REPORT

OF THE

EIGHTH MEETING

OF THE

BRITISH ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE;

HELD AT NEWCASTLE IN AUGUST 1838.

VOL. VII.

LONDON:

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BRITISH MUSEUM
NATURAL HISTORY

CONTENTS.

REPORTS OF RESEARCHES IN SCIENCE.

OBJECTS AND RULES

OF

THE ASSOCIATION.

OBJECTS.

The Association contemplates no interference with the ground occupied by other Institutions. Its objects are,—To give a stronger impulse and a more systematic direction to scientific inquiry,—to promote the intercourse of those who cultivate Science in different parts of the British Empire, with one another, and with foreign philosophers,—to obtain a more general attention to the objects of Science, and a removal of any disadvantages of a public kind which impede its progress.

RULES.

MEMBERS.

All Persons who have attended the first Meeting shall be entitled to become Members of the Association, upon subscribing an obligation to conform to its Rules.

The Fellows and Members of Chartered Literary and Philosophical Societies publishing Transactions, in the British Empire, shall be entitled, in like manner, to become Members of the Association.

The Officers and Members of the Councils, or Managing Committees, of Philosophical Institutions, shall be entitled, in like manner, to become Members of the Association.

All Members of a Philosophical Institution recommended by its Council or Managing Committee, shall be entitled, in like manner, to become Members of the Association.

Persons not belonging to such Institutions shall be elected by the General Committee or Council, to become Members of the Association, subject to the approval of a General Meeting.

SUBSCRIPTIONS.

The amount of the Annual Subscription shall be One Pound, to be paid in advance upon admission; and the amount of the composition in lieu thereof, Five Pounds.

Subscriptions shall be received by the Treasurer or Secretaries.

If the annual subscription of any Member shall have been in arrear for two years, and shall not be paid on proper notice, he shall cease to be a member.

MEETINGS.

The Association shall meet annually, for one week, or longer. The place of each Meeting shall be appointed by the General Committee at the previous Meeting; and the Arrangements for it shall be entrusted to the Officers of the Association.

GENERAL COMMITTEE.

The General Committee shall sit during the week of the Meeting, or longer, to transact the business of the Association. It shall consist of the following persons:—

1. Presidents and Officers for the present and preceding years, with authors of Reports in the Transactions of the Association.

2. Members who have communicated any Paper to a Philosophical Society, which has been printed in its Transactions, and which relates to such subjects as are taken into consideration at the Sectional Meetings of the Association.

3. Office-bearers for the time being, or Delegates, altogether not exceeding three in number, from any Philosophical Society publishing Transactions.

4. Office-bearers for the time being, or Delegates, not exceeding three, from Philosophical Institutions established in the place of Meeting, or in any place where the Association has formerly met.

5. Foreigners and other individuals whose assistance is desired, and who are specially nominated in writing for the meeting of the year by the President and General Secretaries.

6. The Presidents, Vice-Presidents, and Secretaries of the Sections are *ex officio* members of the General Committee for the time being.

SECTIONAL COMMITTEES.

The General Committee shall appoint, at each Meeting, Committees, consisting severally of the Members most conversant with the several branches of Science, to advise together for the advancement thereof.

The Committees shall report what subjects of investigation they would particularly recommend to be prosecuted during the ensuing year, and brought under consideration at the next Meeting.

The Committees shall recommend Reports on the state and progress of particular Sciences, to be drawn up from time to time by competent persons, for the information of the Annual Meetings.

COMMITTEE OF RECOMMENDATIONS.

The General Committee shall appoint at each Meeting a Committee, which shall receive and consider the Recommendations of the Sectional Committees, and report to the General Committee the measures which they would advise to be adopted for the advancement of Science.

LOCAL COMMITTEES.

Local Committees shall be formed by the Officers of the Association to assist in making arrangements for the Meetings.

Committees shall have the power of adding to their numbers those Members of the Association whose assistance they may desire.

OFFICERS.

A President, two or more Vice-Presidents, one or more Secretaries, and a Treasurer, shall be annually appointed by the General Committee.

COUNCIL.

In the intervals of the Meetings, the affairs of the Association shall be managed by a Council appointed by the General Committee. The Council may also assemble for the despatch of business during the week of the Meeting.

PAPERS AND COMMUNICATIONS.

The Author of any paper or communication shall be at liberty to reserve his right of property therein.

ACCOUNTS.

The Accounts of the Association shall be audited annually, by Auditors appointed by the Meeting.

OFFICERS AND COUNCIL, 1838-9.

Trustees (permanent.)—R. I. Murchison, Esq. John Taylor, Esq.

President.—His Grace the Duke of Northumberland.

Vice-Presidents.—The Bishop of Durham, F.R.S., F.A.S. Rev. W. Vernon Harcourt. Prideaux John Selby, Esq., F.R.S.E.

President elect.—The Rev. William Vernon Harcourt, F.R.S.

Vice-Presidents elect.—Marquis Northampton, Pres. Royal Soc. Earl of Dartmouth. Rev. T. Robinson, D.D. John Corrie, Esq., F.R.S.

General Secretaries.—R. I. Murchison, Esq., F.R.S. Rev. G. Peacock, D.D., F.R.S.

Assistant General Secretary.—Professor Phillips, *York.*

Secretaries for Birmingham.—George Barker, Esq. Joseph Hodgson, Esq. Follett Osler, Esq. Peyton Blakiston, M.D.

General Treasurer.—John Taylor, Esq., 2, Duke Street, Adelphi.

Treasurers to the Birmingham Meeting.—J. L. Moilliet, Esq. James Russell, Esq.

Council.—Dr. Arnott. F. Baily, Esq. Rev. Dr. Buckland. R. Brown, Esq. The Earl of Burlington. Professor Clark. Dr. Daubeny. G. B. Greenough, Esq. Professor Graham. J. E. Gray, Esq. Robert Hutton, Esq. Rev. L. Jenyns. Sir Charles Lemon, Bart. Charles Lyell, Esq. J. W. Lubbock, Esq. Dr. Lardner. Professor Owen. Sir J. Rennie. Major Sabine. Colonel Sykes. Rev. Professor Whewell. Professor Wheatstone. Captain Washington.

Secretary to the Council.—James Yates, Esq., 49, Upper Bedford Place, London.

Local Treasurers.—Dr. Daubeny, Oxford. Professor Henslow, Cambridge. Dr. Orpen, Dublin. Charles Forbes, Esq., Edinburgh. William Gray, jun., Esq., York. William Sanders, Esq., Bristol. Samuel Turner, Esq., Liverpool. Rev. John James Tayler, Manchester. James Russell, Esq., Birmingham. William Hutton, Esq., Newcastle-on-Tyne. Henry Woolcombe, Esq., Plymouth.

I. Table showing the Places and Times of Meeting of the British Association, with Presidents, Vice-Presidents, and Local Secretaries, from its Commencement.

Presidents.	Vice-Presidents.	Local Secretaries.
The EARL FITZWILLIAM, D.C.L., F.R.S., F.G.S., &c. YORK, September 27, 1831.	Rev. W. Vernon Harcourt, M.A., F.R.S., F.G.S.	William Gray, jun., F.G.S. Professor Phillips, F.R.S., F.G.S.
The REV. W. BUCKLAND, D.D., F.R.S., F.G.S., &c. OXFORD, June 19, 1832.	Sir David Brewster, F.R.SS.L. & E., &c. ... Rev. W. Whewell, F.R.S., Pres. Geol. Soc. ...	Professor Daubeny, M.D., F.R.S., &c. Rev. Professor Powell, M.A, F.R.S., &c.
The REV. ADAM SEDGWICK, M.A., V.P.R.S., V.P.G.S. CAMBRIDGE, June 25, 1833.	G. B. Airy, F.R.S., Astronomer Royal, &c.... John Dalton, D.C.L., F.R.S......................	Rev. Professor Henslow, M.A., F.L.S., F.G.S. Rev. Wm. Whewell, F.R.S.
Sir T. MACDOUGAL BRISBANE, K.C.B., D.C.L., F.R.SS.L. & E. EDINBURGH, September 8, 1834.	Sir David Brewster, F.R.S., &c. .... Rev. T. R. Robinson, D.D.	Professor Forbes, F.R.SS.L. & E., &c. Sir John Robison, Sec. R.S.E.
The REV. PROVOST LLOYD, LL.D. DUBLIN, August 10, 1835.	Viscount Oxmantown, F.R.S., F.R.A.S. ...... Rev. W. Whewell, F.R.S., &c.	Sir W. R. Hamilton, Astron. Royal of Ireland, &c. Rev. Professor Lloyd, F.R.S.
The MARQUIS OF LANSDOWNE, D.C.L., F.R.S., &c. BRISTOL, August 22, 1836.	The Marquis of Northampton, F.R.S.......... Rev. W. D. Conybeare, F.R.S., F.G.S.......... J. C. Prichard, M.D., F.R.S.	Professor Daubeny, M.D., F.R.S., &c. V. F. Hovenden.
The EARL OF BURLINGTON, F.R.S., F.G.S., Chan. Univ. Lon. LIVERPOOL, September 11, 1837.	The Bishop of Norwich, P.L.S., F.G.S. ...... John Dalton, D.C.L., F.R.S...................... Sir Philip Grey Egerton, Bart., F.R.S., F.G.S. Rev. W. Whewell	Professor Traill, M.D. Wm. Wallace Currie. Joseph N. Walker, Pres. Royal Institution, Liverpool.
The DUKE OF NORTHUMBERLAND, F.R.S., F.G.S., &c. NEWCASTLE-ON-TYNE, August 20, 1838.	The Bishop of Durham, F.R.S., F.S.A. ...... The Rev. W. Vernon Harcourt, F.R.S., &c.... Prideaux John Selby, Esq., F.R.S.E.	John Adamson, F.L.S., &c. Wm. Hutton, F.G.S. Professor Johnstone, M.A., F.R.S.
The REV. W. VERNON HARCOURT, M.A. BIRMINGHAM, August 26, 1839.	The Marquis of Northampton .... The Earl of Dartmouth .... The Rev. T. R. Robinson, D.D. .... John Corrie, Esq., F.R.S.........................	George Barker, Esq., F.R.S. Peyton Blakiston, M.D. Joseph Hodgson, Esq., F.R.S. Follett Osler, Esq.

II. Table showing the Members of Council of the British Association from its Commencement, in addition to Presidents, Vice-Presidents, and Local Secretaries.

General Secretaries.	Rev. Wm. Vernon Harcourt, F.R.S., &c. 1832—1836. Francis Baily, V.P. and Treas. R.S. ......1835. R. I. Murchison, F.R.S., F.G.S. ....1836—1838. Rev. G. Peacock, F.R.S., F.G.S., &c. ...1837, 1838.
General Treasurer.	John Taylor, F.R.S., Treas. G.S., &c. ...1832—1838.
Trustees (permanent).	Charles Babbage, F.R.SS.L. & E., &c. (Resigned.) R. I. Murchison, F.R.S., &c. John Taylor, F.R.S., &c.
Assistant General Secretary.	Professor Phillips, F.R.S., &c. ....1832—1838.

Members of Council.

G. B. Airy, F.R.S., Astronomer Royal ......1834, 1835.
Neill Arnott, M.D. ....1838.
Francis Baily, V.P. and Treas. R.S. ....1837, 1838.
George Bentham, F.L.S. ....1834, 1835.
Robert Brown, D.C.L., F.R.S. ....1832, 1834, 1835, 1838.
Sir David Brewster, F.R.S., &c. ....1832.
M. I. Brunel, F.R.S., &c. ....1832.
Rev. Professor Buckland, D.D., F.R.S., &c. 1833, 1835, 1838.
The Earl of Burlington ....1838.
Rev. T. Chalmers, D.D., Prof. of Divinity, Edinburgh ....1833.
Professor Clark, Cambridge ....1838.
Professor Christie, F.R.S., &c. ....1833—1837.
William Clift, F.R.S., F.G.S. ....1832—1835.
John Corrie, F.R.S., &c. ....1832.
Professor Daniell, F.R.S... ....1836.
Dr. Daubeny ....1838.
J. E. Drinkwater ....1834, 1835.
The Earl Fitzwilliam, D.C.L., F.R.S., &c....1833.
Professor Forbes, F.R.SS.L. & E., &c.........1832.
Davies Gilbert, D.C.L., V.P.R.S., &c. ......1832.
Professor Graham, M.D., F.R.S.E. ....1837.
Professor Thomas Graham, F.R.S. ....1838.
John Edward Gray, F.R.S., F.L.S., &c. ......1837, 1838.
Professor Green, F.R.S., F.G.S. ....1832.
G. B. Greenough, F.R.S., F.G.S. ....1832—1838.
Henry Hallam, F.R.S., F.S.A., &c. ....1836.
Sir William R. Hamilton, Astron. Royal of Ireland ....1832, 1833, 1836.
Rev. Prof. Henslow, M.A., F.L.S., F.G.S....1837.
Sir John F. W. Herschel, F.R.SS. L. & E., F.R.A.S., F.G.S., &c. ....1832.
Thomas Hodgkin, M.D. ....1833—1837.
Prof. Sir W. J. Hooker, LL.D., F.R.S., &c. 1832.
Rev. F. W. Hope, M.A., F.L.S. ....1837.
Robert Hutton, M.P., F.G.S., &c. ....1836, 1838.
Professor R. Jameson, F.R.SS. L. & E. ......1833.
Rev. Leonard Jenyns ....1838.

Sir C. Lemon, Bart., M.P. ....1838.
Rev. Dr. Lardner ....1838.
Professor Lindley, F.R.S., F.L.S., &c. ....1833, 1836.
Rev. Provost Lloyd, D.D. ....1832, 1833.
J. W. Lubbock, F.R.S., F.L.S., &c., Vice-Chancellor of the University of London 1833—1836, 1838.
Rev. Thomas Luby..................................1832.
Charles Lyell, jun., Esq. ....1838.
William Sharp MacLeay, F.L.S. ....1837.
Patrick Neill, LL.D., F.R.S.E....................1833.
Richard Owen, F.R.S., F.L.S. ....1836, 1838.
Rev. George Peacock, M.A., F.R.S., &c.......1832, 1834, 1835.
Rev. Professor Powell, M.A., F.R.S., &c.......1836, 1837.
J. C. Prichard, M.D., F.R.S., &c................1832.
George Rennie, F.R.S. ....1833—1835.
Sir John Rennie...................................1838.
Rev. Professor Ritchie, F.R.S. ....1833.
Sir John Robison, Sec. R.S.E. ....1832, 1836.
P. M. Roget, M.D., Sec. R.S., F.G.S., &c. ...1834—1837.
Major Sabine ....1838.
Rev. William Scoresby, B.D., F.R.SS. L. & E. 1832.
Lieut.-Col. W. H. Sykes, F.R.S., F.L.S., &c....1837, 1838.
Rev. J. J. Tayler, B.A., Manchester............1832.
Professor Traill, M.D. ....1832, 1833.
N. A. Vigors, M.P., D.C.L., F.S.A., F.L.S....1832, 1836.
Captain Washington, R.N.........................1838.
Professor Wheatstone.............................1838.
Rev. W. Whewell ....1838.
William Yarrell, F.L.S. ....1833—1836.

Secretaries to the Council.
Edward Turner, M.D., F.R.SS. L. & E...1832—1836.
James Yates, F.R.S., F.L.S., F.G.S.......1832—1838.

OFFICERS OF SECTIONAL COMMITTEES AT THE NEWCASTLE MEETING.

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE.

President.—Sir J. F. W. Herschel, Bart., F.R.S.
Vice-Presidents.—Francis Baily, Esq., F.R.S. Sir D. Brewster, Knt. Sir W. R. Hamilton, Knt. Rev. D. Robinson.
Secretaries.—Rev. Professor Chevalier. Major Sabine, F.R.S. Professor Stevelly.

SECTION B.—CHEMISTRY AND MINERALOGY.

President.—Rev. W. Whewell, F.R.S., P.G.S.
Vice-Presidents.—Dr. T. Thomson. Dr. Daubeny. Professor Graham.
Secretaries. — Professor Miller. H. L. Pattinson, Esq. Thomas Richardson, Esq.

SECTION C.—GEOLOGY AND GEOGRAPHY*.

President for Geology.—C. Lyell, Esq., F.R.S., V.P.G.S., &c.
President for Geography.—Lord Prudhoe.
Vice-Presidents.—W. Buckland, D.D., F.R.S., V.P.G.S. John Buddle, Esq., F.G.S.
Secretaries for Geology.—W. C. Trevelyan, Esq., F.R.S.E., F.G.S. Captain Portlock, R.E., F.R.S., F.G.S.
Secretaries for Geography.—Captain Washington, R.N.

SECTION D.—ZOOLOGY AND BOTANY.

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Vice-Presidents.—R. K. Greville, LL.D. Rev. L. Jenyns, F.L.S. Rev. F. W. Hope, F.R.S.
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SECTION E.—MEDICAL SCIENCE.

President.—T. E. Headlam, M.D.
Vice-Presidents.—Professor William Clark, M.D., F.G.S. John Yelloly, M.D., F.R.S. John Fife, Esq.
Secretaries.—T. M. Greenhow, Esq. J. R. W. Vose, M.D.

* By a resolution of the General Committee at the Newcastle Meeting, the title of this Section will in future be Geology and Physical Geography.

SECTION F.—STATISTICS.

President.—Col. Sykes, F.R.S., V.P. Statistical Society of London.

Vice-Presidents.—Sir Charles Lemon, Bart., M.P., F.R.S. G. R. Porter, Esq. Charles W. Bigge, Esq.

Secretaries.—James Heywood, Esq. W. R. Wood, Esq. Wm. Cargill, Esq.

SECTION G.—MECHANICAL SCIENCE.

President.—Charles Babbage, Esq., F.R.S., &c. &c.

Vice-Presidents.—Bryan Donkin, Esq., V.P. Inst. C.E., &c. Sir John Robison, Sec. R.S.E. G. Stephenson, Esq. Professor Willis.

Secretaries.—R. Hawthorn, Esq. T. Webster, Esq., Sec. Inst. C.E. C. Vignolles, Esq.

CORRESPONDING MEMBERS.

Professor Agassiz, Neufchatel. M. Arago, Secretary of the Institute, Paris. A. Bache, Principal of Girard College, Philadelphia. Professor Berzelius, Stockholm. Professor De la Rive, Geneva. Professor Dumas, Paris. Professor Ehrenberg, Berlin. Baron Alexander von Humboldt, Berlin. Professor Liebig, Giessen. Professor Œrsted, Copenhagen. Jean Plana, Astronomer Royal, Turin. M. Quetelet, Brussels. Professor Schumacher, Altona.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

TREASURER'S ACCOUNT from 31st July 1837 to 31st July 1838.

RECEIPTS.

1838.	£	s.	d.	£	s.	d.
Balance in hand from last year's Account				248	0	6
Compositions from 318 Members	1538	0	0			
Subscriptions 1837, from 1350 Members	1350	1	0			
Ditto 1838, from 29 do.	29	0	0			
Arrears 1836, from 24 do.	24	0	0			
				2941	1	0
Dividend on £4500 in 3 per cent. consols, 6 months to January 1838	67	10	0			
Dividend on £5500 in 3 per cent. consols, 6 months to July	82	10	0			
				150	0	0
Received on account of Sale of Reports, viz. 1st vol., 2nd edit.	47	3	0			
2nd vol.	40	8	0			
3rd vol.	66	16	0			
4th vol.	85	11	0			
5th vol.	181	9	0			
Lithographs	1	1	9			
				422	8	9
				£3761	10	3

PAYMENTS.

	£	s.	d.	£	s.	d.
Expenses of Meeting at Liverpool				279	6	0
Disbursements by Local Treasurers				151	18	10
Purchase of £1000 in 3 per cent. consols				910	0	0
Salaries to Assistant Secretary and Accountant, 18 months to Midsummer last				375	0	0
Grants to Committees for Scientific purposes, viz. for						
Discussion of Tides (1836)	29	0	0			
Do. do. at Bristol	50	0	0			
Level of Land and Sea (1836)	228	7	1			
Do. (Instruments) (1837)	39	1	6			
Meteorology and Subterranean Temp. (1836)	8	6	0			
Do. do. (1837)	23	3	5			
Experiments on the Heart (1836)	5	3	0			
Education (1836)	50	0	0			
Growth of Plants under Glass (1836)	25	0	0			
Do. do. (1837)	50	0	0			
Experiments on Mud in Rivers	3	6	6			
Fossil Ichthyology	100	0	0			
Experiments on Strength of Cast Iron	60	0	0			
Preservation of Animal and Veget. Substances	19	1	10			
Railway Constants	41	12	10			
Duty of Engines in Steam Vessels	100	0	0			
Meteorological Observations, Plymouth	50	0	0			
Construction of Anemometer at do.	40	0	0			
Repairs of Anemometer at do.	10	0	0			
				932	2	2
Paid Richard Taylor for Printing Reports, 5th vol.				418	13	1
Sundry Expenses on Publishing Reports				10	16	7
Sundry Printing, Advertising, &c.				13	13	0
Balance in the hands of the Bankers	543	17	11			
Ditto Treasurer	84	6	4			
Ditto Local Treasurers	41	16	4			
				670	0	7
				£3761	10	3

The following Reports on the Progress and Desiderata of different branches of Science have been drawn up at the request of the Association, and printed in its Transactions.

VOL. I.

On the progress of Astronomy during the present century, by G. B. Airy, M.A., Astronomer Royal.

On the state of our knowledge respecting Tides, by J. W. Lubbock, M.A., Vice-President of the Royal Society.

On the recent progress and present state of Meteorology, by James D. Forbes, F.R.S., Professor of Natural Philosophy, Edinburgh.

On the present state of our knowledge of the Science of Radiant Heat, by the Rev. Baden Powell, M.A., F.R.S., Savilian Professor of Geometry, Oxford.

On Thermo-electricity, by the Rev. James Cumming, M.A., F.R.S., Professor of Chemistry, Cambridge.

On the recent progress of Optics, by Sir David Brewster, K.C.G., LL.D., F.R.S., &c.

On the recent progress and present state of Mineralogy, by the Rev. William Whewell, M.A., F.R.S.

On the progress, actual state, and ulterior prospects of Geology, by the Rev. William Conybeare, M.A., F.R.S., V.P.G.S., &c.

On the recent progress and present state of Chemical Science, by J. F. W. Johnston, A.M., Professor of Chemistry, Durham.

On the application of Philological and Physical researches to the History of the Human Species, by J. C. Prichard, M.D., F.R.S., &c.

VOL. II.

On the advances which have recently been made in certain branches of Analysis, by the Rev. G. Peacock, M.A., F.R.S., &c.

On the present state of the Analytical Theory of Hydrostatics and Hydrodynamics, by the Rev. John Challis, M.A., F.R.S., &c.

On the state of our knowledge of Hydraulics, considered as a branch of Engineering, by George Rennie, F.R.S., &c. (Parts I. and II.)

On the state of our knowledge respecting the Magnetism of the Earth, by S. H. Christie, M.A., F.R.S., Professor of Mathematics, Woolwich.

On the state of our knowledge of the Strength of Materials, by Peter Barlow, F.R.S.

On the state of our knowledge respecting Mineral Veins, by John Taylor, F.R.S., Treasurer G.S., &c.

On the state of the Physiology of the Nervous System, by William Charles Henry, M.D.

On the recent progress of Physiological Botany, by John Lindley, F.R.S., Professor of Botany in the University of London.

Vol. III.

On the Geology of North America, by H. D. Rogers, F.G.S.

On the philosophy of Contagion, by W. Henry, M.D., F.R.S.

On the state of Physiological Knowledge, by the Rev. Wm. Clark, M.D., F.G.S., Professor of Anatomy, Cambridge.

On the state and progress of Zoology, by the Rev. Leonard Jenyns, M.A., F.L.S., &c.

On the theories of Capillary Attraction, and of the Propagation of Sound as affected by the Development of Heat, by the Rev. John Challis, M.A., F.R.S., &c.

On the state of the science of Physical Optics, by the Rev. H. Lloyd, M.A., Professor of Natural Philosophy, Dublin.

Vol. IV.

On the state of our knowledge respecting the application of Mathematical and Dynamical principles to Magnetism, Electricity, Heat, &c., by the Rev. Wm. Whewell, M.A., F.R.S.

On Hansteen's researches in Magnetism, by Captain Sabine, F.R.S.

On the state of Mathematical and Physical Science in Belgium, by M. Quetelet, Director of the Observatory, Brussels.

Vol. V.

On the present state of our knowledge with respect to Mineral and Thermal Waters, by Charles Daubeny, M.D., F.R.S., M.R.I.A., &c., Professor of Chemistry and of Botany, Oxford.

On North American Zoology, by John Richardson, M.D., F.R.S., &c.

Supplementary report on the Mathematical Theory of Fluids, by the Rev. J. Challis, Plumian Professor of Astronomy in the University of Cambridge.

Vol. VI.

On the variations of the Magnetic Intensity observed at different points of the Earth's Surface, by Major Edward Sabine, R.A., F.R.S.

On the various modes of Printing for the use of the Blind, by the Rev. William Taylor, F.R.S.

On the present state of our knowledge in regard to Dimorphous Bodies, by Professor Johnston, F.R.S.

On the Statistics of the Four Collectorates of Dukhun, under the British Government, by Col. Sykes, F.R.S.

The following Reports of Researches undertaken at the request of the Association have been published, viz.

Vol. IV.

On the comparative measurement of the Aberdeen Standard Scale, by Francis Baily, Treasurer R.S., &c.

On Impact upon Beams, by Eaton Hodgkinson.

Observations on the Direction and Intensity of the Terrestrial Magnetic Force in Ireland, by the Rev. H. Lloyd, Capt. Sabine, and Capt. J. C. Ross.

On the Phænomena usually referred to the Radiation of Heat, by H. Hudson, M.D.

Experiments on Rain at different elevations, by Wm. Gray, jun., and Professor Phillips.

Hourly observations of the Thermometer at Plymouth, by W. S. Harris.

On the Infra-orbital Cavities in Deers and Antelopes, by A. Jacob, M.D.

On the Effects of Acrid Poisons, by T. Hodgkin, M.D.

On the Motions and Sounds of the Heart, by the Dublin Sub-Committee.

On the Registration of Deaths, by the Edinburgh Sub-Committee.

Vol. V.

Observations on the Direction and Intensity of the Terrestrial Magnetic Force in Scotland, by Major Edward Sabine, R.A., F.R.S., &c.

Comparative view of the more remarkable Plants which characterize the Neighbourhood of Dublin, the Neighbourhood of Edinburgh, and the South-west of Scotland, &c.; drawn up for the British Association, by J. T. Mackay, M.R.I.A., A.L.S., &c., assisted by Robert Graham, Esq., M.D., Professor of Botany in the University of Edinburgh.

Report of the London Sub-Committee of the Medical Section of the British Association on the Motions and Sounds of the Heart.

Second Report of the Dublin Sub-Committee on the Motions and Sounds of the Heart. (See Vol. iv. p. 243.)

Report of the Dublin Committee on the Pathology of the Brain and Nervous System.

Account of the recent Discussions of Observations of the Tides which have been obtained by means of the grant of money which was placed at the disposal of the Author for that purpose at the last Meeting of the Association, by J. W. Lubbock, Esq.

Observations for determining the Refractive Indices for the Standard Rays of the Solar Spectrum in various media, by the Rev. Baden Powell, M.A., F.R.S., Savilian Professor of Geometry in the University of Oxford.

Provisional Report on the Communication between the Arteries and Absorbents on the part of the London Committee, by Dr. Hodgkin.

Report of Experiments on Subterranean Temperature, under the direction of a Committee, consisting of Professor Forbes, Mr. W. S. Harris, Professor Powell, Lieut.-Colonel Sykes, and Professor Phillips (Reporter).

Inquiry into the validity of a method recently proposed by George B. Jerrard, Esq., for Transforming and Resolving Equations of Elevated Degrees: undertaken at the request of the Association by Professor Sir W. R. Hamilton.

Vol. VI.

Account of the Discussions of Observations of the Tides which have been obtained by means of the grant of money which was placed at the disposal of the Author for that purpose at the last Meeting of the Association, by J. W. Lubbock, Esq., F.R.S.

On the difference between the Composition of Cast Iron produced by the Cold and the Hot Blast, by Thomas Thomson, M.D., F.R.SS. L. & E., &c., Professor of Chemistry, Glasgow.

On the Determination of the Constant of Nutation by the Greenwich Observations, made as commanded by the British Association, by the Rev. T. R. Robinson, D.D.

On some Experiments on the Electricity of Metallic Veins, and the Temperature of Mines, by Robert Were Fox.

Provisional Report of the Committee of the Medical Section of the British Association, appointed to investigate the Composition of Secretions, and the Organs producing them.

Report from the Committee for inquiring into the Analysis of the Glands, &c. of the Human Body, by G. O. Rees, M.D., F.G.S.

Second Report of the London Sub-Committee of the British Association Medical Section, on the Motions and Sounds of the Heart.

Report from the Committee for making experiments on the Growth of Plants under Glass, and without any free communication with the outward air, on the plan of Mr. N. I. Ward, of London.

Report of the Committee on Waves, appointed by the British

Association at Bristol in 1836, and consisting of Sir John Robison, K.H., Secretary of the Royal Society of Edinburgh, and John Scott Russell, Esq., M.A., F.R.S. Edin. (Reporter).

On the relative Strength and other mechanical Properties of Cast Iron obtained by Hot and Cold Blast, by Eaton Hodgkinson.

On the Strength and other Properties of Iron obtained from the Hot and Cold Blast, by W. Fairbairn.

Vol. VII.

Account of a Level Line, measured from the Bristol Channel to the English Channel, during the Year 1837–8, by Mr. Bunt, under the Direction of a Committee of the British Association. Drawn up by the Rev. W. Whewell, F.R.S., one of the Committee.

A Memoir on the Magnetic Isoclinal and Isodynamic Lines in the British Islands, from Observations by Professors Humphrey Lloyd and John Phillips, Robert Were Fox, Esq., Captain James Clark Ross, R.N., and Major Edward Sabine, R.A., by Major Edward Sabine, R.A., F.R.S.

First Report on the Determination of the Mean Numerical Values of Railway Constants, by Dionysius Lardner, LL.D., F.R.S., &c.

First Report upon Experiments, instituted at the request of the British Association, upon the Action of Sea and River Water, whether clear or foul, and at various temperatures, upon Cast and Wrought Iron, by Robert Mallet, M.R.I.A., Ass. Ins. C.E.

Notice of Experiments in progress, at the desire of the British Association, on the Action of a Heat of 212° Fahr., when long continued, on Inorganic and Organic Substances, by Robert Mallet, M.R.I.A.

Experiments on the ultimate Transverse Strength of Cast Iron made at Arigna Works, Co. Leitrim, Ireland, at Messrs. Bramah and Robinson's, 29th May, 1837.

Provisional Reports and Notices of Progress in Special Researches entrusted to Committees and Individuals.

The following Reports and Continuations of Reports have been undertaken to be drawn up at the request of the Association.

On the Connexion of Electricity and Magnetism, by S. H. Christie, Sec. R.S.

On the state of knowledge of the Phænomena of Sound, by Rev. Robert Willis, M.A., F.R.S., &c.

On the state of our knowledge respecting the relative level of Land and Sea, and the waste and extension of the land on the east coast of England, by R. Stevenson, Engineer to the Northern Lighthouses, Edinburgh.

On circumstances in Vegetation influencing the Medicinal Virtues of Plants, by R. Christison, M.D.

On Salts, by Professor Graham, F.R.S.

On the Differential and Integral Calculus, by Rev. Professor Peacock, M.A., F.R.S., &c.

On the Geology of North America, by H. D. Rogers, F.G.S., Professor of Geology, Philadelphia.

On the Mineral riches of Great Britain, by John Taylor, F.R.S., F.G.S.

