of itself as a concave film upon the cylinder; and if we burst the bubble, the concave film will start into a plane one. Another method of producing perfectly plane films has been already described.

2. *Convex Films.*—A convex film is frequently produced upon a cylindrical wine-glass, but always upon a conical one; and we can easily convert a plane or concave film, when formed upon a *closed* cylinder, into a convex one, by heating the air within the cylinder. In some cases I have thus converted a plane film into nearly a complete sphere. The same result may be obtained when the film is at one end of a long *open* cylinder, by plunging the lower end in water.

3. *Concave Films.*—Films of this kind are less easily obtained than those which are plane and convex. They are often produced upon a cylindrical wine-glass, as has been already stated; and they may be always produced by depositing a bubble upon the end of an open cylinder.



4. *Plane, Convex*, and *Concave Films.*—All these films may be obtained in succession by the juxtaposition and partial union of two soap-bubbles of the same or different sizes.

Let A be a bubble, deposited upon a wine-glass C or an open cylinder, and B another bubble laid upon A, and kept there by the pipe PQ, supported upon a stand QR. The two bubbles will be separated by a film MN. If the bubble A is equal to B, the film MN will be plane. If A is *greater* than B, MN will be *concave;* and if A is *less* than B, MN will be *convex.* If, when A is *equal* to B, we enlarge B by blowing through QP, MN becomes *convex.* If we then diminish B, by sucking out the air at Q, MN becomes *concave.* In all these three forms the film MN is competely protected from air, and almost completely from extraneous light.*

* After I had used this method of producing the three varieties of films, I found that M. Plateau had long ago discovered the relation between the size of two united soap-bubbles and the curvature of the film which separates them.

In protecting the colouring matter upon films from currents of air, I at first employed watch-glasses and plates of parallel glass, but the protection was not complete; and the phenomena were obscured by the vapour deposited upon them, and by the light reflected from their surfaces. When I discovered the method of forming films beneath bubbles, and by the union of bubbles of different sizes, I was able to protect the colouring matter completely from currents of air, and at the same time to get rid of the aqueous vapour and the reflected light.

In examining films produced upon wine-glasses or other bright substances, the brilliancy of the colours is greatly impaired by the light which these substances reflect; but if we cover the inside of the glasses with black varnish, or blacken it with smoke, we remove entirely the reflected light, and give great distinctness and brilliancy to the phenomena.

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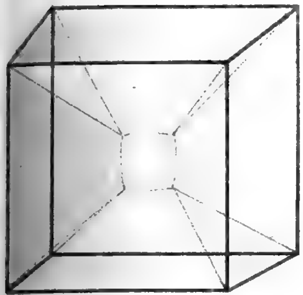

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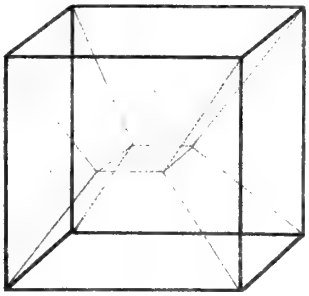

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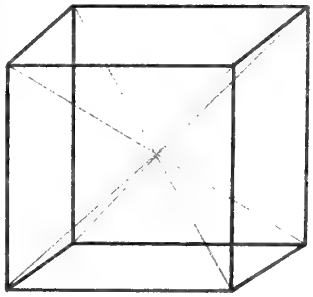

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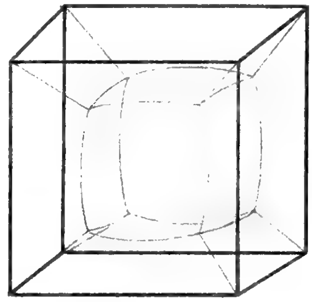

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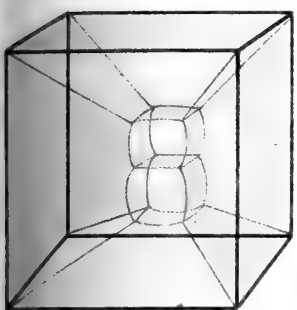

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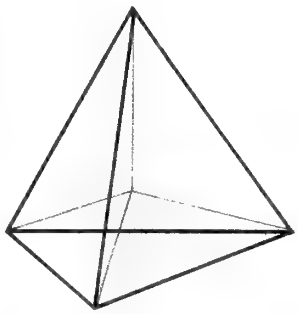

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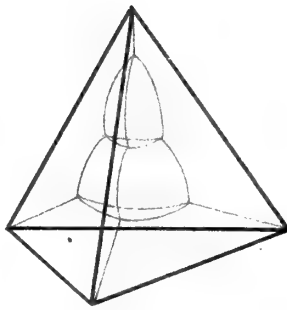

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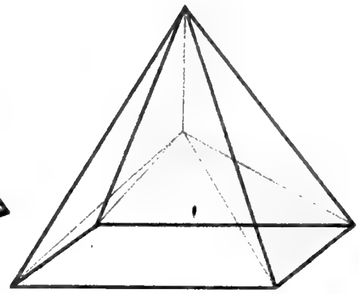

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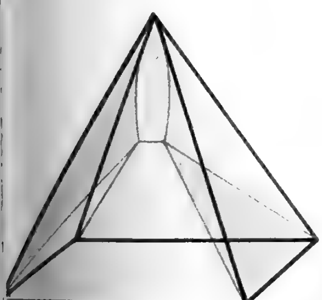

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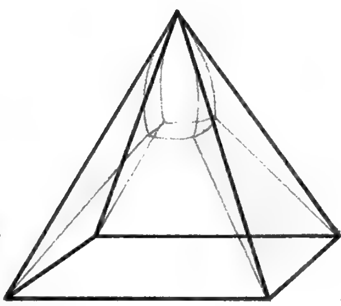

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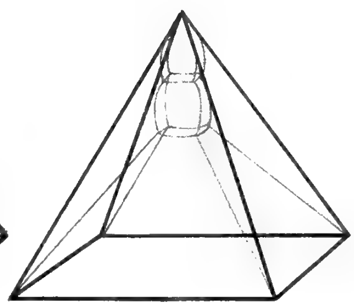

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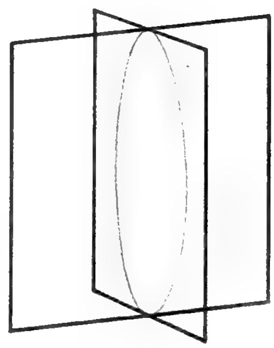

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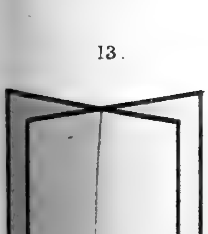

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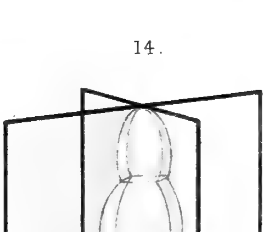

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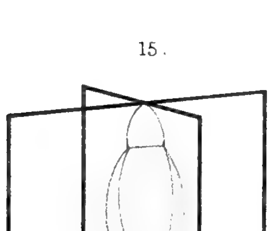

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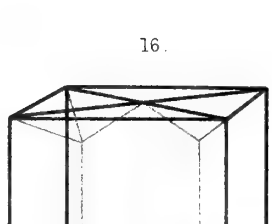

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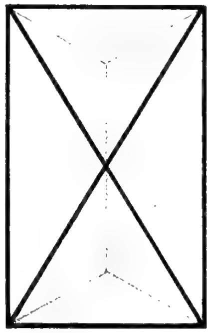

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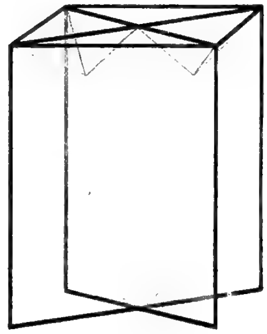

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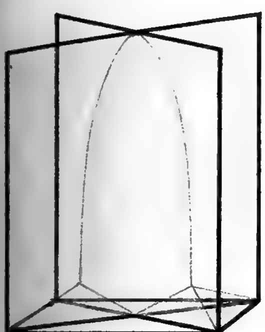

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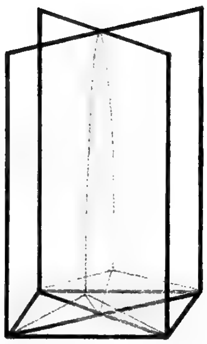

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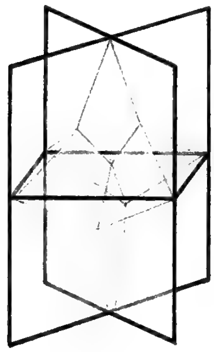

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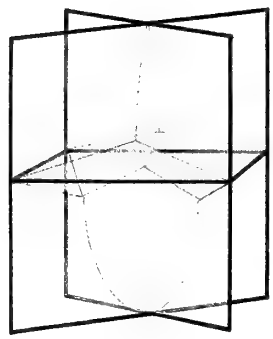

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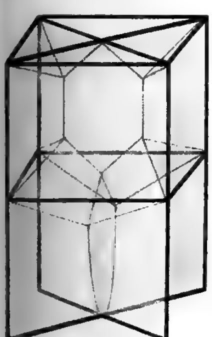

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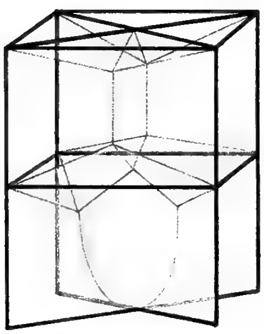

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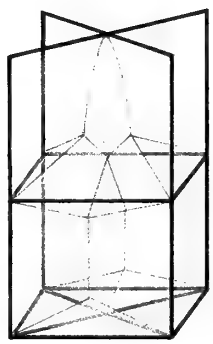

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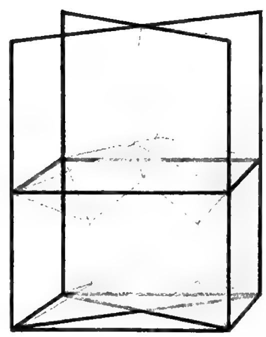

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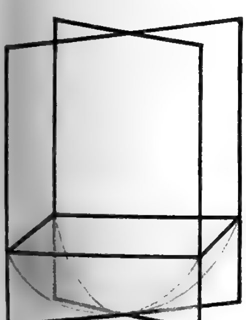

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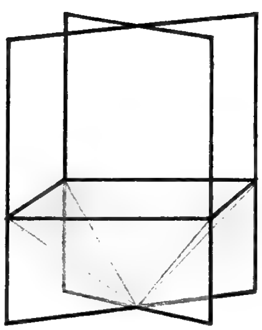

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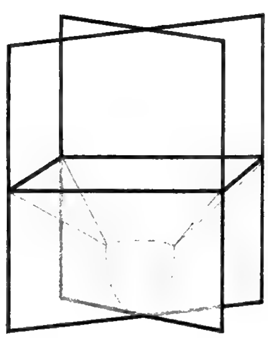

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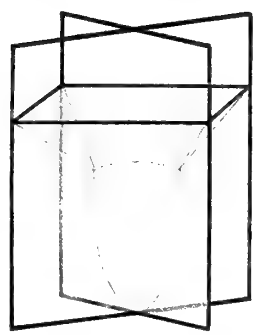

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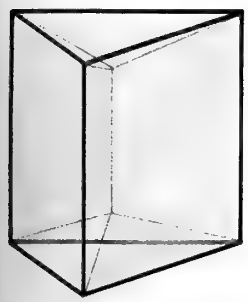

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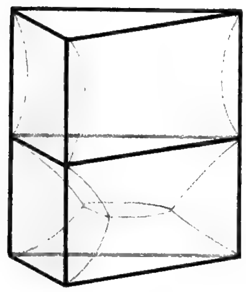

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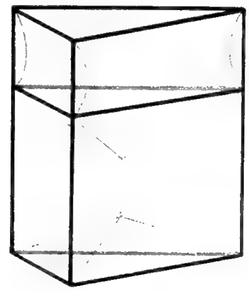

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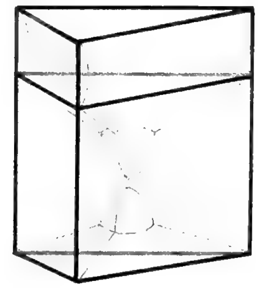

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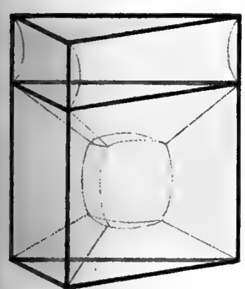

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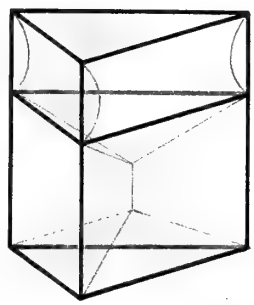

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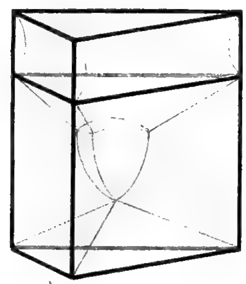

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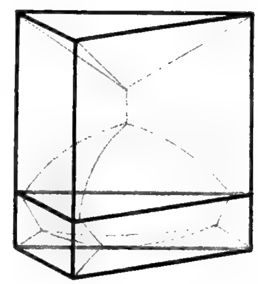

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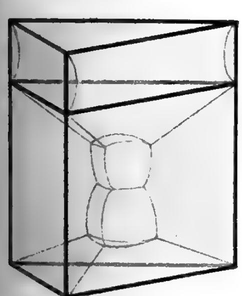

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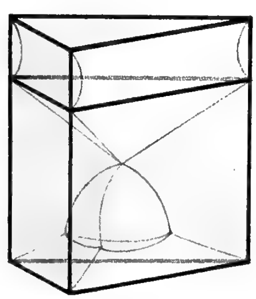

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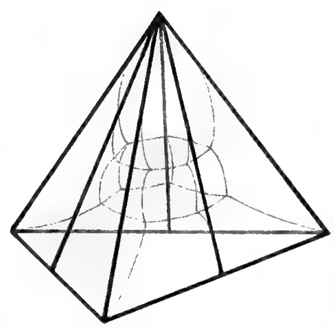

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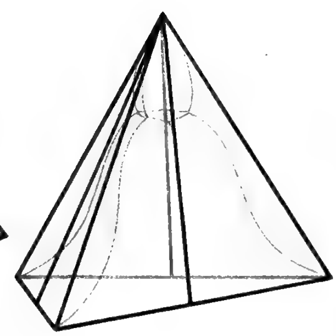

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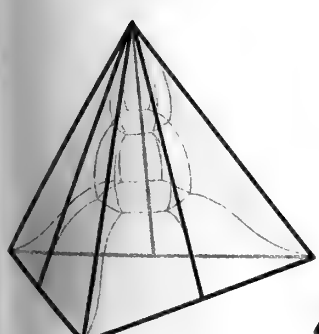

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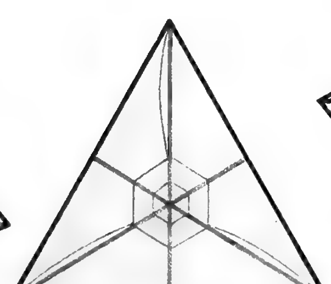

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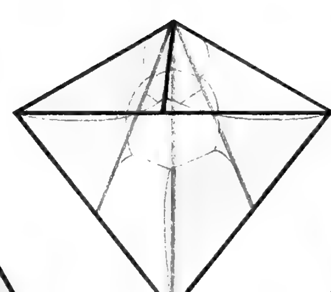

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XXXIV.—*On the Figures of Equilibrium in Liquid Films.*

By Sir DAVID BREWSTER, K.H., F.R.S. (Plates XXXIV., XXXV., XXXVI.)

(Read 4th February 1867.)

In repeating some of the beautiful experiments of Professor PLATEAU, on the Equilibrium of Liquid Films, contained in seven Memoirs, published in the "Transactions of the Royal Belgic Academy,"* and in prosecuting my experiments on the colours of the soap-bubble, I observed several new phenomena which may have escaped the notice of the Belgian philosopher.

In plunging a wire cube in a solution of soap, and lifting it up vertically, Professor PLATEAU found that there was formed within it a polyhedron, as shown in Plate XXXIV. Fig. 1, consisting of *twelve* similar liquid films adhering by capillary attraction to the twelve wires which compose the cube, and a small quadrangular film suspended in the middle of them. In many cases M. PLATEAU found that the vertical quadrangular film was often horizontal, as in Fig. 2; and M. VAN REES discovered, that by blowing very lightly upon one of its sides it was reduced to a simple line, and then reproduced in a horizontal position, from which it could be blown again into a vertical position, as in Fig. 1.

Considering that a system formed of twelve films joined at the centre of the cube, as shown in Fig. 3, would, on account of its perfect symmetry, be a system of equilibrium, M. PLATEAU was surprised to find that it could not be produced and rendered stable, without introducing something solid into the system. To do this, he stretched a fine iron wire from one summit of the wire cube to the opposite summit, and having, after immersion, withdrawn it from the glycerine solution, there was at the central point a small quadrangular film which slowly got less and less, till the system in Fig. 3 was produced. M. PLATEAU subsequently found that this system was permanent, when a small drop of the glycerine solution was accidentally retained at the centre of the system. As this drop could exist only as a sphere, it is not easy to understand how the twelve triangular films could be united with it at their apex, so as to produce a state of stable equilibrium.

In repeating these interesting experiments I found that the polyhedron, with a horizontal quadrangular film, Fig. 2, was always produced in the solution I employed, and by blowing through a narrow tube, either upon the edge or the middle of it, it was changed, as VAN REES found, into a vertical quadrangular film, as in Fig.1; but by a longer blast, it passed into the unstable system of Fig. 3,

* Memoires de l'Academie Belgique, tomes xvi. xxiii. xxx. xxxi. xxxiii. and xxxiv.

and after continuing a very short time in that state, it returned into the system of Fig. 1, and then into the original system of Fig. 2. In one experiment it became permanent, as in fig. 1, and did not pass into Fig. 2. In several experiments the system in Fig. 3 remained permanent, with a drop of fluid in the centre, as observed by PLATEAU, but upon examining it with the microscope, it turned out to be *a hollow cube*, which, as we shall presently see, forms with twelve films a system of stable equilibrium.

If we make these experiments with a wire rhomb, the same results will be obtained with such differences as might be expected from the inclination of the films to each other.

When M. VAN REES had produced the normal polyhedron shown in Fig. 1, he happened to dip the lower part of the wire cube into the solution to the depth of some millimetres, and upon lifting it out again, he obtained the beautiful system shown in Fig. 4, where the quadrangular film is replaced with a hollow cube, all the faces of which are curved. When the wire cube is thus dipped into the fluid, a film, as M. PLATEAU observes, is formed on its lower square. This film imprisons the air between itself and the oblique faces of the polyhedron immediately above it, and rising, forms the hollow cube shown in the figure.

As the new film which produces this effect is coincident with the lower face of the wire cube, the hollow cube which it generates must be invariable in size, containing the same quantity of air which is imprisoned in the lower portion of the polyhedron; that is, the contents of the hollow cube must always be equal to one-fourth of the contents of the wire cube.

In repeating this experiment, which does not always succeed, I discovered a general method of introducing into the normal polyhedron hollow cubes of any size, from the smallest to a size which nearly fills the whole interior of the wire cube. This is done by blowing a bubble of the requisite size, and placing it in the central quadrangle of the polyhedron. The bubble instantly starts into a hollow cube, containing the same quantity of air as the bubble, obliterating the quadrangular film, and forming a system of perfect equilibrium. If the cube is smaller than we wish it to be, we can enlarge it by introducing another bubble. As hollow cubes of every size form a system in perfect equilibrium, we see the reason why the microscopic cube, already mentioned, kept the system shown in Fig. 3 in stable equilibrium.

By the same method we may introduce a second hollow cube beside the first, displacing it from the centre of the polyhedron, and, along with it, taking up a symmetrical position of equilibrium, as shown in Fig. 5. The two cubes are not necessarily equal, but when they are so, the side common to both, and passing through the centre of the polyhedron, is perfectly plane, while all the other sides are curved. The second cube may be inserted on the right or left side of the first, as well as above or below it; but it sometimes happens that the bubble intended to produce it passes into the first cube and enlarges it, instead of taking its place beside it.

This method of inserting *one* or *two* hollow solids of any size in the figures of equilibrium of liquid films, may be extended to the different figures discovered and described by Professor PLATEAU. I have applied it successfully, as shown in Figs. 7, 10, 11; Figs. 6, 8, 9, 12, being the figures given by PLATEAU; and also, as shown in Figs. 14, 15, to a remarkable system of wires, shown in Fig. 13, &c. &c. which had not previously been the subject of experiment.

In Fig. 6, where the wires form a tetrahedron, the bubble is introduced at the centre where the four films meet, and the double three-sided hollow figure which is thus produced is shown in Fig. 8.

In Figs. 8 and 9, given by PLATEAU, where the wires form a quadrangular pyramid, the bubble is introduced where the vertical line or film joins the other four, and the single and double quadrangular figures thus produced are shown in Figs. 9, 10, and 11.

In Fig. 12, given by PLATEAU, where the wires form two rectangles cutting one another at right angles, a plane film, of nearly an elliptical shape, is formed in the centre. It is inclined 45° to the planes of the rectangles, and is attached by each of its sides to twocurvilineal films, which adhere to the four vertical wires. The figure is thus composed of *five* films, *one* plane and *four* of a singularly curvilineal form, as in Fig. 13. The hollow quadrangular figures produced by introducing one or more bubbles into the centre of this elliptical film, or between the curvilineal films, in Fig. 13, is shown in Figs. 14 and 15.

In all these systems of films, when two hollow figures are united, whether they be spheres, lenses, cubes, or any other irregular figures, the film which unites them is *plane* if the contents of the two hollow figures be equal, and *concave* and *convex* if their contents are unequal—the *convex* side being always within the largest figure.

Professor PLATEAU does not appear to have studied the singular effects produced by constructing the system of wires, in Fig. 12, so as to vary the angle formed by the two rectangular planes. With such a movable system we see at once why the elliptical plane is formed between two of the pairs of right angles, rather than between the other pair, for it must necessarily appear between the pair whose angle is greater than 90°; and as it is impossible to join the rectangles with mathematical accuracy at right angles to each other, the oval plane should appear only between the wires whose inclination is greater than 90°. This is true only when the rectangles are lifted perpendicularly out of the soap solution; for if they are lifted out obliquely, the oval film will be formed in the angle which is uppermost, whether that angle is greater or less than 90°, provided that the difference of the angles is not great. In the normal position of the rectangles perpendicular to each other, the major axis of the elliptical film is about four times greater than its minor axis; but if we increase the angle of the planes, the minor axis will gradually increase till it becomes equal to the major axis, the oval plane becoming rectangular when the planes are inclined 180°, a position which cannot be experimentally obtained. While this change is going on, the four curvilineal films, to which the sides of the elliptical plane are attached, are gradually diminishing, and disappear at 180°. During this expansion of the elliptical film, it is not stretched and made thinner, because it appropriates to itself the fluid of the *four* curvilinear films, which at 180° it extinguishes. If we now diminish the angle of the rectangles, the enlarged oval plane will gradually become more elliptical, giving back its fluid to the four curvilineal planes, till at 90° the plane film resumes its normal size.

If, when in this position, we *diminish* the angle of the rectangular planes, the *minor* axis of the elliptical plane will gradually *diminish*. At 45° it will become a straight line and disappear. The elliptical plane will start into the angle of 135°, pushing towards their wires the four curvilineal films, placing itself between them, and in an enlarged state appropriating a portion of their fluid.

Remarkable as these phenomena are, there is one still more remarkable, which requires the testimony of the eye to make it credible. If in the normal or rectangular position of the rectangles we blow upon the oval film, or between the curvilineal ones, a bubble of the proper size, it will replace the system of films with a hollow curvilineal cube, the sides of which will project beyond the faces of the vertical cube, which, having plane faces, would not project beyond the wires. Within the four triangular spaces at the upper side of this cube, will be four summits where the black spot of the first order in NEWTON'S scale will be produced, and at which the bubble will burst. If we now hold the wires vertically, the cubical bubble will burst, and *the system of liquid films which it expelled will reappear, as if it had left its ghost behind it, to recover the elements which the bubble had appropriated!* When the wires were held horizontally this resurrection of the normal system of films did not take place, owing, I presume, to the bubble not bursting symmetrically. The same results will be obtained when the inclination of the rectangles is above 90°.

If we introduce the bubble into the system of wires when empty, the system of liquid films is equally reproduced; but the experiment succeeds better when the bubble is laid upon the oval film, as it thus appropriates the fluid of the different films, and when it bursts there is a greater quantity of fluid for their re-formation. For the same reason, the reproduction of the films is produced if the bubble is burst when it is strongest, by thrusting into it a piece of blotting-paper.

The same restoration of the figures of equilibrium, produced in the tetrahedral and quadrangular systems shown in Figs. 6, 7, 8, 9, and 10, may be effected by introducing bubbles of the proper size, whether they are empty or occupied by their respective films.

These phenomena were so unexpected, that I thought it probable that interesting results would be obtained by the following alterations of the system of wires in Fig. 12.

1st. By uniting the upper and lower ends of all the wires, as shown in Fig. 16.

2d. By uniting only the upper ends, as shown in Figs. 19, 20.

3d. By uniting only the lower ends, as in Figs. 21, 22.

4th. By uniting the middle of the wires, as in Figs. 23, 24.

5th. By uniting the middle, and also the upper and lower ends of the wires, as in Figs. 25, 26, 27, and 28.

6th. By uniting the wires at various points successively from their lower ends, till they reached the position in the second system, as in Figs. 29, 30, 31, and 32.

1st. In the *first* system, Figs. 16 and 17, we have *four* vertical and equal rectangles, and *eight* horizontal and equal right angled triangles, each of which is concerned in producing the complex figure of equilibrium, shown in Figs, 16, 17, and 18, and consisting of twenty-one films. As the wires are bound together, we cannot vary the angle of the rectangles from 90° to 180°, in order to see that the same remarkable changes will take place, as by varying the angle in fig. 12; nor can we show, that by diminishing the angle from 90° to 45°, the central film will become a line, and then start into the angle of 135°, but we can prove it by uniting the wires when the rectangles are variously inclined to each other.

In the normal state of this system, and in its various conditions from 90° to 180°, two of the right-angled triangles, and two at the bottom in front of the plane central film, contain a plane liquid film, as in Fig. 18,* and the other two ahollow triangular pyramid of films. When the central film is reduced to a line, and at 45° should start into the angle of 135°, the law of equilibrium demands that the four plane films should sink into triangular pyramids, and the four triangular pyramids rise into plane films!

If we now introduce bubbles of different sizes into the different angles of this system of wires, the figures of equilibrium are very various and beautiful, changing with the inclination of the rectangular wire planes, and the size of the bubble.

2d. When the rectangles are united only at their *upper* ends. In this case, the system shown in Figs. 19, 20 consists of thirteen films, the central one of which is a semi-ellipse nearly, with an angular summit upon its minor axis. The four films adhering to this film, and the vertical wires, are curved, and the remaining eight are the same as those shown in Fig. 18.

3d. When the rectangles are united only at their *lower* end. In this case the system shown in Figs. 21, 22 is the same as that in Figs. 19, 20, *inverted.*

4th. When the *middle* of the rectangles only are united. In this case the system shown in Figs. 23 and 24 consists of *sixteen* films—those in the upper half being similar to those in the lower half.

* Fig. 18 is an enlarged vertical view of the upper face of figs. 16, 17.

5th. By uniting the *middle* and the *upper* ends of the wires. In this case the system shown in Figs. 25, 26 consists of *twenty-four* films.

6th. When the *middle* and the *lower* ends of the wires are united. In this case the system shown in Figs. 27 and 28 consists of *twenty-four* films, and is the preceding one inverted.

7th. When the wires are united at several points successively from their lower ends, till they approach the position of the second system, in Figs. 19, 20, the fluid never reaches the four triangles at the top. In this case the films are *eight* in number in Figs. 29 and 30, and *nine* in Figs. 31 and 32. The *curved* films in Fig. 29 are changed into *plane* ones in Fig. 30 by raising the movable wire; andby raising it higher, a portion of a plane elliptical film is added beneath the quadrangular pyramid, and this film increases in altitude till the movable wire reaches the top, when the whole figure is similar to that in Figs. 19 and 20.

Professor Plateau has given the figure of equilibrium in an equilateral triangular prism when its height is equal to one of its sides. It is very simple, consisting only of eight films, as shown in Fig. 33. By dividing its height by means of a movable equilateral triangle, I have obtained very curious figures. When the movable wire bisected the height of the prism, I obtained two different figures—the one as frequently as the other, and yet they had not the slightest relation to each other. The most careful adjustment of the movable triangle did not help me to determine which of the two belonged to the bisection. I found, however, upon dividing the height of the prism unequally, that the one figure belonged to the larger, and the other to the smaller half—the prism, in the position of indifference, being guided in its choice of figure by some trifling cause which I failed to ascertain. These figures are shown in Figs. 34, 35, 36, 37, 38, 39, 40, 41, and 42.

Having obtained such curious systems of films by dividing the equilateral prism into portions of different lengths, I was anxious to see the effects produced in the triangular and quadrangular pyramids by wires passing from their apex to the middle of the sides of their base.

The systems of films produced in the triangular pyramid are shown in Figs. 43, 44, 45, 46, 47, where the hexagonal figures are very beautiful. The hexagonal faces are plane, convex, or concave, according as the contents of the hollow figures which they separate are equal or unequal—the face being generally concave. A view of the hexagons, as seen by looking down through the apex of the system, is shown in Fig. 46, while Fig. 47 represents them as seen by looking upwards through the base of the pyramid. The curves of contrary flexure, and the convex ones joining the films adhering to the six vertical curves, and the curved films which they bound, are not easily represented by simple lines.

The systems of films produced in the quadragular pyramid are similar to those in the triangular pyramid, the figures being octogonal in place of hexagonal, as shown in Fig. 48.

The figures of equilibrium of liquid films are finely seen in the union of spherical bubbles and hollow lenses. When two spheres are brought into contact they are united by a film common to both.* When the one is laid above the other, the lower part of the upper one starts into union with the upper part of the lower one, and forms a single circular film. If the upper sphere is retained by the pipe that blows it, it may be rotate upon the other, as upon a joint, without any change in the uniting film. If we now blow through this pipe we can enlarge the upper sphere, and if we suck through it, we can diminish it; so that we can at pleasure make the two spheres equal or unequal. When the upper sphere is equal to the lower, the uniting film is a circular plane. When it is greater than the lower, the uniting film is *convex*, and when it is less, the uniting film is *concave*, the convex side being always turned to the larger sphere.†

When two lenses are united by a film, the film is *plane*, *convex*, and *concave*, according as the one lens is *equal* to, or *greater* and *less* than the other. A double and equally convex lens may be formed at pleasure, as we shall presently see; but I have not been able to bring the lenses into union. They are, however, frequently found in that position in experiments with the wire cube.

The production of plane, convex, and concave films by the union of two bubbles, and the protection of these films from aerial currents by the superincumbent bubble, I found of great use in studying the colours which they produced.

The formation of films, by immersion, upon open and closed vessels of different shapes, their deposition on the same vessels from bubbles, and their motion within certain vessels, which I believe has never been observed, present many curious phenomena.

When a film is formed by immersion on the rim of a closed cylindrical vessel it is generally plane, but sometimes concave, and rarely convex; but in all cases it may be made of any degree of convexity by the application of heat to the vessel. When the vessel is open at both ends the film is always plane.

When the film is formed on a closed cylindrical vessel by the deposition of a bubble upon its rim, it is always plane. The bubble leaves the lower part of itself upon the rim as a *plane* film, but quite separate from the rest of the bubble which stands over it.

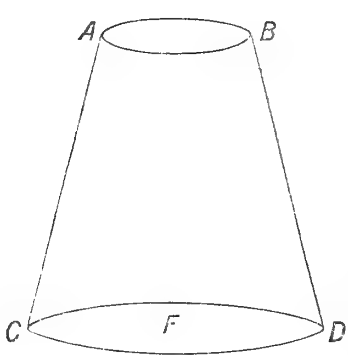
If we use a thick metallic ring with a broad rim, which itself gives a plane film by immersion, a bubble laid upon it deposits a *concave* film, which at first appears to be the lower end of the bubble, but it is quite independent of it in position, as the bubble rests upon the upper and outer side of the ring, while the concave film adheres to the inner and lower end of the ring. They are, however, so related to one another, that when the bubble bursts the concave films starts

* See the preceding paper, p. 503.

† The relation between the size of the spheres and the curvature of the uniting film was first observed by M. PLATEAU.

into a plane. When the diameter of the bubble is equal to that of the ring, the ring will form its equator, and the bubble will be a perfect sphere passing into a plane film when burst. If, when the bubble is a complete sphere, with its metallic equator, we place another upon it, and alter its size by blowing and sucking through the pipe that holds it, we shall observe the production of *plane*, *convex*, and *concave* films, which has been already mentioned. When the bubble bursts, a double hollow convex lens is often formed with particular solutions, though a plane film is most frequently the result.

When a bubble is placed upon the mouth of an open cylinder like the chimney of a lamp, the glass being quite dry, it will deposit a film, which will immediately move down the tube about an inch, and the bubble will burst, leaving another film in its place. Both these films sometimes remain, the *uppermost* being *convex* and the other *concave*. If we close the lower end of the tube by plunging it in a tumbler of water, a bubble laid upon its mouth will deposit a plane film there. If we now lift the tube slowly, the plane film will descend, becoming concave before it bursts. By continuing to lift the tube the truncated bubble will grow less and less, till it becomes a plane film about the eighth of an inch below the rim of the tube. If we now depress the tube the film will gradually rise to the primitive bubble. When the bubble is small it descends about one or two inches as a convex film. If the diameter of the primitive bubble is a little less than the diameter of the tube, it generally deposits itself within the tube in the form of a semicylindrical film.



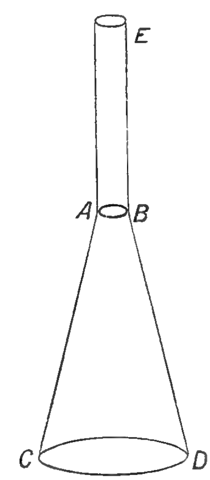
In all conical tubes closed at one end, the film, taken up by immersion, or deposited by a bubble of considerable size, is always convex. The film is also convex in closed cylindrical tubes if the mouth is slightly widened as in test tubes. If the bubble deposited upon cylindrical tubes of this kind is very small, but not less in diameter than their own, the film is plane, and sometimes slightly concave, descending a little in the tube. If the bubble is smaller still, it forms itself into the semicylindrical film already mentioned.

If we employ a truncated cone the phenomena and motion of the films are very remarkable. When the cone ABDC is closed at CD, the other end, AB, gives by immersion a *plane* film. When a bubble is laid upon AB it forms a concave film. Upon admitting the air gradually at CD, the concave film descends, *expanding* itself into a larger plane film which breaks at CD. The bubble then follows it, leaving a plane film at AB. When the cone ABCD is closed at AB, it gives, by immersion, a *convex* film, which by the gradual admission of the air at AB descends as a plane film contracting itself to AB.

If, when both ends of the cone are open, we take up, by immersion, a film upon the larger side BC, it will rise to the top AB, gradually *contracting* itself, and remaining at AB. If there happens to be a drop of fluid retained at F, its weight will prevent the film from rising, but if we run off the drop F at one side, the film will immediately rise to AB. The height of the glass cone ABDC is $1\frac{1}{2}$ inch, its breadth AB 1·1 inch, CD 1·7, and its angle 13°.

When the bubble, placed at AB, is not much larger in diameter than AB, it will descend with the film, leaving another film at AB. Upon breaking AB the film CD will rise to AB.

If we employ a glass funnel ABCD of the same size and form as the preceding figure, and close it at E, the film which it takes up at CD will be *convex*, but if we open it at E it will take up a plane film which will ascend, breaking at AB, and leaving a film there. If we now take up another film by dipping CD the eighth or tenth of an inch in the solution, the compression of the air will drive the film AB out of the tube at E. If we replace AB by making a film at CD ascend to AB, and dip CD into the solution so slightly as not to compress the air and drive out AB, we shall obtain a *convex* film at CD, which will remain there. By breaking AB with a strip of blotting paper, the *convex* film CD, with its coloured rings, will become plane and rise to AB, the rings formed on the convex film being changed to bands when the film has become plane.



I was anxious to ascertain what was the largest angle of a cone at which it would raise the film at its mouth to the aperture at its apex. Upon using cones made of thin card-board, I found that the film rose rapidly when the angle was upwards of 90°, but when it reached 120° or 130°, a film could not be lifted from the soap solution. It was probable, however, that the cone did lift the film and carry it to its apex, as there was always the appearance of fluid at that place. By placing the solution in a vessel shallower than the height of the cone, and observing that there was no film at the apex previous to lifting it out of the fluid, I always found that a film was at the apex, and consequently that a film had been formed at the mouth of the cone, but rushed so rapidly upwards that it could not be seen there.

XXXV.—*On the Third Co-ordinate Branch of the Higher Calculus.* By Edward Sang, Esq.

(Read 15th January 1866.)

That part of universal arithmetic which relates to the changes of variable quantities, and which is known under the titles—*Fluxions, The Higher Calculus, The Infinitesimal Calculus, The Theory of Functions,* has been divided into two branches, called respectively the *Differential* and the *Integral Calculus;* the one of these being regarded as the converse of the other; and every problem connected with variation has been supposed to require either or both of the processes known as *differentiation* and *integration.*

This two-sided view of the higher calculus arose naturally in the course of its development. When we study the changes of a variable quantity, our attention is called to its differences and to the velocity of the change. The first branch of the subject is thus that which teaches us to pass from the variable quantity to its differential or differential coefficient; and, as our attention has been engrossed by the mutual relations of the variable and the differential, the converse problem "to pass back again from the differential to the integral" seems to be its complete complement.

Just so, in the progress of algebra proper, attention was drawn to the squares and cubes of numbers and to their higher powers. Thereafter it was directed to the inverse problem "from the power to compute the number or root," and thus algebra was divided into two parts treating, respectively, of Involution and Evolution; these two processes seeming to be the complete converses of each other.

However, later arithmeticians, regarding the subject more comprehensively, have considered that there are three related things, the root, the index, and the power, and that, therefore, there are three branches of the subject or three problems—the first, when the root and index are given and the power sought; the second, when the index and the power are given and the root sought; and the third, when, the root and the power being both given, the index is sought. The last of these problems was resolved by Nepair; it is the exponential or Logarithmic problem.

In the present paper it is my object to indicate that the Theory of Variables has, like Algebra proper, a third co-ordinate branch bearing to the differential and integral calculi relations somewhat analogous to those which the Theory of Logarithms bears to Involution and Evolution.

In the process of differentiation, the variable quantity is regarded as a function or dependent of some primary variable, and the ratio of its change to the change of this primary is sought for; this ratio being the differential co-efficient, or, as LAGRANGE calls it, the *derived function*. Here we have three connected variable quantities, the *primary*, the *function*, and the *derivative;* and thus we have three distinct problems characteristic of the three great branches of the calculus.

In the first of these branches the leading problem is this,—having given the relation between the primary variable and its function, to discover the derivative. In the second branch, the relation between the primary variable and the derived function being given, the primitive, or, as it is called, the integral, is sought. While the province of the third branch is to discover the primary, when the relation between the primitive and derivative functions is given. This third branch may thus be called the Calculus of Primaries.

This classification of the different branches of the calculus of variables is complicated by the existence of various orders of derivatives. Besides the three variables quantities, viz., the Primary, the Function, and the Derivative, there is the order of derivation to be considered, and, therefore, it may be argued, there must be four complementary problems, the fourth problem being that in which the order of derivation is the *quæsitum*, the Primary, the Function, and the Derivative, being the *data*. The mutual relations of these problems have been obscured partly by circumstances incidental to the subject, and partly by the peculiarities of the notation employed. The notation used by NEWTON, and revived in a slightly modified form by LAGRANGE, is essentially defective; while that contrived by LEIBNITZ, and now commonly in use, is cumbrously redundant. In order to denote the derivative of a function, LAGRANGE places an accent over it; to indicate the second derived function he places two accents, and so on. The radical defect of this notation is apparent, when we consider that a variable quantity U may be regarded as a function of one or of another primary, and that the symbol $'U$ contains no indication of the primary; thus $^{\text{IV}}U$ is simply the fourth derivative of the variable U. On the other hand, the notation of LEIBNITZ is explicit on this score, the symbols $\frac{d^4U}{dx^4}$ and $\frac{d^4U}{dy^4}$ indicating the fourth derivatives of the variable U, regarded in the one case as a function of x, in the other case as a function of y; these symbols also paint, as it were, the process of derivation; they, however, contain a redundancy of parts: the symbol of derivation is twice written, and so also is the index of the order. Now, essentially, there are four indications to be made; we need the sign of derivation, its order, the function to be operated on, and the primary in regard to which the derivation is to be made. The sign of derivation may be given by a conventional arrangement of the characters, just as in products and powers; there remain, therefore, three things to be symbolised,—the variable operated on, the primary or argument of which it is regarded as a function, and the order of derivation. The relative positions of these symbols is a matter of mere taste or convenience. I have adopted the arrangement of writing the numeral of the order and the primary variable as ante-subponents to the function—thus I use the formula ${}_{2t}U=Z$ to denote that Z is the second derivative of U, regarded as a function of t. It is impossible to indicate the proposed relation with fewer letters, and any more would be redundant. The same relationship is expressed, according to LEIBNITZ's notation, by

$$\frac{d^2U}{dt^2}=Z.$$

When we desire to indicate the opposite relationship, that is, to state that U is the second primitive of Z, the usual notation is $\iint Zdt^2=U$, whereas by using the ordinary sign of reversion, viz. —, we may indicate the same thing by ${}_{-2t}Z=U$, that is to say, U is the second primitive of Z, regarded as a function of t. In this way the expressions, etc.,

$$_{-3t}x,\ _{-2t}x,\ _{-1t}x,\ x,\ _{1t}x,\ _{2t}x,\ _{3t}x,\ \text{etc.},$$

denote a series of functions of t, each one of which is the derivative of the preceding, or the primitive of the succeeding term.

In the general equation ${}_{nt}U=z$, three variable quantities and the constant number n are combined; and, as I have already said, the most comprehensive form of the problem is "from any three of these to find the fourth." But, since n is necessarily constant, that case of the general problem in which n is the *quæsitum*, must differ essentially in its nature from the other three; nay more, unless the given relations among the three variables t, z, v, be such that z is one of the derivatives or primitives of v, regarded as a function of t, the problem can have no solution, it is indeed altogether unmeaning.

Thus, if ${}_{nt}\sin t=t^3$ were proposed, that is, if it were demanded, "how many times must $\sin t$ be derivated until the result t^3 be arrived at?" our only reply would be, that the question has no meaning, for t^3 is not any derivative of $\sin t$, the only derivatives of this function being $\cos t$, $-\sin t$, $-\cos t$, and $+\sin t$.

To those who uphold the continuity of algebraic expressions, and claim even for the square root of a negative quantity a real existence, this argument must appear quite inconclusive, because, between the members of the above written series, there may be intermediate terms; thus, there may be such a term as ${}_{\frac{1}{2}t}x$ or as ${}_{2\frac{1}{3}t}x$; there may be some yet undiscovered operation, two performances of which may produce the same effect as one derivation; and although such expressions may appear unmeaning to us, they may not be more so than the analogous symbols $a^{\frac{1}{2}}$, $a^{2\frac{1}{3}}$ appeared to the earlier algebraists. It is impossible to assert that some such operation may not yet be discovered, although, at present, we can form no conception of its nature. There may even be processes such as to give significance to the symbol ${}_{\frac{1}{10}t}x$; there may be any number of terms interpolated into the above series, and we cannot say that t^3 is not one of the endless numbers of terms between $\sin t$, $\cos t$, $-\sin t$, $-\cos t$.

In a paper which I gave in the *Annals of Philosophy*, for August 1829, it is shown that if u and v be two functions of some primary, which for the sake of conciseness we may suppress, the n^{th} derivative of their product uv, takes the form

$$ {}_{n}(uv) = {}_{n}u \,.\, v + \frac{n}{1}\, {}_{n-1}u \,.\, {}_{1}v + \frac{n}{1}\,\frac{n-1}{2}\, {}_{n-2}u \,.\, {}_{2}v + \text{ etc.} $$

which is quite analogous to the n^{th} power of the binome $u+v$; and it is also shown, that the n^{th} primitive (or integral) of the same product is

$$ {}_{-n}(uv) = {}_{-n}u \,.\, v - \frac{n}{1}\, {}_{-(n+1)}u \, {}_{1}v + \frac{n}{1}\,\frac{n+1}{2}\, {}_{-(n+1)}u \,.\, {}_{2}v - \text{ etc.} $$

which is also the counterpart of $(u+v)^{-n}$.

It is thus seen that these expressions are true whether a positive or a negative value be assigned to n, and the question may well be propounded, "Do these formulæ hold good when n has a fractional value?"

Without entering farther into this subject, it is enough for me to repeat the remark, that in the present state of our knowledge, we can form no idea of such fractional derivation; and that, therefore, the fourth branch of the general problem, viz., that in which the order of derivation is sought for, has for us scarcely any significance. There remain, then, only the three branches, the calculus of differentials or derivatives, that of integrals or primitives, and lastly the calculus of primaries.

The great majority of problems in theoretical mechanics belong to this third branch. The velocity of a moving point is generally dependent on its position; now, the velocity is the first derivative of the position regarded as a function of the time, and thus the problem really is, "from the known relation between the function and its derivative, to determine the primary, viz., the time." This problem is commonly resolved by a very simple process,—the first derivative of x, regarded as a function of t, is the reciprocal of the first derivative of t, regarded as a function of x; and so, by causing t to appear as the function, and x as the primary, we convert the problem into another belonging to the integral calculus. This convertibility of the problem has somewhat concealed its true nature.

The first example which we have on record of the use of the laws of change in scientific research, belongs to this third branch of the subject, and clearly exhibits its true nature. NEPAIR regarded his two flowing quantities, his *Artificialis* and *Naturalis*, as connected by this law that, while the velocity of change in the former is uniform, that in the other is variable, and proportional to the *Naturalis* itself. In modern language he made the *Artificialis* his primary variable, and prescribed the condition that the first derivative of the function should be proportional to that function (Constructio, sect. 26). In his first logarithms, also, he supposes, that at the outset the two velocities are alike. From these premises, he constructed his three auxiliary tables which are really what we now call *antilogarithmic.* After the computation of these tables of *radicales*—after the compilation of his *Constructio;* and, probably, even after the completion of the *Canon Mirificus,* he began to view the matter from the other side, and to think of his system as applicable to calculation in general; that is, he viewed the *Naturalis* as the argument, the *Artificialis,* to which he now gives the expressive name *Logarithm,* as the function. Inverting the relations of primary and function, he states in his appendix, article Habitudines, sect. 2, that the velocity of the increase of the logarithm is inversely proportional to the number (*sinus*). Thus, the genesis and computation of logarithms given by NEPAIR, is a perfect example of the transition from the equation $\frac{\delta x}{\delta t} = \phi x$, which belongs to the calculus of primaries, to the new equation $\frac{\delta t}{\delta x} = \frac{1}{\phi x}$, which belongs to the integral calculus.

The great problem in mechanics, "having given the law of attraction to compute the motion of a body," belongs to the second chapter of our calculus, for the attraction is the second derivative of the position regarded as a function of the time, and thus all mechanical problems of this class are typified by the general formula $\frac{\delta^2 x}{\delta t^2} = \phi x$, or ${}_{2t}x = \phi x$, in which the law connecting the function with its second derivative being given, that which connects it with the primary is sought. In this case also, the problem can be brought back to the calculus of integrals, the artifice being to multiply each member of the equation by the first derivative, and then to obtain by integration the square of the velocity in terms of the position.

The calculus of primaries has thus been unfortunate in that its first two chapters have been absorbed by the Integral Calculus. But this absorption ceases when we have to do with derivatives of a higher order, for then the resources of integration fail us. As yet, no artifice has been discovered whereby the equation $\frac{\delta^3 x}{\delta t^3} = \phi x$, or ${}_{3t}x = \phi x$, can be rendered integrable, and problems involving such equations must be resolved by methods special to themselves; or, as not unfrequently happens, must be let alone.

There are many problems connected with the geometry of motion and with mechanics, which resist all the powers of the calculus; thus, although we know the law connecting the curvature of an elastic plate with its angular tension, that is, although we be able to write down an equation of the shape in differentials, we are unable thence to arrive at that shape; in other words, we are unable to integrate. And thus our progress in mechanical science, even in matters of daily occurrence, is arrested, not by our ignorance of the mechanical principles involved, but by the imperfection of our attainments in arithmetic.

The integral calculus may almost be described as a collection of artifices by which a great variety of proposed differentials may be transformed into other differentials of which the integrals are known, not by integration proper, but by previous differentation. These artifices must necessarily bring us to known functions, or to combinations of known functions. Now the number of simple functions employed in the calculus is very limited; the usual enumeration gives us the *potence* or algebraic function, the *exponential*, the *logarithmic* function, the *sine*, and the *cosine*, in all five; and even of these five two must be removed, because the logarithmic and exponential functions are converse to each other, while the cosine is only a variety of the sine; and thus we have but three kinds of simple relationship from which we seek to compound all others.

When we contrast this paucity of material with the multifariousness of the work to be accomplished, we need not be surprised that so much remains to be done; it is rather matter of astonishment that so much has been performed. The mutual relations of physical phenomena are too various, too complex, to be represented by any combinations of such a small number of functions; and our hopes of passing beyond the present limits of applicate analysis must be founded on an extension of the groundwork, and on an enlargement of our plan of operation.

In the calculus of primaries, the first problem which presents itself, that in which the relation of the function to its derivative is of the very simplest kind, is to investigate the nature of those functions which are equal to their derivatives. The solution of this problem leads us to classes of functions of which the exponential and the circular are cases; that is to say, the whole of the actual calculus is only the exposition of part of the simplest proposition in the theory of primaries. It is, then, not unreasonable to hope that the farther cultivation of this theory may enable us to resolve problems which have hitherto resisted all our efforts.

In arranging the parts of this theory, we may place under one general head all those cases in which the relation between the function and one of its derivatives is given, and we may call problems belonging to this head *pure problems.* Thus, when we investigate the motion of a body which is drawn toward a fixed point by an attraction depending on the distance, the relation between the function and its second derivative is given; or, if we be inquiring into the form assumed by an elastic plate, of uniform breadth, when vibrating, the relation between the ordinate and its fourth derivative is prescribed; these investigations lead to pure problems.

When two or more derivatives are combined in the relationship, we may give the name *mixed problems* to those which result. For example, the motion of a body in a resisting medium is affected by its velocity, so that the differential equation must exhibit the second derivative of the position, in terms of that position, and of the first derivative.

And again, when the primary itself is involved, we may call the problem *adfected.* Thus, if we were to propose for investigation the motion of a body which is urged to a fixed point by an attraction proportional directly to the distance, and inversely to the time, the differential equation would take the form $-_{2t}\,x = \frac{x}{t}$, which may be called adfected. The resolution of this equation leads to a series of functions, of which the generic character is contained in the equation $x + n\,_{1t}x + t\,_{2t}x = 0$. One of these gives the form assumed by a flexible uniform chain suspended by one end and making minute oscillations.

This faint outline may serve to give an idea of the scope and classification of problems belonging to the third branch of the calculus. It makes room, as is obvious, for many physical problems which have already been resolved, and which have been regarded as belonging to the calculus of integrals. And its chapters would need to be multiplied, for the purpose of including those numerous cases in which two or more functions of the same primary are combined.

It may naturally be expected that, when broaching the subject of a new and higher branch of the calculus, I should be prepared with numerous exemplifications of its power and utility. However, I am truly in the position of a naturalist, who, having stumbled upon some unlooked-for combination of organs, some duck's bill upon the head of a quadruped, is compelled to invent a new genus, a family, even a tribe, to contain his solitary example, and who is nervously anxious to see the propriety of his extensive generalisations verified by the success of his fellow-labourers in the fields of science.

XXXVI.—*On Functions with Recurring Derivatives.* By EDWARD SANG, Esq.

(Read 5th March 1866.)

The subject of the following paper is the first proposition in the Calculus of Primaries. The business of that calculus is to discover the relation between the primary variable and its function, when the relation subsisting between the function and its derivative is known. The simplest relationship between two variable quantities is *proportionality* when they are heterogeneous, or equality, when they are of one kind; and the case of proportionality can always, by a change in the unit of measure, be brought to an equality of the representative numbers; so that our first proposition becomes this: "To investigate the nature of those functions which reappear among their own derivatives." Since this reappearance must necessarily be periodical, I shall use the name *Functions with Recurring Derivatives.*

My attention was first drawn to functions of this class by observing that the method of solving Algebraic Equations which I published in 1829, can readily be extended to equations into which a recurring function enters. In that publication an example is given of the solution of an equation of the form $ax + b \sin x + C = O$, and this example serves as a type for all equations composed of an algebraic and a recurring function.

1. If, after having differentiated a function several times, we come to the function itself as a differential coefficient, it is obvious that the continuation of the process must reproduce the same series of derivatives, and that the group or period must recur again and again. Hence these functions may be arranged in *orders,* according to the number of terms in the group; thus the circular functions *sine* and *cosine* belong to the fourth order.

2. Hence the sum of all the functions forming a complete period must be a recurring function of the first order.

3. When the values, corresponding to a given argument, of all the functions of a period are known, their values corresponding to another argument can be computed.

Let, for example, ϕt represent a function of the argument t, such that its third derivative is again ϕt; that is, such that ${}_{3t}\phi t = \phi t$; then must we have ${}_{4t}\phi t = {}_{1t}\phi t$, ${}_{5t}\phi t = {}_{2t}\phi t$, ${}_{6t}\phi t = {}_{3t}\phi t$, etc. On the supposition that t becomes $t+u$, the new function becomes, according to TAYLOR's theorem—

$$\varphi(t+u) = \varphi t + {}_1\varphi t\frac{u}{1} + {}_2\varphi t\frac{u^2}{1.2} + {}_3\varphi t\frac{u^3}{1.2.3} + {}_4\varphi t\frac{u^4}{1.2.3.4} + \text{etc.}$$

which becomes, in our present example—

$$\varphi(t+u) = \varphi t\left\{1 + \frac{u^3}{1.2.3} + \frac{u^6}{1...6} + \frac{u^9}{1....9} + \text{etc.}\right\}$$
$$+ {}_1\varphi t\left\{\frac{u}{1} + \frac{u^4}{1...4} + \frac{u^7}{1....7} + \frac{n^{10}}{1...10} + \text{etc.}\right\}$$
$$+ {}_2\varphi t\left\{\frac{u^2}{1.2} + \frac{u^5}{1...5} + \frac{u^8}{1....8} + \frac{u^{11}}{1...11} + \text{etc.}\right\}$$

4. If A, B, C be the values which ϕt, ${}_1\phi t$, ${}_2\phi t$ assume when $t = o$, the above expression becomes

$$\varphi u = \mathrm{A}\left\{1 + \frac{u^3}{1.2.3} + \text{etc.}\right\} + \mathrm{B}\left\{\frac{u}{1} + \frac{u^4}{1....4} + \text{etc.}\right\} + \mathrm{C}\left\{\frac{u^2}{1.2} + \frac{u^5}{1...5} + \text{etc.}\right\}$$

and thus it seems that all recurring functions of the third order are compounds of multiples of the three functions—

$$1 + \frac{t^3}{1.2.3} + \frac{t^6}{1....6} + \frac{t^9}{1......9} + \text{etc.},$$
$$\frac{t}{1} + \frac{t^4}{1....4} + \frac{t^7}{1....7} + \frac{t^{10}}{1....10} + \text{etc.}, \quad \text{and}$$
$$\frac{t^2}{1.2} + \frac{t^5}{1....5} + \frac{t^8}{1....8} + \frac{t^{11}}{1......11} + \text{etc.},$$

which may be called the fundamental functions of the third order.

Similarly, the fundamental functions of any other order, say the fourth, are—

$$1 + \frac{t^4}{1...4} + \frac{t^8}{1....8} + \frac{t^{12}}{1.....12} + \text{etc.},$$
$$\frac{t}{1} + \frac{t^5}{1....5} + \frac{t^9}{1....9} + \frac{t^{13}}{1.....13} + \text{etc.},$$
$$\frac{t^2}{1.2} + \frac{t^6}{1...6} + \frac{t^{10}}{1....10} + \frac{t^{14}}{1....14} + \text{etc.},$$
$$\frac{t^3}{1.2.3} + \frac{t^7}{1.....7} + \frac{t^{11}}{1....11} + \frac{t^{15}}{1......15} + \text{etc.}$$

5. The most important of all recurring functions is that which is equal to its own first derivative; if ϕt represent such a function, we must have

$$\varphi t = {}_1\varphi t = {}_2\varphi t = {}_3\varphi t = \text{etc., and}$$
$$\varphi(t+u) = \varphi t\left\{1 + \frac{u}{1} + \frac{u^2}{1.2} + \frac{u^3}{1.2.3} + \text{etc.}\right\}, \text{ whence}$$
$$\varphi u \quad = \mathrm{A}\left\{1 + \frac{u}{1} + \frac{u^2}{1.2} + \frac{u^3}{1.2.3} + \text{etc.}\right\};$$
$$\varphi t \quad = \mathrm{A}\left\{1 + \frac{t}{u} + \frac{t^2}{1.2} + \frac{t^3}{1.2.3} + \text{etc.}\right\};$$

and if A be unit, that is, if ϕt represent the fundamental recurring function of the first order, we have

$$\varphi(t+u) = \varphi t \,.\, \varphi u\ ;$$

that is to say, the function of the sum of two arguments is the product of the functions of those arguments separately. This is merely a statement, in modern notation, of the idea which guided NEPAIR to the invention of logarithms.

Extending the above equation, we find

$$\varphi(t+u+v)=\varphi t.\varphi u.\varphi v,$$

which, on supposing t, u, v to be all alike, gives in general

$$\varphi(nt)=(\varphi t)^n.$$

If, according to the usual practice, we put e for the value which this function assumes when the primary is unit, that is, if we put

$$e=1+\frac{1}{1}+\frac{1}{1.2}+\frac{1}{1.2.3}+\frac{1}{1.2.3.4}+\text{ etc.},$$

we easily obtain for every integer value of n

$$\varphi n=e^n=1+\frac{n}{1}+\frac{n^2}{1.2}+\frac{n^3}{1.2.3}+\text{ etc.}$$

Also, if we put $nt=1$, or $t=\dfrac{1}{n}$,

$$\varphi 1=e=\varphi\left(\frac{1}{n}\right)^n,\quad\text{whence }\varphi\left(\frac{1}{n}\right)=e^{\frac{1}{n}}$$

so that the above development of e^n is true also of fractional values of n, wherefore

$$\varphi t=e^t=1+\frac{t}{1}+\frac{t^2}{1.2}+\frac{t^3}{1.2.3}+\text{ etc.}$$

Thus the exponential function is reached in the first step of our researches into the theory of recurring functions.

The properties of this remarkable function are too well known to be in need of any elucidation here. I may, however, indicate a rapid approximation to the n^{th} root of e, obtained by help of BROUNCKER'S continued fractions. On expanding the series for $e^{\frac{1}{n}}$ into a chain-fraction, we obtain the successive quotients 1, $n-1$, 1, 1, $3n-1$, 1, 1, $5n-1$, 1, 1, $7n-1$, 1, etc.; and on computing from these the series of converging fractions, we find that the 1st, 4th, 7th, 10th, etc., of them form the following progression:—

$$\begin{array}{ccccc} & 2n & 6n & 10n & 14n \\ \dfrac{1}{-1} & \dfrac{1}{1} & \dfrac{2n+1}{2n-1} & \dfrac{12n^2+6n+1}{12n^2-6n+1} & \dfrac{120n^3+60n^2+12n+1}{120n^3-60n^2+12n-1}\ \text{etc.,}\end{array}$$

in which the successive multipliers $2n$, $6n$, $10n$, etc., form an arithmetical progression, of which the common difference is $4n$.

6. The converse of the exponential function is the logarithmic; thus, while e^t is called the exponential function of t, t itself is called the logarithm of e^t. If we

put x for e^t our original equation $\phi t = {}_{1t}\phi t$ takes the form $x = {}_{1t}x$, and, observing that ${}_{1x}t$ is the reciprocal of ${}_{1t}x$, we have

$${}_{1x}t = x^{-1} \ .$$

The continuation of the derivation gives us ${}_{2x}t = -1.x^{-2}$; ${}_{3x}t = +1.2.x^{-3}$; ${}_{4x}t = -1.2.3.x^{-4}$; etc. ; putting log x for t, and applying TAYLOR's theorem, the well-known development of a logarithm results, viz.—

$$\log (x + y) = \log x + \frac{y}{x} - \frac{1}{2}\frac{y^2}{x^2} + \frac{1}{3}\frac{y^3}{x^3} - \text{ etc.}$$

7. Having said enough concerning the recurring function of the first order to show its place in the Theory of Primaries, I proceed to consider those functions which reappear as their own second derivatives.

According to what has already been explained, the fundamental functions of this order are—

$$\left.\begin{array}{l} 1 + \dfrac{t2}{1.2} + \dfrac{t4}{1...4} + \dfrac{t6}{1....6} + \text{ etc.} \\ \dfrac{t}{1} + \dfrac{t3}{1.2.3} + \dfrac{t5}{1....5} + \dfrac{t7}{1....7} + \text{ etc.} \end{array}\right\}$$

These functions possess very remarkable properties; and in order to exhibit these clearly, it is convenient to give distinguishing names to the two functions.

If the values of t be represented by the abscissæ, and the corresponding values of the first of the above functions by the ordinates of a series of points, those points indicate a curve which we can show to be the catenary; the length of the curve reckoned from the point corresponding to $t = o$ being the second of the two functions. On this account, and in allusion to the construction of a chain bridge, I shall designate the former function the suspensor, the latter function the catena; that is to say, I shall put

$$\text{sus } t = 1 + \frac{t^2}{1.2} + \frac{t^4}{1...4} + \frac{t^6}{1....6} + \text{ etc.}$$

$$\text{cat } t = \frac{t}{1} + \frac{t^3}{1.2.3} + \frac{t^5}{1....5} + \frac{t^7}{1.....7} + \text{ etc.}$$

each being the derivative of the other.

8. The differential coefficient of the square of one of these functions is just equal to that of the square of the other function,

$$\text{or } {}_{1t}(\text{sus } t)^2 = 2 \text{ sus } t \,.\, \text{cat } t = {}_{1t}(\text{cat } t)^2$$

and, consequently, the difference between these squares must be constant; now when $t = o$ that difference is unit, wherefore for all values of t

$$\text{sus } t^2 - \text{cat } t^2 = 1 \ ;$$

wherefore if sus t be the absciss and cat t the ordinate of a point, that point lies in an equilateral hyperbola, so that these functions may be called *hyperbolic functions.*

9. By following the course of reasoning given in section 3, we obtain

$$\begin{aligned}
\text{sus}\,(t+u) &= \text{sus}\,t\,.\,\text{sus}\,u + \text{cat}\,t\,.\,\text{cat}\,u && (1)\\
\text{sus}\,(t-u) &= \text{sus}\,t\,.\,\text{sus}\,u - \text{cat}\,t\,.\,\text{cat}\,u && (2)\\
\text{cat}\,(t+u) &= \text{cat}\,t\,.\,\text{sus}\,u + \text{sus}\,t\,.\,\text{cat}\,u && (3)\\
\text{cat}\,(t-u) &= \text{cat}\,t\,.\,\text{sus}\,u - \text{sus}\,t\,.\,\text{cat}\,u && (4)
\end{aligned}$$

which are the counterparts of the four elementary formulæ of trigonometry.

10. By manipulating the four equations of the preceding section, we can obtain theorems exactly analogous to those of the angular calculus; thus, by additions and subtractions,

$$\begin{aligned}
\text{sus}\,(t+u) + \text{sus}\,(t-u) &= 2\,.\,\text{sus}\,t\,.\,\text{sus}\,u && (5)\\
\text{sus}\,(t+u) - \text{sus}\,(t-u) &= 2\,.\,\text{cat}\,t\,.\,\text{cat}\,u && (6)\\
\text{cat}\,(t+u) + \text{cat}\,(t-u) &= 2\,.\,\text{cat}\,t\,.\,\text{sus}\,u && (7)\\
\text{cat}\,(t+u) - \text{cat}\,(t-u) &= 2\,.\,\text{sus}\,t\,.\,\text{cat}\,u && (8)
\end{aligned}$$

Hence putting $t = nu$

$$\begin{aligned}
&\text{sus}\,(n+1)u + \text{sus}\,(n-1)u = 2\,\text{sus}\,nu\,.\,\text{sus}\,u && (9)\\
&\text{cat}\,(n+1)u + \text{cat}\,(n-1)u = 2\,\text{cat}\,.\,nu\,.\,\text{sus}\,u && (10)\\
&\text{sus}\,(n-1)u - 2\,\text{sus}\,nu + \text{sus}\,(n+1)u = \text{sus}\,nu\,.\,2(\text{sus}\,u-1)\\
&\text{cat}\,(n-1)u - 2\,\text{cat}\,nu + \text{cat}\,(n+1)u = \text{cat}\,nu\,.\,2(\text{sus}\,u-1)
\end{aligned}$$

and thus a table of the values of catenarian functions may be constructed by help of second differences, just as in the case of the trigonometrical canon; the expression $2(\text{sus}\,u - 1)$ taking the place of $2(1 - \cos u)$. The same analogy may be extended to differences of the fourth and sixth orders, and so on.

11. By putting successively $t = u$, $t = 2u$, $t = 3u$, we can form the expressions for the catenarian functions of multiple arguments; thus

$$\begin{aligned}
\text{sus}\,2u &= \text{sus}\,u^2 + \text{cat}\,u^2\\
&= 2\,.\,\text{sus}\,u^2 - 1 \qquad = 2\,.\,\text{cat}\,u^2 + 1\\
\text{cat}\,2u &= 2\,.\,\text{sus}\,u\,.\,\text{cat}\,u\\
\text{sus}\,3u &= 4\,\text{sus}\,u^3 - 3\,\text{sus}\,u\\
\text{cat}\,3u &= 4\,\text{cat}\,u^3 + 3\,\text{cat}\,u\\
\text{sus}\,4u &= 8\,\text{sus}\,u^4 - 8\,\text{sus}\,u^2 + 1\\
\text{cat}\,4u &= 8\,\text{cat}\,u^4 + 8\,\text{cat}\,u^2 + 1\\
\text{sus}\,5u &= 16\,\text{sus}\,u^5 - 20\,\text{sus}\,u^3 + 5\,\text{sus}\,u\\
\text{cat}\,5u &= 16\,\text{cat}\,u^5 + 20\,\text{cat}\,u^3 + 5\,\text{cat}\,u
\end{aligned}$$

and so on.

12. Since the sum of the functions forming a period is a recurring function of the first order, we have

$$\begin{aligned}
&& \text{sus}\,t + \text{cat}\,t &= e^t\\
&\text{but} & \text{sus}\,t^2 - \text{cat}\,t^2 &= 1\\
&\text{wherefore} & \text{sus}\,t - \text{cat}\,t &= e^{-t}
\end{aligned}$$

$$\text{and}\qquad \text{sus}\,t = \frac{1}{2}\left(e^t + e^{-t}\right);\quad \text{cat}\,t = \frac{1}{2}\left(e^t - e^{-t}\right),\quad \text{as}$$

is indeed evident from the ordinary operation for separating the terms containing the odd from those containing the even powers of the argument. Hence, in general,

$$\text{sus } nt = \frac{1}{2}\left(e^{nt} + e^{-nt}\right) ; \quad \text{cat } nt = \frac{1}{2}\left(e^{nt} = e^{-nt}\right) .$$

13. If we raise the expression for sus t to the n^{th} power, and reply the one-half of the development upon the other half, we find

$$2^n . \text{ sus } t^n = e^{nt} + e^{-nt} + \frac{n}{1}\left(e^{(n-2)t} + e^{(2-n)t}\right) + \frac{n}{1}\frac{n-1}{2}\left(e^{(n-4)t} + e^{(4-n)t}\right) + \text{etc.}$$

wherefore

$$2^{n-1} \text{ sus } t^n = \text{sus } nt + \frac{n}{1} \text{ sus}\left(n-2\right)t + \frac{n}{1}\frac{n-1}{2} \text{ sus}\left(n-4\right)t + \text{ etc.}$$

in which expression we have to distinguish the two cases of n even and n odd.

This counterpart of a celebrated trigonometrical equation is obtained by the very same process, only in this case the operations are all real; whereas the supposition $2 \cos t = x + x^{-1}$ used in the trigonometrical investigation is notoriously contrary to fact and to possibility.

By treating the function cat t in the same way, we can obtain, according as n is odd or even, the formulæ

n odd

$$2^{n-1} \text{ cat } t^n = \text{cat } nt - \frac{n}{1} \text{ cat}\left(n-2\right)t + \frac{n}{1}\frac{n-1}{2} \text{ cat}\left(n-4\right)t - \text{ etc.}$$

n even

$$2^{n-1} \text{ cat } t^n = \text{sus } nt - \frac{n}{1} \text{ sus}\left(n-2\right)t + \frac{n}{1}\frac{n-1}{2} \text{sus}\left(n-4\right)t - \text{ etc.}$$

the last term being halved when n is even.

My intention at present is not so much to give a detailed treatise on the properties of these functions as to show their close relationship to the sine and cosine of the angular calculus. A striking example of the manner in which the catenarian supplement the office of the circular functions is seen in their application to equations of the third degree.

14. All cubic equations may be brought to the form $z^3 \pm a z + b = o$, and, by making $z = x \sqrt{\frac{4a}{3}}$, these again can be changed into

$$4x^3 \pm 3x + \sqrt{\left(\frac{27b^2}{4a^3}\right)} = o$$

On comparing this last with the four equations

$$\begin{aligned} 4x^3 - 3x - \cos 3t &= o \\ 4x^3 - 3x + \sin 3t &= o \\ 4x^3 - 3x - \text{sus } 3t &= o \\ 4x^3 + 3x - \text{cat } 3t &= o \end{aligned}$$

we observe that if $\sqrt{\left(\frac{27b^2}{4a^3}\right)}$ be equal to any one of the functions of $3t$, the value of

x must be the corresponding function of t. Now, whenever $27b^2$ is greater than $4a^3$, the first two forms fail us; in such event, we must have recourse to the third equation, and when a is positive, we must use the fourth. Thus, by help of tables of angular and catenarian functions, we can resolve all equations of the third order.

15. As another instance of this close affinity, I may cite, using the notation of LEIBNITZ, the integral $\int \frac{d\varphi}{a + b\cos\varphi}$.

When a is greater than b, we make the assumption

$$\frac{b + a\cos\varphi}{a + b\cos\varphi} = \cos\psi, \text{ which gives}$$

$$\frac{\sqrt{(a^2 - b^2)}\, d\varphi}{a + b\cos\varphi} = d\psi, \text{ so that}$$

$$\int \frac{d\varphi}{a + b\cos\varphi} = \frac{1}{\sqrt{(a^2 - b^2)}} \cos^{-1} \frac{b + a\cos\varphi}{a + b\cos\varphi}$$

but when a is less than b this method of reduction fails us; in that case we may assume

$$\frac{b + a\cos\varphi}{a + b\cos\varphi} = \operatorname{sus}\psi \quad \text{when}$$

$$\frac{\sqrt{(b^2 - a^2)}\, d\varphi}{a + b\cos\varphi} = d\psi \quad \text{and therefore}$$

$$\int \frac{d\varphi}{a + b\cos\varphi} = \frac{1}{\sqrt{(b^2 - a^2)}} \operatorname{sus}^{-1} \frac{b + a\cos\varphi}{a + b\cos\varphi}.$$

Conversely for the analogous integrals, we find

$$\int \frac{d\varphi}{a + b\operatorname{sus}\varphi} = \frac{1}{\sqrt{(a^2 - b^2)}} \operatorname{sus}^{-1} \frac{b + a\operatorname{sus}\varphi}{a + b\operatorname{sus}\varphi}.$$

$$\int \frac{d\varphi}{a \times b\operatorname{sus}\varphi} = \frac{1}{\sqrt{(b^2 - a^2)}} \cos^{-1} \frac{b + a\operatorname{sus}\varphi}{a + b\operatorname{sus}\varphi}.$$

to which may be added

$$\int \frac{d\varphi}{a + b\operatorname{cat}\varphi} = \frac{-1}{\sqrt{(a^2 + b^2}} \operatorname{cat}^{-1} \frac{b - a\operatorname{cat}\varphi}{a + b\operatorname{cat}\varphi}.$$

16. From these examples, it is apparent that the properties of recurring functions of the second order offer a fair field for the exertions of the analyst, inasmuch as they promise to give unity to investigations which have hitherto exhibited abrupt changes.

The inquiry into the form of an equilibrated bridge, affords a good instance of their utility in physical researches. Since the whole space between the roadway and the arch stones is, in this case, filled up, the weight must be proportional to the surface of the projection on the side of the bridge. Now, in all arches, this weight is proportional to the tangent of the inclination of the arch line; this tangent is the derivative of the vertical ordinate regarded as a function of the horizontal absciss, while this ordinate itself is the derivative of the area, wherefore the area of the side elevation is proportional to its second derivative, and is therefore expressed by a recurring function of the second order; and so also must be its derivative the vertical ordinate; hence the outline of the arch must be of the nature of a catenary.

17. The compounds of these catenarian functions are not less interesting than the functions themselves. Thus, we may form compounds analogous to the secant, cosecant, tangent, and cotangent of the angular calculus, and, for want of other symbols, we may designate these by underlines; thus we may put

$$\underline{\sec}\, t = \frac{1}{\operatorname{sus} t}; \quad \underline{\operatorname{cse}}\, t = \frac{1}{\operatorname{cat} t}; \qquad \underline{\tan}\, t = \frac{\operatorname{cat} t}{\operatorname{sus} t}; \qquad \underline{\cot}\, t\, \frac{\operatorname{sus} t}{\operatorname{cat} t}.$$

whence we readily find

$$\underline{\tan}\, t^2 + \underline{\sec}\, t^2 = 1; \qquad \underline{\operatorname{cat}}\, t^2 - \underline{\operatorname{cse}}\, t^2 = 1,$$

and by differentiation

$${}_{1t}\underline{\sec}\, t = -\,\underline{\sec}\, t\,.\,\underline{\tan}\, t; \qquad {}_{1t}\underline{\operatorname{cse}}\, t = -\,\underline{\operatorname{cse}}\, t\,.\,\underline{\cot}\, t\,;$$

$${}_{1t}\underline{\tan}\, t = +\,\underline{\sec}\, t^2; \qquad {}_{1t}\underline{\cot}\, t = -\,\underline{\operatorname{cse}}\, t^2\,;$$

$${}_{1}\log \operatorname{sus} t = \underline{\tan}\, t; \qquad {}_{1t}\log \operatorname{cat} t = \underline{\cot}\,{}^2 t\,;$$

$${}_{1t}\log \underline{\tan}\, t = \underline{\tan}\, t + \underline{\cot}\, t = \underline{2 \operatorname{cse}}\, 2t\,.$$

18. By taking the inverse of these functions we obtain another set of differentials; thus, on putting $\operatorname{sus} t = x$, we have $\operatorname{cat} t = \sqrt{(x^2 - 1)}$ and $t = \overset{-1}{\operatorname{sus}}\, x$, so that the equation ${}_{1t}\operatorname{sus} t = \operatorname{cat} t$ becomes

$${}_{1x}\overset{-1}{\operatorname{sus}}\, x = (x^2 - 1)^{-\frac{1}{2}}; \text{ similarly}$$

$${}_{1x}\overset{-1}{\operatorname{cat}}\, x = (x^2 + 1)^{-\frac{1}{2}};$$

$${}_{1x}\overset{-1}{\underline{\sec}}\, x = \frac{-1}{x\sqrt{(1 - x^2)}};$$

$${}_{1x}\overset{-1}{\underline{\operatorname{cse}}}\, x = \frac{-1}{x\sqrt{(1 + x^2)}};$$

$${}_{1x}\overset{-1}{\underline{\tan}}\, x = (1 - x^2)^{-1};$$

$${}_{1x}\overset{-1}{\underline{\cot}}\, x = (1 - x^2)^{-1}.$$

These inverse functions are only new forms for well-known logarithmic expressions, thus

$$\overset{-1}{\text{sus}}\ x = \log\{x + \sqrt{(x^2 - 1)}\}; \qquad \overset{-1}{\text{cat}}\ x = \log\{x + \sqrt{(x^2 + 1)}\}$$

$$\overset{-1}{\underline{\text{sec}}}\ x = \log\frac{1 + \sqrt{(1 - x^2)}}{x}; \qquad \overset{-1}{\underline{\text{cse}}}\ x = \log\frac{1 + \sqrt{(1 + x^2}}{x};$$

$$\overset{-1}{\underline{\text{tan}}}\ x = \tfrac{1}{2}\log\frac{1 + x}{1 - x}; \qquad \overset{-1}{\text{cot}}\ x = \tfrac{1}{2}\log\frac{x + 1}{x - 1};$$

but they serve to give unity of structure to those alternate integrals which are possible or impossible in circular functions, according as x is greater or less than unit.

19. The analogy of these compound catenarian or hyperbolic functions to the trigonometrical lines, is clearly shown by their geometrical representatives—thus, having measured OA, OB each equal to unit from the two sides of a right angle, let fig. 1, OC be laid off equal to some value of sus t, and draw the ordinate

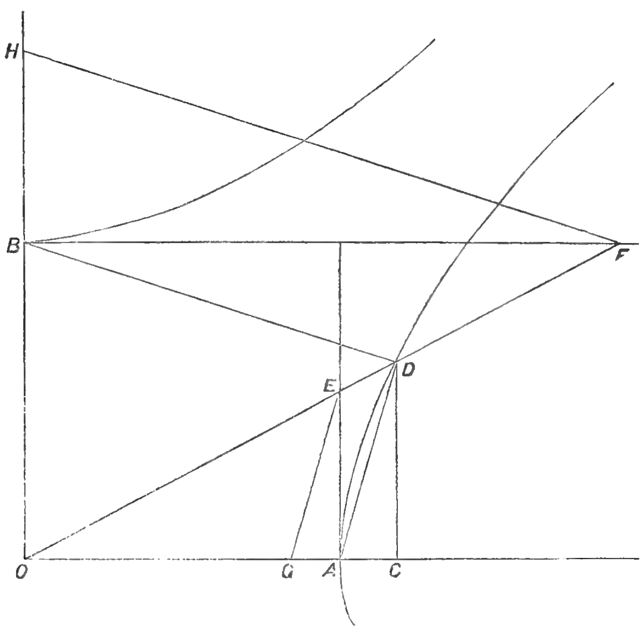


Fig. 1.

CD equal to the corresponding value of cat t, then D is a point in the equilatera hyperbola of which OA, OB are the two semi-axes.

If we draw the radius-vector OD, and suppose D to be carried to a small distance along the curve, the surface of the sector AOD will be augmented by a quantity which, in all curves, is represented by $\frac{1}{2}\{\text{OC}\,.\,\delta\text{CD} - \text{CD}\,.\,\delta\text{OC}\}$, wherefore, in the present instance, the increment of the sector AOD is $\frac{1}{2}\{\text{sus}\ t\,.\,\text{sus}\ t - \text{cat}\ t\,.\,\text{cat}\ t\}\,dt$, which, since $\text{sus}\ t^2 - \text{cat}\ t^2 = 1$, becomes $\frac{1}{2}\,dt$, therefore twice the sector AOD represents the primary variable t. Through A and B draw two perpendiculars meeting the radius-vector and its continuation in E and F, then AE $= \overline{\tan}\ t$, BF $= \overline{\cot}\ t$: also draw EG parallel to DA, FH parallel to DB, then OG $= \overline{\sec}\ t$, OH $= \overline{\text{cse}}\ t$.

We shall afterwards reach the functions sine and cosine when treating of recurring derivatives of the fourth order, and shall have the analogous construction shown in fig. 12. There OC is made equal to the function cos t, while CD is made equal to the sin t; but it will be shown of those functions that $\cos t^2 + \sin t^2 = 1$, wherefore D must be a point in the circumference of a circle described from O; AE is then the tangent, BF the cotangent; and, to keep up the analogy, if we draw EG parallel to DA, FH parallel to DB, OG becomes the secant, OH the cosecant of t, t being represented by the double of the sector OAD.

20. If, as in fig. 2, the absciss OT be made proportional to the primary t, and the ordinate TB to the function sus t, OA being the linear unit, the locus of the

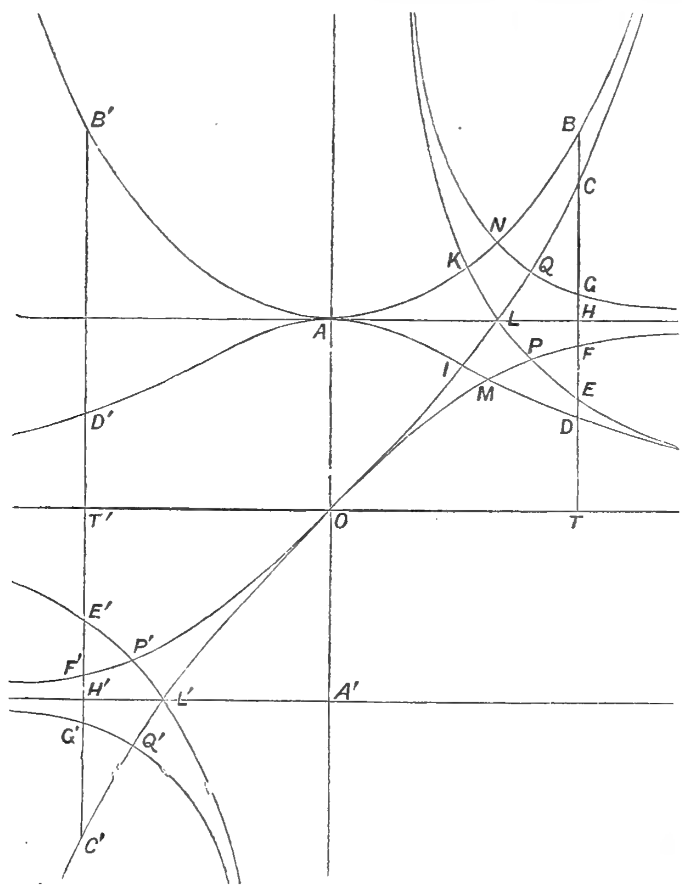


Fig. 2.

point B may be shown to be the catenary thus. When OT increases by a minute quantity dt, the ordinate TB increases by cat $t \,.\, dt$, and therefore the increment of the arc AB must be $\sqrt{(dt^2 + \text{cat } t^2 \,.\, dt^2)}$; now $1 + \text{cat } t^2 = \text{sus } t^2$, wherefore the increment of the arc is sus $t \,.\, dt$, which is just the increment of cat t, so that the length of the arc AB must be cat t. Now the tangent of the inclination at B is also cat t, and it is the property of all arches that the tangent of the inclination is proportional to the weight reckoned from the horizontal part of the curve, and consequently the curve of which the ordinate is sus t is that which a flexible chain assumes.

Since the ordinate TB is the derivative of the area OABT regarded as a function of OT, that area must also be proportional to the function cat t; that is to say, the surface AOTB is proportional to the length of the curve AB.

21. If along the line TB we make TC proportional to the conjugate function cat t, the point C is in a curve C′OC, which we may call the companion to the catenary; this curve crosses the axis at the origin O, so that at the distance $-t$ the ordinate T′C′ appears on the opposite side; and the area OTC is proportional to HB, the excess of TB above the linear unit OA. The positive branches of these two curves approach more and more closely as t is taken of greater value.

22. By making TD a third proportional to TB and TH, we obtain the representative of the function which, for want of a better notation, we have indicated by the symbol $\underline{\text{sec}}\ t$. The curve traced by the point D rises to touch the catenary at the point A, and approaches on the positive and negative sides to the line of abscissæ. When the arc AB is unfolded, the extremity of the tangent describes, as is well known, the line called the Tractory; now if, on the surface of an oblique circular arch, a line be drawn crossing all the lines of pressure at right angles, this line indicates the proper course for the joints of the voussoirs; it is a line of double curvature, and I have shown, in a paper on the *construction of oblique arches*, read before the Society of Arts for Scotland in 1835 (*Edin. New Phil. Jour.* for April 1840), that the projection of this line upon the plane of the parapet is the *tractory*, while the projection of the same line upon a plane crossing the roadway at right angles is a modification of the curve D′AD; on that account I have called it the *companion to the tractory*.

23. If we now make TE a third proportional to TC and TH, TE becomes the representative of the function $\underline{\text{cse}}\ t$. The curve traced by E has the continuation of OA for one asymptote, and the line of abscissæ for another. For negative values of t it appears on the opposite sides of those lines, the two branches of this curve being disconnected. It crosses the companion to the catenary at t where that line crosses the parallel to OT through A.

24. Making TB : TC :: TH : TF, and TC : TB :: TH : TG we obtain TF the representative of $\underline{\text{tan}}\ t$, and TG that of $\underline{\text{cot}}\ t$. The line of *tangents* osculates and crosses the companion to the catenary at O, and has the lines AH and A′H for asymptotes; and it is remarkable that this line, to which in the abovementioned paper I have given the name *double-logarithmic*, is the projection of the same line of double curvature upon the horizontal plane. The curve of *cotangents*, traced by the points G and G′, consists of two detached branches, the positive branch

having the continuation of OA, and the parallel AH for asymptotes; the other of like dimensions approaching to the lines OA′ and A′H′.

25. I may conclude this short notice of the properties of recurring derivatives of the second order by considering the case of proportionality. Let it be proposed to investigate the nature of the function ϕt when it is proportional to its second derivative, that is when ${}_{2t}\phi t = c\phi t$, c being a constant multiplier.

By taking the successive derivatives of this we obtain—

$$
{}_3\phi t = c \,.\, {}_1\phi t \;;\quad {}_4\phi t = c^2 \,.\, \phi t \;;
$$
$$
{}_5\phi t = c^2 \,.\, {}_2\phi t \;;\quad {}_6\phi t = c^3 \,.\, \phi t \;;\quad \text{etc.},
$$

whence, according to TAYLOR's theorem,

$$
\begin{aligned}
\phi\,(t+u) = \quad & \phi t \left\{ 1 + \frac{cu^2}{1\,.\,2} + \frac{c^2u^4}{1\,.\,2\,.\,3\,.\,4} + \text{etc.} \right\} \\
& + c^{-\frac{1}{2}}\,{}_1\phi t \left\{ \frac{c^{\frac{1}{2}}\,u}{1} + \frac{c^{\frac{1}{2}}\,u^3}{1\,.\,2\,.\,3} + \text{etc.} \right\}
\end{aligned}
$$

If now we suppose that the value of ϕo is A, while that of $c^{\frac{1}{2}}{}_1\phi o$ is B, A and B being two constants, the above expression becomes

$$
\phi u = \mathrm{A}\,.\,\mathrm{sus}\,(u\sqrt{c}) + \mathrm{B}\,.\,\mathrm{cat}\,(u\sqrt{c}) \;,
$$

which formula includes all functions which are proportional to their second derivatives.

It is here to be remarked that c cannot have a negative value; the equation,

$$
{}_2\phi t = -\,c\,.\,\phi t \;,
$$

belongs properly to recurring functions of the fourth order, under which head it will afterwards be considered.

THIRD ORDER.

The functions of the second order of recurrence are only compounds of the exponential function, and might almost have been passed over, if my sole object had been to exhibit what is novel. The functions of the third order, however, cannot be produced by compounding those of the previous orders, and we have now to touch upon ground entirely new.

26. According to what has already been explained, the fundamental recurring functions of the third order are—

$$
\begin{aligned}
& 1 + \frac{t^3}{1\,.\,2\,.\,3} + \frac{t^6}{\ldots.6} + \frac{t^9}{\ldots.9} + \text{etc.} \\
& \frac{t}{1} + \frac{t^4}{\ldots 4} + \frac{t^7}{\ldots 7} + \frac{t^{10}}{\ldots 10} + \text{etc.} \\
& \frac{t^2}{1\,.\,2} + \frac{t^5}{\ldots.5} + \frac{t^8}{\ldots.8} + \frac{t^{11}}{\ldots..11} + \text{etc.}
\end{aligned}
$$

For the sake of conciseness in language, it would be advantageous to have distinct names for these. The task of finding appropriate appellations is, however, more difficult than that of finding a designation for a new asteroid or a new metal; without attempting it, I shall content myself with a simple piece of notation.

Placing the trigon $\triangle$ as the general symbol for recurring functions of the third order, we may indicate the separate cases by writing the index of the first term within it; in this way we have

$$\triangle_{0} t = 1 + \frac{t^3}{1.2.3} + \frac{t^6}{\dots 6} + \text{etc.},$$

$$\triangle_{1} t = \frac{t}{1} + \frac{t^4}{\dots 4} + \frac{t^7}{\dots .7} + \text{etc.},$$

$$\triangle_{2} t = \frac{t^2}{1.2} + \frac{t^5}{\dots .5} + \frac{t^8}{\dots .8} + \text{etc.},$$

and we shall afterwards use a similar notation for functions of the fourth order, these being represented by a tetragon having the appropriate numbers inscribed, thus— $\boxed{0}\,t$, $\boxed{1}\,t$, $\boxed{2}\,t$, $\boxed{3}\,t$.

27. Every recurring function of the third order may be represented by the formula

$$\mathrm{A}\triangle_{0} t + \mathrm{B}\triangle_{1} t + \mathrm{C}\triangle_{2} t$$

in which A, B, C are constant multipliers, which may be positive, zero, or negative. In general, if ϕt represent such a compound function, ${}_1\phi t$ and ${}_2\phi t$ being its derivatives, we have

$$\phi(t+u) = \phi t \,.\, \triangle_{0} u + {}_1\phi t \,.\, \triangle_{1} u + {}_2\phi t \,.\, \triangle_{2} u ,$$

and, in the case of the fundamental functions themselves,

$$\triangle_{0}(t+u) = \triangle_{0} t \,.\, \triangle_{0} u + \triangle_{2} t \,.\, \triangle_{1} u + \triangle_{1} t \,.\, \triangle_{2} u , \qquad (1)$$

$$\triangle_{1}(t+u) = \triangle_{1} t \,.\, \triangle_{0} u + \triangle_{0} t \,.\, \triangle_{1} u + \triangle_{2} t \,.\, \triangle_{2} u , \qquad (2)$$

$$\triangle_{2}(t+u) = \triangle_{2} t \,.\, \triangle_{0} u + \triangle_{1} t \,.\, \triangle_{1} u + \triangle_{0} t \,.\, \triangle_{2} u ; \qquad (3)$$

these three equations give by addition

$$\triangle_{0}(t+u) + \triangle_{1}(t+u) + \triangle_{2}(t+u) = (\triangle_{0} t + \triangle_{1} t + \triangle_{2} t)(\triangle_{0} u + \triangle_{1} u + \triangle_{2} u) , \text{ or}$$

$$e^{t+u} = e^t \,.\, e^u ;$$

they are analogous to the values of sus $(t+u)$ and cat $(t+u)$ given in article 9; but they cannot, like those, be converted into functions of the difference $t-u$ by a change of sign.

28. The sum of the cubes of three recurring functions exceeds three times the continued product of those functions by a constant quantity.

The first derivative of the sum of the cubes

$$\triangle_{0} t^3 + \triangle_{1} t^3 + \triangle_{2} t^3 \qquad \text{is}$$

$$3\triangle_{0} t^2 \,.\, \triangle_{2} t + 3\triangle_{0} t \,.\, \triangle_{1} t^2 + 3\triangle_{1} t \,.\, \triangle_{2} t^2$$

which is also the first derivative of

$$3\triangle_0 t\,.\,\triangle_1 t\,.\,\triangle_2 t$$

wherefore the difference

$$\triangle_0 t^3 + \triangle_1 t^3 + \triangle_2 t^3 - 3\triangle_0 t\,.\,\triangle_1 t\,.\,\triangle_2 t$$

must be constant. This proof applies to all recurring functions of the third order; in the case of the fundamental functions we have for the value $t = o$,

$$\triangle_0 o^3 + \triangle_1 o^3 + \triangle_2 o^3 - 3\triangle_0 o\,.\,\triangle_1 o\,.\,\triangle_2 o = 1\ ,$$

and, therefore, the sum of the cubes exceeds three times the continued product of those functions by unit.

29. If we substitute $-t$ for u in the equations of article 27, we have

$$\begin{aligned}
\triangle_0 o = 1 &= \triangle_0 t\,.\,\triangle_0(-t) + \triangle_2 t\,.\,\triangle_1(-t) + \triangle_1 t\,.\,\triangle_2(-t)\\
\triangle_1 o = 0 &= \triangle_1 t\,.\,\triangle_0(-t) + \triangle_0 t\,.\,\triangle_1(-t) + \triangle_2 t\,.\,\triangle_2(-t)\\
\triangle_2 o = 0 &= \triangle_2 t\,.\,\triangle_0(-t) + \triangle_1 t\,.\,\triangle_1(-t) + \triangle_0 t\,.\,\triangle_2(-t)
\end{aligned}$$

from which we can obtain the functions of $-t$ in terms of those of $+t$.

On eliminating $\triangle_1(-t)$, $\triangle_2(-t)$ from these equations, there results this

$$\triangle_0 t^2 - \triangle_1 t\,.\,\triangle_2 t = \{\triangle_0 t^3 + \triangle_1 t^3 + \triangle_2 t^3 - 3\triangle_0 t\,.\,\triangle_1 t\,.\,\triangle_2 t\}\,.\,\triangle_0(-t)$$

wherefore, according to article 28,

$$\triangle_0(-t) = \triangle_0 t^2 - \triangle_1 t\,.\,\triangle_2 t\ ;\quad \text{and similarly,} \qquad (4)$$

$$\triangle_1(-t) = \triangle_2 t^2 - \triangle_0 t\,.\,\triangle_1 t\ , \qquad (5)$$

$$\triangle_2(-t) = \triangle_1 t^2 - \triangle_0 t\,.\,\triangle_2 t\ . \qquad (6)$$

The sum of these three functions must be e^{-t}, now on dividing *unit* under the form

$$\triangle_0 t^3 + \triangle_1 t^3 + \triangle_2 t^3 - 3\triangle_0 t\,.\,\triangle_1 t\,.\,\triangle_2 t = 1$$

by e^t under the form $\triangle_0 t + \triangle_1 t + \triangle_2 t$ we obtain the very sum in question, viz.,

$$\triangle_0 t^2 + \triangle_1 t^2 + \triangle_2 t^2 - \triangle_0 t\,.\,\triangle_1 t - \triangle_0 t\,.\,\triangle_2 t - \triangle_1 t\,.\,\triangle_2 t = e^{-t}\ . \qquad (7)$$

30. If, for the sake of conciseness, we put Qt (quadratics) for the sum of the squares, and Pt for the sum of the products of these functions, that is, if

$$\begin{aligned}
Qt &= \triangle_0 t^2 + \triangle_1 t^2 + \triangle_2 t^2\\
Pt &= \triangle_0 t\,.\,\triangle_1 t + \triangle_0 t\,.\,\triangle_2 t + \triangle_1 t\,.\,\triangle_2 t
\end{aligned}$$

we obtain, on taking the successive derivatives,

$$\begin{aligned}
{}_1Pt &= Pt + Qt & \qquad {}_1Qt &= 2Pt\\
{}_2Pt &= 2Pt + {}_1Pt & \qquad {}_2Qt &= 2Qt + {}_1Qt
\end{aligned}$$

and hence each of these new functions is such that its second derivative exceeds the double of the function by the first derivative. Continuing the derivations we find

$$\begin{array}{ll} {}_3\mathrm{P}t = 2\mathrm{P}t + 3\,{}_1\mathrm{P}t & {}_3\mathrm{Q}t = 2\mathrm{Q}t + 3\,{}_1\mathrm{Q}t \\ {}_4\mathrm{P}t = 6\mathrm{P}t + 5\,{}_1\mathrm{P}t & {}_4\mathrm{Q}t = 6\mathrm{Q}t + 5\,{}_1\mathrm{Q}t \\ {}_5\mathrm{P}t = 10\mathrm{P}t + 11\,{}_1\mathrm{P}t & {}_5\mathrm{Q}t = 10\mathrm{Q}t + 11\,{}_1\mathrm{Q}t \\ \text{etc.} & \text{etc.} \end{array}$$

The progression of the numerical coefficients in each of these series is obvious; one term augmented by the double of the preceding gives the succeeding term of the series, so that the general formula is

$${}_n\mathrm{P}t = 2\,\frac{2^{n-1}-(-1)^{n-1}}{2\quad -(-1)}\,\mathrm{P}t + \frac{2^n-(-1)^n}{2\quad -(-1)}\,{}_1\mathrm{P}t$$

and similarly for the function $\mathrm{Q}t$. Hence, by TAYLOR's theorem,

$$\mathrm{P}(t+u) = 2\mathrm{P}t\left\{\frac{1}{2} + 0\frac{u}{1} + 1\frac{u^2}{1.2} + 1\frac{u^3}{1.2.3} + 3\frac{u^4}{\dots.4} + \text{etc.}\right\}$$

$$+ {}_1\mathrm{P}t\left\{1\frac{u}{1} + 1\frac{u^2}{1.2} + 3\frac{u^3}{1.2.3} + 5\frac{u^4}{\dots 4} + 11\frac{u^5}{\dots.5} + \text{etc.}\right\}$$

These equations are true for the P and Q of any set of recurring functions of the third order. For those with which we have at present to do, let us put $t = o$, and in the resulting formulæ change u into t, then since

$$\mathrm{Q}t + 2\mathrm{P}t = e^{2t},\ \mathrm{Q}t - \mathrm{P}t = e^{-t}\ , \quad \text{we have}$$

$$\mathrm{Q}t = \frac{1}{3}\left(e^{2t} + 2e^{-t}\right) = 1 + 0\frac{t}{1} + 2\frac{t^2}{1.2} + 2\frac{t^3}{1.2.3} + 6\frac{t^4}{\dots 4} + 10\frac{t^5}{\dots.5} + \text{etc.},$$

$$\mathrm{P}t = \frac{1}{3}\left(e^{2t} - e^{-t}\right) = \quad 1\frac{t}{1} + 1\frac{t^2}{1.2} + 3\frac{t^3}{1.2.3} + 5\frac{t^4}{\dots.4} + 11\frac{t^5}{\dots.5} + \text{etc.}$$

31. When $t = o$, the values of the three functions are $\triangle o = 1$, $\triangle o = 0$, $\triangle o = 0$, wherefore on attributing a small increment δt to this zero, the function $\triangle o$ becomes $1 + o\delta t + o\delta t^2 + \frac{1}{6}\delta t^3$; it is in a state of conjoined maximum and minimum, and augments very slowly. The function $\triangle o$ becomes $o + 1\,.\,\delta t + o\delta t^2 + o\delta t^3$, increasing at the same rate with the primary, and the line representing it must cross the line of abscissæ at an angle of 50^c (45°). And the function $\triangle o$ becomes $o + o\delta t + \frac{1}{2}\delta t^2 + o\delta t^3$, it is therefore in a state of minimum, the radius of curvature of the line which represents it being unit. These phases are shown in figure 3, in which OA is the linear unit.

As t continues to increase, the value of $\triangle t$ approaches and becomes equal to that of $\triangle t$, that is, the corresponding curves intersect at some point B; the value of t at that instant is a root of the equation $\triangle t - \triangle t = o$; we may denote this root by T, in other words we may suppose $\triangle\mathrm{T}$ to be equal to $\triangle\mathrm{T}$.

32. If in the equations of article 27 we put t and u each equal to T, we obtain

$$\triangle_0 2\mathrm{T} = \triangle_0 \mathrm{T}^2 + 2\,\triangle_0 \mathrm{T} \,.\, \triangle_2 \mathrm{T}$$
$$\triangle_1 2\mathrm{T} = 2\triangle_0 \mathrm{T}^2 + \triangle_2 \mathrm{T}^2$$
$$\triangle_2 2\mathrm{T} = \triangle_0 \mathrm{T}^2 + 2\,\triangle_0 \mathrm{T} \,.\, \triangle_2 \mathrm{T}\,,$$

wherefore, on measuring a distance of 2T along the line of abscissæ, we reach the point at which the line of $\triangle_0 t$ is crossed by that of $\triangle_2 t$.