On Vision, by Professor C. Wheatstone, F.R.S.

On the application of a General Principle in Dynamics to the Theory of the Moon, by Professor Sir W. Hamilton.

On Isomeric Bodies, by Professor Liebig.

On Organic Chemistry, by Professor Liebig.

On Inorganic Chemistry, by Professor Johnston, F.R.S.

On Fossil Reptiles, by Professor Owen, F.R.S.

On the Salmonidæ of Scotland, by Sir J. W. Jardine.

On the Caprimulgidæ, by N. Gould, F.L.S.

On the state of Meteorology in the United States of North America, by A. Bache.

On the state of Chemistry as bearing on Geology, by Professor Johnston.

On Molluscous Animals and their Shells, by J. E. Gray, F.R.S.

On Ornithology, by P. J. Selby, F.R.S.E.

On the Specific Gravity of Steam, by a Committee, of which Mr. B. Donkin is Secretary.

On the Geographical Distribution of Pulmoniferous Mollusca, by E. Forbes, F.L.S.

Reports requested, Researches recommended, and Desiderata noticed by the Committees of Science at the Newcastle Meeting.

REPORTS ON THE STATE OF SCIENCE.

Prof. Bache, of Philadelphia, was requested to furnish a Report on the state of Meteorology in the United States, for the next meeting of the Association.

Prof. Johnston was requested to prepare a Report on the present state of Chemistry as bearing upon Geology.

Mr. J. E. Gray, F.R.S., was requested to prepare a Report on the present state of our knowledge of Molluscous Animals and their Shells.

Mr. Selby was requested to draw up a Report on the present state of knowledge of Ornithology, for an early meeting.

Mr. Bryan Donkin (Secretary), Dr. Ure, Dr. Faraday, and Mr. Cooper were requested to Report as to the state of our knowledge on the Specific Gravity of Steam generated at different Temperatures ; Mr. Donkin to act as Secretary.

Mr. E. Forbes was requested to Report on the present state of the knowledge of the Geographical Distribution of Pulmoniferous Mollusca in Britain, and the circumstances which influence this distribution.

The Council were requested to apply for a Report on the present state and recent discoveries in Geology.

Specific Researches in Science involving applications to Government or public bodies.

MAGNETICAL OBSERVATIONS.

Resolved,—1. That the British Association views with high interest the system of Simultaneous Magnetic Observations which have been for some time carrying on in Germany and in various parts of Europe, and the important results towards which they have already led; and that they consider it highly desirable that similar series of observations, to be regularly continued in correspondence with and in extension of these, should be instituted in various parts of the British dominions.

2. That this Association considers the following localities as particularly important:

Canada,	Van Diemen's Land,
Ceylon,	Mauritius, or the
St. Helena,	Cape of Good Hope;

and that they are willing to supply Instruments for the purpose of observation.

3. That in these series of observations, the three elements of horizontal direction, dip, and intensity, or their theoretical equivalents, be insisted on, as also their hourly changes, and on appointed days their momentary fluctuations.

4. That this Association views it as highly important that the

deficiency yet existing in our knowledge of Terrestrial Magnetism in the Southern Hemisphere should be supplied by observations of the magnetic direction and intensity, especially in the higher latitudes, between the meridians of New Holland and Cape Horn; and they desire strongly to recommend to Her Majesty's Government the appointment of a naval expedition directed expressly to that object.

5. That in the event of such expedition being undertaken, it would be desirable that the officer charged with its conduct should prosecute both branches of observations alluded to in Resolution 3, so far as circumstances will permit.

6. That it would be most desirable that the observations so performed, both in the fixed stations and in the course of the expedition, should be communicated to Prof. Lloyd.

7. That Sir John Herschel, Mr. Whewell, Mr. Peacock, and Prof. Lloyd be appointed a Committee to represent to Government these recommendations.

8. That the same gentlemen be empowered to act as a Committee, with power to add to their number, for the purpose of drawing up plans of Scientific cooperation, &c. &c., relating to the subject, and reporting to the Association.

9. That the sum of 400*l.* be placed at the disposal of the above-named Committee, for the purposes above mentioned*.

ASTRONOMY.

Sir J. Herschel and Mr. Baily were requested to make application to Government for increase in the instrumental power of the Royal Observatory at the Cape of Good Hope, and the addition of at least one assistant to that establishment.

SCIENTIFIC RESEARCHES IN INDIA.

Resolved,—1. That the British Association regard the measurement of an arc of longitude in India comparable in extent to the meridional arc already measured in that country, as a most important contribution to other facts illustrative of the earth's true figure, and, by a necessary consequence, to the progress of astronomy.

2. That the verification and comparison of the standards of the Indian and English surveys, as compared with the proposed Parliamentary standard, is indispensable to the correct knowledge of the meridional and parallel arcs.

* The application to Government on this subject has been successful, the command of an expedition to the Antarctic regions being entrusted to Capt. J. C. Ross.

3. That pendulum observations at the principal elevations, or contiguous plains, and on the sea-coast, if possible, on the same parallels of latitude, will afford results of great value to physical science.

4. That observations for the determination of the Laws of Refraction in the elevated regions of the Himalayas, and at the Observatories of Madras and Bombay, will be a most important service to science.

5. That it is highly desirable also that magnetical observations should be made in India similar to those which are carrying on in other parts of the world, and which are justly regarded with so much interest.

6. That a topographical map of India, upon a large scale, accompanied by statistical and geological information, would be highly desirable *.

ORDNANCE SURVEY.

Resolved,—That a Committee be appointed to inquire how far, in the future progress of the Ordnance Survey, the several metalliferous and coal-mining districts could be represented on a larger scale. The Committee to consist of Mr. Greenough, Mr. Griffith, Mr. De la Beche, and Major Portlock.

MINING RECORDS.

Resolved,—1. That it is the opinion of this Meeting, that, with a view to prevent the loss of life and of property which must inevitably ensue from the want of accurate mining records, it is a matter of national importance that a depository should be established for preserving such records of subterranean operations in collieries and other mining districts.

2. That a Committee be appointed to draw up a Memorial and to communicate with the Government in the name of the British Association, respecting the most effectual method of carrying the above resolution into effect.

3. That the Committee consist of the following gentlemen, with power to add to their number: The Marquis of Northampton, Sir Charles Lemon, Sir Philip Egerton, John Vivian, Esq., Davies G. Gilbert, Esq., J. S. Enys, Esq., W. L. Dillwyn, the President of the Geological Section of the British Association, the President for the time being of the Geological Society of Lon-

* These Resolutions have been submitted to the consideration of the Directors of the East India Company; and, in particular, the recommendation for magnetical observations has been promptly acceded to.

don, the Professors of Geology at Oxford, Cambridge, London, and Durham, H. T. De la Beche, Esq., John Taylor, Esq., John Buddle, Esq., Thomas Sopwith, Esq.

Specific Researches in Science involving Grants of Money.

The following new Recommendations were adopted by the General Committee*.

That it is desirable that the meteorological observations made at the equinoxes and solstices, agreeably to the recommendations of Sir John Herschel, Bart., should be collected together, as far as is practicable, and reduced to an uniform mode of expression, so that comparisons may be made of the same, with a view of deducing results that may lead to the improvement and elucidation of meteorology.

That Sir John Herschel be requested to superintend the same, and that the sum of 100*l.* be placed at his disposal for that purpose.

That it is desirable that the whole of the stars observed by Lacaille at the Cape of Good Hope, the observations of which are recorded in his *Cœlum Australe Stelliferum*, should be reduced.

That Sir J. Herschel, Mr. Airy, and Mr. Henderson be a Committee for carrying the same into effect.

That the sum of 200*l.* be appropriated to that purpose.

That it is desirable that a Revision of the Nomenclature of the Stars should be made, with a view to ascertain whether or not a more correct distribution of them amongst the present constellations, or such other constellations as it may be considered advisable to adopt, may be formed.

That Sir J. Herschel, Mr. Whewell, and Mr. Baily be a Committee for that purpose, and to report on the same at the next meeting of the Association.

That the sum of 50*l.* be appropriated to defray the expenses that may be incurred in this inquiry.

That 100*l.* be placed at the disposal of Sir D. Brewster and Professor Forbes, for the purpose of procuring Hourly Meteorological Observations, to be made at two parts in Scotland, one at Fort George, on the coast, and the other at some central part, at a great elevation above the sea.

That it appears to the Committee desirable to diffuse in this

* For a general synopsis of money grants sanctioned at the Newcastle Meeting, see p. xxvii.

country the knowledge of the Scientific Memoirs published on the Continent, and that, for this object, 100*l.* be placed at the disposal of a Committee, consisting of Dr. Robinson, Sir John Herschel, Sir D. Brewster, and Professor Wheatstone, with power to add to their number, towards procuring the translation and publication of such memoirs as they may approve.

That Mr. Pattinson and Mr. Richardson be requested to undertake experiments to ascertain whether any perceptible Galvanic influence is exerted by the Stratified Rocks of the neighbourhood of Newcastle, and that 20*l.* be placed at their disposal to meet the expenses of such experiments.

That Dr. Arnott and Dr. Yelloly be a Committee for the purpose of improving Acoustic Instruments (in reference to diseases of the ear), with 25*l.* at their disposal.

That Mr. Cargill, Mr. Wharton, Mr. Buddle, Mr. Forster, Professor Johnston, and Mr. Wilson be a Committee for inquiries into the Statistics of the Collieries of the Tyne and Wear, with 50*l.* at their disposal.

That Sir John Robison (Secretary), and Mr. J. S. Russell, and Mr. James Smith be a Committee for instituting Experiments on the Forms of Vessels, with 200*l.* at their disposal.

Researches not involving Grants of Money or application to Government.

The Meteorological Committee was requested to furnish a System of Meteorological Instructions for the next meeting of the Association.

A Committee was formed, consisting of Mr. Greenough, Mr. De la Beche, Mr. Buddle, and Mr. Griffith, to draw up a proper form and scale of the Sections to be sent to the Geological Society by the engineers and proprietors of railways.

The following gentlemen were appointed a Committee to investigate the Salmonidæ of Scotland, and directed to place themselves in communication with Mr. Shaw, who has offered to submit his experiments on that subject to their inspection: Mr. Selby, Dr. Parnell, Mr. J. S. Menteith, Professor R. Jones, Dr. Neill, Sir W. Jardine, Bart., Secretary.

The following gentlemen were appointed members of a Committee constituted for the purpose of investigating the Insects of the genera *Eriosoma* and *Aphis*, which attack the Pines of this country: Mr. Spence, F.R.S., R. K. Greville, LL.D., Sir W. Jardine, Bart., Mr. Selby, Secretary.

The Committee on Diseases of the Lungs in Animals was reappointed.

The Committee for obtaining a complete account of the Fauna of Ireland was altered so as to consist of Capt. Portlock, Mr. R. Ball, Mr. W. Thompson, Mr. Vigors, Mr. Halliday, and Dr. Coulter, who was requested to act as Secretary.

The following Resolutions relating to the Conduct of the Meetings, &c. were adopted by the General Committee.

That Section C. be styled henceforth the Section of Geology and Physical Geography.

That the several Sections be empowered, at the desire of their respective Committees, to divide themselves into subsections as often as the number and importance of the communications delivered in may render such divisions desirable.

That with a view to facilitate and extend the intercourse of persons engaged in investigating the same departments of science, the rooms in which the Sections are appointed to be held be open in future at 10 o'clock, *vice* 11, so that an hour may be allowed for conversation before the Chair is taken and the reading of papers is commenced.

That Members of the Association, when subscribing their name and address, be invited or enabled to enter also in a separate column the Section to which they wish to attach themselves.

Synopsis of Sums appropriated to Scientific Objects by the General Committee at the Newcastle Meeting.

SECTION A.

*	1. For the Reduction of Meteorological Observations, under the superintendence of Sir J. Herschel	£100	0
*	2. For the Reduction of Lacaille's Stars, under the superintendence of Sir J. Herschel, Mr. Airy, and Mr. Henderson	200	0
*	3. For the Revision of the Nomenclature of the Stars: Sir John Herschel, Mr. Whewell, and Mr. Baily	50	0
	4. For a Level Line from the Bristol to the English Channel, (an additional grant): Mr. Whewell, Col. Colby, Mr. Greenough, and Mr. Griffith	100	0
	5. For Tide Discussions: Mr. Whewell	100	0
	6. For the Reduction of Stars in the *Histoire Céleste*: Mr. Baily, Mr. Airy, and Dr. Robinson	500	0
	7. To extend the Royal Astronomical Society's Catalogue: Mr. Baily, Mr. Airy, and Dr. Robinson	500	0
*	8. For Magnetical Observations, (Instruments, &c.): Sir J. Herschel, Mr. Whewell, Mr. Peacock, and Mr. Lloyd	400	0
	9. To the Committee on Waves: Sir J. Robison and Mr. J. S. Russell	50	0
*	10. For the Translation of Foreign Scientific Memoirs: Dr. Robinson, Sir J. Herschel, Sir D. Brewster, and Prof. Wheatstone	100	0
	11. For Tabulating Meteorological Observations: Mr. Harris and Mr. Osler	15	0
	12. To complete the Repair of an Anemometer at Plymouth: Mr. Osler	8	10
	13. For the Expenses of the Meteorological Observations at Plymouth (additional grant): Mr. W. S. Harris	40	0
*	14. Hourly Meteorological Observations: Sir D. Brewster and Mr. Forbes	100	0
		£2263	10

Section B.

15. For Researches on Atmospheric Air: Mr. W. West	£ 40	0
16. For Experiments on the Action of Sea Water on Cast and Wrought Iron: Mr. Mallet and Prof. Davy	50	0
17. For Experiments on the Action of Water of 212° on Organic Matter: Mr. Mallet	10	0
18. For Chemical Constants: Prof. Johnston . .	30	0
*19. For Galvanic Experiments on Rocks in vicinity of Newcastle: Mr. Pattinson and Mr. Richardson	20	0
	£150	0

Section C.

20. For the Promotion of Fossil Ichthyology: Dr. Buckland, Mr. Murchison, and Prof. Sedgwick	£105	0
21. For Researches on the Mud of Rivers: Mr. Bryce and Mr. De la Beche	20	0
†22. For the Promotion of our Knowledge of British Fossil Reptiles, by a Report on that subject: Mr. Greenough, Mr. Lyell, and Mr. Clift .	200	0
	£325	0

Section D.

23. For Experiments on the Preservation of Animal and Vegetable Substances, Prof. Henslow, Mr. Jenyns, Dr. Clark, and Prof. Cumming . .	£6	0

Section E.

24. For Experiments on the Sounds of the Heart: Dr. Roget, Dr. Williams, and Dr. Dodd, &c. &c.	£50	0
25. For Experiments on the Lungs and Bronchi: Dr. Williams	25	0
*26. For Experiments on Medico-Acoustic Instruments: Dr. Arnott and Dr. Yelloly . . .	25	0
	£100	0

Section F.

27. Inquiries into the State of Education in Schools in England: Col. Sykes, Sir C. Lemon, and Mr. G. R. Porter	£150	0
28. Inquiries as to the State of the Working Population: Sir C. Lemon, Col. Sykes, and Mr. G. R. Porter	100	0
*29. Inquiries into the Statistics of the Collieries of the Tyne and Wear: Mr. Cargill, Mr. Wharton, Mr. Buddle, Mr. Forster, Prof. Johnston, and Mr. Wilson	50	0
	£300	0

Section G.

30. Researches in the duty performed by the Cornish Engines: Mr. John Taylor and Mr. Rennie .	£50	0
31. For Inquiries into the Speed of American Steamers: Mr. W. Fairbairn, Dr. Lardner, Mr. J. S. Russell, and Mr. John Taylor . .	50	0
32. For a Report on the Duty and Engines not in Cornwall: Mr. W. Bryan Donkin, Mr. James Simpson, Mr. G. H. Palmer, and Mr. T. Webster, Sec.	50	0
*33. For Experiments on the Forms of Vessels: Sir J. Robison, Mr. J. S. Russell, and Mr. James Smith	200	0
34. For Experiments on the Hot-blast Iron as compared with Cold-blast Iron: Mr. Hodgkinson, Mr. W. Fairbairn, and Mr. P. Clare, Sec. .	100	0
35. For Railway Constants: Mr. H. Earle, Dr. Lardner, Mr. Locke, Mr. Rennie, and Mr. MacNeil	20	0
36. For Apparatus used in Researches regarding Marine Steam Engines: Mr. J. Scott Russell .	17	0
37. For completing an Instrument for Investigating the Duty of Marine Steam Engines: Dr. Lardner, £50, Mr. Russell, £28, and Mr. W. Fairbairn, £33.	111	0

Section A.	£2263	10	0
— B.	150	0	0
— C.	325	0	0
— D.	6	0	0
— E.	100	0	0
— F.	300	0	0
— G.	598	0	0
	£3742	10	0

The above grants expire at the Meeting in 1839, unless the Recommendations shall have been acted on, or a continuance of the grant applied for by the Sectional Committees, and ordered by the General Committee. Those marked thus *, relate to subjects on which no previous resolution has been adopted: the grounds for such new grants will be found in the previous pages. The mark † is affixed to grants for objects previously recommended by the Association, but without grants of money. The others are renewals or continuations of former grants for objects which have been detailed in previous volumes.

In grants of money to Committees for purposes of Science, the member first named is empowered to draw on the Treasurer for such sums as may from time to time be required. The General Committee does not contemplate, in the grants, the payment of personal expenses to the members.

Arrangement of the General Evening Meetings.

On Monday evening, August 20, the President, His Grace the Duke of Northumberland, having taken the Chair in the Central Exchange, the ADDRESS of the General Secretaries was read by R. I. Murchison, Esq.

On Tuesday evening, in the same room, the attention of the Meeting was called to the collection of Models in the Exhibition Room, and addresses, explanatory of particular INVENTIONS or PROCESSES, were delivered by C. Babbage, Esq., the Rev. Dr. Robinson, and Professor Willis.

On Wednesday evening, the Green Market, fitted up for the purpose, was opened for *Promenade and Conversation*, and Mr. Addams explained and exemplified the process by which the SOLIDIFICATION OF CARBONIC ACID GAS is effected.

On Thursday evening, Abstracts of the Proceedings which had taken place in the Sections were read by the Presidents of the Sections in the Central Exchange.

On Friday evening, the Assembly Rooms, enlarged for the occasion, were opened for *Promenade and Conversation*.

On Saturday evening, the CONCLUDING GENERAL MEETING of the Association took place in the Central Exchange, when an account of the PROCEEDINGS OF THE GENERAL COMMITTEE was read by the Rev. Professor Peacock.

ADDRESS

BY

MR. MURCHISON.

GENTLEMEN,—At the conclusion of the first Septenary which has elapsed since the establishment of the British Association, the Council have deemed it expedient to direct us to prepare a general and comprehensive view of its past progress and future prospects. In virtue, therefore, of the commission thus entrusted to us, we shall endeavour to perform a task which we cannot approach without a feeling of apprehension and anxiety, impressed as we are with the difficulty of duly appreciating the prominent labours of our associates, and of estimating their bearing and probable influence on the advancement of science. The space of time, however, which is allotted to this address, will not allow us to attempt an analysis of all the past proceedings of the Association. We are therefore compelled to confine ourselves to a few allusions to the reports of former meetings, (dwelling more particularly on the last,) and to a statement of the great principles which form the basis of our constitution, and which have directed and regulated its practical operations. And if, in thus stating the aims of the Association, and the principles on which it proceeds, we should be guilty of repeating some things which have been better said before, we trust it will be borne in mind, that from the migratory character of our meetings, and the change which that character implies in the body of members present at each, a probability arises, that those principles may not be sufficiently understood, if they are not from time to time restated and re-explained.

It would be superfluous for us to speak of those objects of the Association which are the most obvious, and which undoubtedly constitute the highest enjoyment these meetings afford us—the union of congenial minds—the mutual communication, without let or hindrance, of the knowledge we have acquired in our respective pursuits—the scintillations of new ideas struck out in private conversation and public discussion. "This feast of reason and this flow of soul," agreeable and instructive as it is, requires no comment; and though it contributes most essentially to all the purposes for which we are assembled, and gives life to all our proceedings, it is however on no account to be regarded as the chief aim and business of our meetings. That which has, from the first, gentlemen, been laid down as the highest object for which we meet, is to supply the great defect under which science has formerly laboured, of depending solely on individual and insulated efforts, by combining its cultivators into a body politic, calculated to give force and consistence to those efforts, and exercise a powerful influence both on its own members and on the public mind; thus marshalling a scattered militia into an organized and effective army, and converting desultory incursions into a regular and progressive march. The want of some such public authority in matters of science as belongs to an union like the present, could not be more strikingly shown than by the service which, on two occasions, the British Association has rendered to astronomy, in obtaining from Government the means of effecting the laborious and expensive reductions of the observations, first of the planets, and lastly of the moon, which had been made by Bradley, Maskelyne, and Pond. The existence and liberal support of the noble establishment at which these observations were made, bore evidence that our rulers have not been insensible to the immense importance of astronomical inquiries to a great maritime nation; but that so much of the precious ore which had been accumulated during the greater part of a century, by the successive labours of the greatest practical astronomers of any age or nation, should have remained unwrought and nearly useless for the highest applications of science, amidst the vain and oft-repeated regrets of those who were the most competent judges of its value,—what does this prove, but that there were no councillors of sufficient weight, number, or influence, not only to offer advice to the Government, but also to secure attention to it when offered?

In whatever degree the practical value of science may be beginning to be understood and appreciated among us, business of more proximate interest and more obvious urgency engrosses the attention of our public functionaries and legislators: numerous projects are presented to them rarely reduced to a practical form, among which they know not how to distinguish which *are*, and which *are not* deserving of national encouragement; and the consequence has been, with few exceptions, the general discouragement of all,—a consequence neither conducive to the reputation, nor serviceable to the interests of so great a country. But a representation on subjects of science proceeding from such an Association as this bears a public character, and carries with it a degree of weight which does not, and ought not, to attach to individual applications; and as long as the same judgement and forbearance which have hitherto characterized its course in this respect shall continue to be exercised,—as long as special care is taken to ask nothing of Government but *what it belongs to the national interests or honour to effect*, and *what cannot be effected but by national means*,—so long doubtless that attention will be paid to our recommendations which they have already begun to receive. Wisely cautious and reserved in exerting its influence, the Association has hitherto made but few applications to the government. Besides those to which we have before referred, and which were immediately complied with, another was made regarding the slow progress of the trigonometrical surveys of England and Scotland: the latter probably owes its recent acceleration to the attention thus first drawn to the subject, subsequently reinforced by a deputation from the Royal Society of Scotland; but the survey of England, which, having commenced nearly half a century ago, has not yet reached the Trent, is still in abeyance; and these northern districts in which we are assembled, and which comprise so large a part of the staple riches of the country, continue without their due share of the advantages which would attend its execution. There have been likewise two other national objects on which the Association has expressed its opinion; the one being the establishment of a magnetical observatory in Great Britain, the other an expedition for the purpose of making magnetical observations in the Antarctic seas. For the attainment of the former of these objects no interference was found necessary, an arrangement every way satisfactory being determined upon for connecting it with the Royal Observatory at Greenwich, on the recommendation of the Board of Visitors. The publication, in the present volume of our Transactions, of an elaborate report on the variations of the magnetic intensity of the earth, (unquestionably one of the most valuable which has hitherto appeared in them, whether we consider the laborious reductions it has required, or the important conclusions to which they lead,) recals our attention to the latter point. The subject is one of such deep interest, that we hope we shall not be thought to trespass too much on the time of the meeting, if we repeat some of Major Sabine's remarks upon it in his own words:—

"I have already adverted to what the influence of the Association may effect, in causing the spaces yet vacant on the map, in the British possessions in India and Canada, to be filled. But beyond all comparison, the most important service of this kind, which this or any other country could render to this branch of science, would be by filling the void still existing in the southern hemisphere, and particularly in the vicinity of those parts of that hemisphere which are of principal magnetic interest. This can only be accomplished by a naval voyage; for which it is natural that other countries should look to England. That the nations that have made exertions in the same cause do look to England for it, cannot be better shown than by the following extract of a letter of M. Hansteen's, which I take the liberty of introducing here, both for this purpose, and because it expresses in so pleasing a manner the praise that is so justly due to his own country, and which

I am sure will be cordially responded to by all who cultivate science in this country, and particularly by those who know the kindly feeling with which Englishmen are ever welcomed in Norway.

" C'est le Storthing (la Chambre des Députés) de la Norvége, qui a donné les frais à l'expédition en Sibérie. On a fait cela dans un tems où on a refusé les dépenses pour un château de résidence pour sa Majesté à Christiania. Dans un tems, où une telle économie a été nécessaire, il est tres honorable, qu'une Chambre, composée de toutes les classes du peuple, même d'un grand nombre de paysans, a *unanimement* résolu de donner les frais pour une expédition purement scientifique, dont les résultats n'auront jamais aucune utilité économique pour la patrie, et dont on ne comprenait pas la haute valeur scientifique. Regardé les ressources très-bornés de notre pays, c'est une générosité presque sans exemple.

" Comme la petite Norvége a fourni toutes les observations entre les méridiens de Greenwich et de Ochozk, et entre les parallèles de 40° et 75° de latitude boréale, il ne me semble pas une demande trop grande ou immodeste à l'Angleterre, si grande, si riche, si puissante, qui a nécessairement un plus grand intérêt dans toutes les sciences combinées avec la navigation, de fournir toute la partie méridionale de la carte. Une telle entreprise doit réfléchir une splendeur à la nation, et payera à la fin les frais par des résultats aussi utiles pour les sciences que pour la navigation. Il ne faut plus dans notre tems laisser l'avancement des sciences au hasard. Par des observations fragmentaires et discontinués on a tâché avec grande peine d'étudier les phénomènes magnétiques de la terre pendant deux ou trois siècles. Par deux ou trois expéditions arrangées *exprès pour ce but*, on pourrait en peu d'années avoir une collection plus complète, et d'une plus grande utilité pour la théorie."

The subject has in every way a claim on this country. The existence of four governing centres, and the system of the phenomena in correspondence therewith, was originally a British discovery. The sagacity of our countryman Halley was the first to penetrate through the complexity of the phenomena, and to discern what is now becoming generally recognised. England was also the first country which sent an expedition expressly for magnetic observation, namely, that of Halley in 1698 and 1699. Whilst approving and cordially co-operating in magnetic inquiries of other kinds which have their origin in other countries, it is right that we should feel a peculiar interest in that in which we have ourselves led the way, especially when its object is subordinate to none. As the research would require to be prosecuted in the high latitudes, a familiarity with the navigation of such latitudes would be important in the person who should undertake this service; and a strong individual interest in the subject itself would be of course a most valuable qualification. I need scarcely say, that the country possesses a naval officer* in whom these qualifications unite in a remarkable degree with all others that are requisite; and if fitting instruments make fitting times, none surely can be better than the present. Viewed in itself and in its various relations, the magnetism of the earth cannot be counted less than one of the most important branches of the physical history of the planet we inhabit; and we may feel quite assured, that the completion of our knowledge of its distribution on the surface of the earth would be regarded by our contemporaries and by posterity as a fitting enterprise of a maritime people, and a worthy achievement of a nation which has ever sought to rank foremost in every arduous and honourable undertaking.

The course pursued by the Association in reference to this object is well calculated to show the system of its operations, and the active but yet unintrusive and guarded spirit in which it prosecutes its aims. It was proposed at one of our meetings by the Committee of the Physical Section, that a representation should be made to government of the advantage which would accrue to science from an expedition to the Southern Ocean, devoted to the purpose chiefly of instituting magnetical observations. This proposal first underwent the revision of the Committee of Recommendations, and then obtained the sanction of the General Committee of scientific members; subsequent circumstances, however, being considered by the Council as unfavourable to the success of the application, it was not urged at that time upon the government, yet the object was not lost sight of. The Association next procured reports to be drawn up, (from one of which we have quoted the foregoing paragraph,) presenting a luminous exposition, both from published and unpublished sources, of the present state of our knowledge of the magnetism of the earth, and of the reasons which there are for wishing to extend to the Southern hemisphere those researches which in the Northern have led to such important conclusions; and thus has the way been prepared through information thus communicated to the public for pursuing the intended course with advantage, and making a more effectual application to the government. There may be, once in an age, or in many ages, an individual animated by so lofty an ardour for the advancement of a favourite branch of knowledge, as to engage, at his own cost, in an enterprise (like a recent survey of the southern skies) which it might have become a nation to take upon itself; and there may be an individual whose disinterested munificence may extend to the point of rendering labours of this magnitude as available to the public as if the state itself had contributed its aid; but such sacrifices to science are not only uncommon, they are in general impracticable; and there are numerous most important data and elements for philosophical reasoning, with all its

* Capt. James Ross, R.N.

train of practical utilities, which individuals cannot be expected to undertake, unless provided with pecuniary assistance. We have already said, that one of the principles on which the Association proceeds, is not to look to government for anything which can be otherwise attained; but when this Institution was established, the founders of it foresaw that it might itself be made applicable, in a great degree, to the object of supplying funds for such undertakings. The resources of other societies are employed on their publications or collections; and it is one of the rules of the German scientific "Reunions," that they shall possess no property. *Our objects* were more extensive than *theirs*, and, therefore, *our plan* was different. We have accumulated property and expended it, to give wings to investigation, partly by providing instruments and materials for carrying on certain determinate inquiries, and partly in defraying the expense of labour, especially labour of that kind, which, whilst it is of the highest value in its results, possesses no attractions in its execution, and would meet with no adequate remuneration.

The present volume of our Transactions contains many proofs of the service which the Association has rendered by such applications of its pecuniary means. In the account there rendered of the *discussion* of observations of the Tides, Mr. Lubbock (the Reporter) thus explains the manner in which the last grant of money placed at his disposal has been employed. He reports that two gentlemen had been engaged by him to discuss the observations which had been accumulated at Liverpool and the London Docks,—the one series continued during nineteen years, and consisting of 13,391 observations, the other carried on for thirty-five years, and including 24,592 observations,—and also to examine carefully the *establishment and average height of high water* in order to ascertain the fluctuation to which these quantities are subject; and after bearing testimony to the pains and accuracy with which the work has been executed, and stating the conclusions which result from these laborious calculations, adds, that "they never could have been undertaken but for the interest which has been felt on the subject by some of the most distinguished members of the Association, and but for the pecuniary grants which have at different times been devoted to this object," expressing, at the same time, a well-grounded hope "that when these results (which have since been published in the Philosophical Transactions) are carefully examined, they will not be found disproportionate in value to the great labour and expense which have been required for their attainment." A service of a similar description has been rendered to astronomy in the determination, by Dr. Robinson, of the disputed *Constant of Nutation* from the Greenwich Observations. Of this work, which involves much labour of reduction, and which, to use the words of the eminent astronomer who executed it, the powerful aid of the Association has enabled him to perform, a brief statement only, comprising the method employed and the general results, is given in our Transactions the fuller details requiring, as the author mentions, a different mode of publication. And this, Gentlemen, leads us to remark how unfounded were the apprehensions of those who feared that this Institution would divert the springs from which other societies are supplied; whereas the instances before us prove that their Transactions have been enriched instead of being impoverished by our operations. There is a further remark which we are prompted to make on the work accomplished under Mr. Lubbock's superintendence, by a reference which his report contains to a subject of great interest to the science of Geology. "I conceive (he says) that the best, if not the only method of investigating alterations in the height of the land above the water, in any given locality where the water is influenced by the tides, will be to examine carefully whether any alteration has taken place in the value of the (tide) constants D and E for that place, the height of high water being, of course, always reckoned from some fixed mark in the land." The meeting will here perceive one of those connexions between departments of inquiry apparently remote, which show how much each is concerned in the advancement of another, and ought to prevent any jealousy respecting the distribution and allotment of our funds. There is, indeed, no part of the proceedings of the Association which requires to be regarded with more care than the disposal of its grants, and our constitution has been framed with a particular regard to this point. In the first place, every section of science has its own committee, from whose deliberations every proposal of a grant must emanate. Secondly, these proposals are all submitted to a central committee, which recommends such of them as it deems unobjectionable to the General Committee for final adoption or rejection. By these means the best provision has been made for preserving the administration of our pecuniary resources pure, judicious, and consistent. So far as any rule of allotment has been followed, it seems to have been only to assign the largest grants to the most determinate, and at the same time expensive investigations; but the Association has not deemed it expedient to restrict itself to these. Whenever the committee of any section has been desirous of confiding any inquiry involving an outlay of money to a competent person, the committees of revision and approval have always been anxious to comply with the recommendation. The meeting will observe with satisfaction that the first step towards the solution of the geological question alluded to by Mr. Lubbock, has been taken under the superintendence of the committee appointed for the purpose. Mr. Whewell, to whose more special superintendence the conduct of this work was intrusted, reports that a line has been leveled by Mr. Bunt from Bridgewater to Axmouth, to be thence continued to the Bristol Channel; and such marks have been left as will allow of repeating or extending the levels, and comparing at a future period the height of the several fixed points.

We are thus led to say a few words about Geology, a science which is rapidly advancing to take its permanent station among the more accurate natural sciences. It is now six years since Mr. Conybeare laid before us his eloquent general view of its then existing state; but the lapse of a much shorter period in a science which is making such vigorous shoots, would present sufficient materials for a report which should enumerate and define the latest conquests it has achieved. The fact is, that the very literature of this subject is so vast, that none but the most practised and laborious geologists can keep pace with its progress; and though the anniversary discourses of the successive Presidents of the Geological Society generally contain a sketch of the works and memoirs which have appeared in the course of the preceding year, still we are convinced, that a condensed retrospect of the progress of geology, which should embrace a somewhat larger period and a wider range, executed from time to time at the request of the Association, would not only be grateful to geologists, but would also tend to combine the discoveries and promote the advancement of this science. But besides general reports on geology, this Association will, it is hoped, encourage a continued attention to the consideration of mineral veins, since there is no branch of geology of such direct public interest as the results of the miner's discoveries. In a clear and instructive report formerly read by our Treasurer, Mr. John Taylor (himself a most experienced and able miner), he expressed a wish, which we trust to see accomplished, that miners would hereafter not rest satisfied with such observations and knowledge as the mere practice of their art required, but would extend and combine their inquiries in such manner as to make them the foundation of more general and comprehensive views, and would tend to connect more intimately than heretofore the science of geology with practical mining. This subject, so important in its bearing upon the production of our mineral wealth, cannot be too strongly recommended to the attention of the Geological and Mechanical Sections of the Association. We venture, indeed, to hope that the Newcastle meeting will be pre-eminently marked by the diffusion of much sound mining knowledge, flowing as it must from the meeting together of the most experienced Cornish miners with those of Durham and Northumberland. We are further encouraged to indulge in this expectation from knowing that this meeting is honoured by the presence of an Austrian nobleman, long valued by English geologists, and whose thorough acquaintance with mineral veins, and all their complicated *faults* and changes, well entitle him to occupy the high station confided to him by his sovereign*. And here we cannot but observe, that as, with all its mineral wealth, Great Britain is the only country in Europe without a national school of mines, so much the stronger is the call upon the British Association to promote the analysis of every natural phenomenon and useful invention connected with the art of mining. But while we make this appeal, we cannot assemble in this neighbourhood without congratulating the University of Durham on having led the way to the establishment of a school of mines and engineering, in which the principles and knowledge of this branch of science are regularly taught; and we further feel gratified, that so important a charge has been intrusted to men distinguished for their scientific attainments, including in their numbers one of the earliest promoters of the British Association, and one of its local secretaries at this meeting †.

In the arrangements of the Association, the sciences of Mineralogy and Chemistry have been united. Such an union may be justified, not merely by its convenience in the distribution of our labours, but by the close alliance which subsists between those sciences, in all that concerns the connexion of chemical composition with crystalline forms, presenting so many remarkable relations of very recent discovery, and leading so rapidly, as Mr. Whewell has, on more than one occasion, so clearly shown, to enlarged views of the true principles of mineralogical qualification. But whilst we fully recognise the connecting links which unite those sciences, we trust that this partial and temporary separation, which the active and somewhat absorbing study of palæontology has almost necessarily occasioned, will not be of long continuance, and that the laws of crystallization, which constitute its alliance with another science, will in the progress of our knowledge give as much importance to its connexion with the study of the crystalline structure of vast masses of the surface of the globe, as in the most searching analysis of its minutest particles. Let not, however, the exclusive advocates of any one theory of the proper relation of those sciences induce us to abandon inquiries so pregnant with remarkable conclusions, and which truly constitute the great basis and framework of modern geology: for the more minute and laborious our investigations, the more certainly do we make out that many rocks which were once supposed to be made up of inorganic matter, are in truth composed of animal remains. And do we not look for the presence among us of a distinguished philosopher of Berlin, who, above all others, has eliminated this discovery, and who, by the powers of the microscope, has revealed to us the skeletons of millions

* Count Breunner, Director of the Imperial Mines, Foreign Member of the Geological Society.

† Professor Johnston.

of once living and perfect animalcules inclosed in a single cubic inch of solid stone. Well, indeed, may we quote the recent work of Lyell, who, rejoicing in this great discovery, exclaims with the poet,—

"The dust we tread upon was once alive."

In noticing the labours of the Section of Geology and Geography, we have to observe, with regret, that the latter science has not hitherto received at our meetings that amount of attention to which it is justly entitled. When we consider the advances which the science has recently made under the auspices of the Royal Geographical Society of London, we cannot but lament that the British Association did not, at an earlier period, request a report from some one of its members upon the present state of our geographical knowledge, and upon those departments of it in which our researches might be most advantageously prosecuted. The annual reports of the Secretary of the Geographical Society,—particularly the last report of Capt. Washington, and the admirable discourse recently delivered by its President, Mr. W. R. Hamilton,—have in great measure supplied this deficiency, making the public acquainted both with much that has been done, and much that remains to be worked out in this very important branch of knowledge. But though we have thus been partially anticipated, we feel satisfied that such a report, by bringing into prominent notice, before the whole body of the Association, a statement of those great geographical problems, whose solution is most specially desired or most easily effected, may serve to secure for the promotion of geography the application of some portion of those funds which have been hitherto exclusively appropriated to other sciences.

The merits of the Statistical Section have been already made manifest, by the collection of a great variety of very important data. On this occasion we have to notice a very perspicuous and well-arranged report, which appears in our Transactions, upon the statistics of a large province of Hindostan, which sufficiently proves that a statist, who would really contribute to the advancement of statistical science by collecting facts in distant regions, must possess no slight qualifications. In vain, in the absence of other essential branches of knowledge, may he accumulate half-digested and ill-assorted observations; he must also combine, as in the person of Colonel Sykes, the acquirements of the naturalist and geologist with those of an accomplished soldier and of a man of general information.

The accumulation of such facts is obviously a very fit part of the labours of this Association, for they prove statistics to be truly a science of method. This science occupies the same relation to political economy in its most comprehensive sense, which astronomical observations held relatively to astronomy before the discoveries of mechanical philosophy enabled recent philosophers to make those early observations perform a mighty part in testifying the great primal truths of physical philosophy, and applying them to explain, and even to predict, the varied motions and phenomena of the earth and heavens. Such a stage there must be in every inductive science,—one in which immediate straining after comprehensive truths would be rash, while the marshalling and classing phenomena is a task full of usefulness and hope. Those only who mistake the stage of discovery in which statistical observers are now placed,—who do not see that at present observation without premature speculation is the one and necessary step towards wide truths,—will either be impatient to weave rash theories from our present imperfect materials, or to scoff at the unscientific character of those who labour patiently to increase and arrange them. The analogy between the early stages of astronomy and the actual position of statistics might be made more complete. The *secular* character of many classes of statistical observations necessary to elucidate difficulties and disentangle truth might be easily demonstrated, but enough has been said for the purpose of indicating the really scientific character of this useful branch of our Institution.

It has fallen to our precursors to comment on the advances in Natural History which have been made by the Section of Zoology and Botany; and although, on this occasion, we are not presented with any report upon these sciences, you all know how ably they have been elucidated at former meetings, by a Lindley, a Jenyns, and a Richardson; and also with what vigour that section has prosecuted its inquiries under the auspices of a Henslow and a Macleay. We must, however, here allude to the distinguished Northumbrian naturalist who occupies one of our vice-chairs, and express our hopes that Mr. Prideaux Selby may soon be called upon to contribute what is yet a desideratum—a report upon the present state of the science of Ornithology.

We have hardly ventured to allude to the separate proceedings of the Sections, for any discourse which should attempt to analyze their labours or to do justice to their usefulness would occupy too large a portion of your time. And besides this consideration, you, Gentlemen, are all aware, that these Sectional Meetings give rise to the Reports we have been considering, and also to the various practical researches which are carried out by the employment of your own funds, or by demands upon the country. If, therefore, the Reports constitute our high claim upon the literature of science, the proceedings of the Sections must be viewed as the fresh current of scientific enterprise, which continually vivifies and renovates the whole body of the Association.

Among the investigations which are proceeding under the auspices of the Association, those which originated in the Committee of the

Medical Section, including several subjects of physiological interest reported upon in the present volume, are remarkable for that spirit of *co-operative* labour which has not been common in this country, and which it is one of the happiest effects of these meetings to facilitate and encourage. In like manner, a question of great interest as regards one of the most important products of our mineral wealth and national industry, which had been discussed with more than common warmth and earnestness at former meetings of the Association, has been examined by an analysis, performed by one of the most distinguished chemists of the present day, of the iron produced by the application of the hot and cold blast respectively; which was undertaken at the request of the Chemical Committee, combined likewise with experiments, on an extensive scale, upon its relative strength and other properties, which were commenced at the desire of the Mechanical Section, by Messrs. Hodgkinson and Fairbairn, whose profound and extensive knowledge of practical mechanics so well qualified them for a task which they have executed with singular ability, enterprise, and skill. The experiments on Waves, which are detailed in Mr. Russell's report in our present volume, were likewise undertaken at the request, and carried on by the aid of the funds of the Association. The accurate conception of a wave, its origin, propagation, and laws, is one of the most difficult and fundamental of those which are required in many of the delicate and embarassing inquiries of natural philosophy; and the experiments of Mr. Russell are well calculated to illustrate and confirm many of the results which the mathematician has deduced from the theory of fluid motion. Adhering, therefore, to our design of mainly noticing those parts of our recent transactions which illustrate the prominent points in our system of operations, we shall conclude our remarks by noticing a report by Prof. Johnston, on a new and curious subject of chemical inquiry, as affording a good example of the execution of an object which the Association has had much in view. The discovery that there exist definite chemical substances, which are capable, under certain conditions, of assuming more than one crystalline form, not deducible from nor referable to each other, and accompanied with different physical properties; and furthermore, that there are instances of substances which are capable (independently of any change of composition) of undergoing some internal transmutation sufficient to vary even their chemical affinities: these are discoveries which, pointing out a new road to the investigation of the hidden mysteries of molecular attractions, peculiarly deserve to be verified and extended. But it so happens that they have been little studied or prosecuted in our country; and, therefore, the Chemical Committee, in accordance with one of the prominent designs of the Association, selected this particular point as the subject of the Report on Dimorphism, printed in this volume, which gives a fuller statement than we before possessed, of the facts arrived at by foreign experimenters, the reasonings founded upon them, and the questions which are left for future inquirers to solve. This is the precise point at which the Association aims in the reports on the state of our knowledge, which occupy the chief space in its publications; they are not intended, like the articles in an encyclopædia, to *teach and diffuse* science, but to *advance* it—to show what *has been* done, with a specific view to what there *remains to do*—to look forward to conquests to come, rather than backward on those which are past—to survey the border territory, and reconnoitre the *debateable* land. We have in this, as in other respects, followed in the steps of him who gave the original sketch of an Institution like the present. The great teacher of inductive science and experimental philosophy, who first showed the importance of knowing the lines which divide knowledge from ignorance, and in the memorable list of DESIDERATA which he drew up, did more for "the progression of the sciences" than would have been done by any discoveries he could have made.

Having thus endeavoured to elucidate, by reference to some portions of its recent transactions, the comprehensive system of this Association, and to mark the real value of its corporate influence, its pecuniary resources, and its concentrated intelligence, I would lastly notice that part of the system which has given occasion to our present muster in this prosperous and splendid city—the migratory character of our meetings. In these migrations there is a double advantage; the Association gains much by them, and perhaps the places it visits do not gain less; for its visits may sometimes have the effect of drawing genius from obscurity, and giving an impulse to powers which might never have been exerted, and a direction to labours which might otherwise have been misapplied. To our own body two great advantages are derived: one is, that the wave, in rolling along, gathers to it all the scattered science of the land, and that a more general and powerful union is thus formed than could ever be collected by an Institution resting on a fixed point: the second is, that varied objects of interest and different opportunities of utility are offered by circumstances proper to the different places which the Association visits; thus the lofty tower of York furnished means for the best experiments that have been made on the phenomena of rain; Liverpool contributed its contingent to our knowledge of the tides; whilst Bristol carried a line from sea to sea, to ascertain the permanence or the mutations of the level of the land and water. And does not this city and vicinity, Gentlemen, also present its own peculiar objects of speculation and opportunities of research? Is not the optical philosopher interested in its celebrated glass-works? Can the chemist contemplate with indifference those conspicuous and truly magnificent establishments which exhibit, on so grand a scale, the application of those processes, which have been deduced and perfected in his laboratory, to productions so important in our manufactures and arts? Can the geological or physical inquirer stand near its mines—those vast storehouses of nature for the uses of art, the theatre of the most beautiful of all the applications of science to the purposes of humanity—without having his curiosity awakened? or contemplate those deep excavations, the most accessible of any that have been carried into the bowels of the earth, without being tempted to investigations which may lead perhaps to a better understanding of the internal condition and structure of our globe? Or can we survey the architectural creations which surround us in the place in which we are assembled, where order and magnificence have replaced confusion and meanness, with a rapidity more resembling the illusions of an Arabian tale than the sober anticipations of experience, without being encouraged in our own efforts by witnessing such noble results of *individual* enterprise, genius, and arrangement, which have associated the triumphs of art with those of manufactures and commerce, and combined the refinements of wealth with the most varied productions of industry?

> "Hîc portus alii effodiunt; hîc alta theatris
> Fundamenta locant alii, immanesque columnas
> Rupibus excidunt, scenis decora alta futuris."

Finally, Gentlemen, there is another reason for these migrations, which it would be highly ungrateful in us to overlook, which is equally felt by the Association and by the place which it visits—the warmth of hospitality which we see these visits call forth, the union of hearts and the excitement of kind and friendly feeling acting on all our objects, like oil on the wheels of a vast and powerful machine, without which its every movement would be retarded, and its whole power brought to a stand. Never, indeed, can the vitality of this Association be impaired, so long as the leaders who have borne the bark of science along the waves shall lay stoutly to their oars. Assembling for a common cause, and confiding in each other, may they ever glory in having knit together all classes in the love of science; and whether presided over, as on this occasion, by a noble duke, alike illustrious for his just appreciation and generous encouragement of our pursuits, or in the ensuing year by some one eminent in their cultivation, we shall, we trust, go on waxing in strength, and holding out the cheering example of a great and triumphant commonwealth of science!

REPORTS

ON

THE STATE OF SCIENCE.

Account of a Level Line, measured from the Bristol Channel to the English Channel, during the Year 1837–8, *by* Mr. Bunt, *under the Direction of a Committee of the British Association. Drawn up by the Rev.* W. Whewell, F.R.S. *one of the Committee.*

1. At several of the meetings of the British Association it was suggested, that the exact determination of the relative level of three points considerably distant from each other on the coasts of this island might throw light upon several important questions. Such a determination, it was represented, might especially be made subservient to the solution of the two important problems, —how far the position of the earth's surface is permanent—and what ought to be understood by "the level of the sea." For if, as some geologists think, many parts of the earth's surface are slowly changing their position, such a change is extremely difficult to prove or disprove by observations made at any one point. But if three points were at one time determined to be in one horizontal surface, and were at a subsequent period found to be at different heights, their relative elevations at the second epoch would not only establish the fact of a change in the position of the earth's surface, but would enable us to determine, by an easy calculation, the angle through which this part of the surface had been elevated, and the axis about which the elevation had taken place. And with regard to the level of the sea, it is well known that surveyors and naval men are in the habit of assuming the surface of *low water* of spring tides to represent this level. Now not only is such a surface extremely indefinite (varying very considerably with the parallax and declination of the moon and sun) but it is not in fact, not even approximately, a *level* surface at all. The level of the sea, thus determined, would be

twenty feet lower on some parts of the coast of England than on others. And although men of science have very generally seen the propriety of taking *mean water*, (the mean of low and high water,) for the level of the sea, this selection, till confirmed by some actual observations of facts, might appear arbitrary and insecure. But if it be found that the mean water is at the same level at different and distant points of the coast, where the low water is at different levels, the propriety of taking mean water for the level of the sea will probably be generally acknowledged.

It may be observed, moreover, that the question of the permanence or change of the height of any point of the coast (unconnected with a system of interior leveling) cannot be decided by observation, except by reference to the level of the sea; and therefore to determine what *is* the level of the sea is important also to the geologist. On coasts where there are tides, the question of the stability of the land involves the question of the laws of change of the water.

2. For these reasons it was considered desirable to ascertain by careful and exact levelling the relative heights of certain points of the coast of England, and to refer these points to the sea by adequate tide observations. The British Association at its meeting in 1834, voted a sum of money (500*l*.) to be employed upon this object, and appointed a Committee to decide upon and direct the requisite operations. The same thing was done at the subsequent meeting in 1835. But the difficulty of fixing upon a plan of operations and of selecting the means of carrying it into effect by a joint deliberation of a large Committee scattered over the whole empire, prevented any active steps being taken towards the attainment of the object. At the meeting at Bristol in 1836, in order to remedy this inconvenience, those members of the Committee who had the opportunity of conferring with each other after the separation of the Association took upon themselves the task of directing the execution of the plan. And it appeared to them desirable that a person should be selected to perform the leveling operation for the Association, independently of any other surveys which might be going on; for no materials collected for the purpose of any other survey could, in accuracy and other conditions, answer the purposes contemplated by the Committee. They considered themselves fortunate in being able to engage Mr. Bunt of Bristol in this service, having entirely satisfied themselves of his accuracy and scrupulousness in observing, and of his clear apprehension of the nature of the operation. They also took the precaution of directing Mr. Bunt to execute a preparatory level from Bristol

to Portishead, (a distance of eleven miles) and back to Bristol, in order to ascertain the degree of accuracy which could be attained in this operation. The total amount of error resulting from this operation was 1·07 inches; but there appeared grounds for believing that the uncertainty of the result was very much smaller than this quantity; and this belief has been confirmed by the general course of the subsequent operations.

3. An excellent telescope level was constructed by Mr. Simms for the Association, to be used on this service, and also a leveling staff, for which however Mr. Bunt afterwards found it convenient to substitute one of his own construction. This is described in the Appendix to this account.

4. The extremities of the line selected were on the north coast of Somerset and the south coast of Devon, as affording the case where coasts belonging to separate seas could most easily be brought into connexion. A north and south line being thus obtained, it was proposed to extend the operation to the eastward, so as to obtain a third point under suitable conditions.

The first line selected for leveling*, on a careful inspection of the country, was one proceeding from Bridgewater up the river Parret by Langport to Ilminster, Chard, Axminster, and thence to the mouth of the river Axe, which was fixed upon as one of the terminal points where tide observations were to be made. Bridgewater was connected with the sea at the other extremity by a line, which, skirting the Quantocks, reached the shore in the first instance at Stolford opposite the Wick rocks; but was afterwards carried further to the west in order to reach a more solid rock, and terminated at East Quantocks head near Watchett.

5. The leveling from Bridgewater to Axmouth was begun May 16, and ended July 8, 1837. From Bridgewater to Wick rocks the operations of leveling and preparing for tide observations were carried on in October 1837. Tide observations were made at Axmouth from January 4, to February 2, 1838, and at Wick rocks from November 9, to December 9, 1837; and again at Axmouth, simultaneously with Portishead, July 14 to 21, 1838.

The line thus leveled crossed no very great elevations, and was for the most part very conveniently even. The highest point was at White's House near Chard, where it attained a height of 280 feet.

6. The extension of the leveling process to any considerable distance east or west of this line was a matter of difficulty; the ground in both directions consisting of a series of hills and val-

* See Plate I., (the map).

leys of considerable magnitude. Such a country would require to be leveled by a series of very short distances; and this circumstance would not only add greatly to the labour and expense of the operation, but would render doubtful, in a very material degree, the accuracy of the result. It was therefore judged advisable to be content with extending the level north-eastward to Portishead and Bristol. By this means the east and west extent of the surface surveyed became nearly equal to the original north and south line; and the level line which rests upon the Quantocks at one extremity, crosses the Mendips and the Leigh Down Hills, connecting a great number of different geological formations.

7. This line from Bridgewater to Portishead was leveled between May 15, and July 6, 1838; and tide observations were made at the latter place in May 1837, and July 1838.

8. Both in the leveling and in the tide observations, every precaution was taken to avoid mistakes and to ensure accuracy. As leveling operations of a very delicate kind have rarely been performed, and are nowhere sufficiently described, it is considered worth while to record the method employed in this instance, and an Appendix is added containing this description. It may here be observed, that the most important precaution, that of making the distances of the staff from the telescope equal in the fore observation and the back observation, was throughout attended to; and that all the lines were leveled in both directions, proceeding from the beginning to the end of the line, and then returning back from the end to the beginning.

9. By employing this method of verification, an apparent error in the process is brought into view, for which it is difficult to account, but which is so constant in its occurrence that we cannot help supposing it to depend on some general cause. The error consists in this;—that in proceeding with the leveling operation along a line which is really level, the further end constantly appears, from the observation, to be the lower end; and the amount of this depression appears to increase with the distance. Hence, when we go to the end of a line and then return to the starting point, we find the resulting elevation of the point lower than its real elevation. The difference arising from this cause is never considerable, but is always in the same direction, and generally (in the same series of operations) greater in proportion as the distance is greater. Thus in the line from Bristol to Portishead (11 miles) it was 1·07 inches; from Bridgewater to Axmouth (40 miles) it was 4·11 inches; from Bridgewater to East Quantockshead (16 miles) it was 1·94 inches; from Bridgewater to Portishead (29 miles) it was 7·6 inches.

10. It is very difficult to explain the cause from which this seeming error arises, or even to conceive any cause from which it can arise. The errors arising from the curvature of the earth, and from any permanent refraction, are eliminated by the condition of equal distances in the fore and back observations. The difference does not seem to arise from the effect of the sun's rays on the instrument, for it is not removed by shading the instrument with white paper; nor from any rise of the peg between the fore and back observation, for it is not confined to soft ground. It appears to go on increasing with the time during which the observations are continued, and is such an error as would result, if we suppose that in every interval of time between the back and the fore observation, something takes place by which the staff is apparently (by refraction or otherwise) less elevated (or more depressed) at the fore observation than it had been at the preceding back observation. For these elevations are supposed to be equal in the process; and if the elevation of the fore point by refraction or any other cause be the smaller, the point will appear to be lower when it is really on the same level. This statement, however, is made rather with a view of explaining the nature of this error than of assigning its cause.

11. But since it is thus probable that this apparent error arises from some constant and general cause, it is clear that we shall get rid of its effects in each case by taking the mean of the first and last results. We may therefore suppose the mean difference of levels obtained by leveling between two points, first in one direction and then in the other, to be accurate within limits very much smaller than the errors above mentioned. We may venture to confide in this result to a fraction of an inch.

12. The relative heights of the parts of the lines surveyed being determined by the operations of which we have been speaking, marks were fixed at various points, by means of which the position of the line now measured may hereafter be again discovered. These marks are the following. A place was selected in the solid rock on the shore just below the fort at Portishead; and in this was inserted horizontally a cylinder of iron, two inches diameter and fifteen inches long, containing in its centre a brass wire one eighth of an inch in diameter, which marks the position of the standard point, about eight feet above the highest high water. This mark is on the property of James Adam Gordon, Esq., of Naish House, who kindly gave permission for its being placed there. The mark at East Quantockshead is on a farm called Perry Farm, the property of J. F. Luttrell, Esq., of Dunster Castle. It consists in a block of granite, a ton and half weight (the gift of the corporation of

Bridgewater) in which is inserted horizontally, without lead, a copper cylinder an inch and a half diameter and fourteen inches long. In order to prevent this bolt being drawn, it is fastened with a copper key passing through a transverse hole into a notch in the bolt, and the transverse hole is filled with lead. A similar block of granite, (also presented by the corporation of Bridgewater) with a similar copper bolt, is the mark at Wick rocks, in the parish of Stogursey near Bridgewater, which stands on the property of Sir Peregrine P. Acland, Bart. The mark at Axmouth is a similar block of granite, procured by J. H. Hallett, Esq., of that place, on whose property the mark stands, and who has manifested a great disposition to forward the operations in every way. The kindness and liberality of the gentlemen who have been mentioned, on whose ground the marks have been inserted, have much forwarded the undertaking, and deserve the best acknowledgement the British Association can make. These gentlemen are also willing to perpetuate the obligation which science thus owes them, by allowing themselves to be considered the guardians of the permanent level marks thus existing on their property; and this is a kindness the more valuable, since the British Association neither has nor can have any valid right to such services. The marks of which this statement contains the record, may hereafter be of great consequence in settling important questions of a scientific nature, if their preservation be, as we do not doubt it will be, kept in mind by the proprietors of the estates above mentioned.

There is also a bolt inserted in the wall of the church at Axmouth; and it is intended to place a similar mark in the church at Uphill, a village situated where the level line crosses the western extremity of the Mendips.

With the permanent level points at Axmouth, Wick rocks, and Portishead, the surface of the sea was compared by means of tide observations made at first for a month at each of those places. In pursuance of the views already stated, the mean of high and low water was taken as representing the level of the sea. In fact, this level of "mean water" is so nearly constant, that even a few days will give its position with tolerable accuracy; and observations continued for a fortnight, which of course includes spring tides and neap tides, give the result with great precision. The first result was, that while the level of mean water at Axmouth and at Wick rocks did not differ by more than a small fraction of an inch, the level of mean water at Portishead was four inches and a half lower than at the other places. As however it appeared possible that this difference might result from the observations being made at different times of the year, further observations were made *simultaneously* at Axmouth and at Portishead, from July 16 to 30, 1838; the result of which was that the level at Portishead is nine inches higher than that at Axmouth.

13. The difference between the result of the first and second set of tide observations at Axmouth (1·29 feet in the mean level,) was such as to require examination. It appeared possible that this difference might arise from some of the inequalities which affect the tide, and depend upon the time of year; one set of observations having been made in January 1838, and the other set in July. I therefore requested Mr. Bunt to examine the Plymouth observations of high and low water for the same period (with which observations I was supplied by the Admiralty). The result of this examination was that the mean sea level at Plymouth was only one fiftieth of a foot higher in January than in July last: and it therefore appears certain that no annual inequality of the tides is the cause of the difference. I am led to ascribe it to the circumstance, that in the observations of January, the low water at Axmouth was taken within the bar at the mouth of the river. In July, the low water, within this bar, was certainly higher by a foot or two than it was on the outside; and though the bar had altered its position in the intermediate time, I have little doubt that it was in such a condition in January as to vitiate the observations of low water. The observations of low water made in July last, simultaneously with those at Portishead, were made entirely outside the bar.

14. Taking the simultaneous observations made at Axmouth and Portishead in July, 1838, as the most free from obvious objections, we obtain the following results respecting the comparative level of the sea at the two extremities of our line. The measures of level were all referred to a certain zero point, assumed 100 feet below the point where the operations began. The level of the mean tide above this zero was

at Axmouth . . .	71·96
at Portishead . . .	72·69
difference . .	·73 foot;

or something less than nine inches. But the range of spring tides at Axmouth was 5 feet above and below this level; at Portishead 17·87 feet above and below. Hence we have for the relative levels of high and low water at spring tides

	High Water.	Low Water.
at Axmouth . .	76·96 . . .	66·96
at Portishead . .	90·56 . . .	54·82
differences . .	13·60 . . .	12·14

Thus the sea at Portishead is at high water 13·6 feet higher and at low water 12·14 feet lower than at Axmouth. And if we take the extreme tides which occurred during the observations, the differences are still greater; for the greatest range of tides at Axmouth was 10·8 feet, and at Portishead 41·1 feet. And the difference of the halves of this is 15·1 feet, which is greater by 2·23 feet than the difference of the ranges just employed. Also these elevations of the ocean are nearly contemporaneous; for the high water at Axmouth occurs (at a mean) forty-four minutes earlier than at Bristol, and at Portishead two minutes and three quarters later than at Bristol.

We have in these results a very strong indication that the *mean tide* is what we must take as the level of the sea, for it would be difficult to believe that the level of the sea is fourteen feet higher at Portishead than at Axmouth, or sixteen feet lower, which are the consequences of taking for the level high water or low water at spring tides*.

We may add, that at another of our stations, the Wick rocks, by a month's observations in November, 1837, the mean level was 73·11, or 3·8 *inches* higher than at Portishead. Perhaps a portion of this difference may be due to the inevitable errors of the operations. The range of the tides at this place is nearly the same as at Portishead, and the time of high water (at the mean) about thirty-seven minutes earlier than Bristol, and therefore about seven minutes later than Axmouth.

15. The general result to which we are led is that *the mean tide must be taken as the level of the sea*. This result had already been arrived at by various persons. Capt. Denham had asserted it as the consequence of his observations at Liverpool; and Mr. Walker had been led to the same conclusion by the tides of Plymouth. I had also pointed it out as the result of the Plymouth observations in the *Philosophical Transactions* for 1837.

16. But these conclusions were supported only by observations made at a single place; namely, by its appearing that the height of mean tide was nearly constant, (varying at most only a few inches) while both high and low water varied by many feet. And so far as I have yet seen the evidence, it seems probable that though the change of the level of mean tide during a fortnight be small, there really is some regular change of this level tide, produced by the effects of the moon and sun. But the smallness of the changes of this level, as it is now announced, rests upon quite different evidence, and appears to indicate a perma-

* In Plate II. I have represented the relative range of the tides at these places as observed.

nence of a more rigorous nature. The mean level of mean water at one point of the coast of the island, taken for a semilunation (and probably still more if taken for several lunations), may be asserted to agree with the mean level at another point taken in the same manner, within a very few inches. Perhaps the agreement, if places situate on the open sea were taken, is still nearer; for Portishead, and even Wick rocks, may be affected by the narrowness of the Bristol Channel, which may elevate the low water there, as it certainly does in a river. It appears very probable that the level of mean tide at different places on the open coast agrees as nearly as the operation of leveling can determine.

17. This result is not only very curious in itself, but pregnant with important practical consequences. It is very clear, from the slightest consideration of our results, that nothing but error and confusion can result from processes, such as have often been employed up to the present time, in which heights are determined from "the level of the sea," this level being understood to be that of low water spring tides. Such heights are not measured from a *level* at all, but from a surface of which some parts are sixteen feet lower than others within the limits of our operations, and probably above twenty feet, if we take the extreme cases on the shores of our island. The only method of stating heights which can have any pretensions to accuracy, is that of reckoning them from a conventional fixed datum upon the solid land; to which datum the sea as well as the land must be referred by proper leveling operations.