Similarly, if we put 2T for t, and T for u in the same equations, we have

$$\triangle_0 3\mathrm{T} = 2\triangle_0 \mathrm{T}^3 + 6\triangle_0 \mathrm{T}^2 \,.\, \triangle_2 \mathrm{T} + \triangle_2 \mathrm{T}^3$$
$$\triangle_1 3\mathrm{T} = 3\triangle_0 \mathrm{T}^3 + 3\triangle_0 \mathrm{T}^2 \,.\, \triangle_2 \mathrm{T} + 3\triangle_0 \mathrm{T} \,.\, \triangle_2 \mathrm{T}^2$$
$$\triangle_2 3\mathrm{T} = 3\triangle_0 \mathrm{T}^3 + 3\triangle_0 \mathrm{T}^2 \,.\, \triangle_2 \mathrm{T} + 3\triangle_0 \mathrm{T} \,.\, \triangle_2 \mathrm{T}^2\,,$$

showing that the functions $\triangle_1 t$ and $\triangle_2 t$ have again become equal to each other; wherefore the distance, measured on the line of abscissæ, from the intersection of the curves 1 and 2 at the origin to their next intersection, is 3T.

33. It may be shown that this order of intersection continues indefinitely in the following manner. Let w and x be two roots of the equation $\triangle_1 t - \triangle_2 t = o$, then is $w + x$ also a root of the same equation;

$$\text{for} \qquad \triangle_1(w + x) = \triangle_1 w\, \triangle_0 x + \triangle_0 w \,.\, \triangle_2 x + \triangle_2 w \,.\, \triangle_2 x$$
$$\text{and} \qquad \triangle_2(w + x) = \triangle_2 w\, \triangle_0 x + \triangle_1 w \,.\, \triangle_1 x + \triangle_0 w \,.\, \triangle_2 x\,,$$

but by hypotheses, $\triangle_1 w = \triangle_2 w$, $\triangle_1 x = \triangle_2 x$, wherefore the above values may be written

$$\triangle_1(w + x) = \triangle_1 w \,.\, \triangle_0 x + \triangle_0 w \,.\, \triangle_1 x + \triangle_0 w \,.\, \triangle_1 x$$
$$\triangle_2(w + x) = \triangle_1 w \,.\, \triangle_0 x + \triangle_1 w \,.\, \triangle_0 x + \triangle_0 w \,.\, \triangle_1 x\,,$$

which are identic. From this it follows that the equation $\triangle_1 t - \triangle_2 t = o$ is satisfied by every value of t which is a multiple of 3T.

34. The same curves, viz., those representing the functions $\triangle_1 t$ and $\triangle_2 t$, intersect each other on the opposite side of the origin, and on ordinates situated also at the distance 3T from each other; for, according to what has been shown in article 29,

$$\triangle_1(-w) = \triangle_2 w^2 - \triangle_0 w \,.\, \triangle_1 w$$
$$\triangle_2(-w) = \triangle_1 w^2 - \triangle_0 w \,.\, \triangle_2 w\,,$$

wherefore if $\triangle_1 w$ be equal to $\triangle_2 w$, $\triangle_1(-w)$ must also be equal to $\triangle_2(-w)$, and thus the two curves intersect each other on all ordinates placed at distances $\pm 3n\mathrm{T}$ from the origin.

35. Again, if w be such a value of t as to make $\triangle_1 w = \triangle_2 w$, and if T be added to w, we have $\triangle_0(w + \mathrm{T}) = \triangle_0(w + \mathrm{T})$. For

$$\triangle_0(w + \mathrm{T}) = \triangle_0 w \,.\, \triangle_0 \mathrm{T} + \triangle_2 w \,.\, \triangle_1 \mathrm{T} + \triangle_1 w \,.\, \triangle_2 \mathrm{T}$$
$$\triangle_1(w + \mathrm{T}) = \triangle_1 w \,.\, \triangle_0 \mathrm{T} + \triangle_0 w \,.\, \triangle_1 \mathrm{T} + \triangle_2 w \,.\, \triangle_2 \mathrm{T}\,;$$

but, according to hypotheses $\triangle_1 w = \triangle_2 w$, and (article 31) $\triangle_0 \mathrm{T} = \triangle_1 \mathrm{T}$, wherefore these values become

$$\triangle_0(w + \mathrm{T}) = \triangle_0 w \,.\, \triangle_0\mathrm{T} + \triangle_1 w \,.\, \triangle_0\mathrm{T} + \triangle_1 w \,.\, \triangle_2\mathrm{T}$$
$$\triangle_1(w + \mathrm{T}) = \triangle_1 w \,.\, \triangle_0\mathrm{T} + \triangle_0 w \,.\, \triangle_0\mathrm{T} + \triangle_1 w \,.\, \triangle_2\mathrm{T}$$

which again are identic.

36. Lastly, the curves $\triangle_0 t$ and $\triangle_2 t$ intersect on the ordinates corresponding to the abscissæ $w + 2\mathrm{T}$, for similarly

$$\triangle_0(w + 2\mathrm{T}) = \triangle_0 w \,.\, \triangle_0 2\mathrm{T} + \triangle_2 w \,.\, \triangle_1 2\mathrm{T} + \triangle_1 w \,.\, \triangle_2 2\mathrm{T}$$
$$\triangle_2(w + 2\mathrm{T}) = \triangle_2 w \,.\, \triangle_0 2\mathrm{T} + \triangle_1 w \,.\, \triangle_1 2\mathrm{T} + \triangle_0 w \,.\, \triangle_2 2\mathrm{T}\ ;$$

but, according to article 32, $\triangle_0 2\mathrm{T} = \triangle_2 2\mathrm{T}$, wherefore

$$\triangle_0(w + 2\mathrm{T}) = \triangle_0 w \,.\, \triangle_0 2\mathrm{T} + \triangle_1 w \,.\, \triangle_1 2\mathrm{T} + \triangle_1 w \,.\, \triangle_0 2\mathrm{T}$$
$$\triangle_2(w + 2\mathrm{T}) = \triangle_1 w \,.\, \triangle_0 2\mathrm{T} + \triangle_1 w \,.\, \triangle_1 2\mathrm{T} + \triangle_0 w \,.\, \triangle_0 2\mathrm{T}$$

which also are identic.

Hence it follows that the intersections of the curves take place as under

of $\triangle_0$ and $\triangle_1$ at $(1 \pm 3n)\mathrm{T}$
of $\triangle_0$ and $\triangle_2$ at $(2 \pm 3n)\mathrm{T}$
of $\triangle_1$ and $\triangle_2$ at $\pm 3n\mathrm{T}$

n being any integer number whatever.

37. This distance T, or rather its triple 3T, bears a very remarkable analogy to the π of the angular calculus; and it becomes a matter of some interest to determine its value in numbers. In order to this determination we observe that the first derivative of $\triangle_0 t - \triangle_1 t$ is $\triangle_2 t - \triangle_0 t$; the second derivative $\triangle_1 t - \triangle_2 t$; while the third derivation brings us back to the original function $\triangle_0 t - \triangle_1 t$. Hence we have the subjoined calculation—

$\triangle_1 t - \triangle_2 t$	$\triangle_2 t - \triangle_0 t$	$\triangle_0 t - \triangle_1 t$	t
0·00000 00000	−1·00000 00000	+1·00000 00000	0·
+1·00000 00000	0·00000 00000	−1·00000 00000	1·
− ·50000 00000	+ ·50000 00000	·00000 00000	
·00000 00000	− ·16666 66667	+ ·16666 66667	
+ 4166 66667	0000 00000	− 4166 66667	
− 833 33333	+ 833 33333	000 00000	
00 00000	− 138 88889	+ 138 88889	
+ 19 84127	00 00000	− 19 84127	
− 2 48016	+ 2 48016	0 00000	
00000	− 27557	+ 27557	
+ 2756	0000	− 2756	
− 251	+ 251	000	
00	− 21	+ 21	
+ 2	0	− 2	

$\triangle_1 t - \triangle_2 t$	$\triangle_2 t - \triangle_0 t$	$\triangle_0 t - \triangle_1 t$	t
+ ·53350 71951	− ·65970 01534	+ ·12619 29583	1·
+ 2523 85917	+ ·10670 14390	− ·13194 00307	·2
− 1319 40031	+ 252 38592	+ 1067 01439	
+ 71 13429	− 87 96002	+ 16 82573	
+ 84129	+ 3 55671	− 4 39800	
− 17592	+ 3365	+ 14227	
+ 474	− 586	+ 112	
+ 3	+ 14	− 17	
+ ·54626 98280	− ·55131 86090	+ ·00504 87810	1·2
+ 4 64488	+ 502 56824	− 507 21312	·0092
− 2 33318	+ 2137	+ 2 31181	
+ 709	− 716	+ 7	
+ 0	+ 2	− 2	
+ ·54629 30159	− ·54629 27843	− ·00000 02315	1·2092
+ 0	− 2315	+ 2315	− 00 04238
+ ·54629 30159	− ·54629 30158	·00000 00000	1·20919 95762

which gives for T the value 1·20919 95762. In the year 1850, being in Constantinople, and having leisure, I computed this value to twenty-five places, it is

$$T = 1·20919\ 95761\ 56145\ 23372\ 93856\ ;$$

its relation to the number $\pi = 3·14159$, etc., might here be pointed out, but I prefer to postpone this consideration until the number π itself arise in the course of our inquiry.

38. Having now determined all those ordinates on which the curves representing the three ternary functions $\triangle_0 t$, $\triangle_1 t$, $\triangle_2 t$ cross each other, we proceed to investigate the distances intercepted by them on those ordinates. In article 29 it has been shown that the sum of the squares of these functions exceeds the sum of their products in pairs by e^{-t}, wherefore

$$(\triangle_0 t - \triangle_1 t)^2 + (\triangle_1 t - \triangle_2 t)^2 + (\triangle_2 t - \triangle_0 t)^2 = 2e^{-t}$$

so that the three lines must keep nearer and nearer to each other as t is taken greater; and thus they soon merge so closely into one line that it is impossible to delineate them separately. This line into which they merge must evidently be that of which the ordinates are $\frac{1}{3} e^{t}$.

39. Since for the absciss T we have $\triangle_0 T - \triangle_1 T = 0$, we must, according to the preceding article, have $(\triangle_0 T - \triangle_2 T)^2 = e^{-T}$, or

$$\triangle_0 T - \triangle_2 T = e^{-\frac{1}{2}T}$$

and consequently, the intervals intercepted on the ordinates drawn at O, T, 2T, 3T, etc., are in continued progression, the common ratio of the progression being

$$e^{-\frac{1}{2}T} = ·54629\ 30158\ 73601\ 37743\ 46503$$

the inverse ratio being

$$e^{-\frac{1}{2}\mathrm{T}} = 1{\cdot}83051\ 94665\ 55609\ 67148\ 01998\ .$$

40. If we take any absciss u less than T, and draw an ordinate to cut the three lines; and if beyond T at the same distance u we draw another ordinate, the distances intercepted on these ordinates, between the curves, are reciprocally proportional. For since $\overset{0}{\triangle}\mathrm{T} = \overset{1}{\triangle}\mathrm{T}$, the expressions for the functions of $\mathrm{T} + u$ may be written

$$\overset{0}{\triangle}(\mathrm{T} + u) = \overset{0}{\triangle}\mathrm{T}\,\{\overset{0}{\triangle}u + \overset{2}{\triangle}u\} + \overset{2}{\triangle}\mathrm{T} \,.\, \overset{1}{\triangle}u\ ,$$
$$\overset{1}{\triangle}(\mathrm{T} + u) = \overset{0}{\triangle}\mathrm{T}\,\{\overset{0}{\triangle}u + \overset{1}{\triangle}u\} + \overset{2}{\triangle}\mathrm{T} \,.\, \overset{2}{\triangle}u\ ,$$
$$\overset{2}{\triangle}(\mathrm{T} + u) = \overset{0}{\triangle}\mathrm{T}\,\{\overset{1}{\triangle}u + \overset{2}{\triangle}u\} + \overset{2}{\triangle}\mathrm{T} \,.\, \overset{0}{\triangle}u\ ;$$

wherefore,

$$\overset{1}{\triangle}(\mathrm{T} + u) - \overset{0}{\triangle}(\mathrm{T} + u) = (\overset{0}{\triangle}\mathrm{T} - \overset{2}{\triangle}\mathrm{T})\,\{\overset{1}{\triangle}u - \overset{2}{\triangle}u\}$$
$$\overset{0}{\triangle}(\mathrm{T} + u) - \overset{2}{\triangle}(\mathrm{T} + u) = (\overset{0}{\triangle}\mathrm{T} - \overset{2}{\triangle}\mathrm{T})\,\{\overset{0}{\triangle}u - \overset{1}{\triangle}u\}\ ;$$

that is to say, the intervals intercepted on the ordinates between T and 2T are proportional to those intercepted on the ordinates between O and T, these latter being reduced in the ratio of $e^{-\frac{1}{2}\mathrm{T}}$ to 1, that is, of ·54629, etc., to 1.

The same law extends for every interval between nT and $(n + 1)$ T, and that on both sides of the zero-point; so that if the complete details of the intersections on the ordinates between O and T were once computed, those for every other interval could thence be easily obtained.

41. If, on the other side of the origin, we measure off Ou' equal to Ou, and draw an ordinate at u', the distances intercepted thereon are again proportional to those intercepted on the ordinate at u, only in a different order. For, according to article 29, we have

$$\overset{0}{\triangle}(-u) = \overset{0}{\triangle}u^2 - \overset{1}{\triangle}u \,.\, \overset{2}{\triangle}u$$
$$\overset{1}{\triangle}(-u) = \overset{2}{\triangle}u^2 - \overset{0}{\triangle}u \,.\, \overset{1}{\triangle}u$$
$$\overset{2}{\triangle}(-u) = \overset{1}{\triangle}u^2 - \overset{0}{\triangle}u \,.\, \overset{2}{\triangle}u$$

and, consequently,

$$\overset{0}{\triangle}(-u) - \overset{2}{\triangle}(-u) = \{\overset{0}{\triangle}u - \overset{1}{\triangle}u\}\, e^t\ ;$$
$$\overset{2}{\triangle}(-u) - \overset{1}{\triangle}(-u) = \{\overset{1}{\triangle}u - \overset{2}{\triangle}u\}\, e^t\ .$$

Hence, if we measure the distance Tu'' backwards from T and equal to Ou, the distances intercepted on the ordinate drawn through u'' are again proportional to those intercepted at u; and thus we may expect that, for the ordinate which bisects the distance OT, the intercepted distances should be alike.

For the purpose of examining into this matter, let us assume some absciss v, such that $\overset{0}{\triangle}v - \overset{1}{\triangle}v = \overset{1}{\triangle}u - \overset{2}{\triangle}v$, and let us compute the distances intercepted on the ordinate at $2v$.

We have (article 27):

$$\begin{aligned}
\triangle_0 2v &= \triangle_0 v^2 + 2\triangle_1 v \,.\, \triangle_2 v \,, \\
\triangle_1 2v &= \triangle_2 v^2 + 2\triangle_0 v \,.\, \triangle_1 v \,, \\
\triangle_2 2v &= \triangle_1 v^2 + 2\triangle_0 v \,.\, \triangle_2 v \,,
\end{aligned}$$

whence

$$\triangle_0 2v - \triangle_1 2v = (\triangle_0 v - \triangle_2 v)(\triangle_0 u - 2\triangle_1 v + \triangle_2 v) = 0$$

so that the two curves $\triangle_0$ and $\triangle_1$ intersect each other on the ordinate at $2v$; in other words, v must be the half of T. From this latter consideration it follows that, in tabulating the intersections, it would be enough to carry the computations as far as to $\frac{1}{2}$T, just as in the construction of the trigonometrical canon it is sufficient to make the calculations up to half a right angle.

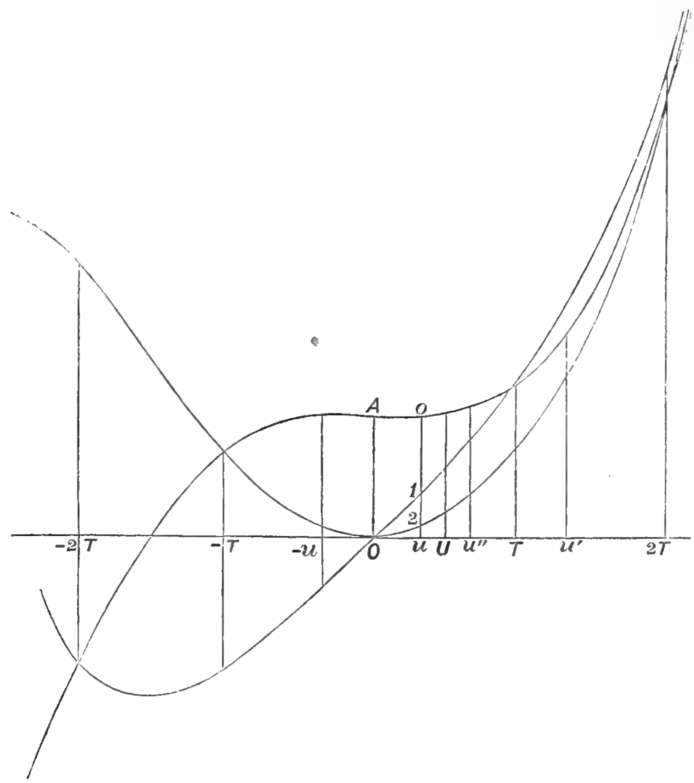


Fig. 3.

42. The relative positions of these successive intersections may be clearly exhibited by the following artifice. Let us suppose that on one of the ordinates, as that at u, figure 3, a plane is set perpendicular to the plane of the paper, and that through the points u, 2, 1, 0 lines are drawn normal to the picture; these lines being represented generally by the horizontal lines of figure 4. This arrangement being made for each successive ordinate, let an equilateral trigon ABC (fig. 4) be constructed so that its corners may be upon the

three horizontal lines drawn through 0, 1, and 2 respectively. Then, if the ordinate be supposed to move uniformly along the OT of figure 3, the side of the trigon ABC will decrease in geometrical progression, while, at the same time, it has a uniform angular motion, making one complete revolution when u moves through the distance 6T, that is, six times OT. Also the middle point of the trigon will describe a logarithmic curve, of which the ordinate is $\frac{1}{3}e^t$.

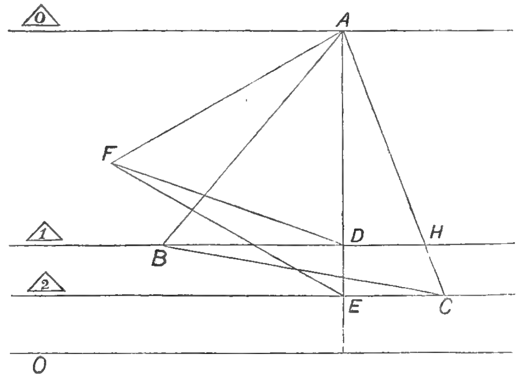


Fig. 4.

Lemma.

In order to demonstrate the truth of these assertions, let us draw through A the ordinate ADE, upon AE construct the equilateral trigon AFE, and join FD; then, since AC and AE are divided similarly at H and D, the trigons ABH, HBC are similar to AFD, DFE. Now the square of FD is obviously $AE^2 - ED \cdot DA$, that is, $DE^2 + ED \cdot DA + DA^2$; and it can be very easily shown that $BC^2 = \frac{4}{3} FD^2$, wherefore

$$BC^2 = \tfrac{4}{3}\{DE^2 + ED \cdot DA + DA^2\}$$

On putting for ED and DA their values $\triangle_1 t - \triangle_2 t$ and $\triangle_0 t - \triangle_1 t$, the above formula becomes

$$BC^2 = \frac{4}{3}\left\{ \triangle_0 t^2 + \triangle_1 t^2 + \triangle_2 t^2 - \triangle_0 t \cdot \triangle_1 t - \triangle_0 t \cdot \triangle_2 t - \triangle_1 t \cdot \triangle_2 t \right\}$$

and this again, according to article 29, passes into

$$BC^2 = \frac{4}{3} e^{-t}$$

wherefore the side of the equilateral trigon is given by the formula

$$BC = \sqrt{\frac{4}{3}} \cdot e^{-\frac{1}{2}t}$$

so that, while the ordinate moves over the distance OT, the side of the equilateral trigon is diminished in the ratio of $e^{-\frac{1}{2}T}$ to unit; that is just in the ratio which has already been found for the intervals on the ordinates.

When the plane of the equilateral trigon passes along the line OA, the base BC is horizontal; as the plane moves from O towards V the end B is raised higher than C, and when that plane passes along the ordinate at V the side AC has become upright; that is to say, the trigon has made one-twelfth part of a revolution. As the plane is moved beyond V, the turning continues, and for the ordinate at T the trigon has made one-sixth part of a turn, the side BA being now horizontal. In this way the rotation continues at the rate of one turn for the distance 6T. For each interval of $\frac{1}{2}$T the uniformity of the angular motion is obvious; it remains to be shown that the same uniformity holds good at intermediate positions; that is, that the angle of inclination is proportional to the distance O*u*. This part of the demonstration, however, may be conveniently reserved until we shall have reached the genesis of angular functions.

43. I shall conclude this slight sketch of the doctrine of ternary recurring derivatives by an investigation into the sum of their cubes.

Temporarily, for the sake of abbreviation, let us put C*t* as the symbol for the sum of the cubes of the ternary functions; that is, let

$$\begin{aligned}
\mathrm{C}t &= \triangle_0 t^3 + \triangle_1 t^3 + \triangle_2 t^3 \qquad\text{, then}\\
{}_1\mathrm{C}t &= 3\{\triangle_0 t^2 \cdot \triangle_2 t + \triangle_1 t^2 \cdot \triangle_0 t + \triangle_2 t^2 \cdot \triangle_1 t\}\ ,\\
{}_2\mathrm{C}t &= 9\{\triangle_0 t \cdot \triangle_2 t^2 + \triangle_1 t \cdot \triangle_0 t^2 + \triangle_2 t \cdot \triangle_1 t^2\}\ ,\\
{}_3\mathrm{C}t &= 9\{\triangle_2 t^3 + \triangle_0 t^3 + \triangle_1 t^3 + 6\triangle_0 t \cdot \triangle_1 t \cdot \triangle_2 t\}\ ;
\end{aligned}$$

now it has been shown (article 28) that

$$\triangle_0 t^3 + \triangle_1 t^3 + \triangle_2 t^3 - 3\triangle_0 t \,.\, \triangle_1 t \,.\, \triangle_2 t = 1$$

wherefore ${}_3\mathrm{C}t = 27\mathrm{C}t - 18$, and consequently the subsequent derivatives follow thus:—

$$\begin{aligned}
{}_4\mathrm{C}t &= 27\ {}_1\mathrm{C}t\\
{}_5\mathrm{C}t &= 27\ {}_2\mathrm{C}t\\
{}_6\mathrm{C}t &= 27^2\ \mathrm{C}t - 27 \,.\, 18\\
{}_7\mathrm{C}t &= 27^2\ {}_1\mathrm{C}t\\
{}_8\mathrm{C}t &= 27^2\ {}_2\mathrm{C}t\\
{}_9\mathrm{C}t &= 27^3\ \mathrm{C}t - 27^2 \,.\, 18\ \text{, and so on.}
\end{aligned}$$

From these we obtain, by TAYLOR'S Theorem,

$$\mathrm{C}(t+u) = \frac{2}{3}\left\{1 - \triangle_0 3u\right\} + \mathrm{C}t \cdot \triangle_0 3u + {}_1\mathrm{C}t \cdot \frac{1}{3}\triangle_1 3u + {}_2\mathrm{C}t \cdot \frac{1}{9}\triangle_2 3u\ ,$$

but when $t = o$, $\mathrm{C}t = 1$, ${}_1\mathrm{C}t = 0$, ${}_2\mathrm{C}t = 0$ wherefore

$$\mathrm{C}u = \frac{2}{3} + \frac{1}{3}\triangle_0\, 3u$$

and thus we have the following values of the sums of the triple products,

$$\begin{aligned}
\triangle_0 t^3 + \triangle_1 t^3 + \triangle_2 t^3 &= \frac{2}{3} + \frac{1}{3}\triangle_0 3t\ ,\\
\triangle_0 t^2 \cdot \triangle_2 t + \triangle_1 t^2 \cdot \triangle_0 t + \triangle_2 t^2 \cdot \triangle_1 t &= \frac{1}{3}\triangle_2 3t\ ,\\
\triangle_0 t \cdot \triangle_2 t^2 + \triangle_1 t \cdot \triangle_0 t^2 + \triangle_2 t \cdot \triangle_1 t^2 &= \frac{1}{3}\triangle_1 3t\ ,\\
\triangle_0 t \cdot \triangle_1 t \cdot \triangle_2 t &= -\frac{1}{9} + \frac{1}{9}\triangle_0 3t\ .
\end{aligned}$$

Fourth Order.

44. According to the principles already laid down, the fundamental functions of the Fourth Order are,

$$\boxed{0}\,t = 1 + \frac{t^4}{\ldots 4} + \frac{t^8}{\ldots .8} + \frac{t^{12}}{\ldots .12} + \text{etc.}\,,$$

$$\boxed{1}\,t = \frac{t}{1} + \frac{t^5}{\ldots .5} + \frac{t^9}{\ldots .9} + \frac{t^{13}}{\ldots ..13} + \text{etc.}\,,$$

$$\boxed{2}\,t = \frac{t^2}{1\,.\,2} + \frac{t^6}{\ldots .6} + \frac{t^{10}}{\ldots .10} + \frac{t^{14}}{\ldots .14} + \text{etc.}\,,$$

$$\boxed{3}\,t = \frac{t^3}{1\,.\,2\,.\,3} + \frac{t^7}{\ldots ..7} + \frac{t^{11}}{\ldots .11} + \frac{t^{15}}{\ldots .15} + \text{etc.}\,,$$

and it is clear that the values of the first and third of these are the same for $-t$ as for $+t$, while those of the second and fourth merely change their signs. Hence a table of these functions for positive values of the primary can be made to serve also for negative values.

The construction of the table can be readily accomplished thus. Having prepared and titled five columns, four for the functions and one for the primary, we place at the heads of these a set of corresponding values; in order thence to compute the values corresponding to a new state of the primary, we multiply each by the increment δt of the primary and write the product in the column belonging to the function of next higher title. These results, which form the second line, are now multiplied by $\frac{1}{2}\delta t$, the products being placed in the adjoining column; the numbers entered in the third line are multiplied by $\frac{1}{3}\delta t$; those in the fourth line by $\frac{1}{4}\delta t$, and the work is carried on until the terms become insignificant. The removals, it must be carefully observed, are to be from column $\boxed{0}\,t$ to column $\boxed{1}\,t$, from $\boxed{1}\,t$ to $\boxed{2}\,t$, from $\boxed{2}\,t$ to $\boxed{3}\,t$, and lastly from $\boxed{3}\,t$ back to $\boxed{0}\,t$. The scheme of the calculation being this—

$\boxed{0}\,t$	$\boxed{1}\,t$	$\boxed{2}\,t$	$\boxed{3}\,t$	t
$\boxed{3}\,t\,.\,\delta t$	$\boxed{0}\,t\,.\,\delta t$	$\boxed{1}\,t\,.\,\delta t$	$\boxed{2}\,t\,.\,\delta t$	$+\,\delta t$
$\frac{1}{2}\boxed{2}\,t\,.\,\delta t^2$	$\frac{1}{2}\boxed{3}\,t\,.\,\delta t^2$	$\frac{1}{2}\boxed{0}\,t\,.\,\delta t^2$	$\frac{1}{2}\boxed{1}\,t\,.\,\delta t^2$	
$\frac{1}{6}\boxed{1}\,t\,.\,\delta t^3$	$\frac{1}{6}\boxed{2}\,t\,.\,\delta t^3$	$\frac{1}{6}\boxed{3}\,t\,.\,\delta t^3$	$\frac{1}{6}\boxed{0}\,t\,.\,\delta t^3$	
etc.	etc.	etc.	etc.	
$\boxed{0}\,(t+\delta t)$	$\boxed{1}\,(t+\delta t)$	$\boxed{2}\,(t+\delta t)$	$\boxed{3}\,(t+\delta t)$	$t\,+\,\delta t$

An example of the actual calculation is subjoined. Beginning at $t = 0$, the values of the quaternary functions are 1, 0, 0, 0. From these the values of the functions $\boxed{0}\cdot 1$; $\boxed{1}\cdot 1$;

$\boxed{0}\, t$	$\boxed{1}\, t$	$\boxed{2}\, t$	$\boxed{3}\, t$	t
1·00000 00000	0·00000 00000	0·00000 00000	0·00000 00000	0·
41667	·10000 00000	500 00000	16 66667	·1
	833	14		
1·00000 41667	0·10000 00833	0·00500 00014	0·00016 66667	0·1
1 66667	·10000 04167	1000 00083	50 00002	·1
2 50000	8333	500 00208	50 00004	
1 66667	8333	278	16 66674	
41667	4167	208	7	
0	833	83	4	
0	0	14	1	
1·00000 66668	0·20000 26666	0·02000 00888	0·00133 33359	0·2
13 33336	·10000 66667	2000 02667	200 00089	·1
10 00004	66667	500 03333	100 00133	
3 33338	33333	2222	16 66778	
41670	8333	833	56	
1	833	167	17	
0	0	14	3	
1·00033 75017	0·30002 02499	0·04500 10124	0·00450 00435	0·3

$\boxed{2}\cdot 1$ and $\boxed{3}\cdot 1$ are computed by assuming the addition $\delta t = \cdot 1$; in this first computation the numerous zeroes are omitted. From these, by making another addition $\delta t = \cdot 1$, the values of the functions of ·2, and thence, again, those of the functions of ·3 are computed.

45. The functions of the sum $t + u$ are given in terms of those of t and u separately, by the four following equations:—

$$\boxed{0}(t+u) = \boxed{0}\,t\,.\,\boxed{0}\,u + \boxed{1}\,t\,.\,\boxed{3}\,u + \boxed{2}\,t\,.\,\boxed{2}\,u + \boxed{3}\,t\,.\,\boxed{1}\,u\,, \qquad (1)$$

$$\boxed{1}(t+u) = \boxed{1}\,t\,.\,\boxed{0}\,u + \boxed{2}\,t\,.\,\boxed{3}\,u + \boxed{3}\,t\,.\,\boxed{2}\,u + \boxed{0}\,t\,.\,\boxed{1}\,u\,, \qquad (2)$$

$$\boxed{2}(t+u) = \boxed{2}\,t\,.\,\boxed{0}\,u + \boxed{3}\,t\,.\,\boxed{3}\,u + \boxed{0}\,t\;\;\boxed{2}\,u + \boxed{1}\,t\,.\,\boxed{1}\,u\,, \qquad (3)$$

$$\boxed{3}(t+u) = \boxed{3}\,t\,.\,\boxed{0}\,u + \boxed{0}\,t\,.\,\boxed{3}\,u + \boxed{1}\,t\,.\,\boxed{2}\,u + \boxed{2}\,t\,.\,\boxed{1}\,u\,; \qquad (4)$$

and similarly those of the differences $t - u$ are

$$\boxed{0}(t-u) = \boxed{0}\,t\,.\,\boxed{0}\,u - \boxed{1}\,t\,.\,\boxed{3}\,u + \boxed{2}\,t\,.\,\boxed{2}\,u - \boxed{3}\,t\,.\,\boxed{1}\,u\,; \qquad (5)$$

$$\boxed{1}(t-u) = \boxed{1}\,t\,.\,\boxed{0}\,u - \boxed{2}\,t\,.\,\boxed{3}\,u + \boxed{3}\,t\,.\,\boxed{2}\,u - \boxed{0}\,t\,.\,\boxed{1}\,u\,, \qquad (6)$$

$$\boxed{2}(t-u) = \boxed{2}\,t\,.\,\boxed{0}\,u - \boxed{3}\,t\,.\,\boxed{3}\,u + \boxed{0}\,t\,.\,\boxed{2}\,u - \boxed{1}\,t\,.\,\boxed{1}\,u\,, \qquad (7)$$

$$\boxed{3}(t-u) = \boxed{3}\,t\,.\,\boxed{0}\,u - \boxed{0}\,t\,.\,\boxed{3}\,u + \boxed{1}\,t\,.\,\boxed{2}\,u - \boxed{2}\,t\,.\,\boxed{1}\,u\,. \qquad (8)$$

These follow at once from the development of the functions of $t + u$ and $t - u$, by help of TAYLOR'S Theorem. By combining these eight equations in various ways, we may obtain a vast number of formulæ, many of which are singularly interesting.

In order to fix the mutual relations of these functions easily in the mind, we may construct a regular tetragon, and mark, as in the margin (fig. 5), the numbers 0, 1, 2, 3, at the corners, to stand for the functions $\boxed{0}$, $\boxed{1}$, $\boxed{2}$, $\boxed{3}$ respectively. Each one is then the derivative of the succeeding, or the primitive of the preceding, taken in the order in which they are written. The six couples which can be made among these four functions may then be represented by the six lines joining the corners of the figure, of which four are lateral, and two diagonal.

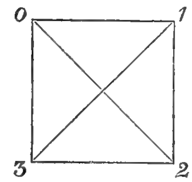


Fig. 5.

Let now a piece of paper be cut of the size of the square, and let there be written the same figures, 0, 1, 2, 3, at its four corners. If we place this paper upon figure 5, so as that the 0 may agree with the 0, we obtain the appearance shown in figure 6; and if we suppose the outer numbers to indicate functions of t, while the inner numbers indicate the corresponding functions of u, this figure 6 will at once picture the value of $\boxed{0}\,(t+u)$ in equation 1.

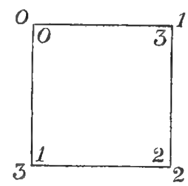


Fig. 6.

If we turn the inner paper until its 0 coincide with the 1 of figure 5, we obtain figure 7, which, in the same way, represents the value of $\boxed{1}\,(t+u)$; and so similarly of figures 8 and 9, which picture the values of $\boxed{2}\,(t+u)$ and $\boxed{3}\,(t+u)$.

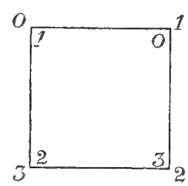


Fig. 7.

This same artifice may be applied to recurrences of higher orders, and it might have been used also for the ternary functions.

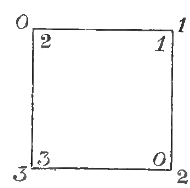


Fig. 8.

46. If we make $t = u$, equations (5) and (7) of the preceding article give,

$$\boxed{0}\,o = 1 = \boxed{0}\,t^2 - 2\boxed{1}\,t \,.\, \boxed{3}\,t + \boxed{2}\,t^2 \qquad (9)$$

$$-\boxed{2}\,o = 0 = \boxed{1}\,t^2 - 2\boxed{0}\,t \,.\, \boxed{2}\,t + \boxed{3}\,t^2 \qquad (10)$$

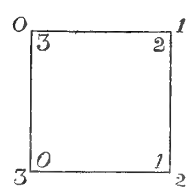


Fig. 9.

These are two cases of a general proposition which may be exhibited thus:—

Let $\boxed{A}\,t$, $\boxed{B}\,t$, $\boxed{C}\,t$, $\boxed{D}\,t$ be the symbols of any four recurring functions, then I say that the sum of the squares of one diagonal pair differs from twice the product of the other diagonal pair by a constant quantity.

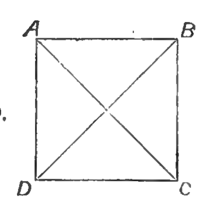


Fig. 10.

For the derivative of the sum $\boxed{A}\,t^2 + \boxed{C}\,t^2$ is $2\boxed{A}\,t \,.\, \boxed{D}\,t + 2\boxed{C}\,t \,.\, \boxed{B}\,t$; while that of $2\boxed{B}\,t \,.\, \boxed{D}\,t$ is the very same, and, consequently, for all quaternary functions, $\boxed{\scriptstyle A}t^2 - 2\boxed{\scriptstyle B}t\,.\,\boxed{\scriptstyle D}t + \boxed{\scriptstyle C}t^2 = \text{constant},$

as also $\boxed{\scriptstyle B}t^2 - 2\boxed{\scriptstyle A}t\,.\,\boxed{\scriptstyle C}t + \boxed{\scriptstyle D}t^2 = \text{constant}.$

47. By substracting equation 10 from equation 9 we obtain

$$1 = \boxed{\scriptstyle 0}t^2 + 2\boxed{\scriptstyle 0}t\,.\,\boxed{\scriptstyle 2}t + \boxed{\scriptstyle 2}t^2 - \boxed{\scriptstyle 1}t^2 - 2\boxed{\scriptstyle 1}t\,.\,\boxed{\scriptstyle 3}t - \boxed{\scriptstyle 3}t^2\,, \quad \text{or}$$

$$1 = \{\boxed{\scriptstyle 0}t + \boxed{\scriptstyle 2}t\}^2 - \{\boxed{\scriptstyle 1}t + \boxed{\scriptstyle 3}t\}^2 \quad ; \qquad (11)$$

now $\boxed{\scriptstyle 0}t + \boxed{\scriptstyle 2}t$ is evidently the function sus t, while $\boxed{\scriptstyle 1}t + \boxed{\scriptstyle 3}t$ is cat t, wherefore the above equation amounts to the property of catenarian functions that sus t^2 — cat $t^2 = 1$.

48. By adding together the same equations we have, on the other hand,

$$1 = \boxed{\scriptstyle 0}t^2 - 2\boxed{\scriptstyle 0}t\,.\,\boxed{\scriptstyle 2}t + \boxed{\scriptstyle 2}t^2 + \boxed{\scriptstyle 1}t^2 - 2\boxed{\scriptstyle 1}t\,.\,\boxed{\scriptstyle 3}t + \boxed{\scriptstyle 3}t^2\,, \quad \text{or}$$

$$1 = \{\boxed{\scriptstyle 0}t - \boxed{\scriptstyle 2}t\}^2 + \{\boxed{\scriptstyle 1}t - \boxed{\scriptstyle 3}t\}^2 \quad . \qquad (12)$$

The differences $\boxed{\scriptstyle 0}t - \boxed{\scriptstyle 2}t$ and $\boxed{\scriptstyle 1}t - \boxed{\scriptstyle 3}t$ will be at once recognised as cos t and sin t respectively; and I shall at once give these titles, without, however, in the meantime attaching any signification to them, that is to say, for shortness' sake we shall put

$$\boxed{\scriptstyle 0}t - \boxed{\scriptstyle 2}t = \cos t\ ; \qquad \boxed{\scriptstyle 1}t - \boxed{\scriptstyle 3}t = \sin t \qquad (13)$$

and then equation (12) becomes

$$1 = \cos t^2 + \sin t^2 \qquad (14)$$

Taking the derivatives of equations (13) we have

$$\left.\begin{aligned} \boxed{\scriptstyle 3}t - \boxed{\scriptstyle 1}t &= {}_1\cos t = -\sin t \\ \boxed{\scriptstyle 0}t - \boxed{\scriptstyle 2}t &= {}_1\sin t = +\cos t \end{aligned}\right\} (15)$$

and thus these functions cos t, sin t are recurring functions of the Fourth Order, the order of derivation being

$$\cos t\,,\ -\sin t\,,\ -\cos t\,,\ +\sin t\,;\quad +\cos t\,, \text{ etc.}$$

49. If we measure distances along the line of abscissæ, corresponding to successive values of the primary variable t, and, if we set up ordinates proportional to the values of the four functions, we shall obtain four curves characteristic of these functions. For the case $t = 0$, the curve $\boxed{\scriptstyle 0}$ must pass through the point A (fig. 11), situated at the distance 0A equal to the linear unit.

Since the first derivative of $\boxed{\scriptstyle 0}t$ is there zero, the curve $\boxed{\scriptstyle 0}$ must at A be parallel to the line of abscissæ; and, since its second derivative $\boxed{\scriptstyle 2}t$ is also zero, the radius of curvature at A must be infinite, that is to say, the curve must be quite flat at A. Moreover, the third derivative $\boxed{\scriptstyle 2}t$ being also zero, and its fourth derivative, viz., itself, being positive, A must correspond to a minimum ordinate, and the curve must rise on either side of A; also the curve must be symmetrically placed on either side of the axis 0A.

The curve $\boxed{\scriptstyle 1}$ passes through the origin 0, and since the derivative of $\boxed{\scriptstyle 1}t$, viz.,

$\boxed{0}t$ is there unit, the curve $\boxed{1}$ must there be inclined at 50^c ($45°$) to the line of abscissæ. Also, since the values of $\boxed{1}t$, with their signs changed, are those of $\boxed{1}(-t)$, it follows that any straight line drawn through 0 to cut the curve $\boxed{1}$ on the one side, cuts it also at a like distance on the other side of the origin. Since the curve $\boxed{1}$ is nearly straight at 0, the second derivative of $\boxed{1}t$ being there zero, and since $\boxed{0}$ is nearly straight, and horizontal at A, the curves $\boxed{0}$ and $\boxed{1}$ must meet each other on some ordinate not far from the value $t = 1$. Now the values of the four functions when $t = 1$ are,

$$\boxed{0}1 = 1{\cdot}04169\,14703$$
$$\boxed{1}1 = 1{\cdot}00833\,60892$$
$$\boxed{2}1 = 0{\cdot}50138\,91645$$
$$\boxed{3}1 = 0{\cdot}16686\,51044\ ,$$

so that the difference $\boxed{0}1 - \boxed{1}1$ is only ·03335 53811; dividing this by its first derivative, viz., $\boxed{3}1 - \boxed{0}1$, we find the correction ·03, and thus 1·03 is nearly the value of the abscissa of the intersection of the two curves; the true value of this abscissa obtained by the subjoined computation is P = 1·03841 56373, and those of the corresponding functions are,

$$\boxed{0}P = 1{\cdot}04848\,12871$$
$$\boxed{1}P = 1{\cdot}04848\,12871$$
$$\boxed{2}P = 0{\cdot}54089\,53047$$
$$\boxed{3}P = 0{\cdot}18688\,01785\ .$$

$\boxed{0}t$	$\boxed{1}t$	$\boxed{2}t$	$\boxed{3}t$	t
1·04169 14703	1·00833 60892	0·50138 91645	0·16686 51044	1·
500 59531	3125 07441	3025 00827	1504 16749	·03
22 56251	7 50893	46 87612	45 37512	
45375	22563	7509	46876	
352	340	169	56	
0	2	2	1	
1·04692 76213	1·03966 42131	0·53210 87763	0·18236 52239	1·03
153 18679	879 41920	873 31794	446 97137	·0084
1 87728	64338	3 69356	3 66794	
1027	526	180	1034	
2	2	1	0	
1·04847 83649	1·04846 48918	0·54087 89095	0·18687 17205	1·0384
29152	1 63563	1 63561	84377	·00001 56
1	0	1	1	
1·04848 12802	1·04848 12481	0·54089 52656	0·18688 01583	1·03841 56
70	391	391	202	00373
1·04848 12872	1·04848 12872	0·54089 53047	0·18688 01785	1·03841 56373

This abscissa P, farther than that it marks the intersection of the two curves $\boxed{0}$ and $\boxed{1}$, is of no immediate interest to us.

The line $\boxed{0}$ having now crossed and passed under the line $\boxed{1}$ is next met by the line $\boxed{2}$ at a point, see figure 11, of which the abscissa is OΠ; putting Π to denote the value of this abscissa, we must have $\boxed{0}\,\Pi = \boxed{2}\,\Pi$. Now for $t = 1{\cdot}5$ we have

$$\boxed{0}\,1{\cdot}5 = 1{\cdot}21157\ 34084\ 55475\ 11794$$
$$\boxed{1}\,1{\cdot}5 = 1{\cdot}56338\ 72208\ 49435\ 96389$$
$$\boxed{2}\,1{\cdot}5 = 1{\cdot}14083\ 62067\ 87772\ 20785$$
$$\boxed{3}\,1{\cdot}5 = 0{\cdot}56589\ 22342\ 45381\ 53295$$

hence, by the subjoined process, which is exactly analogous to the preceding, we obtain

$\boxed{0}\,t$	$\boxed{1}\,t$	$\boxed{2}\,t$	$\boxed{3}\,t$	t
1·21157 34085 3961 24564 279 50487 8 93736 12121 79 2	1·56338 72208 8481 01386 138 64360 6 52178 15640 170 1	1·14083 62068 ·10943 71055 296 83549 3 23502 11413 219 2	0·56589 22342 7985 85345 383 02987 6 92616 5661 160 3	1·5 ·07
1·25407 15074 51 71221 3970 1	1·64965 05943 99 82409 2058 1	1·25327 51807 131 31219 3973 1	0·64965 09114 99 76070 5226 1	1·57 ·00079 6
1·25458 90267 2126	1·65064 90412 4100	1·25458 86999 5394	0·65064 90412 4100	1·57079 6 03268
1·25458 92393	1·65064 94512	1·25458 92393	0·65064 94512	1·57079 63268

$$\Pi = 1{\cdot}57079\ 63268$$
$$\boxed{0}\,\Pi = 1{\cdot}25458\ 92393$$
$$\boxed{1}\,\Pi = 1{\cdot}65064\ 94512$$
$$\boxed{2}\,\Pi = 1{\cdot}25458\ 92393$$
$$\boxed{3}\,\Pi = 0{\cdot}65064\ 94512$$

And here it is to be observed that the equation $\boxed{0}\,\Pi - \boxed{2}\,\Pi = 0$ is (article 49, equation 12) necessarily accompanied by this other, $\boxed{1}\,\Pi - \boxed{3}\,\Pi = 1$.

If, in equations 1, 2, 3, 4 (article 45), we suppose t and u to be each equal to the above abscissæ Π, we obtain for the quaternary functions of 2 Π the formulæ

$$\begin{aligned}
\boxed{0}\,2\Pi &= 2\cdot\boxed{0}\,\Pi^2 + 2\cdot\boxed{1}\,\Pi\cdot\boxed{3}\,\Pi \\
\boxed{1}\,2\Pi &= 2\cdot\boxed{0}\,\Pi\cdot\{\boxed{1}\,\Pi + \boxed{3}\,\Pi\} \\
\boxed{2}\,2\Pi &= 2\cdot\boxed{0}\,\Pi^2 + \boxed{1}\,\Pi^2 + \boxed{3}\,\Pi^2 \\
\boxed{3}\,2\Pi &= 2\cdot\boxed{0}\,\Pi\cdot\{\boxed{1}\,\Pi + \boxed{3}\,\Pi\} \quad ;
\end{aligned}$$

which show that the values of $\boxed{1}\,2\Pi$ and $\boxed{3}\,2\Pi$ are alike; that is to say, the two curves $\boxed{1}$ and $\boxed{3}$ cross each other on the ordinate drawn through π, if the distance 0π be made double of 0Π .

It is also seen that the value of the difference

$$\boxed{0}\,2\pi - \boxed{2}\,2\pi \quad \text{is} \quad -\boxed{1}\,\pi^2 + 2\boxed{1}\,\pi\,.\,\boxed{3}\,\pi - \boxed{3}\,\pi^2\,,$$

that is

$$-\{\boxed{1}\,\pi - \boxed{3}\,\pi\}^2 \text{ or } -1\,;$$

so that the distance intercepted on the ordinate at 2Π, between the curves $\boxed{0}$ and $\boxed{2}$ is unit.

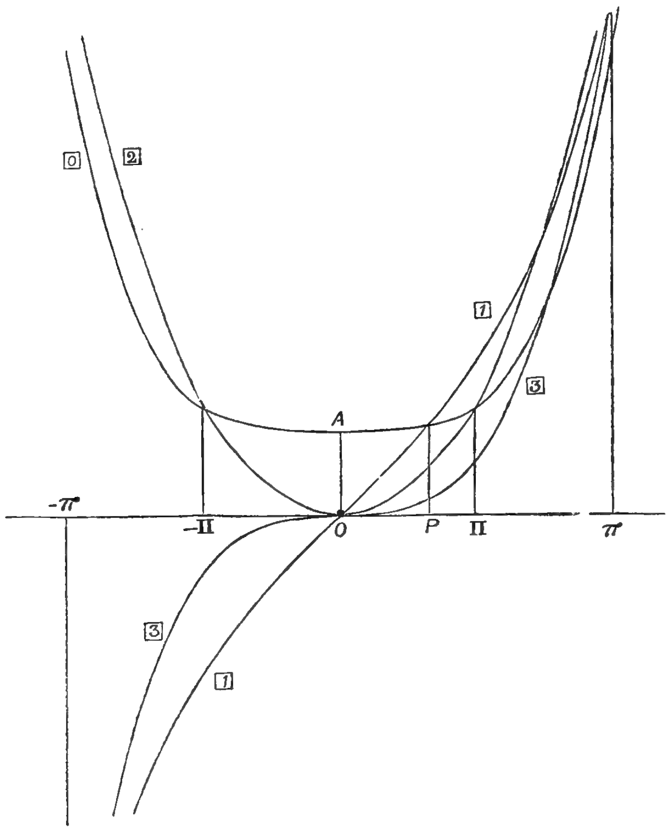


Fig. 11.

By following a train of reasoning analogous to that which was used for ternary functions, it may be shown that the intersection of the curves $\boxed{0}$ and $\boxed{2}$ are on ordinates corresponding to the abscissæ $(2n - 1)\,\Pi$, while those of $\boxed{1}$ and $\boxed{3}$ are on the alternate ordinates $2n\,\Pi$, n being any integer number taken either positive or negative; and also that on the ordinate which passes through the intersection of the one pair of lines, the distance intercepted between the other pair is the linear unit. Also, it may be proved that for the ordinates at $(n + \frac{1}{2})\Pi$ the intercepted distances are alike, each being represented by $\sqrt{\frac{1}{2}}$.

50. Leaving, for the present, the consideration of the four fundamental functions, we may give our attention to the differences $\boxed{0}\,t - \boxed{2}\,t$ and $\boxed{1}\,t - \boxed{3}\,t$, which we have already agreed to represent by the symbols cos t and sin t. By subtracting the value of $\boxed{2}\,(t+u)$ from that of $\boxed{0}\,(t+u)$, as given in article 45, we find

$$\boxed{0}\,(t+u) - \boxed{2}\,(t+u) = \{\boxed{0}\,t - \boxed{2}\,t\}\{\boxed{0}\,u - \boxed{2}\,u\} - \{\boxed{1}\,t - \boxed{3}\,t\}\{\boxed{1}\,u - \boxed{3}\,u\}$$

that is

$$\cos(t+u) = \cos t \,.\, \cos u - \sin t \,.\, \sin u\,,$$

and similarly we can obtain

$$\sin(t+u) = \sin t \,.\, \cos u + \cos t \,.\, \sin u\,,$$

so that the computation of the values of these compound functions may be made independently of those of the elementary functions, almost as we have already done for the binary functions sus t and cat t.

If, having laid off OA (fig. 12), equal to the linear unit, we make OC

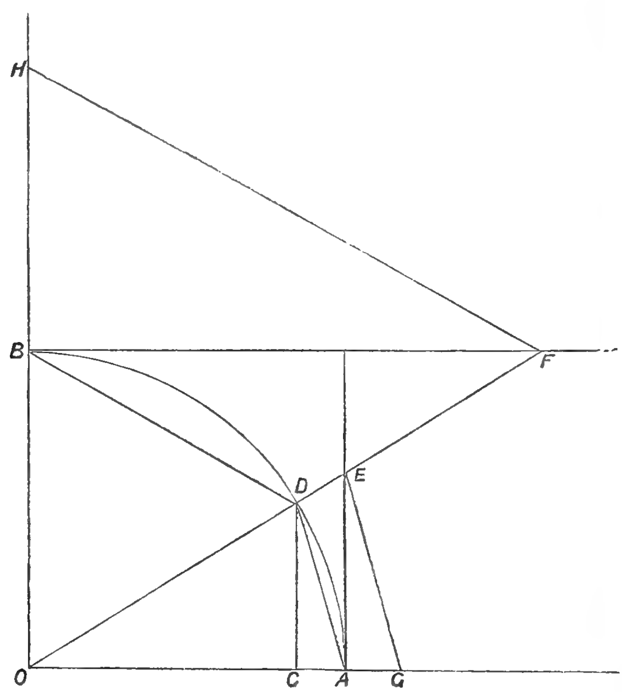


Fig. 12.

equal to the difference $\boxed{0}\,t - \boxed{2}\,t$, and set up the perpendicular CD equal to $\boxed{1}\,t - \boxed{3}\,t$; then we have $OC^2 + CD^2 = OA^2$, wherefore the point D must be in the circumference of a circle described from O as a centre with OA as a radius. Proceeding now as in article 19, let us suppose the point D to be carried to a small distance along the curve, then the increment of the sector AOD must be represented by $\frac{1}{2}\{OC\,.\,\delta CD - CD\,.\,\delta OC\}$, that is by $\frac{1}{2}\{(\boxed{0}\,t - \boxed{2}\,t)^2 + (\boxed{1}\,t - \boxed{3}\,t)^2\}\,\delta t$ or by $\frac{1}{2}\delta t$, and consequently the double of the area AOD must represent the primary variable t. Also, since the increment of the curve is, in all cases, $\sqrt{\{\delta CD^2 + \delta OC^2\}}$, and since $\delta CD = OC\,.\,\delta t$, $\delta OC = -CD\,.\,\delta t$, it follows that the increment of the curve is just δt, so that the arc AD stands for the primary t.

Now when t is made equal to the above value of Π, the function $\boxed{0}\,t - \boxed{2}\,t$ becomes zero, so that the point C is then at O; wherefore Π must represent the length of the quadrantal arc AB. The number π is therefore half of the well-known value $\pi = 3{\cdot}14159\,26536$ for the length of the semicircumference of a circle of which the radius is unit; and therefore, moreover, the functions $\boxed{0}\,t - \boxed{2}\,t$ and $\boxed{1}\,t - \boxed{3}\,t$, are in reality the cosine and sine of the arc t.

In this way we may imagine the whole doctrines of trigonometry as imported into our investigation, and as forming a scholium to the fourth case of the first problem in the calculus of primaries.

51. Having now arrived, in the regular course of our inquiry, at the measure of angular position, and at the circular functions, we may resume the consideration of the angular motion of the trigon ABC (fig. 4), which was left off in article 42.

If we denote the inclination of the base BC by θ, we have

$$\sin\theta = \frac{DE}{BC} = \frac{1}{2}\sqrt{3}\,.\,e^{\frac{1}{2}t}\left\{\triangle_1 t - \triangle_2 t\right\} \text{ whence}$$

$$\sin\theta^2 = \frac{3\triangle_1 t^2 - 6\triangle_1 t\,.\,\triangle_2 t + 3\triangle_2 t^2}{4\{\triangle_0 t^2 + \triangle_1 t^2 + \triangle_2 t^2 - \triangle_0 t\,.\,\triangle_1 t - \triangle_0\,.\,\triangle_2 t - \triangle_1 t\,.\,\triangle_2 t\}}$$

$$\cos\theta^2 = \frac{4\triangle_0 t^2 - 4\triangle_0 t\,.\,\triangle_1 t - 4\triangle_0 t\,.\,\triangle_2 t + \triangle_1 t^2 + 2\triangle_1 t\,.\,\triangle_2 t + \triangle_2 t^2}{4\triangle_0 t^2 - 4\triangle_0 t\,.\,\triangle_1 t - 4\triangle_0 t\,.\,\triangle_2 t + 4\triangle_1 t^2 - 4\triangle_1 t\,.\,\triangle_2 t + 4\triangle_2 t^2}$$

and consequently

$$\cos\theta = \frac{1}{2}e^{\frac{1}{2}t}\left\{2\triangle_0 t - \triangle_1 t - \triangle_2 t\right\}$$

Taking the differential of $\sin\theta$, we have

$$\cos\theta\,.\,\delta\theta = \frac{1}{4}\sqrt{3}\,.\,e^{\frac{1}{2}t}\left\{2\triangle_0 t - \triangle_1 t - \triangle_2 t\right\}\delta t$$

$$= \frac{1}{2}\sqrt{3}\,.\,\cos\theta\,.\,\delta t \qquad \text{wherefore}$$

$$\delta\theta = \frac{1}{2}\sqrt{3}\,.\,\delta t \quad \text{and} \quad \theta = t\sqrt{\frac{3}{4}}\ ;$$

and thus, in order that the base BC may make half a turn, we must have

$$\theta = \pi \quad \text{and} \quad 3\,\mathrm{T} = \sqrt{\tfrac{4}{3}} \cdot \pi \quad .$$

The value of 3T, found in article 37, is

$$3\mathrm{T} = 3{\cdot}62759\ 87283\ 68435\ 70118\ 81568$$

while

$$\pi = 3{\cdot}14159\ 26535\ 89793\ 23846\ 26434$$

and, on seeking the ratio of these by BROUNCKER's method of continued fractions, we find the quotients 1; 6, 2; 6, 2; 6, 2, the group 6, 2, being repeated ten times. It was this recurrence of the quotients, which I observed in 1850, that led me to seek for a rigid demonstration of this remarkable relation between the intervals of the intersections of the ternary curves, and those of the intersection of the quaternary ones.

52. The observation that $\sqrt{\tfrac{3}{4}}$ is the sine of 120°, while $-\tfrac{1}{2}$ is its cosine, led at once to the following unexpected generalisation.

Having assumed α any constant angle, let us put

$$\phi t = e^{t\,.\,\cos\alpha} \,.\, \sin(t\,.\sin\alpha) \ ,$$

then, by taking the successive derivatives, we obtain

$$_{1t}\phi t = e^{t\,.\,\cos\alpha} \,.\, \sin(\alpha + t\sin\alpha)$$

$$_{2t}\phi t = e^{t\,.\,\cos\alpha} \,.\, \sin(2\alpha + t\sin\alpha)$$

$$_{3t}\phi t = e^{t\,.\,\cos\alpha} \,.\, \sin(3\alpha + t\sin\alpha)$$

and in general

$$_{nt}\phi t = e^{t\,.\,\cos\alpha} \,.\, \sin(n\alpha + t\sin\alpha)$$

wherefore, if α be taken the nth part of the entire circumference, that is if $\alpha = \frac{2\pi}{n}$, the nth derivative of ϕt comes to be ϕt itself, and we have a series of recurring functions of the nth order. It is to be observed, however, that these are not the fundamental functions.

If we make $n = 1$, $\alpha = 2\pi$, $\cos\alpha = 1$, $\sin\alpha = 0$, ϕt becomes $e^{+t}\,.\sin(0t)$, which is quite useless. If $n = 2$, $\alpha = \pi$, $\cos\alpha = -1$, $\sin\alpha = 0$, and $\phi t = e^{-t}$ $.\sin(0t)$, which also is unavailable. But when we put $n = 3$, we have $\alpha = \tfrac{2}{3}\pi$ $= 120°$, $\cos\alpha = -\tfrac{1}{2}$, $\sin\alpha = \sqrt{\tfrac{3}{4}}$, and

$$\phi t = e^{-\frac{1}{2}t} \,.\, \sin(t\sqrt{\tfrac{3}{4}})$$

$$_{1t}\phi t = e^{-\frac{1}{2}t} \,.\, \sin(120° + t\sqrt{\tfrac{3}{4}})$$

$$_{2t}\phi = e^{-\frac{1}{2}t} \,.\, \sin(240° + t\sqrt{\tfrac{3}{4}})$$

And if we make $n = 4$, we have $a = \frac{\pi}{2}$, $\cos a = 0$, $\sin a = 1$, whence

$$\phi t = e^{ot} \sin t \qquad \text{or} \qquad \phi t = \sin t \quad .$$

These results are remarkable in this respect, that for binary functions they give the multiplier e^{-t}, which agrees with the difference between sus t and cat t; for ternary functions $e^{-\frac{1}{2}t}$, which corresponds with the diminution of the side BC of the equilateral trigon; and for quaternary functions e^{o}, in accordance with the fact that the intervals intercepted between the two curves $\boxed{0}$ and $\boxed{2}$ are the same for t as for $n\pi \pm t$, whatever may be the value of the integer number n.

The functions obtained from the formula $\phi t = e^{t \,.\, \cos a} \,.\, \sin\,(t \,.\, \sin a)$ are not fundamental, but compound functions, and do not indicate a general solution of the equation $_{nt}x = x$. Thus the absolutely general solution of $x = {}_{3t}x$ is

$$x = \mathrm{A}\,\triangle_0 t + \mathrm{B}\,\triangle_1 t + \mathrm{C}\,\triangle_2 t$$

in which A, B, C, may be any coefficients, positive or negative; but this generality could not be obtained from the above three functions, ϕt, $_1\phi t$, $_2\phi t$.

In order to render this matter, which is of importance in physical investigations, quite clear, we may express the above ternary functions in terms of the fundamental ones, and, contrariwise, seek to deduce the fundamental functions from them. For this purpose we shall suppose that the first of them is the x of the preceding equation; that is,

$$e^{-\frac{1}{2}t} \,.\, \sin\left(t\sqrt{\tfrac{3}{4}}\right) = \mathrm{A}\,\triangle_0 t + \mathrm{B}\,\triangle_1 t + \mathrm{C}\,\triangle_2 t \;,$$

$$e^{-\frac{1}{2}t} \,.\, \sin\left(120^\circ + t\sqrt{\tfrac{3}{4}}\right) = \mathrm{B}\,\triangle_0 t + \mathrm{C}\,\triangle_1 t + \mathrm{A}\,\triangle_2 t \;,$$

$$e^{-\frac{1}{2}t} \,.\, \sin\left(240^\circ + t\sqrt{\tfrac{3}{4}}\right) = \mathrm{C}\,\triangle_0 t + \mathrm{A}\,\triangle_1 t + \mathrm{B}\,\triangle_2 t \;.$$

Giving in these formula to t the value zero, we obtain

$$\mathrm{A} = 0 \;, \qquad \mathrm{B} = \sin 120^\circ = \sqrt{\tfrac{3}{4}} \;; \quad \mathrm{C} = \sin 240^\circ = -\sqrt{\tfrac{3}{4}} \;;$$

whence the three values

$$\phi t = e^{-\frac{1}{2}t} \,.\, \sin\left(t\sqrt{\tfrac{3}{4}}\right) = \sqrt{\tfrac{3}{4}}\left\{\triangle_1 t - \triangle_2 t\right\}$$

$$_1\phi t = e^{-\frac{1}{2}t} \,.\, \sin\left(120^\circ + t\sqrt{\tfrac{3}{4}}\right) = \sqrt{\tfrac{3}{4}}\left\{\triangle_0 t - \triangle_1 t\right\}$$

$$_2\phi t = e^{-\frac{1}{2}t} \,.\, \sin\left(240^\circ + t\sqrt{\tfrac{3}{4}}\right) = \sqrt{\tfrac{3}{4}}\left\{\triangle_2 t - \triangle_0 t\right\}$$

which give ϕt, $_1\phi t$, $_2\phi t$, in terms of the fundamental ternary functions. But if we seek the values of $\triangle_0 t$, $\triangle_1 t$, $\triangle_2 t$, we are met by the difficulty that the sum of the three expressions is zero, and that virtually we have only two equations whereby to determine three unknown quantities. We are thus forced to bring in the condition

$$\triangle_0 t + \triangle_1 t + \triangle_2 t = e^t \ ,$$

by help of which we obtain the rather complex values

$$\triangle_0 t = \frac{1}{3}e^t + \frac{1}{3}e^{-\frac{1}{2}t}\left\{ 2\cos\left(t\sqrt{\tfrac{3}{4}}\right) \right\} ,$$

$$\triangle_1 t = \frac{1}{3}e^t + \frac{1}{3}e^{-\frac{1}{2}t}\left\{ -\cos\left(t\sqrt{\tfrac{3}{4}}\right) - \sqrt{3}\,.\,\sin\left(t\sqrt{\tfrac{3}{4}}\right) \right\} ,$$

$$\triangle_2 t = \frac{1}{3}e^t + \frac{1}{3}e^{-\frac{1}{2}t}\left\{ -\cos\left(t\sqrt{\tfrac{3}{4}}\right) - \sqrt{3}\,.\,\sin\left(t\sqrt{\tfrac{3}{4}}\right) \right\} .$$

53. To return from this digression to the subject of quaternary functions, we observe that

$$\operatorname{sus} t = \boxed{0}\,t + \boxed{2}\,t \ , \quad \operatorname{cat} t = \boxed{1}\,t + \boxed{3}\,t$$

$$\cos t = \boxed{0}\,t - \boxed{2}\,t \ , \quad \sin t = \boxed{1}\,t - \boxed{3} \ ,$$

wherefore

$$\boxed{0}\,t = \frac{1}{2}\left\{ \operatorname{sus} t + \cos t \right\} ,$$

$$\boxed{1}\,t = \frac{1}{2}\left\{ \operatorname{cat} t + \sin t \right\} ,$$

$$\boxed{2}\,t = \frac{1}{2}\left\{ \operatorname{sus} t - \cos t \right\} ,$$

$$\boxed{3}\,t = \frac{1}{2}\left\{ \operatorname{cat} t - \sin t \right\} .$$

Now, all recurring functions of the fourth order are expressed by the general formula

$$\mathrm{A}\boxed{0}\,t + \mathrm{B}\boxed{1}\,t + \mathrm{C}\boxed{2}\,t + \mathrm{D}\boxed{3}\,t$$

in which A, B, C, D are any numerical coefficients; and, consequently, they may also be expressed by

$$a \operatorname{sus} t + b\cos t + c \operatorname{cat} t + d\sin t \ ,$$

so that the theory of quaternary recurring functions becomes a compound of the well-known doctrines of Trigonometry with the analogous and complementary doctrines of the catenarian, or, as some may prefer to call them, the hyperbolic functions. It would be easy to multiply formulæ connected with these functions, many of them interesting, on account of their relations to other researches; but as my present object is only to indicate the general features of the inquiry, I shall leave these, and proceed to apply the quaternary functions to the solution of a problem in Mechanics, which has resisted all the powers of the integral calculus.

XXXVII.—*On the Application of the Principle of Relative, or Proportional, Equality to International Organisation.* By Professor Lorimer.

(Read 18th March 1867.)

Aristotle has a saying, which he has frequently repeated and which is often quoted, to the effect that the same degree of precision is not attainable in all branches of inquiry, and that it would be just as absurd to exact demonstration from a politician or an orator, as to accept probable reasoning from a mathematician. It is a saying full of truth and acuteness. To the cultivators of ethical and political philosophy, for whom it was intended, it is invaluable both as an encouragement and a warning; and yet, in behalf of the latter more especially, I often wish that it had never been said. Proceeding from such a master, I am persuaded that it has often tempted them to rest satisfied with a degree of success far short of the limits which the nature of their subjects really imposed; whilst, on the other hand, it has afforded an apology for excluding social and political philosophy from the meditations of learned bodies like this. I do not mean that they have been formally excluded. I know that the constitution of this, and of most similar societies, has always embraced the social as well as the physical sciences. But so rarely have those of us who were occupied with the former availed ourselves of the privileges of Fellowship, that it has come to be regarded almost as a matter of admission on our part, that our subjects defy scientific treatment: that when we talk of tracing out laws of social wellbeing or progress, we use words which either have no meaning at all, or which indicate a very faint analogy between the methods which we affect to follow and those really employed in the physical sciences: and that pretty nearly all that can be done is to hand us and our subjects over to the companionship of party politicians and popular declaimers.

It is not surprising that this view should prevail, especially amongst those whose notions of the necessity of scientific precision, in other departments of study, are the strictest. It is rare to find a mathematician, or an astronomer, who does not despise politics; and I myself sympathise with their feelings to so great an extent, that it will not diminish the reverence with which I have been accustomed to regard them, nor shall I affect to view it as a mark of inhospitality, though some of the very ablest of those who listen to me should expect me to apologise for the subject which I am about to introduce to their notice this evening.

I am, however, so deeply impressed with the momentous character of the interests which centre in the social sciences, when rightly understood, that I cannot admit the propriety of prefacing with an apology any attempt to advance them, however humble. They are the sciences of civilisation, *in the first instance*—the sciences on the more or less successful cultivation and application of which the physical sciences, which constitute so prominent a feature in the civilisation of our day, ultimately depend; for the physiologist or the entomologist, I fancy, would find himself just as much from home amongst a horde of savages as a political philosopher; and a botanist or a geologist would have very little chance of pursuing his studies in peace, even in an old society that had fallen into anarchy, or was a prey to chronic revolution.

But if the cultivators of physical science, in place of asking the cultivators of social science to apologise for their subject, were to ask them to apologise for themselves,—not for the studies which they pursue, but for the manner in which they pursue them,—the request would be difficult to put aside, however bitter might be the sarcasm which it implied. And of all the consequences of that despair of precision, to which ARISTOTLE has been but too successful in reconciling us, there is none which has brought greater reproach on our science than the want of any proper criterion of truth or falsehood. From the inability of its professors to distinguish between the difficult and the impossible,—between schemes which ought never to be relinquished, and schemes which ought never to have been entertained,—the faith of the thoughtful has been shaken, and in the thoughtless practical world without, whilst men have wasted their energies and shed their blood in wrestling with problems that were permanently insoluble, they have tamely abandoned others of the gravest import which presented no difficulties that were necessarily insuperable.

I am strongly persuaded that this reproach would never have arisen, or at all events would not have been merited, had we habituated ourselves and others to regard our subject as a *science*, in the ordinary sense of a *systematic inquiry into nature;* and not as a series of random observations, in which the contingent and the necessary, the permanent and the accidental, were hopelessly and inextricably mixed up, and from which any conclusion, or no conclusion, might equally have been deduced. Had a more absolute point of view been occupied and steadily maintained, and a severer method been rigidly adhered to, we should, long ere now, have got hold of canons of criticism which would have enabled us to judge of the merits both of existing institutions and legislative schemes, with a degree of confidence which no vague estimate of their supposed utility, past or prospective, could possibly warrant.

It is quite true that in political problems, as they present themselves in the concrete, the contingent element is so large as to prevent us from almost ever arriving at anything beyond a probable solution. The historical method, when applied exclusively, is inadequate, because the past, even if our knowledge of it were complete, does not exhaust the present, still less the future; and when we resort to the philosophical method, we speedily become aware, that however cautious may be our application of it, fifty, or five hundred accidents may occur, which will so impede the action of necessary causes, as to render them, in a given place or for a given time, wholly inoperative. But it is a mistake to suppose that social differ altogether from physical problems in the presence of these disturbing elements, or that the separation of the accidental from the necessary is impossible in the one case more than in the other. We say, for example, that a wall will not stand if it is cracked, or that a body will fall if a vertical line through its centre of gravity falls without its base. But there are many cracked walls in this city that are very old; and the hanging tower of Pisa has stood for more than six centuries. It is nearly as old as the British Constitution, and, judging by present appearances, is, I fear, very likely to survive it. Such exceptions, of course, invalidate the rule only to the extent of showing, that cracked walls, or hanging walls, *may* stand for centuries. They reduce the presumption that such walls will tumble down within a given period to a probability; but they do not prevent us from distinguishing between the principles of physics, which *ultimately* condemn them, and the physical accidents which hold them up for a time. In the same way, though a social institution violates a principle which it cannot abrogate, or ignores a fact which it cannot alter, we must not on that account pronounce its temporary realisation to be impossible, or predict its immediate miscarriage. But, so far from believing in its permanent stability, if we know that the accidents on which it leans are transitory, and that the laws which it violates are unchangeable, if it does not right itself scientifically we may predict its practical downfall, with a confidence bordering very closely on certainty. On the other hand, if a projected institution cannot be shown to violate any such principle, or to assume as facts of nature what are not facts of nature, then there is scarcely any amount of past failure, or present difficulty, which will entitle us to exclude it from the category of attainable objects. The failures, for anything that appears, may have been accidents; and if we venture to condemn it on the strength of them, or to apply to it any of those epithets behind which ignorance and mental indolence are so eager to take shelter, we run the risk of encountering the ridicule which its ultimate success will not fail to bring down on us.

Political Methodology, viewed as a branch of applied logic, has risen in the hands of some of its recent cultivators almost to the dignity of a separate science. By eliminating impossible schemes, and thus circumscribing the sphere of political effort, it has already given evidence of its practical value for the generations that are to follow us, if not for that to which we belong. Within the State forms of government, after which the vulgar still aspire as the ideal forms of society, have been shown by its means to be permanently irreconcileable with order, and if with order, then with liberty, which is possible only through order, and so ultimately with civilisation itself. I would gladly see the efforts of this Society directed to rendering political method more extensively available in this direction. The abiding problem of national politics I believe to be, the establishment not of the perfect State, but of *a perfect harmony between the State and the society of which it professes to be the expression;* and this problem I by no means regard as insoluble, if it were rationally and honestly dealt with. But such considerations as these would involve us in discussions which, in present circumstances, it might be difficult for us to conduct in the abstract and totally dispassionate spirit which ought always to characterise the labours of those who seek after absolute truth; and all that I shall attempt, in the meantime, is to point out to you two principles the realisation of which I believe to be impossible, and which have, nevertheless, been sought to be realised, in conjunction with most schemes of national, and I think with every scheme of international organisation which has as yet been propounded. That the latter class of schemes, from whatever cause, have miscarried in point of fact, none of you, I suppose, will have any disposition to deny; and the second part of my task will, consequently, consist of an inquiry whether, by the abandonment of the principles in question, and the substitution of their opposites, we may not hope to advance somewhat nearer to the solution of what is proclaimed on all hands to be the central problem of international jurisprudence, the establishment, viz., of a self-supporting and self-vindicating international legislature and executive.

1. The first of these principles is *finality*. In national politics this principle is exhibited in those arbitrary, and, in some States, impassible lines between classes, which science has long ago condemned, and which practical men are now everywhere engaged in obliterating. In schemes of international organisation, this principle has sought to manifest itself in the establishment of final and permanent international relations, or in the maintenance of what is technically called a *status quo*.

2. The second principle is *absolute equality of rights and obligations*. In internal politics, this principle is the basis of the form of government called Democracy. In external politics, it has exhibited itself in the custom of assigning equal votes to all the members of the family of nations not absolutely excluded from the Council Board, however widely they may differ in real power and importance.

In order that you may trace the action of these principles in international politics, I must beg you to permit me two or three sentences of historical retrospect.

It is now somewhat more than two centuries since the old dream of a Universal Empire, divinely instituted, and divinely upheld—the dream of Dante and the mediæval publicists—was abandoned, and men began to speculate on the possibility of substituting for it an European Confederation which should be self-governing and self-supporting. The doctrine of the Balance of Power, it is true, was by no means new at the Peace of Westphalia; still less at the Treaty of Utrecht, when the name came into use. But many circumstances in the then condition of Europe lent to it an importance which it had not formerly possessed; whilst, by the institution of Permanent Embassies, which may be roughly ascribed to the same period, it was hoped that it might be worked out in practice in such a manner as to render every State that was admitted into the family of nations substantially responsible for the existence and independence of every other.

For our present purposes, then, we may assume that for two centuries, more or less, men have been striving after external organisation; and we need go no farther than the events of the last few years to convince ourselves that they have striven in vain; for never was there a period in the history of civilisation when the mutual obligations of independent communities were less recognised and acted on than at the present time. The common empire of the middle ages, never realised it is true, but never abandoned—the common church, realised beyond most human conceptions—the common language and literature which bound together the cultivated classes,—all these have been swept away, and have found, as yet, no substitutes. Dissimilar in their creeds and their institutions, their blood and their speech, the different nations of Europe, now thoroughly severed in all but their material interests, far from cherishing the sympathies of a common citizenship, scarcely exhibit those of common humanity. Armed to the teeth with the most ingenious weapons of destruction—weapons which in the end can avail only to the strongest—the full publicity and the rapid transmission of intelligence, from which so many humanising results were anticipated, seem as yet to have served scarcely any purpose but to enable rival nations to watch each other with ever-growing feelings of jealousy and distrust.

With such an experience of the fruitlessness of past effort it is not wonderful if, at times, we feel tempted to abandon all attempts at international organisation and mutual aid, and vaguely to hope that whilst separate States maintain the most absolute political independence, those indefinite influences to which we give the names of civilised opinion, moral pressure, and the like, may play amongst independent nations a part with which the most highly cultivated and most Christian communities would be very sorry to entrust them within their own borders. Yet within the State, these influences operate more potently than without it, because citizens know what fellow-citizens mean. If within the State, then, these influences become efficacious only by perfecting organisation, and thus asserting the dominion of order more unequivocally and emphatically, what reasonable hope can we entertain, that, in the great world without they will become self-acting and supply, the place of order altogether? To invoke them for such a purpose, is surely little better than to hide from ourselves, by a cloud of words, a despair to which these very words bear witness the moment that we attempt to bring them in contact with reality, or even to fix them down to a definite sense.

But is this despair of external organisation justified by the amount of experience which these fruitless efforts to realise it as yet afford? Have we seen and done enough to warrant us in handing it over finally to the limbo of unattainable aspirations? or are there not rather points of view in which, whilst no obstacle that is insuperable in point of principle meets us, we ought to take courage from the very magnitude and difficulty of the task?

Compared with the events that make up the history of individual communities, cosmopolitan phenomena manifest themselves very slowly—so slowly, indeed, as to resemble the geological changes in the structure of the earth, rather than the mechanical changes which the works of man effect on its surface. The fact is one which all nations recognise in their ordinary speech, for we measure the progress of nations by years, or, at most, by centuries, whilst we distribute the history of mankind, on a wider scale, into Eras. But the tardiness of these greater social operations is a fact, the bearing of which on our present subject is very little regarded. We are startled, perhaps, when it occurs to us that it is only about sixty years since the Holy Roman Empire ceased to be, in name, the central institution of our own Europe. Sixty years is within the memory of man; and we should certainly look for some traces, in our present condition, of the influences of an Universal Monarchy by divine right, which had existed so recently. But when we are told that, for all practical purposes, the Holy Roman Empire perished two, or perhaps three centuries earlier—that it received its death-blow at the Reformation, and finally expired during the Thirty Years' War, leaving nothing but its shadow on the earth—we thoughtlessly hand it over to a previous stage of political existence, almost as if the sphere of its action had been on another planet. But what are two or three centuries in the history of the world? Compare them with the age of that very Empire, whether we take it from the battle of Pharsalia, or from the foundation of the Frankish Monarchy! Or, again, the life of any single Greek State was comparatively short. Sparta was regarded as a wonder of old age; and Sparta lasted, I think, only some 700 years. But if we take the Era of Greek influence, or even of the preponderance of Greek institutions, we must begin before Homer and come down to Roman times.

It is the same if we take the periods when the hegemony of the historical world was in Shemitic, and Egyptian hands.

Now if, in place of measuring the period during which the modern world has been attempting to shape itself anew, by the brief periods required for the growth and decay of national institutions, we compare it to those in which organisations of a world-wide character have been developed, we shall see reason to pause before we pronounce a confident opinion on the possibility or impossibility of so vastan enterprise. Even if men's efforts had been well directed, their failure during two centuries of reaction against mediæval influences would not warrant their abandonment on the ground of mere lapse of time.

But have they been well directed? The history of this period, if I am not mistaken, is pregnant with lessons of warning not less emphatic than the lesson of encouragement which we derive from its comparative brevity. It tells us,—and I think the teaching of all history is to the same effect,—that the power and importance of separate communities, not only absolutely, but, what is far more important for our purpose, relatively to each other, have been continually changing, and, consequently, that what we ought to have provided for was the organisation, not of a stable body all the members of which possessed and retained definite and specific functions, but of a body in perpetual flux the members of which were changing and would continue to change their functions, their rights, and their responsibilities, relatively to each other and to the whole. But when we look into what has been really done or attempted, whether by statesmen and diplomatists for the establishment and maintenance of the Balance of Power, or by speculative politicians in their schemes for the creation of a European Confederation, or their aspirations after a Perpetual Peace, we find that the effort has invariably been to fit a final and unchangeable system to the requirements of a society which was anything but final. The very same error which dictated the search after the Perfect, or Ideal State, was thus repeated in the sphere of external politics.

To substantiate this statement by a satisfactory criticism of all, or even of any one of these schemes, would lead me beyond the limits which your time assigns to me. Those which received the sanction of diplomacy are embodied in the treaties which have followed all our great wars, and belong to general history; and I shall probably recall the general character of the other class sufficiently to your recollection when I mention the well-known names of their authors, St Pierre, Rousseau, Kant, Bentham, Cobden, and of one, the latter phases of whose much-contested policy seem to combine the practical sagacity of the statesman with the dispassionate thoughtfulness of the philosopher—I mean the Emperor Napoleon III. Should you find leisure to re-examine these various projects I believe you will find that, without a single exception, they have proposed, not only to reconstruct the map of Europe, but, when so reconstructed, to stereotype it and to guarantee, or attempt to guarantee, its permanence.

Now, it is obvious that such an interference with the natural course of events, with the ebb and flow of human fortunes, inasmuch as it assumed the possibility of controlling the strong, could have been effected only by an amount of unanimity on the part of the weak which was very unlikely to be permanent. Our utmost confidence in the doctrine of the Balance of Power could barely bring such an occurrence within the reach of possibility. But supposing it possible that the territorial divisions of 1648 or of 1713 should have been preserved to our day, would their preservation have been just? Social existence of which political organisation without the State, as within the state, must strive to become the expression, is very far from having culminated in any direction. Not only internal development but even external aggrandisement thus frequently result from causes which are not only blameless, but in the highest degree commendable. Take, for example, a process which is constantly going on amongst States of kindred blood. The more progressive community constantly absorbs the less progressive, not by physical, but by moral and intellectual conquest. Of this we have an example in the action of Germany on Denmark, and indeed, I believe, on the whole of Scandinavia. Whatever may be the real boundary line, *for the present*, between Danes and Germans, I suppose there is not the least doubt that that line is gradually shifting *northwards;* or, in other words, that Danes are voluntarily becoming Germans, and not Germans Danes. The cause is simply the greater attractive power of the more numerous and more active body. Now, this change is one made, not by States or governments at all, but by private individuals, in the exercise of their private rights. Berlin and Vienna are more tempting fields of enterprise than Copenhagen and Stockholm: the literature of Germany is cosmopolitan, that of Scandinavia is local; and ambitious Scandinavian parents educate their sons—and ambitious Scandinavian youths educate themselves—not for a Scandinavian, but a German career. What I have said of Germany and Denmark was alleged of France and Savoy; and the real justification of the annexation was, not the pretended plebiscite, but the fact, if fact it was, that the political absorption was only a formal recognition of a moral absorption which had already deprived the lesser country of all the characteristics of a separate state. Many Frenchmen will tell you that a similar process of moral amalgamation is going on between France and Belgium. From what I remember of Savoy twenty-five years ago, I should think the allegation, as regarded it, was not altogether destitute of truth. As regards Belgium I shall offer no opinion; but this I will say, that if such facts ever do become *faits accomplis*, the sooner they become *faits de droit* the better.

In the effort, then, to struggle against inevitable change, and what at any rate *may* be legitimate progress, in the systems in question, we have got hold of the principle of *finality*, the first of the false principles which I indicated, and the presence of which alone would, in my opinion, be sufficient to account for two centuries of failure. Before we proceed to consider the possibility of its elimination from future schemes, let us turn to the other.

The *second* principle, you will remember, was *the absolute equality of all recognised States, great and small.*

As to the fact of the presence of this principle in the schemes in question, I must again presume on your historical and political knowledge. It was not the principle which guided the negotiators at Münster in the reconstruction of the old Empire, the German Empire, as it had come to be called. The Empire, or rather the Confederation which *they* constituted, recognised and gave effect to the relative importance of the various states, by means of a complicated, but not on that account an inefficient mechanism. The good as well as the evil of feudality still clung to it, as it did to the society to which it professed to correspond. The earlier schemes of general European organisation were modelled on that of this central body, and, consequently, they exhibit the principle of equality less conspicuously than the later ones. But absolute citizen equality was the principle which the American, and, above all, the French Revolution, brought into prominence, and sought to substitute for the arbitrary and impassable barriers between class and class into which feudalism had degenerated, and which constituted the false element of finality in national organisation, social and political. As international politics came under the influence of these events, this false principle exhibited itself more and more. Since the Congress of Vienna the tendency has been to temper equality only by exclusion; and either to limit the seats at the European council board to the five great powers—the so-called "Pentarchy"—amongst whom absolute equality was the rule, or, if the lesser powers were admitted, to admit them, nominally at least, on the same footing.

But *is* the principle of absolute equality of rights and obligations between States that are unequal in importance really false in theory and unrealisable in practice? We are willing, you will say, to admit the absurdity of attempting to stereotype the map of Europe; but as to the possibility, or propriety, of recognising absolute political equality, whether within the State or without, there is, at any rate, much diversity of opinion.

Now this diversity of opinion—great as it is, and terrible as have been, and I fear may yet be, its effects—is traceable, if I am not greatly mistaken, to a defect in the popular mind, on which ARISTOTLE, with his usual perspicacity, had put his finger more than 2000 years ago. "The vulgar," he says, "do not distinguish." And in this modern Europe of ours, for nearly a century now, they have lost sight of a distinction which ARISTOTLE did them the farther favour to point out to them. The distinction to which I refer is that between *absolute*, and *relative* or *proportional*, *equality*.

The two are, in truth, neither more nor less than two different manifestations of the principle of justice. They differ not in themselves, but in the manner of their application, and in the subject-matter with which they deal.

Following, and giving definiteness, as usual, to PLATO's conception of what, in its origin, was probably the teaching of SOCRATES on the subject, ARISTOTLE gave to these two forms of applied justice the names of the διορθωτικὸν δίκαιον and the διανεμητικὸν δίκαιον,—names which the schoolmen and jurists rendered, whether happily or not I shall not stop to inquire, by *justicia correctiva* or *commutativa*, and *justicia distributiva.*

The object of *diorthotic* or corrective justice, ARISTOTLE explained to be to give to each a perfectly fair, unbiassed, and, in this sense, *equal* opportunity of vindicating whatever might be due to him, whether the amount might be greater or smaller than that which was due to his neighbour. This was what we call Equality before the Law;" and justice demanded that equality, *in this sense*, should be *absolute.* There was to be no distinction whatever of rich or poor, of male or female, of old or young, of wise or foolish.

The object of *dianemetic* justice, on the other hand, was to ascertain *how much* was due to each, and to *rank* them accordingly. Here was still equality—*perfect* equality—but it was equality which was no longer absolute, but relative; it was *proportioned* to the facts which the claimants respectively established, with reference to the goods which they had acquired or inherited, or the powers and faculties which God had given them, and which their own efforts and the circumstances of their lives had developed. So far all is clear. There can be no doubt that this was what ARISTOTLE meant, and as little doubt, I think, that he was right.*

As to the application of the doctrine there is great confusion in the text, as we possess it now, and even Sir ALEXANDER GRANT has not made much of it. What it seems to indicate is, that diorthotic justice, or absolute equality, is applicable to private, and dianemetic justice, or relative equality, to public questions. So read, it excludes democracy, which rests on diorthotic, and ignores dianemetic justice altogether from the category of governments that are realisable in accordance with justice; and as this is known to have been ARISTOTLE's opinion, it is an interpretation which has satisfied most of his commentators, and I confess that it satisfied me for many years. But I am persuaded now that this could not have been ARISTOTLE's meaning; and if the text really amounts to this, it must be in consequence of some blunder or other, which may very possibly have originated with EUDEMUS. There cannot, I think, be the least doubt that *both* principles come into play in every department of jurisprudence, and are called into action in the decision of every case, from the most insignificant question of private right to the most momentous question of international policy. And the method of their action is this: the first principle, that of absolute equality, governs the

* The best minds of the middle ages preserved a perfectly clear conception of the *proportio.* "Jus," says DANTE, "est realis et personalis hominis ad hominem proportio, quæ servata servat societatem, corrupta corrumpit;" and THOMAS AQUINAS, "Materia justiciæ est exterior operatio, secundum quod ipsa, vel res qua per eam utimur, debitam proportionem habet ad alteram personam; et ideo medium justiciæ consistet in quadam proportionis æqualitate rei exterioris ad personam exteriorem," RÖDER's Natur-recht, vol. i. p. 115. It would be interesting to inquire when the vulgar conception of equality assumed the aspect of a speculative doctrine. It certainly is older than HOBBES, and is traceable as far back, at anyrate, as to the attempts of the Jesuits to found a Theocracy by levelling down all secular distinctions before the Church.

conduct of the suit, or of the investigation, whatever form it may take, and whether it be conducted for judicial or legislative purposes; the second, relative equality, governs the decision of the cause, whether that decision be pronounced in a Small Debt Court or in a Congress of Nations.

As an illustration of the mutual action of these principles in private law, take the familiar case of the distribution of a bankrupt-estate. One man has invested L.5, and another has invested L.50, in the concern. *As suitors*, the law puts them on a footing of *absolute* equality. No preference is given to Jew or to Gentile, to noble or to simple, to white skin or to black skin. So far they are dealt with diorthotically. But then the dianemetic or distributive principle comes into play; and, supposing the estate to yield 1s. per L., the one man would get 5s., and the other would get 50s. The *distribution* has reference to the objects of the suit, not to the suitors, and is wholly dianemetic. But so far is the dianemetic principle from acting alone, that it is in virtue of the diorthotic principle that it assigns 50s. to a man who may possibly be a millionaire and a scoundrel, and 5s. to a man who may be a pauper and a saint.

And just in the same way, the presence of both principles is indispensable to the decision of questions of a public nature. There, too, justice demands that the dianemetic principle shall act diorthotically. The action of the diorthotic principle in public is less obvious than in private law, because the State, in a proximate sense, is the source of the rights which it recognises; and in this sense its whole function seems to consist in *distributing*, and not in recognising rights. Still, even the State distributes, *or ought to distribute*, on a principle—on what, in the absolute sense at least, must be regarded as a foregone conclusion; and the recognition and fair application of this principle rests on, and implies the action, not of dianemetic or distributive, but of diorthotic or corrective justice. Suppose that the suffrage is claimed by a particular class of persons whose right to it has hitherto been ignored or denied. What they ask the State to do is, not to make new rights in their favour, but to recognise rights which they allege exist in their persons already. Their plea is that they are *entitled* to the suffrage on some existing *ground*, as they call it—property, education, a hearth and a chimney, or simple humanity. Whatever the ground may be, they demand that it shall be diorthotically recognised; but there the diorthotic principle stops. They don't ask the State to *give* them means on which they may ground their rights —for this would be to ask, not for *recognition*, but for *revolution;* and the same would be the case were they to ask the State to make them equal in all, or in any of these respects. The utmost limits to which the doctrines of the positive school of jurisprudence can be carried, with safety to the rights of private property, are *the recognition of the right of every man to the conditions of self-help.* Life is God's gift, and life involves freedom, and freedom involves the external conditions on which its exercise depends,—support to the impotent, instruction to the ignorant, constraint to the criminal. So far, I think, we may go, even where these conditions are dependent on the active interposition of our fellow-men. But equality is neither a consequence of life, nor a condition of freedom. It may or may not result from free energising; and it is only where it is present as a fact that it comes within the scope of a rational representative system. It was ROUSSEAU'S doctrine, that all men *ought* to be equal, and not HOBBES' assumption, that all men *are* equal, and ought *consequently* to be *recognised* as such, which brought about the Revolutions of last century, and which threatens us still. But this claim to be heard in vindication of rights which are alleged to exist, is neither more nor less than a claim to equality before the law—a claim which the constitution of this country recognises in the freedom of the press, the right to petition Parliament, to hold meetings, to form associations, and in many other ways quite as much as in the right to sue. With reference to such a claim the State, in its legislative capacity, holds, *mutatis mutandis*, precisely the position which the judge holds when the claims of two private parties are urged before him. And, in a still wider sphere, the existing States that form the commonwealth of nations assume the same position with reference to every new claimant for what, in international law, is technically known by the very significant and appropriate term of *recognition*.

If justice is to be done, then, if the principles of the science of jurisprudence—of a science which, in its minutest application, is an interpretation and an exposition of nature, are to be followed in any department whatever, there must be *proportion between the claimants* and *the things granted to them* (οἷς καὶ ἐν οἷς), in order that those who are unequal may not have equal things (μὴ ἶσοι ὀυκ ἶσα ἕξουσι), or the reverse. It is far stranger, and far more sad, than that ARISTOTLE should have failed to recognise the operation of the distributive principle in private law (if he did fail to recognise it), that the Christian world of Europe should for nearly a century have practically banished it from public law, in which ARISTOTLE held that it reigns supreme.

Such, then, being the two false principles which, since the Peace of Westphalia in 1648, have vitiated our schemes of international organisation, can we get rid of them for the future? Can we shadow forth a European or Cosmopolitan Constitution, self-sustaining and self-vindicating, which shall make provision for legitimate progress and righteous development, and for inevitable retrogression, whilst it takes cognizance of existing diversities of power?

To anything approaching to a Confederation, in the stricter sense of a single Composite State, there is, I think, the objection which exists to all Confederations, and of which we have just seen the consequences so terribly exhibited, first in America, and then in Germany. In a Confederation there are always two forces at work, a centrifugal force and a centripetal force—the tendency of the first of which is to pull it to pieces, and the tendency of the second of which is to centralise it, till it becomes a homogeneous State. A perfect and permanent balance between these forces I believe to be a practical impossibility; and for this reason, I regard all Confederations as transitional forms of government. Where absolute union must not be aimed at, even as an ultimate object, as is the case in the present instance, some modification of Federal organisation is, of course, inevitable. But the looser the bond, the less, I believe, will be the danger of its rupture; and I consequently concur in the latest opinion of KANT, whose great mind was much occupied with this great subject before it experienced the eclipse which darkened his last days—an opinion in which he was partially anticipated by GROTIUS—to the effect that it is to the creation, not of a Confederation, in any sense of the word with which we are as yet familiar, but of a Permanent Congress of Nations, or International Parliament, that we must direct our endeavours.

Such a Congress, I think, would obviate the errors I have indicated, and satisfy the great desideratum of a self-vindicating International Legislature and Executive, if it were constituted in accordance with something like the following scheme:—

1. That its meetings should be annual, taking place in the autumn between the Sessions of the various National Assemblies; and that the places of meeting should be Belgium and Switzerland alternately, or one of the Swiss Cantons, say Geneva, set apart as neutral European ground.

2. That each State should be represented by two deputies, both of whom should be present at the meetings of the Congress, but one of whom only should be entitled to speak and to vote.*

3. That each State should be entitled to vote *in proportion to its real power and importance for the time being.*

4. That in order to fix this proportion, it should be the *first* business of each Congress to ascertain the relative importance of each State, on the basis—

a. Of population.

b. Of free revenue.

c. Of exports and imports.

5. That each State be entitled to propose, and push to a vote, any question of international politics in which it might be interested.

6. That each State be bound to supply a contingent of men, or money, proportioned to the number of votes assigned to it, for the purpose of enforcing the decrees of the Congress, *by arms,* if necessary.

7. That the representatives of any State which should make war without the sanction of the Congress be excluded from its next meeting; and that the conduct of such state be judged of in the absence of its own representatives on a

* This is a proposal of BENTHAM'S, and I think there is much good sense in it, greatly as I dissent from his general principles.

written statement and oral hearing of counsel, by the representatives of the other States.

8. That all purely national questions be excluded from the deliberations of the Congress; but that the Congress itself should determine whether any question brought before it were or were not of this kind.

9. That civil wars, as opposed to rebellions, be within the jurisdiction of the Congress, the Congress itself being entitled to judge what internal commotions possess the character of civil wars.

10. That all questions brought by individual States before the Congress, be submitted to it by the representatives of such States—first, *scripto*, and then *viva voce*.

11. That a Judicial Tribunal be constituted, to the decision of which it should be competent for the Congress to remit any matter which it conceived to demand judicial determination.

12. That there should be a final appeal from this Tribunal to the Congress itself, in a manner analogous to that in which the judgments of our Supreme Courts may be carried to the House of Lords.

13. That the Judges of this Court be appointed by the Congress, each State voting in proportion to its real weight, ascertained as above.

14. That the Presidents, both of the Congress itself and of the Judicial Tribunal, be appointed or re-elected at each meeting of the Congress; but that the ordinary judges of the tribunal should hold their offices *ad vitam aut culpam*.

15. That the presidents and judges, being officers of the Congress, be paid by the Congress, *and paid very highly;* but that the representatives receive no remuneration, except such as should be granted them by their respective States.

16. That the expenses of the Congress be defrayed by an international tax, to be fixed by the Congress. That the said tax be proportioned to the number of votes enjoyed for the previous year by each State, and be levied by the several States on their own inhabitants.

Many provisions of a more special kind would, of course, suggest themselves, were the scheme to assume a practical shape, but the preceding, I think, will sufficiently indicate its general character.

You will gather from the care with which I have made provision for the forcible execution of the decrees of the Congress, that I am not of the number of those who cherish very sanguine expectations of the possibility of finally and totally abolishing war. Bad as war is, we must never forget that it is a secondary evil to injustice. We must be "*first pure*, and *then* peaceable;" and it is only when an efficient substitute can be found for war, that its abolition can be rationally, *or righteously*, desired. I certainly believe that the decisions of a body which should take cognizance of the real power and importance of its various members, would have a very much better chance of being accepted in lieu of the verdict of battle, than those of a body of which all the members voted equally. The chances would then be many, that individual States would gain no more by fighting than by voting, and to assume that, in such circumstances, they would prefer to vote, is surely to credit them with no very wonderful measure either of humanity or of wisdom. But even the pacific baton is an emblem of physical force, and without the State, as within the State, *the balance must rest on the sword.* War will cease, and ought to cease, only when the wicked cease from troubling; and I am very much of the opinion of the meditative and sarcastic Dutch innkeeper, of whom KANT relates in his Essay, that he had a churchyard painted over his door for a sign, with the superscription "Perpetual peace." On the other hand, however, I subscribe to Dr WHEWELL's view, that whilst war exists, the problem of its abolition is one on which all students and professors of International Law are bound perpetually to labour, because every approximation to its solution is a gain to humanity; and it is the feeling which I entertain of the very solemn character of this obligation which has emboldened me to make so serious a claim on your indulgence.

[Since the preceding paper was read to the Society, an event has occurred which must give fresh impulse to every effort to substitute diplomacy for war. In announcing the results of the Luxemburg conference to the French Legislature, on the 13th May 1867, the Marquis DE MOUSTIER said—

"The Government thinks it useful especially to point out that *for the first time*, the meeting of a Conference, instead of following a war, and confining itself to sanctioning its results, has succeeded in anticipating it, and preserving the benefits of peace. This is a precious indication of the new tendencies which prevail in the world, and over which the friends of progress and civilisation should rejoice."]

J. L.

XXXVIII.—*Some Mathematical Researches.* By H. Fox Talbot, Esq.

(Read 29th April 1867.)

I.—*On Cubic Equations.*

It is well known, that whenever the three roots of a cubic are all *real,* the solution of the equation by Cardan's rule becomes illusory. This is the more remarkable, because, *à priori,* one might have expected that the rule would only fail when the roots were imaginary. Numerous researches have been made by mathematicians on this subject; but they have not succeeded in removing this obstacle; and the only mode of finding the roots of a cubic, when all three are real, has been, by successive approximations, or the use of trigonometrical tables, or (in the case of one root being a whole number), by tentative methods and trials (which often succeed without much difficulty, when the coefficients of the equation are small numbers).

I have found, however, that there exists a certain class of cubic equations which can be solved by a process quite different from that of Cardan, and therefore not subject to any similar cause of failure. It is, moreover, exceedingly direct and simple, requiring no extraction of the cube root.

I shall suppose, for the sake of brevity, that the cubic equation wants its second term. If otherwise, the second term must be taken away by the usual rule. This being premised, the process which I speak of can be employed whereever the equation has a root of the form $a + \sqrt{b}$, where a, b, are whole numbers or rational fractions, and $\sqrt{b}$ is a Surd *in its lowest terms.*

Since $a + \sqrt{b}$ is a root, it follows that $a - \sqrt{b}$ is also a root, and the third root is $-2a$, since by hypothesis the second term of the equation is wanting. All three roots are therefore real, and all the coefficients are either whole numbers or rational fractions.

Notation.—Let $x^n - px^{n-1} + qx^{n-2} - \&c. = 0$ be any equation. I denote the coefficients in the usual way, by p, q, r, &c. One root of the equation will be x, and I usually denote the other roots by y, z, &c.

It is well known that $p = x + y + z + \&c.$, $q = xy + yz + xz + \&c.$, $r = xyz + \&c.$, and so forth. I adopt the abbreviated notation $p = \mathrm{S}\,x$, $q = \mathrm{S}\,xy$, $r = \mathrm{S}\,xyz$, and so on; S standing for "the sum of," and $\mathrm{S}\,xy$ meaning the sum of all the combinations which can be made of two roots multiplied together; $\mathrm{S}\,xyz$, of three roots, and so on.

Most treatises on Algebra give easy rules by which to compute the values of

$S\,x^m$, m being any whole number, in terms of the given coefficients p, q, r, &c. (WOOD's *Algebra*, sixth edition, p. 192); and from these the values of binary compounds like $S\,x^m y^m$, and ternary like $S\,x^m y^m z^m$ can be computed, provided each root has the same index m.

I shall now return to cubic equations, which are the more immediate subject of this paper. The value of CARDAN's rule, when it can be applied, consists in this, that it gives an accurate result by a direct process, without guesses or tentative trials. Its inconvenience is, that the calculation is often very prolix, requiring two extractions of the cube root, although the root may be a whole number. Thus, in the example chosen by WOOD (*Algebra*, p. 172), viz., $x^3 + 6x - 20 = 0$, it is necessary first to extract the cube root of $10 + \sqrt{108}$, and then that of $10 - \sqrt{108}$, and to add these partial results together; which being done, their sum is found to be 2; not, however, without some rather refined arguments (see p. 132) to prove that it is *exactly* 2. Now, from the mere inspection of the equation $x^3 + 6x - 20 = 0$, an arithmetician would not be long in perceiving that he could solve it by supposing $x = 2$, since $8 + 12 = 20$; a process so much shorter, that it is worth while to explain why it must be disallowed. It was long ago perceived that if one of the roots of an equation was a whole number, it would necessarily be found among the divisors of the last term; and could, therefore, with more or less trouble, be found. But though this is no doubt the fact, still it cannot be admitted among the *scientific* modes of solving the equation. For, though it succeeds perfectly in an easy equation like the last, in which the last term 20 has only 2 and 5 for its prime divisors, yet in other equations the number of divisors may be so great that it would be nearly impossible to try them. For example, suppose the last term of a cubic to be 30^{100}, and that the roots are known to be whole numbers from the nature of the question which produced them. Then, since 30 is the product of the primes 2, 3, 5, it is certain that each root is a number of the form $2^a\,3^b\,5^c$. But how many trials would it not require before one of the roots, which we will suppose, for instance, to be $2^{37} \,.\, 3^{51} \,.\, 5^{19}$, would be hit upon?

For this reason, tentative processes are regarded as of doubtful value. A direct and unerring process, however long, is required, if the solution is to be regarded as a scientific one. Now, it will readily be conceded, that if ever an accurate solution is effected of equations of the 5th and higher degrees, the value of their roots will be expressed by radicals of great complication.

Nevertheless, this will not be considered to detract from the merits of the solution. The importance of such a problem is purely *theoretical;* and, therefore, provided only that the process be direct and unerring, its length has nothing whatever to do with the question; it is not intended to be used in practice, but is merely a speculation of the mind. Something similar to this is seen in the famous theorem called WILSON's Theorem, which gives a direct and certain answer to the question, whether any given number N is a prime or not? The solution is complete, but the actual calculation, from its length, in most cases. seems impossible.

In the solution of any algebraic problem, a knowledge of arithmetic may be presupposed. As soon as the required operation is indicated, we conclude that arithmetic has performed it, by its often tedious, but steady and unerring rules; and the task of the algebraist has ended when he has shown how his problem falls within the scope of those rules.

I come now to the more immediate subject of this paper.

(1.) Let the proposed cubic be

$$x^3 + qx - r = 0$$

which will become $$y^3 + qy - r = 0$$

and $$z^3 + qz - r = 0$$

if the other roots are written instead of x. Adding the three equations together. we have

$$(x^3 + y^3 + z^3) + q(x + y + z) - 3r = 0$$

But $$x + y + z = 0 \therefore x^3 + y^3 + z^3 = 3r$$

or in my notation $$\mathrm{S}x^3 = 3r$$

(2.) Since $$x^3 = -qx + r$$

and $$y^3 = -qy + r$$

$\therefore$ their product $$x^3y^3 = q^2xy - qr(x + y) + r^2$$

Whence $$x^3y^3 + y^3z^3 + z^3x^3 = \mathrm{S}x^3y^3 = q^2\mathrm{S}xy - 2qr\mathrm{S}x + 3r^2$$

For, the process being the same with regard to each of the roots, the sum of the equations can be inferred from any one of them.

The above value of $\mathrm{S}x^3y^3$ reduces itself (since $\mathrm{S}xy = q$, $\mathrm{S}x = 0$) to

$$\mathrm{S}x^3y^3 = q^3 + 3r^2$$

(3.) There are six products of the form x^2y. It is evident that the sum of all six is equal to

$$xy(x + y) + xz(x + z) + yz(y + z)$$

But $x + y = -z$, $\therefore$ the first term becomes $-xyz$ or $-r$. Similarly for the two other terms; $\therefore$ the sum of six products like $x^2y = -3r$. But here a more important question arises. If we separate the six products into two groups of three each, *taken in order*, namely,

$$m = x^2y + y^2z + z^2x$$
$$n = y^2x + z^2y + x^2z$$

The first may be conveniently represented by $\mathrm{S}x^2y = m$, and the second by $\mathrm{S}y^2x = n$, and we are led to the inquiry, What are the values of m and n, considered separately? This problem may be solved as follows:—

We have just found that their sum $m + n$ is equal to $-3r$. Let us seek the value of their product mn.

Multiply $m = x^2y + y^2z + z^2x$ by the first term of n, which is y^2x, and we get

$$x^3y^3 + y^3 . xyz + x^2y^2z^2$$

Then multiply by the second and third terms of n, and take the sum of all the results, and we evidently shall have

$$mn = \mathrm{S}x^3y^3 + r\mathrm{S}x^3 + 3r^2$$

$\therefore$ substituting the values already found of

$$\mathrm{S}x^3 = 3r \quad \text{and} \quad \mathrm{S}x^3y^3 = q^3 + 3r^2$$

we find $\qquad mn = q^3 + 9r^2$

Now we have found $\qquad (m + n)^2 = 9r^2$

and from this subtracting $\qquad 4mn = 4q^3 + 36r^2$

we get $\qquad (m - n)^2 = -4q^3 - 27r^2$

from whence the separate values of m and n follow at once.

(4.) If the roots of a cubic, written in any order, are x, y, z, the differences of the roots, *taken in order*, are $x - y$, $y - z$, $z - x$. The other three differences, $y - x$, &c., are merely the negatives of the three first.

THEOREM.—In any cubic equation, wanting the second term, the product of the three differences of the roots, or,

$$\pm \overline{x - y} . \overline{y - z} . \overline{z - x} = \sqrt{-(4q^3 + 27r^2)}$$

For, by actual multiplication, we find

$$\begin{array}{c} z - x \\ y - z \\ \hline yz - xy - z^2 + xz \\ \\ x - y \\ \hline xyz - x^2y - xz^2 + x^2z - y^2z + xy^2 + yz^2 - xyz \; . \end{array}$$

Omitting the first and last terms, which destroy each other, the result is $(-x^2y - y^2z - z^2x) + (y^2x + z^2y + x^2z)$, or $-\mathrm{S}x^2y + \mathrm{S}y^2x$, or, according to our previous notation, $-m + n$. If we take the product of the other three differences of the roots, we get $m - n$.

But we have found the value of $m - n$ to be $\sqrt{-(4q^3 + 27r^2)}$; therefore the theorem is proved. Several important consequences follow. In the first place, if the roots are whole numbers, their differences are so; whence this theorem.

"If the roots of the cubic $x^3 + qx - r = 0$ are whole numbers, the quantity $4q^3 + 27r^2$ is necessarily the negative of a square." This theorem is, I believe, due to LEGENDRE. To give a few examples of it—

Example 1. Let $x^3 - 7x + 6 = 0$, the roots are $1, 2, -3$, $q = 7$, $r = -6$, $4q^3 = -1372$ and $27r^2 = 972 \therefore -4q^3 - 27r^2 = 1372 - 972 = 400$, which is the square of 20.

Example 2. Let $x^3 - 97x + 264 = 0$, the roots are $3, 8, -11$, $q = -97$, $r = -264$. Hence $-4q^3 - 27r^2 = 1768900$, which is the square of 1330.

Example 3. Let $x^3 - 19x + 30 = 0$, the roots are $2, 3, -5$, $q = -19$, $r = -30$. Hence $-4q^3 - 27r^2 = 3136$, which is the square of 56.

Since this function of the coefficients $\sqrt{-(4q^3 + 27r^2)}$ plays an important part in the theory of cubic equations, I propose to distinguish it by the symbol ϕ. We have therefore $\phi =$ product of differences of roots. As a verification, let us resume the three last examples.

Example 1. ϕ was found $= 20$. Roots were $1, 2, -3$, $\therefore$ their differences (taken in order), $-1, 5, -4$, the product of which three numbers is 20.

Example 2. ϕ was found $= 1330$. Roots were $3, 8, -11$, $\therefore$ their differences $-5, 19, -14$. And $5 \times 19 \times 14 = 1330$.

Example 3. ϕ was found $= 56$. Roots were $2, 3, -5$, $\therefore$ their differences $-1, 8, -7$. And $8 \times 7 = 56$.

These theories afford an easy solution of any cubic $x^3 + qx - r = o$, which has a root of the form $a + \sqrt{b}$; where a and b are either integers or rational fractions, and $\sqrt{b}$ is a Surd in its lowest terms.

If $a + \sqrt{b}$ is one root, the other roots will be $a - \sqrt{b}$ and $-2a$. The equation having these roots will be

$$x^3 - (3a^2 + b)\,x + 2a\,(a^2 - b) = o$$

whence

$$q = -(3a^2 + b) \text{ and } r = -2a\,(a^2 - b).$$

Let us calculate the value of ϕ or $\sqrt{-(4q^3 + 27r^2)}$ in this equation. First, we have $-q^3 = (3a^2 + b)^3$ and $r^2 = 4a^2(a^4 - 2a^2b + b^2)$. Therefore we have,

$$\begin{aligned}
-\;q^3 &= 27\,a^6 + 27\,a^4b + 9\,a^2b^2 + b^3 \\
\therefore -\;4q^3 &= 108\,a^6 + 108\,a^4b + 36\,a^2b^2 + 4b^3 \\
-\;27r^2 &= -108\,a^6 + 216\,a^4b - 108\,a^2b^2 \\
\therefore -4q^3 - 27r^2 &= \quad * \quad + 324\,a^4b - 72\,a^2b^2 + 4b^3 \\
&= 4b\,(81\,a^4 - 18\,a^2b + b^2) \\
\therefore \sqrt{-(4q^3 + 27r^2)} &= \pm 2\sqrt{b}\,(9\,a^2 - b).
\end{aligned}$$

But since in this equation we know the roots, a much shorter way of finding ϕ, is to take the product of the differences. Since the roots are $a + \sqrt{b}$, $a - \sqrt{b}$, $-2a$, their differences, taken in order, will be $2\sqrt{b}$, $3a - \sqrt{b}$, $-3a - \sqrt{b}$, the product of which gives $\phi = 2\sqrt{b}\,(b - 9a^2)$ the same as before. Now, by hypothesis a and b are integers or rational fractions, therefore $2(b - 9a^2)$ is rational, and therefore $\phi = \sqrt{b}$ multiplied by a rational quantity.

This conclusion is obviously of great importance, since it shows that we have only to compute the value of ϕ in the given equation $x^3 + qx - r = o$, and we obtain $\phi = R\sqrt{b}$, where R is some rational quantity. The unknown quantity which we have called b separates itself naturally, so to speak, and comes to light, being left by itself under the radical sign.

And, once that b is known, a is easily found from the equation $q = -(3a^2 + b)$. And, therefore, the three roots of the equation $a + \sqrt{b}$, $a - \sqrt{b}$, and $-2a$ become known.

I will now proceed to apply this theory to some numerical examples, which will make it perfectly clear.

I will first observe that the solutions obtained resemble, in one respect at least, the theorem given in most treatises of algebra. "If an equation has two equal roots they can be found." You do not perceive from the aspect of the equation that it has two equal roots, but you are desired to make use of a certain process, and then the equal roots (if any) will appear. So here it is not apparent that an equation has roots of the form $a + \sqrt{b}$, but if it has, and $\sqrt{b}$ be in its lowest terms, both a and b can be found.

Examples of this mode of solution.

Example 1. Let $x^3 - 16x - 24 = 0$. Here $q = -16$, $r = 24$. We compute ϕ as follows:—

$$\begin{aligned} -q^3 &= 4096 \;\therefore\; -4q^3 = 16384 \\ r^2 &= 576 \;\therefore\; 27r^2 = 15552 \\ \therefore\; \phi^2 &= -4q^3 - 27r^2 = 832 \end{aligned}$$

And ϕ will be $\sqrt{832}$, which we must reduce to its lowest terms. We find that 832 is divisible 6 times by 2, the successive quotients being 416, 208, 104, 52, 26, 13. Hence $\phi^2 = 2^6 . 13$ and $\phi = 2^3\sqrt{13}$. The number 13 left under the radical sign is therefore $= b$. To find a we proceed as follows: $b = 13$, $q = -16$ $\therefore\; b + q = -3$, but this is equal to $-3a^2$ $\therefore\; a = 1$. Therefore the roots of the equation are $1 + \sqrt{13}$, $1 - \sqrt{13}$ and -2.

Example 2. Let the equation be $x^3 - 105x - 50 = 0$. Here $q = -105$, $r = 50$. Therefore $-4q^3 - 27r^2$ or ϕ^2 comes out 4563000. This is divisible by 10^3, the quotient is 4563 which is 3 times divisible by 3, the quotients being 1521, 507, 169, and the last number is seen to be the square of 13. Hence $\phi^2 = 10^3 . 3^3 . 13^2$. Extracting the square root, the rational part is $10 . 3 . 13$ or 390, and the irrational part is $\sqrt{10 . 3}$. Hence $\phi = 390\sqrt{30}$. But the rational part is of no use, and need not be calculated. It is sufficient to *omit* all numbers of which *even* powers occur in the value of ϕ^2, and instead of any *odd* power of a number to write the first power, or the number itself. Thus, instead of writing

$\phi^2 = 10^3 . 3^3 . 13^2$, it suffices to write $\phi^2 = 10.3$ and $\phi = \sqrt{10.3}$. Hence the value of b in this equation is 10×3 or 30, and since $q = -105$, $b + q = -75 = -3a^2$, $\therefore a = 5$ and the roots of the equation are $5 + \sqrt{30}$, $5 - \sqrt{30}$, -10.

Example 3. Let $x^3 - 15x + 4 = 0$, here $q = -15$, $r = -4$ and $\phi^2 = -4q^3 - 27r^2 = 13068$. This is twice divisible by 2, the quotients being 6534 and 3267, then 3 times divisible by 3, the quotients being 1089, 363, 121, and the last number is the square of 11 $\therefore \phi^2 = 2^2 . 3^3 . 11^2$, which reduces itself, by expunging the square factors, to 3. But we have seen that b is what ϕ^2 becomes after the omission of all square factors, $\therefore b = 3$ $\therefore b + q = 3 - 15 = -12 = -3a^2$ $\therefore a = 2$, and the roots are $2 \pm \sqrt{3}$, -4.

Example 4. Let $x^3 - 80x + 200 = 0$. Here $q = -80$, $r = -200$ $\therefore \phi^2 = -4q^3 - 27r^2 = 968000$. First divide by 10^2 which leaves quotient 9680, then 4 times by 2, the quotients being 4840, 2420, 1210, 605, which, divided by 5, leaves 121 or 11^2. Hence $\phi^2 = 10^2 . 2^4 . 5 . 11^2$, and omitting the square factors, we find $b = 5$. Then $b + q = 5 - 80 = -75 = -3a^2$ $\therefore a = 5$ and the roots are $5 \pm \sqrt{5}$, -10.

Example 5. Let $x^3 - 30x + 36 = 0$. Here $q = -30$, $r = -36$ and ϕ^2 comes out $= 73008$. Divide 4 times by 2, and the last quotient is 4563. Then divide 3 times by 3, and the last quotient 169 is seen to be the square of 13. Hence $\phi^2 = 2^4. 3^3. 13^2$, and omitting the square factors, $b = 3$ whence $b + q = 3 - 30 = -27 = -3a^2$, whence $a = 3$, and the roots are $3 \pm \sqrt{3}$, -6. When the coefficients are large numbers, the trouble of solving numerical equations naturally increases. But this is a mere affair of arithmetic, and the principles of solution remain unaltered. I will give an example or two with large coefficients.

Example 6. Let $x^3 - 1456\,x - 456 = 0$. Here $q = -1456$, $r = 456$ and $\phi^2 = -4q^3 - 27r^2$ comes out 12340892992, a number which is 6 times divisible by 2. Continuing the reduction, we finally obtain $\phi = 8 . 719\sqrt{373}$, whence $b = 373$ $\therefore b + q = 373 - 1456 = -1083 = -3a^2$ whence $a^2 = 361$ $\therefore a = 19$. Hence the roots are $19 \pm \sqrt{373}$, -38. Let us verify this result with regard to the root -38,

$$\begin{array}{rr} -38^3 = & -54872 \\ -456 = & -\quad 456 \\ \hline & -55328 \end{array} \qquad\qquad \begin{array}{r} 1456 \\ 38 \\ \hline 11648 \\ 4368 \\ \hline +55328 \end{array}$$

Therefore the equation is satisfied.

Example 7. Let $x^3 - 160x + 504 = 0$. Here $q = -160$, $r = -504$. Therefore $-4q^3 - 27r^2$ comes out $= 9525568$. This number is 3 times divisible by 4, and then it is found to be divisible by 13. The quotient is 11449, which a table of squares shows to be the square of 107. Hence $\phi^2 = 2^6 . 13 . 107^2$, whence omitting the square factors $b = 13$. And $b + q = 13 - 160 = -147 = -3a^2$

$\therefore a=7$, and the roots are $7 \pm \sqrt{13}, -14$. The same rules apply when the coefficients are fractional, of which I will give some examples.

Example 8. Let $$x^3 - \frac{15}{4}x - \frac{11}{4} = 0$$

Hence $$q = -\frac{15}{4} \text{ and } r = \frac{11}{4}. \quad \text{Hence } -4q^3 = \frac{3375}{16}$$

and $$27r^2 = \frac{3267}{16} \therefore \phi^2 = \frac{108}{16} = \frac{27}{4} \text{ and } \phi = \frac{3}{2} \cdot \sqrt{3}$$

$$\therefore b = 3 \quad \text{Hence } b + q = 3 - \frac{15}{4} = -\frac{3}{4} = -3a^2 \therefore a = \frac{1}{2},$$

and the roots are $\frac{1}{2} \pm \sqrt{3}, -1$.

Example 9. Let $$x^3 - \frac{26}{15}x + \frac{8}{135} = 0.$$

Here $-4q^3 - 27r^2$ comes out a fraction, of which the numerator is 209952 and the denominator $3 . 15^3$. First let us take the latter, which may be written $3^4 . 5^3$, but omitting the square factors, this reduces itself to 5. The numerator is 5 times divisible by 2, the last quotient being 6561, which $= 3^8$. Hence omitting square factors the numerator $= 2$. Therefore

$$\phi^2 \text{ or } b = \frac{2}{5}.$$

Again $$b + q = \frac{2}{5} - \frac{26}{15} = -\frac{4}{3} = -3a^2 \therefore a = \frac{2}{3}.$$

Hence the roots are $$\frac{2}{3} \pm \sqrt{\frac{2}{5}}, \; -\frac{4}{3}.$$

The same rules apply when the roots are imaginary of the form $a \pm \sqrt{-b}$, provided that $\sqrt{b}$ is a surd *in its lowest terms.*

Example 10. Let $x^3 - 22x + 84 = 0$. Here $q = -22$ $r, = -84$, whence $-4q^3 - 27r^2$ is a *negative* quantity -147920. This number is 4 times divisible by 2, and then once by 5, the last quotient is 1849, which is the square of 43. Hence $\phi^2 = 2^4 . 5 . 43^2$ *taken negatively.* And omitting square factors, ϕ^2 becomes $b = -5$. Hence $b + q = -5 - 22 = -27 = -3a^2 \therefore a = 3$, and the roots are $3 \pm \sqrt{-5}, -6$.

Example 11. Let $x^3 - 68x + 320 = 0$. Here $-4q^3 - 27r^2$ is negative, and $= 1507072$. This number is divisible 8 times in succession by 2, then once by 7, and the last quotient is 841, which is the square of 29, therefore $\phi^2 = 2^8 . 7 . 29^2$ taken negatively, and omitting square factors ϕ^2 becomes $b = -7$. Hence $b + q = -7 - 68 = -75 = -3a^2 \therefore a = 5$, and therefore the roots are $5 \pm \sqrt{-7}, -10$.

It makes no difference whether the given cubic wants the second term or possesses it. In the latter case, it must be taken away by the usual rule, which will change the root from $a + \sqrt{b}$ to $\left(a - \frac{p}{3}\right) + \sqrt{b}$ (p being the coefficient of the second term.) If p is non-divisible by 3, this will cause the new equation to have fractional coefficients. But these cause no difficulty. See examples 8 and 9.

I have sufficiently shown how roots of the form $a + \sqrt{b}$ can be found, provided that $\sqrt{b}$ is a surd in its lowest terms. I will now proceed to consider roots of the form $a + k\sqrt{b}$, where, for simplicity, I will suppose a, b, k integers, $\sqrt{b}$ a surd in its lowest terms, and that the equation wants its second term.

THEOREM.—"k is always a factor of $\frac{\phi}{2}$."

For, since ϕ is the product of the differences of the roots, if the roots are $a + k\sqrt{b}, a - k\sqrt{b}, - 2a$, the differences taken in order will be $2k\sqrt{b}, 3a - k\sqrt{b}, -3a - k\sqrt{b}$, the product of which is $2k\sqrt{b}\,(k^2b - 9a^2) = \phi$, whence $\frac{\phi}{2} = k\sqrt{b}\,(k^2b - 9a^2)$, of which it is evident that k is a factor. It is also evident, that by computing the value of ϕ, $\sqrt{b}$ becomes known, although a and k continue unknown. But, since k is a factor of $\frac{\phi}{2}$, it may sometimes be easily discovered. This will best be explained by a few examples. It must be remembered, that since the roots x, y, z, are $a \pm k\sqrt{b}$, and $-2a$, the coefficient $q = xy + xz + yz = xy - (x+y)^2 = a^2 - k^2b - 4a^2 = -3a^2 - k^2b$, whence $3a^2 + k^2b = -q$.

Example 1. Let $x^3 - 245x - 482 = 0$.

Here $\phi^2 = -(4q^3 + 27r^2) = 52551752$, which, being decomposed in the usual way, we find $\frac{\phi}{2} = 11 \cdot 233\sqrt{2}$, from whence we learn that $b = 2$, and that the roots are therefore of the form $a \pm k\sqrt{2}$. Since then k is a factor of $\frac{\phi}{2}$, it must either equal 11 or 233. But it must also satisfy the general equation $3a^2 + bk^2 = -q$, which in this instance is $3a^2 + 2k^2 = 245$; and since the number 233 is evidently much too large, the true solution must be $k = 11$. Hence $3a^2 = 245 - 2k^2 = 245 - 242 = 3$, whence $a = 1$. Therefore, the three roots are $1 + 11\sqrt{2}$ $1 - 11\sqrt{2}$, and -2.

Before going further, I wish to make some general remarks. All cubic equations, whose coefficients are whole numbers, may, I believe, be divided into three classes.

(1.) All the roots integers.

(2.) Only one root an integer.

(3.) No integer roots.

It is of the second class only that I am now treating. Of course, if an equation has *two* integer roots it must have *three*, else the coefficients would not be whole numbers. Supposing, then, that a cubic equation has one integer root, and no more, then the two other roots must be of the form $a \pm k\sqrt{b}$, where a, b, k, are integers, and $\sqrt{b}$ is a surd in its lowest terms. I have shown how the roots can be found when $k = 1$, and I am now inquiring whether they can be found when k has other values, because that would amount to a general solution of this class (which I have called the second class) of cubics. The example which I have just given shows that it can be readily effected in certain cases, but how far is the method general?

Example 2. Let $x^3 - 975x - 9972 = 0$. In this equation $\frac{\phi}{2}$ comes out $= 3 . 17 . 181\sqrt{3}$. Whence $b = 3$ and the roots are seen to be of the form $a \pm k\sqrt{3}$. Hence, k being a factor of $\frac{\phi}{2}$, must equal either 3, 17, or 181, unless it be the product of two of them, as 3, 17 = 51. But we have also $3\,a^2 + b\,k^2 = -q$, or $3\,a^2 + 3k^2 = 975$; whence $a^2 + k^2 = 325$. Hence it is plain that k cannot be so large a number as 181, or even as 51; therefore it must be either 3 or 17. If we try 3, we get $a^2 = 325 - 9 = 316$, which is not a square. We must therefore have $k = 17$. This gives on trial $a^2 = 325 - 289 = 36$; whence $a = 6$. The roots of the equation are therefore $6 + 17\sqrt{3}$, $6 - 17\sqrt{3}$, and -12.

It will be observed that in this instance the problem of solving the cubic is converted into that of finding two squares such that their sum $a^2 + k^2$ may $= 325$. Of course they are easily found. The general case gives $3a^2 + bk^2 = -q$, where b and q are known integers. This is easily solved by trial when the coefficient q is not too large. This, therefore, is an indeterminate problem of the second degree. If its solution is regarded as within the domain of ordinary arithmetic, the solution of cubic equations *of the second class* must be considered as effected. At any rate, the problem is transformed into a very different one. It will be observed that unless we had found the value of $\sqrt{b}$ from the properties of the function ϕ, we could not have effected the last-mentioned transformation; for b would have remained unknown. We come to the conclusion that this method reduces the solution of the *general* cubic (of the second class) to the solution of the indeterminate problem $3a^2 + bk^2 = -q$, where b and q are given.

Example 3. Resuming the former example 6 in p. 7 of this memoir $x^3 - 1456\,x - 456 = 0$, in which we found $b = 373$, the equation to be solved is $3a^2 + 373k^2 = 1456$. But here every value of k, even $k = 2$, is too large. The only remaining factor of $\frac{\phi}{2}$ is *unity*. Therefore we must have $k = 1$. The value $k = 1$ gives $3a^2 = 1456 - 373 = 1083$, which gives $a = 19$, as we found before.

Example 4. Take the former example 7, $x^3 - 160x + 504 = 0$, in which we found $b = 13$. Hence the equation to be satisfied is $3a^2 + 13k^2 = 160$. There are two solutions, viz., $a = 7$ with $k = 1$, and $a = 6$ with $k = 2$. To decide between them, we take the root $-2a$, which is -14 in the first case, and -12 in the second case; and, substituting these numbers in the original equation, $x^3 - 160x + 504 = 0$, we find that -14 satisfies the equation, while -12 fails. Therefore the root is $7 + \sqrt{13}$. Hence we see that there may be a *plurality of solutions* of the equation $3a^2 + bk^2 = -q$; but they are easily found, when the coefficients of the given equation are of moderate magnitude.

I think it may be of some interest to add, to these examples of my method, WOOD's own example, in illustration of CARDAN's rule, taken from his Algebra, 6th edition, p. 172. He proposes to find the roots of $x^3 + 6x - 20 = 0$ where $q = 6$, $r = 20$. Since q is positive, two roots must be imaginary. Computing $-4q^3 - 27r^2 = \phi^2$ we find that it equals -11664. This number $= 2^4 \cdot 3^6$, multiplied by -1. Hence $\phi = 2^2 \cdot 3^3\sqrt{-1}$, and, consequently, we discover the value of $b = -1$.

The formula $3a^2 + bk^2 = -q$ becomes $3a^2 - k^2 = -6$, or $3a^2 + 6 = k^2$. It is easy to satisfy this by putting $a = \pm 1$, $k = 3$. We find on trial that the negative sign is required. Therefore the roots are $-1 + 3\sqrt{-1}$, and $-1 - 3\sqrt{-1}$, and 2. To verify this, we may observe that the two former are the roots of the quadratic $x^2 + 2x + 10 = 0$, which, being multiplied by $x - 2 = 0$, gives back the proposed cubic.

PART II.—*Some properties of Cubic Equations whose Roots are whole numbers.*

Let $x^3 + qx - r = 0$ be the proposed cubic, wanting the second term. I shall suppose q to be negative, because otherwise the equation would have impossible roots. Let the roots x, y, z be whole numbers, either positive or negative.

Since $\quad x + y + z = 0, \quad$ therefore $\quad z = -(x + y)$

Hence $\quad q$, or $xy + xz + yz = xy - (x + y)^2$

or, $\quad -q = x^2 + xy + y^2$.

This may be shown in another way. Since $x^3 + qx - r = 0$, and $y^3 + qy - r = 0$, $\therefore$ by subtraction $x^3 - y^3 = -q(x - y)$, $\therefore -q = x^2 + xy + y^2$, as before.

THEOREM 1.—"The coefficient q is either of the form $3n$ or $3n + 1$."

Demonstration.—All numbers are of one of the three forms, $3n$, $3n+1$, or $3n+2$. Therefore $x - y$ is of one of those forms. And therefore $(x - y)^2$ is of one of the two forms $3n$ or $3n+1$. Add $3xy$, which makes no difference, since it is a multiple of 3. Therefore, $x^2 + xy + y^2$ is of the form $3n$, or else of the form $3n + 1$. Therefore the coefficient q is of one of those forms.

It is remarkable that this theorem should not be found, as I think, in the treatises of Algebra, for it furnishes a *negative test* as to whether the roots of a given equation, $x^3+qx-r=0$, are whole numbers. If, for any reason, it is supposed that they are so, let the coefficient q (taken positively) be divided by 3; then, if it leaves the remainder 2, the roots cannot be whole numbers.

Hence, if the roots of the equation *are* whole numbers, the coefficient q cannot be taken *ad libitum*. Indeed, by substituting various numbers for x and y in the formula $q=x^2+xy+y^2$, it will be seen that, of the numbers below 100, only 27 can be values of q. These numbers are 3, 7, 12, 13, 19, 21, 27, 28, 31, 37, 39, 43, 48, 49, 52, 57, 61, 63, 67, 73, 75, 76, 79, 84, 91, 93, 97. It will be seen that all these numbers are of the form $3n$ or $3n+1$, never of $3n+2$. Of all these values of q only the number 91 occurs *twice*, namely, when we suppose $x=1$, $y=9$, or else $x=5$, $y=6$. From whence we may infer, that if the roots of $x^3+qx-r=0$ are whole numbers, they may, *in most cases*, be determined from the value of q alone; but that if q has more than one value, r has the same number of values.

It is also observable that this list contains every prime number (of form $3n+1$) below 100. Does this law continue? and may we infer that integer values of x and y always exist, which will satisfy $x^2+xy+y^2=q$, q being any prime of form $3n+1$?

N.B.—Since this was written, I have tried the second *century* of numbers, or those between 100 and 199, including the latter. I find that only 28 of these can be values of q or x^2+xy+y^2, and of these only *two* occur twice, namely 133 and 147. And I find that every prime of the form $3n+1$ is found in the list; therefore the induction holds good so far. There is a well-known theorem "that every prime number of the form $4n+1$ is the sum of two squares, and in one way only." Perhaps it is true (which I only offer as a conjecture) that "every prime number of the form $3n+1$ is of the form x^2+xy+y^2, and that in one way only."

We have seen that the equation $-q=x^2+xy+y^2$, expresses the relation between any two roots x and y. The solution of this equation gives $2y+x=\sqrt{-4q-3x^2}$. But $2y+x=y-z$, because $x+y+z=0$. Whence we derive this theorem: If x be any root of the equation $x^3+qx-r=0$, the difference of the other two roots $=\sqrt{-4q-3x^2}$, which may be called R. Since, then, $y+z=-x$ and $y-z=\text{R}$, we obtain $y=\frac{\text{R}-x}{2}$, $z=\frac{-\text{R}-x}{2}$. This seems much easier than the common method, which prescribes (when one root a is known) that we should divide the equation by $x-a$, and solve the resulting quadratic. Again, since $2y+x=y-z$ is a whole number, $\sqrt{-4q-3x^2}$ must be a whole number: from which the important consequence follows, that in any equation $x^3+qx-r=0$ whose roots are integers, $-4q-3x^2$ is necessarily a square, whichever of the roots is taken for x.

Example 1. Let the roots be 3, 7, -10. Then $q = 21 - 70 - 30 = -79$, $\therefore -4q = 316$. Since the values of x are 3, 7, -10, the values of x^2 are 9, 49, 100, and those of $3x^2$ are 27, 147, 300. Hence the values of $-4q - 3x^2$ are 289, 169, 16, which are all square numbers.

Example 2. Let the roots be $x=11$, $y=13$, $z=-24$, whence $-4q=1732$. Since the values of x are 11, 13, -24, those of $-4q-3x^2$ are—

$$\begin{aligned} 1732 - 363 &= 1369 = 37^2 \\ 1732 - 507 &= 1225 = 35^2 \\ 1732 - 1728 &= 4 = 2^2. \end{aligned}$$

Extracting the square roots, and taking the three roots x, y, z, cyclically, or in regular order, we have

$$\begin{aligned} y - z &= 37 \\ z - x &= -35 \\ x - y &= -2. \end{aligned}$$

The sum of these three equations gives $0=0$, since $x+y+z=0$ by hypothesis.

I come now to the principal object of this paper, which is to inquire under what circumstances, or in what cases, the value of one of the roots can be found, since the two others immediately follow. We have seen that $-4q - 3x^2$ is necessarily a square, therefore, in the first place, x^2 must be such a square as *not to exceed* $\frac{-4q}{3}$; and, in the next place, whatever square N^2 is tried, it is necessary that $3N^2$, subtracted from $-4q$, should leave a square remainder. But if it were necessary to try all the squares N^2 which answer the first condition, that of not exceeding $\frac{-4q}{3}$, such a process would be impracticable from its length (except in equations with small coefficients).

While meditating upon this subject, I have met with a theorem which appears to me rather of a novel kind, and which may perhaps open some new views in the theory of equations. First, I must define what I understand by "Approximate roots." If two roots x, y, of the equation $x^3 + qx - r = 0$ which has integer roots, are so nearly equal (regard being paid to their magnitude) that $2(x+y)+1$ is greater than $\frac{(x-y)^2}{3}$, then I call them "approximate roots." Such numbers, for example, are 17 and 34, because $2(x+y)+1 = 103$, which is greater than $\frac{(x-y)^2}{3}$ or $96\frac{1}{3}$. But 17 and 35 are not "approximate numbers," because $2(x+y)+1 = 105$, which is less than $\frac{(x-y)^2}{3}$ or 108. It does not, therefore, follow that the numbers are at all nearly *equal*, because they are *approximate*. Indeed, the difference between them may be very large, provided the numbers themselves are both of them large.

This *definition* of "approximate roots" having been given, my theorem is the following:—

"If an equation $x^3 + qx - r = 0$ has integer roots, and if two of them are approximate roots, then the square root of the quantity $\frac{-4q}{3}$, *rejecting decimals*, expresses one of the roots of the equation *accurately*."

The *character* of this theorem is like those to which I have already adverted, contained in books of Algebra. "If an equation has two equal roots; or if it has two equal roots with opposite signs; or if it has three roots in arithmetical progression, these roots can be found." It is not obvious that any given equation has these peculiarities; the theorems which I have cited only say that *if it have* either of them, the roots can be found. So my theorem only says, "Approximate roots, *if they exist*, can be found." Only that it has this advantage, that, far from requiring *equal* roots, which can only seldom occur, it suffices that the roots should be *ejusdem generis* so to speak, or, in other words, not very unequal, regard being had to their magnitude.

I will first give a few numerical examples, and then investigate the theory of the subject.

Example 1. Let $$x^3 - 2023x + 29478 = 0.$$

Here $$q = -2023 \therefore \frac{-4q}{3} = 2697\frac{1}{3}$$

the square root of which is ± 51 + some decimals. It will be shown hereafter that we should choose the negative sign; therefore, rejecting the decimals, try if -51 is the root, and it will be found to succeed. The work stands as follows:—

$$\begin{array}{rl} 2023 \times 51 = & 103173 \\ & 29478 \\ \hline & 132651 \end{array} \qquad -51^3 = -132651$$

Now to find the other two roots. We have shown that $-4q - 3x^2$ is a square $= R^2$, and that the root $x = -51$, and that $y = \frac{R - x}{2}$, $z = \frac{-R - x}{2}$.

To compute the value of R. We have $x^2 = 51^2 = 2601$.

$$\begin{array}{rl} \text{We have also } -4q = & 8092 \\ 3x^2 = & 7803 \\ \hline \text{Difference} = & 289 \end{array}$$

Hence $R = \sqrt{289} = 17$. And therefore $y = \frac{17 + 51}{2} = 34$, and $z = \frac{-17 + 51}{2} = 17 \therefore$ the three roots are -51, 34, and 17.

The success of the process is owing to 17 and 34 being *approximate* numbers, although one of them is double of the other.

Example 2. Let $$x^3 - 73x + 72 = 0.$$

Here $$q = -73 \therefore \frac{-4q}{3} = 97\frac{1}{3}$$

the square root of which is $9 +$ some decimals; therefore, rejecting the decimals, try -9 for the root, which succeeds. For, we find

$$\begin{array}{rrl} x^3 = -729 \text{ and } & -73x = & 657 \\ & +72 = & 72 \\ \hline & & 729 \end{array}$$

To compute the value of R, we have $x^2 = 81$, and $-4q - 3x^2 = 292 - 243 = 49$. Hence $R = \sqrt{49} = 7$, and $y = \frac{R - x}{2} = \frac{7+9}{2} = 8$, $z = \frac{-R - x}{2} = \frac{9-7}{2} = 1$. Hence the roots are 1, 8, and -9. I have given this example because the roots 1 and 8 are "approximate," though one is eight times greater than the other. This is, I believe, the extreme limit of their relative magnitudes, which does not occur in any other instance.

It will be observed also that in this example the approximate roots 1 and 8 differ by 7. This is also a kind of limit, and gives rise to the following theorem or corollary:—"Whatever the magnitude of the roots of $x^3 + qx - r = 0$, whose roots are integers, if two of them differ by 7, or by a smaller number, they can all be found by a process only requiring the extraction of the square root."

Example 3. Given $x^3 - 1477x + r = 0$, to find the roots? Here $q = -1477$. I have not deemed it necessary to give the value of r in this and the next examples, for the following reason. The values of the roots usually depend upon q alone: q being given, r has generally only one value, at any rate it has but a paucity of values, and as the solution found must always suit *one* of those values, I have omitted the consideration of it to avoid the introduction of high numbers, r being generally a much higher number than q. If an arbitrary number were suggested for r, it would only follow that such a proposed equation would not have *whole numbers* for its roots, and it is such only that we are considering at present. Since $-q = 1477$, $\frac{-4q}{3} = 1969\frac{1}{3}$. The square root of this is $44 +$ decimals. Try therefore -44 for the root, and it is found to succeed.

For, since $-4q = 5908$, subtract $3 \cdot 44^2 = 5808$, and there remains 100, which is a square number. Hence $z = -44$ and $R = \sqrt{100} = 10$. Therefore $x + y = 44$, $x - y = 10$, whence $x = 27$, $y = 17$. As a verification, we find that these values give for q or $xy + xz + yz$ or $17 \cdot 27 - 17 \cdot 44 - 27 \cdot 44$ the number -1477, which was given in the proposed equation.

Example 4. Let us try higher numbers.

Suppose $x^3 - 2089168\,x + r = 0$. This value of q gives $\frac{-4\,q}{3} = 2785557\frac{1}{3}$.

The square root of this number is $1668 +$ decimals, and in fact -1668 is found to be the root. For we have $-4q = 8356672$, from which if we subtract 3 times the square of 1668 or 8346672, there remains 10000, which is a square number.

To find the other roots. Since $z = -1668$, and $R = \sqrt{10000} = 100$, $x + y = 1668$ and $x - y = 100$. Hence $x = 884$ and $y = 784$.

Investigation of the Theory of the above Method.

Since $x + y + z = 0$, one root must have an opposite sign to the other two.

To fix the ideas, let two roots x, y, be *positive*, then the third or negative root z is the largest of the three, since it equals the sum of the other two. Let the equation be as before, $x^3 + qx - r = 0$. We have found that $-q = x^2 + xy + y^2$, whence $-4q = 4x^2 + 4xy + 4y^2$. Subtract from this $3z^2 = 3(x+y)^2 = 3x^2 + 6xy + 3y^2$, and we obtain $-4q - 3z^2 = x^2 - 2xy + y^2 = (x-y)^2 = D^2$ (putting D for the *difference* of the roots). Hence we find, as before (though more directly), that if z be the greatest of the three roots $\sqrt{-4q - 3z^2} = D$, which is the difference of the two smaller and positive roots. But since $-4q$ is a positive quantity, let us put it $= +Q$. In the following lines, I think I shall be clearer, if I neglect the negative sign of z for the present, and speak only of its *magnitude;* restoring the negative sign at the end of the inquiry. We have seen that $3z^2$ always falls short of Q, by the quantity D^2. Now, let us inquire what are the conditions necessary, in order that $3(z+1)^2$ may not fall short of Q, but may exceed it? Evidently that circumstance requires that the increment $3(2z+1)$ should exceed D^2, or that $2z+1$ should exceed $\frac{D^2}{3}$; that is, that $2(x+y)+1 > \frac{D^2}{—}$. Accordingly, this is the definition of "approximate roots," which I have proposed in the preceding pages. Hence it follows then, that whenever the roots are approximate, although z is less than $\frac{\sqrt{Q}}{3}$ (as it always is), yet $z+1$ is greater than $\frac{\sqrt{Q}}{3}$, that is, z is greater than $\frac{\sqrt{Q}}{3} - 1$. Let $\frac{\sqrt{Q}}{3}$ be equal to the whole number A + the decimal portion d, so that d is some positive quantity *less than unity.* Hence, we have proved that z is less than $A + d$, but greater than $A - 1 + d$; therefore, *à fortiori*, it is greater than $A - 1$. But it must be remembered that z is a whole number. How can it be greater than $A - 1$, yet less than $A + d$? Evidently in one way only; it must be *equal to* A. But A is the square root of $\frac{\sqrt{Q}}{3}$, *minus* the decimal portion d; and, therefore, restoring the value of $Q = -4q$, we have $z = \sqrt{-\frac{4q}{3}}$, *rejecting the decimals,* which is what we undertook to prove. But since z is the negative root of the equation, we must, of course, give the negative sign to this square root.

For simplicity, I have hitherto supposed that the cubic equation wants its second term, but if that is not the case, it makes no material difference, because, an equation with integer roots, having its second term, can, with a little trouble, be converted into one equally with integer roots, but wanting its second term.

The method of solution which I have pointed out is sufficiently simple. I was desirous of knowing what degree of generality it had. With this view I made the following trial of it:—Since in the first century of numbers there are twenty-seven which can be values of q, and in the second century twenty-eight, the total is fifty-five. But since three of these are found to occur twice, I add them, which gives in all fifty-eight cases. I found by trial, that in fifty of these cases the method was successful, and that it only failed in eight. It may therefore be recommended, so long as the coefficient q is of moderate magnitude.

But what is to be done, in case the number obtained proves not to be a root? (which merely shows that there are no "approximate" roots in the equation.) Are we to abandon the solution of the equation by this method? We need not do so, for I find that the number obtained, if not the root, is generally a good approximation to the root, and that by a kind of easy *supplementary* process the true root is generally obtainable. This will be best shown by taking a numerical example.

Rule of Second Approximation.—Suppose the equation $x^3 - 1533\,x - r = 0$ to be proposed for solution. Here $-q = 1533 \therefore \frac{-4q}{3} = 2044$, the root of which is 45 + decimals. In order to try whether -45 is the root, take from $-4q = 6132$, three times 2025, or 45^2. The remainder is 57, which is not a square number, and therefore -45 is not the root. Now, to find the root, assume that -45 is an approximation to it, and proceed as follows. The differences between the successive squares 45^2, 44^2, 43^2, &c. &c., are $2 \cdot 44 + 1$, $2 \cdot 43 + 1$, &c. &c., or 89, 87, 85, 83, &c. &c. Multiply these by three, and we get the series 267, 261, 255, &c., in which each successive term diminishes by *six*.

Then taking the number 57, which was the first remainder, when -45 was tried for the root, add to it the terms of the foregoing series one by one, till a square arises, which (if the numbers are high) may give the computer the trouble of consulting "a table of squares." Thus:—

$$\begin{array}{lll} 45 & \text{gives} & 57 \\ & & \underline{267} \\ 44 & ,, & 324 \\ & & \underline{261} \\ 43 & ,, & 585 \\ & & \underline{255} \\ 42 & ,, & 840 \\ & & \underline{249} \\ 41 & ,, & 1089 = 33^2 \end{array}$$

Therefore, -41 is the root, and 33 is the difference of the two other roots x and y. Therefore, $x + y = 41$, and $x - y = 33$, whence $x = 37, y = 4$, therefore the roots are 4, 37, -41.

In this example the roots 4 and 37 are far from being "approximate," which is the reason why the root was not found at once.

These problems may be treated in another manner. If $x^3 + qx - r = 0$, and x, y, z, are the roots, we have seen that $-4q - 3x^2 = D^2$, where D denotes $y - z$, as before. Now, though D is unknown at present, we may make two different suppositions concerning it:—*First,* that it is *small; secondly,* that it is *large,* in comparison with the root x. If we make the first supposition we may try to solve the equation, put in the shape $\frac{-4q}{3} - x^2 = \frac{D^2}{3}$, by supposing (since D is small) that x^2 is that square which is nearest to $\frac{-4q}{3}$ but smaller than it. This supposition is often justified by the result, on trial. But if not, let us make the other supposition, and try to solve the equation (put in the shape $-4q - D^2 = 3x^2$), by supposing (since D is large) that D^2 is that square which is nearest to $-4q$ but smaller than it. This also very often succeeds and gives at once the value of $3x^2$, and thence of x. The first of these methods is essentially the same with that described in the preceding pages, but the second is different. If both of them are employed, I find that one or other of them solves every case of the equation $x^3 + qx - r = 0$, provided the coefficient q does not exceed 200. How much further the success of the method extends I have not yet had leisure to ascertain. I will conclude by giving an example. We find that the equation $x^3 - 1533\,x - r = 0$ was not soluble by the *first* method without the help of a "second approximation," but it is readily soluble by the *second* method, as follows:—

Since $-q = 1533$, therefore $-4q = 6132$. Assume D^2 to be the greatest square which is less than 6132, this will be 6084. Hence $-4q - D^2 = 48$. Putting this $= 3x^2$ we get $x^2 = 16$, which, being a square number, shows that we are right. Therefore $x = 4$. And since $D^2 = 6084$, $D = 78$. Hence $y + z = -4$ and $y - z = 78$, whence $y = 37$, $z = -41$, and $x = 4$.

We may therefore draw this conclusion, that "If the equation $x^3 + qx - r = 0$ has integer roots, and q is a number less than 200, the roots can always be found by the simple extraction of the square root."

XXXIX.—*On Centres, Faisceaux, and Envelopes of Homology.* By Rev. HUGH MARTIN, M.A., Member of the Mathematical Society of London, and Examiner in Mathematics in the University of Edinburgh. Communicated by Professor KELLAND.

(Read 1st April 1867.)

One of the theorems of a paper which Professor KELLAND did me the honour to read to the Society, in March 1865, opens up a field of geometrical investigation so interesting and fertile, that I venture to ask attention to some of the results of a partial examination of it in the following series of propositions. I think it right to explain, that I do not venture to expect attention to them on account of any importance attaching to them individually, but on account of their number and somewhat elegant relations. Considered individually, they may be of little importance, having no claim to rank, so to speak, among propositions of a *planetary* magnitude. But a system of *moons*, however diminutive, may become interesting if they present elegant relations among their mean motions and longitudes; and an orbit that would be grudged to a pigmy planet may be willingly accorded to a host of *planetoids*. If this is still too exalted language in which to speak of the following results, I can at least confidently affirm that they indicate a direction in which an analyst of very moderate attainments may easily discover for himself a shower of *meteors*.

It is well known that when straight lines are drawn from the angles of a triangle through any point in its plane, they intersect the sides in three points, which form the angular points of a triangle so related to the first that the intersections of their corresponding sides range in a straight line. As the triangles are said to be in homology,* we may conveniently designate the point as the Centre of Homology, and the resulting straight line as the Line of Homology,—the line represented in the former paper by $\phi(P_1)$. If we have a second point, P_2, inverse to the former, we have a second line of homology, which may be called the inverse line.

Now, if the centre of homology is subjected to motion according to a given law, or in a given curve, the line of homology displaces itself, so as, in its varying positions, to constitute a *faisceau.* The Envelope of this *faisceau* may then be inquired for. Simultaneously the inverse centre of homology will move in a

* I fear this terminology may give an aspect of pretentiousness to the paper which is far from being intended. But if I was to avoid the indefinite title, "*On a Certain Class,*" &c., I confess I could find no other sufficiently descriptive.

curve that may be found; and the inverse line of homology, displacing itself, will generate another *faisceau*, whose envelope also may be sought. Farther, the lines of homology, the direct and inverse, will in general intersect, and the point of intersection, partaking of the motion of the system, will describe a locus, which may also be made the subject of inquiry. The points in which perpendiculars from the centres of homology meet their respective, direct, and inverse lines of homology will also present certain loci when the centre of homology describes any given curve, and the perpendiculars themselves will generate *faisceaux*, whose envelopes may be sought.

Farther; this system of problems may be generalised and greatly extended. The line, called the line of homology, has originated in a particular geometrical consideration; but it may be considered as unshackled from its geometrical genesis. The conception may be idealised, and conceived of as not restricted to the straight line, the curve of the first degree, but as a curve of any degree whatever. It may be a function of any degree in the variables, and of any degree in the co-ordinates of the centre as parameters; and we may thus have *faisceaux* of curves of homology of any order, and envelopes corresponding to them as before. Moreover, the general problem may be inverted, and instead of inquiring, "What envelope will the *faisceau* of homology generate when the centre of homology moves in a given curve?" we may inquire, "In what curve must the centre of homology move, in order, with a given curve of homology, to beget a given envelope?"* Or the inquiry may take yet another direction, "What must be the form of the curve of homology in order that its *faisceau*, generated by the centre of homology moving in a given curve, may produce a given envelope?" Some instances of the problem, in all these forms, will be found in the following pages. It is evident that the general inquiry may be prosecuted in such directions as would task the uttermost resources of modern discoveries in the theory of linear transformation and of canonical forms. With the exception of an instance of elimination by the aid of the Jacobian and its differential coefficients, we shall not pursue it to the necessity of laborious calculations; and shall, for the most part, restrict ourselves to the conic sections, and to such forms of these as are most manageable, and do not demand intricate elimination. These forms are,—*First*, the conic circumscribing the triangle of reference, and of the general form,

$$\frac{u}{\alpha}+\frac{v}{\beta}+\frac{w}{\gamma}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (1);$$

Second, the conic touching the three sides of the triangle, viz.,

$$\pm(u\alpha)^{\frac{1}{2}}\pm(v\beta)^{\frac{1}{2}}\pm(w\gamma)^{\frac{1}{2}}=0 \quad . \quad . \quad . \quad . \quad . \quad (2);$$

* The idea of the inverse of the problem of the Envelope seems first to have occurred to BOOLE. See his paper—characterised by his usual high generality and beautiful originality—in "Cambridge and Dublin Mathematical Journal," vol. vii. p. 156.

Third, the conic with respect to which the triangle is self-conjugate, viz.,

$$u^2\alpha^2 + v^2\beta^2 + w^2\gamma^2 = 0 \quad . \quad . \quad . \quad . \quad . \quad (3);$$

And, *fourthly*, the conic touching two sides of the triangle of reference in the points where the third side meets them, viz.,

$$k^2\alpha^2 = \beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4).$$

SECTION I.—*The Faisceau of Homology being Straight Lines.*

I. If the centre of homology move in a straight line, the *faisceau* of homology (being straight lines) will envelope a conic touching the three sides of the triangle of reference.

Let the centre of homology move in the straight line

$$l\alpha + m\beta + n\gamma = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (5).$$

Retaining the co-ordinates of the centre in the form in which they appeared in the former paper, f^{-1}, g^{-1}, h^{-1} (co-ordinates of P_1), and substituting these in (5), we have

$$lf^{-1} + mg^{-1} + nh^{-1} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (6).$$

The equation of the straight line of homology is,

$$f\alpha + g\beta + h\gamma = 0 = u \quad . \quad . \quad . \quad . \quad . \quad . \quad (7).$$

Now, by the theory of envelopes,

$$du = 0 = \alpha df + \beta dg + \gamma dh \quad . \quad . \quad . \quad . \quad . \quad . \quad (8).$$

Differentiating (6), $\quad 0 = - lf^{-2}df - mg^{-2}dg - nh^{-2}dh \quad . \quad . \quad . \quad . \quad . \quad (9).$

Multiplying (8) by an indeterminate coefficient p, and adding (9), we have

$$(p\alpha - lf^{-2})\,df + (p\beta - mg^{-2})\,dg + (p\gamma - nh^{-2})\,dh = 0 \quad . \quad . \quad . \quad (10).$$

Now, as p is indeterminate, and as it is the ratios merely of the three parameters f, g, h, we are concerned with, reducing them virtually to two—with which the fact that they are homogeneously involved accords—we are at liberty to make two suppositions. Let these be that the coefficients of df and dg in (10) shall vanish. This makes the coefficient of dh also vanish; and we have,

$$f = \pm\sqrt{\frac{l}{p\alpha}}\,; \quad g = \pm\sqrt{\frac{m}{p\beta}}\,; \quad h = \pm\sqrt{\frac{n}{p\gamma}} \quad . \quad . \quad . \quad (11).$$

Substituting these values in (7) gives,

$$\pm (l\alpha)^{\frac{1}{2}} \pm (m\beta)^{\frac{1}{2}} \pm (n\gamma)^{\frac{1}{2}} = 0 \quad . \quad . \quad . \quad . \quad . \quad (12);$$

which is the equation of a conic touching the three sides of the original triangle.

II. If the centre of homology move in a conic touching the three sides of the original triangle, the linear *faisceau* of homology will envelope the curve,

$$\pm (l\alpha)^{\frac{1}{3}} \pm (m\beta)^{\frac{1}{3}} \pm (n\gamma)^{\frac{1}{3}} = 0 \quad . \quad . \quad . \quad . \quad . \quad (13).$$

The proof of this is, *mutatis mutandis*, the same as before. And generally,—

III. If the centre of homology move in the curve

$$\pm (l\alpha)^{\frac{1}{n}} \pm (m\beta)^{\frac{1}{n}} \pm (n\gamma)^{\frac{1}{n}} = 0 \quad . \quad . \quad . \quad . \quad . \quad (14),$$

the linear *faisceau* of homology will envelope the curve,

$$\pm (l\alpha)^{\frac{1}{n+1}} \pm (m\beta)^{\frac{1}{n+1}} \pm (n\gamma)^{\frac{1}{n+1}} = 0 \quad . \quad . \quad . \quad . \quad . \quad (15).$$

IV. If the centre of homology move in a conic with respect to which the original triangle is self-conjugate, namely,

$$l^2\alpha^2 + m^2\beta^2 + n^2\gamma^2 = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (16),$$

the linear *faisceau* of homology will envelope the curve,

$$(l\alpha)^{\frac{2}{3}} + (m\beta)^{\frac{2}{3}} + (n\gamma)^{\frac{2}{3}} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (17).$$

The proof of this is the same as before; and it follows also from III., n being taken equal to $\frac{1}{2}$.

Of course, in (16), that the conic may not be imaginary, one or other of the terms l, m, n, must be affected with the coefficient $\sqrt{(-1)}$; but the equation is maintained in this form for the sake of symmetry.

V. If the centre of homology move in a conic circumscribing the original triangle, the linear *faisceau* of homology will be a *faisceau pivotante.*

The equation of the circumscribing conic is

$$\frac{l}{\alpha} + \frac{m}{\beta} + \frac{n}{\gamma} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18).$$

Replacing α, β, γ, by the co-ordinates of the centre, we have,

$$lf + mg + nh = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19).$$

And, as before,

$$f\alpha + g\beta + h\gamma = 0 = u \quad . \quad . \quad . \quad . \quad . \quad . \quad (20).$$

And the result is that

$$\alpha : \beta : \gamma :: l : m : n \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21).$$

That is, the line of homology revolves round the fixed point l, m, n. This is a case of what the French writers call *courbes pivotantes*,* the curve here being of the first degree, the straight line. The number of *pivots* about which a *faisceau* of curves of the degree s revolves, or through which every member of the *faisceau pivotant* passes, is of course s^2. We shall meet with a case of this subsequently, in reference to curves of the second degree—conic sections.

VI. If the centre of homology move in the circle circumscribing the original

* The reader will find this subject elegantly treated in "*Études Analytiques sur la Théorie Général des Courbes Planes*," *par* M. FELIX LUCAS (*Paris*, 1864); where, among other results, a very pretty proposition concerning a property of conics circumscribing the same quadrilateral (due to M. LAMÉ), is thus generalised:—*Les polaires d'ordre quelconque d'un point du plan, relativement aux diverses courbes d'un faisceau pivotant, forment elles-mêmes un faisceau pivotant.*

triangle, the linear *faisceau* of homology will revolve round a point whose co-ordinates are in the ratios of the sides of the triangle.

This is an immediate corollary from the former, the equation of the circumscribing circle being, as is well known,

$$\frac{a}{\alpha} + \frac{b}{\beta} + \frac{c}{\gamma} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (22).$$

VII. If the centre of homology describe a straight line, the inverse centre will describe a conic circumscribing the original triangle; the linear *faisceau* will envelope a conic touching its three sides, while the inverse linear *faisceau* will revolve round a fixed point.

This singular system of movements is merely a combination of propositions I. and V.,—it being remembered that the co-ordinates of the two centres are mutually inverse.

VIII. If the centre of homology describe a conic touching two sides of the triangle in the points where the third side meets them, the inverse centre will describe a second conic similarly situated; the linear *faisceau* will envelope a third conic similarly situated, and the inverse linear *faisceau* a fourth conic, also similarly situated.

The equation of the conic which, by hypothesis, the centre describes is,

$$k^2 \alpha^2 = \beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad (23);$$

or, substituting the co-ordinates of the centre,

$$k^2 gh - f^2 = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (24).$$

Also
$$f\alpha + g\beta + h\gamma = 0 = u \quad . \quad . \quad . \quad . \quad . \quad . \quad (25).$$

Differentiating as before, multiplying by an indeterminate coefficient p, &c., we have,

$$f = \frac{p\alpha}{2}; \quad g = -\frac{p\gamma}{k^2}; \quad h = -\frac{p\beta}{k^2} \quad . \quad . \quad . \quad . \quad (26).$$

And substituting in (25), we find, as the equation of the envelope of the direct linear *faisceau*,

$$k^2 \alpha^2 = 4\beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad (27).$$

Farther, while the centre moves in $k^2\alpha^2 = \beta\gamma$, it is evident, inverting the co-ordinates, that the inverse centre moves in

$$\frac{\alpha^2}{k^2} = \beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad (28),$$

and that, by the portion of the proposition already proved, the inverse *faisceau* must envelope

$$\frac{\alpha^2}{k^2} = 4\beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad (29);$$

which completely proves the proposition.

It is also evident that if the centre of homology now move in the direct envelope of homology (27), the direct linear *faisceau* of homology will now envelope

$$k^2\alpha^2 = 16\beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (30),$$

the inverse centre will move in

$$\frac{\alpha^2}{k^2} = 4\beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (31),$$

and the inverse linear *faisceau* will envelope

$$\frac{\alpha^2}{k^2} = 16\beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (32);$$

and so on, *en suite*, while the resulting envelopes are successively taken as the curve in which the centre moves,—the n^{th} envelope of the direct *faisceau* being, of course,

$$k^2\alpha^2 = (4)^n.\beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad (33),$$

and of the inverse *faisceau*,

$$\frac{\alpha^2}{k^2} = (4)^n.\beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (34).$$

IX. If the centre of homology move in the curve

$$\frac{l^2}{\alpha^2} + \frac{m^2}{\beta^2} + \frac{n^2}{\gamma^2} + \frac{mn}{\beta\gamma} + \frac{nl}{\gamma\alpha} + \frac{lm}{\alpha\beta} = 0 \quad . \quad . \quad . \quad . \quad . \quad (35),$$

the linear *faisceau* will envelope a conic section.

We have, $$l^2f^2 + m^2g^2 + n^2h^2 + mngh + nlhf + lmfg = 0 \quad . \quad . \quad (36),$$

and, $$f\alpha + g\beta + h\gamma = 0 = u \quad . \quad . \quad . \quad . \quad . \quad . \quad (37).$$

Differentiating (35) and (37), and neglecting the indeterminate coefficient, as its square will divide out in the final result, we have

$$\left.\begin{aligned} 2lf + mg + nh &= \frac{\alpha}{l}, \\ lf + 2mg + nh &= \frac{\beta}{m}, \\ lf + mg + 2nh &= \frac{\gamma}{n}. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (38).$$

Eliminating g and h, and neglecting the numerical coefficient, which would divide out as merged in the indeterminate multiplier, we have,

$$\left.\begin{aligned} lf &= \frac{3\alpha}{l} - \frac{\beta}{m} - \frac{\gamma}{n}; \\ \text{or,}\quad f\alpha &= \frac{3\alpha^2}{l^2} - \frac{\alpha\beta}{lm} - \frac{\gamma\alpha}{nl}. \\ \text{Similarly,}\quad g\beta &= \frac{3\beta^2}{m^2} - \frac{\beta\gamma}{mn} - \frac{\alpha\beta}{lm}. \\ \text{And,}\quad h\gamma &= \frac{3\gamma^2}{n^2} - \frac{\gamma\alpha}{nl} - \frac{\beta\gamma}{mn}. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (39).$$

Substituting these in (37), we have, as the envelope required, the conic section,

$$3\left(\frac{\alpha^2}{l^2}+\frac{\beta^2}{m^2}+\frac{\gamma^2}{n^2}\right)-2\left(\frac{\beta\gamma}{mn}+\frac{\gamma\alpha}{nl}+\frac{\alpha\beta}{lm}\right)=0 \quad . \quad . \quad (40).$$

Let it now be supposed that the centre of homology moves in this last curve, and let it be required to find the envelope of the inverse *faisceau.*

We have now, $\quad 3\{(lf)^{-2}+(mg)^{-2}+(nh)^{-2}\}-2\{(mngh)^{-1}+(nlhf)^{-1}+(lmfg)^{-1}\}=0,$

and $\quad f^{-1}.\alpha+g^{-1}.\beta+h^{-1}\gamma=0=u,$

which, with the exception of the numerical coefficients, are identical with (36) and (40), the variables to be eliminated being merely inverted, and l, m, n also inverted. Moreover, the process reverses the change of numerical coefficients, as exhibited between (35) and (40), and the result is

$$l^2\alpha^2+m^2\beta^2+n^2\gamma^2+mn\beta\gamma+nl\gamma\alpha+lm\alpha\beta=0 \quad . \quad . \quad (41);$$

a conic, and somewhat singularly related to the original curve, namely (35).

X. If the centre of homology move in a conic circumscribing the original triangle, required the locus of the intersection of the direct and inverse lines of homology.

As the point whose locus is required is on both the lines, we have simultaneously,

$$\alpha f+\beta g+\gamma h=0 \quad . \quad . \quad . \quad . \quad . \quad (42),$$

and

$$\frac{\alpha}{f}+\frac{\beta}{g}+\frac{\gamma}{h}=0 \quad . \quad . \quad . \quad . \quad . \quad (43).$$

Also the equation of the circumscribing conic is,

$$lf+mg+nh=0 \quad . \quad . \quad . \quad . \quad . \quad (44).$$

From (42) and (44), we have,

$$\frac{f}{\begin{vmatrix} m, & \beta \\ n, & \gamma \end{vmatrix}}=\frac{g}{\begin{vmatrix} n, & \gamma \\ l, & \alpha \end{vmatrix}}=\frac{h}{\begin{vmatrix} l, & \alpha \\ m, & \beta \end{vmatrix}} \quad . \quad . \quad . \quad . \quad . \quad (45),$$

and, by (43),

$$\frac{\alpha}{\begin{vmatrix} m, & \beta \\ n, & \gamma \end{vmatrix}}+\frac{\beta}{\begin{vmatrix} n, & \gamma \\ l, & \alpha \end{vmatrix}}+\frac{\gamma}{\begin{vmatrix} l, & \alpha \\ m, & \beta \end{vmatrix}}=0 \quad . \quad . \quad . \quad (46),$$

or, $$\frac{mn}{\beta\gamma}(\alpha^2-\beta^2-\gamma^2)+\frac{nl}{\gamma\alpha}(\beta^2-\gamma^2-\alpha^2)+\frac{lm}{\alpha\beta}(\gamma^2-\alpha^2-\beta^2)+l^2+m^2+n^2=0 \quad (47);$$

which is the locus required—a curve of the third order. If $\alpha=0$, we have

$$(n\beta-m\gamma).(\beta^2-\gamma^2)=0 \quad . \quad . \quad . \quad . \quad (48);$$

that is to say, the curve cuts the side of the triangle in points where $\beta = \pm \gamma$, and $\beta : \gamma :: m : n$. And similarly for the intersections with the other sides. The geometrical interpretation is, that the curve cuts the sides of the triangle in the points where the bisectors of the interior and exterior opposite angles cut them; as also, in the points determined by drawing straight lines from the angles to the opposite sides respectively, through the points l, m, n; that is, the pole of the line $\{ l\alpha + m\beta + n\gamma = 0 \}$ in which the inverse centre of homology moves, while the direct centre describes the circumscribing conic. Considering the triangle as a curve of the third order, the $3^2 = 9$ points, in which it intersects the third-order locus thus found, are in this manner somewhat elegantly determined.

XI. If the centre of homology move in a conic touching two sides of the triangle where the third side meets them, required the locus of the intersection of the direct and inverse lines of homology.

Here we have to eliminate from the three equations,

$$\left.\begin{aligned} f^2 - k^2 gh &= 0, \\ \alpha f + \beta g + \gamma h &= 0, \\ \alpha gh + \beta hf + \gamma fg &= 0. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (49);$$

and the result is,

$$\frac{(\beta^2 - \gamma^2)^2}{\alpha^2 \beta\gamma} - \frac{\beta^2 + \gamma^2}{\beta\gamma} + k^2 + \frac{1}{k^2} = 0 \quad . \quad . \quad . \quad . \quad (49');$$

a curve of the fourth order, which is not altered by interchanging β and γ,—as it evidently ought not to be; nor by inverting k,—which evidently also it ought not to be, since while the centre of homology moves in

$$k^2 \alpha^2 = \beta\gamma,$$

the inverse centre moves in

$$\frac{\alpha^2}{k^2} = \beta\gamma,$$

and if these are interchanged, the lines of homology are simply interchanged, and the result of elimination ought to be unaltered, as we see is the case.

XII. Required the locus of the intersection of the direct and inverse lines of homology, when the centre of homology moves in a conic with respect to which the triangle is self-conjugate.

Taking the equation of the curve in which the inverse centre moves, the square of the equation of the direct line of homology, and the equation of the inverse line cleared of fractions, we have to eliminate f, g, h, from the three following equations:—

$$u = lf^2 + mg^2 + nh^2 = 0 \quad . \quad . \quad (50);$$

$$v = \alpha^2 f^2 + \beta^2 g^2 + \gamma^2 h^2 + 2\beta\gamma gh + 2\gamma\alpha hf + 2\alpha\beta fg = 0 \quad . \quad . \quad (51);$$

$$w = \alpha gh + \beta hf + \gamma fg = 0 \quad . \quad . \quad (52).$$

Forming the Jacobian,

$$\begin{vmatrix} \frac{du}{df}, & \frac{dv}{df}, & \frac{dw}{df} \\ \frac{du}{dg}, & \frac{dv}{dg}, & \frac{dw}{dg} \\ \frac{du}{dh}, & \frac{dv}{dh}, & \frac{dw}{dh} \end{vmatrix} = \mathrm{J} \quad . \quad . \quad . \quad . \quad . \quad (53),$$

we have,

$$(\alpha f + \beta g + \gamma h) \times \begin{vmatrix} lf, & \alpha, & \beta h + \gamma g \\ mg, & \beta, & \gamma f + \alpha h \\ nh, & \gamma, & \alpha g + \beta f \end{vmatrix} = \mathrm{J} \quad . \quad . \quad . \quad . \quad (54),$$

That is,—

$$(\alpha f + \beta g + \gamma h) \cdot \left\{ \begin{matrix} l\alpha\beta fg + l\beta^2 f^2 - l\gamma^2 f^2 - l\gamma\alpha hf + m\beta\gamma gh + m\gamma^2 g^2 - \\ m\alpha^2 g^2 - m\alpha\beta fg + n\gamma\alpha hf + n\alpha^2 h^2 - n\beta^2 h^2 - n\beta\gamma gh \end{matrix} \right\} = \mathrm{J} \quad (55).$$

Multiplying the part within brackets by the coefficient αf, and differentiating with respect to f, we have

$$\left\{ \begin{matrix} 2l\alpha^2\beta fg + 3l\alpha\beta^2 f^2 - 3l\alpha\gamma^2 f^2 - 2l\gamma\alpha^2 hf + m\alpha\beta\gamma gh + m\gamma^2\alpha g^2 - \\ m\alpha^3 g^2 - 2m\alpha^2\beta fg + 2n\gamma\alpha^2 hf + n\alpha^3 h^2 - n\alpha\beta^2 h^2 - n\alpha\beta\gamma gh \end{matrix} \right\} \quad (56).$$

Differentiate, with respect to f, the part within the brackets of (55) in which f appears, and multiply by $(\beta g + \gamma h)$, and we have

$$\left\{ \begin{matrix} l\alpha\beta^2 g^2 + 2l\beta^3 fg - 2l\beta\gamma^2 fg - l\alpha\beta\gamma gh - m\alpha\beta^2 g^2 + n\alpha\beta\gamma gh + \\ l\alpha\beta\gamma gh + 2l\beta^2\gamma hf - 2l\gamma^3 hf - l\gamma^2\alpha h^2 - m\alpha\beta\gamma gh + n\gamma^2\alpha h^2 \end{matrix} \right\} \quad . \quad (57).$$

Hence

$$(56) + (57) = \frac{d\mathrm{J}}{df} \quad . \quad . \quad . \quad . \quad . \quad . \quad (58).$$

Now, in this sum, we find in $\frac{d\mathrm{J}}{df}$—

$$\left.\begin{array}{lll} \text{Coefficient of} & f^2 & = 3l\alpha(\beta^2 - \gamma^2). \\ \quad " & g^2 & = \alpha\{(l - m)\beta^2 + m(\gamma^2 - \alpha^2)\} \\ \quad " & h^2 & = \alpha\{(n - l)\gamma^2 + n(\alpha^2 - \beta^2)\} \\ \quad " & gh & = 0 \\ \quad " & hf & = 2\gamma\{(n - l)\alpha^2 + l(\beta^2 - \gamma^2)\} \\ \quad " & fg & = 2\beta\{(l - m)\alpha^2 + l(\beta^2 - \gamma^2)\} \end{array}\right\} \quad . \quad . \quad . \quad . \quad (59).$$

And symmetrically for $\frac{d\mathrm{J}}{dg}$ and $\frac{d\mathrm{J}}{dh}$.

Now, it is well known that

$$\frac{d\mathrm{J}}{df} = 0; \quad \frac{d\mathrm{J}}{dg} = 0; \quad \frac{d\mathrm{J}}{dh} = 0 \quad . \quad . \quad . \quad . \quad . \quad (60).$$

These, with $u = 0$; $v = 0$; $w = 0$, give 6 equations in $f^2, g^2, h^2, gh, hf, fg$: whence we have the following determinant:—

$$\begin{vmatrix} l, & \alpha^2, & 0, & 3\alpha \,.\, l(\beta^2-\gamma^2), & \beta \,.\, \overline{(l-m)\alpha^2+l(\beta^2-\gamma^2)}, & \gamma \,.\, \overline{(n-l)\alpha^2+l(\beta^2-\gamma^2)} \\ m, & \beta^2, & 0, & \alpha \,.\, \overline{(l-m)\beta^2+m(\gamma^2-\alpha^2)}, & 3\beta \,.\, m(\gamma^2-\alpha^2), & \gamma \,.\, \overline{(m-n)\beta^2+m(\gamma^2-\alpha^2)} \\ n, & \gamma^2, & 0, & \alpha \,.\, \overline{(n-l)\gamma^2+n(\alpha^2-\beta^2)}, & \beta \,.\, \overline{(m-n)\gamma^2+n(\alpha^2-\beta^2)}, & 3\gamma \,.\, n(\alpha^2-\beta^2) \\ 0, & 2\beta\gamma, & \alpha, & 0, & 2\gamma \,.\, \overline{(m-n)\beta^2+m)\gamma^2-\alpha^2)}, & 2\beta \,.\, \overline{(m-n)\gamma^2+n(\alpha^2-\beta^2)} \\ 0, & 2\gamma\alpha, & \beta, & 2\gamma \,.\, \overline{(n-l)\alpha^2+l(\beta^2-\gamma^2)}, & 0, & 2\alpha \,.\, \overline{(n-l)\gamma^2+n(\alpha^2-\beta^2)} \\ 0, & 2\alpha\beta, & \gamma, & 2\beta \,.\, \overline{(l-m)\alpha^2+l(\beta^2-\gamma^2)}, & 2\alpha \,.\, \overline{(l-m)\beta^2+m(\gamma^2-\alpha^2)}, & 0, \end{vmatrix} = 0 \quad (61).$$

which is the equation of the locus required, and which I have not yet calculated out. (See *Postscript*,—where l^2, m^2, n^2, are written for l, m, n.)

Hitherto we have proceeded on the supposition of the *faisceaux* being curves of the first order,—that is, straight lines. We shall now suppose them to be conics, and we shall take the four special forms, which for brevity we shall call the circumscribing, the self-conjugate, the tri-tangent, and the bi-tangent conics.

SECTION II.—*The Faisceau of Homology being Circumscribing Conics.*

XIII. (1.) Required the envelope of a *faisceau* of circumscribing conics, while the centre of homology moves in a straight line.

Let the circumscribing conic of homology be

$$\frac{f^s}{\alpha} + \frac{g^s}{\beta} + \frac{h^s}{\gamma} = 0 = u \quad . \quad . \quad . \quad . \quad . \quad . \quad (62),$$

and the straight line in which the centre moves

$$l\alpha + m\beta + n\gamma = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (63),$$

$$i.e. \qquad \frac{l}{f} + \frac{m}{g} + \frac{n}{h} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (64).$$

Differentiating (62) and (64), introducing an indeterminate coefficient, adding, making the coefficient of the differentials to vanish as before, and substituting in (62) the values thus found for f, g, h, we have, as the envelope required,

$$\left(\frac{l^s}{\alpha}\right)^{\frac{1}{s+1}} + \left(\frac{m^s}{\beta}\right)^{\frac{1}{s+1}} + \left(\frac{n^s}{\gamma}\right)^{\frac{1}{s+1}} = 0 \quad . \quad . \quad . \quad . \quad . \quad (65).$$

Giving to s the appropriate values, -2, -3, $-\frac{3}{2}$; we find as follows:—

XIV. If the centre of homology move in a straight line, the *faisceau* of circumscribing conics

$$\frac{1}{f^2\alpha} + \frac{1}{g^2\beta} + \frac{1}{h^2\gamma} = 0$$

will envelope the straight line

$$l^2\alpha + m^2\beta + n^2\gamma = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (66).$$

XV. If the centre of homology move in a straight line, the *faisceau* of circumscribing conics

$$\frac{1}{f^3\alpha} + \frac{1}{g^3\beta} + \frac{1}{h^3\gamma} = 0$$

will envelope the tri-tangent conic

$$\pm (l^3\alpha)^{\frac{1}{2}} \pm (m^3\beta)^{\frac{1}{2}} \pm (n^3\gamma)^{\frac{1}{2}} = 0 \quad . \quad . \quad . \quad . \quad (67).$$

XVI. If the centre of homology move in a straight line, the *faisceau* of circumscribing conics

$$\frac{1}{f^{\frac{3}{2}}\alpha} + \frac{1}{g^{\frac{3}{2}}\beta} + \frac{1}{h^{\frac{3}{2}}\gamma} = 0$$

will envelope the self-conjugate conic,

$$l^3\alpha^2 + m^3\beta^2 + n^3\gamma^2 = 0 \quad . \quad . \quad . \quad . \quad (68).$$

XVII. (2.) Required the envelope of a *faisceau* of circumscribing conics, when the centre of homology moves in a given circumscribing conic.

In this case, instead of (64), we have

$$lf + mg + nh = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (69).$$

If, therefore, instead of f, g, h, we take as variables their inverses, and consider s as negative, our new variables will be involved precisely as in (62), and (64) and (65) will represent the envelope we now seek, provided for s we substitute $-s$, which gives

$$(l^s\alpha)^{\frac{1}{s-1}} + (m^s\beta)^{\frac{1}{s-1}} + (n^s\gamma)^{\frac{1}{s-1}} = 0 \quad . \quad . \quad . \quad (70).$$

Therefore, giving to s the appropriate values 2, 3, $\frac{3}{2}$; we find as follows :—

XVIII. If the centre of homology describe a circumscribing conic, the *faisceau* of circumscribing conics

$$\frac{f^2}{\alpha} + \frac{g^2}{\beta} + \frac{h^2}{\gamma} = 0$$

will envelope the straight line

$$l^2\alpha + m^2\beta + n^2\gamma = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (71).$$

XIX. If the centre of homology describe a circumscribing conic, the *faisceau* of circumscribing conics

$$\frac{f^3}{\alpha} + \frac{g^3}{\beta} + \frac{h^3}{\gamma} = 0$$

will envelope the tri-tangent conic

$$\pm (l^3\alpha)^{\frac{1}{2}} \pm (m^3\beta)^{\frac{1}{2}} \pm (n^3\gamma)^{\frac{1}{2}} = 0 \quad . \quad . \quad . \quad . \quad . \quad (72).$$

XX. If the centre of homology describe a circumscribing conic, the *faisceau* of circumscribing conics

$$\frac{f^{\frac{3}{2}}}{\alpha} + \frac{g^{\frac{3}{2}}}{\beta} + \frac{h^{\frac{3}{2}}}{\gamma} = 0$$

will envelope the self-conjugate conic

$$l^3\alpha^2 + m^3\beta^2 + n^3\gamma^2 = 0 \quad . \quad . \quad . \quad . \quad (73).$$

XXI. *Cor.* From the preceding we see, that since the inverse centre of homology describes a circumscribing conic, when the direct centre describes a straight line, and *vice versa*, it follows that the direct and inverse *faisceaux* of circumscribing conics have the same envelope. Compare (66) and (71); (67) and (72); (68) and (73).

XXII. (3.) Required the envelope of a *faisceau* of circumscribing conics, when the centre of homology moves in a given self-conjugate conic.

In this case, instead of (64), we have

$$\frac{l}{f^2} + \frac{m}{g^2} + \frac{n}{h^2} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (74).$$

If, therefore, instead of f, g, h, we take as variables their squares, and consider s as having *half* its former value, our new variables will again be involved precisely as in (62) and (64), and (65) will represent the envelope we now seek, provided for s we read $\frac{1}{2}s$, which gives

$$\left(\frac{l^s}{\alpha^2}\right)^{\frac{1}{s+2}} + \left(\frac{m^s}{\beta^2}\right)^{\frac{1}{s+2}} + \left(\frac{n^s}{\gamma^2}\right)^{\frac{1}{s+2}} = 0 \quad . \quad . \quad . \quad . \quad (75).$$

Hence giving s the appropriate values, -4; -3; -6: we have as follows:—

XXIII. If the centre of homology move in the self-conjugate conic,

$$l\alpha^2 + m\beta^2 + n\gamma^2 = 0 ,$$

the *faisceau* of circumscribing conics

$$\frac{1}{f^4\alpha} + \frac{1}{g^4\beta} + \frac{1}{h^4\gamma} = 0$$

will envelope the straight line

$$l^2\alpha + m^2\beta + n^2\gamma = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (76).$$

XXIV. If the centre of homology move in the same self-conjugate conic, the *faisceau* of circumscribing conics

$$\frac{1}{f^3\alpha} + \frac{1}{g^3\beta} + \frac{1}{h^3\gamma} = 0$$

will envelope another self-conjugate conic, namely,

$$l^3\alpha^2 + m^3\beta^2 + n^3\gamma^2 = 0 \quad . \quad . \quad . \quad . \quad . \quad (77).$$

XXV. If the centre of homology move in the same self-conjugate conic, the *faisceau* of circumscribing conics

$$\frac{1}{f^6\alpha} + \frac{1}{g^6\beta} + \frac{1}{h^6\gamma} = 0$$

will envelope the tri-tangent conic

$$\pm\left(\frac{\alpha}{l^3}\right)^{\frac{1}{2}} \pm \left(\frac{\beta}{m^3}\right)^{\frac{1}{2}} \pm \left(\frac{\gamma}{n^3}\right)^{\frac{1}{2}} = 0 \quad . \quad . \quad . \quad . \quad (78).$$

XXVI. (4.) Required the envelope of a *faisceau* of circumscribing conics, when the centre of homology moves in a given bi-tangent conic, $k^2\alpha^2 = \beta\gamma$.

Here we have

and

$$\left.\begin{array}{l} \dfrac{f^s}{\alpha} + \dfrac{g^s}{\beta} + \dfrac{h^s}{\gamma} = 0 = u \\ f^2 - k^2gh \quad = 0 \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (79).$$

Hence, differentiating, and merging the factor $(s - 1)$ in the indeterminate coefficient, which we may suppress, as it divides out in the final substitution, we have

$$f^{s-2} = 2\alpha \quad . \quad . \quad . \quad . \quad . \quad . \quad (1')$$

$$g^{s-1} = -k^2h\beta \quad . \quad . \quad . \quad . \quad . \quad . \quad (2')$$

$$h^{s-1} = -k^2g\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad (3')$$

$(2') \times (3')$ $$(gh)^{s-2} = k^4\beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad (4')$$

$(2') \div (3')$ $$\left(\frac{g}{h}\right)^s = \frac{\beta}{\gamma} \quad . \quad . \quad . \quad . \quad . \quad . \quad (5')$$

$(4')$ gives $$(gh)^s = k^{\frac{4s}{s-2}}(\beta\gamma)^{\frac{s}{s-2}} \quad . \quad . \quad . \quad . \quad . \quad (6')$$

$(5') \times (6')$ $$g^{2s} = k^{\frac{4s}{s-2}}(\beta\gamma)^{\frac{s}{s-2}} \times \frac{\beta}{\gamma} \quad . \quad . \quad . \quad . \quad (7')$$

$$\frac{g^{2s}}{\beta^2} = \frac{k^{\frac{4s}{s-2}}(\beta\gamma)^{\frac{s}{s-2}}}{\beta\gamma}$$

$$= k^{\frac{4s}{s-2}}(\beta\gamma)^{\frac{2}{s-2}} \quad . \quad . \quad . \quad . \quad . \quad (8')$$

$$\frac{g^s}{\beta} = \frac{h^s}{\gamma} = \pm k^{\frac{2s}{s-2}}(\beta\gamma)^{\frac{1}{s-2}} \quad . \quad . \quad (9')$$

By $(1')$ $$\frac{f^s}{\alpha} = 2^{\frac{s}{s-2}}\alpha^{\frac{2}{s-2}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (10')$$

By (79), $(9')$, $(10')$ $$2^{\frac{s}{s-2}}\alpha^{\frac{2}{s-2}} = \pm 2k^{\frac{2s}{s-2}}(\beta\gamma)^{\frac{1}{s-2}}$$

$$\alpha^2 = \left(\frac{k^s}{2}\right)^2 \beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (80);$$

the required envelope, which is another bi-tangent conic, very neatly related to the former.

XXVII. Required the locus of the fourth point of intersection of the direct and inverse circumscribing conics of homology, when the centre of homology describes a given circumscribing conic.

[It may be noted here, that the direct and inverse circumscribing conics of homology are two members of a *faisceau* of *courbes pivotantes*, passing through the $2^2 = 4$ points, namely, the three angles of the triangle and the point whose locus is now sought. If the direct circumscribing conic is

$$\frac{f}{\alpha} + \frac{g}{\beta} + \frac{h}{\gamma} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (81),$$

the inverse is

$$\frac{1}{f\alpha} + \frac{1}{g\beta} + \frac{1}{h\gamma} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (82);$$

and any member of the *faisceau pivotant* to which they belong will be represented by

$$\frac{f + \frac{\lambda}{f}}{\alpha} + \frac{g + \frac{\lambda}{g}}{\beta} + \frac{h + \frac{\lambda}{h}}{\gamma} = 0;$$

where λ may have any value from 0 to $\pm\infty$, the direct conic being represented when $\lambda = 0$, and the inverse conic when $\lambda = \pm\infty$].

To find the locus required, we have to eliminate f, g, h, between (81) and (82) and

$$lf + mg + nh = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (83).$$

Now, these variables, the parameters, are involved in these three equations precisely as in (42), (43), (44); and the two sets of equations are identical, if for α, β, γ we read their inverses. Hence inverting α, β, γ in (47), we get the locus required, namely,

$$mn\left(\frac{\beta\gamma}{\alpha^2} - \frac{\beta^2 + \gamma^2}{\beta\gamma}\right) + nl\left(\frac{\gamma\alpha}{\beta^2} - \frac{\gamma^2 + \alpha^2}{\gamma\alpha}\right) + lm\left(\frac{\alpha\beta}{\gamma^2} - \frac{\alpha^2 + \beta^2}{\alpha\beta}\right)$$

$$+ l^2 + m^2 + n^2 = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (84);$$

a curve of the 6th order, and which circumscribes the triangle.

XXVIII. Required the locus of the same point when the centre of homology moves in the bi-tangent conic $k^2\alpha^2 = \beta\gamma$. Here again the elimination is the same, as in (48′) and α, β, γ have to be inverted in (49). This gives

$$\alpha^2\left(\frac{\beta^2 - \gamma^2}{\beta^3\gamma^3}\right)^2 - \frac{\beta^2 + \gamma^2}{\beta\gamma} + k^2 + \frac{1}{k^2} = 0 \quad . \quad . \quad . \quad (85),$$

and which is the same curve if the bi-tangent conic were not $k^2\alpha^2 = \beta\gamma$, but $\alpha^2 = k^2\beta\gamma$.

XXIX. Required the locus of the same point when the centre of homology moves in a self-conjugate conic.

The same consideration as that used in the last two propositions shows that this locus is to be got by inverting α, β, γ in the determinant (61).

SECTION III.—*The Faisceau of Homology being Self-Conjugate Conics.*

XXX. (1.) Required the envelope of a *faisceau* of self-conjugate conics, when the centre of homology moves in a straight line.

Here we have

$$f^s\alpha^2 + g^s\beta^2 + h^s\gamma^2 = 0 = u \quad . \quad . \quad . \quad . \quad (86),$$

and

$$\frac{l}{f} + \frac{m}{g} + \frac{n}{h} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (87).$$

Proceeding as before, the envelope is

$$(l^s\alpha^2)^{\frac{1}{s+1}} + (m^s\beta^2)^{\frac{1}{s+1}} + (n^s\gamma^2)^{\frac{1}{s+1}} = 0 \quad . \quad . \quad . \quad (88).$$

Giving s appropriate values, namely, 1; -3; 3: we have as follows:—

XXXI. If the centre of homology move in a straight line, the *faisceau* of self-conjugate conics

$$f\alpha^2 + g\beta^2 + h\gamma^2 = 0$$

will envelope, or inscribe, the quadrilateral represented by

$$\pm (l)^{\frac{1}{2}}\alpha \pm (m)^{\frac{1}{2}}\beta \pm (n)^{\frac{1}{2}}\gamma = 0 \quad . \quad . \quad . \quad . \quad (89).$$

XXXII. If the centre of homology move in a straight line, the *faisceau* of self-conjugate conics

$$\frac{\alpha^2}{f^3} + \frac{\beta^2}{g^3} + \frac{\gamma^2}{h^3} = 0$$

will envelope the circumscribing conic,

$$\frac{l^{\frac{3}{2}}}{\alpha} + \frac{m^{\frac{3}{2}}}{\beta} + \frac{n^{\frac{3}{2}}}{\gamma} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (90).$$

XXXIII. If the centre of homology move in a straight line, the *faisceau* of self-conjugate conics

$$f^3\alpha^2 + g^3\beta^2 + h^3\gamma^2 = 0$$

will envelope the tri-tangent conic

$$\pm (l^{\frac{3}{2}}\alpha)^{\frac{1}{2}} \pm (m^{\frac{3}{2}}\beta)^{\frac{1}{2}} \pm (n^{\frac{3}{2}}\gamma)^{\frac{1}{2}} = 0 \quad . \quad . \quad . \quad . \quad (91).$$

XXXIV. (2.) Required the envelope of a *faisceau* of self-conjugate conics when the centre of homology moves in a circumscribing conic.

On the same consideration as before, s in (88) must be read $-s$, which gives the required envelope

$$\left(\frac{l^s}{\alpha^2}\right)^{\frac{1}{s-1}} + \left(\frac{m^s}{\beta^2}\right)^{\frac{1}{s-1}} + \left(\frac{n^s}{\gamma^2}\right)^{\frac{1}{s-1}} = 0 \quad . \quad . \quad . \quad (92).$$

Giving appropriate values to s, viz., -1; 3; -3: we find as follows :—

XXXV. If the centre of homology move in a conic circumscribing the triangle, the *faisceau* of self-conjugate conics

$$\frac{\alpha^2}{f} + \frac{\beta^2}{g} + \frac{\gamma^2}{h} = 0$$

will envelope, or inscribe, the quadrilateral represented by

$$\pm (l)^{\frac{1}{2}}\alpha \pm (m)^{\frac{1}{2}}\beta \pm (n)^{\frac{1}{2}}\gamma = 0 \quad . \quad . \quad . \quad . \quad (93).$$

XXXVI. If the centre of homology move in a conic circumscribing the triangle, the *faisceau* of self-conjugate conics

$$f^3\alpha^2 + g^3\beta^2 + h^3\gamma^2 = 0$$

will envelope the circumscribing conic

$$\frac{l^{\frac{3}{2}}}{\alpha} + \frac{m^{\frac{3}{2}}}{\beta} + \frac{n^{\frac{3}{2}}}{\gamma} = 0 \quad . \quad . \quad . \quad . \quad . \quad (94).$$

XXXVII. If the centre of homology move in a conic circumscribing the triangle, the *faisceau* of self-conjugate conics

$$\frac{\alpha^2}{f^3} + \frac{\beta^2}{g^3} + \frac{g^2}{h^3} = 0$$

will envelope the tri-tangent conic

$$\pm (l^{\frac{3}{2}}\alpha)^{\frac{1}{2}} \pm (m^{\frac{3}{2}}\beta)^{\frac{1}{2}} \pm (n^{\frac{3}{2}}\gamma)^{\frac{3}{2}} = 0 \quad . \quad . \quad . \quad . \quad (95).$$

XXXVIII. *Cor.* Since the inverse centre of homology moves in a circumscribing conic when the direct centre moves in a straight line, and *vice versa*, it follows, as before, that the direct and inverse *faisceaux* have the same envelope. Compare (89), (93); (90), (94); (91), (95).

XXXIX. (3.) Required the envelope of a *faisceau* of self-conjugate conics when the centre moves in a self-conjugate conic.

Here we have $$f^s\alpha^2 + g^s\beta^2 + h^s\gamma^2 = 0,$$

and $$\frac{l}{f^2} + \frac{m}{g^2} + \frac{n}{h^2} = 0;$$

and, by the consideration formerly employed, reading for s in (88) $\frac{s}{2}$ we have the envelope

$$(l^s\alpha^4)^{\frac{1}{s+2}} + (m_s\beta^4)^{\overline{s+2}} + (m_s\gamma^4)^{\frac{1}{s+2}} = 0 \quad . \quad . \quad . \quad . \quad (96).$$

Giving appropriate values to s, viz., 2; -6; 6: we find as follows :—

XL. If the centre of homology move in a self-conjugate conic, the *faisceau* of self-conjugate conics

$$f^2\alpha^2 + g^2\beta^2 + h^2\gamma^2 = 0$$

will inscribe the quadrilateral

$$\pm \sqrt{l}\alpha \pm \sqrt{m}\beta \pm \sqrt{n}\gamma = 0 \quad (97).$$

XLI. If the centre of homology move in a self-conjugate conic, the *faisceau* of self-conjugate conics

$$\frac{\alpha^2}{f^6} + \frac{\beta^2}{g^6} + \frac{\gamma^2}{h^6} = 0$$

will envelope the circumscribing conic

$$\frac{l^{\frac{3}{2}}}{\alpha} + \frac{m^{\frac{3}{2}}}{\beta} + \frac{n^{\frac{3}{2}}}{\gamma} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (98).$$

XLII. If the centre of homology move in a self-conjugate conic, the *faisceau* of self-conjugate conics

$$f^6\alpha^2 + g^6\beta^2 + h^6\gamma^2 = 0$$

will envelope the tri-tangent conic

$$\pm\left(l^{\frac{3}{2}}\alpha\right)^{\frac{1}{2}} \pm \left(m^{\frac{3}{2}}\beta\right)^{\frac{1}{2}} \pm \left(n^{\frac{3}{2}}\gamma\right)^{\frac{1}{2}} = 0 \quad . \quad . \quad . \quad . \quad . \quad (99).$$

XLIII. (4.) Required the envelope of a *faisceau* of self-conjugate conics when the centre moves in the bi-tangent conic

$$k^2\alpha^2 = \beta\gamma .$$

Here we have $\qquad f^s\alpha^2 + g^s\beta^2 + h^s\gamma^2 = 0 = u.$

and $\qquad f^2 - k^2gh = 0.$

Differentiating as before, suppressing the factor $(s - 1)$ as merged in the indeterminate coefficient which is suppressed because it divides out in the final substitution, we have

$$f^{s-1}\alpha^2 = 2f \therefore f^s\alpha^2 = \frac{2^{\frac{s}{s-2}}}{\alpha^{\frac{4}{s-2}}} \quad . \quad . \quad . \quad . \quad . \quad . \quad [1]$$

$$g^{s-1}\beta^2 = -k^2h$$

$$h^{s-1}\gamma^2 = -k^2g$$

$$\therefore \quad (gh)^{s-2} = \frac{k^4}{(\beta\gamma)^2} \therefore (gh)^{\frac{s}{2}} = \pm\left(\frac{k^2}{\beta\gamma}\right)^{\frac{s}{s-2}} \quad . \quad . \quad . \quad . \quad [2]$$

and $$\left(\frac{g}{h}\right)^{\frac{s}{2}} = \pm\frac{\gamma}{\beta} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad [3]$$

and $$g^s\beta^2 = h^s\gamma^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad [4]$$

[2] × [3] $$g^s = (k^2)^{\frac{s}{s-2}} . (\beta\gamma)^{-\frac{s}{s-2}} . \frac{\gamma}{\beta}$$

$$\therefore \quad g^s\beta^2 = (k^2)^{\frac{s}{s-2}} . (\beta\gamma)^{-\frac{2}{s-2}}$$

Substituting in u; $$\frac{2^{\frac{s}{s-2}}}{\alpha^{\frac{4}{s-2}}} = -2 . \left(\frac{k^s}{\beta\gamma}\right)^{\frac{2}{s-2}}$$

$$\therefore \quad \frac{2^s}{\alpha^4} = \pm 2^{s-2} . \left(\frac{k^s}{\beta\gamma}\right)^2 : \qquad \pm \text{ as } s \text{ is even or odd.}$$

If s is even $\qquad k^s\alpha^2 = 2\beta\gamma$: — the envelope required.

XLIV. It is evident that if the centre move in this last bi-tangent, the envelope will be

$$k^{\frac{s^2}{2}} \alpha^2 = 2^{\frac{s+2}{2}} \beta\gamma .$$

If, again, in this, the envelope will be

$$k^{\frac{s^3}{4}} \alpha^2 = 2^{\frac{s^2+2s+4}{4}} \beta\gamma .$$

And so on *en suite;* the law being obvious, namely, that the t^{th} envelope will be

$$k^{\frac{s^t}{2^{t-1}}} \alpha^2 = \left(2 \, . ^{\frac{s^{t-1} + 2s^{t-2} + 4s^{t-3} + 8s^{t-4} + \&c.}{2^{t-1}}}\right) \beta\gamma \quad . \quad . \quad . \quad (100).$$

or

$$\frac{k^{2 \cdot \left(\frac{s}{2}\right)^t}}{2 .^{\frac{1-\left(\frac{s}{2}\right)^t}{1-\frac{s}{2}}}} \, . \, \alpha^2 = \beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (100').$$

—a somewhat curious result.

SECTION IV.—*The Faisceau of Homology being Tri-tangent Conics.*

XLV. (1.) Required the envelope of a *faisceau* of tri-tangent conics when the centre of homology moves in a straight line.

Here we have

$$\left.\begin{aligned} \pm f^s \, . \, \alpha^{\frac{1}{2}} \pm g^s \beta^{\frac{1}{2}} \pm h^s \gamma^{\frac{1}{2}} &= 0 \\ \text{and} \qquad \frac{l}{f} + \frac{m}{g} + \frac{n}{h} &= 0 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (101).$$

and the envelope, found as before, is

$$\pm \left(l^{2s} . \alpha\right)^{\frac{1}{2(s+1)}} \pm \left(m^{2s} . \beta\right)^{\frac{1}{2(s+1)}} \pm \left(n^{2s} \gamma\right)^{\frac{1}{2(s+1)}} = 0 \quad . \quad . \quad . \quad . \quad (101').$$

Hence, giving to s the appropriate values, namely, $-\frac{1}{2}$; $-\frac{3}{2}$; $-\frac{3}{4}$: we find as follows:—

XLVI. If the centre of homology move in a straight line, the *faisceau* of tri-tangent conics

$$\pm \sqrt{\frac{\alpha}{f}} \pm \sqrt{\frac{\beta}{g}} \pm \sqrt{\frac{\gamma}{h}} = 0$$

will envelope the straight line

$$\pm \frac{\alpha}{l} \pm \frac{\beta}{m} \pm \frac{\gamma}{n} = 0 \quad . \quad . \quad . \quad . \quad . \quad (102).$$

XLVII. If the centre of homology move in a straight line, the *faisceau* of tri-tangent conics

$$\pm \sqrt{\frac{\alpha}{f^3}} \pm \sqrt{\frac{\beta}{g^3}} \pm \sqrt{\frac{\gamma}{h^3}} = 0$$

will envelope the circumscribing conic

$$\frac{l^3}{\alpha} + \frac{m^3}{\beta} + \frac{n^3}{\gamma} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \qquad (103).$$

XLVIII. If the centre of homology move in a straight line, the *faisceau* of tri-tangent conics

$$\pm \frac{\alpha^{\frac{1}{2}}}{f^{\frac{3}{4}}} \pm \frac{\beta^{\frac{1}{2}}}{\gamma^{\frac{3}{4}}} \pm \frac{\gamma^{\frac{1}{2}}}{h^{\frac{3}{4}}} = 0$$

will envelope the self-conjugate conic

$$\frac{\alpha^2}{l^3} + \frac{\beta^2}{m^3} + \frac{\gamma^2}{n^3} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \qquad (104).$$

XLIX. In general; in order that the envelope may be of the form

$$\lambda\alpha^t + \mu\beta^t + \nu\gamma^t = 0$$

we must evidently have $\qquad \dfrac{1}{2s + 2} = t \text{ or } s = \dfrac{1 - 2t}{2t}$

And the *faisceau*

$$\pm f^{\frac{1-2t}{2t}} \alpha^{\frac{1}{2}} \pm g^{\frac{1-2t}{2t}} \beta^{\frac{1}{2}} \pm h^{\frac{1-2t}{2t}} \gamma^{\frac{1}{2}} = 0$$

gives the envelope

$$\pm l^{1-2t} \, . \, \alpha^t \pm m^{1-2t} \, . \, \beta^t \pm n^{1-2t} \, \gamma = 0 \quad . \quad . \quad . \quad . \qquad (105).$$

L. (2.) Required the envelope of a *faisceau* of tri-tangent conics when the centre of homology moves in a conic circumscribing the triangle.