18. As a specimen of the doubt and confusion which have hitherto prevailed on this subject, I may quote a passage from Mr. Telford's report on the project of a ship canal, intended to connect the Bristol Channel with the English Channel, and following nearly the same course as our level line. He says "the total distance from Beer Harbour [near Axmouth] to Bridgewater Bay [in which are Wick rocks] is forty-four miles five furlongs. The fall from the summit to high water at an ordinary tide in Bridgewater Bay is 231 feet; but by taking another tide at Beer, the fall was found to be 233 feet."

The vague mode in which this result is expressed, an "ordinary tide" being taken in Bridgewater Bay, and "another tide" at Beer, without any indication whether any correction was required for the difference of tides, and whether the result could pretend to any accuracy, is, I conceive, an instance of the impossibility of referring any elevations to the sea in a satisfactory manner, till it is determined how we are to allow, not only for the difference of high and low water, but for the different heights of different high waters; that is, till a proper discussion of tide observations is combined with a system of leveling operations.

I may add that this result is certainly erroneous; for it gives the high water in Bridgewater Bay only two feet higher than on the south coast of Devon; whereas by our observations, which certainly cannot err a foot, the former level is at least fourteen feet higher than the latter. The difference, which is perhaps not to be wondered at in rough leveling such as that performed in a preparatory survey for a canal, is twentyfold greater than our operations, carried backwards and forwards, and only differing one or two inches in the result, allow us to consider as possible.

19. We may observe, in conclusion, that the result of our operations, namely, that the mean tide in different points of the coast is at the same level within a few inches, is of no small practical value. For this being so, the level of any place within a moderate distance of the coast may be determined, for the purposes of canals or railroads, or any similar undertakings, with reference to the level of places hundreds of miles distant, by taking a fortnight's observations of high and low water, and then leveling a few miles into the interior of the country.

20. It may perhaps be said that the conclusions thus stated depend upon a single comparison; that of the south shore of the Bristol Channel, with the north shore of the English Channel. When it is recollected that there are, omitting the smaller flexures, some hundreds of miles of coast between the two extremities of our line, and that the tides at the extremities differ as forty feet and twelve feet, I think it is impossible not to allow great value to the result; the operations being, as I conceive, of unimpeachable accuracy. But I am at the same time quite ready to admit that it would be highly desirable to have the result corroborated by other comparisons of the same kind, especially by a comparison of the east and west shores of England. For this purpose it might be desirable to carry a level line from Bristol, which is already connected with our operations, to London. The expense of this, if performed in the same manner as that which I have described, would be great; but it appears to be worth consideration, whether this expense might not be much reduced by observing the waters of existing canals.

21. I will add, that such an extension of our level to London, and in like manner to Plymouth, Liverpool, and other principal ports of the empire, would be desirable in another view. As I have already said, we cannot speak with accuracy of any level except a conventional one; and as each of these ports has its own tide scales to which the rise and fall of the sea's surface is referred, it would be desirable to compare the absolute position

of these scales with regard to a level surface. We may in this manner, and in no other, learn the true form of the ocean at any time; besides the practical advantages, which, as I have said, would flow from having standard levels in various parts of the island. I may mention, that the kingdom of the Netherlands already possesses such a system of levels, by which all points of its surface are referred to a certain zero at Amsterdam.

Whether such an extension of the level line measured for the Association be desirable, may best be determined by the Committee of the Physical Section. In the mean time I trust that what has already been done possesses no small value, being, so far as I am aware, the first attempt of the kind, executed with great care, and I see every reason to think, with great accuracy.

22. The following are the heights of the marks above the zero point.

	Feet.
Iron bar at Portishead Fort	102·5795
Temporary mark at Wick rocks (Station N° 810)	99·4833
Copper bar in granite block, Axmouth	83·6513
Copper bar in Axmouth church	89·5318
Copper bar in Uphill church. (This is not yet inserted. The + cut on the east end of the church is at the height)	205·8305
Copper bar at Perry Farm, East Quantockshead	244·4365
Copper bar at Stolford	125·1114
Level of mean water at Portishead	72·69
——— Wick rocks	73·11
——— Axmouth	71·96

Account of the Leveling Operations between the Bristol Channel and the English Channel, by THOMAS G. BUNT.

PREVIOUSLY to my commencing the leveling which I had received instructions from Professor Whewell to undertake on account of the British Association, I was desirous of deriving such assistance as might be obtained from any published account of a similar enterprise, in which due attention had been paid to the niceties which the operation requires, and the best means for ensuring accuracy ascertained and pointed out. All the ordinary treatises on leveling are of the most elementary and superficial kind; and the only account I have met with which could at all assist me is that given by Captain Lloyd in the Philosophical Transactions for 1831, which details with clearness, and at considerable length, every particular connected with his leveling from Sheerness to London; a scientific enterprise of similar character to that in which I was about to engage. This memoir of Captain Lloyd I regard as one of considerable value, and have derived from it much information and assistance. Most of his arrangements appear to me to be very judicious, and several of them I have either adopted or imitated. On one important point, however, I am obliged to differ from him, to which I shall have occasion to advert presently.

The instruments made for this undertaking were a spirit-level, and brass leveling-staff, by Simms, London. The telescope, though only 14 inches in length, was found to bear the high magnifying power of 26 so well under all circumstances, that the other eye-piece with which it is furnished was never employed. The glass spirit-tube is so nicely ground, that the position of the air-bubble is sensibly altered by raising or lowering either end of the tube $\frac{1}{100.000}$th part of an inch. In the focus of the telescope are a horizontal and two vertical hairs, which latter afford a very convenient means of measuring the distance of a station, within about the $\frac{1}{100}$th part of the truth, by counting the number of intercepted divisions of a scale made for the purpose, and held horizontally over the station by an assistant.

The legs which were made to support the level, although very strong, were found to vibrate so much from the action of the wind, as to render it difficult to take a correct observation, except in perfectly calm weather. It was also next to impossible to level the spirit-tube, unless by accident, for want of a slower and more delicate motion than that afforded by the parallel plate screws. I therefore ordered a very strong stool to be made by a carpenter, the top of which was a thick board 12 inches in diameter. The level was then detached from its former support, and fastened to a circular piece of mahogany, which rested by three foot-screws on the top of the stool, and was firmly secured to it by a stout wooden screw, with a nut at bottom, passing through both the circular boards. On trying this apparatus, I found that a more delicate vertical motion was still wanted, which was at length perfectly attained by causing one of the three foot-screws to rest on a small brass lever at a very short distance from the fulcrum, while the farther end, furnished with a fine screw and milled head, communicated about $\frac{1}{12}$th of its own vertical motion to the foot-screw of the level, affording a very simple and delicate means of adjustment.

The level, although now incomparably steadier than before, was still found liable to disturbance from the wind, when it blew with any considerable force; to protect it from which we carried with us a piece of canvas, 6 feet square, nailed to two poles, which were sharpened at the bottom, to enter the ground. This screen being held firmly by two men on the windward side of the instrument, sheltered it so completely, that I was able to proceed in windy weather, with but little interruption.

The brass leveling staff was employed in leveling between Bristol and Portishead; but being found inconvenient, and liable to get out of repair, was obliged to be laid aside. The staff which I subsequently constructed and used, is of wood, 9 feet long, and 2 inches wide, a single piece of straight-grained oak. On the face are two different scales of equal parts. One is the common scale of feet and hundredths of a foot; the other has larger divisions, in the proportion of 19 to 16 nearly, or more exactly, as 1·18702 to 1: an aliquot ratio of the scales having been purposely avoided. Both of these are reckoned upwards from a common zero at the bottom of the staff. The centesimal divisions of the foot are produced in strong black lines towards the left, and large figures denoting feet and tenths placed against them, so that the height may be read off at the telescope to the $\frac{1}{100}$th part of a foot at a distance of 150 or 200 yards. These marks are also useful for directing the assistant where to fix the vane, by calling the division to him, especially when the reading was near the top of the staff. A stud of wire, about half an inch long, projects from the bottom of the staff, and a hole is bored to receive it in the top of the peg which is driven into the ground at every station, and on which the staff rests during the observation. A small spirit-cup with a glass cover, screwed to the lower part of the staff, serves to adjust it to a vertical position, in which it is held fast by a clamp attached to three strong legs, jointed and folding together, in the usual manner.

The vane is a small mahogany box, about 3 inches in each dimension, open at the ends to admit the staff, which slides through it. Two large wooden screws at the back of the vane clamp it very firmly to the staff, and preclude all danger of shifting. In front is a frame of brass, about 2 inches square, sliding within an outer frame of brass screwed to the vane, with a range of motion of about half an inch, either upwards or downwards, being moved by a large vertical screw with a milled head working through the lower part of the outer frame. A square aperture, corresponding with the inside of this frame, is cut through the mahogany, in order that the divisions of the staff may be seen. A small ivory door moving on a hinge, is fitted into the sliding frame, on which are drawn two thick black lines, crossing each other at a small angle, and a black ring with a white circular spot within, at the centre, or intersection. Attached to the inside of the sliding frame, and exactly behind the centre of the white circle, is a vernier, nearly in contact with the face of the staff, which divides the hundredth of a foot into five parts, of 20 ten-thousandths each, so that the observation is read off and recorded to four decimal places. The white circular spot, and the angular spaces between the lines, may be bisected by the horizontal wire of the telescope, with great exactness. In favourable weather, I have usually found the average error, or the difference of a single reading from the mean of the number taken to be about $\frac{1}{250}$th of an inch, on a distance of 88 yards, or about a quarter of a second of angle. (See Wood-cut at the end of this paper.)

When the vane was raised so near the top of the staff as to be out of the reach of the hand, the adjusting screw was worked by a long fork of stout wire thrust into holes made in the milled head to receive it. A groove made in the upper part of the staff receives the fork when it is not in use.

In leveling, I proceeded regularly in the following manner. Two equal distances, usually of 4 chains or 88 yards each, having been measured forwards from the last station, the level was placed at the end of the first distance, and, at the second, a strong wooden peg driven firmly into the ground, for the fore station, the level being exactly midway between the stations. When, (as happened in a very few instances,) I was prevented from making the fore and back distances equal, compensating unequal distances were immediately afterwards taken, so that the sums of the two sets of distances were kept equal throughout. The staff being held vertically on the back station peg, by the means before described, and the first observation taken, the height was read off and written down by the assistant in a rough minute-book which he carried for the purpose. The vane was then purposely thrown out, by turning back the screw, the level re-adjusted, and a second reading taken. If these readings agreed within 20 10.000ths (about $\frac{1}{40}$th of an inch), the staff was brought forward to me, when I read off and inserted the last reading, according to both scales, in separate columns of my book; the mean of both readings was also inserted in a third column, after my assistant and myself had called over and compared the last reading. The assistant then read off and called to me the last reading from the large scale, as a check on what I had entered in my book. The needle bearing and distance in links, being also inserted in their respective columns, completed the back observation. The process in taking the fore observation was the same, except that instead of having the staff brought to me to be read, I had then to carry forward my level to the staff.

A rigid adherence to this system rendered it improbable that a wrong reading could be written down, without immediate detection:—in fact, such an instance does not appear to have occurred. Had it even been so, a discrepancy must have existed between the columns of different scales, which would have been readily detected on casting up and comparing the totals, at the end of the day. From erroneous readings, therefore, it is evident, there was little or nothing to fear; but these are far from being the only, or the principal sources of error. On one or two occasions, we were very near committing a mistake, in beginning at a different station from the one on which we had previously closed. This would have occasioned an error, perhaps of large amount, which could only have been detected by the second and independent series of levels, taken over the ground in an opposite direction. For this reason alone, I should not consider it safe to depend on one course of levels only, whatever may have been the precautions used to guard against error.

The total length of my line of leveling between Portishead and Axmouth, besides the branch lines to Bristol and East Quantockshead, is about 74 miles. This distance was divided into separate stages; each of which, averaging about 10 miles in length, was twice leveled over, first in one direction, and then in the opposite, before the next stage was commenced. It is very remarkable, that with a few partial exceptions, the heights of all the points touched upon by both series, came out *less* by the levels *returning*, than by the levels *going*: so that the first station, or starting-point, always appeared lower when I returned, than it was at my setting out. But as the height of this point is the same in both cases, the error must, of course, be thrown on the distant point, or station at which the returning levels commenced, which reverses the first apparent differences, and makes all the heights in the second series progressively greater than those in the first, the most distant point having the greatest error. The following table gives the differences thus found at 20 points along the line between Portishead and Axmouth, the height, in every instance, coming out greater from the series of levels returning towards Portishead.

No. of Station in Minute-book.	Miles from Portishead.	Height greater by 2nd than 1st Levels. Feet.
1683	0	0·0000
1631	3	0·0633
1593	6	0·1557
1562	9	0·2703

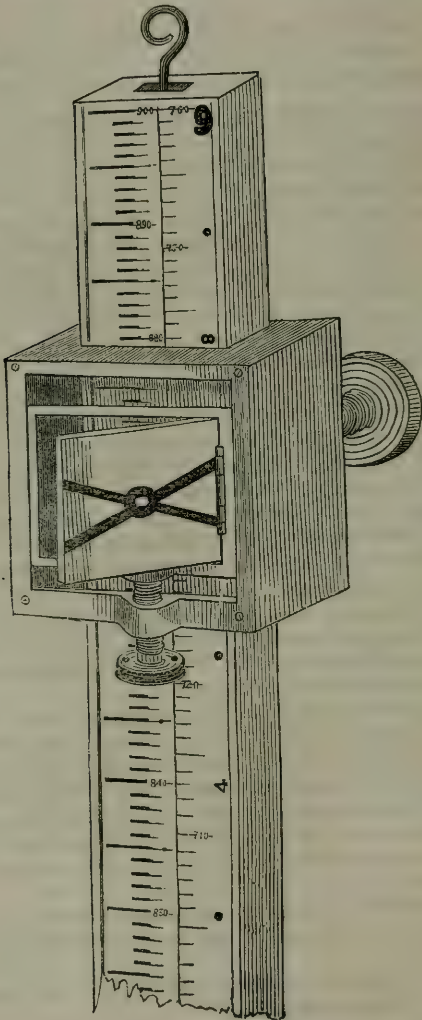
No. of Station in Minute book.	Miles from Portishead.	Height greater by 2nd than 1st Levels. Feet.
1527	12	0·3501
1278	15	0·3796
1229	18	0·4591
1178	23	0·5339
1128	27	0·5734
759	30	0·6352
1	33	0·6888
45	37	0·6956
63	39	0·7170
114	43	0·7532
177	49	0·8237
210	52	0·8622
246	56	0·9021
248	59	0·9208
402	63	0·9373
462	68	0·9714
656	74	1·0294

After the most careful examination of every circumstance which could possibly tend to occasion these curious differences, I am inclined to believe that they arise principally from rapid variations in the amount of atmospheric refraction which occur during the time that elapses in a single observation, and that the progression of the error is in some way or other connected with the progressive changes of the average temperature during the course of the day, from about eight in the morning till six or seven in the evening,—the usual limits of my working hours. These variations in the refraction are much greater and more sudden in summer than in winter, especially during the forenoon of a hot and sultry day, when there are frequent alternations of cloud and sunshine, and copious exhalations of moisture from the ground. On such occasions I have sometimes known the sudden clearing away of a cloud from the sun followed almost in an instant, by a change in the apparent height of the vane amounting to $\frac{1}{10}$th of an inch, or more, on a distance of only 88 yards. At other times the change has been more gradual, so that several successive readings, taken at intervals of two or three minutes, have all either increased or diminished progressively. Different seasons or states of the weather may therefore fully account for the more rapid increase of these differences at certain times than at others, such as the above table presents, in which the errors are found proportionably greater between Portishead and Bridgewater, than between Bridgewater and Axmouth;

the latter distance having been levelled over in the summer of 1837, and the former in that of 1838. For the same reason it appears much better to divide the distance into stages and finish them one at a time, than to go over the whole in one direction, before returning upon any part of it; it being much more probable that errors depending on the state of the atmosphere will balance each other in the former than in the latter case.

My own experience, therefore, leads to the conclusion, that no levelling can be expected to give a correct result, unless it be performed in opposite directions, and the mean of both results be taken; instead of depending, as Captain Lloyd appears to have done, on the consistency of separate sets of successive readings. I have myself invariably found (as that gentleman also did,) the agreement of these to be almost identical, both in the going and in the returning series, notwithstanding the great progressive difference of these two series of levels from each other; of which progression not the smallest trace is discoverable in the separate columns of the same series. I have entered the more minutely into this subject, because I am not aware that any one has described, or even noticed the existence of such differences before; and should feel much interest in reading the statements of any experienced person who had been engaged in a similar undertaking, and had conducted it with sufficient care to render the *law* of the errors in any degree discernible.

SKETCH OF THE NEW LEVELLING STAFF AND VANE.



Report on the Discussions of Tides, prepared under the direction of the Rev. W. Whewell, F.R.S., *by means of the grant of money made for that purpose by the Association.*

The grant of money made at the last meeting of the Association, has enabled me to continue the discussions of tide observations which I had already carried on for some time, and to obtain some results which I hope will be considered as valuable. I engaged Mr. Bunt to proceed with those discussions, according to methods which I had previously framed, and instructed him in the execution of; and at present, his skill in the application of these methods being improved by practice, and stimulated by a great zeal and love for the subject, I believe his work (which I produce to the meeting,) will be found of extraordinary accuracy and clearness. I am fully persuaded that in consequence of the advantage of the plan pursued, and of the excellent manner in which Mr. Bunt has executed it, the exactness of the results is of a most unexpected kind: for example, it is quite clear that the tables for semimenstrual inequality and for lunar parallax, (if not for declination,) obtained by our methods from a year's observations, are as good as those previously obtained from the discussion of nineteen years' observations. And the proof of this is found, not only in the regularity which the curves expressing the corrections exhibit without any arbitrary improvement whatever, but also in the complete symmetry of the curves above and below the mean; the parallax correction curves for 60′ and for 54′ (3′ above and below the mean 57′) are of exactly the same form.

I have given an account of the results of these discussions in a memoir read before the Royal Society, and printed in their *Transactions*, entitled "On the Determination of the Laws of the Tides from short series of Observations," being the ninth series of my tide researches. An account is also there given of the method pursued by Mr. Bunt in these discussions. I may mention here the questions of which I have in that paper attempted the solution.

1. To which transit of the moon ought we to refer the tide?

2. How does a change of the epoch affect the semimenstrual inequalities?

3. How does a change of the epoch affect the (lunar) parallax correction of the times?

4. How does a change of the epoch affect the (lunar) declination correction of the times?

5. How does a change of the epoch affect the parallax correction of the heights?

6. How does a change of the epoch affect the declination correction of the heights?

7. Does the parallax correction of the heights vary as the parallax?

8. Does the parallax correction of the times vary as the parallax?

9. Does the declination correction of the heights vary as the square of the declination?

10. Does the declination correction of the times vary as the square of the declination?

11. Can the laws of the corrections be deduced from a single year?

12. Are there any regular differences between the corrections of successive years?

13. Do the corrections of different places agree in laws and amount?

The *epoch* here spoken of is that transit of the moon, anterior to the tide, and to which the tide is referred. The question examined is, whether we obtain the closest accordance with the observations by taking a transit one day, one and a half day, or two days anterior to the tide which we consider.

Although I have given the answers to these questions in the memoir in the *Philosophical Transactions* already referred to, I here lay before the Association the curves*, the comparison of which exhibits these answers, and exhibits indeed the result of my discussions more clearly and exactly than words can do.

The careful examination to which we have subjected the Bristol tides, has shown us that there are scarcely any irregularities in these phænomena which we have not reduced, or may not hope to reduce, to empirical laws, which laws constitute the first step to the solution of our great tidological problem, the explanation of the phænomena on hydrodynamical principles. I may add that the Report on Waves by Sir John Robison and Mr. Russell, included in the reports of the seventh meeting of the Association, contains highly valuable materials, likely to assist us in the further prosecution of this subject. The unexplained residue, which, in our method of discussion, exhibits the difference between observation and our tables as hitherto corrected, although it is small (upon the average two or three minutes in time, and as many inches in height in a tide of forty feet), is so far seemingly subject to some rule as to offer a promise of additional laws of correction, and I should be desirous of discussing this residual quantity with such an object.

* These curves are given in Plates 3, 4, 5, 6, 7, 8.

Account of the Progress and State of the Meteorological Observations at Plymouth, made at the request of the British Association, under the direction of Mr. W. Snow Harris, F.R.S. (*Drawn up by Mr. Harris.*)

The Meteorological Instruments, now in operation, are as follow:

1. A Wind Gauge invented by the Rev. W. Whewell.
2. A Wind Gauge invented by Mr. Osler, of Birmingham.
3. The Barometer.
4. The Wet-Bulb Thermometer.
5. The common Thermometer.

Professor Whewell's instrument has been carefully attended to by Mr. Southwood, of Devonport. The results of the register accompany this communication. In consequence of Mr. Southwood's removal from Devonport, the instrument, together with the wood work employed in its erection on his house, have been preserved: it will be again set up as soon as possible.

Ten pounds, voted to defray the expense incurred in the erection, repair, &c., of this instrument, since its employment after the Meeting at Bristol, have been paid to Mr. Southwood.

The Wind Gauge lately invented by Mr. Osler, and exhibited to the Physical Section at the last Meeting at Liverpool, has at length been set up in a very excellent situation, at the house of Mr. Cox, Optician, Devonport. I am sorry that many unavoidable delays in the manufacture, &c. &c. of this machine have interfered so much with its final completion, that I am unable to send any well digested result of its action. It is, however, now at work, and the Association will, I have little doubt, be amply rewarded for the trouble and expense incurred on account of it.

Forty pounds was voted for this instrument; of this 30*l.* has been paid to Mr. Osler. The attendant expenses on it have amounted to 20*l.* This includes the erection of an apartment of wood in which the instrument works, carriage from Birmingham, clock for the register, and sundry other expenses of a minor kind.

As the daily register must be carefully attended to it will be necessary to provide some slight remuneration for the person employed for this purpose. I should therefore feel obliged if the Committee would recommend the sum of 10*l.* for the general current expenses of the next year, should they so think fit. The machine appears an extremely valuable one, and when its register is taken in connexion with that of the barometer and the tides, &c., will I have no doubt afford very valuable information, since it registers the force and direction of wind, with the amount of rain for every instant in twenty-four hours.

The observations with the barometer are complete up to June last, all the observations having been reduced. I have not, however, been enabled to arrange in Tables more than those of the year ending January 1, 1838. These observations being for one year only, I have thought it undesirable to write any detailed report of them. I may, however, be permitted to lay before the Section, as an approximative result, the march of the atmospheric pressure through one mean day, as shown in Table A, Plate 9, and deduced from 8760 observations; from which some idea may be formed of the probable horary oscillation in this place, a subject of singular interest in meteorology. It appears by the result of the hourly observations for the year 1837, that the horary oscillation amounts to 0·0144 of an inch.

The hours of max. being 11 A.M. and 9 P.M.

The hours of min. being 5 A.M. and 3 P.M.

The line of mean pressure appears to be crossed 4 times in the 24 hours, viz. between 2 and 3 A.M., and between 7 and 8 A.M.; between 12 and 1 P.M., and between 6 and 7 P.M.

The deviations being $\left.\begin{array}{l}\cdot 0065 -\\ \cdot 0035 +\end{array}\right\}$ for the max. and min. A.M.

and $\left.\begin{array}{l}\cdot 0092 -\\ \cdot 0096 +\end{array}\right\}$ for the max. and min. P.M.

The neg. sign indicates the depression below the line of mean pressure, the pos. sign the elevation above it.

The mean pressure by these observations, at 60 feet above the level of the sea, and at a temperature of 55° of Fahrenheit, is 29·9532*.

On the 1st of January, 1839, we shall have completed 2 years of these hourly observations, when general results, entitled to more confidence than those deduced from a single year, will probably be arrived at. It seems therefore desirable, in order to avoid too hasty generalization, not to enter further at present into this question. I avoid for a similar reason any further notice of the register of the hygrometric thermometer, the observations being in a state of progress only.

The register of the ordinary thermometer, first contemplated by the Association at York in 1831, is, I am happy to say, complete for 5 years, and the observations are now reduced up to January last.

* A general type of the daily march of the barometer is given in Table A, Plate 9.

The general results, which accompany this communication, and which are exhibited in Plates 10, 11, 12, must be considered merely as corrections of similar statements exhibited in my first report; the former being arrived at by a more extensive series of observations. It will be seen by an examination of Table III., Plate 12, that the approximations in calculating the hourly temperatures, on the supposition that they may be represented by parabolic abscissæ, are much nearer than in the similar table and plate before given.

Of £50 voted for these observations, £35 has been spent in defraying the expenses attendant on them up to June last, leaving a balance of £15; part of this has been expended in aid of Mr. Osler's wind-gauge.

The labour attendant on the reduction and discussion of the observations made hourly with these last-named instruments being now very considerable, it becomes necessary to employ competent persons to assist in working out the ordinary arithmetical operations, &c. I would therefore suggest to the Committee the propriety of recommending a sum not exceeding £40 for this and other attendant expenses until the next meeting of the Association, when I hope to have the pleasure of submitting to the Section a full report of the results obtained from the respective registers.

W. Snow Harris.

32, Union Street, Plymouth,
August 20, 1838.

Table I.

TABLE I. Showing the Mean Temperature of each Hour for each Month of the Years 1833, 1834, 1835, 1836, 1837, and for the whole Year.

See Plate 10. Mean Temp. Line.

Hour.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Means.
1	43·795	43·493	43·257	44·366	49·504	54·524	57·267	57·315	54·414	50·087	46·542	44·336	49·075
2	43·663	43·258	42·870	44·127	49·063	54·174	56·880	56·891	54·019	49·796	46·354	44·327	48·785
3	43·537	42·856	42·597	43·827	48·629	53·903	56·508	56·533	53·849	49·622	46·307	44·226	48·533
4	43·429	42·772	42·397	43·610	48·473	53·799	56·441	56·238	53·745	49·476	46·231	44·079	48·391
5	43·284	42·584	42·271	43·616	48·685	54·330	56·855	56·136	53·552	49·330	46·149	43·917	48·392
6	43·185	42·467	42·489	44·149	50·200	56·060	58·466	57·195	54·118	49·524	46·082	43·885	48·985
7	43·268	42·577	43·134	46·063	52·999	58·181	60·796	59·277	55·120	49·885	46·230	43·806	50·110
8	43·465	42·996	44·524	48·364	55·715	60·080	62·996	62·065	57·233	51·075	46·771	44·229	51·626
9	44·051	44·246	46·429	50·905	57·894	61·574	64·567	64·320	59·641	53·449	48·019	44·574	53·306
10	45·125	45·752	48·053	52·388	59·350	62·473	65·888	65·971	61·389	55·735	49·415	45·810	54·779
11	46·429	47·322	49·010	53·610	60·382	63·306	66·842	66·880	62·674	57·235	50·599	46·867	55·929
12	47·042	48·265	49·876	54·747	60·933	63·939	67·538	67·491	63·319	58·002	51·404	47·784	56·695
1	47·203	48·619	50·326	54·949	61·329	61·129	67·749	67·560	63·594	58·167	51·669	47·843	56·928
2	46·940	48·469	50·005	54·455	61·463	61·940	68·341	67·590	63·269	57·168	51·323	47·538	56·708
3	46·450	48·053	49·381	53·838	61·052	63·170	67·228	67·056	62·508	56·701	50·631	46·879	56·079
4	46·102	47·039	48·524	52·799	60·435	62·889	66·422	66·269	61·452	55·390	49·729	46·109	55·263
5	45·107	45·880	47·317	51·422	59·237	61·854	65·371	65·098	59·964	54·059	48·837	45·387	54·128
6	44·717	45·150	46·311	49·808	57·600	60·698	64·065	63·681	58·556	53·096	48·425	45·108	53·101
7	44·359	44·830	45·292	48·399	55·604	59·30	62·519	62·119	57·277	52·312	48·123	44·879	52·093
8	44·037	44·549	44·808	47·266	53·939	57·723	60·969	60·644	56·426	51·913	47·885	44·675	51·236
9	43·928	44·283	44·334	46·466	52·637	56·71	59·636	59·528	55·727	51·511	47·583	44·469	50·567
10	43·903	44·031	43·881	45·764	51·681	55·910	59·088	58·788	55·355	50·901	47·366	44·317	50·082
11	43·822	43·860	43·795	45·144	51·008	55·436	58·223	58·684	55·008	50·530	47·109	44·287	49·742
12	43·859	43·791	43·459	44·675	50·363	54·994	57·598	57·607	54·711	50·239	46·910	44·059	49·357
Means	44·612	44·830	45·597	48·531	54·924	58·879	62·010	61·803	57·788	52·717	48·154	45·141	52·078

52·081

Mean temperature of 1833, 1834, 1835, 1836, 1837, by this Table, from 43,824 observations.

See Plate 11.

TABLE II. Showing the Mean Hourly Temperature for each of the Seasons, viz. Spring, Summer, Autumn, and Winter, for the Years 1833, 1834, 1835, 1836, 1837.

Hour.	Spring.	Summer.	Autumn.	Winter.
1	45·709	56·369	50·348	43·875
2	45·353	55·982	50·056	43·749
3	45·018	55·648	49·926	43·539
4	44·827	55·493	49·817	49·427
5	44·857	55·774	49·677	43·262
6	45·613	57·240	49·908	43·179
7	47·399	59·418	50·428	43·217
8	49·534	61·714	51·693	43·563
9	51·743	63·487	53·703	44·290
10	53·264	64·777	55·513	45·562
11	54·334	65·676	56·836	46·873
12	55·185	66·323	57·575	47·697
1	55·535	66·479	57·810	47·888
2	55·308	66·624	57·253	47·649
3	54·757	65·818	56·613	47·127
4	53·919	65·193	55·524	46·417
5	52·659	64·108	54·287	15·458
6	51·239	62·815	53·359	44·992
7	49·765	61·313	52·571	44·689
8	48·671	59·779	52·075	44·420
9	47·812	58·625	51·607	44·227
10	47·109	57·929	51·207	44·084
11	46·649	57·448	50·882	43·989
12	46·166	56·773	50·620	43·903
Means	49·684	60·867	52·912	44·878

Mean 52·085.

See Plate 12.

TABLE III. Showing the Mean Annual Hourly Temperatures for 1833, 1834, 1835, 1836, 1837 at Plymouth, as observed and calculated on the supposition that they may be represented by Parabolic Abscissæ.

	Hours.	Obs. Temp.	Cal. Temp.	Diff.
Morn. Branch A B.	4 30	48·391 m	48·391	0·000
	5	48·391	48·456	− ·065
	6	48·985	48·976	+ ·009
	7	50·110	50·015	+ ·095
	8	51·626	51·559	+ ·067
	8 16	μ 52·078	52·078	·000
Noon Branch B D.	9	53·306	53·468	− ·162
	10	54·779	54·982	− ·203
	11	55·929	56·062	− ·133
	12 P.M.	56·695	56·712	− ·017
	1	M 56·928	56·928	·000
Aft. Branch D E.	2	56·708	56·794	− ·086
	3	56·079	56·369	− ·290
	4	55·263	55·716	− ·453
	5	54·128	54·773	− ·645
	6	53·101	53·560	− ·459
	7	μ 52·093	52·093	·000
Night Branch E A.	8	51·236	51·342	− ·106
	9	50·567	50·689	− ·112
	10	50·082	50·117	− ·035
	11	49·742	49·627	+ ·115
	12	49·355	49·218	+ ·137
	1 A.M.	49·075	48·891	+ ·184
	2	48·785	48·646	+ ·139
	3	48·533	48·483	+ ·050
	4	m 48·391	48·401	− ·010

TABLE IV.

See Plate 10.