Here, by the same consideration as we have already employed in like cases, the envelope required is got from (101′) by reading $-s$ for s, namely,

$$\pm \left(\frac{l^{2s}}{\alpha}\right)^{\frac{1}{2(s-1)}} \pm \left(\frac{m^{2s}}{\beta}\right)^{\frac{1}{2(s-1)}} \pm \left(\frac{n^{2s}}{\gamma}\right)^{\frac{1}{2(s-1)}} = 0 \quad . \quad . \quad . \qquad (106).$$

Hence, giving s the appropriate values $\frac{1}{2}$; $\frac{3}{2}$; $\frac{3}{4}$: we find as follows:—

LI. If the centre of homology move in a circumscribing conic, the *faisceau* of tri-tangent conics

$$\pm \sqrt{f\alpha} \pm \sqrt{g\beta} \pm \sqrt{h\gamma} = 0$$

will envelope the straight line

$$\pm \frac{\alpha}{l} \pm \frac{\beta}{m} \pm \frac{\gamma}{n} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \qquad (107).$$

LII. If the centre of homology move in a circumscribing conic, the *faisceau* of tri-tangent conics

$$\pm \sqrt{f^3\alpha} \pm \sqrt{g^3\beta} \pm \sqrt{h^3\gamma} = 0$$

will envelope the circumscribing conic

$$\frac{l^3}{\alpha} + \frac{m^3}{\beta} + \frac{n^3}{\gamma} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \qquad (108).$$

LIII. If the centre of homology move in a circumscribing conic, the *faisceau* of tri-tangent conics

$$\pm \sqrt{f^{\frac{3}{2}}\alpha} \pm \sqrt{g^{\frac{3}{2}}\beta} \pm \sqrt{h^{\frac{3}{2}}\gamma} = 0$$

will envelope the self-conjugate conic

$$\frac{\alpha^2}{l^3} + \frac{\beta^2}{m^3} + \frac{\gamma^2}{n^3} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (109).$$

LIV. (3.) Required the envelope of a *faisceau* of tri-tangent conics when the centre of homology moves also in a given tri-tangent conic, say,

$$\pm l\alpha^{\frac{1}{2}} \pm m\beta^{\frac{1}{2}} \pm n\gamma^{\frac{1}{2}} = 0.$$

Here we have and

$$\left.\begin{aligned} &\pm f^s\alpha^{\frac{1}{2}} \pm g^s\beta^{\frac{1}{2}} \pm h^s\gamma^{\frac{1}{2}} = 0 \\ &\pm \frac{l}{f^{\frac{1}{2}}} \pm \frac{m}{g^{\frac{1}{2}}} \pm \frac{n}{h^{\frac{1}{2}}} = 0 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (110).$$

Now, $f^{\frac{1}{2}}$, $g^{\frac{1}{2}}$, $h^{\frac{1}{2}}$ are involved in equations (110) precisely as f, g, h are involved in equations (101), provided in equations 101 we substitute $2s$ for s. Making, therefore, this substitution in (101′), we have the envelope now required, namely,

$$\pm (l^{4s}\alpha)^{\frac{1}{2(2s+1)}} \pm (m^{4s}\beta)^{\frac{1}{2(2s+1)}} \pm (n^{4s}\gamma)^{\frac{1}{2(2s+1)}} = 0 \quad . \quad . \quad . \quad (111).$$

Hence, giving s the appropriate values, $-\frac{1}{4}$; $-\frac{3}{4}$; $-\frac{3}{8}$: we find as follows:—

LV. If the centre of homology move in a tri-tangent conic, the *faisceau* of tri-tangent conics

$$\pm \sqrt{\frac{\alpha}{f^{\frac{1}{2}}}} \pm \sqrt{\frac{\beta}{g^{\frac{1}{2}}}} \pm \sqrt{\frac{\gamma}{h^{\frac{1}{2}}}} = 0$$

will envelope the straight line

$$\pm \frac{\alpha}{l} \pm \frac{\beta}{m} \pm \frac{\gamma}{n} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (112).$$

LVI. If the centre of homology move in a tri-tangent conic, the *faisceau* of tri-tangent conics

$$\pm \sqrt{\frac{\alpha}{f^{\frac{3}{2}}}} \pm \sqrt{\frac{\beta}{g^{\frac{3}{2}}}} \pm \sqrt{\frac{\gamma}{h^{\frac{3}{2}}}} = 0$$

will envelope the circumscribing conic

$$\pm \frac{l^3}{\alpha} \pm \frac{m^3}{\beta} \pm \frac{n^3}{\gamma} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (113).$$

LVII. If the centre of homology move in a tri-tangent conic, the *faisceau* of tri-tangent conics

$$\pm \sqrt{\frac{\alpha}{f^{\frac{3}{4}}}} \pm \sqrt{\frac{\beta}{g^{\frac{3}{4}}}} \pm \sqrt{\frac{\gamma}{h^{\frac{3}{4}}}} = 0$$

will envelope the self-conjugate conic

$$\pm \frac{\alpha^2}{l^3} \pm \frac{\beta^2}{m^3} \pm \frac{\gamma^2}{n^3} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (114).$$

LVIII. (4.) Required the envelope of a *faisceau* of tri-tangent conics when the centre of homology moves in a bi-tangent conic, say

$$k^2 \alpha^2 = \beta\gamma.$$

Here we have

$$\pm f^s \alpha^{\frac{1}{2}} \pm g^s \beta^{\frac{1}{2}} \pm h^s \gamma^{\frac{1}{2}} = 0,$$

and

$$f^2 - k^2 g h = 0.$$

Proceeding as before, the envelope is found to be

$$\left(\frac{k^s}{2}\right)^4 \alpha^2 = \beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad (115).$$

another bi-tangent elegantly related to the former.

LIX. If the centre of homology move in this bi-tangent, the bi-tangent envelope will of course be

$$\left(\frac{k^{2s^2}}{2^{2s+1}}\right)^4 \alpha^2 = \beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad (116).$$

If in this, the envelope will be

$$\left(\frac{k^{4s^3}}{2^{4s^2+2s+1}}\right)^4 \alpha^2 = \beta\gamma \quad . \quad . \quad . \quad . \quad . \quad (117).$$

If in this, the envelope will be

$$\left(\frac{k^{8s^4}}{2^{8s^3+4s^2+2s+1}}\right)^4 \alpha^2 = \beta\gamma \quad . \quad . \quad . \quad . \quad (118).$$

And so on, *en suite*, the law being obvious,—the t^{th} envelope being

$$\left(\frac{k^{\frac{(2s)^t}{2}}}{2^{\frac{1-(2s)^t}{1-2s}}}\right)^4 . \alpha^2 = \beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad (118').$$

LX. (5.) Required the envelope of a *faisceau* of tri-tangent conics when the centre of homology moves in a self-conjugate conic, say $l\alpha^2 + m\beta^2 + n\gamma^2 = 0$.

By a similar consideration to that used in LIV., in (101′) for s read $\frac{1}{2}s$, and we have the envelope required, namely

$$\pm (l^s \alpha)^{\frac{1}{s+2}} \pm (m^s \beta)^{\frac{1}{s+2}} \pm (n^s \gamma)^{\frac{1}{s+2}} = 0 \quad . \quad . \quad . \quad (119).$$

Hence, giving to s appropriate values, namely, -1; $-\frac{3}{2}$; -3: we find as follows:—

LXI. If the centre of homology move in a self-conjugate conic, the *faisceau* of tri-tangent conics

$$\pm\frac{\alpha^{\frac{1}{3}}}{f}\pm\frac{\beta^{\frac{1}{3}}}{g}\pm\frac{\gamma^{\frac{1}{3}}}{h}=0$$

will envelope the straight line

$$\frac{\alpha}{l}+\frac{\beta}{m}+\frac{\gamma}{n}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (120).$$

LXII. If the centre of homology move in a self-conjugate conic, the *faisceau* of tri-tangent conics

$$\pm\frac{\alpha^{\frac{1}{3}}}{f^3}\pm\frac{\beta^{\frac{1}{3}}}{g^3}\pm\frac{\gamma}{h^3}=0$$

will envelope the circumscribing conic

$$\frac{l^3}{\alpha}+\frac{m^3}{\beta}+\frac{n^3}{\gamma}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (121).$$

LXIII. If the centre of homology move in a self-conjugate conic, the *faisceau* of tri-tangent conics

$$\pm\sqrt{\frac{\alpha}{f^3}}\pm\sqrt{\frac{\beta}{g^3}}\pm\sqrt{\frac{\gamma}{h^3}}=0$$

will envelope the self-conjugate conic

$$\frac{\alpha^2}{l^3}+\frac{\beta^2}{m^3}+\frac{\gamma^2}{n^3}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (122).$$

SECTION V.—*The Faisceau of Homology being Bi-tangent Conics.*

LXIV. (1.) Required the envelope of a *faisceau* of bi-tangent conics when the centre of homology moves in a straight line.

Let the *faisceau* be

and the straight line

$$\left.\begin{aligned} f^{2s}\alpha^2-(gh)^s\beta\gamma&=0\\ l\alpha+m\beta+n\gamma&=0\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (123).$$

The envelope is

$$\alpha^2=\left(\frac{4mn}{l^2}\right)^s\beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad (124).$$

LXV. (2.) Required the envelope of a *faisceau* of bi-tangent conics when the centre of homology moves in a circumscribing conic. The envelope is

$$\alpha^2=\left(\frac{l^2}{4mn}\right)^s\beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad (125).$$

LXVI. (3.) Required the envelope of a *faisceau* of bi-tangent conics when the centre of homology moves in a self-conjugate conic. The envelope is

$$\alpha^2=\left(\frac{l^2}{4mn}\right)^{\frac{s}{s+2}}\beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad (126).$$

LXVII. (4.) Required the envelope of a *faisceau* of bi-tangent conics when the centre of homology moves in a tri-tangent conic. The envelope is

$$\alpha^2 = \left(\frac{4mn}{l^2}\right)^{\frac{2s}{2s+1}} \beta\gamma \quad . \quad . \quad . \quad . \quad . \qquad (127).$$

LXVIII. (5.) Required the envelope of a *faisceau* of bi-tangent conics when the centre of homology moves in the bi-tangent conic

$$\alpha^2 = k^2 \beta\gamma.$$

The envelope is

$$\alpha^2 = k^{2s} \beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \qquad (128).$$

If the centre of homology move in this conic, the envelope is

$$\alpha^2 = k^{2s^2} \beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \qquad (129).$$

If in this, the envelope is

$$\alpha^2 = k^{2s^3} \beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \qquad (130).$$

And the t^{th} envelope is

$$\alpha^2 = k^{2s^t} \beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \qquad (131).$$

If we equate the indices (inverted when necessary) of (124) and (126); (125) and (126); (124) and (127); (125) and (127); (126) and (127); we get the following somewhat elegant propositions:—

LXIX. Whether the centre moves in a straight line or in a self-conjugate conic, the *faisceau* of bi-tangent conics

$$(gh)^3 \alpha^2 = f^6 \beta\gamma$$

envelopes the same bi-tangent conic

$$(4mn)^3 \alpha^2 = l^6 \beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \qquad (132).$$

LXX. Whether the centre moves in a circumscribing or in a self-conjugate conic, the *faisceau* of bi-tangent conics

$$gh\alpha^2 = f^2 \beta\gamma$$

envelopes the same bi-tangent conic

$$l^2 \alpha^2 = 4mn \beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (133).$$

LXXI. Whether the centre moves in a straight line or in a tri-tangent conic, the *faisceau* of bi-tangent conics

$$f\alpha^2 = \sqrt{gh} \,.\, \beta\gamma$$

envelopes the same bi-tangent conic

$$l\alpha^2 = 2\sqrt{mn} \,.\, \beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (134).$$

LXXII. Whether the centre moves in a circumscribing conic or in a tri-tangent conic, the *faisceau* of bi-tangent conics

$$(gh)^{\frac{3}{2}}\alpha^2 = f^3\beta\gamma$$

envelopes the same bi-tangent conic

$$l^3\alpha^2 = (4mn)^{\frac{3}{2}}\beta\gamma \quad . \quad . \quad . \quad . \quad . \quad . \qquad (135).$$

LXXIII. Whether the centre of homology moves in a self-conjugate or a tri-tangent conic, the *faisceau* of bi-tangent conics

$$(gh)^{\frac{5}{4}}\alpha^2 = f^{\frac{5}{2}}\beta\gamma$$

envelopes the same bi-tangent conic

$$(l^2)^{\frac{5}{3}}\alpha^2 = (4mn)^{\frac{5}{3}}\beta\gamma \quad . \quad . \quad . \quad . \quad . \qquad (136).$$

I have only to add, that while the general problem of this paper becomes very difficult in the case of the general equation of the second degree, by reason of the complicated elimination that is necessary, I have no doubt that this difficulty might be evaded by reducing the general equation to one or other of the forms of the conic which have engaged our attention, by an appropriate transformation of the triangle of reference. If leisure permit, I should be glad to verify this supposition, as well as give certain extensions of the theory, in another paper.

Postscript.—While this is passing through the press, I find that SILVESTER'S "Dialytic method of Elimination as applied to a Ternary System of Equations" (see "Cambridge Mathematical Journal," vol. ii. p. 234), may be used more efficiently than the Jacobian in the last problem of Section I. Writing the equations (50), (52), (51), thus:—

$$\begin{aligned}
0 &= l^2 . f^2 + m^2 . g^2 + n^2 . h^2 + \quad * \quad * \quad * \quad . \quad . \quad [1],\\
0 &= \quad * \quad * \quad * \quad + \alpha . gh + \beta . hf + \gamma . fg \quad . \quad . \quad [2],\\
0 &= \alpha . f + \beta . g + \gamma . h \quad . \quad . \quad [3];
\end{aligned}$$

we have to find four equations in f^2, g^2, h^2, gh, hf, fg. Re-write the three just given (taking twice the second) thus:—

$$\begin{aligned}
0 &= (l^2 f) . f + (m^2 g) . g + (n^2 h) . h \quad . \quad . \quad [4],\\
0 &= (\beta h + \gamma g) . f + (\gamma f + \alpha h) . g + (\alpha g + \beta f) . h \quad . \quad . \quad [5],\\
0 &= (\alpha) . f + (\beta) . g + (\gamma) . h \quad . \quad . \quad [6].
\end{aligned}$$

Eliminating f, g, h, we have, dropping the parentheses,

$$\begin{vmatrix} l^2 f & m^2 g & n^2 h \\ \beta h + \gamma g & \gamma f + \alpha h & \alpha g + \beta f \\ \alpha & \beta & \gamma \end{vmatrix} = 0 \quad . \quad . \quad . \quad [7];$$

that is:—

$$0 = l^2(\beta^2-\gamma^2).f^2 + m^2(\gamma^2-\alpha^2).g^2 + n^2(\alpha^2-\beta^2).h^2 + (m^2-n^2)\beta\gamma.gh + (n^2-l^2)\gamma\alpha.hf + (l^2-m^2)\alpha\beta.fg \quad [8].$$

Multiplying [3] successively by f, g, h, we have

$$\begin{array}{lllllll} 0 = \alpha.f^2 & * & * & * & +\gamma.hf & +\beta.fg & \quad [9]; \\ 0 = * & \beta.g^2 & + & \gamma.gh & + & \alpha.fg & \quad [10]; \\ 0 = * & * & \gamma.h^2 & +\beta.gh & +\alpha.hf & * & \quad [11]. \end{array}$$

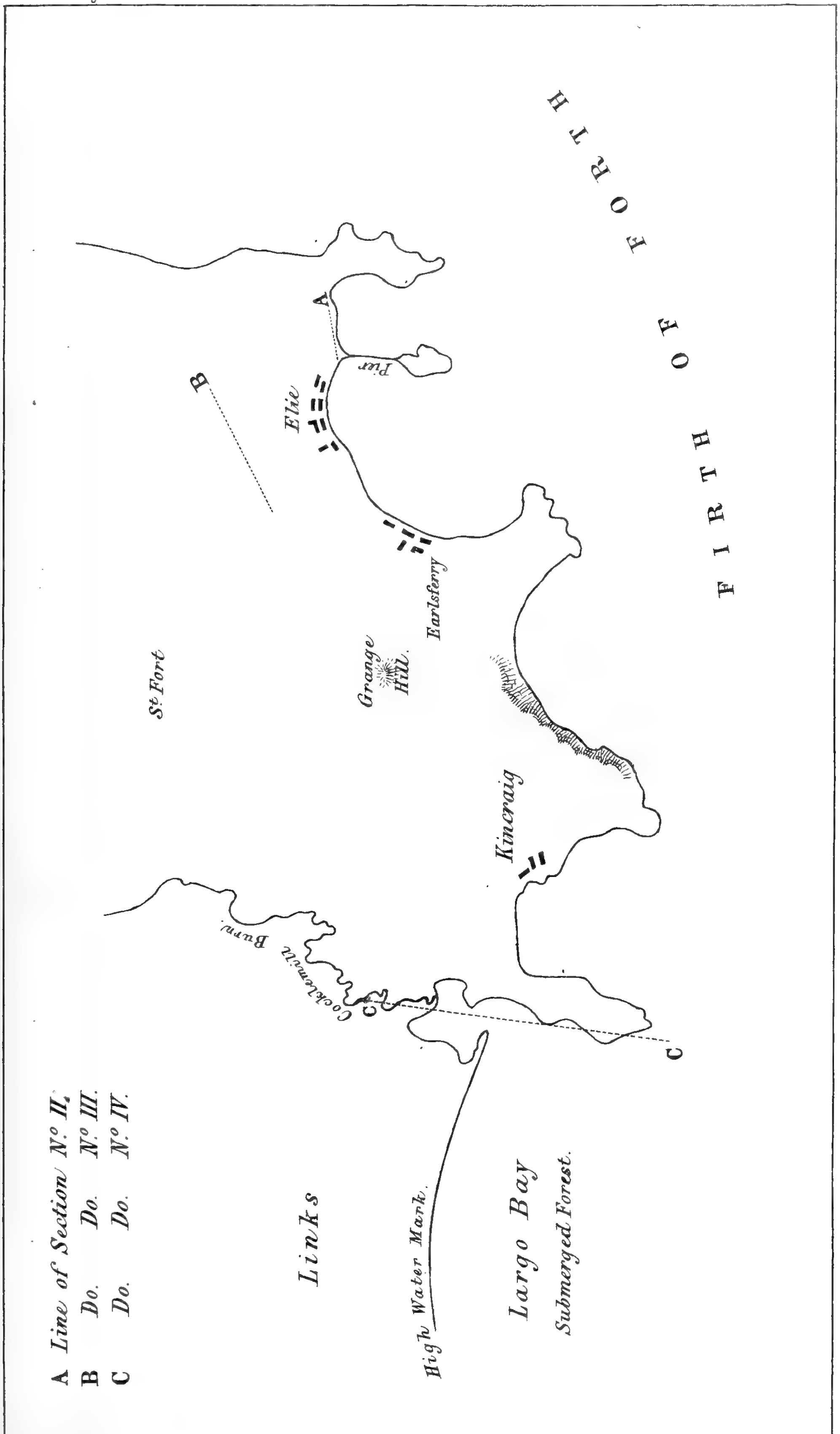
Forming now the determinant of elimination from equations [1], [2], [8], [9], [10], [11], we have, cleared of extraneous factors, the equation of the locus sought, namely,

$$\begin{vmatrix} l^2 & 0 & l^2(\beta^2-\gamma^2) & \alpha & 0 & 0 \\ m^2 & 0 & m^2(\gamma^2-\alpha^2) & 0 & \beta & 0 \\ n^2 & 0 & n^2(\alpha^2-\beta^2) & 0 & 0 & \gamma \\ 0 & \alpha & (m^2-n^2)\beta\gamma & 0 & \gamma & \beta \\ 0 & \beta & (n^2-l^2)\gamma\alpha & \gamma & 0 & \alpha \\ 0 & \gamma & (l^2-m^2)\alpha\beta & \beta & \alpha & 0 \end{vmatrix} = 0,$$

which is, when expanded, the very symmetrical curve,

$$\left(\frac{\beta^2+\gamma^2-\alpha^2}{l\beta\gamma}\right)^2 + \left(\frac{\gamma^2+\alpha^2-\beta^2}{m\gamma\alpha}\right)^2 + \left(\frac{\alpha^2+\beta^2-\gamma^2}{n\alpha\beta}\right)^2 = 4\left(\frac{1}{l^2}+\frac{1}{m^2}+\frac{1}{n^2}\right) - \left(\frac{l^2+m^2+n^2}{lmn}\right)^2.$$

It seems scarcely necessary to add that in equation [1] or (50), the curve in which the *inverse* centre moves, is *legitimately* taken, rather than that in which the centre itself moves, since in the latter case the other two equations change places, and the same elimination has to be effected, the variables being merely inverted.



Reduced tracing of the Elie Coast-line from the Ordnance Survey Map
Six inch Scale.

W. H. McFarlane, Lith^r Edin^r

XL.—*On the Arctic Shell-Clay of Elie and Errol, viewed in connection with our other Glacial and more recent Deposits.* By the Rev. THOMAS BROWN, F.R.S.E. (Plate XXXVII).

(Read 4th March 1867.)

My attention was called to the subject of this paper in May 1862, during a short stay at Elie on the coast of Fife. Close to the friend's house with whom I lived a part of the sea-bank had been laid open by the waves, and among other deposits I found a bed of clay containing fossil shells, such as now live only in the Polar seas. An account of this I had the honour of laying before the Society on the 2d of March 1863. During the following autumn, while residing at Bridge of Earn, I found a similar deposit, with the same species of shells, at Errol on the Tay, and a notice of this I also laid before the Society on the 2d of May 1864, intimating that at some future period I should again ask their attention to the facts thus ascertained and the inferences to be drawn from them. The delay that has taken place has arisen from other occupations, which leave little time for such pursuits, but it has not been wholly without advantage. The cuttings of the East of Fife Railway were carried past the outskirts of Elie, and I had an opportunity of examining the series of beds, while laid open for the time, in a very remarkable way. This last autumn also, while residing on the spot for a few days, I examined with some care a transverse section, nearly at right angles to the two former; and now, in this paper, I shall endeavour first to state in detail the facts connected with these separate localities, and then to bring into one view the general results.

The Errol Section.

The great feature of the Errol district is the level clay of the Carse of Gowrie, so valuable to the agriculturist. Inland, ridges of boulder-clay are found rising from below the carse lands, and on the slope of one of these, near its base, lies this deposit with its Arctic shells. The aspect of it is very different from that at Elie, where the dark colour of the carboniferous shales shows itself in the darkness of the clay, while at Errol the colour is light or reddish, from the red sandstones of the district. The shell-bearing clay is well laid open, occupying the whole side of the brickfield, where it shows the following series:—

Section I.

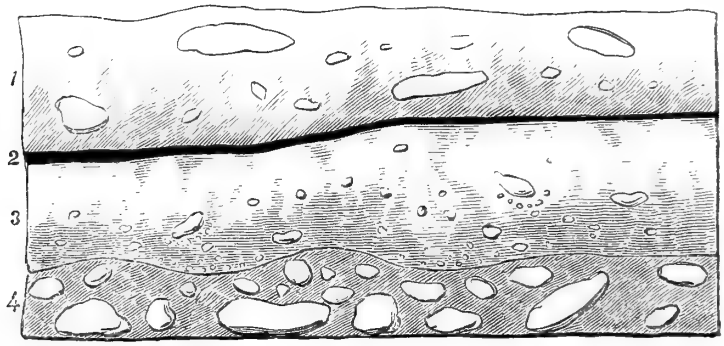


Fig. 1.

1. Immediately beneath the surface a yellowish or brown clay, from 5 to 8 feet thick, lighter in colour, and less compact in structure than the underlying beds.

2. A black band of particularly fine clay, a foot or rather less in thickness.

3. An underlying deposit of dark-red tenacious clay, from 5 to 8 feet in depth.

4. The boulder-clay or till, on which all the above beds rest. Its uneven surface shows that it had been denuded, and there is a marked line of separation between it and the next bed above.

The whole of this series contains boulders and stones, from the size of a marble up to masses of very considerable weight, some of the largest being found near the top of the section. The clay itself is particularly fine, but it requires much care to free it from stones,—every separate portion, as it goes through the machine, requiring to be hand-picked. The fossil shells are found all through the three highest beds. In No. 2 they are particularly abundant, the blackness of the bed being due to the decay of animal matter. They are also very plentiful near the bottom of No. 3, where they are found clustering around and beneath the enclosed boulders—a fact which seems to show that at the time these shells lived this part of the sea-bottom must have been swept by a strong current, and they had found it convenient to shelter themselves under the lee of the stones.

The shells themselves are in beautiful preservation. Though sometimes fractured by pressure or shrinkage, yet the epidermis and other parts are usually as complete as if they were recent. They soon crumble on exposure to the air. I shall afterwards refer to the species, and to the skeleton of a seal found along with them.

The position of the three sections at Elie will be understood by referring to the accompanying sketch map. (Plate XXXVII.)

Elie Shore Section.

This occurs close to the harbour, running eastwards from the pier along high-water mark, where, in 1862, the beds were much more fully laid open than they are now. In the descending order they showed the following series:—

Section II.

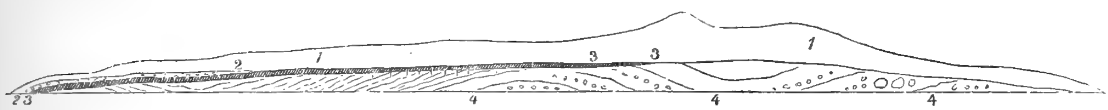


Fig. 2.

1. Immediately below the surface blown sand, in horizontal layers, from 4 to 6 feet, or rather more, in depth. This is the seaward edge of a great sheet of the same kind, which stretches, with intervals, far along the coast, and reaches in some places more than a mile inland. The whole town of Elie is built on it. In the present section, at the point where beds 2 and 3 thin out, the lowest portion of this blown sand has hardened into a kind of sandstone or concrete, rolled fragments of which may be found among the boulders of the beach.

2. A thin bed of sea shingle and shells. Apparently it is a portion of the so-called raised beach, and will be referred to more at length under Section IV.

3. A layer of peat from 5 to 10 inches in thickness. It is *in situ*, the plants which formed it having grown on the spot, as is shown by the traces of their roots going down vertically into the underlying beds. The roots themselves are decayed, but the ferruginous stains, which show their course, are very marked. Of the remains enclosed in this peat, by far the most common is the *Arundo phragmites*, the common reed of our fresh-water marshes, which grows at present freely in the neighbourhood. The thin outer coverings of the underground stems, with their unmistakable jointings, occur in great quantities. There are seeds of various kinds, and decayed wood, but no shells. This seems a portion of the submerged forest of this coast.

At this point there occurs a break in the series which should be carefully noted. These three upper deposits are markedly unconformable to those beneath. When first laid down the lower beds would be approximately horizontal; but, by the action of some force, they must have been thrown into convolutions. The process of deposition was suspended; part of the material already laid down was abraded and swept away; and when, after a time, the process of deposition was again resumed, the newer beds were laid down horizontally across the truncated and upturned edges of the lower beds. This break should be noted as showing a blank in the record.

4. The deposit in which the fossil shells occur. The upper portion of it consists of layers of sand, with partings of finely triturated coal shale, the sand being of an ochreous or deep brown colour. In this portion I observed no fossils. Gradually, on passing downwards, the layers begin to get argillaceous till towards the base, and in the eastern half of the section, the deposit passes into a peculiarly stiff, tenacious, unstratified clay. It is here the shells occur, and in considerable quantities. All the portions of the shells are in many cases beautifully perfect, but they readily go to pieces when exposed to the air.

Elie Inland Section.

This was laid open in the railway cutting, beginning at a point to the east of the railway station, and going west to the bridge beside the schoolhouse. It is distant about the third of a mile from the preceding section, and showed, when first fully laid open, the following series of deposits from above downwards:—

Section III.

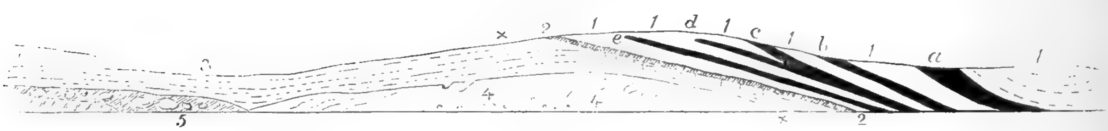


Fig. 3.

1. Blown sand of very considerable depth. All through it contains numerous darker lines, showing former surfaces, and containing land shells, especially the *Succinea putris.* Besides these there were intercalated, at four or five different levels, distinct beds of peat, *a b c d e*, the uppermost of which is 6 feet in depth. They are full of land and fresh-water shells, to which I shall afterwards refer.

2. A bed of peat, from 6 to 10 inches thick. The structure is markedly different from that of the overlying peat beds, more earthy, and bearing a close resemblance to No. 3 of the previous section. Like that it contained no shells. The only organisms I detected in it were the roots of *Equisetum*, which, like those of the *Arundo* in the peat of the shore, indicate a fresh-water origin.

3. A deposit of gravel and sand, with portions of clay all arranged in layers, which are nearly horizontal, but with a gentle dip eastwards, accommodating themselves to the general lie of the ground. To the west of the schoolhouse, where the ground slopes the other way, the inclination of these layers is also reversed, showing that, when this deposit was laid down, the contours of the country had the same outline as now. At some points the gravel was quite green, from the debris of disintegrated basalts and greenstones, which have much the appearance as if they had come from near M'Duff's cave, and if so, the current must have flowed from the west. This whole deposit seems to be a portion of the higher level sands and gravels which are found over the surface of the country,

and on which the mansion-house of Cairnie, for example, and the town of Colinsburgh rest.

At this point there occurs the same break in the succession as in the above section. Though not as prominently shown, it is really present.

4. The Arctic shell-clay. The upper portion consists of sand, for the most part unstratified; but at two points it was observed to be separated into layers by partings of minutely triturated coal shale. This sand interpenetrates and passes into the clay, which corresponds in structure exactly with that of the shore-section, and, like it, contains shells in exactly the same state of preservation.

5. The boulder-clay in three stages.

The highest portion to the east was black, with almost a bluish tinge. At a somewhat lower stage it shows a brown colour, while lowest is the basement bed full of shivers from the shaly rock on which it rests. The boulders are numerous and large, some of them two yards in diameter and striated. Boulders, indeed, are found all through these beds up to the top of the sand forming the upper portion of No. 4, but they are much the most numerous in No. 5.

Transverse Section, Elie.

It seemed desirable to examine these deposits on a line, as nearly as possible, at right angles to the previous sections; and with this view I took the banks of a small stream, which falls into the sea a little to the west of Kincraig—the Cocklemill Burn. A mass of trap, *t*, springs up close to high-water mark, and

Section IV.

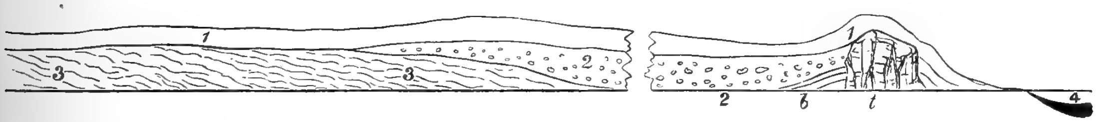


Fig. 4.

behind it there lies a small patch of boulder-clay, *b*. Beginning at the surface, however, and taking the beds in order, we have—

1. Blown sand rising into dunes.

2. The shingle, and shells, and sands of the so-called raised beach. It runs about half a mile up the banks of the stream, and rises at certain points 18 or 20 feet above high-water mark. The shells are the common species of the Frith, and as fresh as those now lying on the shore.

3. The third deposit consists of sands and clays, arranged in contorted layers. Its junction with No. 2 is not seen, but that it is older, and, therefore, underlying, is proved by the much worse preservation of the shells. When first laid open, a

little way above the foot-bridge, the base is found to consist of fine blue clay, passing upwards into a series of numerous sandy and clayey layers, arranged alternately. One of these layers of clay, near the summit, deserves particular attention, from the shells which it contains, and to which I shall afterwards refer (fig. 6.)

4. The submerged forest of Largo Bay. Close to the Shooter's point, where this section strikes the shore, it is found between high and low water mark, and is seen passing out seawards. As it is an outlier, and not in contact with the preceding beds, there may be some difficulty in fixing its position in the order of succession; but there seems good ground for placing it, as we have done, immediately beneath the bed No. 3. The enclosed organisms show that it is the same with the peat, No. 3 of Section II.; and that it is evidently identical with bed No. 2 of Section III.; and we are thus able to show, approximately at least, its true stratigraphical position.

It will be observed that we learn from Section II., that it is older than the so-called raised beach, and newer than the line of non-conformity—the break formerly referred to—while Section III. shows that it is newer than the high-level gravels and sands of the district. I was anxious to ascertain on what it rested, as found on the shore, for the purpose of comparison. Mr Howie, of Largo, who has paid much attention to this deposit, kindly met me on the spot. The peat was dug through, and the underlying strata laid open, when we found the following result.

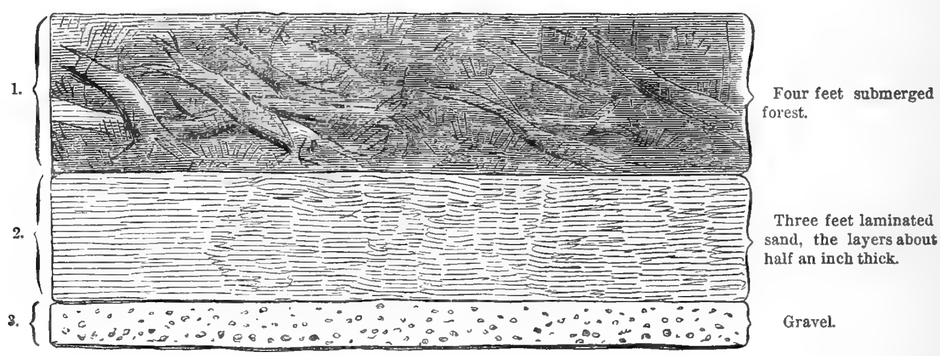


Fig. 5.

It will be observed how closely this corresponds with Section III. The sandy and gravelly layers agree with the deposit which there underlies the peat (2), and seem to indicate that this peat immediately succeeds the high-level gravels of the district, coming in directly beneath the deposit No. 3 of Section IV.

Having thus seen in detail the nature of these separate sections, it may be well now to combine them in one view, and glance at the leading events in their order. My object is to show at what point of our recent geological history the Arctic shell-clay comes in; and perhaps this will be most clearly seen if we begin at the surface, and work our way down step by step to the lower deposits, noticing as we go the evidence as to climate, and the relative height of sea and land. Immediately beneath the surface, then, we find the first stage, comprising the two newest deposits.

I.—*The Blown Sand and Raised Beaches.*

Of the blown sand, by far the best display is at the Elie railway station, where, from the highest point of the synclinal down to the base of the deposit there must be at least 20 to 30 feet of perpendicular depth. The highest bed of the enclosed peat is about 6 feet thick. The growth of this peat at its different levels, and the accumulation of this sand, obviously show that the lower portion of it must be of considerable antiquity. The great feature of the deposit is the profusion of land and fresh-water shells in the peat. I examined the lowest bed with some interest, to ascertain whether any of the species were extinct, but found only the following:—

Succinea putris.	Helix nemoralis.
Limneus pereger.	fulva.
Zua lubrica.	fusca.
Pisidium pulchellum.	pulchella.
Cyclas cornea.	Pupa muscorum.
Carychium minimum.	Planorbis marginatus.

These are all recent, and most of them have been actually found by Dr M'Bain living near Elie. The only thing to be observed is, that the immense numbers of these shells, found in the peat, seem to show that there formerly prevailed some peculiarly favourable conditions for the development of this form of life. At the same time, it is clear that the climate must have been much the same as now, for the species are identical.

Along with this deposit may be classed the so-called raised beach. The great display of it is seen in Section IV., where it is cut open for half a mile across, and is at some points 18 feet in depth, rising to that extent above high-water mark. The materials, consisting of shingle, sand, and shells, have been thrown up by the sea, and apparently at different times, and in a confused way. The shells are all of species now common, and so well preserved, that there is really no reason to think the deposit is any older than some of the oldest portions of the blown sand seen in Section III. The formation of the two may quite well have gone on together—the one inland, and the other at the sea-shore.

There has been a good deal of discussion as to whether this deposit indicates a rise in the land. The point, I must confess, seems still doubtful. The sea in certain states has, we know, the power of throwing up shingle and shells to a considerable height;* and when species of dead shells are found, as here, con-

* See this view well stated by the Rev. W. Wood, in his work on the East Neuk of Fife, p. 320.

fusedly cast together with other materials, I feel as if the evidence, had it stood alone, would have been far from decisive. These two deposits, then, taken together, form the first and most recent stage.

II.—*The Sands and Clays along the Banks of the Stream.*

This second stage is seen only in Section IV. Along the course of the stream are the *present* banks, corresponding with the first stage. But farther back, on either hand, and rising higher, is the deposit in question, which represents a previous state of things. A little above the foot-bridge the stream has cut into it, and laid open the following section already referred to. In this the

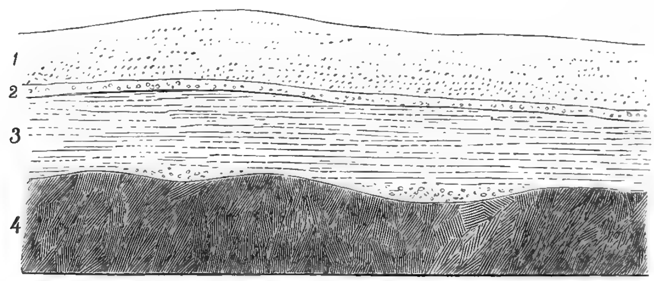


Fig. 6.

bed No. 2 deserves particular attention, as furnishing good evidence of a rise in the bed of the sea. It consists of a layer of fine clay, about a foot in thickness, running all along the section, immediately under the blown sand, and contains a good many shells of a single species, the *Scrobicularia piperata.* It may be termed the *Scrobicularia* bed. Now, the remarkable thing is, that almost the whole of these shells are in an erect position, with the siphonal end uppermost—showing, as every conchologist knows, they are at home just in the position in which they were when alive. The habits of the shell are well enough known. It burrows in clay at the mouths of rivers, sending up its long siphons to draw a continuous stream of water into its gills, so that in this bed the shells are in the position in which they lived. The habitat of the shell also is peculiar and well marked. It enters the mouths of tidal rivers, but never goes out of the reach of the tide. It needs to have not only the fresh water, but the salt also, flowing over the ends of its siphons. Now, the tide runs up the stream past this section, and the point to be specially noted is that the *Scrobicularia* bed is not less than 14 feet above medium high-water mark. The conclusion seems irresistible, that at the time these shells lived the land was not less than 14 feet deeper in the sea than now. There is something very remarkable, also, in the way in which this evidence harmonises with what is found elsewhere round the shores of the Firth. Between Stirling and Bridge of Allan, Dr M'Bain informs me he obtained from the brickfield specimens of this same *Scrobicularia*, about 15 feet above high-water mark. At various points, also, above Stirling, specimens of whales have been found imbedded in the same clay; and in two cases it is recorded that there lay beside them rude spear-heads of deer-horn, with which they seem to have been killed. Curiously enough, at Portobello a bed of the same *Scrobicularia* was found in the brickfield; and it lay, as Dr M'Bain informs me, at the same height of 15 feet above high-water mark. Thus all round the Firth the evidence points to the same conclusion. The land was at least 14 or 15 feet deeper in the water than now. The sea ran above Stirling, where now there is solid ground, and if the evidence as to those deer-horn weapons may be relied on, it would seem that at the time these shells near Elie were sending up their siphons to be swept by the waters of each returning tide, our forefathers, with their rude harpoons, were hunting the whale, where now only the green fields of the farmer are to be found.

Another point not less clear is, that the climate must have been much the same as now. In the above section (fig. 6), the *Scrobicularia* bed is underlaid by two yards of alternating sandy and clayey layers; and among these, near the base, the following additional species of shells were found:—

Tellina solidula.	Mytilus edulis.
Turtonia minuta.	Littorina littorea.
Cardium edule.	Montacuta ferruginosa.
Rissoa ulvæ.	

This group is at once decisive as to the question of climate. Take, for example, the *Rissoa*, which occurs in great abundance. It lives between tide marks, and is exposed to all atmospheric changes. It seems to have been as abundant when these beds were laid down as now, The climate then must have been much the same as the present. Passing this stage, we come to the third in the descending series, viz.:—

III.—*The Submerged Forest.*

This we have seen, in Section II., in the act of passing seaward; it is seen again in Section III, as a thin bed of peat; but it is especially in Largo Bay that it comes out in full force. Somewhat to the west of the point where Section IV. passes, it is 4 feet in depth (fig. 5), showing a great peaty mass, in which willow and hazel, and especially hazel nuts, were found, with other seeds, mosses, and especially the abundant remains of *Arundo phragmites.* Though now lying deep under every tide, yet no single marine organism enters into the bed itself. It sweeps for miles all round Largo Bay, and Dr Fleming has described trunks of trees standing in it rooted in the soil beneath. At Aberdeen, in the Tay, at various points in the eastern and southern coasts of England, and again across the Channel and in Jersey and Guernsey, the same deposits are found, and it seems really impossible to resist the inference, that they indicate a time when the land stood higher above the sea than now. Here, in Largo Bay, a land surface, with rooted stumps of trees, is seen passing out seaward at low-water mark. It may well have been that the Britain of that time stood so high above the water as to have been connected with the Continent. The only thing to be noted is, that if this were the period of such connection, it must have been subsequent to the glacial epoch. At the time of these submerged forests the *Arundo* grew abundantly, as now, in the marsh, the willow put forth its catkins, and the hazel ripened its nuts in the wood, and the whole flora, down even to the mosses, was the same as now. Thus far the climate must have resembled the present, with no trace of glacial cold.

IV.—*High-level Gravel and Sand.*

This has been described in noticing the details of Section III., where it forms bed No. 3. It should be distinguished from the second stage in this enumeration. The latter lies only along the course of the stream, and is a river deposit, with estuarine shells and no gravel, while this fourth stage consists of gravels and sands, utterly without fossils, and scattered at considerable heights all over the surface of the country. It was well displayed in the railway cutting. In one of the layers of rather coarse gravel there were angular patches of fine sand; and their presence could only be explained by supposing that some sandy stratum had been frozen, the current had gained access to it, broken it into fragments, which in a frozen solid state had been transported along with the gravel, and deposited where they now lie as angular masses of fine sand crumbling to the touch, but having their angles sharply defined. If this be the right explanation, then we have here the first indication of glacial cold in these deposits; and, perhaps, it points to the true origin of these masses of high level unfossiliferous stratified gravels and sands found over Scotland. They may be due to the floods attending the breaking up of the glacial epoch.

V.—*The Arctic Shell-Clay.*

Passing over the break in the series formerly referred to, we come to what is the chief object of this paper, the period of the Arctic shell-clay. When my attention was first called to the subject in 1862, no deposit of Arctic fossil shells had been found, either on the Forth or Tay. The first trace, indeed, had been detected by Dr FLEMING, Dr M'BAIN, and the late Mr BRYSON, but only two or three specimens of the shells had been got, and they were held not to be indigenous. The seeming absence of this fossil fauna from our eastern friths had led some geologists of eminence to question the inferences drawn from those of the Clyde. The discovery of the Elie shells, however, was at once decisive. Through the obliging attention of Mr CARRUTHERS of the British Museum, they were submitted to Dr OTTO TÖRELL of Lund, who has himself dredged extensively in the Arctic seas, is the author of an important work on the Shells of Spitzbergen, and is of the highest authority in this special department. He had the goodness not only to name my specimens, but went with me to Elie to examine the shells as they lay in the clay. I have to acknowledge my obligations to him for much valuable information. I made no attempt at a complete collection of the shells. They are so friable, that much time and patience is required to extract them from the clay, and I was content with having obtained the following species, which seemed sufficient to determine the character of the deposit.

1.	Buccinium cyaneum,	Errol.
2.	Natica grœnlandica,	Elie, Errol.
3.	Turritella erosa (polaris),	Elie.
4.	Pecten grœnlandicus,	Elie, Errol.
5.	Crenella decussata,	Elie.
6.	nigra,	Errol.
7.	lævigata,	Elie, Errol.
8.	Leda (Yoldia) truncata,	Elie, Errol.
9.	minuta,	Elie, Errol.
10.	Yoldia hyperborea,	Errol.
11.	(*new species*),	Elie.
12.	Astarte compressa,	Elie.
13.	Nucula inflata,	Elie, Errol.
14.	Dacrydium vitreum,	Errol.
15.	Thracia myopsis,	Elie.
16.	(*new species*),*	Elie, Errol.
17.	Tellina proxima,	Elie.
18.	Saxicava rugosa,	Elie, Errol.
	Fusus *sp.*,	Elie.
	Cylichnia ? *sp.*,	Elie.
	Pleurotoma *sp.*,	Errol.
	Axinus ? *sp.*,	Errol.
	Univalve, unknown,	Errol.

In looking over this list, the first thing to observe is, that it contains no single species which does not now live in the Arctic seas. In this respect the deposit differs markedly from the Clyde beds, where a certain proportion of the species cannot bear the cold of the northern ocean, and are yet lying side by side with boreal species. It becomes a somewhat complicated problem, and would require careful consideration, how such incompatible results are to be explained. But at Elie and Errol the problem is simple. The whole species lying in the deposit are now living in the Polar seas. As to the evidence thus furnished in regard to climate, they may be divided into three classes:—

1. There are some which tell nothing, either for or against a period of Arctic cold. The *Crenella nigra*, for example, reaches its southern limits in the Scottish seas, where it used to live among the oysters of the Frith of Forth. It ranges north to Spitzbergen, and the remarkable thing is, that it does not seem to be affected by the change of climate. The discovery of such shells could furnish no evidence either way.

2. There is a second class which can live indifferently in the British or

* Undescribed, from the Northern Shores of Spitzbergen.

Arctic seas, but they vary markedly with the degree of cold. Of this kind are the *Natica grœnlandica* and the *Saxicava rugosa*. The *Saxicava*, for example, is at home everywhere, from the Polar seas to the Canary Islands; but under an Arctic climate it thickens its shell, and attains a size quite unlike what it does with us.

To show how completely the specimens from this deposit are of the northern type, I place side by side No. 1, the outline of the shell of the full present British size, as given by FORBES and HANLEY, and No. 2 and 3, the same shell, as found in the Elie clay.

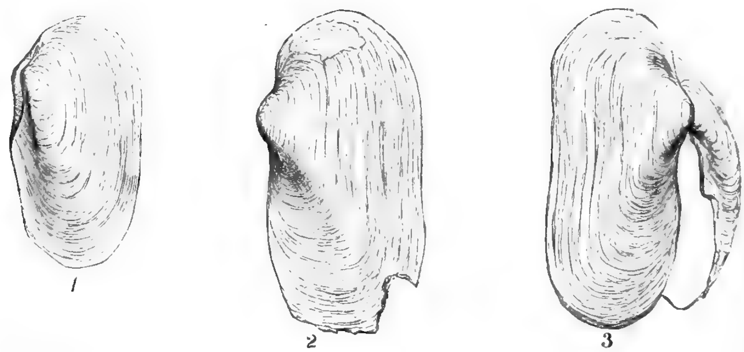


Fig. 7.

The evidence of the Arctic climate is here very marked.

3. The third and by far the most numerous class consists of shells, which require the climate of the Polar seas, and really can live nowhere else.

The two marking and characteristic shells of the deposit are the *Leda truncata* and the *Pecten grœnlandicus*. Both reach their southern limits on the coast of Norway. Both shells were found by Dr TÖRELL living together in the clayey sea-bottom, in front of the great glacier of Spitzbergen, just as they now lie together in this Elie and Errol clay. Another point of importance is the comparative abundance of this *Leda*. It is one of the most abundant shells now at Spitzbergen, and it is beyond all comparison the most abundant shell in this deposit, holding the same place that *Tellina proxima* does in the Clyde beds. The *Tellina*, on the contrary, is rare at Elie and Errol. I obtained only two or three specimens. The details as to the distribution of other species are not less decisive.

Buccinium cyaneum is found in Greenland, &c.

Turritella erosa.—Of this I got only one specimen, and it was not quite perfect. Dr T. says of it, "Almost certainly the same species, yet cannot be positively asserted." It lives in the seas of Greenland.

Thracia myopsis.—Iceland, Greenland, Spitzbergen, in from 60 to 200 fathoms water.

Nucula inflata.—Spitzbergen, 5 to 150 fathoms.

Crenella lævigata.—At my first visit to Elie I got a few specimens, which Dr

TÖRELL marked as "*C. lævigata* most probably, but much injured." Afterwards at Errol the workmen at one place threw them out by spadefuls, well preserved; and on showing them to Dr TÖRELL, he pronounced them most characteristic examples of the species. He had found the shell very abundantly at Spitzbergen, where it forms a great part of the food of the walrus, the large tusks of the animal being used to dig it out of the clay of the sea-bottom.

The new species of the *Yoldia* (No. 11 of the above list) also deserves notice. It is an unpublished species, dredged by Dr T. at Spitzbergen, in 80 degees of north latitude, and the next place where it turns up is this Elie clay. Not less interesting is the *Dacrydium vitreum*, a small shell, of which he had dredged a very few specimens in Spitzbergen, and on these had founded the new genus of Dacrydium.* The next place where it is met with is the clay at Errol, the deposit thus yielding some even of the rarer species of Arctic shells, whose proper habitat is at Spitzbergen, within about ten degrees of the Pole, and close to the limits of perpetual ice.

From all the evidence thus obtained, the conclusion will be held to be irresistible, that at the time these shells lived this country lay under the most rigorous Arctic climate. Like Greenland or Spitzbergen now, the Scotland of that day was wrapped in snow—a land of glaciers and icebergs.

In addition to the shells, I obtained from the Errol brickfield portions of the skeleton of a seal, in regard to which Dr M'BAIN has favoured me with the following note:—"The vertebræ enclosed in the Errol brick-clay are those of a young seal, and, from a comparison of the few detached vertebræ, they appear to me to belong to *Calocephalus vitulinus*. This could only be satisfactorily determined, however, by the discovery of the skull. The loose bones are an atlas the sixth and seventh cervical, and the first dorsal vertebræ. The others are portions of ribs, one with the head and neck, probably the twelfth, and from their size appear to belong to the same individual."

In regard to the relative levels of land and water, it is plain that at the period of this deposit the country must have been sunk far deeper in the sea than now. The Errol deposit at this moment lies more than 40 feet above high-water mark. No one can look on the shells, as they lie in the clay, without seeing that they are at home—not washed up, but in what was once the sea-bottom, where they lived and died. The old and young are so mixed together—the most tender portions of the epidermis, &c., are so preserved—that this cannot be questioned. Lying, therefore, now 40 feet above high water, a rise to that extent at least is undeniable. But there is much more than that. None of the shells are littoral species; and the group would, as a whole, require, when alive, a considerable depth of water over them. Much attention has been paid to the range of depth through which the different species live. If this were exactly ascertained, we should

* Spitsbergens Mollusker I. p. 19.

only have to consider at what point of depth all the above species could meet. Some of them have a wide range, like the *Crenella lævigata*, for example, which is found at all parts of the sea-bottom, from 5 to 200 fathoms. Others, like the *Natica grœnlandica*, have only been got from deep water. On the whole, however, our information as to the range of shells will hardly allow us to do more than form an approximate opinion. The species on which the conclusion must mainly turn is the *Leda truncata*, which is beyond all comparison the shell of the deposit. At Spitzbergen, &c., it is described as ranging from 5 to 30 fathoms; and between these two points, therefore, the degree of depth must lie. But, looking to the shells with which it is associated, it must evidently lie somewhere about the lower portion of its range. The *Thracia myopsis*, for example, is found at 60 fathoms, and ranges from that down to 200. The new species of *Yoldia* was dredged only in 100 fathoms. The presence of such shells would seem to indicate that we are to fix the point somewhere towards the lower range of the *Leda*, say 160 feet. This is the deposit which is now lying more than 40 feet above the sea; and we can hardly be wrong in concluding, that at the time these shells lived the level of the land was at least 150 to 200 feet lower than now.

V.—*Boulder-Clay.*

The lowest stage of these deposits is that of the boulder-clay, which, both at Errol and Elie, is found lying beneath the Arctic shell-clay, and resting immediately on the rock. It would seem that this lowest deposit, so long an enigma, has at last yielded up its secret,—that it is a land deposit, formed at the period when Scotland, like Spitzbergen, lay beneath an immense covering of ice, which wrapped the whole face of the country, hill and dale. Underneath such a covering, possibly thousands of feet in thickness, the rocks would be ground down, and the boulder-clay formed. And thus the absence of fossils is accounted for, inasmuch as none of our usual forms of life could exist beneath such an ice-sheet. And thus we see also how the clay is so peculiarly hard and untractable. This hardness is very strikingly noticed wherever the true boulder-clay is dug into, as at the mantrap, near Leith, or in the neighbourhood of Greenock, where, in the excavations at the harbour, I found them blasting it with gunpowder. It would seem as if the pressure of immense ice-masses passing over it had compacted it into such hardness. This feature was very marked in the railway cutting at Elie.

As to the relations of the boulder-clay and the Arctic shell-clay, there are two questions to be considered :—

1. Is the boulder-clay an antecedent formation, and did the Arctic shell-clay come to be formed only after the formation of the boulder-clay had ceased?

Both at Elie and Errol the shell-clay rests on the other; and it is, of course, plain, that the particular portion of the boulder-clay there present is older than that portion of the shell-clay. But we must take care how we extend this inference to the two deposits as a whole. If we endeavour to picture the time when the shell-clay was laid down, it will be plain that all through it there must have gone on the process of the formation of the boulder-clay. We can tell to a certainty that the greater portion of Scotland was above water when the Elie and Errol shells lived, for the characteristic species, the *Leda truncata*, does not live in more than 50 fathoms of depth. A great part of the country, therefore, stood out of the water, and all over the higher grounds the great ice-sheet was doing its work, in forming the boulder-clay. When the shells lived in the Elie clay, the glaciers were scouring the valleys of the Pentlands, and rising over the sides of Arthur's seat. When the current swept the sea-beds of Errol, the ice-sheet was grinding the flanks of Benvoirlich, and ploughing its way down the solitudes of Glenartney. This is the story of that epoch, which admits of no denial. There was land, and there was sea, and the two formations were cotemporaneous—the Arctic shell-clay under the sea, and the boulder-clay on the land. Take any portion of the shell-clay, and you would probably find that if the whole facts could be brought out, some portion of the boulder-clay was prior, some cotemporaneous, some posterior to it. But as a whole, the two were simultaneous. As surely as there was land and sea, so the land and the sea formations ran on side by side, just as the two processes may be seen this day going on side by side at Greenland or Spitzbergen.

2. Then a second question is, whether the boulder-clay is or is not fossiliferous? Some have been inclined to view it as marking a time of dreariness and desolation, almost as azoic, a period of death, so devoid is it of fossils. This, I believe, is a mistake. If we use the term boulder-clay in a mineralogical sense for a certain clay in a certain mechanical state, then, as a whole, it is unfossiliferous, and we have seen why it must be so. But if we use it in a geological sense as representing a particular epoch, then the fossils of this Arctic shell-clay are the fossils of the boulder-clay. In them we have the marine life of the boulder-clay period. The fossil fish of the Old Red Sandstone belong to their own epoch, none the less that the bed in which they are found may not be sandstone and not red, and in this sense the fossils of the Arctic clay might be termed boulder-clay fossils, as representing the marine life of that epoch of which the boulder-clay was the great feature. Having thus glanced at the details of the different deposits, let us now advert to the

General Results.

The only safe rule in classifying the beds of any geological epoch is by the enclosed fossils. Were we to take the boulder-clays, brick-clays, and gravels, and try to work out the succession by the mineral and mechanical structure, we should be groping in the dark. Dr Fleming showed that there are earlier and later boulder-clays; and this is quite plain, when we contrast that of Errol, underlying the shell-clay, with that of Caithness, as investigated by Mr PEACH. The same thing is true of the brick-clays, and we should thus get into all kinds of confusion did we attempt to make out the series by attending merely to the structure of the beds. It is by examining their fossils or their stratigraphical position, as compared with other fossiliferous deposits, that we shall be safe in our inferences. Proceeding in this way, the whole deposits referred to in this paper may be arranged in three divisions.

I. The Elie and Errol shell-clay, with its underlying and (in the sense explained above) contemporaneous boulder-clay. All over the Forth and Tay districts this is the lowest and oldest portion representing the period of the Arctic cold.

II. There is an intermediate series of deposits. In the above sections described in this paper these beds are awanting, represented only by the line of unconformity formerly referred to as making a break in the series. At a single leap we pass from the rigour of Arctic cold to the present climate. This chasm, however, may quite well be filled up from other parts of the country where we pass through a whole series, in which a less and less degree of cold is gradually indicated. About the centre of this scale would seem to lie the Clyde beds, described by Mr SMITH of Jordanhill, having beneath them those of Aberdeen, so well investigated by Mr JAMESON of Ellon, and above them those of Fort William and Caithness. Step by step we can trace the passing away of the Arctic cold. These intermediate deposits are wholly wanting in the above sections, unless it may be the portion of high-level unfossiliferous gravels (Section III.), which show, as we saw, some trace of glacial cold.

III. The group of deposits representing the present climate.

It will not, I trust, seem presumptuous if I suggest that geologists may yet find in these Elie and Errol deposits the starting-point for a more rigorous classification of our superficial beds throughout central Scotland. In studying the geographical distribution of northern shells as they at present exist, it is well known that conchologists have recognised two great provinces—the Arctic, or most northern, and the Boreal, or sub-arctic, the less northern. Now, as this division holds good for the conchologist in separating the two groups in regard to space, so the geologist may find it hold good in separating two groups of deposits in regard to time. We have in these superficial strata first an Arctic time, with its own set of deposits, when the climate is that of the Polar regions, characterised by shells strictly Arctic, and next a sub-arctic time, also with its special beds, characterised by the group of Boreal shells. The type of the one is the *Leda truncata*, found everywhere in the lower and colder deposit; it represents the Arctic province. The type of the other is the *Tellina proxima*, found everywhere in the Clyde beds in countless numbers.* It is properly a Boreal shell, repre-

* It occurs but very scantily at Elie.

senting the Boreal province and the sub-arctic deposits. The one may shade into the other, just as the provinces of the conchologist do; but rightly viewed they may serve as great landmarks, enabling us to classify and so to advance our knowledge. With this explanation I would submit the following diagram, as embodying a general view of the sequence of deposits through the whole of the sections referred to in the preceding pages. It is not meant that the formation of each underlying deposit was finished before the overlying bed began to be laid down, at least in those cases where such deposits are found in separate localities. Just as the Arctic shell-clay was (in the way explained above) partly cotemporaneous with the boulder-clay, so the blown sand, No. 1, may have been cotemporaneous in part with the raised beach, No. 2, and even with the river deposit, No. 3. It is often impossible to establish the sequence in a rigorous way, so as to exclude the idea of cotemporaneous formation in greater or lesser degree among these superficial beds each in its own separate locality. But allowing for this, the following will, I believe, be found to approximate closely the sequence of the deposits :—

Diagram showing the order of the Superficial Deposits.

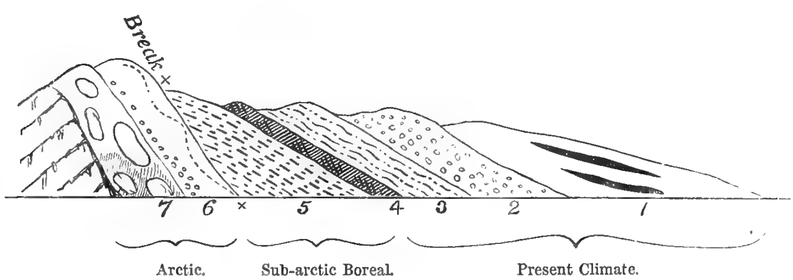


Fig. 8.

1. Blown sand, with peat full of land shells.
2. Raised beach.
3. River sands and clays—*Scrobicularia* beds.
4. Underlying peat—submerged forests—no shells yet found.
5. High-level gravels.

× Break in series of deposits representing the time of the Clyde beds, &c.

6. Arctic shell-clay of Elie and Errol.
7. Boulder-clay.

Fig. 1

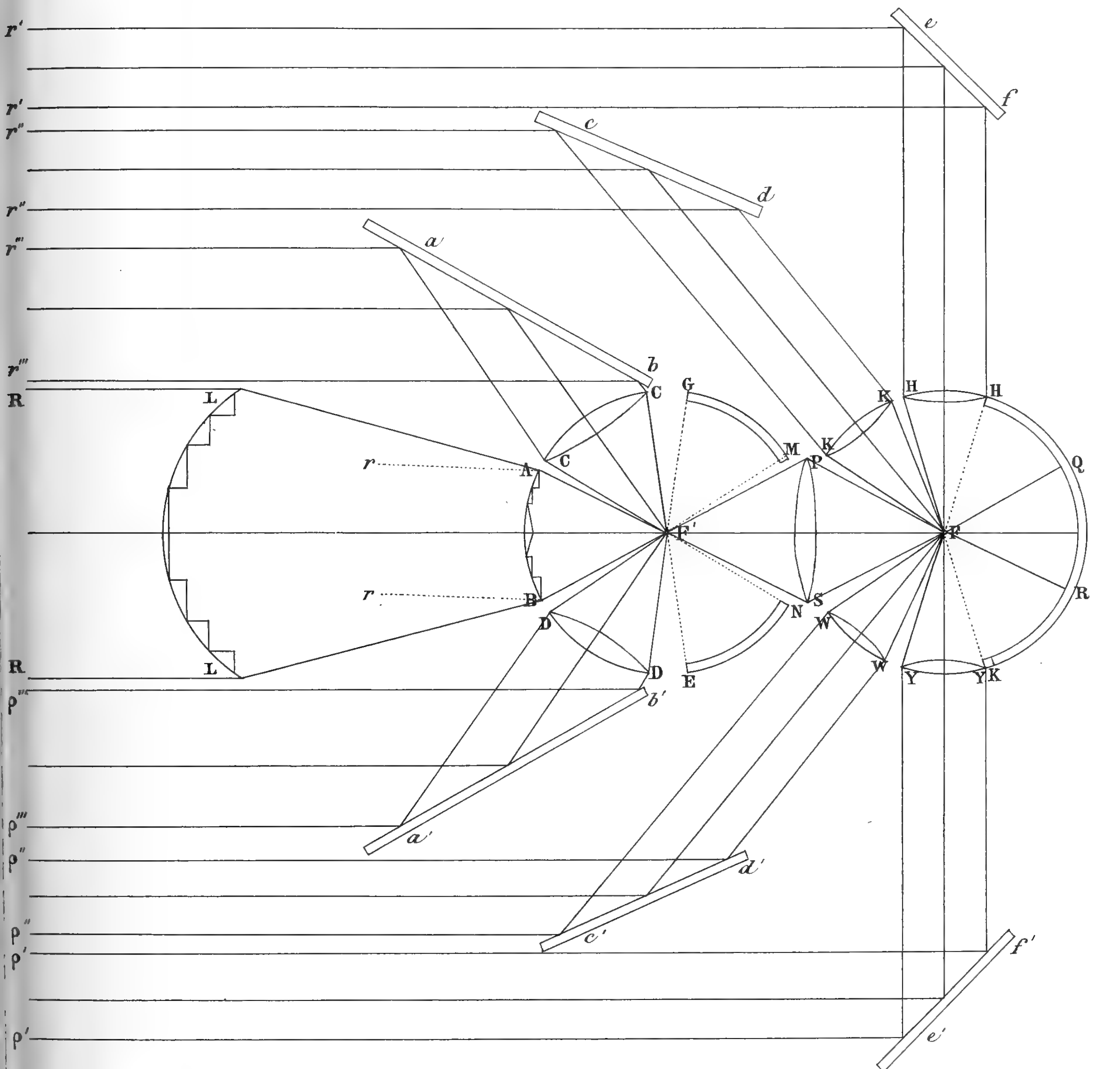
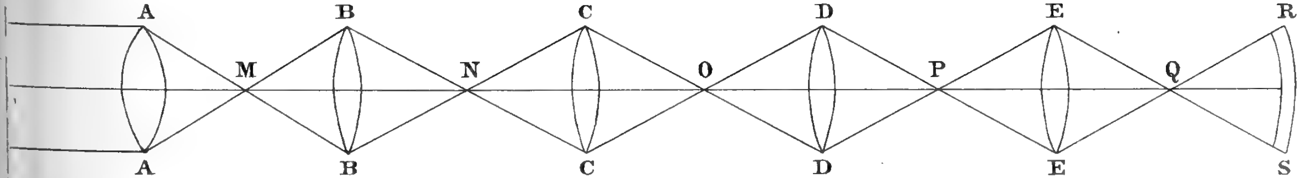


Fig. 2.



W H M^c Farlane, Lith^r Edin^r

XLI.—*Description of a Double Holophote Apparatus for Lighthouses, and of a Method of Introducing the Electric or other Lights.* By Sir David Brewster, K.H., D.C.L., F.R.S. (Plate XXXVIII.)

(Read 29th April 1867.)

In the year 1812 I described an apparatus, by which the light of the sun, or of any luminous body concentrated in the focus of an improved lens, could be returned by reflection from a spherical mirror into the same focus, thus increasing the light and heat produced by the cone of refracted rays. The apparatus contained also a double system of lenses and plain mirrors, by which additional beams of light could be concentrated in the same focus.*

In 1827 I described the very same apparatus as applied in the dioptric system of lights, *the whole of the light* which issues from a lamp being thrown into one wide and parallel beam, constituting what has been called a *holophote*, now in use in every part of the world.†

This apparatus consists of *three* different parts:—

1*st*, Of a lens which refracts into a parallel beam of light a cone of rays, which has for its base the surface of the lens, and for its apex the source of light.

This use of a common lens was made in the lighthouse in the Isle of Portland in 1789.

2*d*, Of a spherical mirror placed behind the flame, which throws into the parallel beam a similar cone of rays, whose base is the mirror, and whose apex is the flame.

3*d*, Of two systems of lenses and plane mirrors, by which the cones of rays, which would otherwise be lost, are thrown into cylindrical beams, which widen the principal parallel beam.

If the single holophote has been found so useful in lighthouse illumination, a double or even a triple holophote, in which all the lenses and mirrors may be reduced in size, must, in particular circumstances, have a peculiar value.

A double holophote, in which the light of two flames is condensed into a wide beam, is shown in Plate XXXVIII. Fig. 1, where F, F′ are the two flames surrounded by lenses either of one or more pieces, and by plane and spherical reflectors, which may be made of speculum metal, of prisms, or of glass silvered behind or before. The cone of rays FPS, issuing from the flame F, is converged

* Edinburgh Encyclopædia, Art. Burning Instruments.

† Edinburgh Transactions, 1827, vol. xi. pp. 55, 56.

to the focus F′, from which it diverges, and falling upon the lens AB, is refracted by it, and the lens LL into the parallel beam RR, or, if we use only the lens AB, into the parallel beam *rr*.

The cone of rays FQR, which radiates from F, is thrown back to F by the part QR of the spherical mirror HQRK, and falling upon the lens PSP, is refracted like the direct cone FPS into the parallel beam RR or *rr*.

The two cones of rays FKK, FHH, intercepted by the lenses KK, HH, are refracted into parallel beams, which, falling upon the plane reflectors *cd*, *ef*, are reflected into the parallel beams $r''r''$, $r'r'$. In like manner, the cones of rays FWW, FYY are refracted by the lenses WW, YY into parallel beams, which, falling upon the plane reflectors $c'd'$, $e'f'$, are thrown into the parallel beams $\rho''\rho''$, $\rho'\rho'$.

The cone of rays FRY, falling upon the part RY of the spherical mirror HQRY, is returned through the flame F, from which it diverges, and falls as a second cone upon the lens KK, which throws it into a parallel beam, which is reflected into the beam $r''r''$ by the reflector *cd*. In like manner the cone of rays FHQ is, by means of the lens WW, and reflector $c'd'$, thrown into the beam $\rho'\rho'$.

The eight cones of rays which issue from the flame F, being the whole of its light, thus passes into the wide compound beam $r'\rho'$, consisting of the principal beam RR, and the four beams $r'r'$, $r''r''$, $\rho'\rho'$, $\rho''\rho''$. The beams $r'r'$ and $\rho'\rho'$ are each composed of *one* cone of rays FHH, FRY, and the beams $r''r''$, $\rho''\rho'$ of two cones of rays, the one direct, and the other reflected.

If we now place a second flame at F′, and surround it with the lenses and reflectors shown in the figure, it will become a second holophote. The cone of rays F′AB, refracted by the lenses AB, LL, will add its light to the central beam RR, and the opposite cone F′PS, refracted by the lens PS′ to F, from which it falls upon the mirror QR, will be reflected through the flame F′, from which it diverges and enters into the beam RR, like the direct cone F′AB.

The cone F′CC, refracted by the lens CC, is reflected by the plane mirror *ab*, so as to form a new beam $r'''r'''$, filling up the space between the beam $r''r''$, and the central beam RR. In like manner the cone F′DD is made to fill up the space between $\rho''\rho''$ and RR, by means of the lens DD and the reflector $a'b'$.

The cone of rays F′GM, intercepted by the spherical reflector GM, as reflected through the focus F′, and by means of the lens DD and the reflector $a'b'$, is thrown into the beam $\rho'''\rho''$. In like manner the cone of rays F′EN, reflected by the spherical mirror EN to the flame F, will pass into the beam $r'''r'''$, by the refraction of the lens CC and the reflector *ab*.

If it is desirable to have, for any special purpose, a more intense and a wider beam, we have only to substitute a lens in place of the mirror QR, and introduce through it the light of one or more holophotes, the cones of rays passing through the lens at QR, adding to the intensity of the central beam RR, the other cones of rays which issue from the new flames producing hollow cylindrical beams, surrounding those shown in the figure.

If we wish to have a beam of great intensity, and of equal brightness throughout, the holophote principle is inapplicable; but in dispensing with the secondary lenses and the lateral reflectors, we obtain a less complex and more manageable apparatus, to which the name of *Cratophote* may be given, from the great intensity of the light which it is capable of producing.

This instrument is shown in Fig. 2, where M, N, O, P, Q are five flames, the light of which we wish to concentrate into one beam RR, MN, a concave mirror, and AA, BB, CC, DD, EE, &c., five, or any other number, lenses. The lenses BB, CC, DD, EE have the same focal length, but the focal length of AA is only one-half that of the rest. The light of the *first* flame M, or the cone of rays MAA, is refracted into the parallel beam RR. The light of the *second* flame N, or the cone of rays NBB, is refracted by the lens BB into its conjugate focus M, and, passing through it, falls upon the first lens AB, and is thrown by it into the beam RR.

In like manner all the other flames or cones of rays, OCC, PDD, and QEE, are converged by the lenses which intercept them to their conjugate foci, and finally enter the beam RR, into which the light of the five cones of rays is concentrated.

By placing a concave mirror RS in the instrument, so as to reflect the cone of rays QRS to Q, they will pass through all the other lenses into the beam RR. In like manner the other cones of rays PEE, ODD, NCC, MBB will be converged to the conjugate foci of the lenses which intercept them, and fall upon the mirror RS, which will send them back by the same process into the beam RR.

The intensity of the beam RR may be increased by diminishing the diameter and the focal length of the lens AB, which, when placed nearer N, will refract the cone of rays MAA into a beam of the same diameter as the lens.

The intense beam of light produced by this apparatus is obviously very different in its character from the compound beam obtained from the single or the double holophote. In the single holophote the beam consists of the central beam RR, Fig. 1, and of the hollow cylindrical beam $\rho'''\rho'''$, which, being less intense, will not be seen at such a distance as RR. In the double holophote the beam consists of the central beam RR, and of *three* cylindrical beams, each of which has a different intensity, and will, therefore, cease to be seen at different distances. It might, therefore, be desirable that each part of a lighthouse beam should have the same intensity, and, consequently, the same penetrating power as in the catoptric system, where the beam is produced by several parabolic reflectors of the same size, placed close to each other. In the dioptric system we can only approximate to this, and that very imperfectly, by combining several small holophotes, and thus producing a wide beam, in which we have several central beams, each of which is surrounded by its feeble cylindrical ones. In this construction we might dispense with the use of large built-up lenses, and employ small lenses of flint-glass, which can now be manufactured at a moderate expense, and of great excellence.

The suggestion to introduce the Drummond or the electric and magnesian lights as *permanent* lights in our lighthouses is not likely to be adopted. Oil and gas light, concentrated optically into a powerful beam, has a sufficiently penetrating power in ordinary states of the weather, and more brilliant lights are required only in fogs and states of the weather when feebler lights cannot be seen.

In my paper on the Illumination of Lighthouses, in our *Transactions* for 1827, I have shown in Plate III. Fig. 1, two methods of occasionally introducing the electric or other light into the central beam of a holophote, without interfering with its ordinary action.* If we consider the front holophote in fig. 1 to be the ordinary dioptric light, the second holophote, with the electric or other light at F, might be used in place of the simpler and less effective one above referred to.

* The method of doing this by lenses, whose conjugate focus is F′, Fig. 1, or by an ellipsoidal mirror, one of whose foci is F, when the spherical mirror (MN, Plate III. Fig. 1, above referred to) is removed, is distinctly shown; but the note describing it was accidentally omitted by the printer.

XLII.—*On a Lower Limit to the Power exerted in the Function of Parturition.*

By J. Matthews Duncan, M.D., &c. &c.

(Read 29th April 1867.)

The dynamics of natural labour have been the field of very little successful study or investigation. The object of the present paper is to make a contribution to this subject. I purpose to show what amount of pressure per square inch is sustained by the ovum in the easiest class of natural labours, and thence to estimate the propelling power exerted in such cases.

It is well known that natural births are ever and anon occurring, in which the ovum is expelled whole, the membranes containing the liquor amnii continuing entire. Into this category many more cases would enter, were it not a generally-followed rule for the attendant to rupture the bag should it advance entire as far as the external parts. Again, as Dr Poppel has pointed out, the attentive observer of a series of easy natural labours has no difficulty in arriving at the conclusion, that in not a few cases the same force which ruptures the bag of membranes is able to, and actually does, complete the delivery.

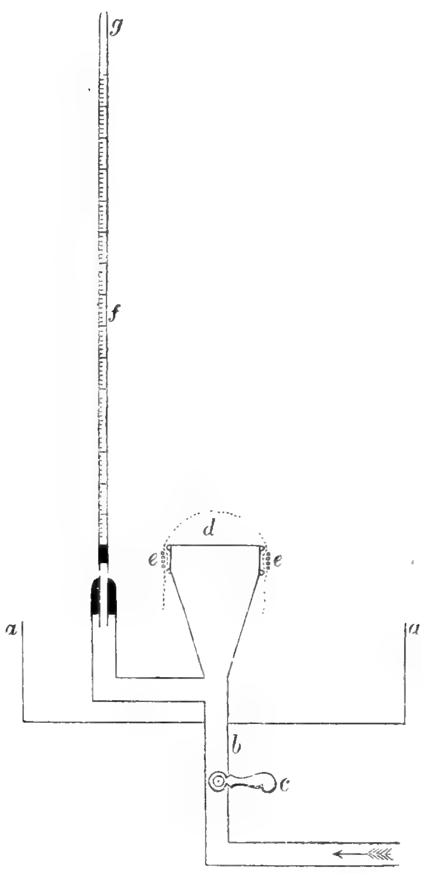
In all such cases, the strength of the membranes to resist impending rupture measures the force exerted in the process of parturition. When the bag is produced without laceration, its strength exceeds, certainly only to a small amount. the power of the labour. When the bag is ruptured at a very advanced stage of labour, as not rarely happens, its strength exceeds the power of labour exerted up till the time of its rupture. When the bag is ruptured by pains, which, without increasing in strength, rapidly and easily terminate the process. then the power of labour is probably only a little greater than the estimate, founded on the strength of the membranes, would indicate.

The strength of the membranes is thus shown to give us a means of ascertaining the power of labour in the easiest class of natural cases.

It might be suggested, that, in cases of persistent membranes, they were specially and unnaturally strong. My own experience lends no support to such a notion. Besides, so far as I know, no obstetrician has used the only means of verifying such a supposition—means such as are exemplified in the experiments to be hereafter related. Obstetricians have judged of the strength of membranes to resist a bursting force by their united thickness, or other less definite qualities, which form no criterion. It is not uncommon to read of the bag being strengthened by decidua; and that such thickening may be a source of strength is a common opinion; but as the decidua is far weaker and less extensible than the other membranes, the opinion is merely a natural delusion.

Experiments at once show that thickness of the membranes is no indication of strength. They also at once show that, for the special purposes of this paper, the amniotic membrane, being the strongest, alone requires to be observed. Long before the amnion is burst, the decidua and chorion have generally given way, and ceased to support the persistent amniotic membrane. The decidual membrane generally gives way first, under a bursting pressure applied to all three membranes. It sometimes does so with a sound as of a gentle fillip. Occasionally it bursts simultaneously with the chorion; and occasionally all three membranes burst at once. The decidua has been found, in the experiments, to burst at a tension of ·35 lb. per linear inch, corresponding under the circumstances supposed to exist in actual labour to a forward pressure of nearly 5 lbs.

As a general statement, it may be said that the chorion behaves like the decidua. It is of more uniform strength than the decidual membrane, and is only a little stronger, the average tensile strength being ·62 lbs. per linear inch, corresponding to a propelling power in labour of nearly 9 lbs. [In taking these averages, experiment 25 is omitted, because its exceptional value indicates almost certainly a mistake.]



The strength of the fœtal membranes lies in the innermost sac, in the amniotic membrane, which appears the thinnest and most delicate of all. To try the strength of it, as well as of the others, I made numerous experiments in the following manner:—They were all performed in the laboratory, and with the apparatus, of Professor Tait, to whose knowledge and skill I am indebted for their value and accuracy. The apparatus used was connected with a pipe in the bottom of an open cistern *aa*. Into this pipe *b* water,* under high pressure, of which there was a convenient supply, could be admitted gradually by a cock *c*. The apparatus expanded upwards from the pipe to its mouth *d*. In one apparatus used, this mouth had an external diameter of 3·35 inches, in the other it had an external diameter of only 2·25 inches. Over the mouth of the apparatus the membranes experimented on were placed, and tied on by a waxed hempen cord, around a broad rim *ee*, immediately beneath the mouth. That the apparatus

* Water is preferable in these experiments to air, because, when it is employed, there is less violent action at the bursting of the membrane.

acted in a fair and satisfactory manner was evident, from the observation that, in almost all the trials, the membrane tested did not burst where it touched the instrument, but in an arc of a circle crossing over the bulged out membrane; or, rarely, in a starlike manner. Connected, by a hollow arm, with the apparatus was a vertical tube g, with scale f of inches and tenths of inches. This tube contained a long column of air, confined in it by a short column of mercury. The rise of the column of mercury compressing the air in the tube indicated the degree of pressure applied to the internal surface of the membrane fixed over the mouth of the conical vessel d. Besides my own supply, I was kindly provided with fresh membranes by Dr Linton and Mr Vacher.

The following table gives, in a categorical form, a narration of each of 100 experiments, as well as the chief calculations founded upon the data obtained from them. The first column gives the number of the trial. The second column gives the number of the set of membranes tested; and it will be seen that generally several experiments were made with the same membranes. The third gives the length of continuance of labour till the time when the membranes were ruptured. The fourth column gives the duration of the first stage of labour. The fifth gives the duration of the second stage of labour. The sixth column contains the state of the os uteri at the time of the rupture of the membranes. The seventh states the stage of labour in which the bag of waters was broken. The eighth, ninth, and tenth columns show how many of the three membranes were tested simultaneously. The eleventh, twelfth, and thirteenth columns show what membranes gave way in each experiment. The fourteenth column states the radius of the circular mouth of the apparatus to which the membranes were tied. The fifteenth gives the barometric pressure at the time of each trial; and it will be observed that the pressure occasionally required a correction which demands explanation. The column of mercury in the apparatus was generally very short, and no correction for its weight was required, the experiments not pretending to an extreme nicety; but occasionally (in the cases noted in the column of remarks) the column of mercury was too long to be neglected, and a correction was made for its length. The sixteenth column gives the length of the column of air enclosed in the vertical tube above the mercury. The seventeenth gives the contraction of this column of air, by the pressure of water which burst the membranes, acting on the short column of mercury. The eighteenth column gives the height of the membrane as it bulged above the mouth of the apparatus, expanded by the water pressure. The nineteenth gives the effective pressure of the water, at the moment of bursting of the membrane, in inches of mercury. The twentieth gives the diameter of the sphere, of which the membrane when bursting approximately formed a portion. The twenty-first column gives the pressure per square inch of the membrane at the time of the bursting of the membrane, or at the time of the experiment's failing from some cause, such as

TABLE OF EXPERIMENT

1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.
							Membranes Tested.			Membranes Burst.				
No. of Experiment.	No. of Case.	Length of Labour till Rupture of Membrane.	Length of First Stage of Labour.	Length of Second Stage of Labour.	State of *Os uteri* at time of Rupture.	Stage in which Rupture occurred.	Amnion.	Chorion.	Decidua.	Amnion.	Chorion.	Decidua.	Radius of Apparatus.	Barometric Pressure
													a	*b*
		h. m.	*h. m.*	*h. m.*	*inch.*									
1	1						×	×	×	×	×	×	1·675	29·
2	,,						×	×	×	×	×	×	,,	,,
3	,,						×	×		×	×		,,	,,
4	,,						×			×			,,	,,
5	2						×	×	×			×	,,	,,
6	,,						×	×					,,	,,
7	,,						×	×		×	×		,,	,,
8	,,						×						,,	,,
9	,,						×	×	×			×	,,	,,
10	,,						×	×					,,	,,
11	,,						×	×		×	×		,,	,,
12	,,						×						,,	,,
13	3						×	×	×	×	×	×	,,	,,
14	,,						×	×	×			×	,,	,,
15	,,						×	×					,,	,,
16	,,						×						,,	,,
17	,,						×						,,	,,
18	,,						×						,,	,,
19	4						×	×	×	×	×	×	,,	,,
20	,,						×	×	×			×	,,	,,
21	4						×	×			×		,,	,,
22	,,						×						,,	,,
23	,,							×			×		,,	,,
24	5	28 35	28 30	0 25		2nd	×	×		×	×		,,	,,
25	,,	,,	,,	,,		,,	×	×			× ?		,,	,,
26	,,	,,	,,	,,		,,	×			×			,,	
27	,,	,,	,,	,,		,,	×	×		×	×		,,	
28	,,	,,	,,	,,		,,	×			×			,,	
29	6	6 0	4 55	1 10		2nd	×	×			×		,,	
30	,,	,,	,,	,,		,,	×			×			,,	
31	,,	,,	,,	,,		,,	×			×			,,	
32	7	6 0				2nd		×	×		×	×	,,	
33	,,	,,				,,		×	×		×	×	,,	
34	8	5 20	6 25	3 15	1½	1st		×	×		×	×	,,	
35	,,	,,	,,	,,	,,	,,	×			×			,,	
36	,,	,,	,,	,,	,,	,,	×			×			,,	
37	9	7 10	7 0	1 30		2nd	×			×			,,	
38	10	6 15	6 5	0 20		2nd	×	×	×	×	×	×	,,	
39	11	6 15	4 10	7 15		2nd		×	×		×	×	,,	2[illegible]6
40	,,	,,	,,	,,		,,		×	×		×	×	,,	
41	,,	,,	,,	,,		,,		×	×		×	×	,,	
42	12	21 0	24 0	A few minutes	2½	1st	×			×			,,	2[illegible]45
43	,,	,,	,,	,,	,,	,,	×			×			,,	

THE DEDUCTIONS THEREFROM.

17. Contraction of Column of Air. λ	18. Bulge of Membrane at time of Bursting. h	19. Effective Pressure at Bursting, in inches of Mercury. $\frac{b\lambda}{l-\lambda}$.	20. Diameter of Sphere of Membrane at Bursting. $h+\frac{a^2}{h}$.	21. Pressure on Square Inch. p $\cdot 49\frac{b\lambda}{l+\lambda}$.	22. Pressure on a Circular Surface of 2·25 inches radius. $1{\cdot}73\frac{b\lambda}{l-\lambda}.\left(h+\frac{a^2}{h}\right)$.	23. Tensile Strength of Membrane. T $\cdot 123\frac{b\lambda}{l-\lambda}.\left(h+\frac{a^2}{h}\right)$.	REMARKS.
2·		3·32		1 63			
2·		3·32		1·63			
1·		1·57		·77			
3·25	1·5	·58	3·37	2·85	33·87	2·41	
·5		·766		·37			Membrane taken close to placenta.
1·5		2·42		1·19			Membrane slipped out.
·75		1·16		·57			
1·25		1·99		·97			Membrane slipped out.
1·		1·57		·77			
1·5		2·42		1·19			Membrane slipped out.
1·25		1·99		·97			
2·		3·32		1·63			Membrane slipped out.
1·25		1·96		·96			Membrane taken close to placenta.
·75		1.15		·56			
1·2		1.88		·92			
·6		·912		·45			
·4		·602		·29			
·6		·912		·45			
1·1		1·72		·84			
·5		·756		·37			
·5		·756		·37			
·8		1·23		·60			A considerable leak in the membrane.
·2		·298		1·46			
2·5	·75	4·98	4·49	2·46	37·58	2·75	Barometer corrected by 1·6 inch.
2·	·75	3·78	4·49	1·85	29·36	2·09	Barometer corrected by 1·6 inch.
2·25	1·	4·40	3·80	2·16	28·96	2·06	Barometer corrected by 1·6 inch.
2·25	·9	4·40	4·02	2·16	30·76	2·17	Barometer corrected by 1·6 inch. A leak in the membrane.
2·5	1·	5·07	3·80	2·48	33·37	2·37	Barometer corrected by 1·6 inch.
1·5	·75	2·83	4·49	1·39	15·06	1·07	Barometer corrected by 1·6 inch.
1·	·75	1·83	4·49	·90	14·21	1·01	Barometer corrected by 1·6 inch.
2·25	·75	4·40	4·49	2·16	31·18	2·43	Barometer corrected by 1·6 inch.
·6	·75	1·06	4·49	·52	8·23	·58	Barometer corrected by 1·6 inch. Birth 30 min. after rupture.
5	1·	·882	3·80	·43	5·81	·41	Barometer corrected by 1·6 inch.
·4	·75	·693	4·49	·34	5·38	·38	Barometer corrected by 1·6 inch.
·9	1	1·61	3·80	·79	10·60	·75	Barometer corrected by 1·6 inch.
·9	1·25	1·61	3·49	·79	9·73	·69	Barometer corrected by 1·6 inch.
·6	1·	1·07	3·80	·52	7·04	·50	Barometer corrected by 1·6 inch.
·3	·75	·526	4·49	·26	4·08	·29	Barometer corrected by 1·6 inch.
·75	·75	1·15	4·49	·56	8·93	·63	
·75	·75	1·15	4·49	·56	8·93	·63	
·4	·25	·604	11·47	·30	11·99	·85	
1·8	1·	2·83	3·80	1·39	18·63	1·32	
·9	·75	1·35	4·49	·66	10·48	·74	

TABLE OF EXPERIMENTS,

1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.
44	,,	,,	,,	,,	,,	,,	×			×			,,	,,
45	13	13 0	12 45	0 45		2nd		×	×			×	,,	,,
46	,,	,,	,,	,,		,,		×			×		,,	,,
47	14	11 0	10 15	0 30 ?		2nd	×			×			,,	,,
48	,,	,,	,,	,,		,,	×						,,	,,
49	,,	,,	,,	,,		,,	×			×			,,	,,
50	,,	,,	,,	,,		,,	×			×			,,	,,
51	,,	,,	,,	,,		,,	×						,,	,,
52	,,	,,	,,	,,		,,	×						,,	,,
53	15			7 30		1st	×	×					,,	28·8
54	,,			,,		,,	×	×			×		,,	,,
55	,,			,,		,,	×						,,	,,
56	,,			,,		,,	×			×			,,	,,
57	16	6 30				2nd	×			×			,,	29·0
58	,,	,,				,,	×			×			,,	,,
59	17	3 30	9 30	1 0	½	1st	×			×			,,	,,
60	,,	,,	,,	,,	,,	,,		×	×		×	×	,,	,,
61	18						×						,,	,,
62	,,						×			×			,,	,,
63	,,						×			×			,,	,,
64	19		6 0	1 0		2nd	×			×			,,	29·2
65	,,		,,	,,		,,	×			×			,,	,,
66	,,		,,	,,		,,	×			×			1·125	,,
67	,,		,,	,,		,,	×			×			,,	,,
68	,,		,,	,,		,,	×						,,	,,
69	,,		,,	,,		,,	×			×			,,	,,
70	,,		,,	,,		,,	×			×			,,	,,
71	,,		,,	,,		,,	×			×			,,	,,
72	,,		,,	,,		,,		×	×		×	×	,,	,,
73	20	9 15	8 20	0 55		3rd		×	×		×	×	,,	,,
74	21		9 0	3 0		,,	×			×			,,	,,
75	,,		,,	,,		,,	×			×			,,	,,
76	22	1 30	2 0	1 0		2nd	×			×			,,	29·8
77	,,	,,	,,	,,		,,	×			×			,,	,,
78	,,	,,	,,	,,		,,	×			×			,,	,,
79	,,	,,	,,	,,		,,	×			×			,,	,,
80	23		Long	1 0			×			×			,,	,,
81	,,		,,	,,			×			×			,,	,,
82	,,		,,	,,			×			×			,,	,,
83	,,		,,	,,			×			×			1·675	,,
84	24	32 30	30 0	4 0		2nd	×			×			1·125	,,
85	,,	,,	,,	,,		,,	×			×			,,	,,
86	,,	,,	,,	,,		,,	×			×			,,	,,
87	,,	,,	,,	,,		,,	×			×			,,	,,
88	,,	,,	,,	,,		,,	×			×			1·675	,,
89	25						×	×		×	×		1·125	29·8
90	,,						×	×		×			,,	,,
91	,,							×			×		,,	,,
92	,,						×			×			,,	,,
93	,,						×			×			1·675	,,
94	,,						×			×			,,	,,
95	,,						×			×			,,	,,
96	,,						×						,,	,,
97	,,						×			×			,,	,,
98	26	10 10	8 30	2 0		2nd	×			×			1·125	,,
99	,,	,,	,,	,,		,,	×			×			,,	,,
100	,,	,,	,,	,,		,,	×			×			,,	,,

THE DEDUCTIONS THEREFROM—*continued*.

17.	18.	19.	20.	21.	22.	23.	
1·3	·8	1·99	4·31	·97	14·83	1·05	
·5	1·	·736	3·80	·36	4·84	·35	
·45		·661		·32			
·7	1·	1·04	3·80	·56	6·85	·49	
·8	1·5	1·19	3·37	·58	6·94	·49	Membrane slipped out.
2·	2·	3·18	3·40	1·56	18 72	1·33	
1·5	1·5	3·29	3·37	1·12	13·35	·95	
1·5	1·5	2·32	3·37	1·14	13·52	·96	Membrane slipped out.
2·	2·	3·18	3·40	1·56	18·72	1·33	Membrane slipped out.
1·5	1·	2·21	3·80	1·08	14·55	1 03	Membrane found to have been injured.
1·25	1·	1·82	3·80	·89	11·98	·85	
2·25	2·	3·45	3·40	1·69	20·31	1·44	
1·25	1·	1·82	3·80	·89	11 98	·85	
2·7	1·1	4·28	3·65	2·10	27·03	1·92	Child born 30 minutes after rupture.
2·5	1·	3 92	3·80	1·92	25·80	1 83	
·5	·5	·700	6·11	·34	7·40	·53	
·3	·5	·416	6·11	2·04	4·40	31	
1·1	·8	1·65	4·31	·81	12·29	·87	Membrane slipped out.
1·25	1·	1·89	3·80	·93	12·44	·88	
2·3	1·5	3·66	3·37	1·79	21·34	1·52	
1·5	1·	2·45	3·80	1·20	16·13	1·15	
1·6	1·5	2·62	3·37	1·28	15 27	1·09	
2·	·75	2·87	2 44	1·41	12·10	·86	
1·7	·5	2 41	3·03	1·18	12·64	·90	An ill-conducted experiment.
2·3	1·1	3·36	2·25	1·65	13·08	·93	
1·9	·75	2·72	2·44	1·33	15·68	1·11	
2 5	·75	3·69	2·44	1·81	15·56	1·11	Burst by air contained between the membrane and water.
1·7	·55	2·41	2·85	1·18	11·88	·84	Burst by air contained between the membrane and water.
·9	·5	1·23	3·03	·60	6·45	·46	
1·2	·6	1·66	2·71	·81	7·78	·51	Membranes not ruptured till after birth of head.
1·2	·6	1·67	2·71	·82	7 83	·56	Membranes ruptured with first pain of labour.
1·7	·8	2·42	2·38	1·19	9·97	·71	
2·	·5	2·98	3·03	1·46	15·62	1·11	A large leak in the membrane.
2·4	·65	3·65	2·60	1·79	16·40	1·17	A small leak in the membrane.
3·2	·75	5·07	2·44	2·48	21·37	1·52	
2·85	·6	4·43	2·71	2·17	20·76	1·48	
2 3	·7	3·48	2·51	1·70	15·10	1·07	
1·8	·6	2·91	2·71	1·43	13·64	·97	
2·1	·5	3·49	3·03	1·71	18·30	1·31	A small leak in the membrane.
1·5	1·05	2·51	3 72	1·23	16·16	1·15	
1·	6	1·58	2 71	·77	7·40	·53	Membranes ruptured in labour by attendant.
1·4	·7	2·30	2·51	1·13	9·98	·71	Membranes ruptured in labour by attendant.
1·7	·75	2·84	2·44	1 39	11·97	·85	Membranes ruptured in labour by attendant.
·9	·55	1·44	2·85	·71	7·10	·50	Membranes ruptured in labour by attendant.
1·4	·85	2·33	4 15	1·14	16·73	1·19	Membranes ruptured in labour by attendant.
3·4	·65	5·36	2 60	2·63	24·08	1·71	Barometer corrected by ·5 inch.
3·6	·8	5·74	2·38	2·81	23·65	1·68	Barometer corrected by ·5 inch.
1·8	·9	2·61	2·31	1·28	10·41	·74	Barometer corrected by ·5 inch.
3·9	75	6·32	2·44	3·10	26·64	1·89	Barometer corrected by ·5 inch.
1·25	·8	2·08	4·31	1·02	15·50	1·10	A leak in the membrane.
1·3	·8	2·19	4·31	1 07	16·32	1·16	
1·45	·85	2·46	4·15	1·20	17·66	1·26	
2·	1·5	3·51	3·37	1·72	20·46	1·45	Membrane slipped out.
1·7	1·	2·93	3·80	1·44	19·29	1·37	Same piece as used in last experiment.
1·5	·6	2 15	2·71	1·05	10·07	·72	Barometer corrected by ·5 inch.
1·6	·7	2·30	2·51	1·13	9·98	·71	Barometer corrected by ·5 inch.
1·8	·6	2·61	2·71	1·28	12 23	·87	Barometer corrected by ·5 inch.

the slipping of the membrane. The twenty-second column gives the pressure on a circular surface of 2·25 inches radius, or equal to the assumed dimensions of the lumen of the passage through which the child is expelled. The twenty-third column gives the tensile strength of the membrane, or, in other words, the weight which a band of it, an inch broad, would bear without giving way.

Professor TAIT has supplied the following formulæ from which the columns of the tables are computed :—

Let b be the height of the barometer, corrected for the short column of mercury in the gauge;

l the length of the air-column before pressure is applied;

λ the contraction of the column, when the membrane bursts.

Then, since the weight of a cubic inch of mercury, at ordinary temperatures, is about 0·49 lbs., we have, for the difference of pressures on opposite sides of the membrane when it bursts, the expression

$$p = 0{\cdot}49\, b\left(\frac{l}{l-\lambda} - 1\right) = 0{\cdot}49\,\frac{b\lambda}{l-\lambda}, \qquad (1)$$

in pounds per square inch. No sensible correction is required for the length of the water-column, when the mercury in the gauge and the membrane were not exactly at the same level.

If T be the force in pounds weight which will just snap a band of the membrane an inch broad, ρ the radius of curvature when the membrane bursts, we have, by a known theorem, the membrane being supposed to form approximately a portion of a sphere,

$$\frac{2T}{\rho} = p \qquad (2).$$

To find ρ, we remark that the external semidiameter of the apparatus a is the radius of the base of a spherical segment, whose height h is measured; and geometry gives at once the equation

$$2\rho = h + \frac{a^2}{h} \qquad (3).$$

Hence, the tensile strength of the membrane is

$$T = 0{\cdot}123\,\frac{b\lambda}{l-\lambda}\left(h + \frac{a^2}{h}\right) \qquad (4).$$

If we assume that the membrane is usually burst, by natural processes, when a portion of it forms a hemisphere of 2·25 inches radius, the requisite pressure in pounds per square inch will be, by (2) and (4)

$$\frac{0{\cdot}245}{2{\cdot}25}\,\frac{b\lambda}{l-\lambda}\left(h + \frac{a^2}{h}\right) \qquad (5).$$

and the effective pressure, on a circular surface of 2·25 inches radius, will then be

$$\pi\,(2{\cdot}25)^2\,\frac{0{\cdot}245}{2{\cdot}25}\,\frac{b\lambda}{l-\lambda}\left(h + \frac{a^2}{h}\right) = 1{\cdot}73\,\frac{b\lambda}{l-\lambda}\left(h + \frac{a^2}{h}\right) \qquad (6).$$

In making such experiments, a small given error in the estimate of the depth of the approximately spherical segment will be of least consequence, when the membrane bursts in a nearly hemispherical form, for by (3)

$$2\delta\varrho = \delta h\left(1 - \frac{a^2}{h^2}\right)$$

and the error in the estimated radius vanishes, if $h = a$. Hence, also, the assumption that, in nature, the rupture takes place when the protruded portion of the membrane is hemispherical, gives a *minimum* value of the whole extruding force.

For the purposes of this paper the greatest value of the Table lies in the twenty-second column, which gives the power of the labour at the time of the rupture of the membranes and evacuation of the liquor amnii, on the supposition that the lumen of the passage opened up was circular, and of 4½ inches in diameter, and that the bulge was hemispherical at bursting. The first striking observation to be made, is the great variation in the strength of the bag of membranes. The force required to rupture the weakest amnion showed that the power of the labour was at least 4·08 lbs.; that for the strongest, a power of 37·58 lbs.; and the average power indicated by the experiments on the amnion, was 16·73 lbs. The average tensile strength was 1·19 lbs. Next, it is to be remarked, that in the cases whose membranes were tried, the power of labour almost certainly exceeded the power required to burst the bag, for it is not probable that a particularly weak small portion, unlike the rest of the membrane, was ruptured in the labours.

In cases 5, 6, 10, 14, 16, 26, the labour did not last above half an hour after the rupture of the membranes; and the greatest power indicated experimentally by rupturing the membranes was in each case respectively 37·58 lbs., 31·18, 4·08, 18·72, 27·03, 12·23.

In case 22, it was particularly observed by me that, so far as I could judge,* the pain rupturing the bag was stronger than any that followed; it may therefore be supposed, that the power terminating labour little exceeded 21·37 lbs., the greatest power indicated by the experiments as rupturing the membranes.

It was only after conceiving the means above described for arriving at the conclusions of this paper, and after the plan of the apparatus had been made by Professor TAIT, that I fell in with an interesting and valuable paper by Dr J. POPPEL of Munich—"Ueber die Resistenz der Eihäute, ein Beitrag zur Mechanik

* The same contractile force of the uterus at different periods of labour, or, to be more exact, at different dimensions of the uterus, will produce greater internal pressure, and, consequently, greater expulsive force, as the uterus is smaller (*vide* equation (2), p. 646); and, the amount of muscular contraction being supposed to be the same, there may be no sign to the attendant or patient of the increase of power. On the other hand, the application of the same principle shows, that, when the curvature of the extruded portion of the membranes is greatest, the difficulty of rupturing them is also greatest. This occurs when the extruded portion is hemispherical; and it is on this supposition that the numbers in column 22 are calculated.

der Geburt," contained in the first part of the twenty-second volume of the "Monatsschrift für Geburtskunde" for 1863. This paper anticipates to a very great degree the plans and results here related. But it may be pointed out that Dr POPPEL has neglected to note some conditions of the experiment, which cannot be omitted without damaging materially the accuracy and value of the trials; especially, he has always supposed the membrane to burst when in a hemispherical form, which is certainly an error, and one whose tendency is always to make the strength of the membrane too little (*vide* equation (2)). He has attached some weight to the part of the amnion tested, considering that greater strength would accompany proximity to the placenta; but my experiments did not confirm this opinion.

Dr POPPEL's apparatus may be sufficiently, though not fully, described as follows:—The membrane to be tried he ingeniously fixed over one or other of two glass vessels, of the diameter of five centimetres or two inches, and of ten centimetres or four inches, respectively. The glass vessels were reagent glasses, from which the bottoms were taken off. The affixed membranes represented the bottoms of the reagent glasses. Into the corks of the glasses a long glass tube was passed. Through this tube mercury was poured into the bottle till it filled it, and mounted into the tube. Its height in the tube at the time of the bursting of the membranes was carefully noted, because from it was estimated the pressure that burst the membrane. In adding the mercury fitfully, Dr POPPEL erroneously supposed that he imitated the pains of labour, a point, it appears to me, of no importance; and besides, his idea was manifestly erroneous, for each succeeding pain is not an addition to a force previously in action—it may even be weaker than its predecessor. In every natural case it is an entirely new force, rising in strength from zero to its acme, and again gradually fading to zero. Dr POPPEL made allowance for the weight of mercury contained in the reagent glass, over and above what was in the vertical glass-tube; but he neglected the important element of the degree of bulging of the membrane or radius of its curvature at time of bursting, with a view to arriving at the diameter of the globe, of which it formed a section at the time of rupture. With this he connects also a statement, that the bulging of the membranes through the mouth of the womb rarely exceeds a hemispherical form, which, though perhaps nearly true, is misleading, if held to be true in regard to the class of cases of persistent membranes specially studied in this paper.

The average strength of the amnion found by POPPEL was, keeping an aperture of 2·25 inches in radius in view, 19·21 lbs.; in my experiments it was 16·73 lbs.

POPPEL experimented on the membranes in seven cases in which they burst "with the birth." The following table gives the strength of the membranes in these cases, according to POPPEL's method of calculating, and the same changed into lbs., as well as increased proportionally from what appertains to a radius of 5 centimetres to what appertains to a radius of $2\frac{1}{4}$ inches, the dimensions used in our experiments :—

No.	Belastung bei 10 Centimeter Durchmesser Kilogramm.	Pressure for Diameter of 2·25 inches in lbs.
	Kilogrammes.	lbs.
1	9·876	27·232
3	2·346	6·469
12	2·134	5·884
13	7·608	20·979
22	4·709	12·985
23	9·461	26·088
28	7·001	19·305

This table gives us, in seven cases, a figure of strength nearly equalling the whole power of labour in these cases. If, in any of the cases, the membranes had persisted after the birth, then the figure in the last column would have certainly exceeded the whole propelling power of labour at any moment during the whole of the labour. Speaking of them, POPPEL remarks, that "if we reflect that the table expresses only the minimum of power for the easiest labours, the figures appear to be quite trustworthy, even though they exhibit great variations. It may therefore be assumed that in a very easy labour a power, varying from 4 to 19 lbs., presses the head through the pelvis." As Dr POPPEL gives the passage transmitting the head a diameter of 4 inches, and as I prefer regarding it as nearer $4\frac{1}{2}$, so I, using meantime POPPEL's experiments and calculations, make the power exerted in an easy labour vary from about 6 lbs. to about 27 lbs., instead of from 4 to 19. I shall not meantime attempt to show whether POPPEL's assumed 4-inch diameter or my assumed $4\frac{1}{2}$-inch diameter is the more likely to be nearest the truth, because it would lead me into a class of questions remote from the subject matter of this paper.

If we observe, that in POPPEL's table of experiments and in mine the power shown to be sufficient to terminate an easy labour was often far exceeded in the course of other labours, we may enunciate the almost certain conclusion that a great mass of easy, and not merely of the easiest, labours is terminated by a power little in excess of that required to rupture the bag of membranes. The strongest membrane found in the experiments indicated, by the pressure required to burst it, an extruding force of $37\frac{1}{2}$ lbs. We may therefore, I think, safely venture to assert as a highly probable conclusion, that the great majority of labours are completed by a propelling force not exceeding 40 lbs.

If we regard the figure of 4 lbs. given by POPPEL as equal to the power exerted in the easiest labour he has observed, or the corresponding figure of 6 lbs. according to my calculations, and keep in mind that the average weight of the adult fœtus exceeds either of these weights, we are led to the conclusion that in the easiest labours almost no resistance is encountered by the child; that it glides into the world propelled by the smallest force capable of doing so; that, with the mother in a favourable position, the weight of the child is enough to bring it into the world—a result which many clinical facts at least appear to confirm.

Having thus given POPPEL's and my own estimate of the force exerted in natural parturition of the easiest kind, I can at present offer nothing positive from which to calculate the strength of labour in the general run of cases. My belief is that in ordinary labours the power exerted is not in general much above the lower limit; but other accoucheurs may see reason to entertain different opinions.

The higher limit of the power exerted in natural parturition has been variously estimated. There is an easy and obvious method of arriving at it. Cases are frequently occurring in which labour is artificially terminated by forceps, in circumstances which leave no doubt that, under delay, they would have come with difficulty to a spontaneous conclusion. The power exerted by the forceps in such cases can be measured. Such measurements are not to be at once taken as the power of labour necessary to finish such cases; but when all of the various sources of error are considered and included, they are of much value. The chief of such sources of error are the neglect of the assistance that may be afforded to the operator by the natural expulsive efforts, and the including of such forces exerted by the forceps as may be unnecessary for carrying on the process; for example, prematurely applied force, or force applied so as to advance the birth too hastily, or force lost by being used in a wrong direction. For the making of observations of this kind by the forceps special instruments have been invented by KRISTELLER and JOULIN.

But forceps-cases do not afford the only evidence available as to the higher limit. Experiments can be made on the dead subject which can be very well relied upon, as reproducing correctly the difficulty encountered in the living, and the power required to overcome it. Such experiments have been made by JOULIN,* and when suitably arranged, give us the power exerted in cases which may be spontaneously terminated by the most powerful parturient efforts; and, it may be added, with great risk of the mother's life.

Speaking of these experiments, JOULIN makes the following remarks:†—"Spontaneous delivery has been sometimes observed in circumstances almost identical. It appears to me, therefore, possible to admit that the figure of 50 kilogrammes (about a hundredweight) of force represents very nearly the maxi-

* Mémoires de l'Académie Impériale de Médecine. Tome xxvii. p. 90, &c. See also his Mémoire sur l'emploi de la Force in Obstétrique. Archives générales de Médecine: numéros Février et Mars 1867.

† Traité complet d'Accouchements, p. 477.

mum of the contractile power of the uterus; for it is necessary to take into account the accessory contingent furnished by the abdominal muscles, which in these instances was awanting. But as this force has not a direct action, it is probable that its actual product scarcely rises above a few kilogrammes."

Having had extensive and varied experience in the use of the forceps in difficult labours, and having also made some rough experiments with the dynamometer, to ascertain the power I have applied by the instrument, I regard M. Joulin's estimate of a hundredweight as the maximum force of the parturient function as far too high. I do not deny that, in very rare cases, such a force may possibly be produced; but I am sure that it is nearer the truth to estimate the maximum expulsive power of labour (including, with the uterine contractions, the assistant expulsive efforts) as not exceeding 80 lbs.

At present, I can divine no method of arriving at an estimate of the expulsive power of ordinary labours, except the following; and I must guard myself from being supposed to recommend its use, in the meantime at least. A fine tube, filled with water and of resisting material, may be introduced into the small pool of liquor amnii which remains after the rupture of the membranes filling up the spaces otherwise vacant on the anterior aspect of the fœtus. This tube should be provided with an aperture at its uterine end; it should be curved, so that when introduced it may lie easily in the pelvis, occupying the least possible space, so that no unnecessary resistance be offered to the advance of the fœtus; its wall should taper to either side, a cross section of it having a long pointed fusiform outline, in order that its presence may not produce on either side of it a channel for the running off of the pool of liquor amnii; lastly, its external end should be in communication with a column of mercury in a vertical tube, enclosing a column of air under only ordinary barometrical pressure. During the pains the rise of the mercury in the tube may be measured, and calculations from these measurements might be made identical with those already given in an earlier part of this paper. By this means, if successfully applied, the force of any labour may be exactly known. And it is scarcely necessary even to suggest how immeasurably valuable to the accoucheur such an estimate would be, substituting, as it would, an experimentally accurate statement of awful importance for the vague notions at present relied on, even when the wisest and most experienced practitioner lends his counsel.

I have already expressed my opinion as to the great practical importance of the inquiry entered upon in this paper. Although it is, as yet, far from completed, there is enough demonstrated to enable Dr Slop, if he have an opportunity, to cast ridicule on the father of Tristram Shandy, who, founding on the statements of Lithopœdus Senonensis, asserts, that the force of a woman's efforts is, in strong labour-pains, equal, upon an average, to the weight of 470 lbs. avoirdupois, acting perpendicularly upon the head of the child !!

Fig. 1.

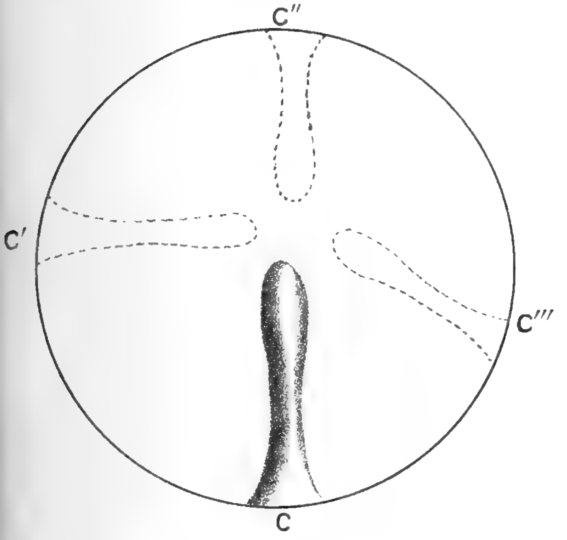


Fig. 2.

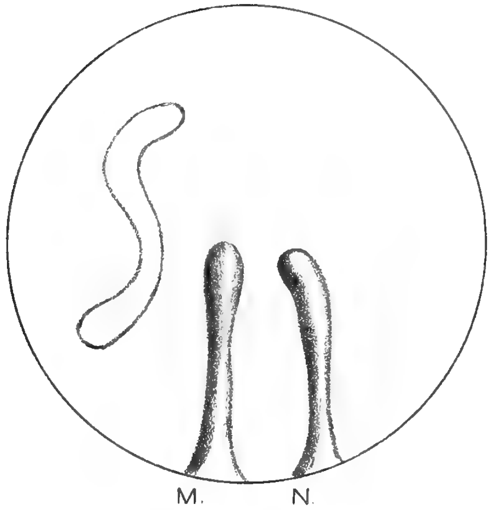


Fig. 3.

Fig. 4.

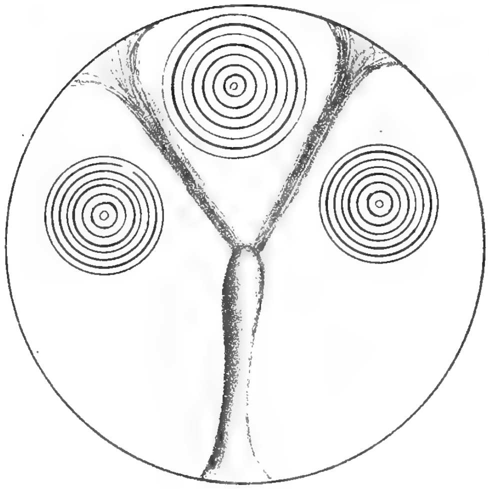


Fig. 5

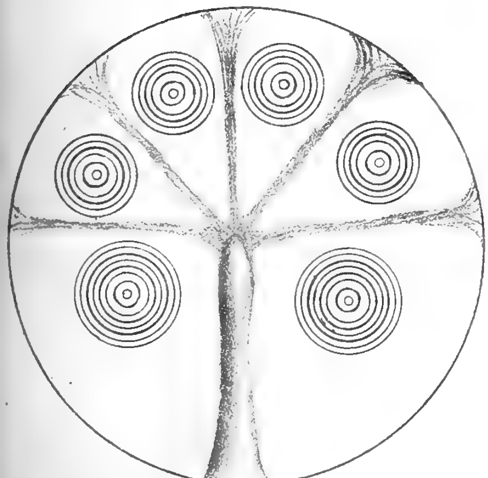
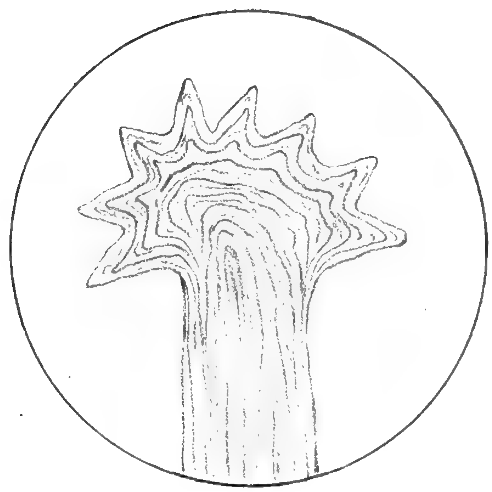


Fig. 6.



XLIII.—*On the Motions and Colours upon Films of Alcohol and Volatile Oils and other Fluids.* By Sir David Brewster, K.H., F.R.S. (Plate XXXIX.)

(Read 4th March 1867.)

In a paper "On the Phenomena of Thin Plates of Solid and Fluid Substances exposed to Polarised Light," published in the "Philosophical Transactions" for 1841,* I had occasion to notice certain motions and colours which I had observed upon films of some of the volatile oils; but as they were unconnected with the subject I was then investigating, I made no attempt to discover their nature and origin. Their apparent similarity, however, to the molecular movements and colours upon the soap-bubble, induced me to resume the subject, and to examine them as exhibited upon films of various evaporable liquids, stretched over apertures differing in size, form, and substance.

If we place a drop of alcohol upon an aperture C, C′, C″, C‴, Fig. 1, held horizontally, about the *fifth* of an inch or less in diameter, a concave lens will be formed upon it. As the alcohol evaporates, a very small plane film will appear in the centre, and will gradually increase in size till it fills nearly the whole aperture. If we hold the film in a vertical position, and examine it by transmitted light, we shall see a current of fluid, C, rising from the circumference of the film, moving rapidly from one part of the circumference to another, occasionally taking a horizontal position, and sometimes descending from the apex and sides of the film, as shown at C′, C″, C‴.

The current C is sometimes broad and flat, and separates into two currents, M, N, Fig. 2, which dance, as it were, opposite each other, assume the form of the letter S, and turn heels over head when they quit the circumference of the film.

These apparent currents generally throw out secondary currents, as in Figs. 3, 4, and 5, and the whole of them continue in rapid motion, exactly like a transparent insect struggling to escape.

When an excess of fluid is placed in the aperture held vertically, it occupies the lower part of it, and the film, no longer circular, appears in the upper part; but notwithstanding this change in its form and condition, the currents upon it present the same phenomena.

If the film is formed upon apertures of an irregular shape, it has, of course, the same shape, but the form and motions of the currents are not changed.

In some cases small currents issue from a part of the circumference of the films opposite the principal current; and in other cases small globules drop from the extremity of the principal one.

* Phil. Trans. 1841, p. 43.

The currents which are produced upon films of the alcohol of commerce appear also upon those of absolute alcohol; but though the films are smaller and less persistent, the currents are more active and varied in their movements. Similar currents are produced upon films of various solutions containing alcohol, of alcoholic solutions containing water and sugar, and of a large number of volatile and fixed oils, which, through the kindness of Dr PLAYFAIR and Dr CHRISTISON, I have been enabled to examine. They appear also, but with less activity, upon films of a solution of New Zealand gum in oil of laurel—the remarkable fluid by which the late Mr DELARUE produced the brilliantly coloured papers which were shown at the Great Exhibition of 1851.

In all these experiments the principal current and its different ramifications are perfectly colourless, and consequently exceed the thickness in NEWTON'S scale at which the colours of thin plates make their appearance. In good and persistent films slight colours appear between the secondary currents; but these, as we shall presently see, are the complementary colours of those seen by reflection.

The various phenomena which I have described may be seen in a magnified form, by placing the films in a beam of divergent light, and they might be exhibited to an audience by means of the magic lantern.

If we now examine the surface of the alcoholic film by reflected light, we shall observe a series of phenomena of a very different kind. The principal current and its branches will be seen almost as distinctly as by transmitted light; but they are accompanied with, or rather they produce, systems of coloured rings of great beauty, shifting their place on the film, expanding and contracting quickly, and rapidly changing their form and their colours. Each pair of systems has on one or both sides a secondary current which stops or disturbs the rotatory motion, which would be communicated to two systems by the action of a single current.

When the film is first formed, especially if it is a very small one, there is only one system which is maintained by the colourless fluid issuing from the margin, sometimes in closely packed bands, of very high orders of colours. The lowest colour is always in the centre of the system, but the central tint is never lower than the white of the first order. When the tint occurs which NEWTON calls the beginning of black, the film always bursts.

This single system of rings is finely seen in films of very old balsam of *copaiba* which I obtained from Dr CHRISTISON'S Museum. The film was wholly occupied by a circular system contracting and expanding quickly, changing its central tint, becoming elliptical, and even of a crescent form, when pushed onward by the thicker fluid from the margin of the film. These movements were kept up for more than an hour without any rotatory motion, and had not the film burst from an accident, they might have continued much longer.

After the single system of rings has appeared upon a film of alcohol, the film gradually increases in breadth from evaporation, and the principal current throws out a secondary current, as in Fig. 3, which throws the colouring matter into two systems of rings that seem to move, the one from right to left, and the other from left to right, like those produced on the soap film by a current of air.

Another secondary current gives rise to three systems of rings, as in Fig. 4, and several such currents to several systems, as in Fig. 5, in which the motions are so rapid that it is difficult to follow them.

In this condition of the film the principal current becomes flat, and itself becomes divided, as it were, into narrow bands, and the various systems of rings unite into one singular system, rudely represented in Fig. 6; the tints beyond the salient points being at the margin of the film, the *white* of the first order gradually rising to the higher tints of NEWTON'S scale. The contracting and expanding motions of this system become slower and slower, and the film generally bursts, scattering its fluid round the aperture upon which it was formed.

In some cases, which are very rare, the whole system of rings disappears with the principal current—the film becomes perfectly quiet and colourless, and, as in the soap film, the colouring matter, in the form of bands and tadpoles, comes upon it from its margin, till the black of the first order covers the film, and causes it to burst. This is a very instructive result.

Owing to the irregular movements of the secondary currents, it is difficult to observe the direction of the motion of the systems of rings in Figs. 3, 4, and 5; but they certainly revolve in opposite directions, like those produced by an artificial current in the soap film. When there is only one system of rings it has not a rotatory movement, because there is no decided current to put the colouring matter in motion; but when there is a principal current, with several secondary ones, as in Figs. 4 and 5, the secondary currents prevent, by their opposite actions, the intermediate rings from revolving.

The systems of rings which I have described are seen with various modifications, in about 70 or 80 volatile and fixed oils, and other liquids, which I have had an opportunity of examining. In most of them the motions of the currents and rings are very rapid. In some a film cannot be obtained, as in the ground nut-oil, the oil from the *Sesamum orientale,* and the purified oil of bitter almonds. In others the film, though it exhibits the play of colours, does not exceed the 50th or 60th of an inch in diameter. In *Elaine,* from olive-oil, and in the oil from the *Jatropha curcus*, a very small film is obtained, which instantly becomes black, and disappears. In Mr DELARUE'S solution of New Zealand gum in oil of laurel, the motions of the rings are very languid, but the film is very persistent; and, what is interesting, I observed a number of black particles pass across the rings, and break their outlines, showing that the colouring matter floated upon the elastic film, as in the soap-bubble. The same effect was sometimes produced by blowing upon the film.

In the preceding experiments the films were obtained upon apertures in zinc, wood, and card-board, and on small rings of glass, platinum, iron, copper, and brass; but in the films upon all these substances the phenomena were the same. The films were more persistent when the apertures which they covered were made in plates of greater thickness, and some slight modifications of the phenomena arose from the same cause.

In reviewing the preceding experiments, it is impossible to resist the conclusion that the colours have the same origin as those on the soap-bubble. In both cases a colourless fluid issues from the circumference of the film, and spreads itself into rings and coloured bands, which are constantly changing their form and their colour. In some films the colouring matter seems to be occasionally recombined with the film, and the colours to reappear, and vary till the film bursts under the black of the first order. In the soap film the motion of the colouring matter is comparatively languid, but the bands and rings are, to a certain extent, under the influence of gravity, assuming a horizontal position during the rotation of a vertical film; but some other influence must be sought for, in order to explain the rapid and long-continued play of colours which is exhibited in films of alcohol and the volatile oils.

XLIV.—*On the Sophists of the Fifth Century*, B.C. By Professor Blackie.

(Read 18th March 1867.)

One of the most remarkable phenomena in our recent historical literature is a tendency to whitewash all characters which had previously presented a black appearance; to prefer the intellectual divination of a subtle modern professor to the plain testimony of a sober old chronicler; and generally to unsettle all things that we had in previous ages been taught to look on as settled. That this tendency, dating from the gigantic excavations of Niebuhr and Wolf, had its origin in an honest love of truth, and a searching scrutiny of evidence, cannot be doubted. That its results have in the main been beneficial is equally certain; but, on the other hand, it is not to be denied that it has sometimes run into the most wanton excesses, and that it has tainted some of the most notable historical productions of our age with a vice which will render it necessary for a future generation to repeat the work now done from a broader point of view, and with a juster criticism. Among the great works which have not escaped this prevalent contagion must be named the History of Greece, by George Grote. In this work, while the democratic institutions of Athens have been vindicated in the most masterly manner, and the political tone of the work may be regarded as, on the whole, sound, the author has in some prominent sections blotted his pages with the peculiarly German rage of substituting conjecture for fact, and overriding testimony by theory. And in doing this he has not only acted more like a German than an Englishman, but he has in some instances proceeded far beyond the bounds of negative criticism and bold assertion which the best German writers have observed. In no part of his work does this tendency, not only to overdo, but altogether to invert the natural order of things, appear more prominently than in his chapter on Socrates and the Sophists. In this part of his work, while he presents himself to the general reader as the chivalrous champion of injured innocence, the accurate weigher of historical evidence sees only another instance of the wonderful effect of a favourite theory in blinding a sensible man to the truth which radiates from the strongest testimony. To the reader of Mr Grote's chapter it must certainly seem as if Socrates had spent his life most stupidly, if not most basely, in fighting with a class of men, of which he himself was one, the best among many good, and that Protagoras was a far more sensible man, and, at bottom, a much more profound philosopher than Plato. The effect produced by this chapter of the history has been rather increased than diminished by the distinguished historian's comment on the Protagoras and other dialogues in his recent work on PLATO. Here also we are regularly given to understand that PLATO was a much overrated man, and that the true objects of human admiration are rather the men whom it was the constant object of his philosophy to refute. This is even a bolder stroke of what, borrowing a phrase from mathematicians, I may call the *invertendo* style of criticism, than any with which the world has been favoured from the disintegrating school of LACHMAN, KÖCHLY, and other trans-Rhenane commentators on the Homeric poems. They, at least, while they annihilated the poet, left us the poem to admire. Here, the divine objects of old reverence are thrown away as idols, and the old recognised idols are set up as the true God.

The great authority of Mr GROTE in all matters of Greek history, and the wide circulation of his work, render it expedient that a public contradiction should be given to his errors from as many independent quarters as possible; and, though I am perfectly satisfied with what I find written on this subject by an excellent scholar, Mr COPE of "Cambridge, in the Cambridge Philological Journal," vol. i., as also by Professor ZELLER, in his *Philosophie der Griechen*, Tübingen, 1856; yet, as my own opinions have been formed altogether independently, and are based on a careful study of PLATO, extending through a series of years, I have thought that a succinct statement of the bearings of this important historical question would not prove unacceptable to the members of this Society. I proceed, therefore, to make a short statement of Mr GROTE's views of this matter, followed by an equally short statement of how, from my point of view, his arguments ought to be met.

Mr GROTE ushers in the statement of his views by this general declaration—"I know few characters in history who have been so hardly dealt with as the Sophists; they bear the penalty of their name in its modern sense;" and the modern sense of the word, according to the whole tenor of the learned gentleman's argument, is about as far removed from the original and genuine sense, as the English word *demon* is from the Homeric word δαίμων. To restore the proper meaning, as he conceives, to this sadly misunderstood word, the learned historian brings forward, according to my analysis, five arguments.

(1) It is plain from PLATO himself—in this case we must suppose an unwilling witness—that many of the Sophists were excellent and sensible men, and in every way capable of being the instructors of youth.

(2.) In fact, the Sophists were the great teachers of the age to which they belonged; and SOCRATES owed his position and his influence altogether to being one of them. The great exhibition of young democratic energy which had culminated at Marathon, was now riding onward triumphantly to another and a higher development. Of this period of transition between the youth and the manhood of the Athenian intellect the Sophists were the natural, the necessary representatives, and the worthy spokesmen.

(3.) PLATO was a man of peculiar idiosyncrasy, a great intellect confessedly, but a crotchety pedant in some matters, and a transcendental dreamer in others. His witness—at bottom the only serious testimony against the Sophists—(for of the great jester ARISTOPHANES in such matters we need take no account) is consequently of no value, and cannot, without the grossest injustice, be quoted against such sober, sensible, and practical thinkers as PROTAGORAS and GORGIAS.

(4.) The immoral teaching, attributed to the Sophists, and set forth by PLATO through the mouth of CALLICLES in the Gorgias and THRASYMACHUS in the Republic, must be a figment; for the whole history of the Athenian democracy shows that such doctrines would have been utterly revolting to them, and men professing such doctrines never would have been allowed the slightest influence in the education of their sons.

(5.) The Sophists, in fact, as a body, had no peculiar system of morals, either bad or good; as little had they any system of philosophic doctrine. They were a profession, not a sect.

(6.) The standing objection made to the Sophists by PLATO, in almost all his Dialogues, that they were a venal and mercantile crew, because they taught philosophy for a fee, need scarcely require refutation at our hands, living, as we do, in a country where the expediency of payment for all sorts of professional work is universally recognised. One does not, indeed, see how the Sophists could have performed their duties as general Hellenic teachers, travelling from land to land, had they not exacted a considerable fee, if it were only to pay their travelling expenses.

These propositions, it will be seen, have a polemical aspect, as indeed it is both the vice and the virtue of Mr GROTE's book generally, that he is everywhere writing down an old view of Hellenic matters, and writing up a new one. In order, therefore, fully to understand the drift of his statements, we must set distinctly before us the old doctrine about the Sophists which he affects to have overturned; and though this might be done by a large array of testimonies from many quarters, it will be sufficient for our present purpose to cite two of the best known authorities, BRUCKER and GILLIES, who may be looked on as the generally recognised exponents of the ante-Grotian doctrine with regard to the Sophists. In his "History of Philosophy," vol. i. p. 549, the erudite old Augsburg theologian says:—"*Erant tum temporis Athenis Sophistæ, magistri docendi, quales Leontinus Gorgias, Thrasymachus, Protagoras Abderites, Prodicus Ceius, Hippias Eleus aliique, qui in eo potissimum artem consistere arrogantibus verbis jactabant, quemadmodum caussa inferior, dicendo superior evadere posset; id quod, docente* CICERONE *sententiarum magis concinnitate argutoque et circumscripto verborum ambitu quam eorum pondere efficere tentabant. Hinc homines vani, ambitiosi, avari, quique soli sibi sapere videbantur, et omnium disciplinarum cognitionem sibi arrogabant, non tantum hanc in utramque partem de quavis re proposita invictis argu-*

mentis disputandi artem publice exercebant, sed et magnificis eam promissis nobilem juventutem brevi tempore se docturos pollicebantur. Quae eo ardentius ad hos nugatores deproperabat, quod ita se utilissimam rationem discere posse speraret, populum in suas partes trahendi, et ex civium ad quos loquendum erat, judicio, et calculo summam rerum ad se trahendi, vel etiam in potestate semel acquisita, flexo populi per istam eloquentiam obsequio, se confirmandi." And Gillies, in his well-known "History of Greece," vol. ii. p. 133, says, in distinct antithesis to Mr Grote, that the "appellation *Sophist*, in its modern sense, pretty faithfully expresses their character," and that "their morality supplied the springs from which Epicurus watered his gardens, and their captious logic furnished the arguments by which Pyrrho laboured to justify his scepticism."

Now, in reference to these opposing views, my assertion is, that the old view, though not exhaustive of the whole truth of the matter, and not recognising certain modifications which tend to soften the harsher lines of the portrait, is on the whole the right view; while the new view, if containing an element of correction in some secondary points, is on the whole a false and misleading view, or rather a total misrepresentation and inversion of the facts of the case. The proof may be given, disposing of Mr Grote's six arguments in their order, as follows:—

(1.) The general character of the Sophists, in their capacity of public teachers, is in no wise affected by the fact that there were great differences in their personal characters, and that some of them, like Protagoras, were, as the world goes, most respectable and reputable men. The scribes and Pharisees in the Gospel history were respectable and reputable enough, no doubt, or had at least many most respectable and reputable men in their body; but not the less were their doctrines false and their teaching pernicious. So much only we may grant to the learned historian, that if any one ever said that there were no men of average respectability among the Sophists, such an assertion is altogether unwarranted, and is contrary to the plainest indications on the very surface of Plato.

(2.) A similar admission may be made with regard to the historical significance of the Sophists generally, without, in the slightest degree, trenching on the ground occupied by Plato. That the Sophists, like everything else in the world, had their good side, might have been assumed, if it could not have been proved; and it is equally certain, that when once a body of men like the Sophists, or the scribes and Pharisees, or the Romish priests, gets a bad name, the defects of character out of which that bad name arose are apt to occupy the whole of the canvass in historical tradition, while their virtues are altogether forgotten, or even denied. Hence arises a necessity for a sort of justification; a justification, however, which, while it may be allowed slightly to qualify, does not in any wise nullify the unfavourable character of the original verdict. A sort of plea in extenuation of this class of men was therefore, in the very nature of the case, to have been expected; and I am indebted to Professor Zeller's admirable *Geschichte* *der Griechischen Philosophie* for a reference to two of the earliest authorities, in which this reaction in favour of the Sophists appears. The one is MEINERS, in his *Geschichte der Wissenschaften*, published at *Lemgo* in the year 1782, and the other that of HEGEL, in his lectures on the history of philosophy delivered on various occasions soon after the commencement of the present century. Professor MEINERS (vol. ii. pp. 172–599) says, "The Sophists deserve not merely to be despised and denounced, but in many views they claim respect and eulogy—a recognition which even their most violent opponents have not refused. They were the great public teachers and enlighteners of Greece; they were a necessary link in the chain of intellectual life in Greece." But while admitting this, the same author says a little further on, that "their morality was right in the teeth of the Socratic morality," and that, "on a review of the whole matter, we must agree with XENOPHON, PLATO, ISOCRATES, and those who followed them, that the Sophists did their country more harm than good, and that they corrupted more hearts than they enlightened heads." This representation deserves special notice as contrasted with Mr GROTE'S; for, while it fully admits the extenuating circumstance, it does not deny the general truth of the crime charged. HEGEL places the palliative circumstance in a stronger light; indeed, he purposely brings it into the foreground, as being, in his phraseology, the one "positive and truly scientific side" of the matter. But by this he means, not that the faults with which the Sophists are generally charged did not really exist, but that whatever faults a faulty thing may possess, its virtues are the only element in it which has any value to a philosophic mind. From this point of view he says, that "the Sophists were the teachers of Greece, by whom intellectual culture (*Bildung*) was brought into existence. They came into the place of the poets and rhapsodists, who were originally the only teachers. Religion in Greece did not teach. Priests offered sacrifices, soothsayers divined the future, but instruction is something quite different." This is admirable; but with this the Berlin notional transcendentalist is far from shutting his eyes to the weak side of these teachers. He proceeds to represent them as practising a logic both superficial and unprincipled. He shows, also the peculiar danger which attached to such a logic when applied to practical purposes in an atmosphere of sensual polytheism. "In our European world," he writes, "intellectual culture appeared under the protection, so to speak, and on the foundation, of a spiritual religion. But when intellectual dexterity had to do only with a religion of the imagination, it readily shook itself loose from any central holding-point, or, at all events, particular subordinate points of view might easily be planted on the pedestal of an ultimate principle." And again, "A man of education and experience always knows how to set things in a good light for the momentary purpose. In the worst action something lies, which being singled out and skilfully presented, makes it defensible. A person must have gone a very short way in his intellectual education if he does not know how to advance fair reasons to justify the worst actions. All the evil that has happened in the world since Adam has happened with the help of fair reasons." From these passages, which I think could not possibly be better expressed, we see how little the granting of Mr GROTE's second argument has to do with the conclusion at which he so sweepingly arrives. The most comprehensive philosophical thinker of the most philosophic country in the world can see with the utmost distinctness that the Sophists were not all black, and yet that they dealt with the most important matters of human concernment in a loose and slippery fashion, which completely justified the attitude of uncompromising hostility constantly assumed towards them by both SOCRATES and PLATO.

(3.) Hitherto Mr GROTE's arguments, so far as they present a mere plea in palliation of the Sophists, have appeared not only plausible, but in the highest degree reasonable; and, had he stopped at this point, there would have been no question at this moment before the learned world on this matter. But, unfortunately, the democratic historian here, by over-pleading his case, betrays the inherent weakness of his cause. He claims a verdict of acquittal for his clients, and can only do so, as we shall now see, by attempting to override an array of historical testimonies, such as, in the general case, would make any but a thorough-paced German ideamonger shrink back in dismay. The witnesses in this case are not few, and they are all on one side. Let us see how the learned historian disposes of them. In the first place, he throws PLATO and ARISTOPHANES, the greatest thinker, and the greatest humorist, of the age, *simpliciter*, out of court; and then, by either overlooking other testimonies, or referring them back to the twin authors of the original calumny, he tells the jury, with a gay confidence, that there is nothing more in the case. But there is a wholesale air about this procedure, which, with a sober-minded man, only acts as a warning to use caution. To commence with the two original framers of the indictment. No doubt ARISTOPHANES was a maker of jests, but he was no mere buffoon. He was a great thinker as well as a great humorist; and his comedies expressly deal with all the principal literary, philosophical, and political questions of the age. Such men are not apt to fling their humorous shafts at a mere imagination. On the contrary, their strength lies in the fact, that the phenomenon which they ridicule has a wide, popular recognition, and is everywhere felt to be a fact. A man of the calibre of ARISTOPHANES could not have written such a comedy as "The Clouds," against such a class of persons as the Sophists, had not such a class of persons existed, any more than the well-known scientific song of "The Origin of Species," attributed to a witty Scotch law-lord, could have existed without a DARWIN and a school of Darwins. The humorist's view of the case, indeed, is not necessarily the scientific view; but it may be, and often is, the true view, or, at all events, represents strongly one true aspect of the case. Otherwise, not only would the humour be pointless, but a great humorist certainly would not meddle with the matter at all. Incidental errors, such as the confounding of SOCRATES with the mass of public teachers, of which he was one, do not affect the fundamental truth of the case. The "Clouds" is a play against the Sophists, not against SOCRATES.

But, however slight the value which a grave man may be inclined to give to the testimony of a great public humorist on a question of philosophy, if it stood alone, the case is completely altered the moment that his laughing testimony is confirmed by the serious witness of a professional thinker. The error which the greatest thinker and the greatest humorist of the age agree in condemning is not likely to have been an imagination. No doubt, in such a case, a great deal depends on the character of the philosopher; and PLATO is not a name likely to forestall favour with a class of minds largely represented in this land, which rejoices to call itself pre-eminently practical, and shares in a more than Napoleonic hatred of all ideology. But let us distinguish. PLATO undoubtedly had his crotchets: he was in some things a most unpractical man, and knew that he was so; unquestionably, also, his theory of ideas may often have been stated in exaggerated language, and with a paradoxical air, which were justly provocative of the opposition which, ever since ARISTOTLE, it has encountered. But the testimony of the philosopher in reference to the Sophists is a thing much broader, and rooted much more deeply, than any of his crotchets about methodising the sexual instinct, or the possibility of his ideal polity. Here we have the fact that a great philosopher of all-commanding mind, the founder of a great and permanent school of thinking, who stood to his age in the same relation that BACON does to ours, makes it the business of his life to write against, and represents his great master, SOCRATES, as having made it the business of his life, to speak against, a class of men who professed certain principles generally esteemed pernicious, but which, according to Mr GROTE's view of the truth, were, in fact, most excellent and laudable. And this testimony, so given, was accepted by the universal voice of antiquity. It met, in fact, no decided contradiction till the epiphany of Mr GROTE. Now, there is nothing altogether impossible in the supposition that Mr GROTE may be right. It may sometimes be given to a NIEBUHR, after a lapse of 2000 years, to reconstruct a history of Rome; but we are not to start with a prepossession in favour of such brilliant novelties. They are rather to be looked on with suspicion, and require strong backing. PLATO, moreover, it must be borne in mind, with all his tendency to one-sided exaggerations, was by no means a narrow-minded, an ungenerous, much less a spiteful or ill-natured man. No man was more in the habit of looking at both sides of a question, and more unlikely to create a man of straw for an adversary. His treatment of PROTAGORAS, GORGIAS, and other Sophists, is what we would call gentlemanly in the highest degree, and gives the reader a sort of guarantee that what he alleges against the general body to which he belonged had some good foundation. In weighing the testimony of PLATO and ARISTOPHANES also, with regard to such a class of men as the Sophists are alleged to have been, we must consider the presumptions and possibilities of the case. Is there anything strange or improbable in the statement, that in a talking town like Athens, full of all sorts of quick-witted and light-witted democratic people, there should have arisen, in an age of intellectual transition, a set of shallow thinkers, who cultivated the faculty of expression at the expense of the faculty of thinking, and exercised their understanding with a clever logical dexterity, rather than with the earnest search after truth? To myself it seems the most natural thing in the world to suppose the existence of such a class of men—a class of men, indeed, almost certain to exist at all times wherever there is a demand for them; and particularly dangerous, as HEGEL remarks, in a country where a sensuous religion exists, altogether divorced from any serious training, either of the intellect or the character.

Starting from these presumptions, I must confess I should be inclined to accept the portrait of the Sophists in every feature, and with its full colouring, as given by the god of the philosophers, and the king of the humorists, even if their testimony in this matter stood alone. But the plain and admitted fact here is, that neither the philosopher nor the humorist do stand alone; they are supported by the consenting voice of antiquity. The heritage of Greek opinion on this subject was transmitted to CICERO; and he says (Acad. II. 23), "*Sophistæ appellantur qui ostentationis aut quæstûs causâ philosophantur.*" Among the Greeks themselves, those whose testimony was of the highest value, and who lived nearest to the time, and who were most interested in the subject, set their seal in the strongest language to the witness of the great idealist. Who are the writers whom a wise judge would call into court, and hear with impartial eagerness in a trial of this kind? SOCRATES and XENOPHON, ISOCRATES and ARISTOTLE—any one of these would be sufficient, in my judgment, to nail down, for an absolute certainty, whatever PLATO and ARISTOPHANES might have previously combined to testify as a prominent fact in the history of Greek intellectual life. Of these four, though the most remote in point of time, ARISTOTLE is the most weighty; and this not only on account of the accurate, inductive, and encyclopædic character of his mind, but specially on account of his known propensity to contradict everything that PLATO says, when it comes in his way. None of the products of that peculiarly Platonic idiosyncrasy, which Mr GROTE brings forward so prominently, does the Stagyrite show the slightest desire to spare. Spartan women and Platonic ideas are two matters, in discussing which he almost seems to lose for a moment the imperturbable judicial coolness of his intellect. But the Sophists he describes in exactly the same language as PLATO, and in language which forms a sufficient justification for the peculiar use of the name in modern times. In Soph. El. I. 6, he says, "Εστι γὰρ ἡ σοφιστικὴ φαινομένη σοφία οὖσα δὲ μὴ καὶ ὁ σοφιστὴς χρηματιστὴς ἀπὸ φαινομένης σοφίας ἀλλ' οὐκ ὄυσης."

The evidence of SOCRATES and XENOPHON need not be specified here in detail. They will be found below in a note, and have been admirably handled by Mr COPE in the Essay to which I previously alluded.* Only to the witness of ISOCRATES I call particular attention, as that of a man who was by the general bent of his mind not at all inclined to sympathise with any transcendental notions of high-strung intellectualists like PLATO, and who as himself one of the most reputable of the class of Sophists to whom GORGIAS belonged, would naturally feel no inclination to bring a charge against any large section of the fraternity, which might serve to increase the natural odium that in not a few quarters had always attached to the name. His words are as follows:—

"Τίς γὰρ οὐκ ἂν μισήσειεν ἅμα καὶ καταφρονήσειε πρῶτον μὲν τῶν περὶ τὰς ἔριδας διατριβόντων, οἱ προσποιοῦνται μὲν τὴν ἀλήθειαν ζητεῖν, εὐθὺς δ'ἐν ἀρχῇ τῶν ἐπαγγελμάτων ψευδῆ λέγειν ἐπιχείρουσιν; οἶμαι γὰρ ἅπασιν εἶναι φανερὸν, ὅτι τὰ μέλλοντα προγιγνώσκειν οὐ τῆς ἡμετέρας φύσεώς ἐστιν, ἀλλὰ τοσοῦτον ἀπέχομεν ταύτης τῆς φρονήσεως, ὥσθ' Ὅμηρος ὁ μεγίστην ἐπὶ σοφίᾳ δόξαν ἐιληφὼς καὶ τοὺς θεοὺς πεποίηκεν ἔστιν ὅτε βουλευομένους ὑπὲρ ἀυτῶν, οὐ τὴν ἐκείνων γνώμην ἐιδὼς ἀλλ' ἡμῖν ἐνδείξασθαι βουλόμενος, ὅτι τοῖς ἀνθρώποις ἓν τοῦτο τῶν ἀδυνάτων ἐστίν.

"Οὗτοι τοίνυν εἰς τοῦτο τόλμης ἐληλύθασιν, ὥστε πειρῶνται πείθειν τοὺς νεωτέρους, ὡς, ἢν αὐτοῖς πλησιάζωσιν, ἅ τε πρακτέον ἐστὶν εἴσονταὶ καὶ διὰ ταύτης τῆς ἐπιστήμης ἐυδαίμονες γενήσονται. καὶ τηλικούτων ἀγαθῶν αὐτοὺς διδασκάλους καὶ κυρίους καταστήσαντες οὐκ ἀισχύνονται τρεῖς ἢ τέτταρας μνᾶς ὑπὲρ τούτων ἀιτοῦντες. ἀλλ' ἐι μέν τι τῶν ἄλλων κτημάτων πολλοστοῦ μέρους τῆς ἀξίας ἐπώλουν, οὐκ ἂν ἠμφισβήτησαν, ὡς οὐκ εὖ φρονοῦντες τυγχάνουσι, σύμπασαν δὲ τὴν ἀρετὴν καὶ τὴν ἐυδαιμονίαν οὕτως ὀλίγου τιμῶντες, ὡς νοῦν ἔχοντες διδάσκαλοι τῶν ἄλλων ἀξιοῦσι γίγνεσθαι. καὶ λέγουσι μὲν, ὡς οὐδὲν δέονται χρημάτων, ἀργυρίδιον καὶ χρυσίδιον τὸν πλοῦτον ἀποκαλοῦντες, μικροῦ δὲ κέρδους ὀρεγόμενοι μόνον ὀυκ ἀθανάτους ὑπισχνοῦνται τοὺς συνόντας ποιήσειν."

(4.) With regard to the moral teaching of the Sophists, Mr GROTE is quite right when he says that such an unblushing assertion of the doctrine that might is right, as is propounded by CALLICLES in the Gorgias, however welcome to DIONYSIUS in his rocky hold at Syracuse, would have been anything but agreeable to the Athenian democracy. But it is not necessary for those who consider that the Sophists were bad, and sometimes very bad moral guides, to maintain that they went about everywhere advocating despotic principles. PROTAGORAS,

* The contrast between the doctrine of SOCRATES and that of the Sophists, in reference to the origin of moral distinctions, is shown distinctly in the discussion between the former and HIPPIAS, in Xen. Mem. iv. 4, 13; and in the same work, i. 2, 6, the well-known objection to receiving μισθός for teaching morality, is stated by SOCRATES exactly as in PLATO. XENOPHON's own opinion is expressed very strongly in the last chapter of the treatise *De Venatione:* "'Οι δε σοφισταὶ δ' ἐπὶ τῷ ἐξαπατᾶν λέγουσι, καὶ γράφουσιν ἐπὶ τῷ ἑαυτῶν κέρδει, καὶ οὐδένα οὐδὲν ὀφελᾶσι, κ. τ. λ."

PRODICUS, and GORGIAS, and the other members of this notable brotherhood, whatever weak points their philosophy might offer to a sharp logician, were men of the world, and not likely to commence their teaching by plucking the beard of their audience, whatever that might be. Neither is there the slightest reason to suppose that all of them, or the majority of them, held immoral opinions with the same grand consistency with which their spokesman proclaims them in the Gorgias. The received doctrine with regard to the sophistical ethics which the learned historian undertakes to refute, is simply this, that by referring our ideas of right altogether to institution and convention, and in nowise to nature and divine necessity, they sapped the foundations of all morality, and made a justification of every iniquity easy to those who chose to argue consistently on their principles. And that there were plenty of men in Athens only too ready to carry such a doctrine to its legitimate practical conclusion, the unprincipled character of many public men in Athens, from ALCIBIADES to ÆSCHINES, sufficiently testifies. The character of the Athenian δῆμος may be placed as high as Mr GROTE, according to a democratic ideal, finds himself warranted to plant it; but it was not the δῆμος properly so-called, that is, the middle and lower strata of the Athenian people, by whom the principles of the slippery sophistical ethics were principally imbibed. It was the sons of the rich men, the oligarchy, the δύνατοι, that had most leisure and most ability to frequent the lectures of such men as PROTAGORAS, and to pay their fees; and how grandly they profited by their instructions, the oligarchic conspiracy of the four hundred in the year 411 B.C., and the government of the thirty tyrants, told to all the world with a signature of blood, whose significance Mr GROTE would be the very last man to misinterpret.

(5.) Mr GROTE'S fifth argument, that the Sophists were not a sect or body of men like the Stoics and the Platonists, holding any particular set of opinions, but only a profession, like our modern literary men, critics, and reviewers, may be disposed of in a single sentence. Nobody ever said that they were a sect, but a class of men following a particular profession, and who were distinguished generally by a certain common character and principles. Of this the French Encyclopædists, to whom the Sophists have been aptly compared, were a notable example.

(6.) The matter of the μισθός, or fee which the Sophists charged for their instructions, must not be looked at from a merely modern point of view. The Sophists were not, like our professors, public servants engaged to give a certain special training to young men, either on receipt of a salary from the public, or of single fees from individual students. They came forward voluntarily with broad general professions, to fit men for public life, by teaching both the art of public speaking and all that effective speaking implies. They professed to teach the wisdom of life, the art of getting on, and especially the art of governing men in popular assemblies. This, it is evident, is a very serious matter, and very different from the attitude that belongs to any modern teacher. What they professed to do could not be done scientifically without discussing the principles of right and wrong, and teaching virtue, ἀρετὴ in fact as well as ῥητορική. This is the point so ably brought out in the Gorgias. Now, the receiving of a fee for a large profession of this kind is a very different thing from paying a price for a pair of boots to a shoemaker, or for so many lessons in grammar to a language master. The question might be raised on the very threshold—*Can virtue be taught?* the famous question, εἰ διδακτὸν ἡ ἀρετὴ, discussed in the Menon and the Protagoras; and the strongest arguments were at hand to prove that, if it was teachable at all, it certainly was not to be taught in the same way that dancing may be learned from a dancing master, or music from a music master. A man goes to a teacher of Sanscrit, for instance, gets so many hours' grammatical exposition, appropriates the cram, passes his examination, gets an Indian appointment, and reposes comfortably upon more than the value of his fee. Here there is a definite *quid* for a definite *quo*, in the most distinct and mercantile sense. But the moral teacher must go to work in a different fashion. He does not offer a marketable article, and therefore cannot expect or demand a market price. For a mere course of lectures on the virtues, with which the scholar is to be duly crammed, will not do the business; it may prove worse than useless. A moral teacher must commence with teaching the student to see his faults, to confess his errors, and to amend his way. No man comes forward with a guinea in his hand to get instruction of this kind. No man expects to be paid for giving good but disagreeable advice to a conceited coxcomb, or a pompous pretender. And, accordingly, in our Christian churches clergymen are paid, not the value of their sermons, but, like the Platonic φύλακες, they receive a general salary for their maintenance. A sermon has no market value. No man paid the Hebrew prophets for their patriotic denunciations. The Athenians paid SOCRATES for his life-long speaking of all truth, and exposing of all sham, with a dungeon and a cup of hemlock. I therefore think that SOCRATES was right in refusing to receive a fee for teaching virtue. Besides, there is an element of convention in this matter which must not be overlooked. No public man in this country is paid, or would receive payment, for serving his country as a member of Parliament; and if PROTAGORAS, or any other accomplished speaker, came forward in Athens professing to teach virtue for a fee, the public conscience was entitled to be offended by the novelty, and to make a strict cross-examination of the individual who made such pretentious professions. One thing is certain, that not only in Athens, but in modern England and everywhere, the public teacher who demands no fee for his services, and can be suspected of not the slightest admixture of mercenary motives, must always stand upon a moral vantage ground that the paid teacher cannot occupy. This is the secret, or part of the secret at least, of the great influence exercised by WHITFIELD and other zealous evangelists in the last century, who, flinging away the golden hopes of ecclesiastical preferment, devoted themselves to field preaching and missionary work among the most abandoned classes, by whom an entirely moral service could be repaid only by a moral reward.

This paper may be most fitly concluded by an articulate statement of the heads of the sophistical doctrine, as I abstract them from the works of Plato, supported by the general testimony of the ancients :—

I. General information and alert intelligence without a philosophical basis, or a scientific method of verification.

II. The art of public speaking, considered merely as a means of moving masses of ignorant men with a view to political advancement, but not necessarily connected either with pure motives, lofty purpose, or business habits.

III. The exercise of a dexterous logic, that aimed at the ingenious, the striking, and the plausible, rather than the true, the solid, and the judicious.

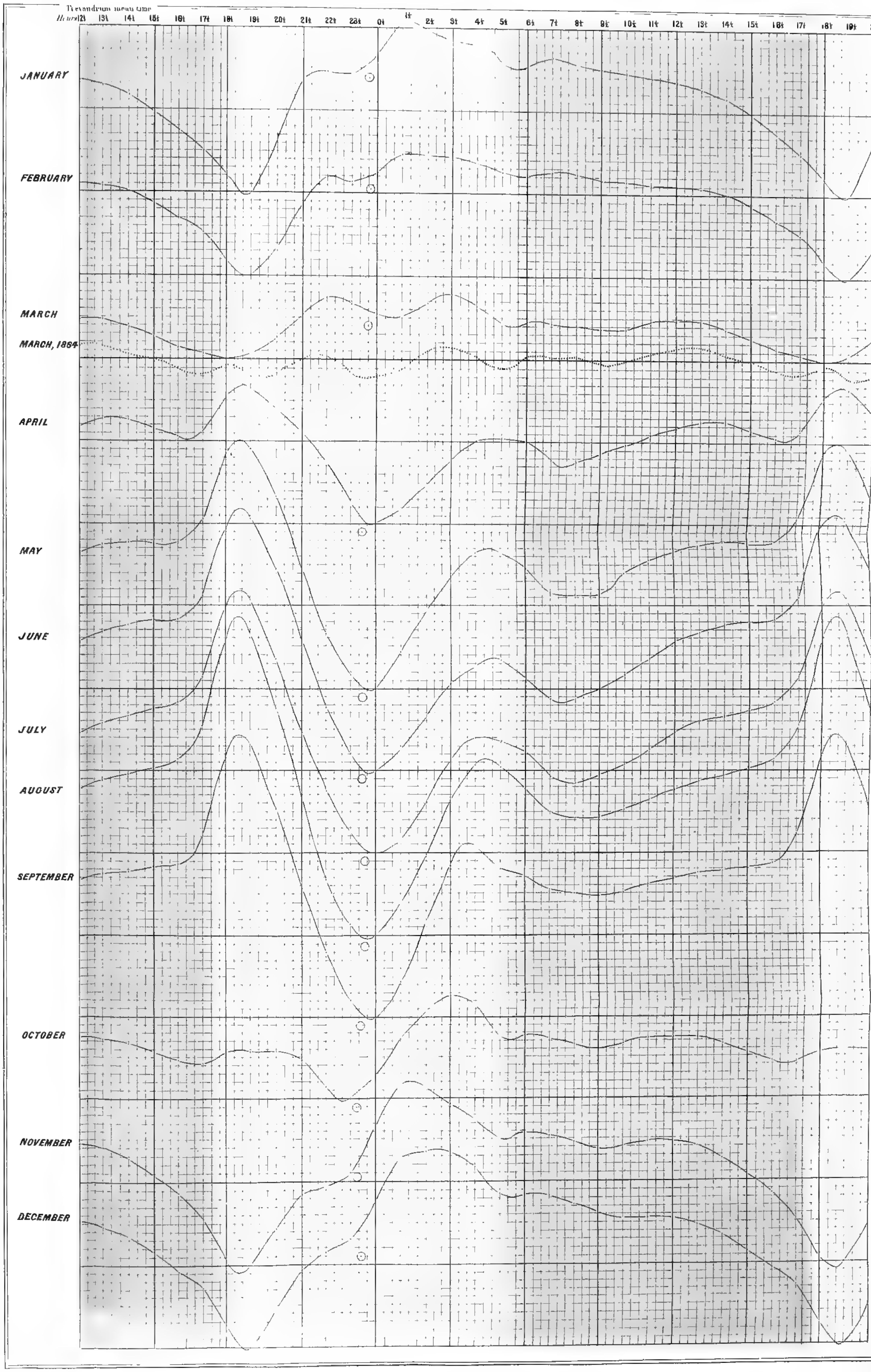
IV. A theory of metaphysics which, by confounding knowledge with sensation, and subordinating the general to the particular, made wisdom consist rather in the expert use of present opportunity, than in the moulding of materials according to an intellectual principle.

V. A theory of morals which, by basing right on convention, not on nature, deprived our sensuous and animal passions of the imperial control of reason, and substituted for the eternal instinct of justice in the human heart the arbitrary enactments of positive law, whose ultimate sanction is the intelligent selfishness of the individual.

PLATE XL.

MONTHLY MEAN CURVES OF THE DIURNAL VARIATION OF MAGNETIC DECLINATION AT TREVANDRUM, DEDUCED FROM 12 YEARS' OBSERVATIONS, (1853-1864.)

By J. A. Broun, F.R.S.



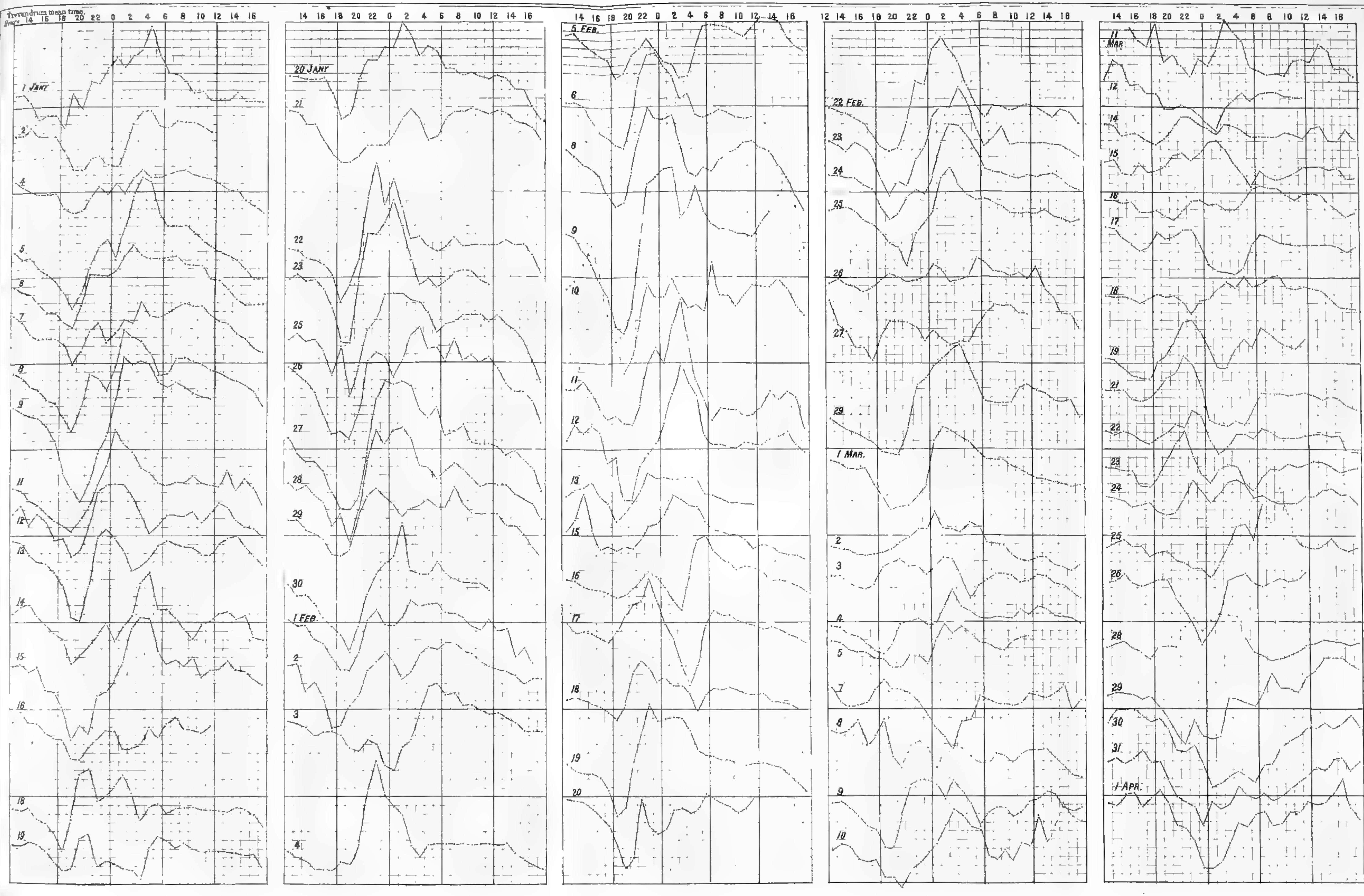
Ascending Curves indicate the movement of the North end of the Needle towards the East. ⊙ *True Noon.*

SCALE. *One inch = One minute of arc.*

The part shaded shows when the Sun is below the Horizon.

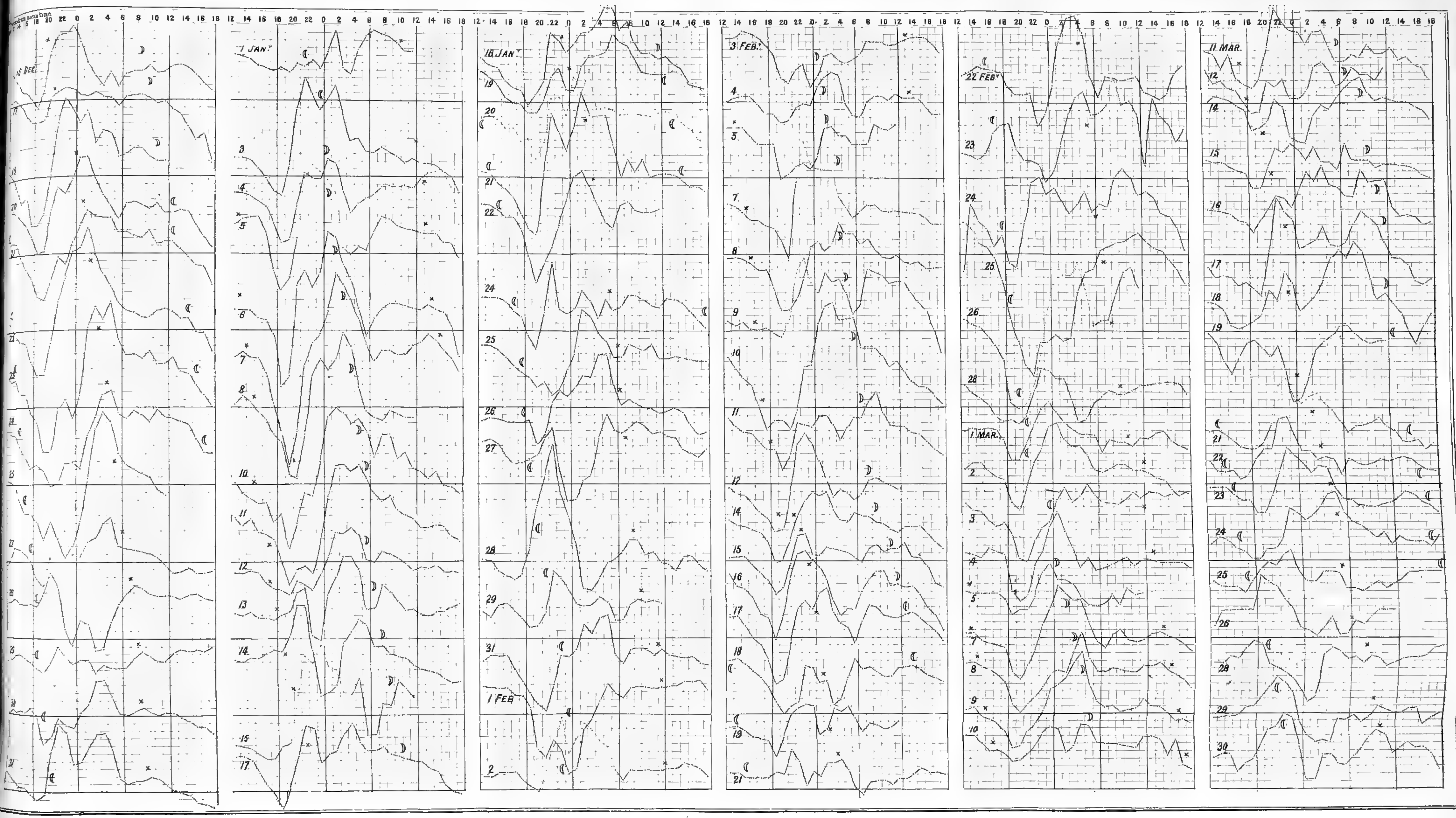
DAILY CURVES OF MAGNETIC DECLINATION, AT TREVANDRUM, IN JANUARY, FEBRUARY, & MARCH, 1864. *BY J. A. BROUN, F.R.S.*

PLATE XLI.



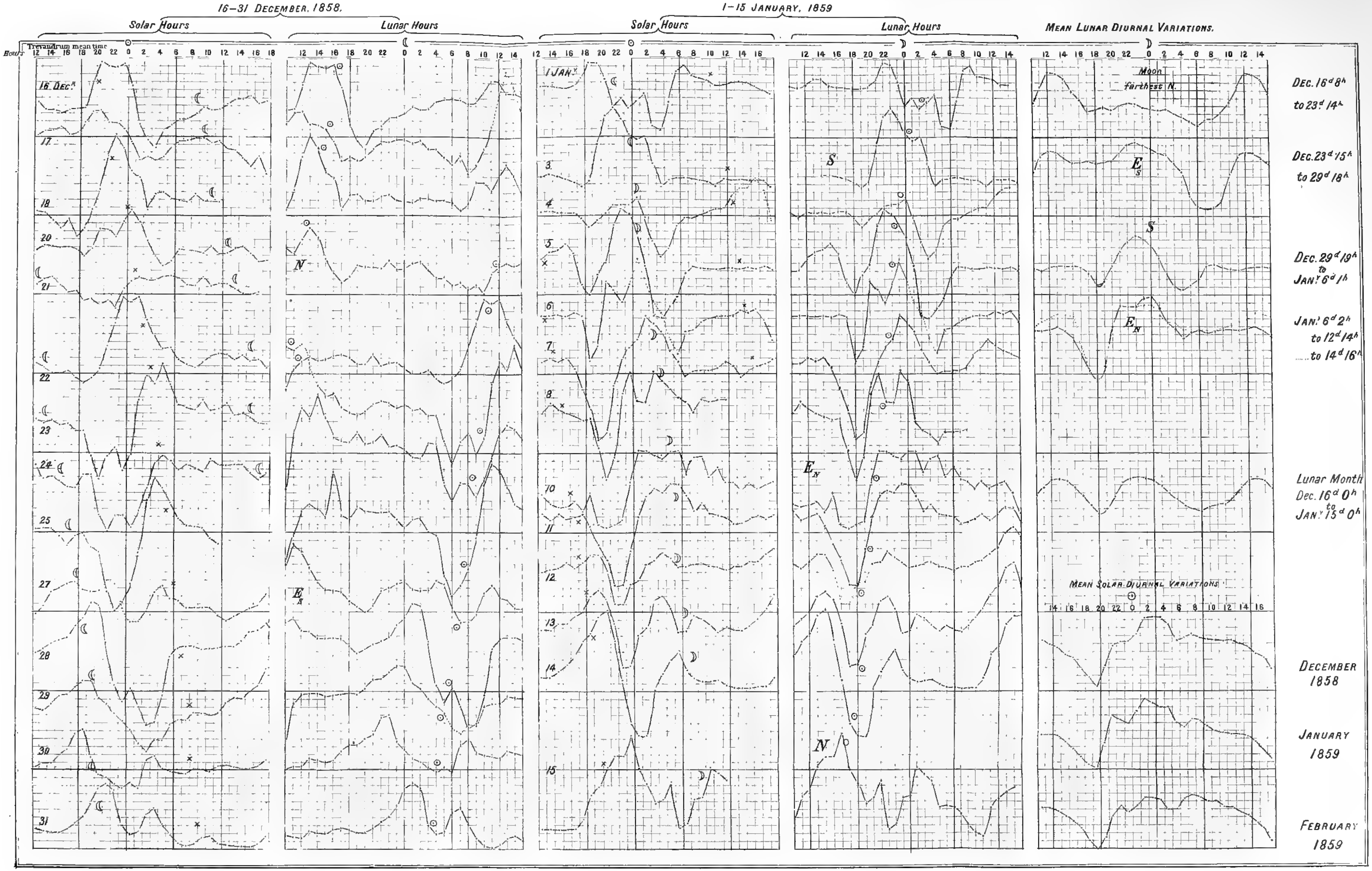
One division = 0'.25. One inch = 2'.5. Ascending Curves indicate movement of the North end of the needle towards the East.

DAILY CURVES OF MAGNETIC DECLINATION AT TREVANDRUM, FROM 18TH DECEMBER, 1858, TILL 30TH MARCH, 1859. BY J. A. BROUN, F.R.S.



( ) Moon on the superior meridian. × Moon on the inferior meridian. One division = 0′.25; One inch = 2′.5. Ascending Curves indicate movement of the North end of the needle towards the East.

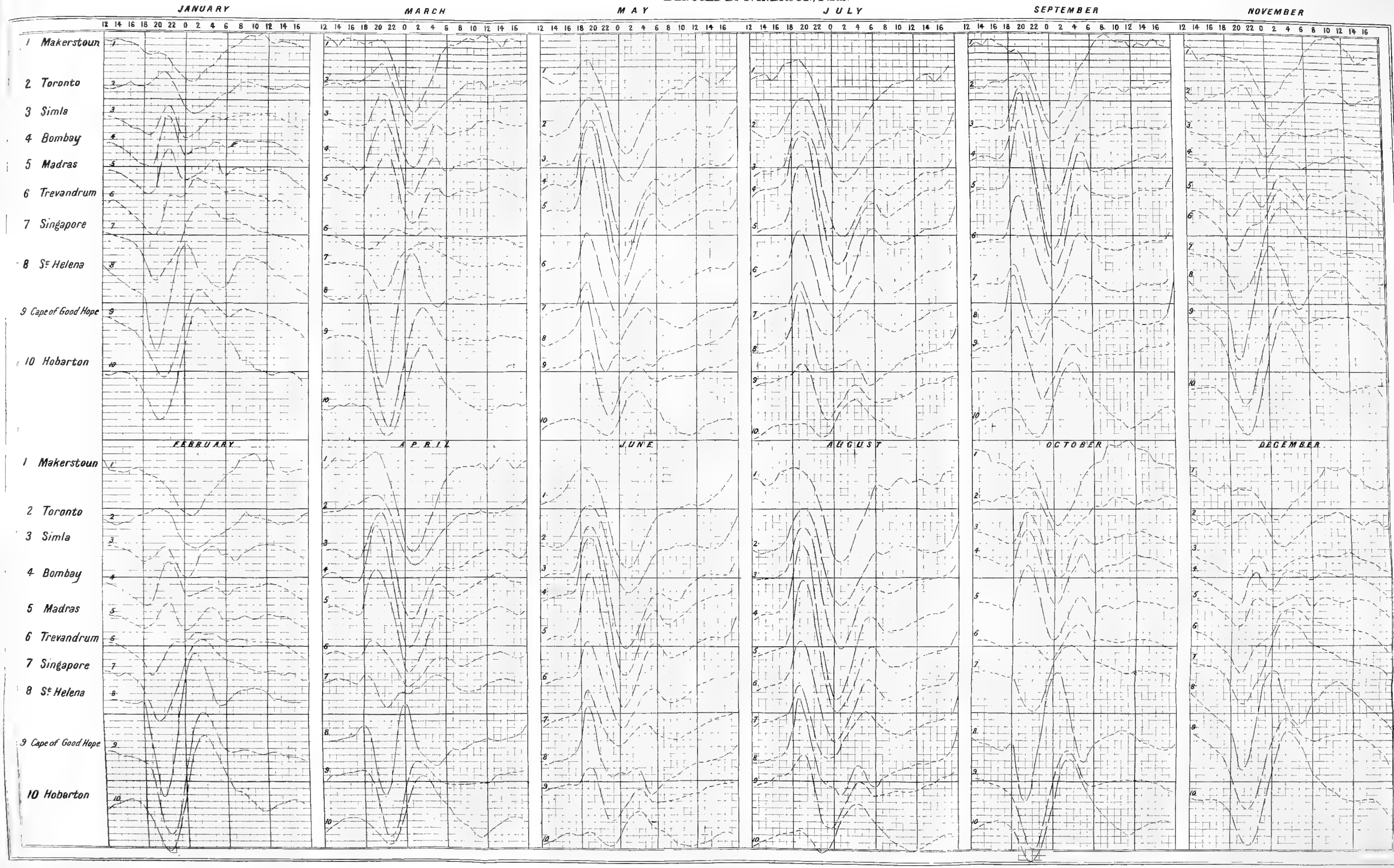
CURVES OF DIFFERENCES OF DAILY MOVEMENTS FROM THE MEAN SOLAR DIURNAL VARIATION, AND RESULTING LUNAR DIURNAL VARIATIONS, AT TREVANDRUM, DEDUCED BY J. A. BROUN, F.R.S. PLATE XLIII.



☾☽ Moon on the superior meridian. × Moon on the inferior meridian. ⊙ Sun on the meridian. One division = 0′.25. One inch = 2′.5. Ascending curves indicate movement of the North End of the needle towards the East.

CURVES OF THE DIURNAL VARIATIONS OF MAGNETIC DECLINATION, AS SHOWN BY A NEEDLE FREELY SUSPENDED IN THE DIRECTION OF THE MAGNETIC INCLINATION, AT TEN STATIONS BETWIXT LATITUDES 56 N. & 43 S., FOR EACH MONTH OF THE YEAR.

DEDUCED BY J. A. BROUN, F.R.S.



Ascending Curves denote increasing Easterly Declination. One Division = 0'.25; One Inch = One minute.

XLV.—*On the Diurnal Variation of the Magnetic Declination at Trevandrum, near the Magnetic Equator, and in both Hemispheres.* By John Allan Broun, Esq., F.R.S., late Director of the Observatory of His Highness the Maharajah of Travancore, G.C.S.I., at Trevandrum. (Plates XL. to XLIV.)

(Read 29th April 1867).

Preceding Observations and Conclusions.

The first observations of the diurnal variation of magnetic declination, made near the equator, seem to have been those of Mr Macdonald, who observed in 1794–95 at Fort Marlborough, Sumatra, 3° 46′ S., and at St Helena. Two conclusions seem to have been deduced from these observations—1*st*, That near the equator the range of the diurnal variation was much smaller than in Europe; 2*d*, That the needle moved in opposite directions south of the equator and in Europe.* This latter conclusion was made use of by M. Arago, in his report made in 1821, on the "Voyage de l'Uranie," as the base of a hypothesis that there must be a line betwixt the two hemispheres on which the magnetic needle moves neither east nor west—that is, remains stationary. M. de Freycinet's observations showed that this line was not the terrestrial equator, and M. Arago supposed it must be the magnetic equator.†

In 1825 M. Arago again alluded to his hypothesis, in his report on M. Duperrey's observations. These seemed to show—1*st*, That the diurnal variation was not extinguished on either equator; 2*d*, That in south latitudes, but with north magnetic inclination, the needle moved as in Europe. M. Arago then suggested, "Peutêtre les changements de declinaison du soleil, qui en Europe occasionnent de si grandes variations dans l'amplitude des oscillations diurnes, amenent ils suivant les saisons sous les tropiques des mouvements de l'aiguille dirigés en sens inverse."‡

M. Arago recurred to the subject in 1835, in his "Instructions pour la Bonite," and he suggests some additional line, some curve of equal magnetic intensity, as the curve of no diurnal variation.§

* Phil. Trans. abridged, vol. xviii. pp. 29 and 355. The first of the conclusions is given by Mr Macdonald. He appears to have found, that the north end of the needle moved east from 7 A.M. till 5 P.M. The observations having been made from June 1794 till March 1795, the result from the observations for the first four months (June to September) should have been nearly the inverse of that from the observations for the remaining months. There can be little doubt that the instrument employed was incapable of showing the variations with much accuracy.

† Œuvres de F. Arago. Instructions, Rapports, etc., p. 152. Voyage de l'Uranie.

‡ *Ibid.* p. 196. Voyage de la Coquille.

§ *Ibid.* pp. 25, 26.

In his examination of observations which seem to have been made with much zeal and care, by M. DE TESSAN, in 1837–38, in different latitudes, at different periods of the year, M. ARAGO says, with reference to the question, whether the hours of maxima and minima of declination are identical over all the earth: "Nous pouvons affirmer qu'il n'en est pas ainsi l'aiguille horizontale atteint les limites de ses excursions diurnes, à des heures différentes suivant les climats."* With this affirmation M. ARAGO destroyed the base of his hypothesis. M. DE TESSAN himself pointed out, "Que la transition de l'un de ces états (the movement in one hemisphere), à l'état opposé (the movement in the other hemisphere), à mesure qu'on passe d'un hémisphère à l'autre, peut se faire aussi par un déplacement graduel des heures critiques auxquelles la direction du mouvement change. Et la realité de ce dernier mode de transition qui *a priori*, est aussi possible que l'autre me paraît indiquée avec beaucoup de vraisemblance par la succession régulière des courbes de notre tableau graphique."†

Yet we find in August 1840, after a study of all the observations made in the different voyages terminating with that of the "Venus," that M. ARAGO thought we should still seek the line of no diurnal variation, "Une courbe le long de la quelle l'aiguille par exception conservera de jour et de nuit absolument la même direction."—"Une courbe qui deviendra aussi l'objet de bien des recherches de bien des voyages."‡ It will be seen in his posthumous work on "Terrestrial Magnetism," that he preserved this idea to the last.

A consideration of the observations examined by M. ARAGO, of the shortness of their duration at each station, and of the difficulties of obtaining accurate observations with an imperfect installation of instruments (signalised by M. DE TESSAN), will explain his varying conclusions, and show that the materials in his possession were insufficient to answer the questions as to the mode in which the diurnal variation of the magnetic needle changed during the year at any station within the tropics, or from one latitude to another, at the same period.

The first series of observations of sufficiently long duration within or near the tropics, and capable of showing what laws the magnetic needle really followed there, were made in the Observatories of Bombay, Madras, Trevandrum, Singapore, and St Helena. The first, second, and fourth of these observatories were established at the expense of the East India Company, the last by the British Government, and the third by His Highness the Rajah of Travancore.

The observations made at Trevandrum, in June to December 1841, under the direction of the late Mr CALDECOTT, F.R.S., were in the possession of the Royal Society of London early in 1842, with the projected diurnal curves of magnetic

* Instructions, Rapports, etc., p. 283. Voyage de la Venus. Report, dated 1840.

† Voyage autour du monde sur la Fregate la Venus. Physique, vol. v. par M. DE TESSAN, 1844, p. 417. See also p. 461.

‡ Œuvres, Rapports. etc., p. 288.

declination for each month. Mr CALDECOTT communicated to that Society the fact shown in his curves, that the law of variation was, to a great extent, inverted in the course of October 1841. Although the fact that the law of diurnal variation for December was nearly the inverse of that for June within the tropics, must have been perceived early by the Directors of other intertropical observatories, yet Mr CALDECOTT was, as far as I am aware, the first who brought the fact with its proof to the notice of a scientific body.*

As will appear hereafter, none of the intertropical observatories, excepting that of Trevandrum, was sufficiently near the magnetic equator to approximate to a solution of the question proposed by M. ARAGO; and Cape Comorin, about 40 miles S.S.E. of Trevandrum, was one of the stations pointed out by this philosopher for that end.

Position of the Trevandrum Observatory.

The observatory at Trevandrum is in latitude 8° 30′ 32″ N. (about $2°\frac{1}{2}$ S. magnetic inclination, and 26′ E. magnetic declination in 1854), and $5^h\ 7^m\ 59^s$, east of Greenwich, on a height nearly 200 feet above the sea. The following results are deduced from observations made under my direction, during the twelve years 1853 to 1864.

Instruments used.

Two declinometers were employed after 1853; one by GRUBB of Dublin, similar to that described in the introductions to the Makerstoun Observations—one by ADIE of London, made according to my own plans. With the former, all the precautions were taken indicated for the Makerstoun declinometer; and the telescope could be compared with the south transit-mark of the observatory (4 miles distant). The second was under a glass-receiver, from which the air was pumped; the suspension apparatus was supported by a tripod of glass rods, so that very little metal was used in the construction of the instrument. A room was built *within* the observatory for this declinometer, having a solid planked ceiling covered like a terrace with plaster, several feet below the roof. The declinometer was read from without the closed room, the telescope pillar forming part of a

* Mr CALDECOTT gave the diurnal curve for the first half, and for the second half of October. In Notes to the Observations, dated 10th November 1841, which were forwarded to the Royal Society of London, in the same month, he deduced,—

From the 1st half of October, max. of E. declin. 7^h A.M.; min. $0^h\ 28^m$ P.M.
,, 2d ,, min. ,, $8\frac{1}{2}$ A.M.; max. $4^h\ 28^m$ P.M.

He attributed the change of law at the time to the monsoon, which broke out during the month; and he caused the observations to be continued every ten minutes, in the month of November, in order to determine whether the change would continue. Mr CALDECOTT's remark was never published by the Royal Society of London, but it will be found among the MS. of Trevandrum Observations, in the archives of the Society, where I have myself seen it. It is necessary to add, that Mr CALDECOTT's observations, though affected by many errors (chiefly due to the imperfect construction of the magnetometer boxes), were sufficiently exact for the determination of this change of law.

well-founded wall. The daily variation of temperature within the closed room was never more than a few tenths of a degree Fahrenheit. The value of a scale division of GRUBB'S declinometer was 15″·35, and of ADIE'S, 16″ nearly; tenths were easily estimated, and as the vibrations in ADIE'S instrument were generally very small, if any, the mean of three readings was supposed accurate to one second of arc. Both declinometers were observed hourly, and one was observed twice at each hour, one minute before, and one minute after the other.*

Diurnal Variations near the Magnetic Equator.

TABLE I.—*Hourly Variations of Magnetic Declination at Trevandrum, deduced from Observations during the twelve years,* 1853 *to* 1864.

Hour, Trevandrum Mean Time.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sep.	Oct.	Nov.	Dec.
H. M	′	′	′	′	′	′	′	′	′	′	′	′
12 28	1·34	1·10	0·47	1·18	1·64	1·54	1·42	1·77	1·66	0·73	1·44	1·54
13 28	1·27	1·07	0·47	1·27	1·74	1·67	1·58	1·89	1·74	0·72	1·40	1·47
14 28	1·15	1·00	0·38	1·26	1·78	1·77	1·66	1·96	1·77	0·67	1·27	1·34
15 28	0·97	0·86	0·26	1·15	1·77	1·80	1·74	2·02	1·81	0·57	1·08	1·15
16 28	0·73	0·69	0·13	1·04	1·78	1·83	1·82	2·14	1·85	0·45	0·85	0·92
17 28	0·50	0·51	0·07	1·13	2·07	2·10	2·13	2·54	2·20	0·41	0·55	0·70
18 28	0·20	0·19	0·00	1·56	2·85	2·95	2·97	3·54	3·19	0·57	0·08	0·26
19 28	0·00	0·00	0·06	1·67	2·90	3·06	3·06	3·64	3·31	0·59	0·00	0·00
20 28	0·64	0·34	0·27	1·40	2·33	2·44	2·35	2·73	2·51	0·57	0·43	0·47
21 28	1·37	0·86	0·53	1·11	1·45	1·56	1·46	1·62	1·57	0·43	0·82	0·93
22 28	1·46	1·19	0·73	0·77	0·65	0·78	0·73	0·67	0·79	0·12	0·95	1·15
23 28	1·41	1·11	0·67	0·26	0·16	0·20	0·23	0·12	0·21	0·00	1·10	1·29
0 28	1·69	1·25	0·53	0·00	0·00	0·00	0·00	0·00	0·00	0·28	1·66	1·78
1 28	2·06	1·48	0·51	0·18	0·42	0·28	0·18	0·40	0·44	0·72	2·14	2·25
2 28	1·98	1·47	0·69	0·47	0·94	0·67	0·61	0·98	1·20	1·07	2·12	2·33
3 28	1·86	1·42	0·79	0·78	1·35	1·05	1·11	1·63	1·85	1·27	1·93	2·32
4 28	1·80	1·37	0·63	1·01	1·64	1·30	1·40	2·08	2·09	1·12	1·75	2·18
5 28	1·57	1·24	0·42	1·02	1·65	1·33	1·35	2·05	1·80	0·73	1·49	1·84
6 28	1·54	1·19	0·44	1·01	1·45	1·15	1·21	1·79	1·70	0·74	1·57	1·82
7 28	1·65	1·26	0·43	0·77	1·15	0·89	0·92	1·50	1·54	0·71	1·54	1·81
8 28	1·57	1·23	0·40	0·76	1·11	0·88	0·84	1·43	1·50	0·67	1·45	1·71
9 28	1·49	1·15	0·37	0·85	1·24	1·00	0·95	1·45	1·47	0·60	1·38	1·61
10 28	1·42	1·15	0·36	0·98	1·38	1·17	1·09	1·52	1·50	0·65	1·39	1·54
11 28	1·39	1·11	0·46	1·09	1·54	1·37	1·26	1·64	1·60	0·72	1·46	1·55

The quantities in this table will be found projected, Plate XL., a consideration of which has led to the following conclusions:—

1*st*, The diurnal variation consists of one marked maximum and one marked minimum of *easterly* declination in each month of the year, and of one or more secondary maxima and minima.

* The details connected with the description of instruments and the precautions taken to prevent errors, will be found in the first volume of the Trevandrum Observations, which I hope will soon be published.

2d, The principal maximum occurs in the six months of April to September at about 7 A.M., and the principal minimum occurs about twenty minutes past noon in the same months.

3d, Nearly the inverse of this happens in the four months of November to February, inasmuch as the principal minimum occurs between $7^h\ 10^m$ and $7^h\ 20^m$ A.M., and the principal maximum occurs betwixt $1^h\ 45^m$ and 3^h P.M.

4th, When we examine the mode in which this inversal takes place in the mean curves, it appears to happen in two ways:—*1st,* In a direct transition during the month of March from a minimum to a maximum at 7 A.M.; and during the month of October from a maximum to a minimum at the same hour; in which transitions the mean movement must necessarily at one time become zero about the critical hour. *2d,* In an apparent *shift* of the time of the afternoon maximum.

5th, The principal minimum, it has been noticed (*2d*), occurs in the months of April to September within a few minutes of twenty minutes past *true* noon; this minimum never wholly disappears in the mean curves at Trevandrum. Owing to the change going on in the law of variation in the months of March and October, it happens about 30^m before noon in the latter month, when it is still the principal minimum, and about one hour after noon in the former, when it is a secondary minimum; it appears as a slight minimum, or as inflexion only in the curves, for the four months of November to February. This conservation of the noon minimum would seem to prove that there is no real inversion at this hour, and that the minimum near noon and the maximum after noon are two distinct and independent phenomena, the former becoming more and more marked from January to July.

6th, The conservation of a minimum near noon in the six months of October to March, has, as a consequence, the appearance of a secondary maximum, or an inflexion of this character betwixt 7 A.M. and noon—generally near 10 A.M.

7th, The principal maximum occurs betwixt $1^h\ 45^m$ and 3^h P.M. in the four months November to February; in March and October it happens near $3^h\ 30^m$ P.M.; and in the remaining six months of April to September, from $4^h\ 20^m$ to $5^h\ 30^m$ P.M.

8th, A quite secondary minimum occurs near 6 P.M. (or almost exactly at sunset) in the six months of October to March; and a more marked minimum appears in the curves for the six months from April to September between 8^h and $9^h\ 30^m$ P.M. A faint minimum can be traced in the curves for the remaining six months, October to March, at nearly the same hour; so that the minimum at sunset is a phenomenon independent of that near 9 P.M.

9th, The north end of the needle moves between 8 P.M. and 7 A.M. towards the east in the five months May to September; it moves towards the west between the same hours in the four months November to February; whereas in the months of March, April, and October, the change from one direction to another produces a secondary minimum near 5 A.M., and this minimum has its representative in an inflexion near the same hour in the curves for May to November.

10*th*, It might be suggested that the minimum or inflexion near 5 A.M. in the six months of April to October is due to the same cause as the minimum in the remaining six months at 7 A.M., and that there is here also a shift of the critical hour.* In any case there cannot be a doubt of the absolute inversal of the mean movements at 7 A.M. during the months of March and October, a phenomenon which has been observed at no other station.

These conclusions as to maxima and minima are deduced from curves which represent the *mean* movement corresponding to the *middle* of each month. I have had to point out, on different occasions, that deductions from mean values may be wholly inaccurate, since it sometimes happens that the observations represent laws which are very different, and the mean may represent none of them. It will be desirable, then, in this instance, to endeavour to determine in what way the law of variation really changes from day to day, especially in the months when the change is marked. I shall, however, consider previously the range of movement of the declination needle.

TABLE II.—*Ranges of the Monthly Mean Movements in Table I., and Monthly Means of all the Daily Ranges during the twelve years*, 1853 *to* 1864.

Months.	Range of Mean A	Mean of Ranges B	Ratio $\frac{B}{A}$	$\frac{B}{1\cdot11}$
January,	2′·06	3′·09	1·50	2·78
February,	1·48	2·54	1·72	2·29
March,	0·79	2·07	2·62	1·86
April,	1·67	2·35	1·41	2·12
May,	2·90	3·23	1·11	2·90
June,	3·06	3·40	1·11	3·06
July,	3·06	3·45	1·13	3·06
August,	3·64	4·02	1·10	3·64
September,	3·31	3·68	1·11	3·31
October,	1·27	2·25	1·79	2·03
November,	2·14	2·70	1·26	2·43
December,	2·33	3·04	1·30	2·74

Ranges of the Mean Movements of the Declination Needle near the Magnetic Equator.

It is evident from the quantities A and from the curves, Plate XL., that there is a tendency to an extinction of the *mean* movement in March and October, and especially so in the former month. The range of the *mean* movement is a minimum in these two months, and it is a maximum in August and December.

The range is greater in August than in December. This fact, and the exist-

* This point will be examined afterwards when the lunar effect has been eliminated.

ence of an inflexion or slight minimum near noon in the months about December, seem to show that Trevandrum belongs more properly to the northern hemisphere (to which it belongs geographically), than to the southern hemisphere, to which it belongs magnetically.

In comparing the curve of February with that of April, and that of September with the curve of November, it would seem probable, that at some other epoch than the middle of the months of March and October, the amount of the range would be still less than that here shown. A consideration of monthly mean curves, corresponding to each week of the year, shows that this is the case; but as the date of minimum movement is not the same in each year, a combination of the means at the same date in different years will not show the smallest amount of the movement. I have, however, projected (Plate XL.) the mean curve corresponding to the middle of March 1864, which shows the mean movement limited to about one-third of a minute of arc (the range of the hourly observations is $0'{\cdot}35$).

We have thus a very near approximation to an extinction of movement in the mean curve for the month of March 1864; and if the mean curve represented the usual daily movement, we might say that we have here a case in which the needle remains stationary night and day. This is, however, not the fact; the result is to a considerable extent arithmetical, and is due, in part at least, to a combination of different movements.

In the third column (B) of Table II. I have given the means of *all* the *daily* ranges for each month. It will be perceived from these, that though the diurnal oscillation is still a minimum in March and October, yet it is a much more marked oscillation than appears in the mean curves. In the year 1856, for which the mean of the daily ranges is least, the mean of those for March is 1·51 and for October 1·76; these, though very small mean ranges, are still more marked than we might expect from the mean curves.

The fourth column of Table II. contains the ratio of the mean of the daily ranges (B) to the range of the mean variation (A); this ratio is greatest in March and October. In the five months of May to September it is nearly constant, or 1·11; while, in the four months of November to February, it increases from 1·26 to 1·72. From these ratios we may conclude that the diurnal variation obeys nearly a constant law in the months of May to September; while, in the other months, either considerable differences occur in the times of maxima and minima, or the law changes sign, as in March and October.

Column five contains the *hypothetical* ranges of mean curves, when we suppose the agreement of the range of the mean curve with the mean of the daily ranges to be the same in all the months, as in May to September, or that $\frac{B}{A} = 1{\cdot}11$. In this case the ranges for December and January approach in magnitude those for June and July.

Variations for each Day in the Month of March.

We are, as yet, still ignorant of the manner in which the law changes from one form to another in March and October. In order to study this question, I have projected the hourly observations during several months. I shall give here the projections of those only for January, February, and March 1859, a year of considerable disturbance, and for the same months in 1864, a year of slight disturbance. Plates XLII. and XLI.

An examination of these curves will show that the law of diurnal variation does *not* pass regularly from one form to another in the month of March, either by a sliding transfer of maximum to minimum, or by a diminution of movement till the inversal takes place. On the contrary, there appears an utter absence of continuity in any way: the curves are sometimes inverted from one day to the next; sometimes follow in part the law for January, in part the law for June; sometimes resemble wholly the one or the other; frequently resemble neither in any way: and this change appears to occur without order.

The daily curves in March may be distributed into four categories. (I shall include also the conclusions from the months of March 1854 and 1856, the curves for which months I have projected, but which it does not appear to me necessary to give here.)

1*st*, Those curves, which resemble in some degree that for January, having a minimum near 7 A.M., and a maximum between noon and 4 P.M.; to this class the curves for the following days belong:—

1854, March 6, 7, 8 ? 18, 22.
1856, „ 3, 4, 5, 6, 7, 19, 20, 21, 24, 29.
1859, „ 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 16.
1864, „ 1, 2, 4, 5, 10, 15.

2*d*, Those resembling the curve for June, in having the maximum about 7 A.M., and a minimum near noon. To this class may be given the curves for

1854, March 14, 16 ? 23, 24, 25, 27.
1856, „ 11, 13, 15 ? 18, 31.
1859, „ 19, 26, 28, 29, 31.
1864, „ 7, 8 ? 11, 12 ? 17 ? 18 ? 25, 26, 29, 30, 31.

3*d*, Those which approach the second class, but have the maximum near 10 or 11 A.M., and minima near 6 A.M., and 1 to 4 P.M., namely,—

1854, March 9, 10, 11, 12, 13, 21.
1859, „ 22, 25, 26, 27 (also April 1, 2, 3, 4, 5, 7, 8).
1859, „ 11 ? 12 ? 13, 14, 17, 18, 25 ? 30, 31 ?
1864, „ 17 ? 19, 21, 23, 24 ?

4*th*. Those which resemble none of these, and happen on the remaining days of each month (not including Sundays, when there were no observations).

The curves resembling that for January occur chiefly towards the beginning of the month, and those resembling the curve for June in the latter part of the month. In some years (as in 1856) the curve preserves a maximum about 10 A.M., before passing to the maximum at 7 A.M. There is no evidence of a gradual shift or inversal (excepting when the mean movement deduced from several days' observations is considered). The law is sometimes partially or wholly inverted in one day—as in March 22, 23, 1854: March 11, 12; 13, 14; 29, 31; 1856: March 19, 21; 1859: and March 5, 7; 8, 9; 18, 19; 24, 25, 26; 28, 29; 1864.

The curves are so irregular and variable in their forms as to prove that *the electric currents producing these motions are in a state of disturbance, and flow in various different directions, from day to day, in the month of March.*

It appeared to me of importance to examine to what extent the form of the mean curves, in the months from November to February, represented the usual diurnal law, and whether the inflexion or slight minimum apparent in these curves near noon was observable in the ordinary daily movement, or was an arithmetical result like the mean curve for March, and due to a combination of different forms. For this end I projected the curves for each day in January and February 1859 and 1864 (see Plates XLII. and XLI.)

Variations for each Day in January and February 1859 *and* 1864.

Commencing with 1864, a year of small disturbance (see Plate XLI.), the minimum of easterly declination occurs generally in the months of January and February at $6^h\ 30^m$ or $7^h\ 30^m$ A.M.; rarely as early as $5^h\ 30^m$ or as late as $8^h\ 30^m$. This regularity of the hour of minimum does not hold for the hour of maximum, which varies betwixt $10^h\ 30^m$ A.M. and $5^h\ 30^m$ P.M. On eight days the maximum occurred near 10 A.M.; on nine days, near noon; on fourteen days, near 2 P.M.; on eight days, near 4 P.M; and on nine days, after 5 P.M. In the mean curve for the month of January 1864, the inflexion near noon is scarcely visible; the maximum is near 2 P.M. In the mean curve for February of the same year, the slight minimum before noon is better marked.

An examination of the several daily curves shows that one-half at least have a minimum or marked inflexion near noon; and such inflexion or minimum does not happen so frequently at any other hour, not an hour of minimum. Although, then, the *mean* curve after the 7 A.M. minimum is a result of different variations, the inflexion or slight minimum observable in the mean curve near noon is probably the representative of a real and persistent phenomenon; and this will appear more evident hereafter.

If we now examine the curves for the same months in 1859, a year of considerable disturbance (see Plate XLII.), and in the mean curves for which there is a secondary minimum at $11^h\ 30^m$ A.M. (see 5th division of Plate XLIII.), we shall perceive much greater differences in the daily movements. In January 1859 the minimum occurs in the first fortnight betwixt $6^h\ 30^m$ A.M. and noon, returning again to $6^h 30^m$ A.M. to reappear near noon at the end of the second fortnight. A similar change of hour of minimum is shown in the latter half of December 1858, the curves for which I have also projected (Plate XLII.) The maximum also happens betwixt $9^h\ 30^m$ A.M. and $4^h\ 30^m$ P.M., the curve becoming nearly inverted about the 14th and 28th of January. This inversal also, it will be seen, does not take place suddenly, but gradually, and without the irregularity shown in disturbances in high north latitudes, as at Makerstoun. Similar, though less marked, differences of movement appear in the curves for February 1859.

Lunar Action: sometimes as great as that of the Sun.

This curious variation of the form of the diurnal curve is *due chiefly to the action of the moon.* Hitherto the lunar action has been supposed so small, compared with the solar action, that it has been concluded we might neglect the former in considering the laws of the latter, and that the lunar action could be made visible only in combinations of masses of observations, as a residual quantity nearly of the second order. It will be found from what follows, that the lunar action is *sometimes as great as, if not greater than,* the solar action.

I shall consider more minutely, on another occasion, the subject of the variations of magnetic declination due to the action of the moon, and shall then state the methods employed by me to obtain the results. It will suffice at present to say, that, assuming the daily curves to be due to solar and lunar actions superposed, if we subtract from each daily curve the mean solar curve of the corresponding month, the remainder may be supposed due to the lunar action, and to other causes, irregular or regular (such as a variation of the solar action). As the monthly mean curves are undergoing a gradual change, those corresponding to the middle of *each* week were obtained, and these mean curves were subducted from the daily curves in the respective weeks. The results for the lunar month, December 16, 1858, till January 15, 1859—a month, showing a marked lunar action, have been projected, Plate XLIII.

I have given—1*st*, The daily curves (see Plate XLII.); 2*d*, The differences obtained as above, under their respective solar hours; and, 3*d*, The differences under their respective lunar hours (see Plate XLIII.)—that is, the moon's nearest hour angle when the observation happened, the latter representing the lunar daily curves, with the addition of irregular or other effects.*

These projections will explain, to a great extent, the curious variations of the epochs in the solar diurnal curves which the curves of differences resemble, excepting that they show, more distinctly, the movement of maximum and minimum

* As the moon takes more nearly 25 solar hours to return to the same meridian, it is assumed that there are 25 lunar hours in 360 degrees.

with the position of the moon. When we examine the differences projected under the lunar hours, the law of lunar action is seen more clearly; and it appears with much regularity, considering that irregular solar and other actions are included. In order to diminish the effect of these irregularities, I have taken the means, and projected—1*st*, The mean of the first six curves, those for the days, December 16^d 8^h,1858, till 23^d 14^h, when the moon had the greatest north declination; 2*d*, The mean of the next five, when the moon was near the equator, going south (Es); 3*d*, Of the next six curves (Dec. 29^d 19^h till January 6^d 1^h), when the moon was farthest south; 4*th*, Of the following five curves, when the moon was near the equator, moving northwards (En); and, 5th, The mean of all the curves in the lunation (see fifth division of Plate XLIII.)

The latter presents two nearly equal maxima and two nearly equal minima; the former near the moon's superior and inferior passages of the meridian, and the latter about six hours before and after. When the moon was farthest north, the maximum near the inferior passage was the greater, the minimum near the moon setting being most marked. With the moon near the equator going south, the maxima were nearly equal, but the moon set minimum was by far the most marked. When the moon was farthest south, the maximum near the superior passage was the greater, and the minima were nearly equal. With the moon near the equator going north, the maximum near the superior passage was the greater, and the minimum near moon-rise by far the most marked.

These results, derived from a single lunation, agree almost exactly with those which I had previously deduced from six years' observations.* The dotted curve for the moon near the equator, moving north, including the observations two days later, or to 14th January, represents better the conclusions previously obtained for this position of the moon, in having nearly equal maxima.

These changes of the law of lunar diurnal variation of magnetic declination understood, it will be perceived that the daily curves (second and fourth divisions of Plate XLIII.) in general follow the law and its changes with considerable fidelity.

The range of the solar diurnal curve for December 1858 and January 1859 is nearly $2'\cdot 2$. The ranges of the mean curves (December 16th 1858, till January 12th 1859) are for the moon:—

Farthest north, ...	$= 1'\cdot 70$
Near the equator, going south, . . .	$= 1'\cdot 95$
Farthest south, ...	$= 1'\cdot 90$
Near the equator, going north, . . .	$= 2'\cdot 70$

I believe that we may conclude, as I have already stated, that the effect of the lunar action is sometimes greater than that of the solar action in the diurnal

* See "Proceedings of the Royal Society of London," vol. x. pp. 482–3.

variation. This is a fact that must be considered in any theory which pretends to account for the variations of the declination magnet.*

It may be supposed that the variable movements in the month of March also are, to some extent, due to the lunar action which shifts its epochs from day to day with the hour for which the moon is on the meridian; and this is no doubt the case; but a first approximation to eliminate the lunar effect from the solar curves has shown me that the irregularity remains after the elimination nearly as before. I shall endeavour hereafter to present the solar curves nearly free from the lunar effect.

I have stated elsewhere, that the lunar law also passes through a period of inversion, near the equator, like the solar law, when the sun passes from one hemisphere to another.† This dependence of the law of lunar action upon the position of the sun complicates the phenomena, and an effort for their separation will be best made when the lunar laws have been investigated.

Diurnal Variation at different Latitudes.

In the preceding pages we have examined the change of law of the diurnal variation of magnetic declination, with the period of the year at a single station near the magnetic equator. It will be desirable now to investigate the mode in

* I have been obliged here to enter upon laws which will be the subject of another paper, in order to explain certain apparent irregularities of solar action. I cannot leave the subject at present without suggesting that the variations of magnetic declination are probably due to currents in an electro-sphere, which, it appears to me, must surround each heavenly body. I have suggested elsewhere that the solar spots are due to disruptions of these currents within the solar atmosphere (below the photosphere, or forming part of the photosphere itself), produced by the planets, and depending for their number and magnitude on the position (latitude and distance) of the latter relatively to the plane of the solar equator. (See a letter addressed to Sir DAVID BREWSTER, December 21st 1857, published in the "Philosophical Magazine," July 1858.) If this idea has any basis, we may suppose it probable that the moon exercises some similar action upon the earth's electro-sphere, an action depending for its amount on the electric tension of the spheres, these again depending upon that of the sun. Some such idea is necessary to explain the fact, that the lunar effect is very variable in its amount, varying from a movement of the free needle of about 5′, in some days, of one lunation, to a tenth part of this movement in succeeding lunations.