Hour.	Summer Months.	Winter Months.	Hour.	Summer Months.	Winter Months.
1	52·898	45·251	1	63·218	50·638
2	52·526	45·044	2	63·176	50·240
3	52·208	44·857	3	62·475	49·684
4	52·051	44·730	4	61·711	48·815
5	52·196	44·589	5	60·491	47·764
6	53·364	44·607	6	59·068	47·134
7	55·406	44·815	7	57·536	46·632
8	57·742	45·510	8	56·161	46·311
9	59·817	46·794	9	55·117	46·018
10	61·243	48·315	10	54·431	45·733
11	62·282	49·577	11	53·917	45·567
12 P.M.	62·994	50·395	12	53·324	45·386
			Means	57·306	46·850

Mean 52·078.

TABLE A, Plate 9. Showing the Mean Pressure of each Hour for the Year 1837.

Hour.	Pressure.	Hour.	Pressure.
1 A.M.	29·9558	1 P.M.	29·9492
2 ...	29·9556	2 ...	29·9467
3 ...	29·9492	3 ...	29·9440
4 ...	29·9474	4 ...	29·9442
5 ...	29·9467	5 ...	29·9463
6 ...	29·9497	6 ...	29·9484
7 ...	29·9519	7 ...	29·9547
8 ...	29·9555	8 ...	29·9596
9 ...	29·9572	9 ...	29·9627
10 ...	29·9567	10 ...	29·9628
11 ...	29·9580	11 ...	29·9621
12 ...	29·9545	12 ...	29·9590
		Mean	29·9532

Tables, &c., of Observations made with Professor WHEWELL'S *Anemometer at Mount Wise, Devonport. From November* 1837 *to June* 1838, *inclusive.*

These Tables comprise:—1. The observations as daily recorded. 2. The reduction of these observations. 3. A summary Table, in which the general result is condensed. Lastly, The charts and general type of the wind, as shown by the respective reductions and summary.

Plate 13, contains the general summary: the month of November 1837 is coloured blue, and marked 11; December is red, and marked 12; January 1838 is again blue, and marked 1. The remaining months continue to be marked 2, 3, &c. The breaks in the continuation of the lines show when the instrument was under repair. The direction of the wind is here only recorded and indicated by dotted lines.

The *black* dotted lines show the resultant magnitude and direction for each month; the five black lines are continued resultants, viz., that marked 1, 2, is the resultant of 11 and 12, that marked 1, 2, 3, of 11 and 12, and so on to the last marked 1 to 8; which is the resultant of 8 months. The scale of this Plate is that of the 400 equal parts to the inch.

Indications of Whewell's Anemometer at Mount Wise, Devonport, 1837, 1838.

NOVEMBER, 1837.

4	W. N.W. 12 3	15	18	E. 4	4
5	N.W. W.S.W. 11 5	16	19	W.S.W. 14	14
6	W.N.W. 3	3	20	W.S.W. 25	25
7	W.N.W. 0	0	21	W.S.W. W. 15 13	28
8	S.S.E. S. 11 26	37	22	W.S.W. S.W. 10 28	38
9	S. 42	42	23	S.W. S.S.W. 21 33	54
10	S.S.W. W.S.W. 2 10	12	24	S.W. Under repair.	
11	S.W. W. 9 6	15	25	W. N.W. Under repair.	
12	W. S.W. 5 5	10	26	S.W. 0	0
13	W. 2	2	27	W.S.W. N.W. 33 5	38
14	W.S.W. 15	15	28	W.S.W. 8	8
15	W. N.N.W. N.N.E. 9 9 12	30	29	W.N.W. N.N.W. 5 7	12
16	N.N.E. 3	3	30	S.W. 12	12
17	E. 11	11			

December, 1837.

1	W.S.W. 3 S.W. 24	27	16	Under repair.	
2	S.W. 0	0	17	Do.	
3	E.S.E. 0	0	18	Do.	
4	E. 4	4	19	Do.	
5	E. 15	15	20	Do.	
6	E.N.E. 6	6	21	Under repair.	
7	N.E. 5	5			
8	N.E. 0	0			
9	N.W. 2	2			
10	N.E. 0	0			
11	N.E. 2	2			
12	N.E. 0	0			
13	E. 0	0			
14	S.E. 0	0			
15	S. 65	65	31	S. 88	88

JANUARY, 1838.

1	S. 112	112	17	E. 2	2
2	S. 59	59	18	E. 10	10
3	S. S.W. 180 40	220	19	E. 20	20
4	W.S.W. 21	21	20	N.E. 5	5
5	W. N.W. 0 0	0	21	E.N.E. 5	5
6	N. 0	0	22	S.E. 18	18
7	E. 0	0	23	E.S.E. S.E. E.S.E. 96 33 105	234
8	E. 12	12	24	S.E. Under repair.	
9	E. E.N.E. E. 31 7 18	56	25	E. "	
10	E. 1	1	26	E. "	
11	N.E. 1	1	27	S.E. "	
12	N.E. 2	2	28	S.E. "	
13	E. 0	0	29	E. "	
14	E. 5	5	30	E. "	
15	E. 12	12	31	E. "	
16	E. 2	2			

FEBRUARY, 1838.

1	E. Under repair.		15	E.S.E. 240	240
2	N.E. "		16	E.S.E. 210	210
3	N.E. "		17	E.S.E. S.W. 103 19	122
4	N.E. "		18	W.N.W. 22	22
5	E. "		19	E.S.E. 9	9
6	S.E. S. " "		20	S.E. 75	75
7	S. S.W. " "		21	S.E. 34	34
8	S.W. "		22	S.E. 25	25
9	W. "		23	S.E. 83	83
10	W. Repaired and replaced.		24	S.E. 83	83
11	E. 49	49	25	S.E. W.S.W. S. 40 65 45	150
12	E.N.E. N. 25 17	42	26	S. S.E. 63 26	89
13	E. 25	25	27	S.E. 65	65
14	E.N.E. E.S.E. 14 49	63	28	S.E. 5	5

MARCH, 1838.

1	S.S.E. 8 S. 15	23	17	W.N.W. 33	33
2	S.E. 5	5	18	W.N.W. 22	22
3	S.E. 5	5	19	W.N.W. 5	5
4	S.E. 30 W.S.W. 5	35	20	W. 53	53
5	W.S.W. 18 N.W. 6	24	21	W. 23	23
6	N.W. 22 W. 31	53	22	W. 7 N.N.W. 5	12
7	W.S.W. 14	14	23	N.N.W. 16	16
8	N.W. 14	14	24	N.N.W. 3	3
9	W.N.W. 6	6	25	N.N.W. 5 W.N.W. 16	21
10	S. 29	29	26	W.N.W. 2 E.S.E. 5	7
11	S. 10 S.E. 21	31	27	E. 5	5
12	E.S.E. 2	2	28	E. 0	0
13	W.S.W. 23	23	29	S.E. 0	0
14	W.S.W. 10	10	30	S.E. 3	3
15	W. 15	15	31	N.W. 0	0
16	W.N.W. 5	5			

April, 1838.

1	E.S.E. 22	22	16	W.N.W. 18	18
2	E.S.E. 5	5	17	W.N.W. Under repair.	
3	N. 8	8	18	N.W. „	
4	N.W. 0	0	19	N.W. „	
5	W.N.W. 0	0	20	N. „	
6	W.N.W. W.S.W. 5 15	20	21	N. Under repair.	
7	W.S.W. 33	33	22	N. „	
8	W.S.W. 22	22	23	N.E. „	
9	W.S.W. 8	8	24	N.E. „	
10	S. 6	6	25	N.N.E. Replaced.	
11	S.S.W. 3	3	26	N.N.E. N.E. N.N.E. 6 10 2	18
12	N. 3	3	27	N.N.E. E.N.E. 8 4	12
13	S. 5	5	28	N.E. E. E.N.E. 3 3 9	15
14	W.S.W. 16	16	29	N.N.E. N. N.N.W. 27 8 5	40
15	N.W. 6	6	30	W.S.W. 40	40

May, 1838.

1	W.S.W. 10 W. 45	55	17	E.S.E. 7	7
2	S.W. 18 S.S.W. 43	61	18	E.S.E. 3 W. 0 S. 0	3
3	S.W. 69	69	19	S.S.W. 25 S. 40	65
4	S.S.W. 15 W. 28	43	20	S.E. 30 S.S.W. 46	76
5	N.E. 78	78	21	S.S.W. 1 S. 19 W. 0 N.W. 0	20
6	W.S.W. 21 S.S.W. 13 E.S.E. 2	36	22	S. 1 S.W. 2 N.N.W. 1	4
7	E.S.E. 6	6	23	W. 2 W.N.W. 15	17
8	E.S.E. 28	28	24	N.N.W. 6	6
9	S.E. 81	81	25	W.N.W. 2 E. 3 S.E. 5	10
10	S.E. 51	51	26	S.E. 26	26
11	S.E. 110	110	27	S.E. 87	87
12	S.E. 19 W.S.W. 2	21	28	E. 55 E.N.E. 111	166
13	E.N.E. 41	41	29	E. 6 S.S.E. 8 S. 4 S.S.W. 15	33
14	N.N.E. 30	30	30	S.W. 31	31
15	N.N.E. 8 S.E. 39	47	31	S.W. 0	0
16	E.S.E. 21	21			

June, 1838.

1	S.W. 0	0	15	S. 39	39
2	S. 4 S.W. 15	19	16	S. 14	14
3	S.W. 20	20	17	S. 12 S.E. 3 S. 4	19
4	S.W. 7 W.S.W. 14 S. 16	37	18	S. 4 S.E. 4	8
5	S.W. 9 E.S.E. 0	9	19	S. 21 W. 19	40
6	S.W. 5 N. 10	15	20	S.W. 56	56
7	N. 18	18	21	S.W. 51 W. 10 S.W. 15 W. 1	77
8	N.N.W. 10	10	22	W.S.W. 40 S.W. 13	53
9	N.N.W. 1 S.W. 2	3	23	W.S.W. 29	29
10	S. 35	35	24	E.S.E. 4	4
11	S. 9 S.W. 21 N. 0	30	25	E.S.E. 0 S. 0 N. 0	0
12	N.W. 5	5	26	This day I left my house and removed the anemometer to Mr. Cox's, Fore Street.	
13	S.S.W. 3	3	27		
14	S. 8	8	28		

J. A. Southwood.

The following calculations with the accompanying Plates represent the result of observations made at Plymouth, with an Anemometer of Mr. Whewell's construction. This instrument gives what Mr. Whewell calls the *integral effect* of the wind, namely, a space proportional to that which a particle of air would pass over in each day in consequence of the wind, taking into account both the strength of the wind and the time during which it blows. These integral effects being put together according to their directions, each day beginning at the end of the preceding so as to form a continuous line, as is done in the Plates, we obtain the path of the wind for each month, or for a longer time. The annual path of the wind at each place will have, it may be expected, a general similarity in different years; and the mean form to which the annual path thus approximates is called the *type* of the wind for each place.

A description of the Anemometer, of the mode of using it, and of the process of reducing the observations is given in the Transactions of the Cambridge Philosophical Society for 1837; vol. vi. Part II.

Reduction of preceding Observations.

NOVEMBER, 1837.

	N.	N.N.E.	N.E.	E.N.E.	E.	E.S.E.	S.E.	S.S.E.	S.	S.S.W.	S.W.	W.S.W.	W.	W.N.W.	N.W.	N.N.W.	N.
Nov. 4th. 6th.												5 ...	12 ...	3 ...	3 11	...	
W./12 W.N.W./3												5	12	3	14		
								N.	2 11	W.S.W.		W.S.W.	4 2 10	W.N.W.		N.W. W.N.W.	10 1
								N.	9			W.	28	N.W.		N.	11
Nov. 8th. 15th.								11	26	2	9	10	6				
S.S.E./10 W./9								...	42	...	5	15	5				
								...	...	...	...	...	2				
								...	...	...	...	...	9				
								11	68	2	14	25	22				
				W.	4 55	S.S.E.		S.S.E.	10 1 10 10	S.S.W. S.W. W.S.W.		W.S.W. S.W. S.S.W.	23 10 0				
				W.	51				99				55				
Nov. 15th. 18th.		12 3			11 4											9 ...	
N.N.W./9 E./4		15	...	...	15											9	
	14 8	N.N.E. N.N.W		N.N.E.	6											N.N.W.	8
	22	N.		E.	21												

	N.	N.N.E.	N.E.	E.N.E.	E.	E.S.E.	S.E.	S.S.E.	S.	S.S.W.	S.W.	W.S.W.	W.	W.N.W.	N.W.	N.N.W.	N.
Nov. 19th. 23rd.										33	28	14	13				
W.S.W. / 4 S.S.W. / 33										...	21	25	...				
										...	...	15	...				
										...	...	10	...				
										33	49	64	13				
									30	S.S.W.		W.S.W.	59				
									34	S.W.		S.W.	34				
									25	W.S.W.		S.S.W.	13				
								S.	89			W.	119				
Nov. 27th. Dec. 1st.										12	..	33		5	5	7	
W.S.W. / 33 S.W. / 24										24	...	8		...	...	...	
										...	...	3		...	..	...	
									...	36	...	44	...	5	5	7	...
									33	S.S.W.		W.S.W.	40			N.N.W.	6
									17	W.S.W.			4	W.N.W.		N.W.	3
													3	N.W.		W.N.W.	2
													3	N.N.W.			
								S.	50							N.	11
								N.	11				50				
								S.	30								

	N.	N.N.E.	N.E.	E.N.E.	E.	E.S.E.	S.E.	S.S.E.	S.	S.S.W.	S.W.	W.S.W.	W.	W.N.W.	N.W.	N.N.W.	N.
Dec. 4th. 11th.			5	6	4										2		
			2	...	15										...		
E./4 N.E./2			7	6	19										2		
	5	N.E.		E.N.E.	5								1	N.W.		N.W.	1
	2	E.N.E.		N.E.	5												
	7	N.		E.	29												
	1	N.		W.	1												
	8	N.		E.	28												
Dec. 15th. S./65									65		40	21					
									88		...	...					
and									112		...	...					
									59		...	...					
									180		...	...					
Dec. 31st. Jan. 4th.									504	...	40	21					
S./88 W.S.W./21									28	S.W.		W.S.W.	19				
									8	W.S.W.		S.W.	28				
								S.	540			W.	47				

	N.	N.N.E.	N.E.	E.N.E.	E.	E.S.E.	S.E.	S.S.E.	S.	S.S.W.	S.W.	W.S.W.	W.	W.N.W.	N.W.	N.N.W.	N.
Jan. 8th. 23rd.			1	7	12	96	18										
			2	5	31	105	33										
E. E.S.E.			5	...	18	...	...										
12 105			...	...	1	...	...										
			...	...	5	...	...										
			...	...	12	...	...										
			...	...	2	...	...										
			...	...	2	...	...										
			...	...	10	...	...										
			...	...	20	...	...										
			8	12	113	201	51										
	5	N.E.		E.N.E.	11			S.E.	35								
	5	E.N.E.		N.E.	5			E.S.E.	80								
					185	E.S.E.											
	10				35	S.E.			115								
								N.	10								
					349												
								S.	105								

	N.	N.N.E.	N.E.	E.N.E.	E.	E.S.E.	S.E.	S.S.E.	S.	S.S.W.	S.W.	W.S.W.	W.	W.N.W.	N.W.	N.N.W.	N.
Feb. 11th. Mar. 4th.				25	49	49	75	8	45		19	65		22			17
				14	25	240	34		63								
E./49 S.E./30						210	25		15								
						103	83										
						9	83										
							40										
							26										
							65										
							5										
							5										
							5										
							30										
				39	74	611	476	8	123		19	65	...	22			17
	16	E.N.E.		E.N.E.	36			E.S.E.	7			W.S.W.	60			W.N.W.	8
					564	E.S.E.		S.E.	333			S.W.	13				
					333	S.E.		S.S.E.	244				20	W.N.W.		N.	35
					3	S.S.E.			13	S.W.						N.	16
									26	W.S.W.			93				
					1010											N.	41
				W.	93				746								
								N.	41								
				E.	917												
								N.	705								
Mar. 4th. 9th.												5	31	6	6		•
W.S.W./5 W.N.W./6												18	...	...	22		
												14	...	...	14		
												37	31	6	42		
									14	W.S.W.		W.S.W.	34			N.W.	29
								N.	31				5	W.N.W.		W.N.W.	2
													29	N.W.			
								N.	17							N.	31
												W.	99				

	N.	N.N.E.	N.E.	E.N.E.	E.	E.S.E.	S.E.	S.S.E.	S.	S.S.W.	S.W.	W.S.W.	W.	W.N.W.	N.W.	N.N.W.	N.
Mar. 10th. 12th. S. / 29 E.S.E. / 2						2	21	...	29 10								
						2	21	...	39								
					1 14	E.S.E. S.E.		S.E. E.S.E.	14 0								
				E.	15			S.	53								
Mar. 13th. 26th. W.S.W. / 23 W.N.W. / 2												23 10	15 53 23 7	5 33 22 5 16 2		5 16 3 5	
												33	98	83		29	
								N.	13 59	W.S.W.		W.S.W.	30 76 11	W.N.W. N.N.W.		N.N.W. W.N.W.	26 33
								N.	46				215			N.	59
Mar. 26th. Ap. 2nd. E.S.E. / 5 E.S.E. / 5					5	5 22 5	3										
					5	32	3										
					30 1	E.S.E. S.E.		S.E. E.S.E.	1 12								
				E.	36			S.	13								

	N.	N.N.E.	N.E.	E.N.E.	E.	E.S.E.	S.E.	S.S.E.	S.	S.S.W.	S.W.	W.S.W.	W.	W.N.W.	N.W.	N.N.W.	N.
April 3rd. 16th.									6	3		15		5	6	...	8
N./8 W.N.W./18									5			33		18	...	...	3
												22					
												8					
												16					
									11	3	...	94	...	23	6	...	11
									2	S.S.W.		W.S.W.	86			N.W.	4
									37	W.S.W.			21	W.N.W.		W.N.W.	9
													4	N.W.			
								S.	50			S.S.W.	1			N.	24
								N.	24								
												W.	112				
								S.	26								
April 26th. 29th.		6	10	4	3											5	8
N.N.E./6 N.N.W./5		2	3	9	...											...	...
		8	...	...	...											...	...
		27	...	...	...											...	...
		43	13	13	3											5	8
													2				
	39	N.N.E.		E.N.E.	12									N.N.W.		N.N.W.	4
	9	N.E.		N.E.	9												
	5	E.N.E.		N.N.E.	17												12
	53			E.	41												
	12			W.	2												
	65	N.		E.	39												

	N.	N.N.E.	N.E.	E.N.E.	E.	E.S.E.	S.E.	S.S.E.	S.	S.S.W.	S.W.	W.S.W.	W.	W.N.W.	N.W.	N.N.W.	N.
April 30th. May 4th.										43	18	40	45				
W.S.W. 40, W. 28										15	69	10	28				
										58	87	50	73				
									53	S.S.W.		W.S.W.	46				
									61	S.W.		S.W.	61				
									20	W.S.W.		S.S.W.	23				
								S.	134			W.	203				
May 5th. 18th.		30	78	41		2	81			13		21					
N.E. 78, E.S.E. 3		8	...	...		6	51			...		2					
						28	110			...		...					
						21	19			...		...					
						7	39			...		...					
						3	...			...		...					
		38	78	41		67	300			13		23					
	35	N.N.E.		E.N.E.	38			S.E.	210			W.S.W.	21				
	54	N.E.		N.E.	54			E.S.E.	26			S.S.W.	5				
	16	E.N.E.		N.N.E.	15				12	S.S.W.							
					61	E.S.E.			9	W.S.W.		W.	26				
	105	N.			210	S.E.						E.	378				
	257	S.						S.	257								
				E.	378							E.	352				
	152	S.															

	N.	N.N.E.	N.E.	E.N.E.	E.	E.S.E.	S.E.	S.S.E.	S.	S.S.W.	S.W.	W.S.W.	W.	W.N.W.	N.W.	N.N.W.	N.
May 19th. 25th.							30	...	40	25	2	...	2	15	...	1	
S.S.W./25 W.N.W./2									19	46	...	...	...	2	...	6	
									1	1	...	...	...	...	...	...	
							30	...	60	72	2	...	2	17	...	7	
					21	S.E.		S.E.	21			S.W.	1			W.N.W.	6
									66	S.S.W.		S.S.W.	28			N.N.W.	6
									1	S.W.			15	W.N.W.			
													3	N.N.W.		N.	12
								S.	148								
								N.	12			W.	49				
												E.	21				
								S.	136								
												W.	28				
May 25th. 29th.				111	3		5	8	4								
E./3 S./4					55		26										
					6		87										
				111	64	...	118	8	4								
	44	E.N.E.		E.N.E.	102			S.S.E.	7								
					82	S.E.		S.E.	82								
					3	S.S.E.											
								S.	93								
				E.	251			N.	44								
								S.	49								

	N.	E.N.E.	N.E.	E.N.E.	E.	E.S.E.	S.E.	S.S.E.	S.	S.S.W.	S.W.	W.S.W.	W.	W.N.W.	N.W.	N.N.W.	N.
May 29th. June 24th.						4	3		4	15	31	14	19		5	10	10
S.S.W. / 15 E.S.E. / 4							4		16	3	15	40	10		...	1	18
									35		20	29	1				
									9		7						
									8		9						
									39		5						
									14		2						
									12		21						
									4		56						
									4		51						
									21		15						
											13						
						4	7	...	166	18	245	83	30	...	5	11	28
					3	E.S.E.		S.E.	5			W.S.W.	76			N.N.W.	10
					5	S.E.		E.S.E.	1			S.W.	171			N.W.	3
									16	S.S.W.		S.S.W.	7				
					8				171	S.W.			3	N.W.			41
									33	W.S.W.			4	N.N.W.			
								S.	392			W.	291				
								N.	41			E.	8				
								S.	351			W.	283				

SUMMARY TABLE.

	N.	S.	E.	W.
1837. Nov. 4 to Nov. 7, 1837.	9	...	...	25
8 „ 15.	...	99	...	51
15 „ 18.	22	...	21	...
19 „ 23.	...	89	...	119
Nov. 27 „ Dec. 1.	...	39	...	50
Dec. 4 „ 11.	8	...	28	...
15.	...	65	...	...
31 „ Jan. 4, 1838.	...	540	...	47
1838. Jan. 8 „ 23.	...	105	349	...
Feb. 11 „ Mar. 4.	...	705	917	...
Mar. 4 „ 9.	17	...	...	99
10 „ 12.	...	53	15	...
13 „ 26.	46	...	...	215
26 „ April 2.	...	13	36	...
April 3 „ 16.	...	26	...	112
26 „ 29.	65	...	39	...
30 „ May 4.	...	134	...	203
May 5 „ 18.	...	152	352	...
19 „ 25.	...	136	...	28
25 „ 29.	...	49	251	...
29 „ June 24.	...	351	...	283
	167	2558	2008	1232
	South 2391.		East 776.	

A Memoir on the Magnetic Isoclinal and Isodynamic Lines in the British Islands, from Observations by Professors Humphrey Lloyd and John Phillips, Robert Were Fox, Esq., Captain James Clark Ross, R.N., and Major Edward Sabine, R.A. By Major Edward Sabine, R.A., F.R.S.

At the meeting of the British Association, held at Cambridge in the year 1833, a resolution was passed, recommending that a series of determinations of the magnetic dip and intensity should be executed in various parts of the United Kingdom.

Early in 1834 Professor Lloyd, who had attended the meeting at Cambridge, proposed to me to unite with him in carrying the recommendation of the Association into effect as far as regarded Ireland. I was at that time employed on the staff of the Army in the south-west district of Ireland, and found it not incompatible with other duties to undertake that portion of the island. Our observations were continued at intervals throughout that year, and until the autumn of 1835, in the summer of which year we were joined by Captain James Clark Ross. A report of our operations, drawn up by Professor Lloyd, was made to the British Association, assembled in that year in Dublin, and was printed in 1836 in the fourth volume of the Association Reports. A re-calculation of the Irish results, incorporating the observations which have been made since in that part of the United Kingdom, has been furnished by Mr. Lloyd, and occupies its appropriate place in this report.

Mr. Robert Were Fox, who was present at the Dublin meeting in 1835, brought with him an apparatus for magnetic observations on a new construction of his own invention, with which, after the meeting, he made several observations of the dip in the course of a tour in the west and north of Ireland. These observations, with others made on his return through Wales, were published in 1836, in the report of the Royal Polytechnic Society of Cornwall for 1835. Several of these observations were made in houses, and were consequently liable to disturbing influences. Mr. Fox has selected eight determinations of the dip in Ireland, and nine in Wales, as free from objection on this account; and with his permission they are now incorporated in the present report.

Having obtained two months leave of absence from military duty in the summer of 1836, I employed them in extending the survey to Scotland, by observations at twenty-seven stations distributed over that country; forming the basis of a memoir on the Scottish Isoclinal and Isodynamic lines, which was printed in the fifth volume of the Association Reports, and published in 1837.

In the same summer Professor Lloyd commenced the magnetic survey of England by a series of observations at fourteen stations, principally in the midland and southern districts; these observations have not been hitherto published, and will be found in their place in the present memoir.

The interest which had been excited at the meetings of the British Association by the Irish and Scotch Magnetic Reports, induced Professor Phillips to provide himself with an apparatus for the dip and intensity; having particularly in view the investigation of the influence which he deemed it possible the configuration of the surface, or the geological character of the district, might have on the position or on the inflexions of the lines representing these phænomena. In the summer of 1837 Mr. Phillips visited and observed at twenty-four stations in England, chiefly in the northern district; these observations are now first published.

In the same summer Mr. Fox determined the dip at twenty stations in the north of England and south of Scotland; and in the summer of 1838 at eight stations in the south of England, extending from London to the Scilly islands; at some of the latter stations he also observed the intensity: these observations form part of the present memoir.

In August 1837 Captain James Ross commenced a series of magnetic observations, which he continued almost uninterruptedly until the close of 1838; they extend over England, Ireland and Scotland generally, and comprehend fifty-eight stations. His observations of the dip and of the intensity are included in the present memoir.

Lastly, between August 1837 and October 1838, I have taken advantage of an interval between military duties, to observe the dip and intensity at twenty-two stations, distributed for the most part round the coasts of England and Wales, and extended into Ireland and Scotland for the purpose of accomplishing a more complete connexion of the different series.

It has been the wish of the four gentlemen connected with me in this undertaking, that I should draw up the memoir of what our joint labours have accomplished. Our observations have been now carried over the whole extent of England, Ireland and Scotland; and may be considered in their combination, and by their extent, to obtain, in some measure, the character of a national work; presenting to the immediate requisitions of science, the actual state of the phænomena of the magnetic dip and intensity in the British islands; and furnishing for distant times the means of a comparison, whereby the secular changes of these elements may be correctly judged of.

It has been found convenient to divide the report into two parts, the first comprising the observations of the Dip, the second those of the Intensity.

Division I.—*Dip.*

In the memoir on the magnetical observations in Ireland (British Association Reports, vol. v.), Mr. Lloyd has noticed the discrepancies which have been occasionally found in the results of observations of the dip made at the same station with different instruments. The observations of Captain Ross at Westbourne Green, which are there related, place these discrepancies in the strongest light. Captain Ross employed eight needles, making from eight to ten observations with each, each observation consisting of eighty readings; i. e. of ten in each of the eight usual positions. The dip at Westbourne Green, resulting from each of these needles considered separately, varied from 69° 01′·5 to 69° 42′·6. On these discordances Mr. Lloyd remarks as follows: "Thus it appears that there is a difference amounting to 41′ in the results of two of the needles used; and that the difference is very far beyond the limits of the errors of observation, will appear from the fact, that the *extreme difference* in the partial results with one of these needles, B (1), does not amount to 4′½, while with the other, (P), the extreme difference is only 2′. In fact, it so happens, that these very needles which differ most widely in their *mean* results are those in which the accordance of the *partial* results is most complete. Of the eight results obtained with needle P, there is one only which differs from the mean of the eight by a single minute; and yet the mean of all the observations with this needle differs by more than 20′ from the mean of any of the others, while its excess above the mean of the entire series amounts to 25′.

"These differences cannot be ascribed to any partial magnetism in the apparatus, for three of the needles (I, P and R) were of the same dimensions, and were used with the same circle, and yet their results, as we see, are widely discordant. We must seek then in the needles themselves the cause of these perplexing discrepancies; and we are forced to conclude that there may exist, even in the best needles, some source of constant error which remains uncorrected by the various reversals usually made; and that accordingly no repetition of observations with a needle so circumstanced can furnish even an approximation to the absolute dip."

I may add to the preceding remarks, that the discordances thus noticed far exceeded the limit of either diurnal or irregular fluctuations of the dip in England, as far at least as these phænomena have hitherto been the subject of observation.

An attentive consideration of the various sources of error to which dip observations might be liable,—of those which were already guarded against, and of those which still remained unprovided for,—induced the belief, that a considerable part at least of the discrepancies in question, and of similar discordances experienced elsewhere, were occasioned by the axle, on which the needle rests on the agate planes, not being perfectly cylindrical. Careful observers on the continent had already noticed defects of workmanship in this respect; and had been led thereby to have needles made, in which the axle, instead of being permanently fixed to the needle, was secured in its place merely by strong friction, and could be taken out, turned a portion of a circle on its own centre of rotation, and replaced; thus enabling the points of the circumference of the axle in contact with the supporting planes to be varied in successive trials. At Captain Ross's desire, Mr. Robinson undertook to have four needles of this description made, for one of which Mr. Frodsham, whose chronometers are so well known for their excellence, undertook to make the axle. On these needles being completed, they were tried each in four different positions of the axle,—that is to say, the axle being secured, an observation of the dip was made in the usual manner, and with the usual reversals:—the axle was then removed, turned on its own centre a portion of a circle, replaced, and the dip again observed:—in like manner, a third and fourth change was made in the position of the axle, and the dip observed at each. The process thus described was twice repeated with each needle. Of the four, Mr. Frodsham's axle proved the best; but the trial clearly manifested in all the imperfection which had been apprehended. The results with the needle furnished with Mr. Frodsham's axle are given in the subjoined table, where that needle is designated as No. 1.

With this experience Mr. Robinson undertook to replace the axles of the other three needles with three which should be the workmanship of his own hands. On these being tried, the discrepancies of each in the four positions were less than of any of the four axles in the former trial, but still amounted to several minutes. The results of the best of Mr. Robinson's axles have been selected for illustration, and are those of No. 2. in the subjoined table.

Table I.

Trials of the Axles of the under-mentioned Dipping Needles.

Needle 1. Frodsham's axle.			Needle 2. Robinson's axle.		
Position of the Axle.	Poles: α direct. β reversed.	Mean Dip.	Position of the Axle.	Poles: α direct. β reversed.	Mean Dip.
1	α 69° 34′·5 β 68 48·2	69° 11′·4	1	α 68° 42′·3 β 70 02·9	69° 22′·6
2	α 70 01·2 β 69 44	69 52·6	2	α 70 15·6 β 68 40·1	69 27·8
3	α 68 34 β 69 52·6	69 13·3	3	α 69 13·5 β 69 39·1	69 26·3
4	α 70 06·8 β 69 43·1	69 54·9	4	α 69 49·8 β 69 08·6	69 29·2
Mean of four positions of the Axle		69 33·05	Mean of four positions of the Axle		69 26·5
	Experiment	repeated.		Experiment	repeated.
1	α 69 43·2 β 68 54·6	69 18·9	1	α 69 54 β 68 43	69 18·5
2	α 70 11·2 β 69 51·9	70 01·5	2	α 68 53·1 β 70 05·8	69 29·5
3	α 69 05·4 β 69 47·1	69 26·3	3	α 69 50·8 β 68 55·2	69 23
4	α 69 42·9 β 69 54·9	69 48·9	4	α 69 02·6 β 69 56·6	69 29·6
Mean of four positions of the Axle		69 38·9	Mean of four positions of the Axle		69 25·15

The observations having been made in a house, the dip observed is not the true dip in London. This is immaterial, as the object of the experiment was solely the agreement or otherwise of the results in the different positions of the axle.