† See "Proceedings of the Royal Society of London," vol. x. p. 1482, 861. I may note here that, as I showed in the "Makerstoun Observations for 1844" (Trans. Roy. Soc. Edin., vol. xviii. p. 354), the difference of the law of solar diurnal variation of declination in Europe for summer and winter was of the same kind as that betwixt the laws for the same periods near the equator, where the movement is inverted; so it followed from the inversion of the lunar law near the equator, with the sun in the northern and southern hemispheres, that in high latitudes, the lunar law should present a greater range in summer than in winter, and that the mean law in high north latitudes should be nearly the opposite of that for high south latitudes. The latter fact should have been evident from preceding observations in mean latitudes of the two hemispheres, and on the discovery of the inversal of the lunar variation with the sun's passage of the equator, I examined these observations for this end. Unfortunately the discussion of the Toronto Observations by General Sabine gave a result nearly the inverse of those derived from the observations at Makerstoun and Prague in the same hemisphere, as I pointed out in the note cited above. General Sabine then discovered that west had been substituted for east in the discussion of the Toronto Observations ; and this correction made, the approximate opposition of the laws for the two hemispheres was at once evident. Of the former fact I satisfied myself by a rediscussion of the "Makerstoun Observations" rejecting the large disturbances; and it has been verified also by the late Mr BACHE, from observations at Philadelphia, though only two years after the result deduced by me of the inversion of the law at the equator had made the conclusion, if not certain, at least extremely probable.

which the law changes with the latitude in each month of the year. For this purpose it will be preferable to reduce the movements of the horizontal needle to those of the needle freely suspended in the direction of the magnetic inclination, since the latter become greatly exaggerated in high latitudes when observed in the more commodious suspension of the horizontal needle.

Stations.

Ten stations have been chosen for this investigation. The following table contains the facts required concerning them :—

TABLE III.—*Data relating to the Stations and Observations of Magnetic Declination considered in this Paper.*

Station. No.	Station. Name.	Latitude.	Longitude.	Height above Sea.	Magnetic Inclination.	Years of Observation.	Directors of the Observatories.
		° ′	h m	Feet.	° ′		
1	Makerstoun,	55 35 N.	0 10 W.	213	71 30 N.	1843–46.	Mr J. A. Broun.
2	Toronto, .	43 40 N.	5 17 W.	...	75 15 N.	1842–48.	Lieutenant Riddell, R.A., Lieutenant Lefroy, R.A., and Lieutenant Younghusband, R.A.
3	Simla, . .	31 6 N.	5 9 E.	8000 ?	41 40 N.	1842–44.	Major-General Boileau.
4	Bombay, .	18 56 N.	4 51 E.	A few feet.	18 44 N.	1851–55. 1857–61.	Captains Montriou and Ferguson of the Indian Navy.
5	Madras, .	13 4 N.	5 21 E.	A few feet.	7 40 N.	1846–50.	Mr T. G. Taylor, Colonel Worster, and Captain Jacob.
6	Trevandrum,	8 31 N.	5 8 E.	200	2 30 S.	1853–64.	Mr J. A. Broun.
7	Singapore,	1 19 N.	6 56 E.	A few feet.	12 40 S.	1841–45.	Captain C. M. Elliot.
8	St Helena,	15 57 S.	0 23 W.	1760	22 0 S.	1842–47.	Lieutenant Lefroy, R.A., and Lieutenant Smythe, R.A.
9	Cape of Good Hope, .	33 56 S.	1 14 E.	A few feet.	53 20 S.	1841–46.	Lieutenant Eardley Wilmot, R.A.
10	Hobarton,	42 53 S.	9 50 E.	105	70 20 S.	1843–48.	Commander Kay, R.N.

1. Transactions of the Royal Society of Edinburgh, Vols. XVII. XVIII. XIX.
2, 8, 9, 10. Observations at the Colonial Observatories, published under the superintendence of Major-General Sabine.
3. Simla Observations. Copy of Abstracts issued from the Simla Observatory.
4. Bombay Observations, printed at Bombay by order of Government.
5. Madras Observations, printed at Madras by order of Government.
6. Observations, unpublished.
7. Singapore Observations, printed by order of Government.

It will be perceived that five of the stations have north magnetic latitude, and five south magnetic latitude, while their distribution varies in longitude betwixt $5^h\ 17^m$ W. and $9^h\ 50^m$ E. The means are not derived from observations in the same years, nor from the same number of years' observations, and they are not therefore strictly comparable. It is conceived, however, that in each case there is a near approximation to the *mean* diurnal variations for each month at each station, so near that the addition of any number of years' observations would not alter in any way the conclusions of this paper.

Comparison of the Mean Diurnal Variations at different Stations.

The monthly mean diurnal variations for each station having been reduced to those of the needle freely suspended in the direction of the magnetic inclination, the resulting values were projected in curves, Plate XLIV., which we shall proceed to consider month by month.

January.—The minimum of *easterly* declination (north end of needle farthest *west*) occurs from Bombay to Hobarton at beween 7 A.M. and 9 A.M. At Simla, there is a minimum at 7 A.M., but rather less marked. This minimum evidently diminishes gradually in importance as we proceed north, being shown at Makerstoun in a very faint degree only. This minimum is followed by a maximum occurring betwixt 9^h and 9^h 30^m A.M., which also becomes less marked as we proceed towards Makerstoun, where it appears as an inflexion on the descending branch of the curve; it disappears in like manner south of Trevandrum, where it is seen as a slight maximum on the ascending branch. The minimum near 8 A.M. can, therefore, be traced from Hobarton to Makerstoun, while the maximum near 9^h 30^m A.M. appears only north of the equator. From Makerstoun to Trevandrum the maximum is followed by a minimum (the principal minimum from Simla northwards) betwixt 1^h 30^m P.M. and noon. This minimum, which is so well marked from Madras northward, almost disappears in the next $4^\circ\frac{1}{2}$ of latitude, being seen faintly at Trevandrum. At Singapore it is no longer visible.

At Trevandrum and the stations south of it, the principal maximum of easterly declination is attained near 2 P.M., excepting at St Helena, where it occurs near noon. As marked, a displacement occurring betwixt Singapore and St Helena as betwixt Singapore and Madras. In the latter case, the mode in which the transition happens is shown by the curve for Trevandrum; as there is no station betwixt Singapore and St Helena, the leap appears more abrupt.

A secondary maximum and following minimum occur in the group Simla to Trevandrum at 4 P.M. and 6 P.M., which become more marked in succeeding months. St Helena and the Cape have a minimum at 5 and 6 P.M., followed by a maximum near 9 P.M. At Makerstoun, the station farthest north, the maximum occurs near 10 P.M.

February.—The remarks made upon the curves for the preceding month apply generally to those for this month. The curve for St Helena differs less from those for Singapore and the Cape than before the maximum occurring betwixt 1 and 2 P.M.

March and April.—In these months, when the sun is near the equator, the opposition betwixt the two hemispheres is well seen. The northern curves have the maximum betwixt 8 and 9 A.M., and the minimum near 1 P.M. The southern curves have the minimum betwixt 9 to 10 A.M., and the maximum betwixt 1 and $2^h\ 30^m$ P.M.; noon at St Helena in April. The passage from one form to another is shown at Trevandrum in March, and at Singapore in April. At St Helena, also, a change has begun, which throws the maximum to noon. The maximum and minimum in the highest north latitudes occur about one hour earlier than the minimum and maximum at the stations farthest south. The passage from the northern to the southern form does not appear in these months to take place by a sliding shift of the epochs; at Trevandrum the curve for March approaches the straight line, and is very nearly that which might have been derived from the mean of the curves for Madras and Singapore in the same month.

May, June, July, and August.—These months may be considered together, as the comparison gives nearly the same conclusions. The first remark is, that when the sun is farthest north, there is really no inversion of the law of movement betwixt the latitudes of 56° N. and 43° S. The movements are, with slight exceptions, as follow:—The north end of the needle moves eastward from after midnight till near 7 A.M., it then turns westward, attaining the extreme westerly position at from 1^h to $10^h\ 30^m$ or 11^h P.M., the former hour farthest north, the latter farthest south, and near noon at the equator. The north end of the needle then moves eastwards till from 3 to 6 P.M., the latter hour at the northern stations, the former at the southern, and near $4^h\ 30^m$ P.M. at the equator. The north end then moves westerly for a short period at all the stations, from Simla southwards, or continues slowly eastwards at the most northern stations. From about 8 P.M. the movement is easterly at all the stations, excepting Hobarton, where the easterly movement does not commence till near midnight.

In general, the curves for these months show a slight shift of the epoch of minimum, and at Hobarton the afternoon maximum becomes the principal one; but there is no inversal of the law of movement.

The curves for St Helena show a curious movement near noon, which appears in some degree the equivalent of the movement at Simla, Bombay, and Madras, in November to February. Thus, at St Helena, in May, June, July, and August, there is the maximum at 7 A.M., a minimum at 10 A.M., a maximum at noon to 3 P.M., and minimum at 5 P.M.; while at Bombay, &c., in November, December, January, and February, there is the minimum at 7 A.M., a maximum at $9^h\ 30^m$ A.M., a minimum at noon, and maximum at 4 P.M. It seems probable that at some station betwixt St Helena and the equator this resemblance of opposition would be better shown.

September.—In this month the minimum, which occurs betwixt noon and 1 P.M., from Trevandrum northwards shifts suddenly to 10 A.M. at Singapore, at which hour it occurs at all the southern stations, so that the shift occurs in this case between $8^\circ\frac{1}{2}$ N., and $1^\circ\frac{1}{2}$ N. The St Helena curve resembles the inverse curve for Simla and Madras in March.

October.—The opposition of the curves for the two hemispheres begins to show itself again nearly as in the curve for April, the Singapore curve for that month resembling the Trevandrum curve for October. The passage from one form to the other is not shown in the same way, nor with the same distinctness, as in March.

November and December.—The curves for these months resemble, generally, those for January and February, the minimum at noon in the Indian curves not being so well marked in December as in the months of November and January. In the winter months the minimum occurs latest at Toronto, which has the highest magnetic latitude.

Ranges of the Diurnal Variations of the Free Needle.

In order to compare the arcs within which the movements of the free needle are performed at the different stations, the following table has been prepared :*—

TABLE IV.—*Ranges of the Mean Diurnal Variations of the Needle freely suspended in the direction of the Magnetic Inclination.*

Months.	Makerstoun.	Toronto.	Simla.	Bombay.	Madras.	Trevandrum.	Singapore.	St Helena.	Cape of Good Hope.	Hobarton.
January,	1′·85	1′·49	1′·22	1′·73	1′·63	2′·06	2′·64	3′·45	3′·08	4′·05
February,	2·25	1·54	1·16	1·39	0·96	1·48	2·89	4·81	4·56	4·28
March,	2·94	2·30	2·69	2·57	2·36	0·79	1·67	4·57	4·35	3·30
April,	3·56	2·48	3·88	4·07	3·71	1·67	1·13	3·06	2·80	2·60
May,	3·48	3·03	4·27	4·72	4·69	2·90	2·24	2·45	2·34	1·56
June,	3·67	3·08	4·37	4·69	4·77	3·06	1·83	3·00	1·93	1·28
July,	3·35	3·05	3·90	4·39	4·57	3·06	1·94	3·17	1·99	1·36
August,	3·42	3·48	4·36	5·42	4·75	3·64	2·60	3·32	2·75	1·87
September,	3·30	2·51	3·99	5·38	5·28	3·31	2·07	2·22	2·56	2·65
October,	3·01	1·74	2·36	2·59	2·37	1·27	2·38	4·11	3·55	3·80
November,	2·39	1·58	0·98	0·90	1·50	2·14	2·97	3·75	3·81	4·08
December,	1·91	1·33	0·78	1·16	1·17	2·33	2·90	3·28	3·26	4·13
Mean,	2·93	2·30	2·83	3·25	3·14	2·31	2·35	3·42	3·08	2·91

Minimum Ranges.

From this table it appears that the minimum movement of the free needle, between the latitudes of 56° N. and 43° S., occurs in January at Simla; it descends to Madras in February, to Trevandrum in March, and to Singapore in April. In May, June, July, and August, the minimum movement occurs at Hobarton (or perhaps between Hobarton and the Cape); in September at Singapore, or between Singa-

* The values in the table are taken from the reduced hourly observations, and not from the curves, which would give the ranges slightly greater in some cases.

pore and St Helena; it happens in October at Trevandrum; in November at Bombay, and in December again it has returned to Simla. The minimum movement of the free needle thus passes from latitude 31° N. (or it may be somewhat north of this latitude) in December and January, to the equator in March and April, and thence to 43° S., in June and July, recurring near the equator in October, and returning to Simla in December.

Maximum Ranges.

The maximum movement of the free needle occurs in January at Hobarton; in February and March, at St Helena (or between St Helena and the Cape); in April, May, June, July, August, and September, at Bombay or Madras (or perhaps betwixt these two stations); in October at St Helena; and in November and December at Hobarton. The maximum movement changes its locality with the period of the year, like the minimum, but it does not proceed so far north; in April and June, perhaps, it approaches Simla. The years from which the Simla results are derived give probably somewhat less than the mean values; those for Madras, on the other hand, give values somewhat too great.

The maximum movement of the free needle never occurs near the equator, as at Trevandrum and Singapore, but passes suddenly, during the minimum movements at these stations, from Bombay to St Helena, or from St Helena to Bombay. The greatest movement of the free needle at any station occurs at Bombay in August and September, and is equal to 5′·42, which is equivalent to a mean diurnal oscillation at Toronto of the horizontal needle of 20′·9; while the greatest monthly mean diurnal oscillation of the horizontal needle at Toronto, deduced from the observations for 1842 to 1848, including the disturbance years of 1847–48, is only two-thirds of this quantity. In whatever way we combine the results to represent the mean movement in each hemisphere, it seems to me probable that the mean movement of the free needle for the *whole earth* is greatest in August.

The smallest movement at any station occurs at Simla and Trevandrum; at the latter from a combination of different movements. The least and greatest movements of the free needle occur nearly under the tropic of Cancer.

When we take the mean of the monthly ranges for each station, we find Toronto to have the smallest mean, and next the equatorial stations of Trevandrum and Singapore.*

* It should be remembered, that we are examining the ranges of the monthly mean diurnal variations, and that these are diminished near the equator, by the change of law occurring in months near the equinoxes.

Change of Epochs of Maxima and Minima.

In order to study the change of epochs of the minima and maxima of the ranges of the mean diurnal variation of magnetic declination at each station, we may divide the stations into two groups—the extra tropical and the intertropical. It will be at once evident, that the epochs of minima and maxima for the northern groups have the same, or nearly the same, position relatively to the December and June solstices, that those for the southern groups have to the June and December solstices.

Thus Makerstoun, Toronto, Hobarton, and the Cape, have only one minimum and one maximum of range. At Makerstoun they occur near the December and June solstices; at Hobarton near the solstices of June and December, respectively. At Toronto the minimum occurs near the December solstice; at the Cape near the June solstice; while at Toronto the maximum range happens two months after the June solstice, it occurs two months after the December solstice at the Cape of Good Hope.

A similar resemblance of opposition may be traced betwixt the northern and southern intertropical stations. Thus, at Simla, Bombay, Madras, and Trevandrum, the minima of range occur in October or November, and in February or March; while at Singapore and St Helena they occur in April or May, and in September. That is to say, in the northern stations one or two months before the December solstice, and near the vernal equinox; while at Singapore and St Helena they happen one or two months before the June solstice, and at the autumnal equinox. In like manner, the maxima occur in the stations from Bombay to Trevandrum in August or September, with the secondary maximum in January or December; while at Singapore and St Helena the secondary maximum occurs in August, and the principal maximum in February.*

General Remarks.

When we consider the curves for each station separately, we perceive that they pass gradually from one form to another from month to month—the changes being generally a gradual shift of the critical hours, and a variation of the relative values of maxima and minima. The chief exception to this remark is to be found in the change at Trevandrum in the month of March. In the Indian group, and at stations even farther north, which I have not considered in this paper, the

* At all the stations there is a kind of double in the epoch of principal maximum, seen more or less markedly in May or June, and in August, in the northern stations; in October or November, and in February, in the southern stations; at Singapore the range for November is slightly greater than that for February. I should also notice, as a deviation from the law of change of minimum from one station to another, the occurrence of the minimum oscillation at Madras in December, instead of November, as its position betwixt Bombay and Trevandrum would indicate. I feel inclined to believe that this deviation would not have appeared had the ranges for Bombay and Madras been derived from observations during the same years.

curves present an apparent anomaly in the movement betwixt 7 A.M. and noon for the four months November to February, which is a combination of the movements in high north and south latitudes at the same period. Yet we find that this form continues in a general way throughout the year, the maximum shifting from $9^h\ 30^m$ A.M. to $7^h\ 30^m$ A.M.

It appears from the first part of this paper, that the law of movement varies with great rapidity from day to day near the magnetic equator about the time of the equinoxes, and especially in the month of March, when the mean curve at Trevandrum approaches the straight line. From the second part it appears that the law of movement varies rapidly with the latitude at the same time of the year near the equator. There is a zone of disturbance near the equator when the sun passes from one hemisphere to another. It will require investigations at other stations to determine how far this zone extends. If we may judge from the epochs of minimum movement, it would appear that the zone is north of the equator when the sun is in the southern hemisphere, that it reaches the equator with the sun when the disturbance has the greatest intensity, and that it passes south of the equator as the sun moves northwards.

The number of stations should be increased betwixt the latitudes of St Helena and Singapore, to show clearly the mode in which the law changes near the equinoxes from one station to another, though there can be little doubt that a station midway betwixt the two would show movements represented nearly by the mean of the curves for St Helena and Singapore. I have myself made observations of magnetic declination simultaneously at Trevandrum, at a station ninety miles north on the magnetic equator, and at Cape Comorin, forty miles south of Trevandrum, during the equinoxes. I shall hereafter show the incremental curves due to these changes of latitudes.*

I have already noticed a fact pointed out by me in the Makerstoun Observations for 1844,† that the difference of ordinates of the curve representing the summer movement at Makerstoun, and of that representing the winter movement at the same place, gave a curve representing the typical movement of the southern hemisphere; so that the relation of the summer curve at Makerstoun to the winter curve at the same station, was the same in kind as that betwixt the two opposite curves seen near the equator. In other words, the summer curve in high north latitudes was only diminished in range by the change which sufficed to invert the curve for the same epoch at the equator.‡

* Observations were also made at a height of 6000 feet above Trevandrum, and twenty miles W.N.W. of it.

† Trans. Roy. Soc. Edin. vol. xviii. p. 354.

‡ It is not meant that the change was the same in amount in high latitudes and near the equator, though, when we consider the variations of the horizontal needle at Makerstoun and Trevandrum, the changes are nearly equal.

If we subtract the curve with maximum movement from that of minimum movement of the *free* needle at each station in the higher latitudes where the curves follow nearly the same law, or

This idea has since then (I have no doubt without any knowledge of my note on the subject) been generalised in several discussions by General SABINE,* who, in subtracting the ordinates of the mean curve for the whole year at any station from the mean curves for the summer and winter half years, has obtained two curves opposed to each other, representing the changes produced in the mean curve by the sun in each hemisphere.

It appears to me that this fact does not merit any greater value than that attributed to it in the original note already cited; since if we represent the two curves for the half years when the sun is north and south of the equator by the equations

$$y_n = a_0 + a_1 \sin(\theta + c_1) + a_2 \sin(2\theta + c_2) + \ldots$$
$$y_s = b_0 + b_1 \sin(\theta + d_1) + b_2 \sin(2\theta + d_2) + \ldots$$

The curve for the mean of the year will be represented by the equation

$$y = \frac{a_0 + b_0}{2} + \frac{a_1 \sin(\theta + c_1) + b_1 \sin(\theta + d_1)}{2} + \ldots;$$

and the curves representing the difference betwixt that for each half year and the mean of the year will be given by the equations

$$y_n - y = \frac{a_0 - b_0}{2} + \left(\frac{a_1}{2}\sin(\theta + c_1) - \frac{b_1}{2}\sin(\theta + d_1)\right) + \ldots$$

$$y_s - y = -\frac{a_0 - b_0}{2} - \left(\frac{a_1}{2}\sin(\theta + c_1) - \frac{b_1}{2}\sin(\theta + d_1)\right) + \ldots;$$

or the two equations will have the form

$$y_n - y = \mathrm{A}_0 + \mathrm{A}_1 \sin(\theta + \mathrm{C}_1) + \mathrm{A}_2 \sin(2\theta + \mathrm{C}_2) + \ldots$$
$$y_s - y = -\mathrm{A}_0 - \mathrm{A}_1 \sin(\theta + \mathrm{C}_1) + \mathrm{A}_2 \sin(2\theta + \mathrm{C}_2) + \ldots;$$

equations to two similar curves with opposed ordinates. When c_1 and d_1 do not differ much, which is generally the case, the singular points in the difference curves will occur nearly at the same time as in the curves for the whole year; or $\mathrm{C}_1 = \frac{c_1 + d_1}{2}$ approximately.

This, however, is true of any curves varying in range, as in the diurnal curve of temperature for example, which obeys a law resembling that of the magnetic

the two curves of maximum movement from each other where they have opposite forms (as the curve of August from that of December at Trevandrum, and that of August from the curve of February at St Helena), we shall find that the change of movement, or the range of the difference curve, is three or four times greater near the equator than in high latitudes, St Helena being the station of greatest change of form, the range of the difference curve being seven minutes and a half (7′·5).

If the variation of the movement of the *free* needle from summer to winter had been nearly as great at Makerstoun, Hobarton, or Toronto, as near the equator, the curve would have been completely inverted. It is simply because the variation of range is less in high latitudes than near the equator that the inversion does not take place there. This fact does not appear when we compare the movements of the horizontal needle.

* It has also been employed by the Rev. P. SECCHI in the same way as by General SABINE. Monthly Notices Roy. Ast. Soc. vol. xv. p. 27.

declination in high latitudes, having a greater range in summer than in winter. A similar operation then performed on the curves of temperature would produce a similar result; but, as we know, the temperature curve is not inverted in the southern hemisphere, the maximum temperature occurring somewhat after noon everywhere.

Though this process of subtracting the mean curve from its two parts cannot have any marked value in the consideration of the phenomenon of the diurnal variation of magnetic declination, yet I desire to point out that the subtraction of curves, or of their equivalent equations, from each other, indicated by me in my original note already cited, may lead in other ways to definite ideas of the increments of movement (the curval increments, if I may so term them), which are due in one place to a change of position of the sun, or which are due for the same position of the sun to a change of latitude. These operations, which have been already performed by me for different places, may be of importance with reference to the comprehension, not only of the rapid changes of law near the equator, but also of the changes due to an approach to the magnetic poles.

Ingenious attempts have been made to connect the diurnal variations of magnetic declination with the electrical currents observed by means of telegraph wires. These currents are, I believe, quite local; it is extremely probable that they produce variations of a second order: others of the first order are due to the polar currents, connected with the polar lights; but these are superposed on the regular solar variations. We have, I think, only to look at the group of curves for the months of May to August, to see the fallacy or the insufficiency of all the hypotheses proposed as yet to explain these variations. We have, it appears to me, to deal with a general (in opposition to local), if not a cosmical cause. The position of the station as regards coasts, centre of continents, or as to height, does not seem to produce any marked variation in the regularity of the law and its change from north to south, in the months referred to.

I have already noticed the great effect of the lunar action, sometimes exceeding that of the sun, and have offered a suggestion, which might lead to a theory. I would only add, at present, that since writing the note, page 680, it has occurred to me that I endeavoured to show in 1850, in the General Results of the Makerstoun Observations (I am not able to cite the page at present), that the aurora borealis was most frequent near the epoch of full moon. Though the number of auroræ, from which this conclusion was derived, was too limited to give it great weight, yet, as the auroræ were sought for on the appearance of the slightest irregularity in the movement of the magnets (a sure premonition) it merited a greater weight than one deduced from masses of observations by different observers, made without system, and noted chiefly when most easily seen—that is, when the moon does not shine. I believe I also pointed out the occurrence of the most brilliant auroræ near the epoch of opposition. These observations and conclusions may give some support to the suggestion I have made as to the action of the moon on the earth's electro-sphere, the tension of which I have supposed to depend upon that of the sun.

I would draw attention again to MAIRAN's theory, of a connection betwixt the zodiacal light and the solar light. I asked before, "Is the zodiacal light not the magnetic [or electric] ether in a luminous state?" * I have frequently observed the zodiacal light in $8\frac{1}{2}$ N. latitude, at Trevandrum, when, according to the observations, the summit of the lens of light must have considerably surpassed the zenith at sunset. Whatever this light may be, or whatever its cause, it cannot fail to act upon the earth and its satellite, and to be acted on by them. That it extends far beyond the earth's orbit in a non-luminous state, and that it is acted on by more distant planets than the earth, seems to me probable; and it may be, that in this action we should seek the connection betwixt the position of the planets and the solar spots; which spots I have already suggested, are disruptions of the electric atmosphere of the sun,† disruptions passing through the photosphere—the photosphere itself being the dense base of the zodiacal light.

* See Letter, dated Trevandrum, 21st December 1857. Philosophical Magazine, July 1858.

† See Letter just cited, where I have also noticed that CASSINI and MAIRAN supposed a connection betwixt the intensity and extent of the zodiacal light and the solar spots.

XLVI.—*On an Application of Mathematics to Chemistry.* By ALEXANDER CRUM BROWN, M.D., D.Sc.

(Read 18th February 1867.)

Prefatory Note, added June 24, 1867.

Since reading this paper, I have seen Sir BENJAMIN BRODIE'S paper on "The Calculus of Chemical Operations," read before the Royal Society of London.

The two papers resemble one another, merely in being applications of mathematical language to chemistry. They entirely differ in method, object, and result.

I may here mention, what I have omitted to state explicitly in the paper, that I have no idea of attempting to substitute a functional notation for that in common use. I only propose to use a functional notation to express certain general and serial relations in those cases where the common atomic notation is inconvenient or obscure.

The full application of Mathematics to Chemistry can only be made when a fundamental physical hypothesis is discovered, from which, by means of mathematical methods, results may be deduced which coincide with the observed facts of chemistry. In the meantime, however, a profitable application of mathematics may be made in another direction. Mathematics is the science by means of which consequences are deduced from laws; and although we have not, as yet, discovered the laws of chemistry, we have what may represent them,—approximate generalisations. To these we can apply mathematical methods, or at least we can express them in mathematical language.

I.—*Definition of Symbols.*

The objects of chemical study are of two classes—1*st*, substances; and 2*d*, processes performed on these substances. In this paper I shall represent the former as operands, and the latter as operators; where the contrary is not specially mentioned, a single operand symbol will be used to indicate one molecule, or chemical unit of a homogeneous substance.

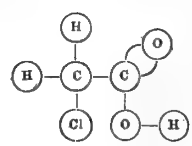
An operator ϕ is defined by the chemical equation or equations connecting a, and $\phi \cdot a$, a being a molecule of a substance, and $\phi \cdot a$ the result of applying the process ϕ to it. Thus if the chemical equation be $a + KHO = \phi \cdot a + H_2O$, the process ϕ as applied to a, is the union of a with a molecule of caustic potash and the simultaneous elimination of a molecule of water.

II.—Combination of Symbols.

The sign $+$ will be used in a purely enumerative sense, to connect operands. Thus, $a + b + c +$ &c., means one molecule of a, and one of b, and one of c, &c. In the same way na means n molecules of a, the sign $+$ and numerical multipliers never being used to indicate combination.

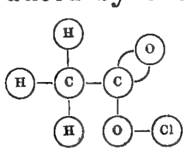
Many chemical processes are capable of being applied either to *one* molecule or to several simultaneously. Thus, if ϕ be defined by the equation $a + NH_3 = \phi\cdot a + 2HCl$, or by a series of equations equivalent to this, ϕ may be performed either on one molecule containing two atoms of chlorine, or on two molecules, each containing one. In the latter case, a becomes $a + b$, and $\phi\cdot a$ becomes $\phi\cdot(a + b)$, here ϕ is not performed on *a and* on b, but on $(a + b)$; that is, partly on a and partly on b, so that $\phi\cdot(a + b)$ is not equal to $\phi\cdot a + \phi\cdot b$. The distributive law does not, therefore, hold good here. $(a + b)$ is, however, the same as $(b + a)$, and generally we may change the order of operands connected by the sign $+$.

In $\phi\cdot(a + b + c +$ &c.$)$, when $a, b, c,$ &c. are identical, we have $\phi\cdot na$, and just as $\phi\cdot(a + b + c +$ &c.$)$ is not equal to $\phi\cdot a + \phi\cdot b + \phi\cdot c +$ &c., so $\phi\cdot na$ is not equal to $n\,\phi\cdot a$; in other words, the commutative law of multiplication does not hold good for operators and numerical multipliers.

If a new process ψ be performed on the operand $\phi\cdot a$, there are two ways in which it may act; 1*st*, the process ψ may act *independently* on a, thus if $a = CH_4$ and ϕ be the replacement of H by COHO, and ψ the replacement of H by Cl, ϕ and ψ may act independently, giving chloracetic acid, 

$\psi\cdot(\phi\cdot a)$

; or the new operator ψ may act on that part of the molecule which has been introduced by the process ϕ, giving in the instance above the acetate of chlorine,



$\psi\cdot(\phi\cdot a)$

$(\psi\cdot\phi\cdot a)$ may therefore have two quite distinct meanings, and it will be advisable to have two forms of notation for it. I shall represent the first by $\left.\begin{matrix}\phi\\ \psi\end{matrix}\right|\cdot a$, and the second by $\psi\cdot\phi\cdot a$, the complex operator $\psi\cdot\phi$ being in the case cited above, the replacement of H by COHO, in which the H is replaced by Cl, and may, therefore, properly enough be said to be the operator ϕ, acted on by ψ.

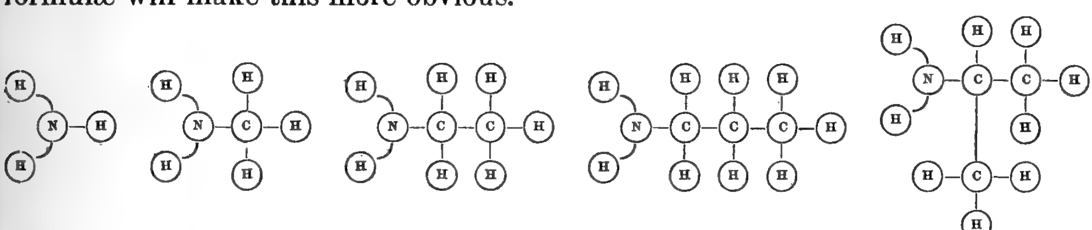
In the first case, which may be called the *vertical multiplication of operators*, I shall assume for the present, that as the operators act independently, their order may be varied without change of meaning, and that the commutative law of multiplication applies.* In the second case, which may be called the *horizontal multiplication of operators*, it is obvious that the order cannot be changed without changing the meaning,—here then the commutative law does not apply, $\phi\cdot\psi\cdot a$ being in general different from $\psi\cdot\phi\cdot a$.

III.—*The Laws of positive Integral Indices, as applied to Operators.*

In the case of vertical multiplication, we may represent $\left.\begin{array}{c}\phi\\ \phi\end{array}\right|\cdot a$ by the symbol $\phi^{2]}\cdot a$, $\left.\begin{array}{c}\phi\\ \phi\\ \phi\end{array}\right|\cdot a$, by $\phi^{3]}\cdot a$, &c., and generally, it is obvious, that $\left.\begin{array}{c}\phi^{m]}\\ \phi^{n]}\end{array}\right|\cdot a = \phi^{m+n]}\cdot a$ and $\left(\phi^{m]}\right)^{n]}\cdot a = \phi^{mn]}\cdot a$, when m and n are positive integers.

In horizontal multiplication, $\phi\cdot\phi\cdot a$, may be written $\phi^2\cdot a$, $\phi\cdot\phi\cdot\phi\cdot a$, $\phi^3\cdot a$, &c., understanding that $\psi\cdot\phi^n\cdot a$ is merely a contraction for $\psi\cdot\phi\cdot\phi\cdots\phi\cdot a$, and also, that $\phi^n\cdot\psi\cdot a$ is $\phi\cdot\phi\cdots\phi\;\psi\cdot a$; in other words, that the complex operator ϕ^n always acts by means of the ϕ at the extreme right of the series, and that an operator acting on it always acts upon the ϕ at the extreme left. With these assumptions, we at once see that the laws of indices $\phi^m\cdot\phi^n\cdot a = \phi^{m+n}\cdot a$, and $\left(\phi^m\right)^n\cdot a = \phi^{mn}\cdot a$, hold good also in the case of horizontal multiplication when m and n are positive integers.

It also follows from these assumptions, that if $\phi^n = \psi$, $\psi\cdot\phi = \phi^{n+1}$, but $\phi\cdot\psi$ may have several values, one of which is ϕ^{n+1}, for in $\phi\cdot\phi^n$, the action of ϕ is restricted to a particular part of ϕ^n, while no such restriction is made in the case of $\phi\cdot\psi$. Thus, if a be ammonia, and ϕ the replacement of H by CH_3, $\phi^2\cdot a$ is ethylamine, and $\phi\cdot\phi^2\cdot a = \phi^3\cdot a$ is propylamine; but if we put $\phi^2 = \psi$, $\phi\cdot\psi\cdot a$ may be either propylamine ($\phi^3\cdot a$) or isopropylamine ($\phi^{2]}\cdot\phi\cdot a$). The graphic formulæ will make this more obvious.



a $\qquad$ $\phi\cdot a$ $\qquad$ $\phi^2\cdot a$ $\qquad$ $\phi\cdot\phi^2\cdot a = \phi^3\cdot a = \phi\cdot\psi\cdot a$ $\qquad$ $\phi^{2]}\cdot\phi\cdot a = \phi\cdot\psi\cdot a$

* It is not by any means certain that this is true, even in the case of the simplest operands; it is almost certain that it is not true in the case of complex operands; but as we have not sufficient data to enable us to form a theory connecting the order in which operators are applied to a molecule with the parts of the molecule upon which they act, I have provisionally assumed the simplest possible law.

ψ being here the replacement of H by C_2H_5, $\phi\cdot\psi$ is the replacement of H by C_2H_5, in which H is replaced by CH_3; ϕ^2 on the other hand, is the replacement of H by CH_3 in which H is replaced by CH_3 and $\phi\cdot\phi^2$, is the replacement of H by CH_3 in which H is replaced by CH_3 in which H is replaced by CH_3.

IV.—*On Negative Integral Indices as applied to Operators.*

Without contradicting any previous assumption, we may define the symbol ϕ^{-1} by the equation $\phi\cdot\phi^{-1}\cdot a = a$, or $\phi\cdot\phi^{-1} = 1$. It is at once obvious that $\phi\cdot\phi^{-1} = \phi^{-1}\cdot\phi$, for if ϕ be the replacement of A by B, ϕ^{-1} is the replacement of B by A, and $\phi\cdot\phi^{-1}$ is the replacement of B by A, in which A has been replaced by B, that is, the replacement of B by B, and similarly $\phi^{-1}\cdot\phi$ is the replacement of A by A; it is different, however, in the case of vertical multiplication, $\left.\begin{matrix}\phi^{-1}\\ \phi\end{matrix}\right|\cdot a$ is not necessarily equal to a, for $\left.\begin{matrix}\phi^{-1}\\ \phi\end{matrix}\right|\cdot a$ expresses the replacement of A by B in one part of the molecule, and of B by A in another part of it, and it is only in particular cases that these two processes will leave the molecule unchanged, $\left.\begin{matrix}\phi^{-1}\\ \phi\end{matrix}\right|\cdot a$ must, of course, be *isomeric* with a.

$\left.\begin{matrix}\phi^{-1}\\ \phi^{-1}\end{matrix}\right|\cdot a$ is from the last chapter $\left(\phi^{-1}\right)^{2]}\cdot a$, and no confusion will arise by writing this $\phi^{-2]}\cdot a$, as the only other meaning which this symbol could have would be $\left(\phi^{2]}\right)^{-1}\cdot a$, and this would be defined by the equation $\phi^{2]}\cdot\left(\phi^{2]}\right)^{-1}\cdot a$ $=a$, which could only mean $\left.\begin{matrix}\phi\cdot\phi^{-1}\\ \phi\cdot\phi^{-1}\end{matrix}\right|\cdot a$, giving as the equivalent for $\left(\phi^{2]}\right)^{-1}$, the expression $\left(\phi^{-1}\right)^{2]}$. We have thus, generally, $\left.\begin{matrix}\phi^{-m]}\\ \phi^{-n]}\end{matrix}\right|\cdot a = \phi^{-(m+n)]}\cdot a$, and $\left(\phi^{-m]}\right)^{n]}\cdot a = \phi^{-mn]}\cdot a$. An expression, $\left.\begin{matrix}\phi^{p]}\\ \phi^{q]}\\ \phi^{r]}\\ \&c.\end{matrix}\right|\cdot a$ (where p, q, r, &c. are any integers, positive or negative) can then always be reduced to the form $\left.\begin{matrix}\phi^{m]}\\ \phi^{-n]}\end{matrix}\right|\cdot a$, by adding all the positive indices to form m, and all the negative indices to form n.

In the case of horizontal multiplication, we have at once $(\alpha\cdot\beta\cdot\gamma\ \dots\ \nu)^{-1}\cdot a$ $=\nu^{-1}\ \dots\ \gamma^{-1}\cdot\beta^{-1}\cdot\alpha^{-1}\cdot a$, and therefore $\left(\phi^{n}\right)^{-1} = \left(\phi^{-1}\right)^{n}$, and we may, therefore, write this ϕ^{-n}; and as $\phi\cdot\phi^{-1}\cdot a = a$, we have $\phi^2\cdot\phi^{-2}\cdot a = \phi^{-2}\cdot\phi^2\cdot a$ $= a$; and, generally, $\phi^{m}\cdot\phi^{-n}\cdot a = \phi^{m-n}\cdot a$; also $\left(\phi^{-m}\right)^{n}\cdot a = \phi^{-mn}\cdot a$ and

$\left(\phi^m\right)^{-n} \cdot a = \phi^{-mn} \cdot a$; or, the ordinary laws of indices hold good in horizontal multiplication where m and n are any integers, positive or negative.*

V.—*Fractional Indices as applied to Operators.*

The symbols $\phi^{\frac{1}{n}}$ and $\phi^{\frac{1}{n}]}$ may be defined thus $\left(\phi^{\frac{1}{n}}\right)^{n} \cdot a = a$ and $\left(\phi^{\frac{1}{n}]}\right)^{n]} \cdot a = a$. When ϕ is of the form ψ^n, the first expression will have a real meaning, and so will the second when ϕ is of the form $\psi^{n]}$. In the opposite case, $\phi^{\frac{1}{n}}$ and $\phi^{\frac{1}{n}]}$ are purely imaginary operators, and it is only when n of them are multiplied together in the first case horizontally, and in the second vertically, that they acquire a meaning. It may, however, be useful to break up an operator into imaginary factors. Thus, if ϕ be the replacement of three atoms of hydrogen by one of nitrogen, $\phi^{\frac{1}{3}]}$ is the replacement of H by $\frac{1}{3}$ N, and where the 3 H atoms replaced by N have different positions in a molecule, it may be convenient to express it in this way. Thus, if a be benzol, ψ the replacement of H by NH_2, and ϕ the replacement of 3 H by N, we may express diazobenzol by $\left.\begin{matrix} \phi^{\frac{2}{3}]} \cdot \psi \\ \phi^{\frac{1}{3}]} \end{matrix}\right| \cdot a$; thus indicating that the process ϕ is performed on $\psi \cdot a$, so that $\phi^{\frac{2}{3}]}$ acts on ψ, and $\phi^{\frac{1}{3}]}$ on a.

In the same way, an operator may be broken up into imaginary factors which are not identical; thus, if ϕ be the replacement of N by O″ and HO, we may put $\phi \cdot a = \left.\begin{matrix} \alpha \\ \beta \end{matrix}\right| \cdot a$ where α is the replacement of $\frac{2}{3}$N by O″, and β the replacement of $\frac{1}{3}$N by HO.

VI.—*On Chemical Groups and Series.*

Those groups which in a former paper (*Trans.* xxiv. 331) I have called chemical genera, and which are characterised by containing a particular radical, the *generic* radical, may each be represented by a generic formula $\phi \cdot X$, where ϕ is an operator introducing the generic radical, and X a molecule or group of

* There is an obvious and important difference between direct and inverse operators, which I may mention here.

If we represent as direct those operators only which express direct processes which can be performed so as to get $\phi \cdot a$ by acting on a, it is plain that if ϕ includes the physical conditions of the process, $\phi \cdot a$ can have only *one* value. It may have a number of conceivable values, and it is the business of the experimental chemist to find out which of these is the real one; but $\phi^{-1} a$ may have more than one *real* value, for ϕ^{-1} is not restricted to mean *one* actual process capable of being performed, but any process, such that $\phi^{-1} a$ shall be by the process ϕ (performed on the part of the molecule, introduced or modified by ϕ^{-1}) reproduce a. Thus, if ϕ be the addition of H_2, and a be alcohol, $\phi^{-1} a$ may represent either aldehyde or oxide of ethylene, for both of these give a when treated by the process ϕ. Examples of this kind might be multiplied to any extent.

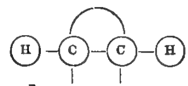
molecules on which ϕ can act. Thus, if ϕ be the replacement of H by COHO, the monocarbon acids, the dicarbon acids, and the tricarbon acids, have the formulæ $\phi \cdot X$, $\phi^{2]} \cdot X$, $\phi^{3]} \cdot X$, respectively.

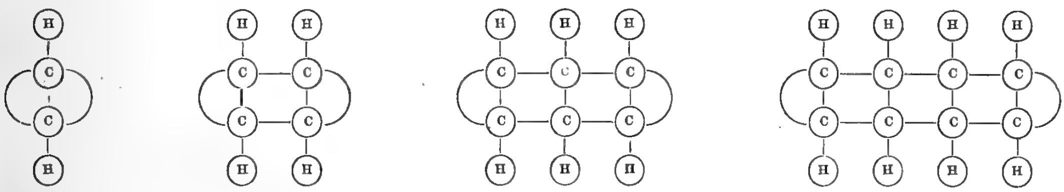
I shall only consider further the application of this method to two other kinds of series, which we may call *functional chemical series.* The first consists of terms of the form a, $\phi \cdot a$, $\phi^{2]} \cdot a$, ... $\phi^{n]} \cdot a$. Here the successive terms are produced by the repetition of the process ϕ *independently* on a, and we may call such series *independent functional series.* We have instances of this in the series CH_4, CH_3Cl, CH_2Cl_2, $CHCl_3$, CCl_4; NH_4I, NH_3CH_3I $NH_2(CH_3)_2I$, $NH(CH_3)_3I$, $N(CH_3)_4I$; C_2H_2, C_2H_4, C_2H_6, or in the so-called homologues of benzol C_6H_6, $C_6H_5CH_3$, $C_6H_4(CH_3)_2$, &c. In all these series we have a common difference between successive terms, and we may observe that all such series necessarily consist of a *definite* number of terms, for the process ϕ can only be performed *independently* on a a definite number of times. If we put $a = \phi^{-n]} \cdot b$, where b is not of the form $\phi^{-1} \cdot x$, n is the number of times ϕ can be performed independently on a, and $n + 1$ is the number of terms in the series.

The other form of functional series is a, $\phi \cdot a$, $\phi^2 \cdot a$, ... $\phi^n \cdot a$. In this the successive terms are derived by the repetition of ϕ on that part of the molecule which was introduced or modified by the previous performance of ϕ, and we may call such a series a successive functional series. In order that such a series may be possible, it is necessary that a be of the form $\phi^{-1} \cdot b$, and that ϕ be of the form $\phi^{-1} \cdot \psi$; in other words, that ϕ can be applied once to a and once to ϕ. We have examples of this kind of series in "homologous" series, such as NH_4I, $NH_3(CH_3)I$, $NH_3(CH_2\overline{CH_3})I$, &c.

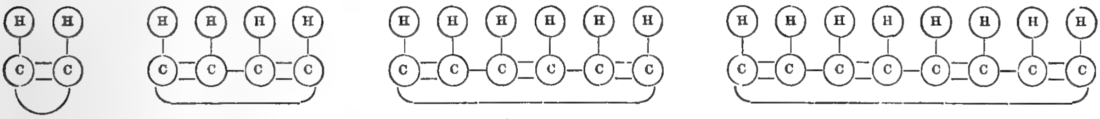
Here the law of derivation of successive terms does not, as in the independent functional series, contain in itself a determination of the number of terms of which the series consists, except where ϕ diminishes the weight of the molecule on which it acts. In this case, however, by inverting the series, we find that there are an indefinite number of terms before a, so that by inversion, if necessary, all such series may be reduced to the form a, $\phi \cdot a \cdot \cdot \cdot \cdot \cdot \phi^n \cdot a$, where a is not of the form $\phi \cdot x$, and is, therefore, the first term.

In a series of this form, if ϕ does not involve x in the expression $\phi\ x$, we have a common difference in weight and composition. This is the case in "homologous" series. If ϕ involve x, there cannot, of course, be a common difference; this case will, however, be examined in a subsequent part of this paper.

There are two varieties of the successive functional series—1st, Where ϕ is a simple addition; and, 2d, Where ϕ is a replacement. In the first, the radical (or radicals) necessarily artiad, must be such that it (or they) can be again added to that part of the molecule which has been introduced or modified by the first ϕ. We have an example of this kind of series in the polymers of acetylene, where ϕ consists in the addition of the radical C_2H_2  This series may be represented in several different ways, and we do not as yet know which is the true one. I append graphic formulæ of two of these ways, to illustrate my meaning—

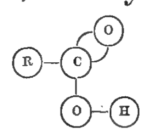


a $\qquad \phi \cdot a \qquad \phi^2 \cdot a \qquad \phi^3 \cdot a$

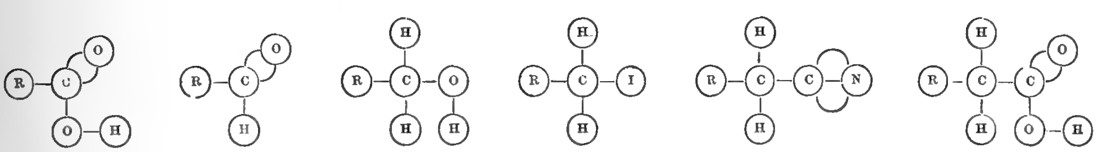


a $\qquad \psi \cdot a \qquad \psi^2 \cdot a \qquad \psi^3 \cdot a$

The second of these is the form proposed by KEKULÉ, and appears on the whole to be the most probable.

In the other variety of successive functional series, ϕ is the replacement of one or more radicals by one or more new radicals. Let the replaced radical or group of radicals, be represented by Γ, and the replacing radical or group of radicals by Λ, and we at once see, first, that Γ and Λ are equivalent; and, second, that Λ must contain Γ, as the process is capable of repetition. When ϕ in $\phi \cdot x$ is independent of x, the difference between Λ and Γ is constant (as indeed Λ and Γ are themselves constant throughout the series), and is the common difference of the series. This is the case in "homologous" series. As an example, we may take the series of the fatty acids. The formula of one of them is 

where R is of the form C_nH_{2n+1}; and taking any of the series of substances through which it is connected with the next higher member of the series, such as



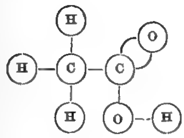
we see that Γ is O'' and $(HO)'$, and Λ is H', H', and $(COHO)'$. Λ thus contains Γ, and the process is capable of repetition. The common difference is here CH_2, but there are instances of series probably of the kind we are now considering (although we cannot at present trace the relation between successive terms) in which we have other common differences. Thus the carbonates, oxalates, and mesoxalates, form a series in which the common difference is CO. In the series —methylic alcohol, ethylenic glycol, glycerine, erythrite, the unknown body $C_5H_5(HO)_5$, and mannite, as also in the series tartronic acid, tartaric acid, the unknown trioxypyrotartaric acid, and mucic acid, we have a common difference of CH(HO). It is quite possible that relations may be discovered between the successive terms of these and similar series, which will show that they are successive functional series; and indeed homology has been assumed upon less evidence in cases where the common difference is the usual one CH_2, as for instance in the series of bases $C_nH_{2n-5}N$ (pyridine, picoline, &c.).

It is instructive to compare *successive* functional series with the *independent* functional series isomeric with them. Thus, ammonia, methylamine, dimethylamine, trimethylamine, with ammonia, methylamine, ethylamine, propylamine: or benzol, toluol, xylol, pseudocumol, &c., with benzol, toluol, ethylbenzol, propylbenzol, &c. KEKULÉ, to whom we are greatly indebted for clearness of conception of the structure of the aromatic bodies, calls both of the last-mentioned series "homologous;" as the word "homologous" has never, as far as I know, been strictly defined, there can be no objection to this; but if we call the series C_6H_6, $C_6H_5\ (CH_3)$, $C_6H_4(CH_3)_2$, &c. homologous, we must apply the same term to the series ammonia, methylamine, dimethylamine, &c. It will be observed, that while no *three* successive terms of an *independent* functional series can belong to the same *successive* functional series, *any* two successive terms may. For $\phi^{n]}\cdot a$ and $\phi^{n+1]}\cdot a$ are $\phi^{n]}\cdot a$, and $\left.\begin{matrix}\phi^{n]}\\ \phi\end{matrix}\right|\cdot a$, and these are the initial terms of the series $\left.\phi^{n]}\right|\cdot a$, $\left|\begin{matrix}\phi^{n]}\\ \phi\end{matrix}\right|\cdot a$. $\left.\begin{matrix}\phi^{n]}\\ \phi^2\end{matrix}\right|\cdot a \cdots\cdot \left.\begin{matrix}\phi^{n]}\\ \phi^n\end{matrix}\right|\cdot a$. Thus, methylamine, dimethylamine, and trimethylamine, are not in successive series, but methylamine and dimethylamine are the initial members of the series which continues methyl ethylamine, methyl propylamine, &c.

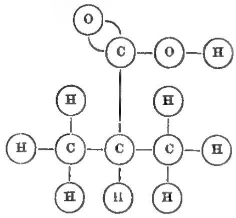
If we restrict the term "homologous series" to successive functional series having the common difference CH_2, we see that one subtance may be the starting point of several homologous series. In this sense, NOAD's toluic acid and α toluic acid are both homologues of benzoic acid. The one series being, benzoic acid, methyl benzoic acid (NOAD's toluic acid), ethyl benzoic acid, propyl benzoic acid, &c.; the other being benzoic acid, α toluic acid, the acid $C_6H_5\cdot CH_2\cdot CH_2\cdot COHO$, &c.

When ϕ in the expression $\phi\cdot x$ involves x, it is obvious that there can be no common difference in the series a, $\phi\cdot a$, $\phi^2\cdot a \cdots\cdot \phi^n\cdot a$.

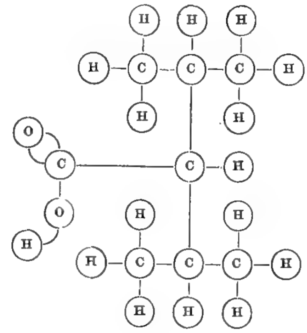
Such series have not been specially examined, but examples can easily be suggested; for instance, let ϕ be the series of processes by which acetic acid is successively converted into acetone, acetonic acid, and isobutyric acid; then ϕ can again be applied to the last mentioned acid, giving isobutyrone, isobutyronic acid, &c., thus giving rise to a series of acids, having the formulæ,—



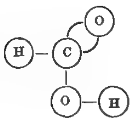
Acetic Acid.



Isobutyric Acid.



Unknown Third Term.

Formic acid  is formally the first term of the series, although as the formic ketone is unknown, we cannot directly prove the relation.

It is, however, unnecessary to dwell longer on such series ; it is sufficient to point out the probability of their existence, in order to show that a common difference is not an essential character of successive functional series.

Fig. 1.

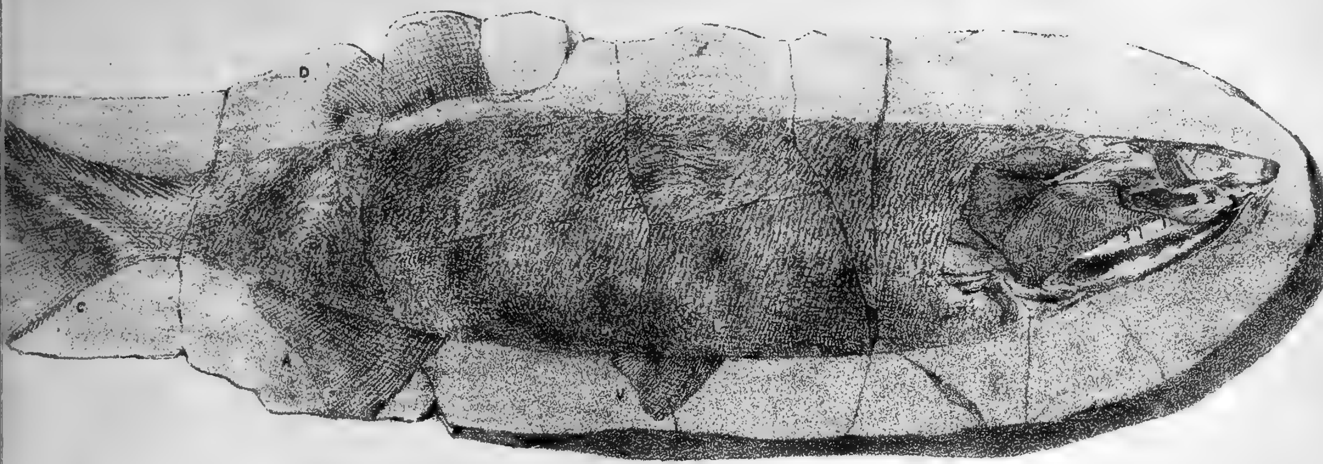


Fig. 3.

Fig. 2.

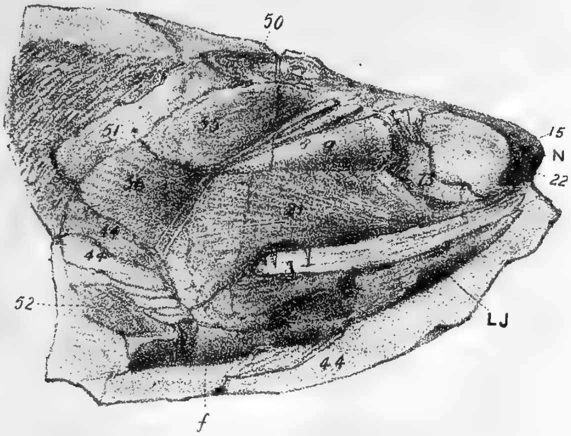


Fig. 5.

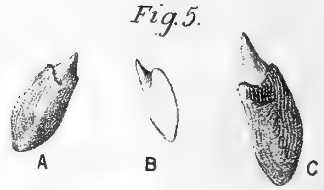


Fig. 6

Fig. 7.

Fig. 4.

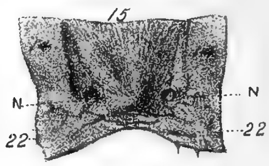


Fig. 8.

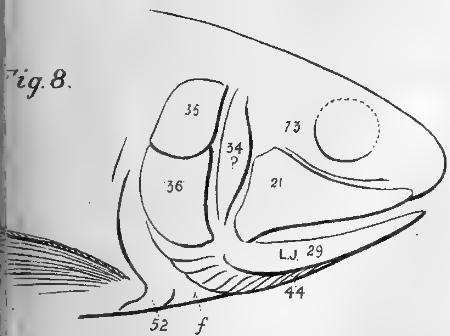


Fig. 9.

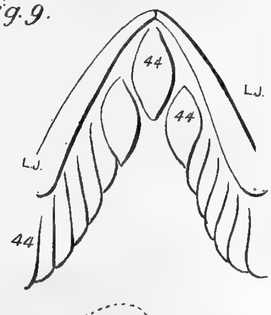


Fig. 12.

Fig. 11.

Fig. 10.

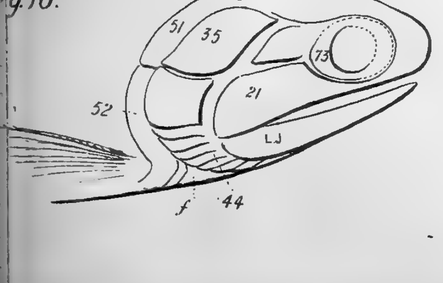
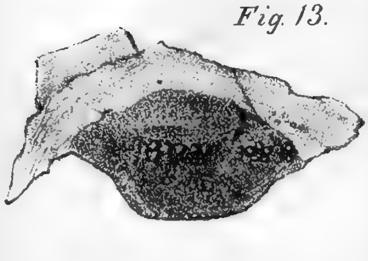


Fig. 13.



XLVII.—*Description of* Pygopterus Greenockii (Agassiz), *with Notes on the Structural Relations of the Genera* Pygopterus, Amblypterus, *and* Eurynotus. By RAMSAY H. TRAQUAIR, M.D., Demonstrator of Anatomy in the University of Edinburgh. Communicated by WILLIAM TURNER, M.B. (Plate XLV.)

(Read 5th March 1866.)

Professor AGASSIZ, in his "Poissons Fossiles," vol. ii. p. 78, has mentioned a species of *Pygopterus* as occurring in the carboniferous shales of Wardie, near Edinburgh, and which he has named *P. Greenockii,* in honour of Lord Greenock, the first collector of ichthyolites from that locality. He has, however, neither given a figure of this species nor any description of it, beyond saying that the known fragments consist of hardly anything but heads, with the anterior portion of the trunk, and that the scales covering this part of the body are higher than broad—a circumstance distinguishing them from the scales of all the other species of the genus.

Having for some time back collected fossils from this locality, I am enabled not only to figure a complete specimen of the fish, but also to describe its structure a little more in detail, though there are still many points concerning which more knowledge would be desirable.

These fishes occur in elongated nodules of clay ironstone, which are almost always divided by transverse fissures into a great many segments. When the nodule becomes detached from its shaly matrix by the action of the sea, these segments become separated and scattered; hence the difficulty of acquiring entire specimens. Heads are, however, more easily found, as the anterior extremity of the fish is usually imbedded in a larger and more compact piece of ironstone than those into which the rest of the nodule divides. To obtain an entire specimen, it is then necessary to find the nodule *in situ,* to extract it from the shale piecemeal, and then carefully to split the separate pieces, which must, lastly, be properly assorted and glued together. The specimens are always much crushed, and often otherwise mutilated; the body of the fish presenting sometimes nothing more than a heap of disjointed scales. The texture of the ironstone is also unfavourable to the complete study of the individual parts, as it is almost impossible to develope or work out the specimen beyond what is exposed by the first splitting of the nodule.

It is also very difficult to obtain a good view of the external sculpturing of the scales and of the bones of the head, as these generally adhere with their outer surface to the matrix; often, indeed, the scales themselves are so split, that neither outer nor inner surface, but a section, is visible.

The specimen shown in fig. 1 equals $14\frac{1}{4}$ inches in length, and $2\frac{5}{8}$ inches in breadth opposite the ventral fins. The other measurements are as follows:—

From tip of snout to opposite the origin of dorsal fin,	. .	8 inches 8 lines.
,, ,, of ventral ,,	. .	6 inches.
,, ,, of anal ,,	. .	9 inches 1 line.
,, ,, of caudal ,,	. .	12 inches 4 lines.

Scales.—These are rather small in proportion to the size of the fish, but are much larger along the back and upper part of the sides than on the belly, where they are very minute. The larger scales are rather narrow, being, as AGASSIZ remarks, much higher than broad. The upper border of each scale projects in a very strong pointed spine, which is received into a corresponding excavation in the deep surface of the scale next above. Below the base of the spine the upper margin of the exposed portion of the scale is seen to be deeply concave, while the lower margin is rounded off into a blunt point, which fits on to the concave notch-like upper margin of the exposed portion of the scale next in order below. The whole of the exposed surface is ornamented by very delicate, nearly parallel, wavy ridges, whose general direction is from above downwards towards the lower angle of the scale, sometimes anastomosing with each other on the way.

The small scales along the body become more nearly equilateral, and those on the continuation of the vertebral column into the upper lobe of the tail-fin are also very small, and of an elongated lozenge shape. The largest scales are, however, those along the upper border of the tail. They are triangular in shape, acutely pointed behind, and notched in front, thus resembling very closely the corresponding scales in *Amblypterus* and *Eurynotus*, but being very much larger in proportion to the other scales. They are placed over each other in an imbricating manner, whereas very little imbrication is observable in the scales of the rest of the body,

Pectoral fins.—The specimen figured does not exhibit the pectoral fins, and I have seen no example of this fin complete to its termination. A fragment in my collection shows the pectoral to have been wide and powerful, the anterior rays strong, and with the transverse articulations farther apart than in the other fins, at least at their commencement. Another fragment shows that the anterior border of the pectoral fin was ornamented by a series of rather small imbricating fulcral scales.

The *ventral fin* of the right side is shown in fig. 1, V, its extremity being cut off, like those of the other fins of this specimen, by the edge of the nodule. Its base is wide, measuring in the present specimen one inch across, and the number of rays may be estimated at no less than 35. In the more posterior finer rays bifurcation may be observed, but the stronger anterior ones are cut off before any such division takes place. The fulcral scales of the anterior margin are not seen, being obscured by the matrix.

The *dorsal fin* (fig. 1, D), measuring 1 inch 8 lines in breadth at its origin, is placed very far back, being nearly opposite the anal, and consists approximately of about 45 rays, which are rather coarse in the anterior two-thirds of the fin, but become finer posteriorly. The transverse articulations of the individual rays are rather close together, but the joints are still longer than broad, and the whole ray bifurcates a little beyond its middle. Fine, closely set fulcral scales are observed along the anterior margin of the fin.

The *anal fin* (fig. 1, A), like the dorsal, is large and strong; its base measures 1 inch 9 lines across. As in the latter, the transverse articulations of the rays are approximated; the joints of the first transverse row are, however, nearly twice as long as broad. The anterior margin is, as usual, set with small fulcral scales.

The *caudal fin* (fig. 1, C) is by far the largest, and, as far as its lower lobe is concerned, the longest of the azygos fins, the portion preserved of this lower lobe being, in the specimen figured, $1\frac{3}{4}$ inch long at its anterior margin; and we may safely suppose it to have been originally at least $\frac{3}{4}$ of an inch longer. It is, however, the worst preserved of all the fins of the specimen, so that all attempts to count its rays are in vain. These are somewhat finer than those of the dorsal and anal fins; their transverse articulations are much approximated, so that the joints are almost square. The stouter rays of the front of the lower lobe are not seen to bifurcate so far as they extend, till cut off by the edge of the nodule; the succeeding finer ones begin to split about their middle. The rays of the upper lobe are too badly preserved to admit of description, further than that they are comparatively short, and very fine. The tail, as a whole, is most typically heterocercal, the extremity of the body being continued along the upper lobe of the caudal fin, where it is defended by the two kinds of scales, lateral and mesial, already described.

On examining another specimen, not figured, I find that the rays of the caudal fin, like those of the anal of *Polypterus*, commenced below the skin in narrow flat pieces, imbricating from before backwards, and not divided by transverse articulations.

Internal skeleton.—I have in no instance found any trace of vertebral bodies, but, scattered among the scales of disjointed specimens, may often be seen pointed bony fragments, which must be the remains of the vertebral apophyses.

The *interspinous bones* of the azygos fins seem to have been well developed, as shown in a specimen not figured, but already referred to in the description of the rays of the caudal fin, and in which these bones, supporting the anal and caudal fin, are seen together, with traces of those belonging to the dorsal.

In connection with the anal fin, I counted at least 15 of these bones, which are very strong in front, the anterior one measuring 11 lines in length. They become smaller posteriorly, but all are expanded at their extremities, these forming apparently a continuous line, to which the fin rays are articulated.

In the tail may be seen, in the same specimen, two sets of ossicles, an upper supporting the large azygos scales of the body prolongation, these being probably of the nature of neurapophyses; and a lower set supporting the rays of the caudal fin, and which may be regarded as the interspinous bones, or those in conjunction with the inferior spinous processes of the caudal vertebræ. These caudal interspinous bones are stout anteriorly, but become very small behind, where they assume a distinct hour-glass shape, being expanded at each extremity, and narrow in the middle, and where it may also be beautifully seen how each interspinous bone supports several fin rays. This must necessarily be the case in all the fins, seeing that the number of rays very considerably exceeds that of their supporting ossicles.

Head.—Figs. 2, 3, and 4 represent various views of the head of the specimen already alluded to in the description of the interspinous bones and of the caudal rays.

Fig. 2 is a profile view of the head, in which the eye is at once struck by the strength of the jaws, the extent of the gape, the very anterior position of the orbit, and the projection of the snout.

Fig. 3 is a view of the bones on the top of the cranium, and fig. 4 represents the snout seen from the front.

Referring to fig. 2, which in fact represents a *cast of the inner surface* of the cranial buckler, the bone having splintered off along with the matrix, leaving the sutures standing up as prominent lines, the lines radiating from the ossific centres being also well displayed, we find that some of the numerous bones exposed are easily enough recognised. Posteriorly, we have the parietals (7) very short, and nearly square shaped, articulating with each other in the middle line, as in *Lepidosteus* and *Polypterus*, there being no supraoccipital interposed as in most *Telostei*. In advance of them, and forming a large part of the vault of the cranium, we have the frontals (11). From the frontals we pass on to the single nasal bone (15), which forms the prominent point of the snout above the mouth. Opposite this prominence the nasal bone is notched on each side for the olfactory opening, and from the prominence itself pass beautifully radiating lines over the whole surface of the bone. On each side of the nasal bone is situated the pre-frontal (14, figs. 3 and 4,) which completes the olfactory notch of the former bone into a nearly round opening, It articulates internally with the nasal, below with the intermaxillary, and behind with the bone to be next described as post-frontal. Externally, it forms the anterior part of the orbital margin of the cranial shield.

The bones (12) behind the pre-frontals, and forming the posterior part of the orbital margin of the cranial shield, I can imagine to be nothing else than the post-frontals, though they are certainly situated rather far forwards. They articulate in front with the pre-frontals, behind with the mastoids, internally with the principal frontals, which they thus completely exclude from the orbital margin.

The *mastoid* (8) is very distinct, forming an elongated plate external to the parietals, and articulating with them, and with the frontal and post-frontal bones. Externally it is in contact with the facial bones; and close beneath its anterior part may be seen the attachment of the suspensory apparatus of the lower jaw. Behind it forms, along with the parietal, the posterior margin of the cranial buckler, and is succeeded in that direction by the suprascapular (50, fig. 2).

Face.—Recurring to the profile view of the head, fig. 2, we observe that the *superior maxillary* bone (21) is of great strength. Posteriorly it is very broad, and overlaps the lower jaw near its articulation. Anteriorly it becomes, in the orbital region, suddenly narrowed to a somewhat slender point, because of its upper border being suddenly beveled off to form a curved margin, along which the suborbital bone (73) articulates. The narrow extremity proceeds forwards below the orbit to come in contact with the intermaxillary, and the external surface is seen in fig. 1 to be ornamented with delicate striæ, which run parallel with the upper and hinder border of the bone. The posterior-inferior angle, overlapping the articulation of the lower jaw, is covered, however, with a minute tuberculation.

The *intermaxillary* (22) is a little bone bearing teeth, placed at the front of the orbit, and beneath the snout and nasal openings. It is in connection above with the pre-frontal; internally and above with the nasal, behind with the superior maxillary; whether it articulates also with its fellow of the opposite side is not seen distinctly. Externally it continues the orbital margin of the cranial shield downwards and somewhat backwards, to which shield it is immovably articulated.

The *lower jaw* (L, J) is stout and curved, the curve having the convexity downwards. Of how many pieces it is composed it is impossible to determine, though there are evidently at least two, viz.—dentary and articular. Its outer surface is covered with a beautiful minute tuberculation, seen in fig. 1.

Teeth may be observed on the superior maxillary and intermaxillary bones, and also on the lower jaw. They are of two descriptions, large ones of about two lines in length, alternating with smaller ones, of about half that size. These teeth are sharply pointed, and of a slender conical form; their external surface is shining and smooth, their transverse section round, and the pulp cavity large. Fig. 7 represents a portion of the edge of a lower jaw, with one large and several smaller teeth attached.

Opercular apparatus.—The *operculum* is an oblong, and somewhat rhomboidal plate, its anterior, superior, and posterior inferior angles being rather acute. Its upper margin is situated close beneath the suprascapular and mastoid bones, and its long axis is directed at a very considerable angle downwards and *back-*

wards, by reason of the wide extent of the gape, and the consequent very posterior position of the articular end of the lower jaw.

The *suboperculum* (36) is nearly square, but its posterior margin is longer than the anterior one, which though also directed somewhat backwards, is not so much so as the anterior margin of the previous bone, with which it consequently forms a slight angle.

I have looked in vain for any very distinct representatives of the *preoperculum* and *interoperculum* of other fishes. On referring to fig. 2, it will be seen that, in front of the operculum, follows the triangular plate (*g*) covering the cheek, a small portion of the suspensory apparatus of the lower jaw being exposed between them above; while in front of the suboperculum follows the hinder border of the upper jaw. If present, the bones in question must then be reduced to very narrow laminæ, so as to render their recognition very difficult, at least in the state of preservation in which these fossils usually occur.

Hyoid bone and branchiostegal rays.—What I consider to be the posterior part of the body of the *hyoid*, may be seen in fig, 1, *h*, with four of the branchiostegal plates behind it. The latter are also shown in fig. 2, the lower ones being broken off behind, so as to expose the underlying coracoid (52). None of my specimens show the branchiostegal rays in complete series, so that their number cannot be exactly ascertained. In one example I counted at least twelve; and there can be no doubt that there were a great many more, probably one-half as many more again. They take the form of oblong imbricating plates, whose posterior margin is broader than the anterior one. One of the upper plates in fig. 2. measures 9 lines in length, and 5 lines in breadth, at the hinder border.

In front of the operculum, and above the posterior part of the superior maxillary bone, is a triangular plate (*g*) with the acutely-pointed apex directed backwards in the angle between the two bones just named. Between it and the operculum above may be seen, as already remarked, a portion of the suspensorium of the lower jaw exposed. This plate may be a member of the "Gesichtspanzer," representing the cuirass of small plates behind the eye and in front of the operculum of *Lepidosteus*. It will also be seen to have exactly the same relative position to the other bones of the head, as the bone covering the cheek and masticatory muscles in *Polypterus*, and which by Agassiz and Müller is reckoned as "preoperculum;" by Huxley as "supratemporal."

In front of this plate is a sickle-shaped suborbital bone (73), surrounding the posterior and lower margin of the orbit, and likewise fitting on to the lunated anterior-superior part of the superior maxillary bone. There are doubtful indications of another, just behind the upper margin; the rest of the chain are crushed inwards, and concealed beneath the ironstone matrix. On examining some other heads, however, it seems probable that the eye was completely surrounded, as in *Lepidosteus*, by a chain of such narrow ossicles.

Shoulder-girdle.—Above the operculum, and behind the mastoid and parietal bones, may be seen a well-marked *suprascapular* (50). Between it and the hinder edge of the cranial shield may be seen faint traces of what I should consider a supratemporal ossicle. The suprascapular is followed by a flat and elongated *scapula* (51), which reaches down, at least, to the middle of the hinder edge of the suboperculum. It is in its turn followed by a large *coracoid* (52), its lower extremity being seen in fig. 2, exposed by the breaking off of some of the branchiostegal plates. This bone, the upper and larger part of which is flattened laterally with a convex posterior margin, forms below, near the origin of the pectoral fin, a prominent angle, being suddenly bent on itself inwards towards the middle line of the throat. Succeeding the lower part of the coracoid in front is a triangular plate (*f*), with the acutely-pointed apex directed forwards, and folded on itself laterally, so as to present both a lateral and an inferior or jugular surface. This is certainly the equivalent of the plate succeeding the lower end of the coracoid in *Polypterus*, and which is also seen in many of the *Crossopterygidæ* of the Old Red Sandstone. It is, in the specimen from which fig. 2 was taken, partly exposed by the breaking away of some of the branchiostegal plates.

These are, then, all the facts that I have been able to elucidate with any certainty regarding the structure of *Pygopterus Greenockii.* Before, however, proceeding to any comparison with the recent Ganoids, let us examine a little into the conformation, as far as can be made out, of the head in certain other upper Palæozoic genera, viz., *Amblypterus* and *Palæoniscus.*

I have not been able to obtain any further description of the bones of the head in *Amblypterus* and *Palæoniscus* beyond that of Agassiz in the second volume of the "Poissons Fossiles." The points to which attention is there directed are:—The projection of the snout in front of the mouth by reason of an expansion of the frontal and nasal bones (ethmoïde); the great extent of the gape; the strength of the jaws, which are furnished with teeth "*en brosse;*" that the orbit is bounded below by a series of suborbitals; that the branchiostegal rays form a series of flattened plates between the two halves of the lower jaw. The opercular apparatus is described as being formed of the usual four pieces, of which the operculum is largest. Of the scapular arch three bones are mentioned, viz., the suprascapular, scapular, and coracoid (*humerus*). The nasal projection is described as wanting in *Amblypterus.*

Not having been myself able to notice any anatomical difference between heads of *Amblypterus* and *Palæoniscus* in such species as have come under my notice, and as my knowledge is principally derived from specimens of *Amblypterus* from the shales of Wardie, I will assume that genus as the type, believing, however, that all statements regarding its general cranial structure hold equally good with regard to *Palæoniscus.* After having examined a great number of specimens of *Amblypterus** from the above-named locality, I have, in figs. 10 and 11, given a

* Principally *A. punctatus* (Agassiz).

sketch of the various bones which I have been able to distinguish. On looking at fig. 10, the first thing which strikes the eye is the nearly exact resemblance presented by the outlines of the bones to those in *Pygopterus*, if we take the above described *P. Greenockii* as an example of the genus. Of the bones of the shoulder-girdle, the suprascapular (50) is readily distinguished as a triangular, somewhat convex, plate at the hinder part of the skull above the operculum. To this succeeds the elongated scapula (51), and then the coracoid (52), which, as in *Pygopterus,* forms an angle at the pectoral fin where it bends inwards on the under surface of the throat towards its fellow of the opposite side. We have also the triangular pre-coracoid plate, which, meeting in the middle line with its fellow of the opposite side, forms with it a pointed process, passing forwards the two lateral series of branchiostegal plates (44).

The opercular apparatus is conformed as in *Pygopterus Greenockii*, the operculum (35) being narrow and of an oblong rhomboidal shape, the suboperculum (36) nearly square; while the same difficulty is experienced in finding a distinct pre- and sub-operculum, so that those bones, if present, must have been very narrow. The *superior maxillary* bone (21), though not so strong as in *Pygopterus*, has essentially the same form, consisting of a stout oblong plate, its posterior margin sloping downwards and backwards, and its superior margin beveled off in a semilunar manner below and behind the orbit. The *lower jaw* (L, J) is stout, and beautifully tapering from its posterior articular extremity towards the symphysis; the gape extends very far back; the teeth in both jaws are minute, and only to be studied by means of a lens; then they are seen to be smooth, and of an acutely-pointed slender-conical form. The *branchiostegal plates* (44, fig. 11) are fourteen on each side, with an azygos one in the middle, immediately behind the symphysis of the lower jaw. The azygos plate, and the anterior one of the lateral series on each side, are of a rhomboidal form; the rest are more narrow, oblong plates, with the hinder margin rather broader than the front. They seem also to be slightly shorter in the middle than at each end of the series. At the lower and posterior border of the orbit may always be seen a sickle-shaped *suborbital* (73), and the examination of some specimens, a little better preserved than others, leads me to the conclusion that the eye was completely encircled by a chain of similar ossicles, but which, however, from the crushing to which the heads have been subjected, cannot be counted or definitely sketched. Behind the orbit, and above the upper jaw-bone, are also one or more plates covering the cheek.

Owing to the crushed state in which the heads of the smaller fishes occur, I have no specimen in my collection which shows the bones of the cranial shield so beautifully as they are displayed in the head of *Pygopterus,* figured Plate XLV., figs. 2, 3, 4. The examination of several specimens in a better or worse state of preservation shows, however, that their cranial bones were essentially similar to those of the last-named fish, the whole cranial shield being apparently a little broader and shorter proportionately. The square-shaped *parietals*, the more elongated *frontals*, the *mastoids*, the *post-frontals* forming the posterior, and the *pre-frontals* the anterior part of the orbital margins of the shield, may all be distinctly recognised. The same line passes along the parietal and frontal on each side which I have noticed in *Pygopterus*. The nasal bone certainly projects forward over the mouth, but is invariably so crushed as to render it almost impossible to tell with any certainty if it be a single bone, as in *Pygopterus*, or double, as in *Lepidosteus* and *Polypterus*. For my part, I should incline to the former opinion.

The head may now be seen to be very similarly constructed in *Pygopterus* and in *Amblypterus*. On comparing the head of *Pygopterus* with that in the recent *Lepidosteus* and *Polypterus*, it will be seen that the gape is still wider, and the articulation of the lower jaw still further back than in the latter genus, so that the suspensory and opercular apparatus are directed obliquely downwards and backwards.

In its relation to the superior maxillary bone, the orbit is still more anteriorly situated than in *Polypterus*, being placed over the anterior part of the upper jaw, whereas in *Lepidosteus* the projection of the snout and the length of the suspensorium carry both jaws forward altogether in front of the eye. The superior maxillary bone is simple, as in *Polypterus*—not divided into a number of pieces, as in *Lepidosteus*. As in both *Lepidosteus* and *Polypterus*, the intermaxillary bone is immovably articulated to the front of the cranium, only a limited amount of motion being allowed to the superior maxillary. There are a greater number of separate bones exposed on the top of the cranium than in *Polypterus*; but the nasal bone is single, thus differing from the nasal both in *Polypterus* and *Lepidosteus*, which is double. The post-frontal is seen on the surface of the skull, but again it is doubtful whether any representatives of the supra or paroccipitals take part, as in *Lepidosteus*, in the formation of the cranial shield. The branchiostegal membrane was strengthened by rays instead of by two "jugular" plates, as in *Polypterus*, but again these rays differ from those of *Lepidosteus* in being many in number, and enamelled on their surfaces.

The bones of the shoulder-girdle are the same as those in *Polypterus*. Lastly, the head of *Pygopterus* differs from that of *Polypterus* in the absence of the spiracle, and the row of ossicles associated therewith. The only undoubted members of the system of superficial facial bones are the suborbitals and the plate covering the cheek; the presence of supratemporals is doubtful; and I have seen no trace of the "*os mobile du nez*," though this, judging from its small size in *Polypterus*, may readily become indistinguishable in a fossil specimen.

Turning now to *Eurynotus* (AG.), a genus not uncommon in the Scottish Carboniferous strata, but rare elsewhere, we find that it has many points of resemblance to the two fossil genera already noticed, but also very many of decided difference.

In his "Poissons Fossiles," AGASSIZ has said very little about the bones of the head of *Eurynotus*, merely noticing the narrow shape of the operculum and sub-operculum, and the proportionally great height of the latter, also observing a triangular plate which he considered as a suborbital bone; lastly, that the teeth were small and obtuse. I regret that my knowledge of the osteology of the head of this fish is very imperfect; what I do know of it is derived from specimens of *Eurynotus fimbriatus* from Wardie, and in figs. 8 and 9 I have sketched the forms of the various bones I have been able to make out.

Regarding the *cranium proper* I can say nothing, the specimens being too much crushed.

The *operculum* is very small in proportion to the size of the fish, and is of a short oblong form, with the posterior-superior and anterior-inferior angles somewhat rounded off. The *suboperculum* is larger, but also oblong in form; its upper margin is concave; the posterior-inferior angle is very much rounded off; a diagonal line from the posterior-superior to the anterior-inferior angle divides the external surface into two areas; and of these the upper one is ornamented by ridges radiating from the anterior-inferior angle of the bone, while the lower area is marked by concentric ridges running parallel with the rounded posterior-inferior margin. In front of the two plates just described is another elongated one which certainly seems to represent a preoperculum. The superior maxillary bone (22, figs. 8 and 12) is most decidedly different from that in *Amblypterus* and *Pygopterus*, consisting of an elongated triangular plate with the apex directed forwards. The lower margin, which is the longest, is seen to be garnished along the whole of its inner edge by small rounded polished teeth, which resemble nothing so much as small and somewhat flattened grains of shot. They are placed irregularly together, the largest ones being about the middle of the series; about one-third from the anterior extremity of the bone they suddenly become excessively minute, and at the same time are not placed so close together. Some are observed to be rather narrowed at the base, but this is not universal. The border next in length is the upper, which slopes towards the apex in front. The short side is posterior, and fits on in front of the plate which I have considered the preoperculum. The external surface of the superior maxillary bone is sculptured by coarse ridges which run parallel to the posterior and upper margins.

Lying on the same piece of stone with the superior maxillary bone, from which fig. 12 has been taken, is a flat irregularly shaped bone (fig. 13), which must appertain to the *palate*. It is undefined at one extremity, and passes into two irregular processes at the other. A large portion of its surface is completely covered by the rounded teeth already described, and which are indeed so thickly placed together, that in many instances their bases assume a polygonal form. The part of the bone bearing the teeth is marked by three longitudinal ridges bounding two shallow furrows. Two of those ridges form the margins of the tooth-bearing part, and of those two one also coincides with the edge of the entire bone; the third passes along midway between them, and carries the largest teeth.

The *lower jaw* (fig. 8, L, J) is moderately stout, and between its two halves are seen branchiostegal plates exactly resembling those of *Amblypterus*. There is, namely, a median lozenge-shaped plate behind the symphysis of the jaw, and the first lateral one on each side is nearly regularly rhomboidal, being very much broader than those which succeed it posteriorly. The number of these plates I have not ascertained; in one example I counted at least 10, but there must be many more.

Of the *pectoral arch*, the only bones to be seen with certainty are the *coracoid* and the plate succeeding it below and in front, which latter is proportionally larger than in *Amblypterus*. The coracoid resembles that of *Amblypterus* in general shape; but its upper part is apparently rather slender, whereas the lower reflected part is very broad. Traces of a *scapula* are seen in one specimen, and also between and behind the scapula and coracoid a semilunar ossicle resembling one of the plates found in a similar situation in *Polypterus*.

On comparing these few facts regarding the head of *Eurynotus* with those more completely elucidated in *Pygopterus* and *Amblypterus*, we see that the first-named genus decidedly differs in the form of the opercular apparatus, and of the superior maxillary bone, in the shape and arrangement of the teeth, and in the smaller extent of the gape, so that the suspensory and opercular bones do not require to be directed so very much backwards towards the articulation of the lower jaw.

On the other hand, *Eurynotus* agrees with *Amblypterus* in the form and arrangement of the branchiostegal rays, in the form of the scales, and in the structure of the fins* and tail, the latter being typically heterocercal, defended along its upper border by a row of V-shaped scales, while the sides of the vertebral prolongation are covered by elongated ones of a lozenge shape. The fins were all furnished with large fulcral scales along their anterior margins, which, in the dorsal fin of a specimen in the museum at St Andrews, may be seen to be arranged in a *double series*. This double arrangement of the fin-fulcra is also recorded of *Palæoniscus* and *Acrolepis* by Müller ("Ganoiden," p. 152).

Agreeing with Müller that the division between the "*Lepidoids*" and "*Sauroids*," the two families in which *Amblypterus* and *Pygopterus* have been placed respectively by Agassiz, is artificial, we must class those two genera and their immediate allies (*Palæoniscus*, *Catopterus*, *Acrolepis*, &c.) in one family of *Palæoniscidæ*, as has been done by Vogt ("Zoologische Briefe," s. 133). Then, accepting meanwhile the great divisions of the Ganoid order proposed by Huxley ("Memoirs of Geol. Survey, Decade X.," 1863), these fishes must come in under the sub-order *Lepidosteidæ*, characterised by the possession of non-lobate paired fins, rhomboidal scales, and branchiostegal rays; and under the family *Lepidotini*,

* Except in the large size and peculiar form of the dorsal fin in *Eurynotus*.

in which the superior maxilla is formed of one piece, and the branchiostegal rays are many and enamelled. Here they must form a distinct sub-family of *Palæoniscidæ*, equivalent to the "*Lepidoidei heterocerci*" catalogued by Sir PHILIP EGERTON in the "Quart. Jour. of Geol. Society," (vol. vi. 1850), but with the addition of *Pygopterus*, *Acrolepis*, and some other allied genera, generally classed as "*Sauroids*," the family being characterised by their wide gape, many rayed fulcrated fins, and by their completely heterocercal tail, the upper border of which is set with a row of imbricating V-shaped scales.

But the last question is, whether *Eurynotus* is also to be considered a member of the *Palæoniscidæ*, or to be transferred to the *Pycnodonts*, which it resembles in the rounded crushing palatal and other teeth. This question has already been discussed by Sir PHILIP EGERTON in the paper above referred to; and the conclusion to which he there arrives, taking the form and structure of the scales and fins into account, is, that *Eurynotus* ought to retain its place among the "*Lepidoidei heterocerci*." But in a paper very recently read before the Geological Society of London, Dr J. YOUNG has removed it from its old associates, and placed it, together with *Platysomus*, the *Pycnodonts*, and two new genera, *Amphicentrum* (YOUNG), and *Mesolepis* (YOUNG), in his family of *Lepidopleuridæ*. I regret that my knowledge of the last-named fishes is not sufficient to enable me to venture an independent opinion as to the relations to them of *Eurynotus*. As regards the configuration of the scales, the structure and general form of the fins, except in the case of the dorsal, *Eurynotus* certainly bears a very strong resemblance to *Amblypterus* and *Palæoniscus*. But the dentition and the shape of some of the bones of the head distinguish it so much from the two last-named genera, that it is not impossible, that, when we come to know more about the cranial structure of *Eurynotus* and that of the apparently allied genus *Mesolepis* (YOUNG), Dr YOUNG's ideas will be found to be substantially correct.

Explanation of Plate XLV.

The various bones are designated and numbered according to the nomenclature used by Professor OWEN in his "Lectures on Comparative Anatomy."

Fig. 1. *Pygopterus Greenockii* (Agassiz), one-half natural size.
D, Dorsal fin; C, Caudal; A, Anal; V, Ventral.

Fig. 2. Head of another specimen seen from the side, diminished one-third.
L, J, Lower jaw; N, Nasal opening; *f*, Pre-coracoid plate; *g*, Triangular cheek-plate.
15, Nasal bone; 21, Superior maxillary; 22, Pre-maxillary; 35, Operculum; 36, Suboperculum; 44, Branchiostegal rays or plates; 50, Suprascapular; 51, Scapular; 52, Coracoid; 73, Suborbital.

Fig. 3. Upper surface of the cranial buckler of the same specimen represented in fig. 2 natural size.

N, N, Nasal openings.

7, 7, Parietal bones; 8, 8, Mastoids; 11, 11, Frontals; 12, 12, Post-frontals; 14, 14, Pre-frontals; 15, Nasal.

Fig. 4. Front view of snout of same specimen.

N, N, Nasal openings.

14, 14, Pre-frontals; 15, Nasal; 22, 22, Pre-maxillaries.

Fig. 5. Scales of *Pygopterus Greenockii.*

A, Scale from the anterior part of the flank of the fish magnified two diameters, showing concentric lines of growth, the external layer of ganoine having scaled off.

B, Outline of a scale from the hinder part of the flank.

C, Scale from anterior part of the flank, magnified three diameters, and showing part of the external striated layer of ganoine.

Fig. 6. Exposed surfaces of scales from posterior part of flank, magnified two diameters. On one of those the external striation is represented.

Fig. 7. Portion of edge of lower jaw, showing two sizes of teeth.

Fig. 8. Diagram of side of head of *Eurynotus.* The numbers apply to the same bones as in fig. 2.

Fig. 9. Diagram of arrangement of branchiostegal rays in *Eurynotus.*

Fig. 10. Diagram of side of head in *Amblypterus.*

Fig. 11. Diagram of head of *Amblypterus,* seen obliquely from the side, and from below.

Fig. 12. Superior maxillary bone of *Eurynotus,* seen from the inside; natural size. From South Queensferry.

Fig. 13. Palate (?) bone of *Eurynotus,* with crushing teeth. From the same locality.

XLVIII.—*On the Physiological Action of the Calabar Bean* (Physostigma venenosum, *Balf.*). By Thomas R. Fraser, M.D., Assistant to the Professor of Materia Medica in the University of Edinburgh. Communicated by Professor Christison, M.D., D.C.L., V.P.R.S.E.

(Read 17th December 1866.)

In 1855, the Professor of Materia Medica in the University of Edinburgh, in a paper read before this Society, directed the attention of physiologists to some of the remarkable properties of the Calabar bean.* In 1862, I presented a graduation thesis to the University of Edinburgh on the "Characters, Actions and Therapeutic Uses of the Ordeal Bean of Calabar." The principal results I had obtained at that time were that this substance causes death by either syncope or asphyxia, the latter being due to an effect on the spinal cord and on the respiratory centres; that the symptoms resemble those of cardiac or pulmonary embarrassment, according to the quantity of the poison administered, and to its rate of absorption; and, also, that the topical application of this agent to the eyeball, or to its neighbourhood, produces a marked and rapid contraction of the pupil and various disturbances of vision.† Since then, and more especially because of the peculiarity of the last of these conclusions, a lively interest has been taken in this substance. Its actions on the eye have been investigated by nearly all the leading ophthalmologists of Europe and of America, and its general physiology has occupied the attention of many distinguished students of biology. Nor have these labours been barren of practical results. Ophthalmic medicine has adopted this agent as one of its important remedies, and there can be little doubt that general medical practice will soon include in its Pharmacopœia a drug of so great energy.‡

The present investigation was undertaken for the purpose of extending and supporting my previous results, with some of which subsequent observers have disagreed; but I purpose to take an opportunity of examining these discrepancies with some detail in a different place. The effects which follow the topical application to the eyeball will be merely alluded to in this paper, as this portion of the subject has not been completed. Enough has, however, been done to convince

* Proceedings of the Royal Society of Edinburgh, vol. iii. p. 280; and Monthly Medical Journal, vol. xx., 1855.

† Edinburgh Medical Journal, 1863, and pamphlet.

‡ Since this sentence was written the *Physostigmatis Faba* has been admitted into the edition of the "British Pharmacopœia," published in 1867.

me of the insufficiency of the views hitherto advanced, and to suggest the advisability of extending my observations.

Preparations.

In 1863, I separated from the kernel, from the spermoderm of the bean, and, also, soon after, from the excrement of a lepidopterous insect which feeds on the kernel,* an amorphous active principle, possessing the general properties of a vegetable alkaloid, for which I proposed the name Eserinia, derived from Eserĕ, the usual name of this ordeal-poison at Calabar; and with it a few experiments were made, some of which have been published. Shortly afterwards, I succeeded in obtaining this alkaloid in, apparently, a state of greater purity, and as a crystalline substance, to which I gave the name Eseria. A crystalline acid, having a similarity to, and being probably identical with, tartaric acid, was also obtained from the kernel at that time. In the present investigation, however, an extract, prepared by acting on the finely pulverised kernel with boiling alcohol (85 per cent.), has been used. This preparation contains a considerable proportion of fatty matter, which prevents its complete solution in water; and, as the division into separate doses of a mere watery suspension would lead to many inaccuracies, it was found necessary to weigh the requisite quantity, separately, for the majority of the experiments. This extract is hygroscopic, which further required that it should be dried and kept in an exsiccator in order to ensure an unvarying preparation.†

Subjects of Experiment and Comparative Effects of Doses.

With few exceptions, the experiments were made with the common frog (*Rana temporaria*), birds, and various mammals. It was found that fatal results were produced with the smallest quantity on birds; and that the largest doses, in proportion to weight, were required by amphibia. A dose of one-sixteenth of a grain proved rapidly fatal to a pigeon weighing nine ounces and three-quarters; whereas a frog, which weighed 726 grains, has recovered from three grains of extract—a quantity sufficient to produce death in a dog of average size.

A. ACTION THROUGH THE BLOOD.

As I have already, in a previous paper, described with considerable detail, the general symptoms which follow the administration of physostigma, it will be unnecessary to give them here. It has also been shown, on the same occasion, that the more rapid the absorption of the poison the more quickly are fatal effects produced, and that the active principle may be absorbed by any living

* On the Moth of the Esere, or Ordeal Bean of Old Calabar. The Annals and Magazine of Natural History, May 1864, pp. 389–393.

† The varying potency of an extract possessing the property of absorbing moisture may unfit it for therapeutic purposes, but the tincture I have already recommended (*op. cit.* sect. iii.) will prove a sufficient substitute, and it has the great advantage of constancy of strength.

tissue. From the following experiment it is proved that prolonged digestion with gastric juice does not impair the energy of Calabar bean:—

Experiment I.

A gastric fistula was formed in a healthy dog, and, some days afterwards, and while the animal was in good health, 500 grains of gastric juice were withdrawn from the stomach. Four hundred grains of this were mixed with half a grain of extract of physostigma, received in a flask with an arrangement to impede evaporation, and placed in a water-oven at a temperature of 98° F. The digestion was continued for twenty-four hours, when the fluid was placed in a capsule and evaporated at 85° F. The resulting extract was finely pulverised, heated with alcohol of 85 per cent., filtered, and again evaporated to dryness. Contact with distilled water removed an acid fluid, which was made alkaline by excess of magnesia, and agitated in a bottle with chloroform. The chloroformic solution was removed by a separating funnel and evaporated, and the resulting brown extract was suspended in distilled water. A drop of this was applied to the conjunctiva over the right eyeball of a rabbit, whose pupil, before the experiment, measured $\frac{13}{50}$ths × $\frac{15}{50}$ths of an inch. In eight minutes, the pupil was $\frac{8}{50}$ths × $\frac{12}{50}$ths; in fifteen minutes, $\frac{6}{50}$ths × $\frac{8}{50}$ths; in twenty minutes, $\frac{4}{50}$ths × $\frac{5}{50}$ths, and it continued in this contracted condition for many hours. The remainder of the fluid was injected under the skin of a young pigeon, and caused its death in eight minutes.

Several small pieces of hard-boiled white of egg were placed in a flask with the remaining 100 grains of gastric juice, and digested under exactly the same conditions, and at the same time, as the extract of physostigma. They were found to be completely dissolved in less than ten hours. There could, therefore, be no doubt as to the activity of the gastric juice which had been employed.

This merely confirms the result before obtained, of fatal effects following the introduction of the poison by the digestive system.

I believe that Brinton first demonstrated that a poison which had been administered by the blood may be excreted by the stomach and intestines.* This was proved with tartar emetic; and, more recently, Taylor has published evidence showing that arsenic also may appear in the stomach although it had not been administered by the alimentary canal.† I took an opportunity of examining if a similar event occurs in poisoning with physostigma.

Experiment II.

Five grains of extract, suspended in water, were injected into the right jugular vein of a dog, and caused the death of the animal in eleven minutes. The stomach was immediately removed, and its contents, along with some of its mucous coat, obtained by scraping, were partially dried at a low temperature, and then boiled with successive portions of spirit (85 per cent.) acidulated with tartaric acid. The tincture was concentrated by distillation, and then evaporated to dryness. The extract was treated with distilled water, filtered, and agitated with ether until the fatty matters were removed. The remaining watery solution was made alkaline by the addition of carbonate of sodium and shaken with ether; and the ethereal solution was distilled. A yellowish, alkaline, amorphous residue was obtained, weighing three-fifths of a grain, and having a disagreeable animal odour. A minute portion of this extract was mixed with two

* Cyclopædia of Anatomy, article "Stomach;" Lancet, 1853, vol. ii. p. 599; and Lectures on the Diseases of the Stomach, 2d edit., 1864, p. 54.

† Guy's Hospital Reports, vol. vi. p. 397.

drops of distilled water and applied to the conjunctiva of a white rabbit, in the presence of my friend, Dr CRUM BROWN. Before the application, both pupils had a diameter of $\frac{13}{50}$ths of an inch, in a full light. At first a little irritation was caused. In thirteen minutes, the pupil had contracted to $\frac{5}{50}$ths, and in eighteen minutes to $\frac{3}{50}$ths; the other pupil still remaining at its original diameter of $\frac{13}{50}$ths. This extreme contraction continued for upwards of an hour; but in two hours the pupil was $\frac{7}{50}$ths, and by the following morning it had resumed its original diameter.

It would, therefore, appear that physostigma, when administered by a vein, finds its way into the stomach—a method of poison-excretion which has been established in the cases of antimony and arsenic.

Although this investigation has for its principal aim the determination of the exact method in which physostigma acts, and the demonstration, as far as possible, of the histological structures which it influences, it may be necessary to describe, at this place, the general symptoms which follow the administration of a poisonous dose. In the case of mammals, I have already entered fully into this subject in a previous paper, from which I extract the following descriptions:*—

"When a *small* fatal dose is administered to one of the lower animals, a train of symptoms is produced usually in the following order:—A slight tremor is first seen, especially at the posterior regions, and this extends forwards to the anterior extremities and the head. The limbs yield immediately afterwards, the posterior becoming generally first paralysed, and the animal lies extended in a state of almost complete muscular flaccidity. A few attempts may be made to recover the normal position, but they are usually ineffectual. The bowels, in most cases, are evacuated, and urine is passed. The pupils generally *contract;* as the symptoms advance, the respiration becomes slow and irregular, with a distinct stertor accompanying both inspiration and expiration, and frothy mucus escapes from the mouth. Muscular twitches occur, and often continue after respiration has ceased. Reflex action cannot be produced by either pinching or pricking the skin. By-and-by the eyelids do not contract when touched or even when the eyeball is pricked. On lifting by the ears, the limbs hang inertly, and the only sign of life is an occasional gasping inspiration, which also soon ceases, and the animal appears dead.

"Consciousness is preserved during the whole time, until the power of expression is lost. During incomplete paralysis, proofs of sensation may be obtained by pinching the ears or pricking the skin. Immediately after death the pupils dilate.

"On opening the body the various muscles which are cut contract. The diaphragm and muscles of the extremities may be excited to action by pinching the phrenic and sciatic nerves, and the contractility of the muscles generally is retained for some time after death. The heart is found acting regularly, and the intestines exhibit distinct vermicular action. The heart may continue its action

* *Op. cit.* sect. ii.

for one hour and a-half after death. Its chambers usually cease to contract in a definite order, the left auricle first losing its spontaneous action, then the right and left ventricles, and, after an interval, the right auricle. The large veins in the thorax are found distended. . . . The lungs are engorged—in two experiments this had proceeded to such an extent that detached portions sank in water. . . .

"When a *large* fatal dose of the kernel is administered, the hind limbs almost immediately yield, and the animal falls. It lies flaccid, and in any position, on the table, and exhibits muscular power only by a few twitches. The pupils contract; in a few cases fluid escapes from the nostrils and mouth, and the lachrymal secretion is increased. Reflex action cannot be produced by irritation, and the respirations, after a few gasps, cease.

"The pupils dilate immediately after death. On opening the body, muscular twitches occur. . . . The heart is found distended and passive; irritation, however, produces contraction for about ten minutes after death. The vermicular action of the intestines is very much diminished, and can scarcely be observed. . . . The mesenteric arteries and veins may be readily distinguished by the colours of their contents."

The following will serve to illustrate the symptoms with frogs:—

Experiment III.

Three grains of extract of physostigma, suspended in twenty minims of distilled water, were injected, by WOOD'S syringe, into the subcutaneous cellular tissue at the back of a light-coloured frog, weighing 430 grains. For four minutes it appeared perfectly unaffected and jumped about normally; after which time some increase occurred in the respiratory irregularity which is always found in frogs. In seven minutes, the respiratory movements of the chest had ceased; but those of the throat continued for other four minutes (eleven after injection). About this time, the movements of the animal were sluggish; the fore legs gradually began to separate until they no longer supported the chest and head; and the posterior extremities were affected in a like manner, and soon after lay extended and flaccid. Weak voluntary movements, however, continued until fifteen minutes after the exhibition of the poison; and, for some time after this, irritation demonstrated the continuance of reflex power. In half an hour, the skin of the frog had undergone a marked change, having become of a dark brown colour. Although now apparently dead, it was not, in a strict physiological sense, really so. Motor nerve-conductivity was retained for many minutes longer; the diastaltic function was not abolished, and, hence, it was possible to show that afferent nerve-conductivity also continued; and the muscular tissue, for many hours, contracted when stimulated, and, in the case of some of the heart chambers, spontaneously and successively did so, for a shorter period.

Into all these, and many other points, it is necessary to enter with detail, and several of them may be overtaken in a somewhat connected manner by examining the cause of what is the most prominent, as well as one of the earliest, of the phenomena described. This is obviously the condition of gradually increasing paralysis.

Action on the Voluntary Muscles.

The peculiar successive tremors, which are observed in warm-blooded animals, at first sight suggest that the paralysis caused by Calabar bean is due to an affection of the muscular system; and the condition of general flaccidity, which so rapidly follows its administration to frogs, appears to favour, as it certainly does not contradict, this opinion. Without pretending that such was the order followed in this investigation, it will, as a matter of convenience, be advisable to examine, in the first place, the effects which are produced on voluntary muscles.

Experiment IV.

A full-grown active rabbit had injected into the subcutaneous tissue of its right flank, three grains of extract, suspended in eleven minims of distilled water. Tremors occurred in two minutes; the anterior extremities soon after yielded; and, in four minutes and thirty seconds, the animal fell, the muscular trembling having increased in vigour and having become general over the body. Respiration ceased in five minutes after the injection, but muscular tremors continued during other three minutes. When the thorax was opened the heart was found dilated and passive. Twenty-four minutes after the administration, galvanic stimulation of the sciatic nerves caused powerful muscular contractions; within thirty-six minutes, these nerves were completely paralysed, though application of the electrodes to any of the voluntary muscles produced marked contractions. These contractions became gradually weaker, but could be distinctly excited until one hour and thirteen minutes after the poison had been exhibited.

The general result in all the other experiments which were performed on warm-blooded animals was the same. Muscular contractility remained after destruction of the function of motor nerves; and this also occurred, in even a more marked manner, with frogs.

Experiment V.—(*Temperature of Laboratory about* 53° *F.*)

By means of Wood's syringe, I injected three grains of extract, in fifteen minims of distilled water, into the lower portion of the abdominal cavity of a frog which weighed 473 grains. The usual phenomena quickly occurred. In sixteen minutes, the sciatic nerve and the neighbouring muscles of the left thigh were exposed and found active.

The muscles were now of a very blue colour, quite distinguishable from their normal appearance; and this colour change was discovered in the serous and fibro-serous tissues also. In about four hours, motor nerve-conductivity was universally destroyed. The heart contracted rhythmically, and at a very reduced rate, until twenty-six hours after the administration, after which, the auricles contracted more frequently than the ventricles, and continued to do so until the heart's action ceased, seventy-three hours after the poison was injected; and, by microscopic examination of the web, it was found that a more or less feeble circulation was all this time maintained. Until this stage, no apparent change occurred in the readiness and vigour with which the striped muscles contracted when directly galvanised; their reaction continued to be alkaline, and they were perfectly flaccid. Soon after the stoppage of the heart's action the blue colour, which has been already mentioned, began to disappear, and in ninety-six hours (four days) the muscles were quite pale. No stiffness was yet observable, and galvanism still induced faint contractions. Rigor mortis commenced soon after this, but its progress was extremely slow, as galvanism produced dimples at the electrodes until 110 hours. When the frog was again examined, at 129 hours after the injection of physostigma, no muscular contraction could be produced by powerful galvanism; rigor mortis was complete; and the reaction of the muscles was found to be acid. Galvanism could produce a very faint contraction of the cardiac muscle, limited to the points of stimulation, until about the time up to which feeble indications of retained contractility could be obtained in the voluntary muscles.

Experiment VI.—(*Temperature of Laboratory between* 52° *and* 54° *F.*)

To a frog, weighing 379 grains, four grains of extract were administered in the same way as in the preceding experiment. Motor nerve-conductivity ceased in two hours and sixteen minutes, by which time the exposed muscles were found to have become blue. The cardiac action continued rhythmical, though much reduced in frequency, until twenty-seven hours and fifteen minutes, after which, the auricles alone contracted spontaneously till forty-four hours, and then, spontaneous cardiac action entirely ceased. During all this time, the muscles were flaccid, contracted vigorously on the application of weak galvanism, and had an alkaline reaction and a blue colour. Soon afterwards, they became paler and slightly stiff, but it was not until seventy hours after the administration of the poison that galvanic stimulation failed to produce any contraction; and then rigor mortis, with an acid reaction of the muscles, set in.

These three experiments distinctly prove the absence of any paralysing effect by physostigma acting through the blood on striped muscle.

Rigor mortis is delayed for an unusual period after apparent death in cold-blooded animals, and its appearance, in mammals and birds, is certainly not hastened. In both classes this change in the condition of muscles is only indirectly affected by this substance, and that through its influence on the cardiac contractions. When the blood supply of the muscles is stopped their function is suspended, and rigidity follows; but the resulting rigor does not seem to be due, in any other than this indirect method, to the action of physostigma.

This may be more clearly demonstrated by detailing one of many experiments in which a portion of the frog was protected from the influence of the poison.

Experiment VII.—(*Temperature of Laboratory between* 52° *and* 54° *F.*)

The right iliac artery was exposed, by removing a portion of the sacrum, and tied in a frog, weighing 878 grains. Two minutes afterwards, three grains of extract, suspended in ten minims of distilled water, were injected into the subcutaneous cellular tissue at the right shoulder. In a few minutes, a condition of general paralysis existed, and shortly afterwards the skin of the tied limb was much paler than that elsewhere, this contrast becoming more marked as the experiment advanced. In an hour and twenty minutes, the sciatic nerves being exposed, it was found that the left was completely paralysed; while galvanism, applied to the right nerve, or that of the limb protected from the action of physostigma, produced active muscular contractions. The muscles of the tied limb were pale as contrasted with those to which the poison had access, and the latter were distinctly blue in colour. The non-poisoned muscles continued active until forty hours; but when examined at forty-nine hours they were acid and stiff, and did not contract when galvanised. In the poisoned parts, the functions of the motor nerves were destroyed in three hours and ten minutes; the non-poisoned, or right, sciatic continued active until thirty-two hours. It was possible to distinguish the heart's impulse on the thoracic walls, and to determine the frequency of its contractions. During the three days that immediately followed the poisoning, these steadily continued at a rate varying from seventeen to twenty-one; and on exposure, at the end of that time, fifteen feeble beats per minute were occurring. Soon after, the usual irregularities were observed, but the circulation was maintained until eighty-two hours after the injection of physostigma, as the microscope demonstrated. During all this period, the muscles everywhere, except in the tied limb, were flaccid, blue and of alkaline reaction, and contracted, though latterly with diminished vigour, when galvanised: those of the tied limb were now putrefying. In 100 hours, the poisoned muscles had lost much of their blue colour, and contracted slowly and partially. In 120 hours, they were slightly stiff, and galvanism produced merely a slow surface depression at each electrode, which continued for a short time after their removal, and gradually disappeared. Such contraction could still be obtained 124 hours, or more than five days, after the injection of the Calabar bean. A similar dimpling could be produced on the heart till nearly the same time, and, therefore, long after it had lost its power of spontaneous contraction.

This experiment affords a very simple means of comparing the effect of mere absence of blood supply with the action of Calabar bean on the irritability of striped muscle. In the former case, irritability was destroyed, and the rigor of death initiated, at some time between forty and forty-nine hours. In the latter, subjection of the muscles during eighty-two hours to the influence of a blood stream conveying Calabar bean was not attended with the slightest injury; their irritability disappeared, however, forty-four hours after the circulation had been stopped by the action of this substance on the heart. Muscular paralysis was in both cases due to stasis of the circulation. It has, however, rarely happened that this irritability has continued so long as five days, in fatal cases of physostigma poisoning. The special circumstances for its production appear to be lowness of temperature and protracted continuance of the cardiac action.

These results may be seen with greater clearness if we tabulate the more important points of the above experiments, and of a few others.

Table of the Periods at which Muscular Contractility was lost, after the administration of Physostigma, and after Blood-stasis by Ligature of Vessels.

Experiment.	Weight of Frog and of Dose, in grains.	In Parts to which the Poison had Access.			In Parts cut off by Ligature.	
		Time when Circulation stopped.	Time when Muscular Contractility was lost.	Interval between stoppage of Circulation and loss of Muscular Contractility.	Nature of Operation.	Interval between local stoppage of Circulation and loss of Muscular Contractility.
V.	473 to 3	73 h.	110 h.	37 h.		
VI.	379 „ 4	28 h.	68 h.	40 h.		
VII.	878 „ 3	82 h.	124 h.	44 h.	Left iliac artery was tied.	In less than 49 h., and in more than 38 h.
VIII.	490 „ 3	52 h.	94 h.	42 h.		
IX.	292 „ 2	21 h.	75 h.	54 h.		
X.	212 „ 1·5	48 h.	104 h.	56 h.		
XI.	400 „ 2	34 h. (nearly)	74 h.	40 h. (about)		
XII.	460 „ 2	80 h.	120 h.	40 h.		
XIII.	620 „ 2	45 h.	88 h.	43 h.	Left iliac artery was tied.	44 h. 8 m.
XIV.	690 „ 3	50 h. 6 m.	72 h.	22 h. (nearly)	Left iliac artery was tied.	In less than 24 h., and in more than 19 h.
XV.	620 „ 5	0 h. 34 m.	33 h.	32 h. 26 m.	Right iliac artery was tied.	In less than 33 h., and in more than 29 h.

After this evidence, it is almost superfluous to remark that physostigma-paralysis cannot be caused by an action on striped muscle, as at least one observer of note has maintained.* Although the effects on muscle have been the first considered, it may be proper to remark here that idio-muscular irritability is the last vital property to disappear in death by Calabar bean, especially as this result has been foreshadowed in several of the experiments already given. Its loss is, moreover, only indirectly caused by physostigma, and the evidence is sufficient to show that it follows the cessation of the blood supply, which is necessary for its manifestation, in tolerably definite periods. As the circulation sometimes ceases more abruptly than at others, so does the loss of muscular irritability sometimes occur more quickly, and without much previous gradual diminution of activity. In the former case, rigor is well marked and comparatively prolonged; in the latter, and chiefly with frogs, it is slight, and the very partial nutritive activity which has been for many hours maintained by a sluggish blood-stream, favours the almost immediate occurrence of decomposition, and, therefore, of a short period of rigor mortis, when the circulation has finally stopped.

In the experiments with mammals and birds, an early and constant symptom was the occurrence of successive muscular contractions of a non-co-ordinate character; and this formed a striking contrast with the flaccid and motionless condition of the muscles which persisted throughout the poisoning of cold-blooded animals. Generally speaking, these contractions were very feeble, and consisted of slight spasmodic twitches, which, in mammals, usually began at the neck and then extended over the body, and which at first involved detached portions of the panniculus carnosus muscle only, and then apparently every muscle of the body and extremities. In the slighter cases, and, I think, where a small dose was being but slowly absorbed, a mere tremulous movement was caused of the head, body and extremities, similar probably to the "tressaillements" which CLAUDE BERNARD describes as occurring during curare poisoning,† and which have likewise been noticed with that substance by WATTERTON, and by MARTIN-MAGRON and BUISSON.‡ In one or two of my experiments, however, this muscular action became so strong, that the animal appeared as if under the action of a poison which produces convulsions. The twitches always became more marked when the poisonous effects were fully developed, they gradually diminished in strength as death approached, and they continued in a slight form for many minutes after it. Exposure of the muscles to the air, and irritation with a knife, during the autopsy, increased their strength, and even originated them in muscles and parts

* NUNNELEY on the Calabar Bean, &c. Lancet, 1863, p. 23, and Pamphlet.

† Leçons sur les Effets des Substances Toxiques et Médicamenteuses, 1857, p. 268.

‡ Action comparée de l'éxtrait de Noix Vomique et du Curare sur l'économie animale. Journal de la Physiologie de l'Homme et des Animaux, tome troisième p. 327, &c.

of muscles from which they had disappeared; and it was then observed that the whole of a muscle seldom twitched at once, but portions of it separately and in succession. I frequently removed a muscle from the dead body, and found that these twitches still continued. In one experiment, where the sartorius had been cut out of a dog, these spasms rapidly followed each other in separate portions of its substance, during ten minutes. Their duration after death varied greatly. Occasionally they ceased before the motor nerves had lost their function, while they frequently continued after their paralysis. The latter effect was well shown in an experiment where seven grains of extract were given to a large dog, by injection into a jugular vein: death, with stoppage of the heart's action and of the respiratory movements, took place in eleven minutes; in twenty-five minutes afterwards, the sciatic nerves were paralysed; and these extraordinary muscular twitches continued for other twenty minutes, or for forty-five minutes after the death of the animal. The central nervous system has no influence in causing or originating these quiverings, for division of a sciatic nerve, before the exhibition of physostigma, did not appear to impede their production. I believe the effect is due to the contact of the poison with the muscular substance itself; and this view is supported by the above facts, and by the circumstance that when a ligature was drawn round the posterior extremity of a rabbit, taking care to exclude the sciatic nerve, the muscles of that limb remained unaffected, after the administration of Calabar bean, while those of the body and of the other extremities were twitching in the usual manner.

Action on the Cerebrum.

A condition of retained consciousness with marked paralysis opposes the idea of the latter symptom being due to coma. Professor Christison has admirably described the coincidence, in his own person, of retained mental vigour with inability for movement. To distinctly prove the absence of any cerebral explanation for this paralysis, a simple experiment was undertaken.

Experiment XVI.

The brain was removed with care from a large frog, and, sometime after this, the animal was found jumping about vigorously.* Two grains of extract, suspended in fifteen minims of

* It appears somewhat startling to assert that complicated movements, of an apparently voluntary character, may continue in frogs after the removal of the brain. I first observed this in an experiment in which the spinal cord had been divided at the base of the skull; and, in describing the condition of the frog in which it was seen, I added a qualifying note ascribing the circumstance to incomplete division of the medulla. Since then, I have occasionally observed the same phenomenon; and the present experiment is conclusive in showing that some of those functions which we are in the habit of ascribing to the cerebral lobes alone, are, in frogs at any rate, shared in by the spinal cord.

May 1867.—Dr Norris enters into this anomaly, and confirms its occurrence, in his admirable paper on Muscular Irritability, in the "Journal of Anatomy and Physiology," No. 2, p. 221, *et seq.* He also refers to Lewes (Physiology of Common Life, vol ii.) as having first prominently announced this curious exception to the generally received views on nerve physiology.

distilled water, were injected into the lower portion of the abdominal cavity. It continued to jump about for four minutes; in five minutes, the first indications of paralysis occurred, at the anterior extremities; and in twenty-five, it was lying flaccid on its belly and chest, without any respiratory movements, but with retained reflex action. The sympoms advanced in their usual order until the complete death of the frog.

An action on the cerebrum cannot, therefore, be the cause of the paralysed condition which is produced. The cerebrum may, notwithstanding, be acted on by physostigma; and the results of several experiments, in which I took various quantities of this substance, appear to favour such an opinion, though, until further investigation, I cannot maintain that the effects produced were not mainly dependent on those perturbations of the circulation which are caused by this poison.

Action on the Spinal Nerves.

1. *Motor or Efferent Nerve-Fibres.*

Experiment XVII.

I injected into the jugular vein of a very large retriever dog, seven grains of extract, suspended in twenty-five minims of distilled water. In three minutes, respiration became gasping; in five, the usual twitching affection of the muscles commenced; and in ten, the dog was lying in a powerless condition. In twenty minutes, respiratory and cardiac action had completely ceased. The right sciatic nerve was then exposed, and galvanic stimulation of its trunk produced vigorous movements confined to the leg whose nerve was stimulated. The same result was obtained when the left sciatic nerve was exposed and stimulated. Both retained their motor conductivity for eight minutes after respiration had ceased; and, for some time longer, the muscles responded to direct galvanic stimulation.

Experiment XVIII.

Five grains of extract were mixed with thirty minims of distilled water, and injected into the abdominal cavity of a large and healthy female cat. Trembling occurred in four minutes, when the cat ran a short distance and fell on her face, after which she lay in any position, flaccid and unresisting. Respiration ceased in ten minutes, but the peculiar twitching of the muscles continued for several minutes longer. The heart was then exposed, and found motionless and full. On galvanising the sciatics, or otherwise stimulating them, the muscles of the posterior extremities contracted vigorously; but no evidence was obtained of the reflex activity of the cord. The sciatic nerves continued active until forty-two minutes after the death of the animal (fifty-two after the administration of the poison); but, when they were galvanised at forty-seven minutes after death, no muscular contraction was produced. The phrenic and brachial nerves also continued active for about the same time.

Experiment XIX.

Three grains of extract, suspended in eleven minims of distilled water, were injected into the subcutaneous cellular tissue in the flank of a full-grown rabbit. Trembling occurred in two minutes, and this continued, with varying strength, until seven minutes; two minutes before which respiration had ceased. The sciatic, phrenic and brachial nerves were galvanised, and found to be active; and such stimulation of the sciatics continued to produce muscular contraction until nine minutes after the cardiac and the respiratory movements had ceased.

Experiment XX.

There was injected into the subcutaneous tissue at the right flank of a rabbit, weighing two pounds, half a grain of extract in fifteen minims of distilled water. The usual tremors rapidly supervened, and in forty-five minutes the animal was dead. During the following thirty-one minutes, galvanism of either sciatic nerve produced contractions of the limb which it supplied.

Experiment XXI.

Half a grain of extract was placed in the mouth of a pigeon. In ten minutes, a profuse flow of saliva and of tears, with occasional passage of fæces, occurred; after which time the bird lay in a helpless condition, with now and then a sudden starting movement. In twenty-four minutes, its respirations had completely ceased. Until eight minutes after death, galvanism of the left sciatic nerve continued to produce movements in the left leg; but in thirteen, the nerve was quite paralysed.

Such experiments were frequently repeated; and in no case did I find that the motor nerves were paralysed before the respiratory movements had ceased, although it has occurred that they have almost immediately afterwards been so. I have found that the interval during which they remain active varies greatly in different animals, and in the same animal according to the dose of poison administered; and I believe that in the latter case the variation is in an inverse ratio. In the rabbit, motor conductivity may be retained for periods ranging from a very few (two or four) to thirty-one minutes. We cannot, therefore, account for either the condition of general paralysis or the cessation of the respiratory movements, which two form the most prominent of the symptoms of physostigma poisoning in warm-blooded animals, by an action on the motor nerves. Experiments XVII., XVIII., XIX., XX. and XXI. are sufficient to prove this. The evidence obtained by experiments with frogs is even more unmistakable. Complete destruction of all the vital functions in this animal never occurred for many hours. In animals of a higher type, the implication of one system so rapidly influences the others that it is often difficult to discriminate between the effects which are caused by the poison and those which are induced as results of the primary action. In the frog, on the other hand, the symptoms advance so slowly from one system to another that it is possible to determine distinctly the sequence of the phenomena which are due to the direct influence of physostigma.

Experiment XXII.

Five grains of extract, suspended in a few minims of distilled water, were injected under the skin over the back of a frog weighing 490 grains. A small quantity escaped in the somewhat vigorous movements which occurred when the frog was liberated. The thoracic respiratory movements ceased in ten minutes, and those of the throat in other four. Twenty-two minutes after the injection, the animal lay on its abdomen in a perfectly flaccid condition; its heart was acting feebly, at the rate of seven per minute; and pinching of the skin anywhere caused but very weak reflected movements. In one hour, no evidence could be obtained by galvanism, or by any usual irritant, of continuance of the diastaltic function of the cord; but the heart was now contracting nine times in the minute. The sciatic nerves were then exposed; and, on galvanising either of them, movements occurred in, and were confined to, the muscles of the limb whose nerve was so stimulated. Both nerves continued to give this evidence of the conductivity of their motor fibres as long as one hour and thirty-eight minutes after the injection of Calabar bean, or one hour and twenty-four minutes after all respiratory movements had ceased. Shortly afterwards they were found paralysed.

Experiment XXIII.

Into the lower portion of the abdomen of a frog, which weighed 620 grains, a mixture of two grains of extract with ten minims of distilled water was injected. In twelve minutes, the frog was lying in a flaccid condition, and respiration had ceased. In two hours, no reflex movement could be excited. The right sciatic nerve was exposed; and its motor conductivity was found to remain. Galvanism, applied to either sciatic nerve, produced muscular contractions in the limb to which the nerve was distributed, until, but not later than, three hours and twenty-one minutes after the administration of the poison, or three hours and nine minutes after respiration had ceased, and until more than one hour and twenty-one minutes after apparent destruction of the reflex function of the spinal cord. In this experiment, it is important to note, the cardiac action was not greatly affected for more than three hours, as the dose of poison administered was comparatively small.

Experiment XXIV.

Six grains of extract, suspended in fifteen minims of distilled water, were injected into the abdominal cavity of a frog, weighing 350 grains. In eight minutes, no cardiac impulse could be discovered; and the heart was then exposed and found motionless, dark and flaccid. Respiratory movements ceased in eight minutes. It was determined, on irritating the skin with sulphuric acid, that reflex movements could not be obtained two hours and a-half after the injection; but they were produced until nearly this time. *The motor conductivity of the sciatic nerves was retained for twenty-nine hours.*

It thus appears that the motor nerves always remain active after the co-ordinated movements of respiration have ceased, and after the condition of complete and flaccid paralysis has existed for long. On this point, therefore, I cannot agree with HARLEY, who considers that physostigma is a respiratory poison only, and that the early production of asphyxia is caused by paralysis of the motor nerves.*

The protracted interval in the last experiment, between the administration of the poison and the loss of motor conductivity, must be looked upon as a very exceptional one, and as due to the poison having so quickly paralysed the heart that the usual phenomena were not produced. In Experiments XVII., XIX., XXI., XXII., and XXIII., the motor nerves appear to have lost their function sooner than naturally happens in death from cardiac paralysis or from asphyxia. But that they really did so must have remained a mere impression, had it not been that we can, in frogs at any rate, definitely prove a special action on the

* Journal de l'Anatomie et de la Physiologie, 1864, p. 141, *et seq.;* and British Medical Journal, Sept. 3, 1863.

motor nerves, by protecting a portion of the animal from the poison. Such a proceeding cannot be avoided by determining the interval which naturally elapses between the moment of death and the time at which loss of function occurs in these nerves. This interval varies greatly in different classes of animals, and also in different individuals of the same species; and, hence, the data which have as yet been accumulated on this point are not available for such purposes as the present.

Experiment XXV.

The sacrum was excised from an average-sized frog, and, in that manner, the lumbar nerves and the abdominal aorta were exposed. The aorta was ligatured above its bifurcation into the two iliacs, and, immediately afterwards, an average poisonous dose of extract was placed in the frog's mouth. In ten minutes, respiration had ceased; but the frog continued jumping about for other four minutes, when it quietly and gradually subsided on its abdomen and chin. Reflex movements could be excited by irritating the skin anywhere until one hour and seven minutes after the administration; but, for some time before this, a great increase in the strength of the stimulant was required. The skin of the posterior extremities had now become paler, while that of the body, anterior to the ligature, had assumed a much darker colour than it had before the experiment; and the exposed heart was found beating twenty per minute, with regularity and in proper rhythm. The brachial nerve was then laid bare, and was found active; *but this condition ceased two hours and eleven minutes from the commencement of the experiment, and both brachials were then perfectly paralysed.* At this time, weak galvanism applied with closely approximated poles to sections of the spinal cord produced no effect; but when the lumbar nerves below the ligature were galvanised they caused vigorous contractions of the posterior extremities. *On the following morning, this motor nerve activity still remained, in the parts protected from the poison:* but the contractions were now feeble, as muscular rigor was commencing below the ligature; and, in a few hours longer, the muscles separated from the circulation ceased to contract. The muscles of the poisoned portion, in which the motor nerves had been long paralysed, were still quite flaccid, alkaline and irritable; and rigor mortis did not occur in them until the following day.

Experiment XXVI.

The right iliac artery and the right ischiadic vein were tied in a frog, weighing 620 grains; and five grains of alcoholic extract of physostigma, suspended in thirty minims of distilled water, were injected into the abdominal cavity. In twenty minutes, voluntary movements had completely ceased, there were no respirations, and the frog lay in a perfectly flaccid condition. Fifty minutes after the administration of the poison, the left sciatic nerve was exposed. Very weak galvanism of the nerve-trunk caused contractions of the limb; and continued to do so, on occasional observations, till two hours and ten minutes from the commencement of the experiment, or till fifty minutes after the respirations had ceased. In other fifteen minutes, however, the nerve was found to be completely paralysed. The right sciatic nerve, which had been protected from the influence of the poison, by ligature of the blood-vessels of the limb, was examined in a similar manner. *Its motor conductivity continued unimpaired for at least five hours longer than that of the poisoned nerve.*

These are examples of numerous experiments which were undertaken for the special purpose of determining whether Calabar bean has any action on the spinal nerves. They prove undoubtedly that it has the power of destroying their motor conductivity; but it has also been demonstrated that this is not the cause of paralysis and death in mammals, or of complete loss of voluntary power in frogs. The next question which suggests itself for solution is the somewhat interesting one of the portion of nerve acted on; for, in the case of more than one toxic substance, it has been determined that loss of motor nerve-function does not of necessity imply that both the periphery and nerve-trunk have been affected.

Experiment XXVII.

Immediately after ligature of the left ischiadic artery and vein, two grains of extract were placed in a subcutaneous cavity at the back of an active large frog. Before twenty minutes, respiratory movements had ceased; while the heart was then acting rhythmically, at the rate of thirty beats per minute. One hour after the administration of the poison, the right (or poisoned) sciatic nerve was exposed, and found active; but in other twenty minutes, strong galvanism applied to any portion of its trunk could not produce contractions in the muscles to which it was distributed, and an examination of the brachial nerves proved them also to be paralysed. The left sciatic nerve was, however, perfectly active. When it was galvanised, movements, confined to that limb, were produced in the muscles below (or distal from) the ligatures. *These ligatures were on the thigh; but stimulation of the nerve above them, or of the lumbar nerves on the same side, was followed by energetic muscular contractions below the points of ligature.*

The muscles were everywhere active, and continued so for several days; and those of the non-poisoned limb were the first to pass into rigor mortis. The sciatic nerve of the non-poisoned limb, and the lumbar nerves of the same side, continued active for many hours; but their loss of function occurred several hours before that of the muscles below the ligatures.

This evidence, which has been frequently confirmed, is in favour of the view that the motor paralysis caused by physostigma is due to an action on the nerve endorgans, or peripheral terminations, and not to one on the trunk. We may obtain even more distinct proof, by a slight modification of this experiment.

Experiment XXVIII.

An incision was made down the centre line in the right posterior extremity, from the back of the knee to the ankle, of a frog, weighing 876 grains; and in this way the gastrocnemius muscle was completely exposed. It was carefully dissected from its connections, excepting that its origin and insertion, and the nerve fibres entering it, were untouched. All its blood-vessels were ligatured, and the cut through the skin was closed by sutures.

Immediately after the above operative procedure, three grains of extract, in fifteen minims of water, were injected under the skin of the back. Reflex movement could not be excited an hour and five minutes afterwards, while the heart still continued to contract. Both sciatic nerves were then exposed. Galvanism of the left produced no contraction; while galvanism of the right caused energetic movements of the limb, which, moreover, did not extend to the toes. Five hours after the administration of the poison, this condition continuing, the left gastrocnemius was exposed, and the right again laid bare by cutting the sutures. Galvanism of the right sciatic demonstrated visibly contraction of the right gastrocnemius, but of no other muscles of that limb; and no result followed stimulation of the left sciatic, although the left gastrocnemius muscle contracted vigorously when the poles were applied to its surface.

It is thus shown that some of the endorgans of a motor nerve may have their conductivity destroyed while others remain active during the retained vitality of the nerve trunk, and that this contrast in condition depends on the access or not of the poison—a clear demonstration of the power of physostigma to paralyse the nerve terminations. This action has been hitherto overlooked.

Calabar bean is, therefore, now added to that very limited class of neurotic agents which affect the motor endorgans. Indeed, only two substances, as far as I am aware, were previously known to possess this remarkable action. For a considerable time after the brilliant, and perhaps unequalled, researches of CLAUDE BERNARD,* curare stood alone as such a substance: when KÖLLIKER discovered that conia has a similar action;† and his observations have been recently confirmed by GUTTMANN.‡

Physostigma is, however, peculiar in the method in which it so acts; a very prolonged contact with the nerve terminations, and a long continued circulation of poison-bearing blood, being apparently necessary. In warm-blooded animals, this paralysis of the motor endorgans may, therefore, be easily overlooked; but in frogs, with localised poisoning, it is conspicuously displayed, as in the experiments which have been given.

It is interesting to remark the different conditions which are produced in the functional vitality of nerves and muscles when physostigma is administered to a frog after the vessels of one of its limbs have been ligatured.

Experiment XXIX.

Immediately after ligaturing the right ischiadic artery and vein of a frog, which weighed 609 grains, two grains of extract, in fifteen minims of distilled water, were injected into the subcutaneous tissue of the back.

One hour and twenty-five minutes afterwards, the heart was found beating seventeen times per minute.

The two gastrocnemii muscles, with their femur attachments, and a portion of each of these bones, along with the sciatic nerves from their terminations in the gastrocnemii to the lumbar plexus, were then removed. These parts were so arranged, that an interrupted current from one DANIELL'S cell and DU BOIS REYMOND'S induction apparatus, could be transmitted simultaneously through either both nerve trunks, or both muscles, by the turn of a key.

a. Examination of the Nerves (one hour and fifty-four minutes after the administration of the poison).—The galvanic current was first passed through the sciatic nerves. Distinct tetanus of the non-poisoned muscle was caused when the secondary coil stood at 63° on the scale; the poisoned was at perfect rest. The current was gradually strengthened by advancing the secondary coil; when this reached 53°, but not before, the poisoned muscle was thrown into tetanus.

b. Examination of the Muscles.—Immediately afterwards, the current was passed directly through both muscles. The poisoned gastrocnemius contracted when the secondary coil reached 63°; the non-poisoned did not do so until this was advanced to between 52° and 53°: that is to say, the poisoned muscle was thrown into tetanus by a weaker current than was required to produce the same effect in the non-poisoned muscle.

* Leçons sur les Sub. Tox. &c., 1857, pp. 238–413.

† Verh. d. phys.-med. Ges. zu Würzburg, 1859, vol. ix., part 2, p. 55, *et seq.*; Virchow's Archiv. x., p. 235; and other papers.

‡ Berliner Klin. Wochenschr., No. 5–6, 1866. Quoted in RUTHERFORD'S Report on Physiology; Journal of Anatomy and Physiology, No. 1, 1866, p. 155.

Experiment XXX.

A similar experiment was performed on another frog, also poisoned with two grains of extract, but weighing only 464 grains.

a. Examination of the Nerves (two hours and twenty minutes after the injection).—The galvanic current was passed along portions of both sciatic nerves simultaneously. When the secondary coil reached 52°, the non-poisoned gastrocnemius was thrown into tetanus, the poisoned remaining inactive. It was then slowly advanced; and at 30°, faint contractions occurred in the poisoned gastrocnemius, of a partial character, as if only a few muscular bundles, and not the whole muscle, were contracting, and which continued for a few seconds only, and did not recur although the secondary coil was advanced, after an interval for rest, to 0° (the strongest current from this arrangement). The stimulus was then applied to the trunk of the non-poisoned nerve above the position of the ligatures, and, therefore, where it must have been in contact with the poison; contraction again occurred when the secondary coil was at 52°.

b. Examination of the Muscles.—When the secondary coil reached 55°, tetanus was produced in the poisoned gastrocnemius; and at 54°·5, the non-poisoned muscle was thrown into tetanus.*

It would thus appear that motor nerve excitability or conductivity is diminished and then destroyed by physostigma (this change being produced at the endorgans), while retained for a long time thereafter in those parts of the same animal which have been guarded from the access of the poison. It is also seen that the effect on idio-muscular contractility is exactly converse; that property being uninjured by the mere presence of physostigma, while diminished and destroyed by stoppage of the circulation.

It is usually asserted that division of a nerve, previous to the exhibition of any substance that affects its vitality, is a sufficient method for determining the position of its primary implication, and, therefore, sufficient for determining the direction in which this extends. This proceeds upon the supposition that when it is the nerve trunk near its origin that is first affected, extension of the poisoned condition to the distal portion will be delayed by intermediate division of the nerve. BERNARD attempts in this way to prove that the primary paralysis of the motor nerve endorgans by curare extends from them, along the trunk of the nerve, towards the cord.† The paralysis of the motor nerves after strychnia is said to proceed in a direction exactly the reverse,—from the origin to the periphery.‡ It appeared of some interest to examine this question with Calabar bean, for with it we would not expect that previous division of the trunk should delay the implication of the nerve endorgans, as this precedes the paralysis of the trunk.

Experiment XXXI.

I exposed the two sciatics of a frog for a short distance, and both equally, and divided the left nerve with a very sharp pair of scissors. A fatal dose of Calabar bean was then administered. When reflex movement could no longer be excited (one hour and two minutes after the administration), the right sciatic was galvanised; but no contractions were caused. The left was stimulated, at the cut extremity of its distal portion, with the same current, and active movements of the left leg and toes followed. The galvanism was repeated, at intervals of five minutes,

* In Experiments XXIX. and XXX. I have thankfully to acknowledge the valuable assistance I obtained from Dr RUTHERFORD.

† *Loc. cit.*, p. 312. ‡ *Ibid.*

for other twenty minutes; with the same negative result in the case of the uncut nerve, and with continuation of activity in the cut one. The energy of the contractions then gradually diminished; but the distal portion of the cut nerve was not paralysed until twenty-eight minutes after the loss of motor nerve conductivity in the portion of its trunk proximal to the spinal cord, as well as in all the other nerves of the body.

Experiment XXXII.

In a second experiment, the distal portion of the divided nerve retained its conductivity forty-five minutes longer than the undivided nerve.

Another experiment contains some further results, which are worthy of being shortly mentioned.

Experiment XXXIII.—(*Temperature of Laboratory*, 56° *F.*)
Performed in August 1866.

I exposed a small and equal portion of each sciatic nerve in a frog weighing 515 grains, and cut through the right nerve-trunk. One grain and a-half of extract was then injected into the cellular tissue under the skin of the back.

In one hour and thirty minutes, no variety of stimulation could excite reflex movements: and when the left, or uncut, sciatic was then gently galvanised, faint contractions of the left toes were all that was produced; while the same interrupted current caused vigorous contractions when applied to the distal portion of the cut nerve. This condition continued, the contractions produced by the left (uncut) nerve becoming gradually fainter, until three hours and twenty-two minutes after the poison had been injected, when this nerve became perfectly paralysed, as well as all the other motor nerve fibres and the proximal portion of the cut sciatic. The distal portion of the cut (right) sciatic seemed all this time quite unaffected; and when it was galvanised, the muscles with which it was connected contracted with vigour. This condition lasted for fifteen minutes, *when galvanism of the uncut nerve again produced a faint twitch of several of the left toes.* By-and-by, it recovered all its former activity, and the return to vitality was shared in by the other temporarily paralysed nerves. On the following morning the frog was perfectly well and jumping about.

From such data it cannot be concluded that the motor nerve fibres are paralysed by a centripetal progression of the poison. There seems only to be a connection between the rapidity of paralysing effect, on the one hand, and the subdivision of nerve substance with facility of contact of poison, on the other; as the motor trunks were undoubtedly affected when it was impossible that they should be influenced by an extension of the poisonous action from their endorgans. I cannot advance any very satisfactory explanation of this delay in the action. Probably it is caused by the irritation of the vaso-motor nerves in the trunk of the sciatic, which the section of the latter at first produces, and which is followed by contraction of the capillaries in the parts supplied by them.* During this contraction, a much smaller quantity of poison-bearing blood is brought in contact with the endorgans of the cut than with those of the uncut nerve, and, as a very prolonged contact of the poison appears necessary, this may be sufficient to account for the delay; while the recovery from the irritation of the section, which soon

* Such contraction has been directly demonstrated by LISTER and others after division of the sciatic nerve. See "An Inquiry regarding the parts of the Nervous System which regulate the contraction of the Arteries;" Philosophical Transactions, 1858, vol. cxlviii., p. 607.

occurs, again allows of a free circulation and of consequent paralysis of the motor endorgans of the divided nerve.

2. *Afferent Nerve Fibres.*

The discussion of the influence that those fibres in the spinal nerves that conduct impressions to the cord exert in producing paralysis, will be a short one, as it can be readily shown that their effect is negative. It will be sufficient to notice that in mammalians it was always possible to obtain evidence of their activity as long as the functions of the spinal cord were retained, and that, therefore, they were in no wise concerned in the production of the general flaccidity and loss of motor power which is caused by Calabar bean. The same evidence was obtained in frogs, and could in them be distinctly shown by localization of the poisoning. In place of the function of these nerves being lessened, I believe that it is generally increased, so that movements may be excited more readily after the action of the poison than before it.

Experiment XXXIV.

I tied the right ischiadic artery and veins of a frog weighing 573 grains, and suspended it by the lower maxilla. Soon after, a silk thread was drawn over various parts of the skin, including the right leg, without exciting any reflex movement. On dipping the feet, separately, into dilute sulphuric acid (five minims of oil of vitriol to twelve ounces of water) reflex movements occurred with each, after 80 beats of a métronome set at 100 in the minute. I then injected into the abdomen two and a-half grains of extract in fifteen minims of water. In one hour and three minutes, on the left foot being dipped into the acid, reflex movements occurred in the right in 190 beats of the métronome; but no movement followed in the left limb in 200 beats, nor when the poisoned foot was placed in stronger acid (10 min. to 12 oz.), while this caused energetic contractions of the non-poisoned portion of the right limb. The reflex activity of the spinal cord was, therefore, very greatly diminished, and still the afferent nerves continued active. The silk thread which was formerly employed was now drawn over the skin of the right leg below the ligatures, and, as before, it produced no diastaltic movement. *On applying it, however, to the skin of the left leg and of the other poisoned regions, twitches constantly occurred in the toes of the right leg, and only occasionally, and of a very feeble character, in the poisoned region.* This was repeated, at intervals, during the next ten minutes, with the same result. After this, the thread ceased to excite diastaltic movements; but the spinal cord had now lost its vitality, and no movement could be produced even when it was directly galvanised.

The afferent nerve fibres, in this experiment, retained their original activity longer than the efferent, and at least up to the time at which they could not be tested, because of the loss of the diastaltic function of the spinal cord. The increase of excitability in the afferent fibres, where these had been acted on by blood conveying physostigma, cannot be due, in the slightest degree, to any spinal cause, for the effect did not occur in the right leg, to which access of the poison had been cut off by ligature. Besides, measurement of the rapidity with which reflex movements followed the application of a stimulus to either poisoned or non-poisoned parts gave such proof of marked depreciation having occurred before the afferent excitability had been increased as is sufficient of itself to eliminate any spinal influence.

ACTION ON THE SPINAL CORD.

Having now excluded the encephalon, muscles and spinal nerves, we are led to conclude that the production of the paralysis by physostigma is due to an action on the spinal cord. Such an opinion was originally expressed by myself after a careful general consideration of the symptoms, but no subsequent investigator has coincided with my theory except LASCHKEWICH,* from whose excellent paper I have derived more than one hint for the further and special examination of the spinal effects of Calabar bean.

I quote the following experiment from my previous paper on this subject, as it affords an excellent example of those general paralytic symptoms that first induced me to refer the principal neurotic action of physostigma to the spinal cord.

Experiment XXXV.

"Five and a-half grains of the fine powder of the kernel were made into pills, and swallowed by a buck rabbit, eight months old.

"A slight degree of paralysis was seen in the posterior regions, in ten minutes, and, soon after, they yielded, the anterior portion of the trunk remaining supported by the fore-limbs. In fifteen minutes, the fore-legs gave way, and fæces were passed. In twenty minutes, the respirations became noisy, reflex action was not abolished, and the pupils contracted. In thirty minutes, the rabbit submitted to be placed in any position. In thirty-five minutes, the respirations became extremely noisy, and accompanied with muscular spasm. Fæces and urine were passed, and reflex action could not be induced by puncturing the skin. General, but slight, muscular spasms now occurred frequently; the eyelids did not contract when the eyeball was pricked, and the respiratory stertor ceased. In forty minutes, a general spasmodic contraction of the muscles occurred, and, in forty-one minutes, all respiratory movement had ceased.

"*Autopsy,* immediate. The cut muscles contracted. The heart was acting at the rate of seventy-two per minute; and this ratio gradually diminished till it ceased, thirteen minutes after death. The brain was rather darker than usual, and no change could be perceived in the spinal cord. The cerebro-spinal fluid was in abnormal abundance. The large veins were distended, and the right chambers of the heart were engorged with dark blood. . . . The vermicular action of the intestines was well marked, and all the viscera contained an abnormal excess of dark blood. The muscular system was flaccid, but contractions could be caused by irritation of the nerves."

This experiment formed one of a series undertaken to discover the smallest dose which could produce death in a full grown rabbit.† The quantity employed, five and a-half grains of the kernel, was the smallest that could do so.

As evidence of the same character, I add the following from many subsequent experiments:—

Experiment XXXVI.

One grain and a-half of extract, suspended in fifteen minims of distilled water, was injected into the abdomen of a small collie dog. The animal was rapidly affected with inability to

* VIRCHOW'S Archiv. Februar, 1866.

† *Op. cit.* sect. v. experiment viii.

stand, tremors, lachrymation, defæcation and urination; and in eleven minutes all respiratory movement had ceased. The spinal cord was immediately exposed, but the strongest galvanism, consistent with localisation of the current, applied to various portions of its substance, failed to excite any movement of the body. A sciatic nerve was then exposed; and slight stimulation of it produced vigorous contractions of the limb, but no reflex movement. Lastly, the thorax was opened; and the heart was found contracting thirty-two times in the minute, in perfect rhythm and with regularity, although the diastolic pause was somewhat prolonged. Thirty-nine minutes after death, the cardiac beats were ten per minute, and the sciatic and other nerves could still transmit excito-motory impressions to their muscles.

Such data are sufficient, after the former results, to prove the action of Calabar bean on the spinal cord of mammalians, as far as it is possible to do so. Where larger doses are given, the evidence is not so distinct; as, along with complete loss of reflex function, the heart is found paralysed at death; and it is well known that, in the animals in question, stoppage of the circulation is rapidly followed by loss of reflex function. Still, from the above, and from other experiments which will follow, it can be conclusively proved that physostigma has a special and primary action on the cord.

Experiment XXXVII.

Performed December 1866.

After tying the left femoral artery and vein of a frog, weighing 430 grains, I injected two grains and a-half of extract into the cellular tissue of the back. In an hour and twenty-two minutes, the reflex function of the cord, as tested by stimulation of the skin by galvanism and by sulphuric acid, was completely destroyed; but the exposed heart was found acting regularly and rhythmically, though only at the rate of twelve beats per minute. The two gastrocnemii muscles, with their attached sciatic nerves, and the portions of femur into which these muscles are affixed, were then removed. The poisoned nerve and muscle were arranged in the usual manner on Du Bois Reymond's modification of Helmholtz's myographion—an apparatus designed to measure the rates of conduction along nerve fibres. The curved lines produced by stimulation of two portions of the nerve, differing in length by one inch and a-half, were found to correspond so exactly that the period during which the impression travelled over the one inch and a-half of poisoned nerve could not be measured. The non-poisoned nerve gave the same result.

This experiment was undertaken to determine whether physostigma gradually lessens the rate of conduction in motor nerves, as curare is stated to do.* It was worthless to answer this question, as was also another immediately afterwards performed with the same result; for the frogs employed were in too irritable a condition: but its value is evident in considering the action of physostigma on the spinal cord. *For the diastaltic function of the spinal cord was completely destroyed, while the poisoned and non-poisoned motor nerves were in so equally active a condition, that the difference between the times in which impressions travelled along two portions of the same nerve, differing in length by one inch and a-half, could not be measured in either, even by a delicate instrument specially adapted for this purpose.*

* A. von Bezold; Monats Bericht der Berlin: Akad. 1859.

In the further investigation of the effect on reflex movements, there is no process which yields so conclusive results as that in which frequent measurements are made of the interval that elapses between the application of a stimulant to the extremity of an afferent nerve and the resulting reflex contraction.*

Experiment XXXVIII.

The spinal cord was divided at the occiput of a frog, weighing 460 grains. It was suspended by the lower jaw, and the reflex activity tested by dipping the web of both posterior extremities into dilute sulphuric acid (ten minims oil of vitriol to twelve oz. of water). The exact time which elapsed between the contact of the foot and the resulting reflex movement was ascertained by the beats of a métronome, set at 100 in the minute. Before the administration of the poison, the reflex movement occurred in twelve beats. Two grains of extract, in water, were injected into the abdomen.

In 5	minutes,	reflex movement occurred in	15	beats.
10	,,	,,	31	,,
15	,,	,,	40	,,
20	,,	,,	57	,,
25	,,	,,	69	,,
30	,,	,,	82	,,
35	,,	,,	106	,,
40	,,	,,	134	,,
45	,,	,,	165	,,
50	,,	,,	181	,,
55	,,	,,	192	,,
1 hour	5 min.	no reflex movement after	250	,,
1 ,,	15 ,,	strong acid caused no movement.		

The sciatic nerves were then exposed; and weak and carefully localised galvanism applied to either trunk caused energetic contractions of the limb below the portion stimulated, which could be obtained until two hours after the injection of the poison.

It seemed important to ascertain the coincident changes that take place in the heart's action; and for this purpose several experiments were undertaken, of which the following is an example. By a slight adjustment of the frog's body, the cardiac impulses are easily seen and counted.

Experiment XXXIX.

A frog, weighing 460 grains, was suspended by its lower jaw. The average of the cardiac contractions, during ten minutes, was forty-five per minute. The two feet were alternately stimulated, every five minutes, by contact with dilute sulphuric acid (10 minims of oil of vitriol to 12 oz. water); a vessel containing the acid being gently raised so that the fluid covered the whole foot. A métronome, set at 100 in the minute, was employed to determine the interval between the application of the irritant and the resulting reflex movement.

* This method of examining reflex activity seems to have been first recommended by Von Türck in 1850 (Ueber den Zustand der Sensibilität nach theilweiser Trennung des Rückenmarks); and its value has been brought more prominently into notice by Dr J. Setschenow (Physiologische Studien über die Hemmungs-mechanismen für die Reflexthätigkeit des Rückenmarks im Gehirne des Frosches, Berlin, 1863).

15 minutes	before poisoning,	the right	foot was	drawn up in	13	beats.
10	,,	,,	left ,,	,,	11	,,
5	,,	,,	right ,,	,,	9	,,

Two grains of extract, in 15 minims of water, were now injected into the subcutaneous tissue of the back.

In 5 minutes,	the left	foot was drawn up	in 14	beats.	
10 ,,	right ,,	,,	22	,,	
15 ,,	left ,,	,,	36	,,	the heart contracted per minute, 36
20 ,,	right ,,	,,	28	,,	
25 ,,	left ,,	,,	39	,,	,, 18
30 ,,	right ,,	,,	52	,,	
35 ,,	left ,,	,,	49	,,	,, 18
40 ,,	right ,,	,,	72	,,	
45 ,,	left ,,	,,	60	,,	,, 17
50 ,,	right ,,	,,	76	,,	
55 ,,	left ,,	,,	88	,,	,, 15
1 h. & 0 min.	right ,,	,,	99	,,	During this interval, the heart's impulse could not be counted, but the frequency of its contractions was probably much lower than 15 per minute.
1 ,, 5 ,,	left ,,	,,	115	,,	
1 ,, 10 ,,	right ,,	,,	111	,,	
1 ,, 15 ,,	left ,,	,,	138	,,	the heart contracted per minute, 16
1 ,, 20 ,,	right ,,	,,	169	,,	
1 ,, 25 ,,	left ,,	,,	186	,,	,, 20
1 ,, 30 ,,	right ,,	,,	192	,,	
1 ,, 35 ,,	left ,,	twitched slightly in	230	,,	,, 20

After this, no reflex movement occurred when either foot was dipped in much stronger acid; but galvanism applied to the exposed sciatics caused vigorous movements, which occurred only in the limb whose nerve was stimulated. The cardiac contractions, in half an hour, became as frequent as twenty-five per minute; and, on the following morning, the thorax was opened, and rhythmical contractions were perceived, at the rate of twenty-four per minute. A microscopic examination was at this time made of the web, and a circulation was discovered in its capillaries.

A few experiments were undertaken in order to eliminate the possible effect of Calabar bean on the motor nerves in producing this gradual depression of reflex activity.

Experiment XL.

The femoral artery and vein in each posterior extremity of a frog, weighing 540 grains, were ligatured, and the animal suspended by the lower maxilla. Both feet were simultaneously dipped in dilute sulphuric acid (10 min. of oil of vitriol to 12 oz. of water); and the interval between the contact and the resulting reflex movement was measured, as before, by the beats of a métronome.

15 minutes	before the poison was injected,	reflex movement occurred in	10	beats.
10 ,,	,,	,,	9	,,
5 ,,	,,	,,	11	,,

I then injected three grains of extract, in ten minims of water, under the skin of the back.

In 10 minutes,	reflex movement occurred in	28	beats.
20 ,,	,,	24	,,
30 ,,	,,	71	,,

In 40 minutes,		reflex movement occurred in	80	beats.
50	,,	,,	101	,,
1 hour and	5 minutes	,,	118	,,
1 ,,	15 ,,	,,	136	,,
1 ,,	20 ,,	,,	150	,,
1 ,,	30 ,,	,,	200	,,
1 ,,	40 ,,	no movement in	200	,,

A stronger acid was substituted (20 min. to 12 oz.).

In 1 hour and	50 minutes,	reflex movement in	111	beats.
2 hours	0 ,,	,,	160	,,
2 ,,	10 ,,	,,	173	,,
2 ,,	20 ,,	,,	198	,,
2 ,,	30 ,,	no reflex movement in	220	,,
2 ,,	35 ,,	strong acid caused no reflex movement.		

The heart was exposed; and it was found contracting in proper rhythm, nineteen times per minute. The sciatic nerves were active.

On the following day, the heart was contracting twenty times per minute; and galvanism of the sciatic nerves caused feeble muscular contractions in the posterior extremities, but no diastaltic movement. The frog ultimately died.

Experiment XLI.

After ligature of its left femoral artery and vein, a frog, weighing 495 grains, was suspended as in the previous experiments. The reflex activity was tested with the following result:—

Métronome, 100 to 1 minute; acid = 5 min. to 12 oz. water.

At 20 minutes before poisoning,	the left leg	was drawn up in	24	beats.
15 ,,	right ,,	,,	19	,,
5 ,,	left ,,	,,	28	,,

Three grains of extract, in water, were then injected subcutaneously at the back.

In 5 minutes,	the right leg	was drawn up in	24	beats.
10 ,,	left ,,	,,	30	,,
15 ,,	right ,,	,,	36	,,
20 ,,	left ,,	,,	56	,,
25 ,,	right ,,	,,	72	,,
30 ,,	left ,,	,,	90	,,
35 ,,	right ,,	,,	150	,,
40 ,,	no movement when either foot was kept in contact with the acid during 200 beats.			

A stronger acid was substituted, of the strength of 10 minims of oil of vitriol to 12 oz. of water.

In 45 minutes,	the right leg	was drawn up in	89	beats.
50 ,,	left ,,	,,	104	,,
55 ,,	right ,,	,,	108	,,
1 hour	left ,,	,,	172	,,

A still stronger acid, 20 minims to 12 oz., was now used.

In 1 hour 5 minutes,	the right leg	was drawn up in	85	beats.
1 ,, 10 ,,	left ,,	,,	118	,,
1 ,, 15 ,,	right ,,	,,	138	,,
1 ,, 20 ,,	left ,,	,,	140	,,

In 1 hour	25 minutes,	the right	leg was drawn up in		145 beats.	
1 „	30 „	left „	„		158 „	
1 „	35 „	right „	„		160 „	
1 „	40 „	left „	„		180 „	
1 „	45 „	right „	„		197 „	
1 „	50 „	no reflex movement of either after			250 „ *	

Both sciatic nerves were found to be active when directly stimulated, but the contractions were confined to the limb whose nerve was galvanised.

This, and the experiment which precedes it, are conclusive in showing that the diminution, and then destruction, of the diastaltic function are not interfered with when physostigma is only prevented from acting on peripheral portions of the reflex apparatus. When, however, it is permitted to act on the nerve endorgans, but is prevented from reaching the centres, the effect is very different.

Experiment XLII.—(*Temperature of Laboratory,* 53° *F.*)

I opened the abdomen of a frog, and, with great care, tied, and, in some instances, cut through, all the blood-vessels that entered the spinal canal from the lower edge of the scapula to the coccygeal extremity of the sacrum, and then divided the spinal column at the higher of these points. By this means, the blood was only prevented from reaching a limited portion of the cord; so that, though unable to convey physostigma to the reflex centre for the posterior extremities, it could still do so to those extremities themselves, and to all other parts of the body. Voluntary movements of the limbs and of the body, anterior to the divided portion of the cord, occurred when the frog was set free; and irritation of the posterior parts was promptly followed by reflected movements confined to them. The heart, which the operation had partially exposed, was contracting sixty-eight times per minute.

A large dose of extract was administered by the mouth. In twenty minutes, only very faint reflex movements could be excited when the anterior feet were irritated, while gentle stimulation of the posterior caused energetic reflex movements; and the heart was acting at the rate of eighteen per minute. In little more than an hour, the diastaltic function was completely abolished in the anterior half of the body, while the conductivity of the brachial nerves remained; and, still, a slight pinch or weak galvanism of the posterior webs was followed by pretty active reflex movements. The heart had now stopped. *The diastaltic activity of the posterior half of the body continued for two hours after it had disappeared in the anterior.*

The mere prevention of the access of physostigma to a segment of the cord, while it was allowed to act directly on all other parts of the body, had, therefore, the effect of delaying the loss of reflex function in the parts connected with that non-poisoned segment. We have, already, frequently seen that protection of the endorgans and of portions of the nerve trunks from the poison does not delay or at all influence the gradual impairment and final destruction of the reflex function that ensue on its access to the cord. The conclusion is only logical, that Calabar bean produces a destruction of the diastaltic power by an action on the spinal cord.

It is not superfluous to observe that I do not in these experiments ignore the effect on the cord of mere stoppage of the circulation. When the heart is quickly

* In all these experiments, the parts which had been dipped in the acid solution were immediately washed with distilled water. The destructive action of the acid was thereby reduced to a minimum.

paralysed, as frequently happens, reflex movement will, on that account alone, be soon impossible. Experiments in which the heart is so affected are nearly worthless as far as the investigation of the spinal action of physostigma is concerned, and it would be needless to detail them. With the object of bringing forward as clear evidence as possible on this subject, I have selected experiments in which the cardiac action was only impeded, and in which it continued after the abolition of reflex power.

It is well known that strychnia causes an exaggeration of the reflex activity. Whether this is produced by an action on the spinal centre, or by one on the afferent nerves, is yet a question in dispute;* but it is obvious that a substance that diminishes this function at the cord, will also diminish the reflex activity consequent on the administration of strychnia, whatever be the special part of the reflex circle affected by that administration. Such a result is produced by physostigma, and therefore, adds to the many proofs of its spinal action.

Experiment XLIII.

I placed a small drop of solution of strychnia (Brit. Pharm.) on the back of a frog. This produced tetanus in four minutes; when a considerable dose of physostigma extract was inserted into the animal's mouth, the manipulations necessary for which excited a series of violent emprosthotonic spasms. Four minutes after the Calabar bean was exhibited, a decided diminution occurred in the frequency and severity of the convulsions; and, in nine minutes, they had lost their tetanic character. In forty minutes, it was difficult to excite even a faint reflex movement by pretty strong galvanism of any part of the body; and, soon after, reflex action had completely disappeared, even when the exposed sciatic nerves were galvanised.

By comparing the last experiment with one in which the strychnia effects were not interfered with, it is easy to show what part the Calabar bean played in thus interfering with the peculiar action of strychnia.

Experiment XLIV.

A frog was selected of the same weight as the last, and in every other respect as nearly resembling it as possible, and a small drop of solution of strychnia (Brit. Pharm.) was placed on its back. Tetanus occurred in four minutes; and violent convulsions of a tetanic character followed each other at intervals, and could be excited by the slightest touch, during the next six hours, after which the observations were stopped.

This evidence may be still further increased, if we produce well-marked paralysis by physostigma and then administer strychnia without causing its peculiar action.

Experiment XLV.

A frog was selected of the same weight as those employed in the two preceding experiments, and a dose of extract of Calabar bean was inserted under the skin of its back. This acted with considerable rapidity, so that, in twelve minutes, respiratory and voluntary movements had ceased, and reflex action was sluggish. A small drop of solution of strychnia (Brit. Pharm.) was then placed on a wound made through the skin of the back. Twenty minutes after this, reflex movement could still be excited; but during all this time no tetanic convulsion, nor even exaggeration

* MARSHALL HALL, BROWN-SÉQUARD, BONNEFIN, MARTIN-MAGRON and BUISSON maintain that strychnia causes tetanus by an action on the spinal cord; CLAUDE BERNARD and STANNIUS are the principal champions of the opposing theory of its action on the sensory nerves.

of reflex action, had occurred. Thirty-eight minutes after the strychnia had been applied, the diastaltic function had disappeared, though galvanism of the exposed sciatics still caused muscular contractions, and though the heart was beating at the rate of twenty-eight per minute.

The subject of opposing physiological actions has been a favourite one with many writers on poisons, and "antagonistic" effects have been largely discussed, as might be expected from their interest and practical applications. Nicotia,* aconitia† and curare‡ have been proposed as counter agents to strychnia, and atropia has been proposed as one to morphia.§ Calabar bean has been, before now, pointed out by myself and others as an opponent in action to strychnia; and, as with curare, its application to the treatment of tetanus has been recommended. I believe that no other drug so directly diminishes reflex action, and is, therefore, so likely to be employed with advantage in tetanus, as physostigma. Curare opposes spasm by paralysing motor nerves, nicotia by destroying muscular contractility; but physostigma attacks (if we may use the word) the spinal cord which is necessarily implicated as the centre of every diastaltic action. There seems to be no reason why it should not always prove a certain cure in traumatic tetanus. Its success in strychnia poisoning will probably depend on the quantity, in relation to the case, that has been administered; as this poison may be considered to have two fatal doses—a smaller, where death is caused by asphyxia or exhaustion, and a larger, where, even if its tendency thereto by asphyxia or exhaustion shall be averted, it will still certainly occur, by the special action of the poison on the histological structures it attacks.‖ Such a substance as Calabar bean may be employed with advantage to prevent death after the administration of the smaller quantity.

Physostigma has lately been proposed as a physiological antidote for atropia poisoning; and KLEINWÄCHTER has had the courage to employ it for this purpose, principally on the ground of its anti-mydriatic property.¶ As will be afterwards shown, these two substances appear to act in opposite modes on the ganglionic system of the blood-vessels; but the nature of their effects on the cerebro-spinal system is such as to make it irrational to anticipate any success in their employment as counter-agents.

* Rev. SAMUEL HAUGHTON, Dublin Quarterly Journal of Medical Science, August 1862.

† E. WOAKES, British Medical Journal, October 1860, &c.

‡ HARLEY, Lancet, 1856; L. VELLA, Comptes Rendus, 1860; CLAUDE BERNARD (opposes the view of counteraction), Leçons, &c., p. 377.

§ GRAVES, Clinical Lectures on the Practice of Medicine; ANDERSON, Effects of Belladona in Poisoning by Opium, 1854; LOPEZ, American Medico-Chirurgical Review, vol. iv. 1859; Dr W. F. NORRIS, American Journal of Medical Science, Oct. 1862; CAMUS (experimentally disproves this asserted antagonism), Gazette Hebdr., 11 Août 1865, and Canstatt's Jahresbericht, &c., Fünfter Band, 1866, p. 123.

‖ It has been found that frogs, after fatal doses of strychnia, may die *without any convulsions*, if care be taken to protect them from all causes of excitation—MARSHALL HALL, Aperçu du Système Spinal, p. 170; CLAUDE BERNARD, Lectures on Experimental Pathology and Operative Physiology, Medical Times and Gazette, 1860, v. ii., p. 25.

¶ Berliner klin. Wochschr, 38, 1864.

To argue from pupil effects alone of an antagonism between the actions of morphia and atropia, I believe to be absurd, while we know almost nothing of how iridal changes are produced by poisons. We shall never have antidotes to active substances until we can produce within the body chemical changes in their composition of such a nature as shall render them inert. By originating a secondary, and apparently counter, action, we may sometimes ward off death; but only where that would have been due to one of the symptoms of a small dose: we do not prevent the fatal action on the tissues of a large dose; and we run the risk of adding a second active substance, which cannot produce any effect without causing a tissue change, and which may, therefore, hasten and render more certain a previously doubtful, fatal result.

The most conspicuous symptom that is caused by physostigma is paralysis; and this necessarily depends on an effect produced on the nervous system, or on the muscular system, or on both. I believe this investigation is sufficient to show that it is due not to an action on the cerebral lobes, on afferent or efferent spinal nerves, or on muscles, but to one on the spinal cord, as a reflex centre. This spinal affection is the result of a primary and special action of Calabar bean; but it is more or less favoured by a simultaneous depression of the heart's action, as will be more conveniently illustrated in the special examination of the cardiac effects.

Action on the Heart.

The heart is affected in a marked manner by Calabar bean; and this has a more or less direct influence in causing death, according to the dose that may have been exhibited. With a large dose, the animal dies by cardiac syncope. With a smaller one, the heart beats are only diminished in frequency, and, as the circulation continues, the spinal cord is more and more affected, until its diastaltic function is destroyed and asphyxia caused. The latter effect is proved in the previous portion of this investigation, and it will be sufficient for the purpose of illustrating the former to quote one of my already published experiments with warm-blooded animals.

Experiment XLVI.

"The skin was raised in the left flank of a large black-and-white female cat, the needle-point of Wood's hypodermic syringe was inserted into the subcutaneous cellular tissue, and ten minims of a syrupy extract were injected" (equivalent to about four grains of the preparation usually employed in this investigation).

"In two minutes, trembling occurred; and, in three, the cat fell. Fluid escaped from the mouth, the pupils contracted, and urine was voided. In five minutes, the respirations became hurried, noisy and laboured. Reflex action could not be excited by severe stimulation, nor did the eyelids contract on irritation of the conjunctiva. The animal became perfectly flaccid, the only symptom of life was an occasional gasp, and this ceased entirely, seven minutes after the administration.

"*Autopsy*, immediate. The pupils were observed to dilate. A few contractions occurred in the muscles that were cut. *The heart was perfectly quiet, and without the slightest action.* . . . . On removing the pericardium, irregular movements occurred in the heart, and a partial contraction could be produced by irritation, fifteen minutes after death. The vessels of the thorax and abdomen were well filled, and could be readily distinguished by the colour of their contents. On incising the left ventricle, blood of the usual arterial hue escaped; and on incising the right, dark blood appeared. Both were allowed to run side by side, when the contrast was distinctly shown."*

We have now to describe the various changes that are undergone by the heart before its contractions finally cease, and to examine the mechanism by which these changes are produced. For the former purpose, several experiments were performed on frogs whose hearts were exposed before the administration of the poison.

Experiment XLVII.—(*Temperature of Laboratory*, 58° *F.*)

A large frog, which weighed 730 grains, was fixed down on its back in such a manner that the circulation in the limbs was not to any extent impeded; and the heart was exposed by the removal of a portion of the sternum. This operation can easily be performed without injuring any large blood-vessel, and, indeed, without causing any loss of blood further than a temporary oozing from the cut surfaces. A few minutes afterwards, its heart-beats were frequently counted, and found to average seventy per minute. I then injected one grain of extract, mixed with a little water, into each thigh (two grains in all).

5 minutes afterwards,	cardiac contractions	= 64 per min.	
10 „	„	= 58 „	
15 „	„	= 43 „	Respirations ceased, except an occasional gasp.
20 „	„	= 39 „	
25 „	„	= 41 „	No respiratory movements.
30 „	„	= 37 „	
35 „	„	= 24 „	Contractions feeble.
40 „	„	= 22 „	
45 „	„	= 22 „	Skin much darker than originally.
50 „	„	= 19 „	
55 „	„	= 13 „	
1 hour 0 min.	„	= 14 „	
1 „ 5 „	„	= 12 „	Heart equally dark in systole and in diastole.
1 „ 10 „	„	= 10 „	
1 „ 15 „	„	= 9 „	
1 „ 20 „	„	= 9 „	
1 „ 30 „	„	= 10 „	
1 „ 40 „	„	= 10 „	Galvanism of sciatics caused neither direct nor reflex contractions. Muscles dark bluish, and active.
1 „ 50 „	„	= 8 „	Surface of heart opal blue in colour.
2 hours 0 „	„	= 8 „	Cardiac contractions extremely feeble.
2 „ 10 „	„	= 8 „	Skin deep olive-brown: the frog was originally a pale one.
2 „ 30 „	„	= 12 „	
3 „ 0 „	„	= 18 „	Spinal nerves still perfectly paralysed.
3 „ 30 „	„	= 21 „	
4 „ 0 „	„	= 24 „	Cardiac contractions quite synchronous, and diastole prolonged.
4 „ 30 „	„	= 26 „	A faint twitch occurred when the left sciatic was galvanised.

* *Op. cit.* section v. experiment iii.

The frog was now left in a cold and moist place until the following morning, when it was found jumping about actively, with its heart contracting forty-eight times per minute. It continued in very much the same condition for other two days, when it was killed.

This experiment is especially interesting because of the very near approach to death that was made. A decided effect was produced on the heart's action, as it was reduced in frequency by from seventy to eighty beats in the minute: respiratory movements were stopped; reflex spinal action was completely prevented; and the spinal motor nerves were, for many minutes, paralysed: and yet the animal revived; and regained all its lost functions except that of the heart, which only partially recovered itself. Such a result could never have been obtained with a warm-blooded animal, as death would soon have been produced by asphyxia, however long the heart might continue to contract. It is well known that the frog may live for many days after the removal of its lungs, as the respiratory function of those organs is shared in by the skin of this animal. The effects of the poison that was given in this case—and the dose was a very small one in proportion to the weight of the frog—had gradually disappeared, and the different tissues had returned to their former vitality, after having been acted upon for some time by a nearly normal blood-stream. Had the circulation ceased, or had the effects on the tissues been greater, and, therefore, more permanent, this return from pseudo-death could not have occurred.

Experiment XLVIII.—(*Temperature of Laboratory,* 58° *F.*)

The heart of a frog, weighing 396 grains, was exposed. After allowing a few minutes for recovery from shock, the number of the cardiac contractions was determined at intervals of five minutes, during twenty minutes, and found to vary little from forty-two beats per minute. One grain of extract, in a few drops of water, was then injected into the subcutaneous tissue of each thigh (two grains in all).

2 minutes afterwards,	cardiac contractions	= 36 per min.	
4 „	„	= 24 „	
6 „	„	= 20 „	Contractions seem feeble, and heart is not so pale during systole as is normal.
8 „	„	= 17 „	
10 „	„	= 17 „	
12 „	„	= 15 „	Heart walls seem nearly as dark during systole as during diastole.
14 „	„	= 14 „	
16 „	„	= 14 „	
18 „	„	= 15 „	Frog is helpless and flaccid.
20 „	„	= 14 „	
25 „	„	= 14 „	Almost no difference of colour during systole, and a great prolongation of diastole.
30 „	„	= 12 „	
35 „	„	= 13 „	
40 „	„	= 12 „	
45 „	„	= 8 „	The ventricular contractions seem more feeble than the auricular.

50 minutes afterwards, cardiac contractions = 8 per min.

1 hour „ = 8 „ The ventricular contraction is scarcely perceptible.

1 „ 5 min. „ are irregular; there being three ventricular contractions per minute, and six auricular.

This irregularity continued for other ten minutes, and then became greater, only one contraction per minute, of the most feeble character, occurring in the ventricles for two and sometimes three in the auricles. During the period of inaction, the heart rested in diastole.

1 hour 20 minutes after poisoning, cardiac contractions = 0 per min.

1 „ 40 „ „ „ = 8 „ All the chambers contract, with long intervals, and in a vermicular manner.

1 „ 50 „ „ „ = 12 „
2 hours „ „ = 12 „
2 „ 10 „ „ „ = 12 „
3 „ 15 „ „ „ Again irregular, and exactly as at one hour and five minutes after the poisoning.

3 „ 40 „ „ „ The irregularity was still further increased, the ventricles being occasionally quite motionless, in a dilated condition, for one minute, while both auricles contracted six and sometimes eight times. The contractions consisted merely of feeble wave-like movements of the different chambers. The surface of the heart, and, still more, the pericardium had for some time assumed a blue colour.

When the heart was again examined, twenty-two hours after poisoning, there was no spontaneous movement. It was then gently stimulated by an interrupted galvanic current, and a very feeble contraction of all the chambers followed, which did not repeat itself, but which could be reproduced by a renewal of the stimulation. For some time after these irritations, feeble and irregular contractions occasionally occurred, one ventricle contracting, and, after several minutes, two or three auricular movements following, but with considerable pauses. Forty-eight hours after the administration, the heart was motionless, dark and dilated; gentle galvanism produced no effect, except that the ventricles became rather paler, but a moderate current caused a contraction of all its chambers, succeeded by perfect quiet.

In sixty hours, the heart was pale and quiet, and no movement could be produced by galvanism. Notwithstanding the paleness of its walls, the heart was not contracted, as its chambers contained a considerable quantity of dark, fluid blood. The colour of its walls was, therefore, due to contraction of their capillaries, caused either by the rigor of death, or, as I am more inclined to believe, by the galvanic stimulation they had been so frequently subjected to. At this time, all the muscles were stiff.

Experiment XLIX.—(*Temperature of Laboratory*, 57° *F.*)

The average number of cardiac contractions in a frog, weighing 415 grains, was fifty-five per minute. Two grains and a-half of extract, in fifteen minims of distilled water, were injected into the subcutaneous tissue of each thigh (five grains altogether).

2 minutes afterwards, cardiac contractions = 52 per min.
4 „ „ = 50 „
6 „ „ = 24 „
8 „ „ = 19 „
10 „ „ = 15 „
12 „ „ = 14 „ No chest respiratory movements.

14 minutes afterwards,	cardiac contractions	= 14 per min.	Systole not quite so pale as originally.
16 „	„	= 13 „	Frog is quite flaccid; it cannot turn from its back, but occasional faint struggles occur.
18 „	„	= 11 „	Contractions very feeble.
20 „	„	= 10 „	Heart as dark during contraction of the ventricles as during dilatation of them.
22 „	„	= 8 „	Irregular and non-synchronous; the auricles sometimes contractting nine or eleven times per minute.
24 „	„	= 8 „	of ventricles, 10 of auricles.
26 „	„	= 8 „	„ 14 „
28 „	„	= 7 „	„ 14 „
30 „	„	= 8 „	„ 14 „
32 „	„	= 0	during one minute, and then a few feeble contractions in the auricles, and again a pause.
34 „	„	= 0 per min.	
36 „	„	= 0 „	
38 „	„	= 0 „	of ventricles, 3 of auricles.
40 „	„	= 8 „	„ 12 „
42 „	„	= 8 „	„ 16 „
45 „	„	= 8 „	„ 16 „
50 „	„	= 9 „	Synchronous.
55 „	„	= 9 „	Skin extremely dark.
1 hour 0 min.	„	= 8 „	No reflex movement on stimulation. Galvanism of the sciatic nerves causes faint twitches of the toes.
1 „ 10 „	„	= 8 „	
1 „ 15 „	„	= 8 „	
1 „ 20 „	„	= 8 „	All the spinal nerves are paralysed; the muscles remaining active and blue.
2 hours 0 „	„	= 8 „	
3 „ 0 „	„	= 8 „	

The observations were now interrupted until twenty hours after the exhibition of the poison, and, at this time, the frog was quite flaccid and dark; the heart was contracting sixteen times in the minute, and the striped muscles were irritable, but otherwise the animal was quite dead. Twenty-eight hours after the exhibition of Calabar bean, the cardiac contractions were seventeen per minute; and at thirty hours they were ten. In forty hours, the heart was found perfectly still, dark and somewhat dilated. Direct galvanism produced a slow contraction of the portion of cardiac muscle included in the circuit; it did not excite a normal heart beat. The striped muscles contracted very sluggishly when galvanised. This idio-muscular contractility was retained in the heart until sixty-nine hours, and in the striped muscles till seventy hours after the administration of the poison. They then became pale, stiff and acid.

In this experiment, a large proportion of the extract had not been absorbed, but escaped from the thighs when the skin was incised to allow the sciatic nerves to be exposed. The effects cannot, therefore, be regarded as those produced by five grains, but must be held to have been caused by a much smaller quantity. Indeed, where three grains were absorbed by a frog of nearly the same weight, the action on the heart was much more decided and marked.

Experiment L.—(*Temperature of Laboratory*, 60° *F.*)

The exposed heart of a frog, weighing 469 grains, was found to contract, on an average, sixty-seven times in the minute. One grain of extract, in five minims of water, was then injected into each thigh, and a third grain was injected into the stomach by means of a narrow caoutchouc tube (total three grains).

2 minutes afterwards,	cardiac contractions	=	60	per min.	
4 ,,	,,	=	54	,,	
6 ,,	,,	=	34	,,	
8 ,,	,,	=	30	,,	
10 ,,	,,	=	21	,,	Very feeble. Heart not quite so pale, now, in systole.
12 ,,	,,	=	13	,,	
14 ,,	,,	=	11	,,	Frog jumped about when set free, but soon fell on its back, and remained there.
16 ,,	,,	=	8	,,	
18 ,,	,,	=	0	,,	Colour of frog has changed from yellowish brown to dark bronze.
20 ,,	,,	=	0	,,	
22 ,,	,,	=	0	,,	of the ventricles, 2 of the auricles.
24 ,,	,,	=	0	,,	
28 ,,	,,	=	0	,,	
30 ,,	,,	=	0	,,	

After this, an occasional auricular contraction occurred, but no spontaneous movement was seen of the ventricles. The heart rested in diastole, and was of a dark colour with a bluish tinge. For twenty minutes after this, a slight stimulus excited a few rhythmical contractions, followed, in a few seconds, by rest in diastole, but capable of being reproduced in the same way. One hour and ten minutes after the administration, no reflex movement could be excited; but the sciatic nerves continued active for longer than thirty hours—the rapid stoppage of the circulation having prevented that prolonged contact with the poison that is necessary for the paralysis of the endorgans. The striped muscles remained contractile until more than forty-eight hours, and contractility was also retained by the cardiac muscle during this period.

A possible source of fallacy is introduced into these experiments by the operations that they require. It seemed, therefore, of some importance to test the effects on the frog's heart of mere exposure to the air. For this purpose, a portion of the sternum was removed from an active frog, in exactly the same manner as in the last four experiments, with the following result:—

5 minutes afterwards,	cardiac contractions	=	62.
10 ,,	,,	=	61.
20 ,,	,,	=	62.
30 ,,	,,	=	59.
40 ,,	,,	=	60.
50 ,,	,,	=	59.
1 hour 0 minutes,	,,	=	55.
1 ,, 30 ,,	,,	=	57.
2 ,, 0 ,,	,,	=	60.
2 ,, 30 ,,	,,	=	56.
3 ,, 0 ,,	,,	=	58.
3 ,, 30 ,,	,,	=	59.
4 ,, 0 ,,	,,	=	59.

It is almost needless to remark that the change in the systolic colour of the heart, from pale to dark, did not occur. The heart's surface was prevented from drying by an occasional drop of water; and this was also done during the experiments in which physostigma was administered.

We have now sufficiently described, as proposed (p. 743), "the various changes that are undergone by the heart before its contractions finally cease;" and from the data given, these may be summarised in their order of occurrence as follows:—

1st, Diminution never preceded by increase, of the frequency of the contractions, with prolongation of the period of rest; 2d, Feebleness of the contractions, with no change of colour on the occurrence of systole; 3d, Irregularity of rhythm, the auricles contracting more frequently than the ventricles, and, for intervals, contracting alone; 4th, Stoppage of all the heart's chambers—If the poison be absorbed quickly and in large quantity, the fifth and sixth effects may not occur; 5th, Renewal of contractions, either by all the chambers at once, or by one or more in the first place; 6th, Gradual recovery to a low rate of action, and continuance at this for from a few minutes to several days; 7th, Stoppage *in diastole* of spontaneous contractions; and 8th, Loss of the idio-muscular irritability of the heart, rigor and change of reaction from alkaline to acid.

This method of affecting the heart distinguishes physostigma from the great majority of cardiac poisons, which may be well represented by *Antiaris toxicaria,** *Tanghinia venenifera,*† *Digitalis,*‡ *Helleborus niger*, *H. viridis*§ and the green resin obtained from *Nerium Oleander.*‖ These produce first irregularity and acceleration of the heart's action, then a diminished frequency, *caused by protraction of the ventricular systole,* and, finally, stoppage of the contractions by "*cessation of the dilatation of the ventricles,* which then remain contracted, white and perfectly empty."¶ In producing cardiac paralysis, physostigma acts in a manner exactly the reverse. It causes no acceleration, it diminishes the frequency of the contractions *by prolonging the ventricular diastole,* and it produces the final stoppage *by cessation of the contraction of the ventricles,* which then remain dilated, dark and full of blood. Very small doses of digitaline and the alcoholico-aqueous extract of *Nerium Oleander* are said to act on the heart in a manner which seems to resemble closely that of Calabar bean,** but no other cardiac poisons appear to share in its peculiarities.

It now remains that we examine the mechanism by which these changes are produced, and endeavour to determine what tissues or structures are influenced by physostigma to effect them. For this purpose, it will be necessary to investi-

* Kölliker, Vulpian, Claude Bernard and others, in various papers.

† M. Eug. Pélikan et Dr Dybkowski; Recherches physiologo-toxicologiques sur l'action de quelques poisons du cœur; and Comptes Rendus, 1865, p. 1209.

‡ *Ibid.* § *Ibid.* ‖ *Ibid.*

¶ On the Application of Physiological Tests for Certain Organic Poisons, and especially Digitaline, by C. Hilton Fagge, M.D., and Thomas Stevenson, M.D. Guy's Hospital Reports, 3d series, 1866, vol. xii. p. 47.

** Nouvelles Recherches sur le poison du *Nerium Oleander.* Note de M. Eug. Pélikan. Comptes Rendus, 1866, p. 237.

gate the possible influences of the cerebro-spinal nervous system, whether exerted through the vagi or through the spinal nerves, and the possible influences of the sympathetic system, whether exerted through the great sympathetic trunks and their branches or through the ganglia contained in the heart's substance. Any effect on idio-muscular contractility has been already abundantly disproved; but it will be necessary to observe how far the impairment and cessation of respiration may explain the cardiac effects in warm-blooded animals.

The paralysis of the heart in diastole and the diminution in the frequency of its contractions by protracted periods of rest in a *dilated condition*, as well as the frequent renewal of its action after a long pause in diastole, might, in the first place, suggest that the inhibitory function of the vagi nerves was being exerted. On this account, it may be advisable to examine their condition during Calabar bean poisoning.

Experiment LI.

An active frog was selected, of the weight of 863 grains, and its heart and two vagi nerves were exposed. The latter were separately tested by galvanism, and each produced stoppage of the heart's action, in diastole. A few minutes afterwards, the average of the heart's contractions was ascertained to be fifty-eight in the minute. Three grains of extract, in a few drops of water, were injected into the subcutaneous tissue of the two thighs—one-half into each.

2 minutes afterwards,	cardiac contractions = 54 per min.	
4 "	" = 47 "	
6 "	" = 40 "	
8 "	" = 37 "	
10 "	" = 36 "	
12 "	galvanism of the left vagus produced stoppage in diastole.	
14 "	cardiac contractions = 34 per min.	
16 "	" = 33 "	Contractions feeble.
18 "	" = 30 "	Heart dark in systole, as well as in diastole.
20 "	" = 24 "	
25 "	" = 20 "	
30 "	" = 19 "	
35 "	galvanism of the left vagus produced stoppage for one minute.	
40 "	cardiac contractions = 15 per min.	
45 "	" = 18 "	
50 "	" = 14 "	Marked pause between auricular and ventricular contractions.
1 hour 0 min.	" = 12 "	
1 " 5 "	galvanism of the left vagus produced stoppage of all the chambers.	
1 " 10 "	cardiac contractions were irregular, on being renewed; only twelve ventricular now occurring to eighteen auricular, per minute.	
1 " 20 "	cardiac contractions continue, with the above irregularity.	
1 " 30 "	cardiac contractions still irregular; galvanism of the left vagus produced stoppage of all the chambers, but a stronger current was required than before.	
1 " 32 "	galvanism of either sciatic nerve caused movements confined to the limb whose nerve was stimulated.	
1 " 40 "	cardiac contractions continue irregular, as above.	

1 hour 50 min. afterwards,	galvanism of the left vagus produced stoppage of all the chambers; and the heart remained at rest, in diastole, for ten minutes, when a feeble auricular contraction occurred, and, soon after, a ventricular.
1 " 55 "	galvanism of either sciatic nerve caused merely faint twitches of the toes.
2 " 10 "	cardiac contractions = 18 per minute; very feeble, but rhythmical.
2 " 15 "	strong and continued galvanism of the left vagus produced no effect on the cardiac contractions.
2 " 18 "	strong and continued galvanism of the right vagus, which had not been irritated during the experiment, produced no effect on the cardiac contractions.
2 " 20 "	strong galvanism applied to either sciatic nerve produced no effect.
2 " 40 "	cardiac contractions stopped. Irritation could produce a few contractions for only a few minutes longer.

From this experiment it is apparent that the vagi retain their inhibitory power over the heart during the whole period that its action is being modified by physostigma. Ultimately, however, they are themselves paralysed, as might be anticipated from the analogies that exist between them and the spinal nerves; and, as this and many other similar experiments prove, the functions of the vagi and of the spinal motor nerves are lost simultaneously, or nearly so.

To illustrate this in warm-blooded animals, it is necessary to exhibit so small a dose of the poison that death shall be caused by asphyxia, and the heart afterwards continue to contract, however irregularly.

Experiment LII.

Half a grain of extract, suspended in ten minims of distilled water, was injected under the skin on the back of a full-grown and active rabbit. The usual symptoms followed; and, in thirty-four minutes, the animal was dead, all respiratory movement having ceased. Immediately after wards, the heart was exposed; and it was found contracting in normal rhythm, twenty-two times in the minute. The right vagus was divided; and the end proximal to the heart was galvanised, with the effect of producing an immediate stoppage, in diastole, for several seconds. The vagi were tested, occasionally, during twenty minutes after the rabbit's death, and their cardiac inhibitory function, as well as their excito-motory power over the stomach and œsophagus, continued active all this time; and, for the same period, the sciatic, intercostal, phrenic and other spinal nerves retained their motor conductivity. Twenty-two minutes after death, the heart was contracting eight times per minute. Galvanism of either vagus could now neither stop the cardiac action nor excite œsophageal or gastric movements. The sciatic and other spinal nerves were tested at twenty-five minutes after death, and found to be perfectly paralysed.

It is, therefore, quite possible, as far, at least, as conveyance by the vagi nerves is concerned, for Calabar bean to act on the heart by exciting the cardiac inhibitory centre in the medulla oblongata. But, if this be the method of its action, the prevention of this possible influence, by division or previous paralysis of the vagi, or by destruction of the medulla oblongata, should render it impossible for Calabar bean to produce its usual effects on the heart.

Experiment LIII.

The heart and the two vagi nerves were exposed and the latter divided in a frog, weighing 700 grains, and, a few minutes later, the cardiac contractions were found to have an average of sixty-six in the minute. Two grains of extract, in ten minims of distilled water, were then injected into each thigh (altogether four grains).

2 minutes afterwards,	cardiac contractions	= 60	per min.	
4 ,,	,,	= 60	,,	
6 ,,	,,	= 53	,,	
8 ,,	,,	= 46	,,	
10 ,,	,,	= 39	,,	
12 ,,	,,	= 31	,,	
14 ,,	,,	= 27	,,	
16 ,,	,,	= 24	,,	
18 ,,	,,	= 22	,,	
20 ,,	,,	= 18	,,	
22 ,,	,,	= 16	,,	All the chambers are of dark colour, even in systole.
24 ,,	,,	= 13	,,	
26 ,,	,,	= 14	,,	
28 ,,	,,	= 12	,,	Ventricular contraction is extremely feeble.
30 ,,	,,	= 10	,,	
35 ,,	,,	= 9	,,	
40 ,,	,,	irregular; six ventricular for twelve auricular.		
42 ,,	,,	stopped in diastole, for thirty-five seconds.		

The heart then resumed its previous unrhythmical action of six ventricular to twelve auricular movements in the minute, and continued to contract, with various changes of irregularity, for many hours longer.

It would needlessly occupy space, were I to narrate any other of the many experiments that were performed with frogs whose vagi had been divided; as the results, and even the details, were in all of them very much the same. It might be proper to instance, at this place, an experiment of the same character on a warm-blooded animal, but I content myself by referring to Experiment LXI., at page 763.

In curare we possess an agent that, within a few minutes after its exhibition, produces complete paralysis of all the motor nerves, including the vagi.* It will, therefore, act as effectually as division, in preventing any inhibitory influence being exerted on the heart.

Experiment LIV.

A small dose of curare was inserted under the skin of a frog's back. Ten minutes afterwards, the animal was perfectly paralysed. At twenty minutes, its heart was exposed, and observed to be contracting fifty-four times in the minute. The vagi and sciatic nerves were tested by galvanism, and their conductivity was found to be completely destroyed. Three grains of extract of physostigma, in water, were injected into the two thighs. The usual cardiac effects were produced within the time that might have been expected, from such a dose; irregularity of the rhythm having occurred in forty-four minutes, and final paralysis, with all the chambers dark, full and dilated, in a few minutes later.

* Claude Bernard, Leçons sur les effects des Subs. Tox. &c., p. 352; Kölliker, *loc. cit.*; Martin-Magron, Journal de la Physiologie, 1859, p. 649, &c.

These are sufficient to prove that Calabar bean influences the heart neither by an action on the inhibitory centres nor by one on the nerves that connect those centres with it. I think they are also sufficient to exclude the rest of the cerebro-spinal nervous system: as the only other spinal nerves connected with the heart, by the branches of the great sympathetic trunks or otherwise, are either sensory, or at any rate afferent, nerves that form through the cord reflex arrangements with the vagi and vaso-motor nerves; or excito-motory ones whose action is to increase the frequency of the heart's contractions, but whose function is only periodically exerted, and is quite unnecessary for the continuance of the ordinary rhythmical occurrence of those contractions.* But, to remove the possibility of any doubt on this point, the following experiment was performed:—

Experiment LV.

The spinal cord was divided between the occiput and first vertebra of a frog, weighing 376 grains, and a wire was passed down the spinal canal, so as to produce complete paralysis, and into the cranial cavity, so as thoroughly to break up and destroy the brain. A short time thereafter, the heart was contracting at the rate of forty beats per minute. One half of three grains of extract, in twenty minims of water, was injected into each thigh.

2	minutes afterwards,	cardiac contractions	= 37	per min.
4	„	„	= 31	„
6	„	„	= 28	„
8	„	„	= 26	„
10	„	„	= 25	„
12	„	„	= 24	„
14	„	„	= 22	„
16	„	„	= 20	„
18	„	„	= 17	„
20	„	„	= 14	„
22	„	„	= 13	„
24	„	„	= 12	„

* Dr POWER observes, in the sixth edition of CARPENTER's "Principles of Human Physiology" (page 217, note), that "the essential cause of the rhythmical action of the heart must still remain an unsolved question." The exact influence of the various nerves that connect the heart with the central nervous systems, appears to be quite as imperfectly ascertained, judging by the contradictory statements and deductions of eminent physiologists. LEGALLOIS and PHILIP WILSON, and, afterwards, BUDGE, SCHIFF, REID, WEBER, MOLESCHOTT, VON BEZOLD and others, have shown that a connection certainly exists; but they have left the details of the question unsettled by the great differences in many of their opinions, as, for example, on the cardiac functions of the vagi. VON BEZOLD, in 1863, attempted to prove the existence, in the spinal cord, of an excito-motory centre, whose stimulation increases not only the number of the beats, but also the blood-pressure—the latter being due to augmented force in the heart's contractions. LUDWIG and THIRY opposed this opinion, and asserted that the increased blood-tension is really an effect of excitation of the vaso-motor nerves. In a recent investigation (Comptes Rendus, 25 Mars 1867), MM. E. and M. CYON give their adherence to the views of LUDWIG and THIRY. They also attempt to show that the spinal cord, through the sympathetic system, supplies the heart with nerves that possess the power of directly accelerating its contractions, and that are antagonistic to the vagi, in that, while the latter diminish the frequency and increase the force of the contractions, the spinal "nerfs accélérateurs," on the other hand, increase the frequency and diminish the force.—*April* 1867.

26 minutes afterwards, cardiac contractions	= 9 per min.		
28 ,,	,,	= 9 ,,	Very feeble. Heart dark during systole.
30 ,,	,,	= 7 ,,	
35 ,,	,,	irregular; four ventricular for six auricular.	

Irregular and unrhythmical contractions continued for many hours afterwards. The final stoppage was in diastole.

It may now be useful to examine what connection exists, as cause and effect, between the impairment and cessation of the respiratory movements and the interference with, and stoppage of, the cardiac contractions, especially as so deservedly distinguished a physiologist as HARLEY has asserted that Calabar bean is a respiratory poison purely, which causes death by destroying the conductivity of the motor nerves of respiration. CHRISTISON, in his investigation, was the first to observe an action on the heart, and he believes that death is caused by paralysis of that organ. My former results were, so far, in accordance with this statement; but they also show that death may often be due to asphyxia, and while I agree with HARLEY in this, I believe him to be in error when he asserts that paralysis of the motor nerves is the cause of such death. The data that have already been given are sufficient to prove that motor nerve conductivity is always retained, in warm-blooded animals as well as in cold, for many minutes after the complete stoppage of respiratory movements, and that such stoppage is due to destruction of the reflex and co-ordinating functions of the medullæ spinalis and oblongata. To complete this evidence, it will be sufficient to show that no connection of cause and effect necessarily exists between the impaired respiratory movements and the cardiac paralysis.

Experiment LVI.

In a large retriever dog, it was found that the mean number of respirations was ten, and the mean number of cardiac contractions 126, during seven minutes immediately preceding the injection of six grains of extract, suspended in water, into the right jugular vein.

1 min.		after the injection, the respirations	= 10,	cardiac contractions	= 78 per min.
1 ,,	30 sec.	,, ,,	= 11,	,,	= 54 ,,
2 ,,	2 ,,	,, ,,	= 9,	,,	= 40 ,,
3 ,,	0 ,,	,, ,,	= 9,	,,	= 8 ,,
7 ,,	0 ,,	,, ,,	= 10,	,,	= 20 ,,
9 ,,	0 ,,	,, ,,	= 10,	,,	= 16 ,,
10 ,,	0 ,,	,, ,,	= 9,	,,	= 9 ,,
10 ,,	30 ,,	,, ,,	= 0,	,,	= 0 ,,

This experiment gives the result that, in one minute and thirty seconds after the poison was administered, the number of cardiac contractions had fallen to less than one-half, while the respiratory movements had increased by one per minute; and it distinctly shows the absence of any respiratory change to cause the marked effects that were produced on the heart's action.

Experiment LVII.

A frog, weighing 460 grains, had its heart exposed by removing a small portion of the sternum. It was acting at the rate of forty-eight beats per minute, while the respirations were seventy-two. Five minutes afterwards, the heart was contracting at the rate of forty-five per minute, while the respirations were seventy-four. One grain and a half of extract, suspended in water, was injected under the skin of each thigh (three grains in all).

In 5 min., heart	= 36 per min.		Respirations = 74 per min.
10 ,, ,,	= 28 ,,		,, = 64 ,,
15 ,, ,,	= 22 ,,		,, = 63 ,,
20 ,, ,,	= 14 ,,	Contractions weak; and heart continues dark during systole.	,, = Frequent gasping movements, which cannot be counted.
25 ,, ,,	= 13 ,,		,, =
30 ,, ,,	= 12 ,,		,, An occasional gasp.
40 ,, ,,	= 10 ,,		,, have stopped.
50 ,, ,,	= 10 ,,		
1 h. 0 ,, ,,	= 8 ,,	Contractions very feeble.	
1 ,, 10 ,, ,,	= 8 ,,		
1 ,, 15 ,, ,,	= 0 ,,	Stopped in diastole.	
1 ,, 20 ,, ,,	= 0 ,,		
1 ,, 30 ,, ,,	= 12 ,,		
1 ,, 40 ,, ,,	= 12 ,,		
1 ,, 50 ,, ,,	= 12 ,,		
2 ,, 0 ,, ,,	= 10 ,,		
2 ,, 30 ,, ,,	= 8 ,,	Contractions irregular; two auricular for one ventricular.	

These two experiments prove distinctly that Calabar bean has a direct influence on the heart, that is quite independent of the indirect influence it exerts on that organ by arresting the respiratory movements. Such arrest does, doubtless, assist the action on the heart, especially during the later stages of the poisoning, by impeding the circulation; and in mammals, when a small dose has been exhibited, such comparatively slight diminution as is first produced in the frequency of the heart's contractions must even be partly caused by the early retardation and cessation of the respiratory movements that constantly occur.

The cardiac action of physostigma is, thus, quite independent of the cerebro-spinal nervous system, and is not a mere effect of the paralysis of respiration. It must, therefore, be caused by an action of a direct nature on the cardiac ganglia, which seem to be the only constant exciters of this organ, however its contractions may be *regulated* by other nerves. The peculiar changes that the heart's action undergoes—the diminution in the frequency of its beats, then their stoppage or irregularity, sometimes followed by renewal of rhythmical contractions, or of independent movements in all the chambers, or in one only—prove that Calabar bean first diminishes the vitality of the exciting ganglia, and then paralyses them. Their influence is, at any rate, maintained until spontaneous movements cease; for, if we divide the ventricles from the auricles at a late stage of the poisoning and when the contractions are unrhythmical, those chambers alone that are still in connection with the great exciter ganglia in the auriculo-ventricular septum will continue their spontaneous movements.

The action is not a very powerful one, and its characteristics may be explained because of that; for the effects of physostigma on the heart appear to be similar to those of *weak forms* of at least two of the ordinary cardiac poisons—of digitaline in minute doses, and of the alcoholico-aqueous extract of *Nerium Oleander*, which is merely the green extract mixed with various impurities, without which it has the ordinary actions of the larger class of those substances that affect the heart.*

ACTION ON THE BLOOD-VESSELS.

The question of the action of Calabar bean on the condition of the vascular system is intimately connected with that of the cardiac effects; and must, therefore, be considered also, that we may complete the examination of the influence of this substance on the circulation. For this purpose, I instituted two sets of experiments. In the first (1), the blood-tension in the arterial and venous systems was observed; and in the second (2), the calibre-changes of the smaller blood-vessels and of the capillaries were investigated.

1. *Examination of the Blood-Tension, and of the Coincident Changes in the Cardiac and Respiratory Movements, and in the Temperature.*

In these experiments, the tension in the arterial system was determined by dividing one of the carotid arteries and connecting the end proximal to the heart with a modification of Poisseuille's hæmadynamometer, in which two indicating columns were connected with the reservoir. The tube of one of these had an extremely small orifice where it dipped into the mercury, and it, therefore, registered the mean pressure only. The orifice of the other had the same diameter as the rest of the tube, and the contained mercury had, therefore, immediately communicated to it every change of pressure, and oscillated synchronously with the heart's beats. The venous pressure was ascertained by a simple hæmadynamometer having one registering column. The indicating columns were divided into inches and tenths of inches.

In the experiments where the temperature was observed, a delicate thermometer was inserted into the subcutaneous tissue at the flank of the animal, and retained there, under the charge of an assistant, during all the time of the experiment.

* Pélikan, *op. cit.*, Comptes Rendus, 1866.

a. *Arterial Tension only.*

Experiment LVIII.

A large and vigorous retriever dog was placed on a table, and tied down by its four limbs.*

Time.	Time after Adminis-tration.	Mean Pressure.	Pressure Oscil-lates between	No. of Cardiac Contrac-tions.	No. of Respira-tions.	Notes of Operations and of Symptoms.
H. M. S.	M. S.			Per Min.	Per Min.	
3 16 0 P.M.	...	...	...	...	20	The right carotid artery was exposed and connected with the hæmadynamometer.
3 16 30	...	6·8	5·2 & 9·0	...	...	
3 19 0	...	7·0	5·0 9·0	86	18	
3 20 30	...	6·9	5·3 9·3	...	18	
3 22 0	...	7·2	5·3 9·3	108	19	
3 24 0	...	7·0	5·1 8·9	...	...	
3 26 20	...	...	...	...	...	Two grains of extract, in forty minims of distilled water, were injected into the subcutaneous tissue of the abdomen.
3 26 30	...	6·8	5·5 9·0	102	...	
3 26 50	...	...	...	...	...	The injection of the poison was completed.
3 29 0	2 10	6·9	4·9 9·2	93	19	Dog is perfectly quiet.
3 33 0	6 10	6·8	4·5 9·2	90	18	A few struggles occurred.
3 35 0	8 10	6·7	4·9 8·5	75	17	
3 38 30	11 40	7·5	5·0 10·0	90	19	Somewhat violent struggles.
3 41 0	14 10	7·8	5·5 9·3	90	15	Muscular twitches over nearly all the body.
3 41 30	14 40	8·0	...	...	17	
3 42 0	15 10	8·4	7·0 10·0	...	21	Muscular twitches have increased in violence.
3 43 0	16 10	8·9	7·0 10·0	...	22	
3 44 0	17 10	8·8	7·5 10·4	...	...	
3 45 0	18 10	8·5	7·0 9·3	106	...	
3 46 0	19 10	8·3	7·0 9·3	104	31	
3 48 0	21 10	8·2	6·5 9·5	98	38	Twitches are now much feebler; great lachrymation and salivation.
3 50 0	23 10	8·15	7·5 9·5	...	9	
3 52 0	25 10	7·9	6·5 9·4	96	40	
3 55 0	28 10	7·65	6·5 9·9	96	37	
3 57 0	30 10	7·55	6·5 8·6	...	30	Urine and fæces passed.
3 58 0	31 10	...	...	...	34	Respirations noisy, from mucus in the trachea.
3 59 0	32 10	7·3	6·0 8·5	100	28	Very fluid fæces passed.
4 0 0	33 10	7·2	6·3 8·5	112	25	
4 3 30	36 40	7·1	6·0 8·0	112	22	The twitches have very much diminished in force.

* I have much pleasure in acknowledging the valuable assistance I derived in this series of experiments from my friend, Dr GAMGEE. I am also indebted, for essential aid in all or several of them, to Dr BRUNTON, and to Messrs PATON, RITCHIE, FINLAY, CATON, HOGG, HOLDEN, WRIGHT, HARDIE, GAIRDNER, GLASCOTT, LUCAS, M'EWAN, HOWIESON, CROMBIE and YOUNG. Without the co-operation of these gentlemen, it would have been quite impossible to obtain the many simultaneous observations which are contained in these experiments, and to which much of their value is due.

Experiment LVIII.—continued.

Time.	Time after Adminis-tration.	Mean Pressure.	Pressure Oscil-lates between	No. of Cardiac Contrac-tions.	No. of Respira-tions.	Notes of Operations and of Symptoms.
H. M. S.	M. S.			Per Min.	Per Min.	
4 6 0 P.M.	39 10	7·0	...	...	24	
4 7 0	40 10	6·9	6·0 & 7·8	116	23	
4 10 0	43 10	6·8	5·8 7·5	120	22	The twitches are now few, faint and occasional.
4 16 0	49 10	6·8	5·6 7·3	128	23	
4 17 30	50 40	6·7	5·8 7·3	...	22	The dog appears almost perfectly well.
4 19 0	...	6·9	5·5 7·5	124	...	It was deemed advisable to administer a second dose at this stage.
4 22 0	...	...	...	...	...	Commenced the injection of four grains of extract, in thirty minims of distilled water, into the subcutaneous tissue of the right flank.
	Time after Second Administration.					
4 22 30	...	...	...	122	21	The injection was completed.
4 23 0	0 30	6·8	5·8 7·5	...	22	
4 24 0	1 30	6·7	6·0 7·5	...	23	
4 27 0	4 30	7·0	5·8 7·5	120	22	Strong muscular twitches have reappeared.
4 28 0	5 30	6·75	6·0 8·0	116	21	Liquid fæces passed, and urine "jetted" out in a full and abundant stream.
4 30 0	7 30	7·0	...	...	21	
4 30 30	8 0	7·35	...	...	...	
4 31 0	8 30	6·9	6·3 8·3	120	...	Respirations are rather laboured.
4 33 0	10 30	6·9	6·0 8·0	...	22	
4 34 0	11 30	7·1	6·0 8·0	118	...	
4 35 0	12 30	7·2	6·3 8·0	...	...	
4 37 0	14 30	7·4	6·0 8·0	...	23	
4 38 0	15 30	7·3	6·5 8·3	116	22	
4 42 0	19 30	7·6	6·0 8·3	120	24	Twitches are so strong as to cause frequent slight spasms.
4 44 0	21 30	7·15	6·4 7·8	112	25	
4 46 30	24 0	7·2	6·5 8·5	...	...	Urine and fæces discharged copiously.
4 47 0	24 30	7·5	6·5 9·5	...	23	
4 48 0	25 30	7·8	6·0 11·0	92	26	
4 49 30	27 0	7·3	5·6 10·0	30	...	
4 50 0	27 30	7·1	...	...	...	
4 50 15	27 45	6·9	...	...	...	
4 50 30	28 0	6·7	5·0 8·5	...	23	
4 50 31	28 1	6·2	...	...	...	
4 50 35	28 5	5·9	...	...	...	
4 50 38	28 8	5·8	...	...	...	
4 50 40	28 10	5·4	4·0 7·0	40	...	
4 51 0	28 30	5·4	4·0 4·7	...	...	The twitches continue with considerable strength.
4 51 30	29 0	6·9	...	...	25	

Experiment LVIII.—continued.