Had the axles been perfect, the same dip should of course have been given in all positions of the axle: we perceive, however, that the differences in the one needle amount to above 40′, and in the other from 7′ to 11′. The results of these experiments fully impressed Mr. Robinson with the necessity of employing more effectual means for ensuring a true figure to the axles of dipping needles; and in several which he has since made, and which have been carefully examined, he has proved successful.

Having exhibited the discrepancies of the earlier needles, it may be satisfactory to show the improvement in some of the later ones; and for that purpose the following observations are given with needles which were afterwards employed in the general observations of this report. The axles of these needles, being made to revolve, were successively tried in four positions, which were, as nearly as could be guessed, a quarter of the circumference apart; had they been precisely so, the needle must have rested on the same points of the axle, in the 1st and 3rd positions, and in the 2d and 4th, (as the poles are reversed in each observation), and the results in those positions should have been the same; but as this can have been only approximately done, each position may be considered as bringing a different set of bearings into play. The observations were made as before, in Mr. Robinson's house, and have therefore no reference to the true dip.

Table II.

Trials of the Axles of the undermentioned Dipping Needles. London, June and July, 1838.

Positions of the Axle.	1st Pair.		2nd Pair.		3rd Pair.	
	R. 4.	R. 5.	R. 6.	R. 7.	W. 1.	W. 2.
	° ′	° ′	° ′	° ′	° ′	° ′
1	69 44·9	69 43·5	69 39·8	69 43·1	69 48·4	69 48·9
2	69 43·8	69 39·9	69 40·4	69 40·8	69 50·4	69 46·0
3	69 38·1	69 46·2	69 41·4	69 47·0	69 49·5	69 46·7
4	69 43·4	69 44·8	69 36·8	69 38·8	69 53·5	69 47·8
Mean...	69 42·5	69 43·6	69 40·0	69 42·4	69 50·5	69 47·4

In all these six needles a great improvement was manifested. The greatest difference occurring in any two positions of the axle of any one of the six needles is 8′, including of course accidental errors of all kinds.

The imperfection of the axle is a source of error, from the effects of which, if it exists, the results can scarcely be freed by any mode of conducting the observation; at least, without going through the very tedious operation of observing round the circumference of the axle on every occasion. When accuracy is desired, therefore, only such needles should be employed, as have been ascertained by preliminary trial to be nearly free from this defect. Needles with revolving axles are easily tried. Those of the ordinary description, in which the axle is permanently fixed, may be examined by observing the angle of inclination shown by the needle when the circle is turned in different azimuths from that of the magnetic meridian, and by computing the dip by means of appropriate formulæ, from the angles shown in the different azimuths. If the axle is perfect the dips so computed should all accord. In the azimuths intermediate between the magnetic meridian and its normal plane, the needle rests successively on all points of the axle comprised in a portion of the quadrant equivalent to the complement of the dip;

and the corresponding points of the other three quadrants become in turns the points of support in the customary processes of the reversals of the poles and circle. If this operation is gone through at any part of the earth on or near the line of no dip, the whole of the quadrant is thereby subjected to examination. In such situations, consequently, this method affords the means of examining the whole circumference of the axle; and in all other localities, as much of the circumference as amounts to four times the complement of the dip. Whatever portion in the latter cases remains unprovided for, may be tested by converting the needle, temporarily, into one on Mayer's principle. This can easily be done by the application of a little wax; the quantity of which may be varied at pleasure, so as to correspond with the weights of different sizes, by which, in Mayer's method, the angles of inclination, from which the dip is computed, are varied in successive observations. By one or other of these processes the true dip at any station can be obtained from any and every inclination of the needle; and every part of the circumference of the axle can consequently be tested.

In what has been said, it has been presumed that there is no magnetism in the circle itself, as, should such exist, it would certainly become the source of discordance in the results derived from different azimuths, or from different weights, independently of any defect in the axle; and so far, therefore, the agreement of the results in such trials (should they be found to agree) indicates with great probability the freedom of the circle from magnetism as well as the goodness of the axle. But Mr. Lloyd has employed and has described in a subsequent part of this report an independent and much more delicate mode of examination for magnetism in the circle.

The customary provision of *two* needles for each apparatus does not alone afford security against the errors which may be occasioned by either of the defects to which I have now alluded. In respect to the axle, if the results of the two needles are accordant, it is thus far satisfactory, that it certainly is not probable that both needles should have accidentally exactly the same imperfection; but if they differ, the observer has no guide as to which is to be preferred; whilst their mean result cannot usually be more than an approximation to the true dip, for it is also improbable that the two needles should have an exactly equal amount of error in opposite directions. As a means of detecting magnetism in the limb, two needles are of no more avail than one; because both are directed to the same point of the circle when observed with at the same station, and, if a disturbing influence exists, both will be subjected to the same error. If, however, one of the needles is temporarily fitted on Mayer's plan,—and the dip is obtained in successive experiments from arcs differing very widely from each other, and distributed generally round the whole circle,—and if the results in such case accord well with each other, and with those of the unweighted needle,—it may be concluded that there is no disturbing influence in the limb.

Those who are desirous of making accurate observations, should regard the preliminary examination of the axle and limb of the apparatus they employ as an indispensable precaution. When these points have been satisfactorily examined, and the instrument is found correct, the natural magnetic direction, both in regard to azimuth and inclination, is the most advantageous for the observation of the dip. It is in the preliminary examination, that the method devised by Mayer, and that of varied azimuths, are chiefly valuable*.

It may now be satisfactory to exhibit the observations that have been made at Westbourne Green in the years 1837 and 1838 with different circles and approved needles. (Table III.) The greater part of these instruments were made by Mr. Robinson since his attention has been particularly directed to the circumstances above noticed; and those who will take the trouble to compare their performance with that of the several needles employed by Captain Ross at the same station in 1835, will have an opportunity of judging how great an improvement has been effected in our English dipping needles since that period. Of the two other instruments not made by Robinson, one was made by Gambey for Captain Fitz Roy, of the Royal Navy, and kindly placed by that officer at my disposal, to be employed in the observations in this report. The excellence of the dipping needles of this artist is too well known to need any comment in this place. The other instrument was made by Mr. Thomas Jordan of Falmouth, the artist employed by Mr. Fox to make the dip apparatus on the construction which he has devised, and which is described in a paper in the 3rd vol. of the "Annals of Electricity, &c." Mr. Fox's needles do not rest on a cylindrical axle supported by planes, but the axle is terminated by exceedingly fine and short cylindrical pivots, which

* The needle employed by Sir Everard Home in the observations published in the last volume of the Phil. Trans. 1838, Part 2, appears, by its results at the Athenæum at Plymouth, and at Ham, near London, to have given dips exceeding the truth by about half a degree. It is probable that a careful examination would trace this error to imperfection in the axle; and in such case errors of a contrary character would exist when the axle should rest on some other points of its circumference, and may have influenced the determinations at some of Sir Everard's foreign stations. By the methods pointed out in this report, a table of errors at different dips might be formed for this needle, by which its results might be corrected. This additional trouble would be well bestowed in perfecting this extensive series, on which so much pains have already been expended.

work in jeweled holes. By means of the "deflectors" which make a part of Mr. Fox's apparatus, the dip may be deduced from readings at various parts of the circle, and there is therefore the same opportunity of discovering errors caused by magnetism of the circle, or by imperfection in the bearings of the axle, as the azimuthal and Mayer's methods furnish in needles of the ordinary construction: the jewel-plate itself is also made to revolve, so that the resting-places of the axle in the jewels may be changed at pleasure. The performance of these needles sufficiently indicates the great care bestowed on their workmanship. As the different observations in Table III. include an interval of eighteen months, they have been rendered more strictly comparable by the addition of a column, in which they are reduced to the common epoch of the 1st January, 1838, by applying a proportional part of the annual rate of decrease of the dip in London at this time, which, from reasons that will be assigned hereafter, is considered to be 2′·4.

Table III.

Observations of Dip at Westbourne Green in 1837 and 1838, with approved Needles.

Artist.	Needle.	Observer.	Date.	Observed Dip.	Deduced Dip, Jan. 1. 1838.
Robinson..	P 1.	Phillips	May 30, 1837	69°22·5′	69°21·1′
......	P 2.	......	......	69 17·9	69 16·5
Gambey...	G 1.	Ross.	Aug. 10, 1837	69 20·6	69 19·7
......	G 2.	......	......	69 19·8	69 18·9
Robinson..	P 1.	Phillips	March 28, 1838	69 19·5	69 20·1
......	P 2.	......	......	69 17·0	69 17·6
Jordan....		Fox	June 8, 1838	69 17·0	69 18·0
Robinson..	W 1.	Ross	June 16, 1838	69 16·2	69 17·3
......	W 2.	......	......	69 12·9	69 14·0
......	R 4.	......	July 6, 1838	69 13·7	69 14·9
......	R 5.	......	......	69 12·8	69 14·0
......	R 6.	......	July 7, 1838	69 14·0	69 15·2
......	R 7.	......	......	69 16·4	69 17·6
......	R 4.	......	Dec. 4, 1838	69 15·5	69 17·7
......	R 5.	......	......	69 12·8	69 15·0
......	R 6.	......	Dec. 10, 1838	69 15·9	69 18·2
......	R 7.	......	......	69 14·4	69 16·7
				Mean......	69 17·2

The subjoined tables, IV., V., VI., VII., VIII., exhibit in detail the azimuthal examinations which have been made of some of the instruments employed in the observations contained in this report; it has appeared the more desirable to give these tables, because the practice of this method is new in this country.

Table IV. contains observations made at Tortington on the 17th of October, 1837, with Captain Fitz Roy's Gambey, and its needle No. 2. The dip is here successively deduced from the angles of inclination observed in azimuths 90° apart from each other. In such case, $\cot^2 \delta = \cot^2 i + \cot^2 i'$, δ being the true dip, and i and i' the angles of inclination in any azimuths 90° apart. In the first example in the table, i is the angle of inclination shown by the needle when the plane of the circle is removed 10° from the magnetic meridian; that is, when it is in the direction of N. 10° E., and S. 10° W; i therefore includes, and is the mean of observation with the poles direct and reversed, and with the index of the azimuth circle at 10° and 190°; i' is in like manner a mean of the angles of inclination with the poles direct and reversed, when the index of the circle is at $(10+90°=)$ 100°, and at $(100+180°=)$ 280°: here $\cot^2 i + \cot^2 i' = \cot^2 69° \; 13'{\cdot}5 + \cot^2 86° \; 15'{\cdot}2 = \cot^2 \delta$; whence $\delta = 68° 56'{\cdot}64$. In the next deduction, the values of i and i' are obtained with the index of the azimuth circle at 20° and 200°, $(20+90°=)$ 110° and 290°, and so forth.

Table IV.

Tortington, Oct. 17, 1837, with Captain Fitz Roy's Gambey, Needle 2. Observer, Major Sabine.

Azimuth.	Poles direct.	Poles reversed.	Mean.	Dip deduced.	Azimuth.	Poles direct.	Poles reversed.	Mean.	Dip deduced.
°	° ′	° ′	° ′	° ′	°	° ′	° ′	° ′	° ′
10	69 15	69 03	69 13·5	68 56·64	50	76 06·5	75 50·7	76 00·9	68 56·40
190	69 07	69 29			230	75 53·2	76 13		
100	86 12	86 28·7	86 15·2		140	73 34·2	73 49·2	73 38	
280	86 25	85 55			320	73 44·3	73 24·3		
20	70 04·5	69 50	70 02·7	68 55·64	60	79 08·7	78 48	79 01·6	68 56·05
200	70 00·2	70 16			210	78 52·8	79 16·5		
110	82 36·2	82 50·7	82 39		150	71 34	71 50·2	71 35·7	
290	82 45·5	82 23·5			330	71 39·2	71 19·5		
30	71 39	71 16·2	71 30·4	68 56·96	70	82 31·7	82 14	82 25·2	68 55·55
210	71 26·2	71 40·3			250	82 18·8	82 36·5		
120	79 09·7	79 26	70 13		160	70 05·7	70 22·5	70 07	
300	79 20·5	78 56			340	70 07·5	69 52·5		
40	73 37·7	73 18·3	73 30	68 56·26	80	86 11	85 44	86 01·5	68 54·90
220	73 23·8	73 40·2			260	85 56	86 14·7		
130	76 07	76 23·2	76 10·4		170	69 09·2	69 30·5	69 13·9	
310	76 16·5	75 55			350	69 13	69 02·3		
					0	68 55·5	68 46	68 56·4	58 56·4
					180	68 52	69 12·2		

The mean of the nine results in the preceding table is 68° 56′·1. Each angle is a mean of four readings. Total number of readings, 272.

Table V. (in two parts) contains observations made by Captain Edward Johnson, R.N., F.R.S., and myself, with the same circle and needle, in the Regent's Park, London, on the 15th and 16th November, 1837. In this case, the reversal of the needle on its supports was made a part of the series, in addition to the reversals in the last table; thus the values of i and i' are each the mean of eight angles instead of four.

Table V.

Observations with Capt. Fitz Roy's Gambey, Regent's Park, London.

Observer, Captain Johnson. 1837.

Nov.	Azimuth.	Poles Direct. Needle. Direct.	Poles Direct. Needle. Reversed.	Poles Reversed. Needle. Direct.	Poles Reversed. Needle. Reversed.	Means.		Dip Deduced.
15.	0°	69° 19′·25	69° 14′·25	69° 16′·75	69° 35′·25	69° 21′·37	69° 25′·25	69° 25′·25
	180	69 19·75	69 24·25	69 43·75	69 28·75	69 29·13		
15 & 16	15	70 0	69 49·5	69 50	70 04·5	69 56	70 01	69 22·08
	195	69 58	70 07·5	70 15·5	69 59	70 05		
	105	84 15·5	84 36	84 37·5	84 28·5	84 29·4	84 25·5	
	285	84 28	84 15	84 13·5	84 30·5	84 21·7		
	30	72 04	71 45·5	71 48·5	72 08	71 56·5	71 59·95	69 25·30
	210	71 54	72 05	72 14·5	72 00	72 03·4		
	120	79 15	79 34·5	79 33·5	79 14·5	79 24·4	79 20·8	
	300	79 26	79 09·5	79 06	79 27	79 17·1		
	45	75 12·5	74 58·5	74 57	75 16·25	75 06·1	75 08·7	69 24·30
	225	75 07	75 16	75 19·5	75 03	75 11·4		
	135	75 03·5	75 11	75 17·5	75 06·5	75 09·6	75 05·4	
	315	75 05	75 00·5	74 52·5	75 07	75 01·2		
	60	79 29	79 10	79 13	79 38	79 22·5	79 25·3	69 24·20
	240	79 26	79 34	79 37	79 15·5	79 28·1		
	150	71 55	72 04	72 12·5	71 52·5	72 01	71 56·3	
	330	71 56·5	71 45	71 46·5	71 58·5	71 51·6		
	75	84 33·5	84 21·5	84 18	84 36·5	84 27·4	84 28·9	69 22·50
	255	84 28	84 31	84 41·5	84 21	84 30·4		
	165	69 56·5	70 04·5	70 17	69 57	70 03·75	70 00·6	
	345	70 03·5	69 51·5	69 48	70 06·5	69 57·4		
16.	180	69 17·5	69 29·7	69 41·5	69 07·75	69 24·11	69 22·3	69 22·30
	0	69 20·5	69 13·5	69 10	69 38	69 20·5		
							General Mean.	69 23·7

Table V.

Observations with Captain Fitz Roy's Gambey, in the Regent's Park, London.

Observer, Major Sabine. 1837.

Nov.	Azimuth.	Poles Direct. Needle. Direct.	Poles Direct. Needle. Reversed.	Poles Reversed. Needle. Direct.	Poles Reversed. Needle. Reversed.	Means.	Means.	Dip Deduced
	°	° ′	° ′	° ′	° ′	° ′	° ′	° ′
15.	0	69 20·8	69 14·5	69 15·7	69 35	69 21·5	69 24·75	69 24·75
	180	69 17·5	69 25·2	69 43	69 26·3	69 28·0		
15 & 16	15	70 03	69 49	69 45	70 05·5	69 55·6	70 01·2	69 22·61
	195	69 59·5	70 10	70 19	69 59	70 06·9		
	105	84 18	84 40·5	84 38·5	84 23	84 30	84 27	
	285	84 32	84 15·5	84 16	84 33	84 24·1		
	30	72 02	71 45	71 46·5	72 11	71 56·1	72 00·5	69 25·72
	210	71 54	72 06	72 12·5	72 07	72 04·9		
	120	79 13	79 35·5	79 32·5	79 23	79 26	79 20·8	
	300	79 24·5	79 12·5	79 02·5	79 23	79 15·6		
	45	75 07·5	74 59·5	74 58·5	75 21·25	75 06·7	75 08·3	69 24·47
	225	75 06	75 14	75 15	75 04·5	75 09·9		
	135	75 04·5	75 10	75 20	75 08·5	75 10·75	75 06·25	
	315	75 07·5	74 57·5	74 54	75 08	75 01·75		
	60	79 24	79 13·5	79 16	79 42	79 23·9	79 26·2	69 24·17
	240	79 24·5	79 36	79 37·5	79 16	79 28·5		
	150	71 50·5	72 03·5	72 14·5	71 54·5	72 00·75	71 55·75	
	330	71 57·5	71 44	71 45	71 56·5	71 50·75		
	75	84 37	84 22·5	84 19·7	84 37·5	84 29·2	84 30·3	69 23·08
	255	84 28·5	84 32	84 36	84 29	84 31·4		
	165	69 55·5	70 05·5	70 18	69 57	70 04	70 00·9	
	345	70 06·5	69 49	69 50·5	70 05	69 57·75		
16.	180	69 17	69 33	69 42·7	69 09	69 25·44	69 24	69 24
	0	69 21	69 13·2	69 12·5	69 43·5	69 22·56		
							General Mean.	69 24·11

Each of the numbers, both in Captain Johnson's and Major Sabine's observations, is a mean of the readings of the two ends of the needle. In the azimuths 0 and 180° each number is also a mean of two distinct observations, between which the needle was raised from its supports, and lowered afresh. At all the other azimuths one such observation by each of the observers was considered sufficient. The total number of readings is 224 by each observer.

Table VI.

Observations with Gambey's Circle and Needle 2 at Dover; by Major Sabine. 1837.

Azimuth.	Face of Needle to face of Circle. Poles Direct.	Poles Reversed.	Mean.	Dip.	Remarks.
° and °	° ′	° ′	° ′		
30 and 210	71 31·1	71 33	71 32	68 53·2	On the side of the hill above Arch-cliff Fort on the 2nd November.
120 and 300	79 04·5	78 59·1	79 01·8		
60 and 240	79 07·1	79 09·8	79 08·5	68 52·9	
150 and 330	71 26·4	71 29·6	71 28		
0 and 180	68 48·8	68 54·8	68 51·8	68 51·8	
Mean.........	....	....	....	68 52·6	
	Face of Needle Reversed.				
30 and 210	71 30·5	71 32·5	71 31·5	68 51·3	Beneath Shakspeare's Cliff on the 7th November.
120 and 300	79 02·2	78 54·5	78 58·4		
60 and 240	79 14·5	79 13	79 13·7	68 52·2	
150 and 330	75 21·7	71 27	71 24·4		
0 and 180	68 52·7	68 54·6	68 53·6	68 53·6	
Mean.........	....	....	....	68 52·4	

Table VII. contains observations by Professor Phillips, with a six-inch circle by Robinson, and its needle 1. The inclination of the needle (i) was observed with the circle in different azimuths (θ), and the dip computed from the inclination found in each azimuth by the formula $\cot \delta = \cot i \sec \theta$.

Table VII.

Observations of the Dip with Mr. Phillips's Circle and Needle 1.

York, Sept. 13, 1838. Azimuth. θ	Inclination i	Dip. δ	Helmsley, Sept. 14, 1838. Azimuth. θ	Inclination i	Dip. δ	Malton, Sept. 15, 1838. Azimuth. θ	Inclination i	Dip. δ
°	° ′	° ′	°	° ′	° ′	°	° ′	° ′
00	70 50·6	70 50·6	00	70 57·4	70 57·4	00	70 51·7	70 51·7
10	71 08·2	70 51·5	10	71 14·2	70 58·0	10	71 08·1	70 52
20	71 53·7	70 49·1	20	72 01·7	70 56·0	20	71 54	70 49·3
30	73 16·9	70 52·5	30	73 21·5	70 57·5	30	73 15·5	70 50·6
40	75 04·6	70 48·5	40	75 13	70 59·5	40	75 03	70 47
50	77 22·5	70 47·3	50	77 31·4	71 00·0	50	77 26·1	70 52·5
60	80 07·9	70 49·9	60	80 16	71 04·0	60	80 05·9	70 45·4
	Mean Dip...	70 48·6		Mean Dip...	70 58·9		Mean Dip...	70 49·8

Table VIII. contains observations by Captain James Ross, with a six-inch circle by Robinson, and its needles R. 4. and R. 6., at Jordan Hill, in September 1838. The dip is here computed by the formula, $\cot^2\delta = \cot^2 i + \cot^2 i'$; and in the final column the dip observed in the ordinary manner, *i. e.* in the azimuths 0 and 180°, is inserted for comparison.

Table VIII.

Observations with Robinson's Needles R. 4. and R. 6., Jordan Hill, September 1838.

Observer, Captain James C. Ross.

Needle R. 4.

Azimuth.	Poles α.		Poles β.		Means.	Dip Deduced.	Azimuth. 0 & 180°
	Needle Direct.	Needle Reversed.	Needle Direct.	Needle Reversed.			
	° ′	° ′	° ′	° ′	° ′	° ′	° ′
60	81 4·6	81 10·4	81 3·5	81 18·5	81 4·1	72 21·6	72 22·2
240	81 4·2	81 3·6	81 3·3	80 44·5			
150	74 31·6	74 27·8	74 19·8	74 32·3	74 32·1		
330	74 47·2	74 29·9	74 40·8	74 28			
Needle R. 6.							
45	77 10·7	77 5·5	77 26	77 26	77 16·7	72 19·4	72 17·7
225	75 5·8	77 24	77 22·8	77 13			
135	77 18·5	77 5	77 25·1	77 21·2	77 19·5		
315	77 15·7	77 23·3	77 21·7	77 22·9			

Annual Alteration of the Dip.

The observations of dip included in this report, extend over an interval of four years and upwards. To reduce these to a common epoch, we require to know the amount of the change which the dip undergoes from year to year. In the Reports on the Magnetic Observations in Ireland and Scotland, an annual decrease of three minutes was provisionally assumed; but we must now endeavour to assign the amount with somewhat greater precision.

In the 21st volume of the Annalen der Physik, M. Hansteen has assembled all the most trustworthy observations of the dip in London, Paris, Berlin, and Geneva during the present century, and the latter part of the last; and has computed from them the most probable amount of the annual decrease of the dip at each of those stations, corresponding to every tenth year, from 1780 to 1830. As the results of this investigation have not been published, I believe, in this country, I have subjoined a table in which they are exhibited.

TABLE IX.

Annual Decrease of Dip.

Year.	Paris.	London.	Berlin.	Geneva.	Mean.
1780	6'·75	4'·90	5'·26	5'·04	5'·49
1790	5·92	4·57	4·71	4·71	4·98
1800	5·11	4·21	4·15	4·38	4·46
1810	4·29	3·88	3·58	4·05	3·95
1820	3·47	3·55	3·02	3·72	3·44
1830	2·61	3·22	2·46	3·39	2·93

The differences which appear in the progression and rate of the annual decrease at the four stations in this table, are probably attributable in far greater proportion to incidental errors in the observations, than to the actual existence of such differences. We may consequently regard the final column, or the mean of the results at the four stations, as affording, in all probability, a more satisfactory conclusion in regard to the rate of change at any one of the stations than is drawn from the observations at that station only.

We may proceed to examine how far this rate of decrease corresponds with the most recent observations made in Britain. In August 1821, I made a series of more than usually careful observations on the amount of the dip in the Regent's Park in London; employing for that purpose a needle on Mayer's principle, with weights of different magnitudes to obviate the liability to any constant instrumental error, and continuing the observations during several days in order that the general result might approximate the more nearly to the true mean dip at the period. These observations were published in the Phil. Trans. for 1822, Art. I.; their final result being a dip of 70° 02'·9, corresponding to the middle of the month of August 1821. To compare with this, we have the observations made in London, at different times and in different localities, by the contributors to this report. It is proper that we should employ for the present purpose only those observations which give entirely independent determinations; viz. those only which are complete in all the requisite positions of the needle and circle, including the reversal of the poles, and which need no correction for instrumental defects. Of such observations we have those at Westbourne Green, already given in Table III.; those in the Regent's Park, contained in Table V.; an observation by Mr. Fox, in May 1838, in a field west of Maiden

Lane, and one of mine, on the 13th of October, 1838, in the gardens of the Palace at Kew. These are collected in the following table.

Table X.

Observations of the Dip in London in 1837 and 1838, with approved Needles.

Date.	Observer.	Dip observed.	Place of Observation.
1837.			
May 30.	Phillips	69° 20′·2	Westbourne Green.
Aug. 10.	Ross	69 20·2	Westbourne Green.
Nov. 15. & 17..........	Johnson & Sabine	69 23·9 *	Regent's Park.
1838.			
March 28.	Phillips	69 18·2	Westbourne Green.
May 22.	Fox	69 19·0	Maiden Lane.
June 8.	Fox	69 17·0	Westbourne Green.
June 16.	Ross	69 14·5	Westbourne Green.
July 6....................	Ross	69 13·3	Westbourne Green.
July 7....................	Ross	69 15·2	Westbourne Green.
Oct. 13.	Sabine	69 16·5	Kew Gardens.
Dec. 4....................	Ross	69 14·1	Westbourne Green.
Dec. 10.	Ross	69 15·2	Westbourne Green.
Mean { corresponding to the beginning of May 1838. }		69 17·3	

We have therefore 70° 02′·9 in August 1821, and 69° 17·3 in May 1838; or a diminution of 45′·6 in 16·7 years, equivalent to a mean annual decrease of 2′·73, corresponding to the middle of the interval, or to the beginning of the year 1830. The

* This is the mean of fourteen results, extremely accordant with each other, obtained in different azimuths; (see Table V.). It will be remarked that it is decidedly the highest of the results from which the mean dip in London has been derived. The observations with the same instrument at Kew, as well as every comparison between this and other instruments, give reason to believe that the high dip in the Regent's Park, in November 1837, is not attributable to any instrumental error. It may then have arisen either from the dip on those days being actually greater by three or four minutes than its general average, or from some local disturbing influence. The locality is the same in which the observations in 1821 were made, and the result in question may on that account appear more strictly comparable with them; but though the locality is the same, it is not one in which we can feel confident that no change may have occurred in regard to magnetic influence. The Regent's Park is certainly not so eligible a situation *now* for magnetic experiments as it was in 1821. These considerations have induced me to derive the London Dip in 1838 for the purpose in the text, from the mean of the observations and localities in Table X, rather than from those in the Regent's Park alone; and not to give to the latter result that additional weight in comparison with the others to which it would seem entitled as derived from observations in so many azimuths.

mean rate for the same year in M. Hansteen's table is $2'{\cdot}93$, which must be regarded as a satisfactory accordance, the difference being less than exists between the rate for that year at any one of the stations in M. Hansteen's table, and the mean of the four stations. We may infer from the accordance, therefore, that both these numbers, $2'{\cdot}93$ and $2'{\cdot}73$, are extremely near the truth; and I have employed that which results from our own observations, namely, $2'{\cdot}73$ corresponding to 1830. Following the progression in M. Hansteen's table, the rate of decrease would become $2'{\cdot}4$ in 1836, which is the middle period of the observations contained in this report. In the reductions to a common epoch, $2'{\cdot}4$ has consequently been employed as the mean annual decrease of the dip in the British Islands between 1834 and 1838. In the absence of any certain knowledge in regard to the unequal distribution of the yearly decrease in the different months of the year, I have regarded it as taking place in the uniform proportion of $0'{\cdot}2$ per month.

In a recent communication to the Royal Irish Academy, Mr. Lloyd has stated the result of thirty-nine observations of the dip in Dublin between October 1833 and August 1836, which, combined by the method of least squares, give $2'{\cdot}38$ for the most probable rate of the annual diminution of the dip in Dublin during that period. This result, though drawn from so limited a period, is in remarkable accordance with the deduction from the observations in London, and furnishes a strong presumption that the rate thus found is applicable both to England and Ireland. In regard to Scotland, no observations have as yet been made, I believe, with this particular object. The general aspect of the observations in Scotland, at different dates, contained in this report, would certainly indicate a less annual change than has been deduced from the observations in England and Ireland; and in every instance in Scotland where observations have been made at the same station and at different periods, either by the same or different observers, the evidence is of the same nature,—the results would be brought into better accord if a smaller rate of decrease were adopted. In the case of the Shetland Islands, the dip observed by Captain Ross at Lerwick in August 1838, $73° 45'$, compared with that observed by Sir Edward Parry and myself in June and November 1818, $74° 22'$, makes a decrease of $37'$ in twenty years, or a yearly diminution of $1'{\cdot}85$, corresponding to the mean epoch of 1828. The observations of 1818 and of 1838 were made in the same garden. The identity of the spot,—the length of the interval,—and the repetition of the observations on different days on both occasions,—all give weight to this comparison; and strengthen the inference, that the rate of annual decrease is less in Scotland than in England. Still, in the absence of more positive data, I have not chosen to make any assumption; and have employed the one rate for the whole of the British Islands. The general result in Scotland, *i. e.* the mass of observations taken collectively, is independent of the amount of this reduction, the sum of the + and − reductions to the mean epoch of the 1st of January, 1837, being very nearly the same: the effect of a less rate of diminution than that adopted would be to increase the dips deduced from the observations in 1836, and to decrease those deduced from the observations in 1837 and 1838; and thus to give a rather more consistent aspect to the whole, without sensibly altering the resulting isoclinal lines.

No correction has been applied for the different hours of the day at which the several observations were made; but the hour is in almost all instances recorded. Professor Phillips had devoted several days of observation to the investigation of the regular horary variations of the dip, and had obtained results remarkably consistent, considering that they were derived from observations with the ordinary dipping needle*; but the recent invention of instruments specially adapted to this object, renders it probable that the phenomena of the periodical changes will be shortly determined with an accuracy hitherto unattainable: in the mean time, it has appeared preferable to apply no correction on this account. It may be proper to remind the reader, that the most perfect correction in this respect would still leave unremedied the influence of the irregular fluctuations, which there is great reason to believe frequently exceed in amount, and occasionally counteract the ordinary periodical movements.

I proceed now to give in detail the observations which comprise the first division of this report; namely, those of the Dip in England, Scotland, and Ireland. It will be convenient to separate these into three sections, commencing with those of England; and it may here be remarked generally, that all the latitudes and longitudes in this Report are taken from the maps published by the Society for Diffusing Useful Knowledge. The longitudes *east* of Greenwich are distinguished by the negative sign prefixed.

* Mr. Phillips's observations at St. Clairs and York, in the summer of 1837, from 7 a.m. to 11 p.m., appear to indicate a morning maximum of dip at 9 or 10 a.m., an evening minimum about 8, with a difference of above 5 minutes, the mean dip recurring about 3 p.m., and the line passing through the three points nearly parabolic.

Section I.—England.

Mr. Fox's observations.—I have arranged in the following table the observations of the dip in England with which I have been furnished by Mr. Fox, and have added thereto the columns containing the latitudes and longitudes, and the dips reduced to the mean epoch of the 1st January, 1837. The results in 1835 were obtained with a six-inch apparatus; those in 1837 with a seven-inch, and those in 1838 with a four-inch apparatus; all the instruments being those of Mr. Fox's construction, and made by Mr. Thomas Jordan of Falmouth.