Time.	Time after Second Adminis-tration.	Mean Pressure.	Pressure Oscil-lates between	No. of Cardiac Contrac-tions.	No. of Respira-tions.	Notes of Operations and of Symptoms.
H. M. S.	M. S.			Per Min.	Per Min.	
4 52 0	29 30	7·2	6·0 & 9·0	...	...	
4 53 0	30 30	7·1	5·9 9·0	...	...	
4 54 0	31 30	6·3	5·9 8·2	...	...	
4 55 0	32 30	6·3	5·0 8·0	24	...	
4 55 20	32 50	5·6	4·0 8·0	...	...	
4 55 30	33 0	6·1	5·0 8·0	...	...	
4 56 0	33 30	5·0	4·5 7·0	...	21	
4 57 0	34 30	4·6	3·5 5·8	22	...	
4 57 15	34 45	4·0	3·5 5·5	...	...	
4 57 30	35 0	4·4	3·5 6·0	...	...	
4 58 0	35 30	4·4	3·0 6·0	...	27	The respirations are extremely shallow and gasping.
4 59 0	36 30	4·0	3·0 5·0	...	...	
4 59 30	37 0	3·5	2·5 4·5	...	...	
5 0 0	37 30	2·5	1·0 3·0	...	...	
5 0 30	38 0	1·8	1·0 2·5	18	4	Respirations are mere gasps.
5 1 15	38 45	1·6	1·0 2·0	...	4	
5 1 30	39 0		1·0 1 5	...	...	Mercurial column has fallen into the reservoir.

In the autopsy, which was immediately made, the heart was found dilated with dark blood. Occasional contractions occurred for twenty minutes after death. The sciatic, intercostal and phrenic nerves were active, and galvanism of a vagus nerve excited vermicular movements of the stomach. The intestinal peristalsis was feeble. Galvanism of the cervical sympathetics produced no contraction of the pupils. A quivering movement continued in the striped muscles for many minutes.

The first portion of this experiment shows that after the administration of the poison a very distinct rise occurs in the arterial tension, while the number of the cardiac contractions rapidly diminishes during this rise, and before the frequency of the respirations has been affected. The second, and larger, dose did not influence the arterial tension so powerfully, nor did it exert so immediate an action on the frequency of the heart's beats. Both doses were administered in such a manner that their absorption was comparatively slow; in the following experiments, physostigma was directly injected into the circulation, and its action was, therefore, more rapidly and energetically produced.

b. *Tension of the Arterial and Venous Systems.*

Experiment LIX.

Performed on a large retriever dog, vigorous and in perfect condition.

Time.	Time after Administration.	Arterial Tension. Mean Pressure.	Arterial Tension. Pressure Oscillates between	Venous Tension.	No. of Cardiac Contractions.	No. of Respirations.	Temperature.	Notes of Operations and of Symptoms.
H. M. S.	M. S.				Per Min.	Per Min.	°	
2 58 0	...	...	...	...	...	...	...	The right carotid artery was exposed, and attached to the hæmadynamometer.
3 1 0	...	...	...	...	...	...	...	The left jugular vein was attached to the second hæmadynamometer.
3 2 0	...	5·8	4·3 & 7·0	...	...	...	...	
3 3 0	...	6·0	5·0 7·0	2·8	...	...	...	
3 4 0	...	6·0	4·5 7·5	2·4	...	...	...	
3 5 0	...	6·0	4·6 7·0	2·2	...	...	...	
3 6 0	...	6·2	5·0 8·0	2·2	...	...	...	
3 7 0	...	7·0	5·0 8·0	1·9	...	...	...	
3 8 0	...	6·1	4·0 7·0	1·7	...	...	...	
3 8 30	...	5·5	5·0 7·0	...	...	...	...	
3 9 0	...	6·3	3·3 7·0	1·7	120	...	...	
3 10 0	...	5·9	3·0 6·7	...	...	8	99·2	
3 12 0	...	5·4	4·0 6·5	1·8	...	...	99·8	The dog seems perfectly calm.
3 13 0	...	5·9	4·0 7·0	...	126	10	...	
3 14 0	...	5·6	4·5 6·5	...	132	...	100·0	
3 15 0	...	5·6	4·5 7·0	...	...	10	...	
3 15 30	...	5·5	5·0 7·0	...	...	...	...	
3 16 0	...	6·6	5·0 7·0	1·8	...	12	99·9	
3 16 15	...	...	...	...	...	...	...	Commenced the injection of six grains of extract, in twenty minims of water, into the right jugular vein.
3 16 30	...	...	...	...	...	...	...	The injection was finished.
3 17 0	0 30	6·0	4·0 7·0	2·8	120	...	...	
3 17 30	1 0	4·3	...	...	...	...	...	The muscles are twitching; fæces passed.
3 17 35	1 5	4·9	...	...	...	...	...	
3 17 45	1 15	6·5	4·0 11·0	...	...	...	...	
3 17 50	1 20	7·0	5·0 11·5	...	...	...	...	
3 18 0	1 30	8·4	5·2 11·9	3·4	78	10	99·9	Urine passed; saliva is escaping from the mouth in large quantity.
3 18 30	2 0	8·5	5·5 12·0	...	...	...	...	
3 19 0	2 30	7·9	5·0 10·0	3·4	...	...	...	The twitches are now severe, and have a spasmodic character.
3 19 15	2 45	7·3	5·0 9·0	...	54	11	100·2	
3 19 30	3 0	6·5	4·5 8·0	3·4	...	...	...	No movement of the eyelids when the cornea or conjunctiva is touched.
3 20 0	3 30	5·4	4·0 8·0	3·4	40	...	100·4	The muscular twitches have become less marked.

Experiment LIX.—continued.

Time.	Time after Adminis-tration.	Arterial Tension. Mean Pres-sure.	Arterial Tension. Pressure Oscillates between	Venous Ten-sion.	No. of Cardiac Contrac-tions.	No. of Respira-tions.	Tem-perature	Notes of Operations and of Symptoms.
H. M. S.	M. S.				Per Min.	Per Min.	°	
3 20 15	3 45	5·0	...	...	...	...	...	Excessive lachrymation.
3 20 30	4 0	4·7	2·5 & 6·0	3·2	...	...	...	
3 20 40	4 10	3·7	3·0 6·0	...	...	...	...	
3 20 50	4 20	3·8	3·0 6·5	3·0	...	...	100·4	
3 21 0	4 30	4·3	3·0 6·0	2·9	32	...	...	Cardiac impulse quite perceptible to the touch.
3 21 15	4 45	3·5	2·0 5·0	...	...	...	100·4	The dog is now quite quiet.
3 21 30	5 0	3·0	2·0 5·0	2·8	...	...	...	Third eyelid is protruded slightly, and the eyeball is directed downwards and inwards.
3 22 0	5 30	2·6	2·0 5·5	2·4	24	8	100·3	
3 22 15	5 45	3·1	3·0 5·0	2·2	...	...	...	
3 22 30	6 0	2·5	2·0 5·0	2·1	...	...	...	Cardiac impulse can still be felt readily.
3 23 0	6 30	2·6	2·0 4·5	2·0	22	...	100·3	
3 23 30	7 0	2·7	1·5 5·0	1·9	...	...	...	Respirations are short and jerking.
3 24 0	7 30	2·5	2·0 3·5	1·8	20	10	100·3	
3 25 0	8 30	2·6	2·0 3·5	1·9	12	...	100·3	An occasional faint muscular twitch occurs.
3 26 0	9 30	2·7	2·3 3·0	1·7	16	10	100·2	Respirations are gasps merely.
3 26 15	9 45	2·5	2·0 3·5	...	...	...	...	
3 26 30	10 0	2·4	2·0 3·0	...	...	6	...	No reflex movement on severe irritation.
3 27 0	10 30	2·2	2·3 2·5	1·7	14	...	100·2	
3 27 30	11 0	1·3	2·0	1·6	...	...	100·2	
3 27 40	11 10	...	...	1·0	...	...	...	Cardiac and respiratory movements have ceased.
3 28 0	11 30	...	...	...	...	...	100·1	
3 33 0	16 30	...	...	...	...	...	100·0	
3 35 0	18 30	...	...	...	...	...	99·7	

The exposed heart was found, in the autopsy, to be perfectly quiet; and only one or two irregular non-synchronous movements could be obtained when it was stimulated. All its chambers were full of blood. Slight peristaltic movements were observed in the small intestines.

Experiment LX.

Experiment LX.

Performed on a large and very active Newfoundland-and-retriever cross-bred dog.

Time.	Time after Administration.	Arterial Tension. Mean Pressure.	Arterial Tension. Pressure Oscillates between	Venous Tension.	No. of Cardiac Contractions.	No. of Respirations.	Temperature.	Notes of Operations and of Symptoms.
H. M. S.	M. S.				Per Min.	Per Min.	°	
2 53 0	...	...	...	...	...	...	...	The arterial hæmadynamometer was connected with the right carotid artery.
2 54 0	...	...	...	...	...	...	...	The left jugular vein was attached to the venous hæmadynamometer.
2 56 0	...	6·0	4·5 & 7·0	...	118	34	98·0	
2 57 0	...	5·8	4·0 7·0	2·4	...	...	...	
2 58 0	...	6·2	5·0 7·5	2·4	...	28	100·1	
3 0 0	...	6·0	5·0 8·0	2·5	...	...	...	
3 3 0	...	6·0	5·0 8·0	2·5	...	...	99·9	
3 4 0	...	6·0	5·0 6·5	...	176	30	...	
3 5 0	...	6·1	4·8 7·0	...	...	...	100·2	
3 6 0	...	6·1	5·0 6·5	...	180	26	...	
3 7 0	...	6·3	6·0 7·0	3·0	...	...	...	
3 8 0	...	6·1	5·0 7·2	3·5	175	...	...	
3 9 0	...	6·0	4·5 7·0	...	...	...	100·1	
3 10 0	...	6·1	5·0 7·0	2·9	160	...	...	
3 10 30	...	...	...	...	...	...	...	Commenced the injection, into the right jugular vein, of seven grains of extract, in twenty-five minims of distilled water.
3 10 40	...	...	...	...	...	...	...	The injection was finished.
3 11 30	0 50	6·0	4·7 7·0	...	...	...	...	
3 12 0	1 10	5·9	4·5 6·5	3·1	108	...	...	Marked twitches occurred.
3 12 30	1 40	6·0	4·5 6·5	3·2	...	...	100·3	
3 13 0	2 50	...	5·0 6·0	3·3	...	25	...	
3 14 0	3 20	5·9	6·0 7·0	3·2	...	...	100·3	
3 14 30	3 50	...	...	3·4	...	...	...	
3 15 0	4 20	5·8	...	4·2	...	22	100·4	
3 16 0	5 20	7·0	5·5 8·0	4·3	42	25	...	Respirations are feeble and short.
3 16 30	5 40	6·5	5·0 8·5	3·8	...	...	100·3	
3 17 0	6 20	6·8	5·5 7·5	3·2	32	...	...	Twitching interferes with, and prevents the counting of, the respirations.
3 18 0	7 20	7·6	6·7 8·9	3·5	32	...	100·5	
3 18 30	7 50	5·0	4·0 5·0	4·2	...	...	...	
3 18 45	8 5	...	...	4·0	...	...	...	
3 19 0	8 20	4·5	3·0 4·0	3·8	24	...	100·5	
3 19 30	8 50	4·0	3·0 4·5	...	...	...	...	Occasional gasps.
3 19 45	9 5	3·1	2·5 3·5	...	...	...	100·3	
3 20 0	9 20	2·6	2·0 2·8	3·5	20	10	...	
3 20 30	9 50	2·4	2·0 2·7	3·2	...	...	100·2	
3 21 0	10 20	2·3	2·0 2·5	2·9	16	13	100·2	
3 21 30	10 50	2·4	2·0 2·4	2·7	...	10	...	Muscles are now *quivering*, not twitching.

Experiment LX.—continued.

Time.	Time after Administration.	Arterial Tension. Mean Pressure.	Arterial Tension. Pressure Oscillates between	Venous Tension.	No. of Cardiac Contractions.	No. of Respirations.	Temperature.	Notes of Operations and of Symptoms.
H. M. S.	M. S.				Per Min.	Per Min.	°	
3 22 0	11 20	1·9	1·5 & 2·3	2·7	8	...	100·1	
3 22 30	11 50	...	...	2·4	6	...	...	No respiratory movements, except an occasional jerking inspiration.
3 23 0	12 20	2·0	2 0 2·1	2·3	...	...	...	
3 23 30	12 50	1·8	1·5 1·7	2·1	...	...	...	
3 24 0	13 20	1·5	1·5 1·6	2·0	...	...	...	
3 24 15	13 35	...	...	1·9	...	...	...	
3 24 30	13 50	1·5	0·6 1·0	1·0	...	...	...	
3 24 45	14 5	0	0	0	0	0	...	The three indicating columns of mercury have subsided into their reservoirs.
3 25 0	14 20	...	...	...	...	...	100·0	
3 26 0	15 20	...	...	...	...	...	99·0	
3 27 0	16 20	...	...	...	...	...	99·3	

The abdomen and chest were immediately opened: the heart was dilated, full and motionless; and no peristalsis could be observed in the intestines. The diastaltic function of the cord was completely abolished, while motor nerve-conductivity was retained, for at least five minutes after death, in the sciatic, phrenic and intercostal nerves.

The principal results of these experiments are indicated so clearly that it is almost superfluous to point them out. During the first stage, the arterial tension diminishes slightly, the venous tension increases and the cardiac contractions rapidly diminish. The frequency of the respirations was increased in only the first experiment. After this, the arterial tension increases, soon arrives at a maximum considerably above its average before the poisoning, and then slowly diminishes; while the venous tension arrives at a high maximum rather later, and by more gradual stages, than the arterial, and in the same gradual manner declines until death. In neither system is the highest point reached before a very considerable fall has been caused in the frequency of the heart's contractions. The temperature rises during the poisoning, and attains its maximum near the time that the blood-pressures have commenced finally to diminish.

As these marked changes in the circulation could be produced before the respiratory function was modified to any important extent, it did not seem to me at all necessary to repeat the experiments with the addition of artificial respiration.

It is interesting to observe the same phenomena produced by physostigma after the division of the vagi nerves.

c. *Tension of the Arterial and Venous Systems after Division of the Vagi Nerves.*

Experiment LXI.

Performed on a young, full-grown and active retriever dog.

Time.	Time after adminis-tration.	Arterial Tension. Mean Pres-sure.	Arterial Tension. Pressure Oscillates between		Venous Ten-sion.	No of Cardiac Contrac-tions.	No. of Respira-tions.	Tem-perature.	Notes of Operations and of Symptoms.
H. M. S.	M. S.					Per Min.	Per Min.	°	
2 38 0	...	...	...		...	...	...	...	The carotid artery, external jugular vein and vagus nerve of each side were exposed.
2 48 0	...	...	...		...	...	...	...	The venous hæmadynamometer was attached to the left jugular vein.
2 49 0	...	...	...		1·9	118	...	99·8	
2 50 0	...	...	...		1·8	...	...	99·2	The right carotid artery was connected with the second hæmadynamometer.
2 51 0	...	...	...		2 0	...	18	98·8	
2 52 0	...	4·9	4·0 &	6·0	...	102	...	98·7	
2 53 0	...	4·7	3·5	5·5	1·5	...	20	99·2	
2 54 0	...	5·0	4·0	6·0	2·2	108	...	99·0	
2 55 0	...	4·2	3·5	5·0	...	...	...	99·3	
2 56 0	...	...	...		...	80	...	99·2	
2 57 0	...	...	...		...	92	...	99·2	
2 58 0	...	4·5	3·0	5·5	2·1	...	...	...	
2 59 0	...	4·4	3·0	5·0	2·1	...	..	...	
3 0 0	...	4·6	3·5	5·5	2·1	90	16	99·2	
3 1 0	...	...	...		...	...	...	99·3	The right vagus nerve was cut through.
3 1 15	...	...	...		...	...	...	99·3	The left vagus was cut through. The respirations became short and spasmodic.
3 2 0	...	...	...		2·5	60	...	99·4	
3 3 0	...	6·0	5·5	6·0	2·6	...	...	99·3	The mercury in the oscillating column moves very rapidly over short spaces.
3 3 30	...	5·8	5·5	6·2	...	...	...	...	
3 4 0	...	5·8	5·8	6·2	2·5	120	...	99·2	
3 4 30	...	5·9	5·5	6·0	2·5	...	...	...	
3 5 0	...	5·9	5·5	6·0	2·5	125	...	99·2	
3 5 30	...	5·9	5·3	5·8	...	140	...	...	
3 6 0	...	5·8	5·5	5·8	2·5	170	30	99·1	
3 6 30	...	5·7	5·3	5·7	...	166	...	...	
3 7 10	...	5·7	5·7	6·1	...	166	...	99·2	Commenced to inject slowly, into the right jugular vein, seven grains of extract in twenty-five minims of water.
3 7 30	...	...	...		...	...	...	...	Finished the injection.
3 8 0	0 30	5·6	5·0	5·5	...	79	20	99·4	
3 8 30	1 0	5·6	5·0	8·0	...	...	...	...	
3 9 0	1 30	...	4·0	7·0	2·8	50	...	99·3	Respiration spasmodic ; great salivation.

Experiment LXI.—continued.

Time.	Time after adminis-tration.	Arterial Tension.		Venous Ten-sion.	No. of Cardiac Contrac-tions.	No. of Respira-tions.	Tem-perature.	Notes of Operations and of Symptoms.
		Mean Pres-sure.	Pressure Oscillates between					
H. M. S.	M. S.				Per Min.	Per Min.	°	
3 9 30	2 0	5·4	4·5 & 7·5	3·2	...	...	99·3	Slight twitches have commenced.
3 9 40	2 10	...	...	3·7	...	...	...	Tears are flowing copiously.
3 10 0	2 30	6·8	5·0 9·0	4·2	45	...	98·5	The respirations cannot be counted, because of the twitching.
3 10 15	2 45	6·9	5·0 9·5	4·5	...	...	...	Fæces passed.
3 10 30	3 0	7·3	5·0 9·5	4·9	...	...	...	Tremors are very strong.
3 10 45	3 15	...	...	5·2	...	...	...	
3 11 0	3 30	6·8	4·5 8·0	5·2	...	...	98·2	
3 11 15	3 45	6·8	4·5 7·5	5·1	...	...	...	
3 11 30	4 0	7·8	6·5 9·5	5·0	...	...	...	
3 11 45	4 15	...	...	5·2	...	...	98·0	
3 12 0	4 30	8·1	6·0 10·0	5·3	42	...	...	
3 12 15	4 45	7·5	6·0 8·0	5·4	...	...	...	
3 13 0	5 30	6·3	5·5 7·5	5·3	56	10	98·2	Respirations are merely gasps.
3 13 30	6 0	5·6	4·0 5·5	...	...	...	...	Neither cornea nor conjunctiva is sensitive.
3 13 35	6 5	4·7	4·0 5·5	5·3	...	...	98·2	Twitches have nearly ceased.
3 13 40	6 10	4·0	...	...	...	...	...	
3 13 45	6 15	3·8	3·0 4·5	5·3	...	...	...	
3 14 0	6 30	3·5	3·0 4·0	5·3	40	an occasional gasp.	98·0	
3 14 30	7 0	3·8	3·0 4·5	5·2	...	...	...	The muscular movements can scarcely be seen, but they can readily be felt.
3 14 45	7 15	3·8	3·0 5·0	4·8	25	Do.	97·7	
3 15 15	7 45	...	...	4·2	...	...	...	
3 15 30	8 0	...	...	3·4	...	...	...	
3 15 45	8 15	...	...	3·0	...	...	...	Urine passed.
3 16 0	8 30	1·5	1·0 1·5	2·4	10	...	...	
3 16 30	9 0	...	...	2·2	...	...	97·6	
3 17 0	9 30	1·0	...	2·2	...	...	...	
3 17 15	9 45	...	...	1·9	...	...	...	
3 17 30	10 0	...	...	1·5	...	...	97·5	
3 17 45	10 15	...	...	1·3	...	...	...	
3 18 0	10 30	...	...	1·0	...	...	97·2	
3 18 30	11 0	..	...	...	...	...	...	
3 20 0	12 30	...	...	...	...	...	97·0	
3 22 0	14 30	...	...	...	...	...	96·8	
3 24 0	16 30	...	...	...	...	...	96·5	

The autopsy was made immediately. Slight, but distinct, peristalsis was observed in the intestines. The heart was dilated and motionless, and its right side contained more blood than in the preceding experiments. The phrenic, intercostal and sciatic nerves were active.

It is, therefore, shown that the same effects are produced on the circulation after the division of the vagi, as when these inhibitory nerves retain their connection with the heart. The action on the frequency of the heart's beats is well illustrated in this experiment; from their number having been considerably increased before the exhibition of the poison, by the division of the vagi nerves.

The distance over which the mercury travels in the oscillating column seems to be increased as the effects of the poison manifest themselves. This appears from all the experiments in this series, but especially from the second and the last. The division of the vagi, in the last experiment, had abnormally diminished the distance of oscillation; and, yet, it became much greater after the poisoning than it had been previous to the nerve-division. It further appears that this increase in the oscillating distance occurs when the arterial tension is about its maximum. It can only be explained by a very decided increase in the force of the cardiac contractions. I believe that this effect on the heart is altogether a reflex one, due to the resistance to the propulsion of the blood, that the augmentation in the general vascular tension must excite. In a normal condition, a stimulus of this nature might be expected to operate by increasing the *number* and not the *strength* of the cardiac contractions; but any tendency to increased frequency is opposed by the action of physostigma, for we have already seen that this substance diminishes the number of the contractions, by prolonging the diastolic pause. Their strength may, however, continue unchanged; and, during the operation of physostigma, a stimulus may even increase it without affecting the number of the beats, so long as the ganglia that initiate the systolic contraction have their excitability merely lowered without being destroyed and the contractile power of the cardiac muscle continues undiminished.

Before discussing any further the changes of blood tension, it will be advisable to examine the condition of the minute blood-vessels during the action of physostigma.

2. *Examination of the Calibre-Changes in the smaller Blood-Vessels.*

The facility with which frequent measurements may be made of the diameter of any selected capillary or minute artery or vein in the web of the frog's foot, is well known to physiologists, and has been taken advantage of by WHARTON JONES,* BENNETT,† LISTER‡ and others, in examining their condition during inflammation. A more general application of such examination of the capillaries to the investigation of the action of poisons would certainly be of great value. It has proved so in the case of nicotia;§ and we cannot consider any research on the actions of a cardiac poison to be complete unless it be done.

* On the State of the Blood and Blood-Vessels in Inflammation, &c. GUY'S Hospital Reports, 1851.

† Principles and Practice of Medicine.

‡ Philosophical Transactions, 1858.

§ CLAUDE BERNARD, Leçons, &c., 1857, p. 399.

In my first experiments, the frog was merely tied down in the manner usual when the circulation in its web is being microscopically examined, but it was found impossible to prevent movements so absolutely as was required to retain the selected vessels in the field of the microscope. I ultimately found it necessary to adopt LISTER's recommendation of dividing the spinal cord several hours before the observations were begun.

Experiment LXII.

I divided the spinal cord at the occiput of a light-coloured frog weighing 529 grains, and, six hours afterwards, placed the web of one of its feet on the stage of a microscope. A small artery, a minute branch and a vein were selected and placed conveniently for measurement.*

A = diameter of larger artery; B = diameter of smaller artery; and C = diameter of vein.

Time.	A.	B.	C.	Notes.
15 minutes before administration of Calabar bean	8·0	2·5	6·5	
10 Do.	8·0	2·5	6·5	Circulation free.
5 Do.	8·0	2·5	6·5	
Time after administration of poison.				Four grains of extract, in twenty minims of distilled water, were injected into the subcutaneous tissue of the abdomen.
6 minutes,	7·8	2·5	6·5	
9 ,,	6·5	2·0	6·0	
10 ,,	5·9	1·8	5·7	
12 ,,	6·8	2·0	6·2	Circulation feeble, oscillating sometimes; vessels are crowded.
14 ,,	7·0	2·0	6·5	
15 ,,	8·5	3·0	6·5	
17 ,,	8·5	3·0	7·0	Almost no circulation.
19 ,,	9·0	3·0	7·5	Faint oscillations only in the artery; considerable crowding in all the vessels.
23 ,,	8·5	3·0	7·5	
29 ,,	8·5	3·0	7·5	Complete stasis.
36 ,,	8·5	3·0	7·5	
39 ,,	9·0	3·0	7·5	
1 hour	9·0	3·0	7·5	
2 hours	9·0	3·0	7·5	

Experiment LXIII.

The web of a frog, of 590 grains weight, was placed in the field of a microscope, after the animal had been prepared in the manner described in the previous experiment.

* NACHET's eye-piece No. 1, and object-glass No. 3, were employed; and the measurements represent divisions of an eye-piece micrometer, each of which equals $\frac{1}{500}$th of an inch with the above glasses.

A = diameter of a small artery; B = diameter of a very small vein.

Time.	A.	B.	Notes.
15 minutes before poisoning,	5·0	3·0	
10 Do.	5·0	3·0	
5 Do.	5·0	3·0	
Time after Administration of Poison.			Two grains of extract, in fifteen minims of water, were injected into the left flank.
5 minutes,	5·0	3·0	
10 "	4·7	3·0	Circulation oscillating.
15 "	4·2	3·0	
20 "	4·2	2·9	
25 "	4·0	2·9	
28 "	4·9	3·0	
29 "	5·7	3·0	
30 "	6·0	3·2	Stasis, and crowded vessels.
35 "	7·0	3·5	Slow circulation.
40 "	7·0	3·5	
45 "	8·0	3·5	Faint oscillations.
50 "	8·0	3·5	
1 hour,	8·0	3·5	
2 hours,	8·0	3·5	

The action of Calabar bean on the minute blood-vessels of the frog's web is, therefore, to contract them considerably first, and then dilate them. The contraction may be influenced, to a slight extent, by the reduction in the frequency of the heart's action; but the succeeding dilatation, during a still greater reduction, would lead us to suppose that it is mainly due to a specific effect on the ganglia and nerves that govern the calibre-changes of the vascular system, because it is by their influence that the final dilatation must be produced.

We are now in a position to explain the changes of blood tension that have been described in mammalians. The slight fall that usually occurs in the mean pressure immediately after the poison has been exhibited, I believe to be solely due to the diminution in the rate of the heart's contractions, which has always been caused by that time. The subsequent rise in both arterial and venous tensions before any considerable embarrassment of the respiration, may be satisfactorily explained by such contraction of the smaller arteries and veins as has been demonstrated to occur in the vessels of the frog's web. It cannot be caused by increased cardiac pressure; for the heart is at the time contracting with only one half its normal rapidity, or with even less; while the greater force of each heart beat the increased oscillating distances appear to indicate, is quite insufficient to account for the high degree of blood tension sometimes attained. The subsequent, more or less rapid diminution of pressure in both arterial and venous systems is the evident result of the great dilatation in the minute blood-vessels, assisted by the weakening of the *vis e tergo* that this poison quickly produces.

Action on the Temperature of the Body.

The temperature of mammalians that are being poisoned by Calabar bean rises slightly, so that a thermometer, placed either in the subcutaneous tissue or in the rectum, will indicate a gradual elevation as soon as well-marked symptoms begin to be produced. The effect on the surface temperature has been already shown in Experiments LIX., LX. and LXI.; that on the internal temperature was frequently observed with rabbits, and may be illustrated sufficiently in the following experiment.

Experiment LXIV.

A full-grown rabbit was placed on its back on a board, and firmly secured by a ligature round each leg.

Time.		Time after Administration of Poison.		Temperature.	Respirations.	Notes.
H.	M.	M.	S.	°	Per Min.	
12	5	...		...	...	The bulb of a delicate thermometer, with Fahrenheit's scale, was secured in the rectum.
12	10	...		99·0	...	
12	20	...		98 5	...	
12	30	...		98·4	...	
12	40	...		98·2	...	
12	50	...		98·0	47	
1	0	...		97·5	...	
1	10	...		97·3	...	
1	20	...		97·0	48	
1	30	...		96·5	...	
2	0	...		96·0	48	
2	10	...		95 8	...	
2	20	...		95·7	48	
2	30	...		95·5	48	
2	40	...		95·5	48	
2	50	...		95·3*	48	
2	56	...		...	...	Half a grain of extract, in ten minims of water, was injected into the subcutaneous tissue of the left flank.
3	1	5	0	95·8	...	Faint tremors of legs and of head.
3	5	9	0	96·3	...	Tremors are now general and stronger; and they prevent accurate determination of the respiratory movements.
3	6	10	0	96·5	...	
3	8	12	0	96·0	...	
3	15	17	0	95·5	...	
3	16	18	0	95·3	72	The muscular tremors have greatly diminished. Respirations are shallow and gasping, and they appear much impeded by bronchial secretion.
3	25	29	0	95·0	...	Quiet; except a few occasional twitches.
3	30	34	0	95·0	24	Respirations are very noisy and laboured.
3	32	36	0	94 5	...	The animal is dead.
3	35	39	0	94·0	...	
3	50	54	0	93·0	...	

* It will be observed that the temperature has gradually fallen in this experiment before the

The inconsiderable elevation of temperature exhibited in these experiments is probably an effect of the general muscular twitching that Calabar bean causes in mammals. It is quite possible that it may be also in part the result of vascular dilatation: but I think the first explanation is sufficient alone; and this additional one seems improbable when we remember that during the dilatation the circulation is extremely sluggish, because of the great diminution in the number of the cardiac contractions that accompanies it.

Action on the Blood.

The blood obtained from animals that have been poisoned by Calabar bean is generally dark in colour, because of the usual cause of death; but if drawn from the left side of the heart after a very large dose of the poison, it has the scarlet hue of arterial blood. It frequently remains semifluid for some time, and then clots loosely. When examined with the spectroscope, no modification has ever been observed in the characters or positions of the normal crurine bands.

In dogs and rabbits, the red blood corpuscles are changed in form, and present various irregularities of outline, among which a well-marked stellar crenation predominates. There can be no doubt that this is an effect produced by physostigma, as I have frequently examined the blood previous to the administration, and found its microscopic characters perfectly normal, and repeated the examination immediately after death, and invariably observed the above modifications. No change is produced in the red corpuscles of birds or frogs, nor in the white corpuscles of any animal I have examined.

For the purpose of detecting any possible effect on the respiratory function of the blood—a subject to which Harley, by his elaborate researches, has directed considerable attention*—two experiments were performed, the results of which agreed very closely. One of these may be given here, but without the numerous details which are necessarily connected with it.

Experiment LXV.

A small quantity of blood was directly removed from the right side of the heart of a Skye-terrier dog, by passing a gum-elastic catheter down the right jugular vein, affixing a syringe with a stop-cock, and withdrawing the requisite amount, according to the ingenious method of Claude Bernard.† The catheter was then detached from the syringe, and, the stop-cock

poison was administered, and apparently because of the constrained position in which it was necessary to retain the rabbit. In other nine experiments of the same description, a similar fall occurred. I endeavoured to find if a stationary, constant point could be obtained, after which the poison might be given; with the following result:—When the thermometer was introduced, the temperature was 97°; in one hour, it had fallen to 96°·3; in two hours, to 95°·6; in three hours, to 95°·3; in four hours, to 94°·7; in five hours, to 93°·3; and in six hours, to 92°. The rabbit was now set free: it was unable to stand; and other four hours afterwards, it was found dead. As this is a very ordinary method of treating rabbits during physiological experiments, it is important to recognise this injury to their vitality, which may occasion many fallacious conclusions if overlooked.

* On the Influence of Physical and Chemical Agents upon Blood; with special reference to the mutual action of the Blood and the Respiratory Gases. Phil. Trans. 1865, p. 687.

† Action de l'Oxyde de Carbon sur le Sang, Leçons, &c., 1857, p. 166.

having previously been closed, a bent steel tube was substituted by which means the blood could be readily passed into a small absorption tube over mercury, without coming in contact with the atmosphere.

The vein was now ligatured, and the dog was poisoned with a moderate dose of the extract. At the moment of death, a second quantity of blood was withdrawn with the same precautions as the first, and it was placed in a similar absorption tube. A nearly equal portion of atmospheric air was added to each tube, and they were frequently shaken during twenty-four hours in a room with a temperature that varied little from 50° F. The gases were then removed and analysed. The following are the results :—

Gases from Blood before Poisoning.		Gases from Blood after Poisoning.	
Volume of blood = 7·77 cub. cent.		Volume of blood = 4·03 cub. cent.	
„ air = 47·95 „		„ air = 48·20 „	
After contact with the blood for 24 hours, air measures 51·65 c. c.		After contact with the blood for 24 hours, air measures 50·54 c. c.	
∴ Apparent exhalation = 3·70 c. c.		∴ Apparent exhalation = 2·34 c. c.	
Composition per cent. :—		Composition per cent. :—	
Oxygen,	20·63	Oxygen,	19·93
Nitrogen,	77·82	Nitrogen,	78·77
Carbonic acid,	1·55	Carbonic acid,	1·30
	100·		100·

These results agree sufficiently to prove that the respiratory function of the blood is not interfered with in physostigma poisoning. Had there been any marked discrepancy, a suspicion of such an action might be raised; but I doubt if this could be really settled without a much more refined method of experiment than was adopted. HARLEY's results seem open to very many objections, as his usual method permitted of an even greater number of fallacies than were possible in the two experiments I performed. At the same time, his paper is an extremely valuable and elaborate one, and contains many conclusions of the highest interest to physiologists.

ACTION ON THE LYMPH-HEARTS OF THE FROG.

The lymph-hearts discovered by MÜLLER* and PANIZZA† in amphibia, have always been found paralysed at an early stage of the poisoning, in the experiments where their condition was examined. As the pulsations of the pair situated one on each side of the sacrum of frogs may be readily determined without any operation, attention was especially directed to them. The time at which they cease to contract is noted in the following experiment; and the previous increase in rapidity that is there mentioned has been observed on other occasions.

Experiment LXVI.

The lymphatic hearts in the ischiadic region of a frog had an average rate of forty-nine contractions in the minute. Five minutes after two grains of extract had been subcutaneously

* Philosophical Transactions, 1833, p. 559.

† Proceedings of the Royal Society of London, vol. ix. p. 559.

administered, these contractions were seventy-two; in ten minutes, they were seventy; in fifteen minutes, they were sixty-two; and in twenty minutes, they were forty-four. At this time, the frog was flaccid, though still possessing the power of feeble voluntary movement; and its respiration had ceased. One minute thereafter, or twenty-one after the poison had been given, the most careful examination failed to detect any pulsation of the lymphatic hearts, their final stoppage having suddenly occurred.

In many other experiments, these symptoms merely repeated themselves.

Action on the Peristaltic Movements of the Abdominal Viscera.

A close analogy exists between the action on the minute blood-vessels and that on the peristaltic movements of the intestines. In mammalians, physostigma seems, in the first place, to increase the vermicular contractions of all the abdominal viscera, and then to diminish them. For some time, the intestines move with increased vigour; they then contract, so as very considerably to diminish their calibre; and, finally, they assume a condition of dilatation with lessened movement. Peristalsis has been invariably observed to continue after death; but if a large dose has been exhibited, it may be very slight and of short duration. Stimulation of the vagi nerves sometimes increases vermicular movements after death, and it then does so very conspicuously in the stomach. I have never succeeded in convincing myself of the activity of the splanchnic nerves in post mortem examinations; but, as the intestinal movements are then usually sluggish, it is a matter of extreme difficulty to judge of the action of their inhibitory nerves. During the progress of the symptoms in rabbits, I have also observed very energetic peristalsis in the cornua and body of the uterus, and even in the ureters. From the former of these effects I should be inclined to recommend physostigma as an oxytocic. As with those of the heart, the special ganglia of the intestines appear to have their functions retained, with diminished activity, for a considerable period after death; and, as with the cardiac muscle, stimulation produces non-peristaltic movements of those abdominal viscera that possess a muscular structure, long after the nerves that govern their rhythmical contractions have been paralysed.

Action on the Pupil.

In many experiments, the condition of the pupil was carefully observed, and its diameter was measured at intervals by means of a graduated glass scale, each division of which represented one-fiftieth of an inch.

The changes that usually occur in mammals may be briefly described as consisting of a short period of slight dilatation and a succeeding one of contraction; and either the latter gradually increases until death, or the pupil first oscillates once or twice between dilatation and contraction. The latter condition is present at death, and after this the pupil again dilates.

These iridal movements are best seen in rabbits; they are sluggish in dogs, more so in certain birds, and least evident in frogs.

The more important of the results may be conveniently arranged in the form of a table.

Table of Pupil-Changes during Poisoning by Physostigma.

Experiment.	Animals.	Average before Poisoning.	Minimum after Poisoning, and Time of Occurrence.	Time of Return to Average before Poisoning.†	Dilatation over Average, and Time of its Occurrence.	
No. LXVII.	Frog	3 × 4*	1·75 × 3 in 46 min.	Not noted	None noted	
,, LXVIII.	Do.	5 × 7	4· × 5 in 29 ,,	4 hours	Do.	
,, LXIX.	Do.	5 × 7	4·5 × 6 in 16 ,,	55 min.	6 × 7 in 1 h. 10 m.	
,, LXX.	Rabbit	13 long diam.	4 in 7 min.	Not noted	None noted	
,, LXXI.	Do.	9 ,,	4 in 5 ,,	15 min.	Do.	
,, LXXII.	Do.	10 ,,	7 in 11 ,,	23 min.	Do.	In 23 minutes, when the pupil had returned to its average, the animal had nearly recovered from a small dose; the second line marks the effects produced by a second and larger dose, 24 minutes after the first.
	Same rabbit	10 ,,	6 in 7 ,,	Not noted	Do.	
,, LXXIII.	Rabbit	10 ,,	3 in 12 ,,	Do.	Do.	
,, LXXIV.	Do.	11 ,,	3 in 16 ,,	58 min.	Do.	
,, LXXV.	Do.	9 ,,	3 in 7 ,,	8 min.	Do.	
,, LXXVI.	Do.	12 ,,	4 in 8 ,,	Not noted	Do.	
,, LXXVII.	Do.	13 ,,	3 in 8 ,,	Do.	Do.	
,, LXXVIII.	Do.	11 ,,	3 in 16 min. 30 sec.	17 min. 30 sec.	Do.	
,, LXXIX.	Frog	4 × 6	3·5 × 5 in 24 min.	Not noted	Do.	
,, LXXX.	Do.	5 × 6	4· × 5 in 25 ,,	2 hours	5 × 6 in 1 h. 46 m.	Eyelids closed, so that it was difficult to see the pupil after 24 minutes. This often happens in frogs.
,, LXXXI.	Do.	5 × 6	3· × 4·5 in 16 ,,	2 hours	None noted	
,, LXXXII.	Do.	5 × 6	4· × 5 in 30 ,,	1 hour 47 min.	Do.	
,, LXXXIII.	Dog	21	17 in 28 min.	29 min.	25 in 30 min.	
,, LXXXIV.	Frog	5 × 6·5	4· × 5 in 19 min.	2 hours 5 min.	None noted	
,, LXXXV.	Do.	4 × 5·5	3·5 × 5 in 11 ,,	22 min.	Do.	
,, LXXXVI.	Do.	6 × 7·5	3· × 5 in 50 ,,	Not noted	Do.	
,, LXXXVII.	Dog	21	15 in 4 min.	4 min. 30 sec.	25 in 5 min. 30 sec.	
,, LXXXVIII.	Do.	10	4 in 5 min. 30 sec.	8 min.	15 in 10 min.	
,, LXXXIX.	Do.	16	3 in 7 min.	9 min. 30 sec.	None before death	
,, XC.	Do.	15	9 in 11 ,,	16 min.	None noted	
,, XCI.	Pigeon	6	4 in 8 ,,	Not noted	None noted	
,, XCII.	Do.	6	4 in 28 ,,	Do.	Do.	General symptoms were very slowly produced.

* The figures represent fiftieths of an inch. † In warm-blooded animals, the periods entered in this column are generally subsequent to death.

In many other experiments, the pupils are described as having contracted during the poisoning, but the exact changes were not measured. In the Table, Experiments LXXV., LXXVIII,, LXXXIII., LXXXVII., LXXXVIII. and LXXXIX. illustrate the rapid change that frequently occurs from contraction to dilatation; and it is obvious that unless special and continued attention be directed to the condition of the pupils, the contracted state will frequently escape detection. HARLEY,* AMÉDÉE VÉE,† NUNNELEY,‡ LASCHKEWICH§ and VAN HASSELT || agree with me in describing contraction of the pupils as one of the effects that follow the internal administration of Calabar bean.

It is, unfortunately, impossible to enter fully into the question of the method in which physostigma produces its effects on the pupil, as the physiology of pupillary changes is yet unsettled, and as even the structural anatomy of the iris is a subject of debate. I am anxious to avoid being committed to any theoretical assertion on this subject, especially as the opinions I previously expressed do not seem so certainly supported by my further experience as to permit of their reassertion.

Many endeavours have been made to arrive at some definite conclusion, and, although this has not yet been attained, as in the state of our knowledge of the anatomy and normal physiology of the iris it could not be, the probable method of action may be indicated with the aid of the following experiment.

Experiment XCIII.

The two sympathetic nerves were exposed at the neck of a white rabbit. Both pupils had a diameter of seven-fiftieths of an inch. The left sympathetic was divided.

In 2 minutes, left pupil = 5, right = 7.

I then endeavoured to fix definitely the strength of the weakest interrupted galvanic current that could so stimulate the portion of the divided sympathetic next to the eye as to produce dilatation of the pupil. For this purpose, DANIELL'S cell and DU BOIS REYMOND'S induction apparatus were employed.

When the secondary coil was at 500, the resulting current produced no effect on the pupil in 30 sec.
„ 480, „ „
„ 400, „ „
„ 300, „ „
„ 250, „ „
„ 200, „ „
„ 150, the left pupil dilated from 5 to 15, immediately.
„ 190, the resulting current produced no effect on the pupil in 30 sec.
„ 185, the left pupil dilated from 6 to 15 in 10 seconds.

One grain of extract, in fifteen minims of distilled water, was injected into the subcutaneous tissue of the right flank. In thirty seconds, tremors occurred; and the symptoms rapidly advanced to a fatal termination, fifteen minutes and thirty seconds after the administration of the poison.

* *Op. cit.* p. 140.
† Recherches sur la Fève du Calabar, 1865, p. 22, &c.
‡ *Op. cit.* p. 12.
§ *Op. cit.* p. 300.
|| Mentioned by DONDERS (Accommodation and Refraction of the Eye: New Sydenham Society, 1864) as having been observed in 1856; and, I am informed by Professor DONDERS, communicated to a scientific society, but not otherwise published by VAN HASSELT.

The examination of the cervical sympathetics was resumed soon after the poison was exhibited, with the following results :—

In 2 min.	after the administration, stimulation of the left sympathetic with the secondary coil at	190=0 in 30 seconds.
4 „	do. do.	185=dilatation from 7 to 15 in 10 sec.
10 „	do. do.	188=0 in 30 seconds.
10 „ 30 sec.	do. do.	185=dilatation from 5 to 15 in 10 sec.
12 „ 30 „	after the administration, left pupil = 4, right pupil = 5.	
13 „ 30 „	do. stimulation of the left sympathetic with the secondary coil at	185=0 in 30 seconds.
14 „ 0 „	after the administration, stimulation of the right sympathetic with the secondary coil at	185=0 „
14 „ 35 „	after the administration, stimulation of the left sympathetic with the secondary coil at	160=0 „
15 „ 30 „	do. do.	90=0 „

The rabbit was now dead, and both pupils had been for more than two minutes previously in a state of extreme contraction.

In 16 min. 10 sec.	after the administration, stimulation of the right sympathetic with the secondary coil at	30 = 0 in 30 seconds.
17 „ 0 „	after the administration, stimulation of the left sympathetic with the secondary coil at	10 = 0 „
18 „ 0 „	after the administration, stimulation of the right sympathetic with the secondary coil at	10 = 0 „

The left iris was at this time exposed by cutting away a portion of the cornea. The electrodes were applied directly to its surface, near the external margin, when a slight and rapid contraction of the iris (dilatation of the pupil) occurred, instantly followed by a rebound to its previous condition. This effect was frequently produced during many minutes after death, but no distinct expansion of the iris (contraction of the pupil) could be caused when the electrodes were applied in the same way to the pupillary margin. It must, however, be added that the pupil was at this time in a very contracted condition.

We learn from this experiment that the cervical sympathetic is paralysed before the death of the animal, while a portion of the apparatus that is immediately concerned in the contraction of the iris retains its vitality for a considerable period afterwards. The cause of the pupillary contraction during poisoning by internal administration is, therefore, in all probability, to be found among those consequences that naturally succeed the removal of the influence of the cervical sympathetic nerve. Without dogmatising on this subject, and feeling content in the meantime with the mere narration of these facts, I am inclined to think that such changes of the iris can only be explained by considering the influences of dilator and constrictor muscles, and also of a system of contractile blood-vessels.

An animal struggles violently during the action of a poison, and the pupil dilates. It is natural to suppose that, in this case, the excited spinal nerves had produced increased action of the dilator muscle, and that thereby the antagonism of the constrictor had been overcome. But if iridal movements are merely the results of spinal or cerebral nerve-force interfering with antagonism between muscles, how account for the continuation of either dilatation or contraction after the death of these nerves? Such conditions should not exist when the causes of interference with antagonism have been removed. I, therefore, venture (in common with others, and notwithstanding the anatomical difficulties that exist) to include an arrangement of contractile blood-vessels among the causes that produce iridal movements.

These blood-vessels will possess the function of erectile texture, and will act either in harmony with the dilator or constrictor muscles, or independently of them. The coincidence I have frequently observed between changes of blood-tension and differences in the size of the pupil also leads me to support this view. At the same time, the influence of an erectile tissue is not in itself sufficient to account for all the pupil changes; they can never be explained satisfactorily without also considering the effects of spinal and of cerebral motor nerves, operating probably on radiating and circular muscular fibres whose action is independent of such tissue. The cervical sympathetic appears to be the channel through which the nerves that originate in the cilio-spinal region pass to the iris. Stimulation of this region, or of the sympathetic nerve, produces dilatation of the pupil: and this does not interfere with the supposition of the existence of a contractile vascular network co-operating with a proper dilator muscle; for then the blood-vessels of this network, being governed by branches of the same nerves, would contract along with the dilator fibres, and the result would be a diminution in the size of the iris, and, consequently, a dilatation of the pupil. In the same way, division or paralysis of the sympathetic would result in iridal expansion; the contraction of the pupil being caused by dilatation of the blood-vessels, assisted, it may be, by the simultaneous contraction of a circular muscle.

B. TOPICAL EFFECTS.

When applied to the Nervous System.

It is obvious that when a poison is applied during life to the substance of any of the central nerve-organs, it will produce its specific action on the system in the ratio of the absorbing power of the organ, and therefore very much in proportion to the local blood supply. Other distinct effects are, however, frequently caused by the concentrated form and other peculiarities of the preparation. The watery suspension of physostigma extract caused no peculiar symptom when applied to the cerebrum of mammalians, birds, or frogs; and as its absorption was slow when so exhibited, the constitutional effects were produced only after long periods. When it was applied to the spinal cord of frogs, peculiar twitchings occurred in the muscles directly connected by motor nerves with the part of the cord in contact with the poison. These twitches soon ceased, and no movements were then caused when this portion of the cord was galvanised. The first effect was probably the result of local irritation merely, while the final paralysis was due to a specific action of physostigma.

From the nearly complete absence of blood-vessels in the trunks of the sciatic nerves, it is possible to localise the effects of a poison to any portion of the trunk. A curious result was produced by the topical application of Calabar bean. It has been shown that this poison does not appear to paralyse the afferent nerve fibres when acting through the blood; or, at least, that under its influence the function of the motor nerves is indubitably very much sooner destroyed than that of the sensory. When, however, the poison is applied to a mixed nerve-trunk, the order wherein these effects are produced is reversed, the afferent nerves being paralysed a few minutes before the efferent. This may be shown with great distinctness if strychnia be given after the local action has continued for some time.

Experiment XCIV.

The spinal cord of a frog was divided at the occiput, the sciatic nerves were exposed, and a piece of gutta-percha parchment was placed under each nerve, so as to isolate it completely. A small pad of cotton wadding *steeped in water*, was applied to the *right* nerve, while a similar pad *steeped in a concentrated mixture of extract of physostigma and water*, was placed on the *left;* care being taken to prevent the diffusion of any of the extract beyond the parchment. At this time, a slight stimulation of either nerve, below or above the pads, caused contraction of the limb and general reflex movements.

The pads were kept moist by an occasional drop of water on the right, and one of watery extract of physostigma on the left; and they were retained in their positions for an hour and forty minutes. They were then removed, and the left leg was carefully washed with distilled water, so as to remove effectually any extract that might have been adhering to the nerve. They were both tested with galvanism, when movements of the left leg followed the application of the poles to any portion of the exposed left sciatic; but no reflected contraction occurred when the nerve was stimulated below the position that had been occupied by the pad. No change had occurred either in the afferent or the efferent conductivity of any portion of the right nerve.

A drop of solution of strychnia was now applied to the wound that had been made in the neck by dividing the cord. Seven minutes after this, very weak galvanism of any portion of the right sciatic nerve caused a spasmodic shock of all the body. The same current produced a like effect when it was applied to the left nerve above the part that had been occupied by the poisoned pad; *but when it was applied to the poisoned part,* or *lower down,* the muscles below the point stimulated alone contracted, *no reflected movements being caused.* The same effects were repeatedly observed for other six minutes, before the lapse of which time the action of the strychnia had manifested itself more violently, and rendered the above peculiarities more distinct and exaggerated.

This experiment has been several times repeated, and has always yielded similar results.

If the poisoned pad be permitted to remain in contact with the nerve for a few minutes after the paralysis of its afferent fibres has been caused, the motor fibres also will have their conductivity destroyed.

Experiment XCV.

The sciatics of a frog were exposed, and were treated exactly as in the previous experiment. The afferent conductivity of that portion of nerve to which physostigma had been applied was lost in two hours, but the motor conductivity was yet retained. The application was continued, with the result that the conductivity of the motor fibres was destroyed within other fifteen minutes.

When applied to Striped and to Unstriped Muscle.

Although physostigma, when acting through the blood, does not destroy muscular contractility, the contact of a concentrated preparation is immediately followed by very rapid paralysis of the portion of muscle to which it is applied.

Experiment XCVI.

The two gastrocnemii muscles of a frog, and portions of the sciatic nerves in the thighs, were exposed and separated from contiguous structures. The muscles were completely isolated by pieces of parchment. A pad *soaked in a concentrated mixture of extract and water*, was placed on the surface of the right muscle, and a similar pad *moistened with water*, was placed on the left: the muscles being at the time completely under the control of their sciatic nerves, and being readily excited to contractions by direct galvanism.

Four minutes afterwards, a weak galvanic current was applied to the left nerve, and produced energetic contraction of all the muscles supplied by the nerve; and the left gastrocnemius muscle also contracted forcibly when directly stimulated. The same current was then applied to the right nerve, and caused pretty active movements of the right leg; in which, however, the gastrocnemius only sluggishly participated.

In fifteen minutes, the right or *poisoned gastrocnemius was perfectly paralysed; while the left muscle appeared to be as active functionally as when the experiment was commenced.*

The pad soaked in the extract was then removed from the right muscle and placed on the left. In five minutes, the contractility of the latter was considerably impaired; and in twenty minutes, no contraction could be produced, even when it was stimulated by very strong galvanism.

It was found that when a portion of intestine in energetic peristalsis, had its surface painted over with a concentrated watery mixture of the extract, it became flaccid; and that when a vermicular contraction ran along towards this portion, it stopped at the margin of the portion, and appeared to *skip over* it, as the peristalsis was resumed at the nearest unpoisoned point of the intestine. Soon afterwards, the poisoned portion of intestine could not be stimulated to contract by strong galvanism.

When applied to the Heart.

For the purpose of examining the topical effects on the heart, frogs were generally employed, but warm-blooded animals were in a few instances made use of.

The conditions of the experiments were varied by the application of the poison to the visceral pericardium, and to the muscular substance of the heart; without, and after, its removal from the body; and by the insertion of the poison into one of the cardiac chambers.

1. *Without its Removal from the Body.*

a. *To the Visceral Pericardium.*

Experiment XCVII.

The exposed heart of a frog was found, during ten minutes, to have an average rate of seventy beats in the minute.

A drop of filtered concentrated solution of the extract was placed on the pericardium.

In 1 minute,	heart = 52 per minute.	
2 ,,	= 40 ,,	
4 ,,	= 36 ,,	
6 ,,	= 30 ,,	
8 ,,	= 24 ,,	
10 ,,	irregular;	eighteen ventricular and twenty auricular contractions.
13 ,,	,,	no ventricular and eleven auricular contractions.
14 ,,	,,	,, ,,
22 ,,	= 8 per minute;	both chambers contract, but the ventricles very feebly.
25 ,,	= 12 ,,	
40 ,,	= 15 ,,	very feeble, but rhythmical.
50 ,,	irregular;	twelve ventricular and fifteen auricular contractions.
52 ,,	,,	no ventricular and eighteen auricular contractions.

The ventricles did not again contract spontaneously, but the auricular action continued for many hours. Voluntary movements were made by the frog until fifty minutes after the poison had been placed on its pericardium.*

b. *To the Heart.*

Experiment XCVIII.

A portion of the pericardium was removed from the exposed heart of a frog; and, during ten minutes that followed this operation, its contractions varied little from forty-five in the minute. A small drop of the same solution of extract as was employed in the previous experiment was placed on the heart's surface.

In ten seconds, heart = 0.

It continued motionless for thirty seconds; then recommenced; but, almost immediately afterwards, the frog struggled violently, and the contractions again ceased for ten seconds.

In 2 minutes,	heart = 27 per minute.	
4 ,,	= 21 ,,	
6 ,,	= 30 ,,	
8 ,,	= 34 ,,	Rhythmical and regular.

At this time, a second drop was applied to the heart.

In 30 seconds, a struggle occurred, and the heart stopped for ten seconds. When it recommenced, the contractions were irregular, only five ventricular to ten auricular occurring in the minute.

In 3 minutes,	there were no ventricular movements and 6 auricular.		
10 ,,	,,	20 ,,	
12 ,,	heart = 23 per minute;	rhythmical but irregular.	
20 ,,	,, = 28 ,,	,,	and regular.
40 ,,	,, = 28 ,,	,,	,,

A third drop was now applied.

In 2 minutes	after the third application, heart irregular,		no ventricular movement, and 24 auricular.
8 ,,	,,	,,	,,
18 ,,	,,	= 7 per minute,	both ventricles and auricles contracting.
25 ,,	,,	= 0	,,

* In this and in the other experiments of the series, a drop of water was occasionally placed on the heart to prevent its surface from drying.

Experiment XCIX.

A young rabbit was killed, and its heart was exposed. During four minutes, the contractions were eighty per minute. A concentrated solution of extract was painted over the greater portion of the heart's surface. In one minute, the contractions had entirely ceased; but, a few seconds afterwards, the left ventricle spontaneously resumed its action, and in two minutes after the application, the whole heart was contracting at the rate of seventy-six per minute.

The application was thrice repeated with similar results. Latterly, however, a longer interval occurred between the suspension and recovery of the cardiac contractility. Paralysis of the heart was ultimately caused by continuing these applications.

2. *After Removal from the Body.*

When the heart is removed from the body and placed in a concentrated solution of extract, its contractions immediately become irregular, and then cease. All the vital properties of its structures are paralysed in one or two minutes.

3. *Insertion of Physostigma into one of the Heart's Chambers.*

Experiment C.

A young rabbit was killed by the destruction of the medulla oblongata. In four minutes afterwards, while the heart was contracting at the rate of fifty per minute, two grains of extract, in five minims of water, were injected by WOOD'S syringe into the right auricle. The action of the heart instantly ceased. However, during the next ten minutes, irritation could still cause single laboured contractions.

TOPICAL ACTION ON THE BLOOD-VESSELS AND PIGMENT-CELLS WHEN APPLIED TO THE WEB OF THE FROG'S FOOT.

WHARTON JONES has examined with great care the changes in the calibre of the blood-vessels that follow the application of various substances to the frog's web.* Solution of atropia produces a marked contraction, a result that I have had occasion to confirm; and it, in this respect, resembles ordinary stimuli, such as galvanism, temporary cold and heat, and various irritants. He further established, and LISTER has supported and extended the statement,† that the contractile power of minute blood-vessels is independent of the central organs of the nervous system, though it may be controlled by them. A few substances were found that cause dilatation of blood-vessels, and among them solution of opium.

This antagonism in the actions of opium and atropia, and the well-known difference in their effects on the pupil, whether acting through the blood or acting by topical application, seem to have an important bearing on the question of how far the movements of the iris are due to calibre-changes of its blood-vessels. The examination of the topical effects of physostigma—a much more powerful

* On the State of the Blood-Vessels in Inflammation, &c.; *loc. cit.*

† An Inquiry regarding the parts of the Nervous System which regulate the Contraction of the Arteries; Philosophical Transactions, vol. cxlviii., 1858.

myositic agent than opium—is thus not only of great intrinsic interest, but also likely to be of considerable importance in explaining its action on the iris. In the experiments that were undertaken for this purpose, the condition of the pigment-cells also was observed.

Experiment CI.

The spinal cord of a light-coloured, large frog was divided between the first vertebra and the occiput; and, thirty minutes afterwards, a portion of a web was placed on the stage of a microscope.

An artery and three branches were selected for examination (A, B, C and D); and during ten minutes, their diameters occupied pretty constantly the following divisions of an eye-piece micrometer :—

A = 3; B = 2; C = 2; D = 1·5. The pigment-cells were diffusely stellate.

A small drop of filtered watery solution of extract was placed on the web.

In 2 minutes,	A = 3·0; B = 2·0; C = 2·0; D = 1·5.	Pigment-cells stellate,	Current of blood seems rather more rapid.
8 „	A = 3·0; B = 2·5; C = 2·5; D = 1·5.	„	„
15 „	A = 3·5; B = 3·0; C = 3·0; D = 1·5.	„	concentrating.
20 „	A = 4·0; B = 3·0; C = 3·0; D = 1·5.	„	concentrated : no rays.
40 „	A = 4·0; B = 3·0; C = 3·5; D = 2·0.	„	„
50 „	A = 4·0; B = 3·0; C = 3·5; D = 2·0.	„	diffusing a few rays.
1 h. 0 min.	A = 4·0; B = 3·0; C = 3·5; D = 2·0.	„	rather more diffuse than at commencement of experiment.
1 „ 10 „	A = 4·0; B = 3·0; C = 3·5; D = 2·0.	„	in nearly extreme diffusion. Circulation free.
1 „ 20 „	A second drop of the solution was placed on the web.		
1 „ 25 „	A = 4·0; B = 3·0; C = 3·5; D = 2·0.	Pigment-cells	concentrating.
1 „ 26 „	A = 4·0; B = 3·0; C = 3·5; D = 2·0.	„	stellate.
1 „ 30 „	A = 4·0; B = 3·0; C = 3·5; D = 2·0.	„	again diffusing.
1 „ 35 „	A = 4·0; B = 3·0; C = 3·5; D = 2·0.	„	in extreme diffusion.
2 „ 30 „	A = 4·0; B = 3·0; C = 3·5; D = 2·0.	„	„

Experiment CII.

An artery and two branches (A, B, C) and a capillary (D) were selected in the web of frog, which was prepared as in the preceding experiment. Their diameters were found to be the following—A = 7; B = 6·5; C = 4; D = 1 : while the pigment-cells in the field were in a state of extreme concentration, there being no rays visible.

A drop of a filtered, strong solution of extract was placed on the web.

In 10 minutes,	A = 9; B = 8; C = 5; D = 1.	Pigment-cells in extreme concentration. Circulation active.
20 „	A = 9; B = 8; C = 5; D = 1.	„ extreme concentration. Circulation active.
40 „	A = 9; B = 8; C = 5; D = 1.	„ stellate. Circulation languid.

The observations were continued until two hours after the application; and, up to that time, no further change had occurred, except that extreme diffusion had appeared in the pigment-cells.

Experiment CIII.

A portion of the web of a frog was arranged for microscopic examination, in the same way as in the two preceding experiments. An artery and two branches (A, B, C) were selected for examination, along with several adjoining pigment-cells.

Before the application, A = 4 ; B = 2 ; C = 1·5 : while the pigment-cells were in the stellate form.

A drop of a filtered, strong solution of extract was placed on the web.

In 5 minutes,	A = 5; B = 2; C = 1·5.	Pigment-cells	concentrated, with a few short rays,	Circulation more rapid; and many capillaries that were before empty now contain an actively moving blood stream.
10 „	A = 5; B = 2; C = 1·5.	„	in extreme concentration.	
20 „	A = 5; B = 2; C = 1·5.	„	„	Circulation continues active.
30 „	A = 5; B = 2; C = 1·5.	„	„	„

The action of physostigma on the calibre of the minute and contractile blood-vessels is thus the reverse of that of atropia.

Lister believes that the pigment-cells possess a nerve apparatus that governs their condition in exactly the same way as the smpathetic ganglia and spinal nerves govern the calibre-changes of the smaller blood-vessels.* If this be so, the long continued concentration, which precedes the diffused condition of the pigment in these cells, would imply that their nerves are more sensitive to irritation than those of the vascular system, and that this pigmentary concentration is the result of a stimulating property of the extract, of so slight a character as to have no effect on the ganglia and nerves of the blood-vessels.

When applied to the Eyeball, or to its immediate neighbourhood.

As this investigation has been confined to the lower animals, the action on the iris only can be here discussed. This has been observed in amphibia, reptiles, birds, and mammalians. The pupil contracts within a few minutes after the application of physostigma to the eyeball, to the cutaneous surface in its neighbourhood, or to the nasal mucous membrane; and, if the quantity have been considerable, this may last for two or three days. It is caused much more rapidly, and maintained for a much longer time, than the often varying state of contraction that has been described as a symptom produced by physostigma acting through the blood. There is no reason to doubt that it is as purely an effect produced by contact with the iris as the opposite state that atropia causes; and so limited is the action, that in the same animal extreme physos-

* On the Cutaneous Pigmentary System of the Frog; Philosophical Transactions, vol. cxlviii., 1858, p. 627.

tigma-myosis may exist in one eye, while atropia-mydriasis is present in the other. It is easy to prove the presence of the extract within the eyeball, after its topical application, by removing the aqueous humour and placing it on the conjunctiva of another animal, when the usual effects of Calabar bean on the pupil will be produced.

In addition to this phenomenon, increased lachrymation, and congestion of the conjunctiva, and, I believe, of the iris itself, are produced.

The question of the effects produced on the eye by the topical application of such substances as physostigma and atropia must always be an interesting one to physiologists. Its full discussion would require a special paper, so numerous are the data to be considered, and so conflicting the opinions that have been expressed. Besides, there are some effects that can only be conveniently examined in man, and which must be included in any satisfactory review of this attractive subject. It is, however, quite within the purpose of the present investigation to consider the method in which the iris is expanded by physostigma. I have already expressed an opinion that iridal changes appear to require the co-operation of special radiating and circular muscular fibres, with a system of contractile blood-vessels possessing to a certain extent the properties of erectile tissue. A mere antagonism between the two former muscular arrangements could not alone account for the effects of either physostigma or atropia; and, indeed, the actions of those substances seem to oppose such a theory. All the muscular fibres in the iris are unstriped, and physostigma relaxes while atropia contracts such fibres. Therefore, were these the only causes of iridal movement, physostigma would produce no effect on the pupil; for it would merely render less energetic the contraction of both, and would not thereby disturb their counterbalancing antagonism. For a similar reason, atropia will merely increase the opposing action of both, without causing any iridal movement.

To reconcile their actions with this anatomical arrangement, it is necessary to make one of the following very improbable suppositions—that the circular muscle at the pupillary margin has different physiological properties from the dilator; or that the one set of fibres is regulated by nervous ganglia that can be stimulated by the contact of substances, which paralyse corresponding ganglia whose power is limited to the other set.

Let it be granted, however, that dilator and constrictor muscles with counterbalancing powers exist, and that, in addition, a system of contractile blood-vessels is present, and the ascertained physiological actions of these opposing substances will be rendered available for the explanation of their peculiar effects. Dilatation of the pupil by atropia will then be due to the successful opposition of radiating muscular fibres and contractile iridal blood-vessels, to unaided constrictor fibres; the contact of atropia causing contraction of all unstriped fibres, including those of the blood-vessels: while contraction of the pupil by physostigma will be intelligibly explained by the universal relaxation of all the contractile tissues of the iris, and the consequent enlargement of its area by the increased accession of blood, which the dilated vessels attract and permit.

The topical action of physostigma on the blood-vessels has been already described, and it certainly supports the view just stated. Confirmatory evidence is also obtained from ADAMÜK's recent experiments on intra-ocular pressure,* which show that this is increased by extract of Calabar bean applied to the conjunctiva and diminished by atropia; for such a difference of effect would necessarily exist, did physostigma augment the blood supply of the iris and atropia decrease it.

The additional effects that follow the topical application to the human eye-ball will be mentioned among the general conclusions.

The following are the conclusions of this investigation:—

A. ACTION THROUGH THE BLOOD.

1. Physostigma has proved fatal to every animal hitherto examined, with the exception of the Esĕrĕ moth. In mammals and birds, death is most rapidly caused when the poison is injected into the circulation or when it is brought into contact with a wounded surface. It follows nearly as quickly, when Calabar bean is introduced into a serous cavity; much more slowly when it is exhibited by the mucous membrane of the digestive system. In rabbits, death has been caused by its application to the Schneiderian, the auditory or the conjunctival mucous membrane. The skin of frogs resists the poison for a long time; but, if it be applied for a considerable period, and with proper precautions, distinct evidence of absorption may be obtained, though death has never been caused by such application.

2. The contact of the extract of Calabar bean with the gastric juice of a dog, for twenty-four hours and at a temperature a little above 95° F., did not, in the slightest degree, modify the energy of the poison.

3. A large dose, given to a mammal or bird, rapidly affects the cardiac contractions, and then paralyses the heart. The respiratory movements are quickly stopped, but the symptoms and post mortem appearances are those of syncope. Such a dose, injected into the abdominal cavity of a frog, affects nearly simultaneously the heart and spinal cord, and very rapidly destroys the vitality of both organs. In this case the motor nerves are only slightly, or not at all affected, and may retain their conductivity for about thirty hours. Evidence of

* Centralblatt, No. 36, 1866; and RUTHERFORD's Report on Physiology, Journal of Anatomy and Physiology, No. II., 1867.

the vitality of the afferent nerves may be obtained as long as the retained vitality of the spinal cord permits of its diastaltic function being examined.

4. In mammals and birds, an average dose produces symptoms of asphyxia. When administered to frogs, a similar dose impairs the function of the spinal cord, and diminishes the rates of the cardiac contractions and of the respiratory movements; and, soon after, the latter cease. In periods varying from one and a-half to four hours afterwards, the motor nerves are paralysed; this paralysis implicating their endorgans first, and their trunks afterwards. From this it must not be inferred that the nerve is paralysed by a centripetal progression of the poison; the only fact demonstrated being that a direct ratio exists between, on the one hand, subdivision of nerve substance, facilitating contact of the poison, and, on the other, rapidity of paralysing effect. Indeed, division of the nerve trunk, previous to the administration of Calabar bean, delayed the paralysis of the endorgans. The afferent nerves retain their activity as long, at least, as the functions of the spinal cord are not lost. The spinal cord and the motor nerves are generally paralysed at about the same time.

5. When a small, but still fatal, dose of Calabar bean is administered to a frog, the effects are the same as those in the previous conclusion, until they arrive at the stage of paralysis of the motor nerves; after this, an interval of several hours may elapse before the functions of the spinal cord are completely suspended. During this interval the *tactile* sensibility of the afferent nerves is increased: so that, if the ischiadic artery and vein of one limb have been tied before the exhibition of the poison, a slight touch of the skin in the poisoned region, which before the administration of the poison caused no effect, will now produce faint twitches of the limb whose vessels are tied; while an ordinary excitant, such as sulphuric acid, will show everywhere a marked diminution in the diastaltic activity, as measured by the métronome.

6. *With a still smaller dose, a frog may have its cardiac contractions reduced by from seventy to eight per minute, its respiratory movements completely stopped, and the endorgans of its motor nerves paralysed, and yet afterwards completely recover.* This has occurred when two grains of extract were administered to a frog, weighing 730 grains.

7. In frogs, the voluntary muscles are unaffected by the poison, and may continue to respond to galvanic stimulation during three or four days after its administration. The contrast between this and the effect of Calabar bean on the motor nerves, may be well shown by ligaturing the ischiadic vessels of one limb before injecting the poison. If, when strong stimulation causes no reflex movement, the two gastrocnemii muscles with their attached nerves be so placed that an interrupted current, from one DANIELL's cell and DU BOIS REYMOND's induction apparatus, may be transmitted simultaneously through either both muscles or both nerve trunks, it will be found in the case of the

muscles that when the secondary coil is slowly advanced contractions will occur with the same current in both muscles, or with a weaker current in the case of the poisoned than in that of the non-poisoned one, this varying with the length of time which has elapsed since the limb was deprived of blood; when the current is transmitted through both nerves, contractions will be produced simultaneously in both muscles, or with a weaker current in the non-poisoned one, or contractions will occur in the non-poisoned muscle only, this also varying with the length of time that may have elapsed since the exhibition of the poison.

8. In mammals and birds, the voluntary muscles are affected in a very remarkable manner. At an early stage of the poisoning, faint twitches occur, which gradually extend over the body, and, at the same time, increase in vigour so as to interfere with the respiratory movements. Shortly before death, they again become mere successive twitches, often requiring the use of the hand to discover their existence. After death, if a muscular surface be exposed, these twitches will still be observed, involving usually different muscular fasciculi at different times, rarely the whole of a muscle at once; and in mammals they may persist for more than thirty minutes after death. They are caused by a direct effect of physostigma on the muscular substance. This is shown by their continuing after paralysis of the motor nerves, by their persisting in a muscle cut out of the body, and by their non-occurrence in parts that have been separated by ligature from the circulation.

9. In mammals and birds, when the dose is large, the heart's action is rapidly made slower and then stopped. In dogs, it may diminish to one-half in three minutes, and cease in ten. In frogs also, a large dose, injected into the abdominal cavity, causes rapid and complete cardiac paralysis. A smaller dose causes either a gradual cessation followed by a renewal at a diminished rate, or a gradual fall, from sixty or seventy to four or six beats per minute, followed by a gradual return to a diminished rate of from eight to twenty per minute. At this stage, and for many hours afterwards, the only signs of vitality are this diminished cardiac action and the power of the voluntary muscles to respond to galvanic and other stimulation. In the frog, where alone these last phenomena have been observed, the heart may continue so to contract for three or even five days, provided the temperature of the apartment be as low as 50° F. After stoppage, galvanism may sometimes cause a renewal of rhythmical contractions; but this can rarely be done, and unrhythmical and partial contractions can alone be excited. Cessation of the heart's contractions occurs in diastole, with all the chambers full.

10. The pneumo-gastric nerves retain their inhibitory power over the heart during the whole time from the diminution to the partial recovery of its action. Soon after this, however, they are paralysed; and this occurs at nearly the same time as the affection of the motor nerves.

11. Division of the pneumo-gastric nerves, or their paralysis by curare, or destruction of the medulla oblongata or spinalis, does not protect the heart from the action of physostigma.

12. The lymphatic hearts of frogs poisoned by Calabar bean soon cease to contract.

13. In rabbits, a large dose paralyses the cervical sympathetic nerves, before the death of the animal. A smaller fatal dose merely diminishes their activity.

14. Before the stoppage of the heart, proofs may be obtained of the vitality of its sympathetic ganglia; but, as striped muscle is not affected by Calabar bean conveyed by the blood, we are obliged to infer from the symptoms respectively produced, that the activity of the cardiac sympathetic system is probably destroyed by a large dose, and lessened by a smaller one.

15. The animal temperature, both external and internal, has been invariably observed to rise in rabbits and dogs, but only slightly and for a short period; after which it slowly falls.

16. The condition of the capillary circulation was examined in the web of the frog. Soon after the exhibition of the poison, the smaller arteries and veins contracted slightly; but, after a short interval, this contraction was succeeded by a rapid and permanent dilatation, in which the calibre of the vessels was considerably above their maximum previous to the poisoning. This capillary dilatation appears to occur all over the body, as is shown by a peculiar blue coloration of the voluntary muscles and of the heart, a similar coloration of the serous and fibro-serous tissues, and a congestion of the blood-vessels in the conjunctiva and iris. This change also occurs, in a less marked manner, in birds and mammals.

17. The general results of experiments in which the arterial and venous tensions were examined were, that the arterial tension first diminished slightly, immediately after the administration of the poison, then gradually increased until it reached its maximum—when the number of cardiac contractions had diminished to at least one-half,—and afterwards rapidly fell; and that the venous tension began to increase immediately after the administration, continued doing so until it slowly reached its maximum—when the arterial tension had considerably diminished,—and then fell, though more gradually than the tension of the arterial system. The number of the cardiac contractions when the venous tension had attained its maximum, was about one-third of the average before the poisoning; the respirations were rather less frequent than before, and the temperature had risen a few tenths of a degree.

18. Physostigma causes extreme diffusion in the pigment-cells of the frog's skin, and thus a very marked change occurs in the colour of the animal during the progress of the symptoms.

19. In dogs, the peristaltic action of the intestines is usually destroyed at death; it may, however, continue a short time afterwards. In rabbits, the intes-

tinal movements are frequently increased in activity before death, and generally continue for a considerable time afterwards.

20. The pupil contracts in all cases of rapid poisoning in mammalians and birds. The contraction may, however, be slight and of short duration; and dilatation may then be observed during the greater portion of the experiment, especially if the dose be a small one. Contraction of the pupil is produced in frogs also.

21. Calabar bean acts as an excitant of the secretory system; increasing the action of the alimentary mucous, of the lachrymal, and of the salivary glands.

22. In the frog, the symptoms of poisoning are not materially altered by removal of the brain, or by division of the cervical portion of the spinal cord.

23. Artificial respiration does not prevent death, in mammals, after the exhibition of a poisonous dose. This is a necessary result of the effects of physostigma on both the cerebro-spinal and sympathetic systems.

24. Congestion of internal organs occasionally occurs; but this is by no means an invariable consequence of a fatal dose.

25. The blood is generally dark after death, but becomes arterialised on exposure to the air; its respiratory capabilities are unaltered; it often clots loosely and imperfectly; and, when examined with the spectroscope, the bands of scarlet crurine are found unchanged. In the rabbit and dog, a microscopic examination demonstrates an invariable change in the coloured corpuscles, which have their outlines distinctly crenated. This change is not caused in the blood of birds or amphibia. The white corpuscles remain unaltered.

B. TOPICAL EFFECTS.

1. When the poison is applied to the surface of a frog's brain, no effect is produced; but when it is brought into contact with the spinal cord, a few twitches occur in the extremities, followed by paralysis of the portion of cord acted upon.

2. When physostigma is applied to a mixed nerve-trunk, in a concentrated form and with proper precautions to prevent absorption by neighbouring parts, first the afferent nerve-fibres are paralysed, and afterwards the efferent.

3. Topical application destroys the contractility of striped and of unstriped muscular fibre. The heart's action is stopped by repeated application to its external surface or to the pericardium. If a small quantity be injected into one of its chambers, paralysis nearly immediately follows.

4. The blood-vessels are dilated when a solution is applied to the web of the frog's foot.

5. The effects of the application of Calabar bean to the eyeball are a somewhat painful sensation of tension in the ciliary region, contraction of the pupil, myopia and astigmatism; with, frequently, congestion of the conjunctival vessels, pain in the supra-orbital region, and twitches of the orbicularis palpebrarum muscle.

PROCEEDINGS

OF THE

STATUTORY GENERAL MEETINGS,

AND

LIST OF MEMBERS ELECTED AT THE ORDINARY MEETINGS,

SINCE JANUARY 3, 1865,

WITH

LIST OF DONATIONS TO THE LIBRARY,

FROM NOV. 28, 1864, TILL NOV. 25, 1867.

PROCEEDINGS, &c.

Monday, 28th November 1864.

At a Statutory General Meeting, His Grace the Duke of Argyll, President, in the Chair, the Minutes of the Statutory Meeting of 23d November 1863 were read and confirmed.

The following Office-Bearers were elected for 1864–65:—

Principal Sir David Brewster, K.H., LL.D., D.C.L., President.

Dr Christison,
Professor Kelland,
Hon. Lord Neaves,
Principal Forbes,
Professor Innes,
Professor Lyon Playfair, C.B., } Vice-Presidents.

Dr John Hutton Balfour, General Secretary.

Dr George James Allman,
Professor P. Guthrie Tait, } Secretaries to the Ordinary Meetings.

David Smith, Esq., Treasurer.

Dr Douglas Maclagan, Curator of Library and Museum.

COUNCILLORS.

Dr William Robertson.
Dr E. Ronalds.
T. C. Archer, Esq.
W. F. Skene, Esq.
A. Keith Johnston, Esq.
Rev. Dr Stevenson.
Dr Stevenson Macadam.
Hon. Lord Jerviswoode.
James T. Gibson-Craig, Esq.
Edward Sang, Esq.
Sir James Coxe, M.D.
Rev. Dr Blaikie.

The following List of Honorary Members was submitted before being printed in the billet of next meeting:—

I. FOREIGN.

Robert Wilhelm Bunsen, Heidelberg.
Auguste de la Rive, Geneva.
Jean Bernard Leon Foucault, Paris.
Elias Fries, Upsala.
Hermann Helmholtz, Heidelberg.
Albert Kolliker, Wurzburg.
Richard Lepsius, Berlin.
Rudolf Leuckart, Giessen.
Theodor Mommsen, Berlin.
Adolphe Pictet, Geneva.
Christian Friedrich Schönbein, Basle.
Karl Theodor von Siebold, Munich.

II. BRITISH.

JOHN STUART MILL, London.
GEORGE GABRIEL STOKES, Cambridge.
ALFRED TENNYSON, Freshwater, Isle of Wight.

The Council reported that they had awarded the Makdougall Brisbane Prize for the biennial period 1862–64 to JOHN DENIS MACDONALD, R.N., F.R.S., Surgeon to H.M.S. "Icarus," for his Zoological Papers published in the Transactions of the Society during the period.

It was moved by Lord NEAVES, and seconded by Dr CHRISTISON, That the cordial thanks of the Society be given to His Grace the DUKE of ARGYLL for his services during the term of his office as President. This motion was carried by acclamation.

It was moved by Lord NEAVES, and seconded by Dr CHRISTISON, That it is expedient that those Fellows who have filled the office of President should, on retiring from that office, become Honorary Vice-Presidents of the Society, and that it be remitted to the Council to carry this into effect. This motion was agreed to unanimously.

With the view of supplying an omission in the retirement of the late Treasurer, J. T. GIBSON-CRAIG, Esq., Dr CHRISTISON moved, seconded by Professor BALFOUR, that the cordial thanks of the Society be returned to Mr J. T. GIBSON-CRAIG, for his efficient services as Treasurer, and for the great attention which he had paid to the business of the Society. This was carried unanimously.

On the motion of Dr BURT the following Gentlemen were appointed to audit the Treasurer's accounts:—

WILLIAM CHAMBERS, Esq. JAMES CUNNINGHAM, Esq. Dr STEVENSON MACADAM.

The Meeting then adjourned.

(Signed) PHILIP KELLAND, *V.P.*

Monday, 27th November 1865.

At a Statutory General Meeting, Professor KELLAND, V.P., in the Chair, the Minutes of the Statutory Meeting of 28th November 1864 were read and confirmed.

The following Office-Bearers were elected for 1865–66.

Principal Sir DAVID BREWSTER, K.H., LL.D., D.C.L., President.
His Grace the DUKE of ARGYLL, Honorary Vice-President, having filled the Office of President.

Professor Kelland,
Hon Lord Neaves,
Principal Forbes,
Professor Innes,
Professor Lyon Playfair, C.B.,
D. Milne-Home, Esq.
} Vice-Presidents.

Dr John Hutton Balfour, General Secretary.

Dr George James Allman,
Professor P. Guthrie Tait,
} Secretaries to the Ordinary Meetings.

David Smith, Esq., Treasurer.

Dr Douglas Maclagan, Curator of Library and Museum.

COUNCILLORS.

A. Keith Johnston, Esq.
Rev. Dr Stevenson.
Dr Stevenson Macadam.
Hon. Lord Jerviswoode.
James T. Gibson-Craig, Esq.
Edward Sang, Esq.
Sir James Coxe, M.D.
Rev. Dr Blaikie.
Dr Christison.
Dr A. Crum Brown.
Dr Burt.
Professor Macdougall.

The following Gentlemen were proposed as Honorary Fellows of the Society:—

I. FOREIGN.

Angelo Secchi, Observatory, Rome.

II. BRITISH.

Lieut.-General Edward Sabine, R.A., President of the Royal Society of London.
Charles Darwin, Esq., M.A., Down, Bromley, Kent.
Arthur Cayley, Esq., Professor of Mathematics, Cambridge.

It was resolved that in future the accounts should be audited by a professional auditor, and that they should be presented at the general meeting in November.

It was moved by Dr Burt, and seconded by John Blackwood, Esq., that George Auldjo Jamieson, Esq., should be appointed to audit the Treasurer's accounts. The motion was agreed to.

The Meeting adjourned.

(Signed) James Coxe, *Chairman.*

Monday, November 26, 1866.

At a Statutory General Meeting, Sir JAMES COXE, Councillor, in the Chair, the Minutes of the Statutory Meeting of 27th November 1865 were read and confirmed.

The following Office-Bearers were elected for 1866–67:—

Principal Sir DAVID BREWSTER, K.H., LL.D., D.C.L., President.
His GRACE THE DUKE OF ARGYLL, Honorary Vice-President, having filled the Office of President.

Hon. Lord NEAVES, Principal FORBES, Professor INNES, Prof. LYON PLAYFAIR, C.B., D. MILNE-HOME, Esq , Dr CHRISTISON,	Vice Presidents.

Dr JOHN HUTTON BALFOUR, General Secretary.

Dr GEORGE JAMES ALLMAN, Professor TAIT,	Secretaries to Ordinary Meetings.

DAVID SMITH, Esq., Treasurer.
Dr DOUGLAS MACLAGAN, Curator of Library and Museum.

COUNCILLORS.

JAMES T. GIBSON-CRAIG, Esq.	Professor KELLAND.
EDWARD SANG, Esq.	Dr MATTHEWS DUNCAN.
Sir JAMES COXE, M.D.	WILLIAM TURNER, M.B.
Rev. Dr BLAIKIE.	Dr JOHN MUIR.
Dr A. CRUM BROWN.	Rev. THOMAS BROWN.
Dr BURT.	JAMES SANDERSON, Esq.

The Treasurer's book was laid on the table, with the Auditor's Report.

On the motion of Dr BURT, seconded by Dr SELLER, GEORGE AULDJO JAMIESON, Esq., was elected Auditor for next year.

The Council recommended the election of the following Honorary Fellows to fill up the vacancies caused by the death of Professor ROGERS and of DR WHEWELL :—

I. FOREIGN.

MICHEL EUGENE CHEVREUL, Paris.

II. BRITISH.

THOMAS CARLYLE, Esq , London.

The Secretary stated that during last year the following vacancies had occurred in the List of the Society:—

Members Deceased,	. .	14
,, Resigned,	. .	4
,, Cancelled,	. .	4
	Total,	22

The present number of Ordinary Fellows was 276.

The Meeting adjourned.

LIST OF MEMBERS ELECTED.

January 3, 1865.

ALFRED R. CATTON, M.A. Rev. FRANCIS REDFORD, M.A.

January 16, 1865.

JAMES STEVENSON, Esq.

March 6, 1865.

Dr JOHN MOIR.

April 3, 1865.

JAMES POWRIE, Esq. CHARLES JENNER, Esq.

April 17, 1865.

CHARLES LAWSON, Jun., Esq.

December 4, 1865.

Dr ALEXANDER KEILLER.

December 18, 1865.

The Right Rev. Bishop MORRELL. WILLIAM EUING, Esq.

January 2, 1866.

Dr FRASER THOMSON. JOHN M'CULLOCH, Esq.
Dr T. GRAINGER STEWART. Colonel Sir JAMES E. ALEXANDER of Westerton.

January 15, 1866.

Dr CHARES MOREHEAD. Professor DAVID MASSON.
DAVID DOUGLAS, Esq.

February 5, 1866.

JOHN MACNAIR, Esq. Professor SPENCE.
THOMAS NELSON, Esq.

February 19, 1866.

ADAM BLACK, Esq. ALEXANDER MACDUFF, Esq. of Bonhard.
THOMAS CONSTABLE, Esq. Dr JAMES DUNSMURE.
Dr ARTHUR MITCHELL.

March 5, 1866.

Dr PATRICK HERON WATSON.

April 2, 1866.

Dr JOHN SMITH. JAMES FALSHAW, Esq., C.E.

April 16, 1866.

John K. Watson, Esq.
David Chalmers, Esq.

January 7, 1867.

T. B. Johnston, Esq.
David Davidson, Esq.
Sir George Harvey.
George F. Barbour, Esq. of Bonskeid.
Peter Waddell, Esq.
George Stirling Home Drummond, Esq. of Ardoch.
Professor Fuller.

January 21, 1867.

Dr Andrew Graham.
A. H. Bryce, LL.D.
Dr Arthur Gamgee.
William Turnbull, Esq.
Francis Deas, LL.B.
Sheriff Hallard.

February 4, 1867.

Dr Thomas R. Fraser.
Thomas Annandale, Esq.
Dr D. R. Haldane.

February 18, 1867.

John M. M'Candlish, Esq., W.S.

March 4, 1867.

James Donaldson, LL.D.
James Richardson, Esq.

March 18, 1867.

James H. B. Hallen, Esq.

April 1, 1867.

Henry Dircks, Esq., C.E.

April 15, 1867.

Dr Charles Gainer.
William Keddie, Esq.

April 29, 1867.

Rev. Dr Lindsay Alexander.

LIST OF THE PRESENT ORDINARY MEMBERS,

Corrected up to November 1, 1867.

IN THE ORDER OF THEIR ELECTION.

PRESIDENT.

PRINCIPAL SIR DAVID BREWSTER, K.H., LL.D., D.C.L.

HONORARY VICE-PRESIDENT, HAVING FILLED THE OFFICE OF PRESIDENT.

HIS GRACE THE DUKE OF ARGYLL, K.T.

Date of Election.

1808 James Wardrop, Esq., F.R.C.S.E., London.
Sir David Brewster, K.H., LL.D., F.R.S., Lond., *Principal of the University of Edinburgh.*
1812 Sir George Clerk, Bart., F.R.S., Lond.
1818 Patrick Miller, M.D., *Exeter.*
1820 Charles Babbage, F.R.S., Lond.
Sir John F. W. Herschel, Bart., F.R.S., Lond.
Dr William Macdonald, F.R.C.P.E., *Professor of Natural History, St Andrews.*
1821 Robert Hamilton, M.D., F.R.C.S.E.
1822 George A. Walker-Arnott, LL.D., *Professor of Botany, Glasgow.*
Sir James South, F.R.S., Lond.
Sir W. C. Trevelyan, Bart., *Wallington, Northumberland.*
1823 Captain Thomas David Stuart, *of the Hon. East India Company's Service.*
Warren Hastings Anderson, Esq.
Alexander Thomson, Esq., *of Banchory.*
Liscombe John Curtis, Esq., *Ingsdon-House, Devonshire.*
Robert Christison, M.D., *Professor of Materia Medica.*
1824 Robert E. Grant, M.D., *Professor of Comparative Anatomy, University College, London.*
Rev. Dr William Muir, *one of the Ministers of Edinburgh.*
1827 Very Rev. Edward Bannerman Ramsay, M.A. Camb., LL.D.
1828 John Forster, Esq., *Architect, Liverpool.*
Thomas Graham, M.A., D.C.L., F.R.S., *Master of the Mint, London.*
David Milne-Home, Esq., *Advocate, of Milne-Graden and Wedderburn.*
Dr Manson, *Nottingham.*
1829 A. Colyar, Esq.
Right Hon. Sir William Gibson-Craig, Bart. *of Riccarton.*

Date of Election.

1829 Right Honourable Lord Colonsay.
Venerable Archdeacon Sinclair, *Kensington.*
James Walker, Esq., W.S.
1830 J. T. Gibson-Craig, Esq., W.S.
James Syme, Esq., *Professor of Clinical Surgery.*
Thomas Barnes, M.D., *Carlisle.*
1831 James D. Forbes, D.C.L., F.R.S., Lond., *Principal of the United College, St Andrews.*
1832 Montgomery Robertson, M.D.
1833 Rear-Admiral Sir Alexander Milne, R.N.
His Grace the Duke of Buccleuch, K.G., *Dalkeith Palace.*
Alexander Hamilton, LL.B., W.S.
1834 Mungo Ponton, Esq., W.S., *Clifton, Bristol.*
Isaac Wilson, M.D., F.R.S., Lond.
Patrick Boyle Mure Macredie, Esq., *Advocate, Perceton.*
William Sharpey, M.D., LL.D., F.R.S., *Professor of Anatomy, University College, London.*
1835 John Hutton Balfour, A.M., M.D., F.R.S., *Professor of Medicine and Botany.*
William Brown, Esq., F.R.C.S.E.
Robert Mayne, Esq.
1836 David Rhind, Esq., *Architect.*
1837 John Scott Russell, Esq., A.M., *London.*
Archibald Smith, Esq., M.A., Camb., F.R.S., *Lincoln's Inn, London.*
Richard Parnell, M.D.
Peter D. Handyside, M.D., F.R.C.S.E.
1838 Thomas Mansfield, Esq., *Accountant.*
1839 David Smith, Esq., W.S.
Adam Hunter, M.D., F.R.C.S.E.
Rev. Philip Kelland, A.M., F.R.S., *Professor of Mathematics.*
Francis Brown Douglas, Esq, *Advocate.*
1840 Alan A. Maconochie Welwood, Esq., *of Meadowbank and Pitliver.*
Martyn J. Roberts, Esq., *Fort-William.*
Robert Chambers, LL.D.
Sir John M'Neill, G.C.B., LL.D.
Sir William Scott, Bart., *of Ancrum.*
Right Rev. Bishop Terrot.
Edward J. Jackson, Esq.
John Mackenzie, Esq.
James Anstruther, Esq., W.S.
1841 John Miller, Esq., *of Leithen.*
James Dalmahoy, Esq.
1842 James Thomson, Esq., *Civil-Engineer, London.*
John Davy, M.D., *Inspector-General of Army Hospitals.*
Robert Nasmyth, Esq., F.R.C.S.E.
1843 A. D. Maclagan, M.D., *Professor of Medical Jurisprudence.*
John Rose Cormack, M.D., F.R.C.P.E., *Orleans, France.*

Date of Election.

1843 Allen Thomson, M.D., F.R.S., *Professor of Anatomy, Glasgow.*
Joseph Mitchell, Esq., *Civil Engineer Inverness.*
Andrew Coventry, Esq., *Advocate.*
John Hughes Bennett, M.D., *Professor of Physiology.*
D. Balfour, Esq., *of Trenaby.*
Henry Stephens, Esq.
1844 Archibald Campbell Swinton, Esq., *of Kimmerghame.*
James Begbie, M.D., F.R.C.P.E.
Sir James Y. Simpson, Bart., M.D., *Professor of Midwifery.*
David Stevenson, Esq., *Civil Engineer.*
Thomas R Colledge, M.D., F.R.C.P.E.
1845 John G. M. Burt, M.D., F.R.C.P.E.
Thomas Anderson, M.D., *Professor of Chemistry, Glasgow.*
1846 A. Taylor, M.D., *Pau.*
Alexander J. Adie, Esq., *Civil Engineer.*
L. D. B. Gordon, Esq., C.E.
L. Schmitz, LL.D., Ph.D., *International Institution, London.*
Charles Piazzi Smyth, Esq., F.R.S., *Professor of Practical Astronomy.*
1847 Sir William Thomson, M.A., Camb. LL.D., F.R.S., *Professor of Natural Philosophy, Glasgow.*
J. H. Burton, Esq., LL.D., *Advocate.*
James Nicol, Esq., *Professor of Natural History, Aberdeen.*
William Macdonald Macdonald, Esq., *of St Martins.*
John Wilson, Esq., *Professor of Agriculture.*
Moses Steven, Esq., *of Bellahouston.*
1848 Thomas Stevenson, Esq., C.E.
James Allan, M.D., *Inspector of Hospitals, Portsmouth.*
Henry Davidson, Esq.
William Swan, Esq., *Professor of Natural Philosophy, St Andrews.*
Patrick James Stirling, Esq.
1849 Sir William Stirling-Maxwell, Bart., Esq., *of Keir and Pollok,* M.P.
William Thomas Thomson, Esq.
W. H. Lowe, M.D., F.R.C.P.E., *Balgreen.*
Honourable B. F. Primrose.
David Anderson, Esq., *of Moredun.*
W. R. Pirrie, M.D., *Professor of Surgery, Aberdeen.*
His Grace the Duke of Argyll, *Inverary Castle.*
The Most Noble the Marquis of Tweeddale, K.T., *Yester House.*
Edward Sang, Esq.
1850 William John Macquorn Rankine, Esq., LL.D., F.R.S., *Professor of Civil Engineering, University, Glasgow.*
Alexander Keith Johnston, LL.D.
Sheridan Muspratt, M.D., *Liverpool.*
James Stark, M.D., F.R.C.P.E. (Re-admitted.)
Lieutenant-Colonel W. Driscoll Gossett, R.E.

Date of Election.

1850 William Seller, M.D., F.R.C.P.E.
Hugh Blackburn, Esq., *Professor of Mathematics, Glasgow.*
J. S. Combe, M.D., F.R.C.S.E.
1851 Sir David Dundas, Bart., *of Dunira.*
E. W. Dallas, Esq.
Rev. James Grant, D.C.L., D.D., *one of the Ministers of Edinburgh.*
1852 Eyre B. Powell, Esq., *Madras.*
Thomas Miller, A.M., LL.D., *Rector, Perth Academy.*
Allan Dalzell, M.D.
James Cunningham, Esq., W.S.
Alexander James Russell, Esq., C.S.
Andrew Fleming, M.D., *Bengal.*
1853 James Watson, M.D., *Bath.*
Lieutenant-Colonel Robert Maclagan, *Bengal Engineers.*
Rev. Dr Robert Lee, *Professor of Biblical Criticism and Biblical Antiquities.*
Rev. John Cumming, D.D., *London.*
Hugh Scott, Esq., *of Gala.*
Græme Reid Mercer, Esq.
1854 Dr John Addington Symonds, *Clifton, Bristol.*
Dr William Bird Herapath, *Bristol.*
Robert Harkness, Esq., *Professor of Mineralogy and Geology, Queen's College, Cork.*
Sir James Coxe, M.D., F.R.C.P.E.
Ernest Bonar, Esq.
1855 Stevenson Macadam, Ph.D.
Robert Etheridge, Esq., *Clifton, Bristol.*
Right Honourable John Inglis, *Lord Justice-General.*
Wyville T. C. Thomson, LL.D., *Professor of Geology, Belfast.*
Dr Wright, *Cheltenham.*
James Hay, Esq.
R. M. Smith, Esq.
1856 David Bryce, Esq.
William Mitchell Ellis, Esq.
George J. Allman, M.D., F.R.S., *Professor of Natural History.*
Honourable Lord Neaves.
Dr Frederick Penny.
Thomas Laycock, M.D., *Professor of the Practice of Medicine.*
Thomas Cleghorn, Esq.
James Clerk Maxwell, Esq., F.R.S., *late Professor of Natural Philosophy, King's College, London.*
1587 John Ivor Murray, M.D., F.R.C.S.E.
John Blackwood, Esq.
W. M. Buchanan, M.D.
Thomas Login, Esq., C.E., *Pegu.*
Edmund C. Batten, M.A., *Lincoln's Inn, London.*
1858 Thomas Williamson, M.D., F.R.C.S.E., *Leith.*

Date of Election.

1858 Robert B. Malcolm, M.D., F.R.C.P.E.
Frederick Field, Esq., *Chili.*
James Leslie, Esq., C.E.
Cosmo Innes, Esq., *Professor of History.*
Rev. Alexander C. Fraser, *Professor of Logic.*
Rev. William Stevenson, D.D., *Professor of Ecclesiastical History.*
1859 William F. Skene, LL.D.
G. W. Hay, Esq.
Robert Russell, Esq.
Joseph Fayrer, M.D., F.R.C.S.E., *Professor of Surgery, Calcutta.*
George Robertson, Esq., C.E.
Lyon Playfair, C.B., F.R.S., *Professor of Chemistry.*
John Brown, M.D., F.R.C.P.E.
Rev. John Duns, D.D.
Lieut. John Hills, *Bombay Engineers.*
Major James George Forlong.
1860 William Robertson, M.D., F.R.C.P.E.
Frederick Guthrie, M.D., *Professor of Chemistry, Mauritius.*
Patrick C. MacDougall, Esq., *Professor of Moral Philosophy.*
George A. Jamieson, Esq.
Rev. Leonard Shafto Orde.
Patrick Dudgeon, Esq., *of Cargen.*
William Chambers, Esq., *of Glenormiston.*
1861 W. A. F. Browne, Esq., F.R.C.S.E, *one of H. M. Commissioners in Lunacy for Scotland.*
Rev. Thomas Brown.
James M'Bain, M.D., R.N.
Peter Guthrie Tait, Esq., *Professor of Natural Philosophy.*
John Muir, D.C.L., LL.D.
William Turner, M.B., *Professor of Anatomy.*
William Lauder Lindsay, M.D.
James Lorimer, A.M., *Professor of Public Law.*
Archibald Geikie, Esq., F.R.S.
George Berry, Esq.
James Young, Esq.
Alexander Eugene Mackay, M.D., R.N.
1862 Rev. William G. Blaikie, D.D.
Henry Cheyne, Esq., W.S.
Edmund Ronalds, Ph.D.
Thomas C. Archer, Esq., *Director of Museum of Science and Art.*
James Hector, M.D.
Nicholas Alexander Dalzell, Esq., A.M.
Hon. Lord Barcaple, LL.D.
Rev. Robert Boog Watson, *Madeira.*
1863 Robert Campbell, Esq., *Advocate.*

Date of Election.	
1863	H. F. C. Cleghorn, M.D.
	John Stuart Blackie, Esq., *Professor of Greek.*
	Edward Meldrum, Esq.
	Charles Lawson, Esq.
	Alexander Peddie, M.D., F.R.C.P.E.
	Right Hon. Lord Dunfermline, *Colinton House.*
	William Jameson, Esq., *Surgeon-Major, Saharunpore.*
	William Brand, Esq., W.S.
	Murray Thomson, M.D.
	John Young, M.D., *Professor of Natural History, University of Glasgow.*
	David Page, LL.D.
	J. G. Wilson, M.D., F.R.C.S.E.
	J. Matthews Duncan, M.D., F.R.C.P.E.
	W. Dittmar, Esq.
	Honourable Lord Ormidale.
	Joseph D. Everett, D.C.L.
	Honourable G. Waldegrave Leslie.
	Honourable Charles Baillie, Lord Jerviswoode.
	James Sanderson, Esq., *Surgeon-Major.*
	Charles Cowan, Esq.
	John Alexander Smith, M.D., F.R.C.P.E.
1864	Alex. Crum Brown, M.D., D.Sc.
	Alex. Wood, M.D., F.R.C.P.E.
	Andrew Wood, M.D., F.R.C.S.E.
	Robert William Thomson, Esq., C.E.
	James David Marwick, Esq.
	Rev. Daniel F. Sandford.
	Robert S. Wyld, Esq., W.S.
	Peter M'Lagan, Esq., *of Pumpherston,* M.P.
	William Lindsay, Esq.
	W. Y. Sellar, M.A., *Professor of Humanity.*
	Robert Hutchison, Esq., *Carlowrie Castle.*
	Rev. John Hannah, D.D., *Glenalmond.*
	William Wallace, Ph.D.
	Robert Dyce, M.D., *Professor of Midwifery, Aberdeen.*
	Arthur Abney Walker, Esq.
	John Foulerton, M.D., F.R.C.S.E., *Manila.*
1865	Alfred R. Catton, M.A., Camb.
	Rev. Francis Redford, M.A., *Rector of Silloth.*
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ERRATA.

In Theorem 7, page 41, and in Theorem VII., page 44, *for* $\left\{\begin{array}{l} A_1B,\ B\,C,\ \&c. \\ B_1C,\ C\,A,\ \&c. \\ C_1A,\ A\,B,\ \&c. \end{array}\right\}$ *read* $\left\{\begin{array}{l} A\,B_1,\ B\,C,\ \&c. \\ B\,C_1,\ C\,A,\ \&c. \\ C\,A_1,\ A\,B,\ \&c. \end{array}\right.$

In Theorem LXXII. page 52, the sentence ought to end at the *comma;* the rest is obviously erroneous.