Table XI.

Mr. Fox's Observations of the Dip in England.

Station.	Date.	Hour.	Lat.	Long.	Dip observed.	Dip deduced, 1 Jan. 1837.	Place of Observation.
Holyhead	Sept. 1, ·35	5½ A.M.	53° 19′	4° 37′	71° 04′	71° 00′·8	Hotel Garden.
Bangor	Sept. 1, ·35	10 A.M.	53 14	4 06	71 02	70 58·8	Hotel Garden.
Carnarvon	Sept. 1, ·35	3 P.M.	53 09	4 14	70 58	70 54·8	Hotel Garden.
Llanberris	Sept. 1, ·35	6½ P.M.	53 07	4 03	70 57	70 53·8	Foot of Snowdon.
Capelcrig	Sept. 3, ·35	7½ A.M.	53 06	3 53	70 48	70 44·8	Hotel Garden.
Malvern	Sept. 5, ·35	8½ A.M. 10½ A.M. 5 P.M.	52 07	2 19	70 11	70 07·8	Mean of 3 Stations.
Ross	Sept. 8, ·35	9½ A.M. 4 P.M.	51 55	2 35	70 00	69 56·8	Hotel Garden.
Neath	Sept. 11, ·35	2 P.M.	51 40	3 46	69 57	69 53·9	Glenvellyn Cottage.
Chepstow	Sept. 9, ·35	8 A.M.	51 38	2 40	69 48	69 44·8	Hotel Garden.
Belsay	Aug. 25, ·37	11 A.M.	55 07	1 53	71 17	71 18·6	
Skiddaw	Sept. 7, ·37	1½ P.M.	54 40	3 09	71 15	71 16·6	The Summit.
Keswick	Sept. 7, ·37	8 A.M.	54 32	3 09	71 14	71 15·6	Near the Lake.
Shull	Aug. 19, ·37	7½ A.M.	54 43	2 00	71 14	71 15·5	
Grassmere	Sept. 9, ·37	8½ A.M.	54 27	3 01	71 13	71 14·6	Behind the Inn.
Darlington	Aug. 21, ·37	7½ A.M.	54 32	1 33	71 07	71 08·5	Polham Hill.
Garstang	Sept. 12, ·37	11½ A.M.	53 54	2 47	70 59	71 00·7	Inn Garden.
Studley Park	Aug. 14, ·37	1 P.M.	54 08	1 34	70 56	70 57·5	
Bussco Bridge	Sept. 12, ·37	4 P.M.	53 39	2 50	70 45	70 46·7	
Near Liverpool	Sept. 23, ·37	8 A.M.	53 25	2 55	70 44	70 45·7	At the Dingle.
Liverpool	Sept. 19, ·37	0½ P.M.	53 25	2 58	70 39	70 40·7	Botanic Garden.
Matlock	Aug. 9, ·37	10 A.M.	53 08	1 32	70 19	70 20·5	Bath Hotel Garden.
London	May 22, ·38 June 8, ·38	5 P.M. 1 P.M.	51 32	0 11	69 19 69 17	69 21·4	Near Maiden Lane. Westbourne Green.
Tooting	June 14, ·38	8 A.M.	51 26	0 10	69 14·5	69 17	The Grove.
Falmouth	July 31, ·38	6 P.M.	50 09	5 06	69 13·5	69 17·3	Mr. Fox's Garden.
Eastwick Park	June 16, ·38	8½ A.M.	51 17	0 19	69 08	69 11·5	
Eastbourne	June 20, ·38	3 P.M.	50 47	−0 16	68 45	68 48·5	Grounds of D. Gilbert, Esq.
Combe-House	July 2, ·38	8½ A.M.	51 31	2 34	69 32	69 35·6	
St. Mary's, Scilly	Aug. 31, ·38	8 A.M.	49 55	6 17	69 26	69 30	
Trescow, Scilly	Aug. 31, ·38	1 P.M.	49 57	6 18	69 27	69 31	

We have in this table the dip observed at twenty-nine stations, of which the central geographical position is 52° 45′ N. and 2° 49′ W. If we desire to express the general result of this series of observations, as to the position of the isoclinal lines, their mean direction, and their mean distance apart in the district of country which the observations comprise, in the manner proposed by Mr. Lloyd in the discussion of the Irish Magnetic lines (British Association Reports, vol. iv. pages 151—156); —and if we call δ the dip at the central position; u the angle which the isoclinal line, passing through the central position, makes with the meridian; r a co-efficient determining the rate of increase of the dip in the normal direction; a and b co-ordinates of distance in longitude and latitude of the several stations from the central position, expressed in geographical miles: and if we make $r \cos u = x$, and $r \sin u = y$;—we may proceed to form equations of condition of the form described in the report on the magnetical observations in Scotland (British Association Reports, vol. v. pages 4 and 5), and to combine them by the method of least squares. It is unnecessary to encumber this report with the details of calculation; and it is sufficient to state, that from the three final equations we obtain $x = + \cdot 2633$; $y = - \cdot 5154$; $u = - 62° \,'41$ (the direction being from N. 62° ′41 E. to S. 62° ′41 W.); $r = 0' \cdot 580$, being the rate of increase of dip in each geographical mile measured in the direction perpendicular to the isoclinal line; and $\delta = 70° \,'22 \cdot 9$ the dip at the central position at the mean epoch of the observations, namely, January 1, 1837.

Mr. Lloyd's Observations.—These observations were made with a $4\frac{1}{2}$ inch circle by Robinson, and two needles, designated as L 3 and L 4, employed also for determinations of the intensity. These needles consequently had not their poles reversed; and the dips observed with them require corrections to produce the true dip. These corrections have been ascertained by Mr. Lloyd, as stated in a subsequent part of this Report, to be as follows:

Needle L 3. $+ 5' \cdot 3$
Needle L 4. $+ 13' \cdot 4$

These corrections have been applied in the following table, in the column entitled Corrected Dip.

Table XII.

Station.	1836.	Hour.	Needle.	Observed Dip.	Corrected Dip.	Place of Observation.
London ...	Apr. 19.	1 P.M.	L 3	69° 25′·0	69° 30′·3	Westbourne Green.
	Apr. 19.	1 28 P.M.	L 4	69 07·8	69 21·2	
	Apr. 21.	2 37 P.M.	L 4	69 13·8	69 27·2	
	Apr. 21.	2 58 P.M.	L 3	69 21·3	69 26·6	
Shrewsbury	Apr. 25.	2 45 P.M.	L 4	70 05·1	70 18·5	
		3 10 P.M.	L 3	70 31·4	70 36·7	
Holyhead .	Apr. 27.	11 15 A.M.	L 4	70 55·6	71 09	Rocky Height near the Town.
		11 30 A.M.	L 3	71 03·0	71 08·3	
		0 40 P.M.	L 4	70 53·4	71 06·8	
		1 7 P.M.	L 3	71 04·0	71 09·3	
		1 20 P.M.	L 3	71 04·7	71 10	
Birkenhead	Aug. 8	9 0 A.M.	L 4	70 36·2	70 49·6	Garden of the Hotel.
		9 35 A.M.	L 3	70 43·6	70 48·9	
		10 0 A.M.	L 3	70 43·0	70 48·3	
		10 20 A.M.	L 4	70 36·2	70 49·6	
Shrewsbury	Aug. 9	11 15 A.M.	L 4	70 17·1	70 30·5	Fields near the River.
		11 40 A.M.	L 3	70 22·1	70 27·4	
		0 7 P.M.	L 3	70 19·4	70 24·7	
		0 20 P.M.	L 4	10 14·6	70 28	
Hereford...	Aug. 10	10 50 A.M.	L 4	69 52·0	70 05·4	In a Plantation one mile from the Town.
		11 20 A.M.	L 3	70 02·6	70 07·9	
		11 45 A.M.	L 4	69 53·2	70 06·6	
		0 5 P.M.	L 3	70 03·2	70 08·5	
Chepstow ..	Aug. 12	11 40 A.M.	L 4	69 32·6	69 46	Near the Castle.
		0 10 P.M.	L 3	69 44·5	69 49·8	
Salisbury...	Aug. 13	10 45 A.M.	L 4	69 09·0	69 22·4	Field near the Town.
		11 10 A.M.	L 3	69 18·5	69 23·8	
Ryde	Aug. 15	11 30 A.M.	L 4	68 57·1	69 10·5	Near the Sea.
		0 0	L 3	69 01·6	69 06·9	¼ of a mile East of the Town.
	Aug. 16	0 20 P.M.	L 4	68 40·5	68 53·9	
		0 45 P.M.	L 3	68 53·8	68 59·1	
Clifton......	Aug. 29	11 15 A.M.	L 4	69 27·0	69 40·4	Durdon Downs.
		11 40 A.M.	L 3	69 39·8	69 45·1	
		0 5 P.M.	L 4	69 30·8	69 44·2	
		0 30 P.M.	L 3	69 35·4	68 40·7	
Ryde	Sept. 24	11 45 A.M.	L 4	68 49·4	69 02·8	
		0 15 P.M.	L 3	68 50·5	68 55·8	
		0 40 P.M.	L 4	68 47·8	69 01·2	
		1 10 P.M.	L 3	68 55·4	69 00·7	
Brighton...	Sept. 27	11 15 A.M.	L 3	68 43·8	68 49·1	Downs N. E. of the Town.
		11 40 A.M.	L 4	68 36·9	68 50·3	
		0 0	L 3	68 44·0	68 49·3	
		0 30 P.M.	L 4	68 36·8	68 50·2	
London ...	Oct. 4	0 45 P.M.	L 3	69 17·4	69 22·7	
		1 20 P.M.	L 4	69 02·6	69 16	
		1 40 P.M.	L 3	69 12·0	69 17·3	
		2 0 P.M.	L 4	69 06·8	69 20·2	
Cambridge	Oct. 8	0 20 P.M.	L 3	69 37·0	69 42·3	Grounds of Trinity College.
		0 40 P.M.	L 4	69 31·0	69 44·4	
		1 10 P.M.	L 3	69 30·5	69 35·8	
		1 35 P.M.	L 4	69 30·1	69 43·5	
Lynn	Oct. 10	0 55 P.M.	L 3	69 51·0	69 56·3	Pleasure-ground near the Town.
		1 25 P.M.	L 4	69 38·6	69 52	
		2 0 P.M.	L 3	69 48·5	69 53·8	
		2 20 P.M.	L 4	69 37·5	69 50·9	
Matlock ...	Oct. 12	0 15 P.M.	L 3	70 27·2	70 32·5	Field N. of the Town.
		0 25 P.M.	L 3	70 25·5	70 30·8	
		0 35 P.M.	L 4	70 13·4	70 26·8	
		0 50 P.M.	L 4	70 13·4	70 26·8	
Manchester	Oct. 14	10 50 A.M.	L 3	70 43·5	70 48·8	Field near the Town.
		11 05 A.M.	L 3	70 44·2	70 49·5	
		11 20 A.M.	L 1	70 34·4	70 47·8	
		11 35 A.M.	L 4	70 31·4	70 44·8	

Table XII. contains the latitudes and longitudes of Mr. Lloyd's stations, and the mean dip at each station: the number of distinct comparisons are, at London 2, Shrewsbury 2, Ryde 2; at each of the other places, 1: in the subsequent calculation, these numbers are taken as the weights.

TABLE XII.

Station.	Lat.	Long.	Dip.	Station.	Lat.	Long.	Dip.
	° ′	° ′	° ′		° ′	° ′	° ′
Holyhead ...	53 19	4 37	71 08·5	Chepstow ...	51 38	2 41	69 47·9
Birkenhead .	53 24	3 00	70 49·1	Clifton.........	51 27	2 36	69 42·6
Manchester.	53 28	2 14	70 47·7	Cambridge ...	52 13	−0 07	69 41·5
Matlock ...	53 08	1 35	70 29·2	Salisbury......	51 04	1 47	69 23·1
Shrewsbury.	52 42	2 46	70 27·6	London	51 32	0 11	69 22·7
Hereford ...	52 04	2 44	70 07·1	Ryde	50 44	1 10	69 01·3
Lynn.........	52 45	−0 25	69 53·2	Brighton......	50 50	0 08	68 49·7

If we combine these fourteen results by the method of least squares, we obtain the following values: $x = +\cdot 2899$; $y = -\cdot 5753$; $u = -63° \ 15'$; $r = 0\cdot 644$; and $\delta = 69° \ 54'$ at the mean geographical position, of which the latitude is 52° 4′, and the longitude 1° 43′ W.

Professor Phillips's Observations.—These were made with a six-inch circle and two needles, by Robinson. At some of the stations marked †, the reversal of the poles was intentionally omitted, from a desire to determine small local differences, under circumstances as similar as possible, the needles being very nearly equilibrated. The table shows which of the observations were thus incomplete; and the comparison of the results at the other stations, before and after the reversal of the poles, shows the probable small limit of error which may have been involved by the omission. With the poles direct, and also with the poles reversed, the mean of four positions was taken, being eight in all; the needle was always inverted on its supports, as well as the circle turned in azimuth: four readings of each end of the needle were generally taken in each position.

TABLE XIII.

Professor Phillips's Observations of the Dip.

Station.	Date.	Hour.	Needle.	Poles, α direct, β reversed.	Mean.	Mean Dip.	Place of Observation.
	1837.			° ′	° ′		
London........	May 30	2 P.M.	1	α 69 22.9			
				β 69 22·1	69 22·5		
			2	α 69 16·6		° ′ 69 20·2	Westbourne Green.
				β 69 19·1	69 17·8		
†Doncaster ...	June 2	6½ P.M.	1	α 70 25·6	70 25·6		
			2	α 70 27·6	70 27·6	70 30·1	Garden of the New Angel Inn.
	— 3	7 A.M.	1	α 70 34·3	70 34·3		
			2	α 70 33·1	70 33·1		
York...........	— 3	2½ P.M.	1	α 70 48·6			
				β 70 47·3	70 47·9		
			2	α 70 52·1			
				β 70 45·3	70 48·7		
		7 P.M.	1	α 70 48·4			
				β 70 44·5	70 46·4		
			2	α 70 45·3			
				β 70 45·3	70 45·3		
	— 5	9 A.M.	1	α 70 50·3			
				β 70 51·6	70 50·9		
			2	α 70 50·7		70 48·6	Stone in Professor Phillips's garden, and stone in the grounds of the Philosophical Society.
				β 70 51·9	70 51·3		
		11 A.M.	1	α 70 51·2			
				β 70 50·5	70 50·8		
			2	α 70 51			
				β 70 51·5	70 51·2		
		7½ P.M.	1	α 70 45·1			
				β 70 46	70 45·5		
			2	α 70 47·5			
				β 70 49	70 48·2		
Thirsk	— 6	3 P.M.	1	α 71 00			
				β 70 59·1	70 59·5		
			2	α 71 00·2		70 59·2	Garden of the Fleece Inn.
				β 70 57·5	70 58·8		
Osmotherley. .	— 6	8 P.M.	1	α 71 1·6			
				β 71 2·3	71 1·9		
			2	α 71 5·7		71 3·2	Garden of the Inn.
Hambleton				β 71 3·3	71 4·5		
End.........	— 7	9 A.M.	1	α 71 3·6			
				β 71 7·4	71 5·5		
			2	α 71 2·1		71 4·0	Top of the mountain.
				β 71 3·1	71 2·6		
Whitby	— 9	7½ A.M.	1	α 70 59·4			
				β 70 57·4	70 58·4		
			2	α 70 56·7		70 57·9	In Mr. Ripley's garden.
				β 70 57·9	70 57·3		
Flamborough.	— 11	8 P.M.	1	α 70 33·8			
				β 70 40·7	70 37·2		
			2	α 70 36		70 36·9	Garden of the Seabird's Inn
				β 70 37	70 36·5		
Scarborough. .	— 13	1 P.M.	1	α 70 40·4			
				β 70 42·5	70 41·4		
			2	α 70 42·3		70 41·9	In Dr. Murray's garden.
				β 70 41·9	70 42·1		

Station.	Date.	Hour.	Needle.	Poles. α direct, β reversed.	Mean.	Mean Dip.	Place of Observation.
	1837.			° ′	° ′		
York	June 14	11½ A.M.		α 70 47·4			
	— 14	11½ A.M.	1	β 70 47·5	70 47·4		
			2	α 70 47·5			
				β 70 48·6	70 48·0		
	— 15	4 P.M.	1	α 70 46·1			
				β 70 45·2	70 45·6		
			2	α 70 46·5		70° 46′·5	Stone in Professor Phillips's garden, and stone in the grounds of the Philosophical Society.
				β 70 49·3	70 47·9		
		8 P.M.	1	α 70 44·9			
				β 70 44	70 44·4		
			2	α 70 41·5			
				β 70 49	70 45·2		
Sheffield.......	— 17	7 P.M.	1	α 70 27·5			
				β 70 31·3	70 29·4		
			2	α 70 27·2		70 29·6	Botanic Garden.
				β 70 32·6	70 29·9		
Birmingham..	July 3	2½ P.M.	1	α 70 9·5			
				β 70 9·1	70 9·3		
			2	α 70 7·7			
				β 70 13·5	70 10·6		
	— 8	6¼ P.M.	1	α 70 4·5		70 07·2	Mr. Wreford's garden at Edgbaston.
				β 70 6·6	70 5·5		
			2	α 70 1·5			
				β 70 5·6	70 3·5		
St. Clairs near Ryde.	— 19	8¾ A.M.	1	α 68 59·1			
				β 68 58·3	68 58·7		
			2	α 68 55·3			
				β 69 0·7	68 58		
	— 20	11 A.M.	1	α 69 1·2			
				β 69 1·2	69 1·2		
			2	α 68 56·8			
				β 69 3·7	69 0·2		
	— 21	2¼ P.M.	1	α 68 59·7		69 1·2	In the garden.
				β 68 55·7	68 57·7		
			2	α 68 58·8			
				β 68 59·5	68 59·1		
	— 22	8¾ A.M.	1	α 69 6·6			
				β 69 9·7	69 8·1		
			2	α 69 3·5			
				β 69 9·9	69 6·7		
York*	Aug. 1	7 A.M.	1	α 70 48·3			
				β 70 53·6	70 50·9		
			2	α 70 32·5			
				β 71 5·3	70 48·9		
		9¼ A.M.	1	α 70 53·5			
				β 70 54·5	70 54		
			2	α 70 35			
				β 71 7·1	70 51·0		
		3 P.M.	1	α 70 52·3		70 51·1	Stone in Professor Phillips's garden, and stone in the grounds of the Philosophical Society.
				β 70 51·7	70 52		
			2	α 70 33·2			
				β 71 7·6	70 50·4		
	— 3	7½ A.M.	1	α 70 49·1			
				β 70 53·2	70 51·1		
			2	α 70 49·7			
				β 70 51·1	70 50·4		

* Needle 2 was subjected to an alteration by Robinson, after the observations of

Station.	Date.	Hour.	Needle.	Poles. α direct, β reversed.	Mean.	Mean Dip.	Place of Observation.
	1837.			° ′	° ′	° ′	
Calderstone...	Aug. 12	10¾ A.M.	1	α 70 44·6			
				β 70 45·6	70 45·1		
			2	α 70 42·6			
				β 70 49·5	70 46·0		
		1¼ P.M.	1	α 70 37·6		70 43·5	In the grounds
				β 70 42·2	70 39·9		of J. N. Walker, Esq.
			2	α 70 44			
Douglas, Isle				β 70 41·6	70 42·8		
of Man......	— 17	3 P.M.	1	α 71 20·5			
				β 71 22·7	71 21·6		
			2	α 71 23		71 22·2	Castle Mona
				β 71 22·5	71 22·7		Inn garden.
†Castleton ...	— 18	8½ A.M.	1	β 71 23·3	71 23·3		
			2	β 71 21·8	71 21·8	71 22·55	In a field ad-
†Peel Town..	— 18	2 P.M.	1	β 71 22·6	71 22·6		joining the
			2	β 71 24·7	71 24·7		Inn yard.
		3¼ P.M.	1	β 71 24	71 24	71 24·0	Near the Inn
			2	β 71 24·6	71 24·6		and on the
†Birkenhead..	— 26	1 P.M.	1	β 70 40·6	70 40·6		Castle Hill.
			2	β 70 39·8	70 39·8		
		2½ P.M.	1	α 70 38·8	70 38·8	70 39·4	Inn garden.
			2	α 70 38·6	70 38·6		
Coed Dhu....	Sept. 20	Noon	1	α 70 40·7			
				β 70 40·2	70 40·4		
			2	α 70 41·3		70 40·9	Grounds of J.
				β 70 41·5	70 41·4		Taylor, Esq.
†Bowness ...	— 25	9 A.M.	1	β 71 18·9	71 18·9		
			2	β 71 17·9	71 17·9	71 18·4	Ullocks Inn,
†Coniston ...	— 25	1 P.M.	1	β 71 19·1	71 19·1		the terrace.
			2	β 71 20	71 20	71 19·5	Field near the
†Patterdale...	— 27	1¼ P.M.	1	β 71 19·9	71 19·9		Inn garden.
			2	β 71 19·4	71 19·4	71 19·6	Inn garden.
†Penrith......	— 28	10¼ A.M.	1	β 71 23·7	71 23·7		
			2	β 71 23·2	71 23·2	71 23·4	In the Castle.
†Carlisle......	— 29	10½ A.M.	1	β 71 27·5	71 27·5		
			2	β 71 29·5	71 29·5	71 28·5	In the Castle.
†Newcastle...	— 30	7 A.M.	1	β 71 18·2	71 18·2		
			2	β 71 18	71 18	71 18·1	Fields west of
	1838.						the town.
London.........	Mar. 28	4½ P.M.	1	α 69 20·4			
				β 69 18·6	69 19·5		
			2	α 69 16·1		69 18·2	Westbourne
				β 69 17·9	69 17		Green.

Table XIV. contains the latitudes and longitudes of Mr. Phillips's stations, and the mean dip at each station reduced to the middle period of his observations, viz. the 1st of August, 1837.

the 22nd July, one of its arms having been originally longer than the other, so as sometimes to touch the circle. By shortening this arm the centre of gravity was slightly displaced, as is shown by the observation of Aug. 1. This was remedied by Mr. Phillips, the same evening, by grinding the other arm.

TABLE XIV.

Station.	Lat.	Long.	Dip, 1 Aug. 1837.	Station.	Lat.	Long.	Dip, 1 Aug. 1837.
Carlisle	54 54	2 54	71 28·5	Whitby	54 29	0 37	70 57·9
Peel Town	54 13	4 43	71 24	York	53 58	1 05	70 48·4
Penrith	54 40	2 45	71 23·4	Calderstone	53 23	2 53	70 43·5
Castleton	54 04	4 40	71 22·5	Scarborough	54 17	0 24	70 41·8
Douglas	54 10	4 27	71 22·2	Coed Dhu	53 11	3 12	70 40·9
Patterdale	54 32	2 56	71 19·6	Birkenhead	53 24	3 00	70 39·4
Coniston	54 22	3 05	71 19·5	Flamborough	54 08	0 08	70 36·9
Bowness	54 22	2 55	71 18·4	Doncaster	53 31	1 07	70 30·2
Newcastle	54 58	1 38	71 18·1	Sheffield	53 22	1 31	70 29·6
Hambleton End.	54 20	1 15	71 04	Birmingham.	52 28	1 53	70 07·2
Osmotherly	54 22	1 18	71 03·2	London	51 32	0 11	69 19·2
Thirsk	54 14	1 21	70 59·2	St. Clairs	50 44	1 08	69 01·2

If we combine these twenty-four results by the method of least squares, we obtain the following values: $x = + \cdot 2658$ $y = - \cdot 5270$; $u = -63° \; 14'$; $r = 0' \cdot 590$; and $\delta = 70° \; 50' \cdot 1$ on the 1st of August, 1837, at the mean geographical position of which the Latitude is 53° 49′, and the Longitude 2° 08′.

Captain Ross's Observations.—In this extensive series no less than fifteen needles were employed. Those designated as R L 1 and R L 2, J, C, C 2, and C 3, were four-inch needles made by Robinson, and used in a circle made by Jones; the remainder R L 3, R L 4, R 3, R 4, R 5, R 6, R 7, W 1, and W 2, were six-inch needles, also by Robinson, and used in a circle by the same artist: R 4, R 5, R 6, R 7, W 1, and W 2, were fitted with revolving axles, and were found on trial to give accordant dips in different positions of the axle: each observation with them recorded in the following tables is a mean of the usual eight positions. For these needles, consequently, no corrections are applied, and it will be seen by the observations at Westbourne Green in June, July, and December, 1838, that all these needles gave very nearly the same dip when used under like circumstances of time and place. Their mean result at Westbourne Green has been employed by Captain Ross as a standard to furnish corrections for the other needles which he had employed previously, and on which he could not rely with equal confidence. Of these, R L 1, R L 2, R L 3, and R L 4, were used for the intensity as well as for the dip, and their poles, therefore, were not reversed. They were always used in pairs, and the correction determined for the mean result of R L 1 and R L 2 was + 3, and that for R L 3 and R L 4, + 16.

The remaining five needles were observed in the usual eight positions, but in consequence of imperfect workmanship required corrections, which, by comparison with the standard needles, were assigned as follows :

J = +7	C 2 = +5	R 3 = −8
C = +2	C 3 = +8	

Wherever these needles are employed, the proper corrections are applied in a column in the table headed "corrected dip."

Table XV.

Captain J. C. Ross's Observations of the Dip.

Station.	Date.	Hour.	Needle.	Poles. α direct, β reversed.	Mean.	Corrected Dip.	Mean Dip.	Place of Observation.
	1837.	h m						
ndon........	Aug. 9	1 0 P.M.	J	α 69° 31'·8				
				β 68 46	69° 8'·9	69° 15'·9		
		3 0 P.M.	C	α 69 0·1				
				β 69 28	69 14·1	69 16·1		
			RL 1	69 27·3			69° 16'·05	Westbourne
			RL 2	68 59·1	69 13·2	69 16·2		Green, Harrow Road.
	— 10	1 10 P.M.	C 1	α 69 4·2				
				β 69 17·8	69 11	69 16		
ishey	July 31	9 A.M.	C	α 68 58·4				
				β 69 41·3	69 19·9	69 21·9		
	— 30	1 P.M.	RL 1	69 35·1				
			RL 2	69 10·1	69 22·6	69 25·6	69 24·5	In the garden
	Aug. 27	5 30 P.M.	RL 2	69 7·6				of Bushey
	— 28	10 30 A.M.	RL 1	69 38·5	69 23	69 26		Lodge.
rtington....	— 15	5 30 A.M.	J	α 69 9·2				
				β 68 25·6	68 47·4	68 54·4		
		8 A.M.	C	α 68 39·8				
				β 69 11·1	68 55·4	68 57·4	68 55·9	In the garden
		Noon	RL 1	69 8·2				of Tortington
			RL 2	68 37·6	68 52·9	68 55·9		House, near
aventry......	Sept. 1	5 15 P.M.	RL 1	69 46·8				Arundel.
	— 2	5 30 P.M.	RL 2	69 26·8	69 36·8	69 39·8		
		Noon	C	α 69 26·5				
				β 69 49·6	69 38·1	69 40·1	69 41·1	In the garden
		3 40 P.M.	J	α 69 57·5				of the Wheat-
				β 69 15·6	69 36·5	69 43·5		sheaf Inn.
rmingham..	— 4	Noon	RL 2	69 52				
		0 30 P.M.	RL 1	70 16	70 4	70 7		
		1 40	C	α 69 45·3				
				β 70 3·9	69 54·6	69 56·6	70 0·5	In a field half
		4 0	J	α 69 26				a mile south
				β 70 16	69 51	69 58		of St. Martin's
afford........	— 7	1 0 P.M.	RL 1	70 19·8				Church.
		2 20	RL 2	69 57·6	70 8·7	70 11·7		In the garden
		4 0	C	α 69 54·6				of the New
				β 70 21·6	70 8·1	70 10·1	70 9·7	Inn. Old
	— 8	4 30 P.M.	J	α 70 24·2				Church, N.
				β 69 36·4	70 0·3	70 7·3		34° W. (true)
								half a mile.

Station.	Date.	Hour.	Needle.	Poles. α direct, β reversed.	Mean.	Corrected Dip.	Mean Dip.	Place of Observatio
	1837.	h m		° ′	° ′	° ′	° ′	
Birkenhead...	Sept. 18	3 30 P.M.	RL 2	70 29				
		4 45	RL 1	70 45·2	70 37·1	70 40·1		
	— 19	9 50 A.M.	C	α 70 20·2				
				β 70 45·1	70 32·6	70 34·6	70 36·2	In the gar
		Noon	J	α 70 52·9				of the Ho.
				β 70 0·7	70 26·8	70 33·8		
Douglas, (Isle of Man).	— 21	11 A.M.	RL 1	71 22·9				
		0 30 P.M.	RL 2	71 20·7	71 21·8	71 24·8		
		2 0 P.M.	C	α 71 2				
				β 71 30·2	71 16·1	71 18·1	71 20·3	In the grou
		4 0 P.M.	J	α 71 35·7				of Castle M
				β 70 46·3	71 11	71 18		na.
Birkenhead...	Oct. 11	Noon	C	α 70 16·3				
				β 70 46·9	70 31·6	70 33·6		
		1 30 P.M.	J	α 70 51·4				
				β 69 58·6	70 25	70 32	70 35·3	In the gar
		2 15 P.M.	RL 2	70 29				of the hot
		2 45 P.M.	RL 1	70 45·8	70 37·4	70 40·4		
Pwllheli	— 14	10 45 A.M.	C	α 70 22·1				
				β 70 38·7	70 30·4	70 32·4		
		1 P.M.	J	α 70 44·1				
				β 69 56·9	70 20·5	70 27·5	70 32·5	In the gard
		2 10 P.M.	RL 1	70 40				of the Fo
		2 50 P.M.	RL 2	70 29	70 34·5	70 37·5		Crosses In
Marlborough.	— 17	1 P.M.	C	α 69 9				
				β 69 33	69 21	69 23		
		2 30 P.M.	J	α 69 40·7				
				β 68 57·3	69 19	69 26	69 25·4	In the wo
		3 50 P.M.	RL 2	69 12·2				S. W. of t
		4 30	RL 1	69 36	69 24·2	69 27·1		Castle Inn.
Clifton.........	— 21	3 50 P.M.	C	α 69 17·7				
				β 69 43·9	69 30·8	69 32·8		
	— 22	10 45 A.M.	J	α 69 50·3				
				β 69 1·3	69 25·8	69 32·8	69 34	In the gard
		1 P.M.	RL 1	69 47·2				of the Roy
		2 10 P.M.	RL 2	69 19·6	69 33·4	69 36·4		Gloucester
								Hotel.
Pembroke	— 25	2 30 P.M.	C 2	α 69 38·1				
				β 69 59·4	69 48·7	69 53·7		
	— 26	10 A.M.	J	α 70 9·6				
				β 69 26·8	69 48·2	69 55·2	69 55·9	In the gard
		11 30 A.M.	RL 1	70 4·7				of the Drag
		0 30 P.M.	RL 2	69 46·9	69 55·8	69 58·8		Inn. Per
								broke Chur
Swansea	— 27	10 20 A.M.	C 2	α 69 5·3				North (mag
				β 70 9·9	69 37·6	69 42·6		half a mile.
		0 30 P.M.	J	α 70 2·8				
				β 69 20·4	69 41·6	69 48·6	69 46·7	On the sand
		1 40 P.M.	RL 1	69 56·4				about half
		2 20 P.M.	RL 2	69 35·2	69 45·8	69 48·8		mile west
								the Pier.

Station.	Date.	Hour.	Needle.	Poles. α direct, β reversed.	Mean.	Corrected Dip.	Mean Dip.	Place of Observation.
	1827.	h m						
combe....	Nov. 2	2 40 P.M.	C 2	α 68° 59'·5				
				β 70 4·1	69° 31'·8	69° 36'·8		
	— 3	10 A.M.	C	α 69 4				
				β 70 4·6	69 34·3	69 36·3		
		11 50	J	α 69 51·2			69° 36'·9	In the garden of Rock Cottage, the residence of B. L. Coxhead, Esq.
				β 69 7	69 29·1	69 36·1		
		1 20 P.M.	RL 1	69 50·9				
		2 30	RL 2	69 19·9	69 35·4	69 38·4		
stow.......	— 14	4 15 P.M	C 2	α 68 43·8				
				β 69 56	69 19·3	69 24·3		
	— 15	8 15 A.M.	RL 2	69 14·5				
		10 A.M.	RL 1	69 36·7	69 25·6	69 28·6	69 25·1	On the sands opposite to the town.
		Noon	J	α 69 45·8				
				β 68 45	69 15·4	69 22·4		
nouth.....	— 18	10 A.M.	C 2	α 68 35				
				β 69 39·8	69 7·4	69 12·4		
		Noon	J	α 69 34·8				
				β 68 46·4	69 10·6	69 17·6	69 16·1	Pendennis Castle, bearing S 69° 17' E 2 or 3 miles.
		1 45 P.M.	RL 2	69 2·4				
		2 30	RL 1	69 28·2	69 15·3	69 18·3		Near the granite pillar at the north end of the meridian line.
d's End...	— 21	0 30 P.M.	C 2	α 68 42·9				
				β 69 37·5	69 10·2	69 15·2		
		3 P.M.	J	α 69 37				
				β 68 48	69 12·5	69 19·5	69 18·5	In a field East of the First & Last Inn in England.
	— 22	10 50 A.M.	RL 1	69 28·9				
		Noon	RL 2	69 7	69 17·9	69 20·9		
mouth	— 28	2 10 P.M.	C 2	α 68 24·3				
				β 69 27·3	68 55·8	69 0·8		
		3 50 P.M.	J	α 69 20				
				β 68 36·8	68 58·4	69 5·4	69 6·2	In the garden of the Athenæum.
	— 29	11 30 A.M.	RL 1	69 22·2				
			RL 2	68 56·8	69 9·5	69 12·5		
Exeter.........	— 30	11 50 A.M.	C 2	α 68 37·1				
				β 69 45·7	69 11·4	69 16·4		
		1 40 P.M.	J	α 69 29·2				
				β 68 46·8	69 8	69 15	69 17·3	In a field, Exeter Cathedral, S.E. 1½ mile.
		3 P.M.	RL 1	69 30·3				
		3 45 P.M.	RL 2	69 4·6	69 17·4	69 20·4		
Weymouth ...	Dec. 2	2 20 P.M.	C 2	α 68 25·1				New Church S. W. by S. ¼ of a mile.
				β 69 29·8	68 57·5	69 2·5		
		4 P.M.	J	α 69 22·8				
				β 68 35·2	68 59	69 6	69 6·7	In the garden of the Bush Hotel.
	— 4	1 45 P.M.	RL 1	69 22·3				
		3 20	RL 2	68 54·7	69 8·5	69 11·5		
Salisbury......	— 5	0 20 P.M.	C 2	α 68 30·7				
				β 69 40·8	69 5·8	69 10·8		
		1 45	J	α 69 31·3				
				β 68 44·8	69 8	69 15	69 14·5	In a field, Salisbury Cathedral w.s.w. (mag.) 1¾ mile
		3 20	RL 2	69 1·5				
		4	RL 1	69 27·7	69 14·6	69 17·6		

Station.	Date.	Hour.	Needle.	Poles. α direct, β reversed.	Mean.	Corrected Dip.	Mean Dip.	Place Observa
	1838.	h m						
Southsea	Dec. 8	11 20 A.M.	C 2	α 68° 21′·4				
				β 69 25·8	68° 53′·6	68° 58′·6		
		1 20 P.M.	J	α 69 17				
				β 68 30·6	68 53·8	69 0·8	69° 0′·4	In the g
		2 40	RL 2	68 49·2				of the
		3 30	RL 1	69 8·4	68 58·8	69 1·8		Hotel.
Guildford......	— 12	2 P.M.	C 2	α 68 20·9				
				β 69 33·7	68 57·3	69 2·3		
		3 30 P.M.	J	α 69 20·1				
				β 68 36·9	68 58·5	69 5·5	69 5·1	In a field
	— 13	11 A.M.	RL 2	68 54				mile east
			RL 1	69 14·8	69 4·4	69 7·4		Town H
London........	Mar. 6	1 P.M.	C 3	α 68 33·6				
				β 69 40·7	69 7·2	69 15·2		
	— 8	3 P.M.	C 3	α 68 33·3				
				β 69 41·6	69 7·5	69 15·5		
	April 10	1 50 P.M.	C 3	α 68 36·2				
				β 69 41·2	69 8·7	69 16·7		
		4 P.M.		α 68 36			69 14·7	Westbou
				β 69 29	69 2·5	69 10·5		Green,
	— 25	11 20 A.M.	R 3	α 69 56·1				row Ro
				β 68 49·8	69 22·9	69 14·9		
		9 A.M.	RL 3	69 3·7				
			RL 4	68 54·5	68 59·1	69 15·1		
Margate.......	— 17	2 P.M.	RL 3	68 48				
			RL 4	68 37·8	68 42·9	68 58·9		
		4 20 P.M.	R 3	α 69 32·8				
				β 68 32·2	69 2·5	68 54·5	68 57·2	In the g
			C 3	α 68 18				of the H
				β 69 22·6	68 50·3	68 58·3		Anchor
York............	— 27	2 30 P.M.	RL 3	70 33·7				
			RL 4	70 23·5	70 28·6	70 44·6		
		4 10 P.M.	R 3	α 71 19·7				
				β 70 17	70 48·3	70 40·3		
	— 28	10 A.M.	R 3	α 71 27·8			70 45·2	In the g
				β 70 25·2	70 56·5	70 48·5		of the
		Noon	RL 3	70 34·5				Keys I
			RL 4	70 28·2	70 31·4	70 47·4		
Scarborough..	May 1	1 40 P.M.	RL 3	70 32·5				
			RL 4	70 21·6	70 27·1	70 43·1		
		3 P.M.	R 3	α 71 28·3			70 43	In the g
				β 70 13·2	70 50·8	70 42·8		of the
								Inn, &
Bridlington...	— 3	9 15 A.M.	RL 3	70 27·4				to the
			RL 4	70 21·3	70 24·4	70 40·4		Church.
		11 10	R 3	α 71 24·8				
				β 70 5·4	70 45·1	70 37·1	70 38·8	In the g
		7 P.M.	RL 3	70 27·3				of the
			RL 4	70 18·7	70 23	70 39		Inn.

tation.	Date.	Hour.	Needle.	Poles. α direct, β reversed.	Mean.	Corrected Dip.	Mean Dip.	Place of Observation.
	1838.	h m						
worth	May 9	8 15 A.M.	R 3	α 71° 12′·3				
				β 69 58·9	70° 35′·6	70° 27′·6	70° 27′·5	In the grounds of Wadworth Hall, the seat of R. J. Coulman, Esq.
		1 30 P.M.	RL 3	70 15·6				
			RL 4	70 7·2	70 11·4	70 27·4		
ngham...	— 12	Noon	R 3	α 71 3·9				
				β 69 45·7	70 24·7	70 16·7		
		1 20 P.M.	RL 3	70 4·4			69 16·3	Nottingham Church, S.S.E. 1½ mile.
			RL 4	69 55·4	69 59·9	69 15·9		
h	— 16	7 15 A.M.	R 3	α 71 12·2				In a garden at Callington.
				β 69 46·2	70 29·2	70 21·2		
		9 30 A.M.	RL 3	70 4·9			70 19·5	Louth Church, S.W. 1 mile.
			RL 4	69 58·5	70 1·7	70 17·7		In the garden of the Wool-pack Inn, River-head.
er	— 21	8 A.M.	R 3	α 70 34·8				
				β 69 18·7	69 56·8	69 48·8		
		4 50 P.M.	RL 3	69 31·8			69 46·1	In the garden of the Post-office, close to the Church.
			RL 4	69 22·8	69 27·3	69 43·3		
stoffe ...	— 24	5 P.M.	R 3	α 70 14·4				
				β 68 57·4	69 35·9	69 27·9		
		6 30 P.M.	RL 3	69 16·7			69 29·2	In the grounds of the Suffolk Hotel.
			RL 4	69 12·1	69 14·4	69 30·4		
ich	— 28	3 20 P.M.	R 3	α 70 2				
				β 68 41·9	69 21·9	69 13·9		
		5 P.M.	RL 3	68 56·3				
			RL 4	69 3·5	68 59·9	69 15·9	69 15·4	In the grounds of the White Horse Inn, 2 miles west of Harwich.
	— 29	3 P.M.	RL 3	69 3·6				
			RL 4	68 57·2	69 0·4	69 16·4		
on........	June 16	1 15 P.M.	W 1	α 69 11·7				
				β 69 20·7	69 16·2			
		3 10	W 2	α 69 13·2				
				β 69 12·6	69 12·9			
	July 6	3 40 P.M.	R 4	α 69 16·1				
				β 69 11·5	69 13·7			
		7 P.M.	R 5	α 69 11		69 14·3	....	Westbourne Green, Harrow Road.
				β 69 14·6	69 12·8			
	— 7	Noon	R 6	α 69 10·1				
				β 69 17·9	69 14			
		2 10 P.M.	R 7	α 69 15·8				
				β 69 17	69 16·4			
	— 7	5 P.M.	RL 3	69 3·25				
	— 10	5 P.M.	RL 4	68 51·6	68 57·4	69 13·4		
astle	Aug. 28	2 P.M.	R 6	α 71 10·4				
				β 71 16·7	71 13·6	....		
		4 P.M.	R 4	α 71 13·4			71 13	In Mr. Newton's nursery grounds.
				β 71 11·4	71 12·4	....		

Station.	Date.	Hour.	Needle.	Poles. α direct, β reversed.	Mean.	Corrected Dip.	Mean Dip.	Place of Observati[on]
	1838.	h m		° ′	° ′			
Stonehouse....	Sept. 1	2 45 P.M.	R 6	α 71 17·3				
				β 71 27·3	71 22·3	....		
		4 15 P.M.	R 4	α 71 25·5			71 24·1	In the gro[und]
				β 71 26·4	71 25·9	....		of Stoneh[ouse]
								the seat of
London........	Dec. 4	10 45 A.M.	R 4	α 69 19·3				Sir Hew
				β 69 11·7	69 15·4	....		rymple
		0 30 P.M.	R 5	α 69 8·3				K.C.B.
				β 69 17·4	69 12·8	....		
	— 10	Noon	R 6	α 69 12·3			69 14·67	Westbour[ne]
				β 69 19·6	69 15·9	....		Green,
		2 P.M.	R 7	α 69 13·9				row Roa[d]
				β 69 14·9	69 14·4	....		

Table XVI. contains the latitudes and longitudes of Captain Ross's stations, with the mean dip at each station reduced to the 1st January, 1838, being the middle period of his observations.

TABLE XVI.

Station.	Lat.	Long.	Dip, 1 Jan. 1838.	Station.	Lat.	Long.	Dip, 1 Jan. 1838.
	° ′	° ′	° ′		° ′	° ′	° ′
Berwick ...	55 45	2 00	71 43·6	Ilfracombe ...	51 12	4 06	69 36·5
Stonehouse .	54 55	2 44	71 25·7	Clifton.........	51 27	2 35	69 33·5
Douglas ...	54 10	4 28	71 19·6	Lowestoffe ...	52 28	−1 50	69 30·2
Newcastle ...	54 58	1 36	71 14·6	Marlborough .	51 25	1 43	69 24·9
York	53 57	1 06	70 46·0	Padstow	50 33	4 56	69 24·8
Scarborough	54 18	0 26	70 43·8	Busby	51 38	0 22	69 23·6
Bridlington .	54 08	0 14	70 39·6	Land's End...	50 05	5 40	69 18·3
Birkenhead .	53 24	3 00	70 35·1	Exeter	50 43	3 31	69 17·1
Pwllheli ...	52 55	4 23	70 32·0	Harwich	51 56	−1 13	69 16·4
Wadworth...	53 28	1 07	70 26·6	Falmouth ...	50 09	5 06	69 15·8
Louth	53 19	0 0	70 18·6	London	51 32	0 11	69 15·4
Nottingham	52 57	1 08	70 15·4	Salisbury	51 04	1 48	69 14·3
Stafford	52 48	2 06	70 09·0	Weymouth ...	50 37	2 27	69 06·5
Birmingham	52 28	1 53	69 59·7	Plymouth ...	50 23	4 07	69 06·0
Pembroke...	51 39	4 54	69 55·5	Guildford ...	51 14	0 34	69 05·0
Cromer	52 56	−1 19	69 47·0	Southsea	50 48	0 58	69 00·2
Swansea ...	51 36	3 55	69 46·3	Margate	51 23	−1 23	68 57·9
Daventry ...	52 16	1 08	69 40·3	Tortington ...	50 50	0 34	68 55·0

If we combine the results at these thirty-six stations by the method of least squares, we obtain the following values: $x = +{\cdot}1974$; $y = -{\cdot}5114$; $u = -68°\ 54'$; $r = 0'{\cdot}548$; and $\delta = 69°\ 53'{\cdot}4$ at the mean geographical position of 52° 16′ N., and 1° 55′ W.

Major Sabine's Observations.—These observations were made at fifteen stations, with a 9½-inch circle, and two needles by Gambey, (Table XVII.); and at twelve stations with a circle of Nairne and Blunt of 11 inches in diameter, and a needle by Robinson, designated as S 2, (Table XVIII.)

Table XVII.

Major Sabine's Observations of the Dip with Captain Fitz Roy's Gambey.

Station.	Date.	Hour.	Needle.	Poles. α direct, β reversed.	Mean.	Mean at the Station.	Place of Observation.
Tortington ...	1837. Aug. 15		1	α 69° 05'·1 β 68 54·8	68° 59'·95	68° 59'·6	In the grounds of William Leeves, Esq.
	— 15		2	α 68 56·4 β 69 02·1	68 59·3		
Birkenhead...	Sept. 17		2	α 70 30·6 β 70 40·0	70 35·3	70 35·1	Garden of the Hotel.
	— 18		2	α 70 33·0 β 70 36·7	70 34·85		
Aberysthwith.	— 21	1 P.M.	2	α 70 20·6 β 70 26·1	70 23·35	70 23·5	On a hill north of the town.
	— 21	2 P.M.	1	α 70 29·3 β 70 17·9	70 23·60		
Dunraven Castle	— 26		1	α 69 52·1 β 69 39·9	69 46·0	69 45·7	In the grounds of the Earl of Dunraven.
	— 26		2	α 69 42·7 β 69 48·6	69 45·65		
	— 28		2	α 69 42·6 β 69 48·2	69 45·4 *		
Tortington ...	Oct. 16		2	α 68 51·8 β 68 56·6	68 54·2	68 54·8	In the grounds of William Leeves, Esq.
	—17&19		2	α 68 55·8 β 68 56·4	68 54·9 †		
Dover	Nov. 2	3½ P.M.	2	α 68 51·1 β 68 54·1	68 52·6 †	68 52·3	On, and beneath the Cliffs.
	— 7	1½ P.M.	2	α 68 51·6 β 68 53·0	68 52·3 †		
	— 6	3 P.M.	1	α 68 54·0 β 68 49·8	68 51·9 †		
Margate	— 9		2	α 68 59·4 β 69 04·1	69 01·75		
	— 9	3½ P.M.	1	α 69 09·9 β 69 00·8	69 05·3	69 02·9	Field behind Marine Terrace.
	— 11		2	α 69 04·1 β 69 03·7	69 02·54 ‡		
Regent's Park, London ...	—15&16		2	α β	69 23·9 §	69 23·8	In Mr. Jenkins's nursery grounds.
	— 16		2	α 69 20·7 β 69 25·6	69 23·1 ‖		
Lew Trenchard	1838. July 19	Noon	2	α 69 13·6 β 69 22·2	69 17·9	69 19·0	In the grounds of W. Baring Gould, Esq.
	— 21	11 A.M.	2	α 69 17·9 β 69 22·5	69 20·2		

* Observed by Viscount Adare. † Observed in various azimuths.
‡ Observed in various azimuths.
§ In various azimuths. Observers, Capt. Johnson, R.N., and Major Sabine.
‖ Observed by Capt. Johnson and Major Sabine.

Station.	Date.	Hour.	Needle.	Poles. α direct, β reversed.	Mean.	Mean at the Stations.	Place of Observation.
Falmouth ...	1838. July 25	9 A.M.	2	α 69° 09′·1 β 69 14·7	69° 11′·9	69° 11′·9	In the grounds of Robert Were Fox, Esq.
Whitehaven...	Aug. 16	3 P.M.	2	α 71 06·8 β 71 15	71 10·9	71 10·9	Fields south of the town.
Newcastle ...	— 28	1½ P.M.	2	α 71 05·8 β 71 12·2	71 09·0	71 09·0	In Mr. Newton's nursery grounds.
Alnwick Castle	— 31	3 P.M.	2	α 71 22·9 β 71 22·2	71 22·6	71 22·6	In the grounds of the Duke of Northumberland.
Stonehouse ...	Sept. 2	5 P.M.	2	α 71 18·9 β 71 20·2	71 19·5	71 19·5	In the grounds of Colonel Sir Hew Dalrymple Ross, K.C.B.
Helensburg...	— 10	8½ A.M.	2	α 72 14·8 β 72 19·2	72 17·0	72 17·0	Fields near the Baths Hotel.
Jordan Hill..	— 11	4½ P.M.	2	α 72 12·6 β 72 15·0	72 13·8	72 14·3	In the grounds of James Smith, Esq.
	— 13		2	α β	72 16·4*		
	— 13		2	α β	72 11·1*		
	— 14		2	α β	72 15·8*		
Worcester Park	Oct. 8	3 P.M.	2	α 69 03·9 β 69 09·6	69 06·7	69 06·75	In the grounds
Kew...........	— 13	4½ P.M.	2	α 69 14·7 β 69 18·2	69 16·4	69 16·45	In the garden of the Palace.

* Observed by Archibald Smith, Esq.

Table XVIII.

Major Sabine's Observations of Dip. Needle, S 2.

Station.	Date.	Hour.	Observed Dip	Mean.	Corrected Dip.	Place of Observation.
	1837.					
Tortington ……	May 17		69° 05′·8	69° 08′·25	68° 58′·65	In the grounds of William Leeves, Esq.
	— 29		69 04·8			
	Aug. 5		69 15·7			
	— 5		69 06·7			
	July 27		69 26·3	69 28·15	69 18·55	Cloister Gardens.
Westminster……	— 27		69 29·8			
Shrewsbury ……	Sept. 19	4 P.M.	70 34·2	70 34·45	79 24·85	Fields near the House of Industry.
	— 19	5 P.M.	70 34·7			
Aberysthwith …	— 21	11¼ A.M.	70 35·6	70 35·5	70 25·9	Hill north of the town.
	— 21	Noon	70 35·4			
Brecon …………	— 22	6 A.M.	70 12·6	70 12·75	70 03·15	Garden of the Hotel.
	— 22	6¾ A.M.	70 12·9			
Merthyr ………	— 23	2 P.M.	70 15·2	70 13·5	70 03·9	Mr. Thompson's grounds.
	— 23	2½ P.M.	70 11·8			
Dunraven Castle	— 25	2½ P.M.	69 58·8	69 57·4	69 47·8	In the Castle-grounds.
	— 25	3½ P.M.	69 57·9			
	Oct. 3	0¾ P.M.	69 55·5			
Tortington ……	— 15	4 P.M.	69 07·9	69 06·1	68 56·5	In the grounds of W. Leeves, Esq.
	— 19	11½ A.M.	69 04·3			
Dover …………	Nov. 2	2 P.M.	69 01·0	69 02·2	69 52·6	On and beneath the Cliffs.
	— 3	3 P.M.	69 03·8			
	— 6	2 P.M.	69 01·8			
Margate ………	— 9		69 08·2	69 10·4	69 00·8	Field behind Marine Terrace.
	— 10	11 A.M.	69 12·6			
Regent's Park, London ………	— 14	1½ P.M.	69 26·9	69 29·72	69 20·12	Mr. Jenkins' nursery-grounds.
	— 14	2¼ P.M.	69 27·2			
	— 14	3 P.M.	69 34·2			
	— 16	2½ P.M.	69 30·6			
	1838.					
Jordan Hill …..	Sept. 13	1 P.M.	72 22·0	72 21·4	72 11·8	In the grounds of J. Smith, Esq.
	— 13	2 P.M.	72 20·8			
Kew ……………	Oct. 13	1½ P.M.	69 24·0	69 24·0	69 14·4	In the gardens of the Palace.

Note on the correction applied in Tables XVIII. to the Dips observed with S 2. This needle being employed for the statical measurement of the variations of the intensity, the poles were not reversed in the dips obtained with it. The "observed dips" in Table XVIII. are consequently a mean of four positions only of the needle and circle; namely, of the circle in the azimuths 0° and 180°, and the same repeated with the needle reversed on its supports; both ends of the needle being read, and ten readings taken in each position. There are twelve stations at which the dip was thus observed with S 2; at eight of these it was also observed with Gambey's instrument, in which the poles of the needle were reversed, and the observation was consequently complete. At the other four stations Gambey's circle was not employed, and we have to deduce from the observations with S 2 the dips that would have been shown by a needle with the poles reversed. In the report of the Magnetic Observations in Scotland, (B. A. reports, vol. vi. page 98,) a correction for this purpose was derived from a comparison of results obtained at Limerick with S 2, and with a needle on Mayer's principle, used in a circle of Nairne and Blunt's; and we have here observations at eight other stations, furnishing materials for a similar comparison between the results of S 2, and of Gambey's instrument.

Table XIX.

Station.	Dips observed. S 2.	Dips observed. Mayer or Gambey.	Error of S 2. $=c$	No. of Sets. S 2. $=n$.	No. of Sets. Mayer or Gambey. $=n'$.	Weight. $\frac{nn'}{n+n'}$. $=N$.	$c \times N$.
Limerick*	71° 14′·63	71° 03′·27	+11′·36	10	5	3′·3	37′·49
Tortington (Aug.)	69 11·2	68 59·6	+11·6	2	2	1·0	11·60
Aberysthwith	70 35·45	70 23·5	+11·95	2	2	1·0	11·95
Dunraven Castle	69 57·4	69 45·7	+11·7	3	3	1·5	17·55
Tortington (Oct.) .	69 06·1	68 54·8	+11·3	2	10	1·7	19·21
Dover	69 02·2	68 52·3	+ 9·9	3	5	1·9	18·81
Margate	69 10·4	69 02·9	+ 7·5	2	5	1·4	10·50
Regent's Park ...	69 29·7	69 23·8	+ 5·9	4	8	2·5	14·75
Jordan Hill	72 21·4	72 14·3	+ 7·1	2	4	1·3	9·23
Kew Gardens......	69 24·0	69 16·45	+ 7·55	1	1	0·5	3·78
						16·1	154·87

Mean error of S 2 when the poles were not reversed.........+9·6

* The observations at Limerick with S 2 and Mayer's needle have been already detailed in the 6th Report of the British Association, page 98. As the comparison of their results is slightly affected by employing a different rate of annual decrease for the purpose of reducing the observations to a common epoch, they are stated afresh.

Needle.	Date.	No. of Sets.	Observed Dip.	January 1836.	Mean, allowing weight for the number of Sets.
S 2	July 1835	4	71° 16′·93	71° 15′·83	71° 14′·63
—	Dec. 1835	3	71 14·6	71 14·5	
—	Feb. 1836	1	71 13·4	71 13·7	
—	May 1836	2	71 12·0	71 12·9	
Mayer	Nov. 1833	2	71 11·7	71 06·6	71 03·27
—	May & June 1836	3	71 00·05	71 01·05	

A correction is therefore required of $-9' \ 6$ to all the dips observed with S 2. The application of this correction produces the final column in Table XVIII., entitled "Corrected Dips."

In Tables XVIII. and XIX., we have, then, the dip observed at fifteen stations with Gambey, and at four additional stations with S 2, making in all nineteen stations, which are inserted in the following table with their geographical positions, and the dips reduced to the mean epoch of the observations themselves, viz. the 1st January, 1838.

TABLE XX.

Station.	Lat.	Long.	Dip, Jan. 1. 1838.	Station.	Lat.	Long.	Dip, Jan. 1. 1838.
	° ′	° ′	° ′		° ′	° ′	° ′
Alnwick Castle	55 25	1 42	71 24·2	Dunraven Castle	51 28	3 37	69 45·0
Stonehouse ...	54 55	2 44	71 21·1	Regent's Park	51 34	0 10	69 23·5
Whitehaven ...	54 33	3 33	71 12·4	Lew Trenchard	50 40	4 10	69 20·3
Newcastle......	54 58	1 36	71 10·6	Kew Gardens...	51 29	0 18	69 18·3
Birkenhead ...	53 24	3 00	70 34·4	Westminster ...	51 31	0 07	69 17·5
Shrewsbury ...	52 43	2 45	70 24·2	Falmouth	50 09	5 06	69 13·3
Aberysthwith	52 24	4 05	70 22·8	Worcester Park	51 23	0 17	69 08·6
Merthyr	51 43	3 21	70 03·2	Margate	51 23	−1 23	69 02·6
Brecon	51 57	3 21	70 02·5	Tortington	50 50	0 34	68 55·5
				Dover	51 08	−1 19	68 51·9

Combining these by the method of least squares, we obtain the following values: $x = +\cdot 2305$; $y = -\cdot 498$; $u = -65° \ 08'$; $r = \cdot 548$; and $\delta = 69° \ 56'\cdot 6$ at the mean geographical position, of which the latitude is 52° 18′, and the longitude 1° 59′.

If now we collect in one view the several values of u and r which have been thus obtained from the observations in England, we have as follows:

TABLE XXI.

Observer.	No. of Stations.	Mean Geographical Position.		Values of	
		Lat.	Long.	u.	r.
		° ′	° ′	° ′	
Fox	29	52 45	2 49	−62 41	0·580
Lloyd...............	14	52 04	1 43	−63 15	0·644
Phillips	24	53 49	2 08	−63 14	0·590
Ross	36	52 16	1 55	−68 54	0·548
Sabine	19	52 18	1 59	−65 08	0·548

If we regard the several values of u and r as entitled to weight proportioned to the number of stations of which each is the representative, we obtain $-65°\ 05'$ and $0'{\cdot}575$ as the mean values of u and r derived from the English series, corresponding to the central geographical position $52°\ 38'$ N., and $2°\ 07'$ W.

Section II.—Scotland.

Observations of Captain J. C. Ross.—These observations were made with Robinson's six-inch circle, and the needles R 4, R 5, R 6, and R 7, which have been already described.

Table XXII.

Captain J. C. Ross's Observations of the Dip, Scotland.

Station.	Date.	Hour.	Needle.	Poles. α direct, β reversed.	Mean.	Mean Dip.	Place of Observation.
	1838.			° ′	° ′	° ′	
Aberdeen ...	July 18.	4 P.M.	R 4	α 72 28·7			
				β 72 25·7	72 27·2		In a field one mile south of the city.
		1 P.M.	R 5	α 72 30		72 27·6	
				β 72 25·8	72 27·9		
Lerwick	— 24	Noon	R 4	α 73 50·3			
				β 73 46·9	73 48·6		
		2·15 P.M.	R 5	α 73 43			
				β 73 41·6	73 42·3		
	— 25	Noon	R 6	α 73 41·8			
				β 73 46·4	73 44·1		Gardie-House, Brassa Island.
	— 27	Noon	R 6	α 73 44·8		73 44·9	
				β 73 47·8	73 46·3		
		1·40 P.M.	R 4	α 73 48·9			
				β 73 43·3	73 46·1		
		3 P.M.	R 5	α 73 43·2			
				β 73 41	73 42·1		

Station.	Date.	Hour.	Needle.	Poles. α direct, β reversed.	Mean.	Mean Dip.	Place of Observation.
	1838.	h m		° ′	° ′	° ′	
Kirkwall......	July 31	1 0 P.M.	R 4	α 73 24·7			In the garden of the Caledonian Hotel.
				β 73 20·1	73 22·4		
		2 40 P.M.	R 5	α 73 20·4			
				β 73 19·4	73 19·9	73 20·4	
		6 40 P.M.	R 6	α 73 16·9			
				β 73 21·1	73 19		
Wick	Aug. 8	Noon	R 4	α 73 27·6			In the garden of Rosebank, the seat of Mr. M'Leay.
				β 73 18·3	73 22·9		
		2 P.M.	R 5	α 73 15·7			
				β 73 17·1	73 16·4	73 19·9	
		3 20 P.M.	R 6	α 73 17·5			
				β 73 23·3	73 20·4		
Golspie	— 10	3 P.M.	R 6	α 73 0·3			Dunrobin Castle, E. ¾ of a mile.—In the wood.
				β 73 6·8	73 3·5		
		4 30 P.M.	R 4	α 73 7·3		73 4·3	
				β 73 3·2	73 5·2		
Inverness......	— 13	3 P.M.	R 4	α 72 51·11			In the garden of the Caledonian Hotel.
				β 72 43·1	72 47·2		
		4 15 P.M.	R 6	α 72 39·5			
				β 72 47·4	72 43·5	72 46·2	
	— 14	1 20 P.M.	R 6	α 72 46·1			
				β 72 49·6	72 47·8		
Culgruff	Sept. 8	2 P.M.	R 4	α 71 36·4			In the grounds of Culgruff, the seat of George Clark Ross, Esq.
				β 71 37·3	71 36·8		
		3 20 P.M.	R 6	α 71 31·8		71 35·7	
				β 71 37·6	71 34·7		
Jordan Hill .	— 13	1 15 P.M.	R 6	α 72 15·6			In the grounds of Jordan Hill, the seat of J. Smith, Esq.
				β 72 19·8	72 17·7		
	— 14	1 15 P.M.	R 4	α 72 21·6		72 20	
				β 72 22·8	72 22·2		
Berwick	— 17	2 30 P.M.	R 6	α 71 35			In a garden half a mile north of the Scotch Gate.
				β 71 41·6	71 38·3		
	— 18	Noon	R 4	α 71 46		71 41·9	
				β 71 45·1	71 45·6		
Dunkeld	— 20	2 30 P.M.	R 6	α 72 23·9			In a plantation of larch, Craigie Barns, S.W. by W. three or four miles.
				β 72 23·1	72 23·5		
		4 20 P.M.	R 4	α 72 26·8			
				β 72 22·8	72 24·8		
	— 21	Noon	R 5	α 72 22·2		72 23·1	
				β 72 22·2	72 22·2		
		2 40 P.M.	R 7	α 72 18·9			
				β 72 24·9	72 21·9		

Table XXIII. contains the latitudes and longitudes of Captain Ross's Scottish stations, and the mean dip at each station at the dates shown in the preceding table. The whole interval in which they are comprised is so short, that no reduction to a common epoch has been applied.

Table XXIII.

Station.	Lat.	Long.	Dip.	Station.	Lat.	Long.	Dip.
	° ′	° ′	° ′		° ′	° ′	° ′
Lerwick . .	60 09	1 07	73 44·9	Aberdeen ...	57 09	2 05	72 27·6
Kirkwall ...	59 00	2 58	73 20·4	Dunkeld	56 35	3 33	72 23·1
Wick.........	58 24	3 05	73 19·9	Jordan Hill...	55 54	4 21	72 20·0
Golspie......	57 58	3 57	73 04·3	Berwick	55 45	2 00	71 41·9
Inverness ...	57 28	4 11	72 46·2	Culgruff	54 58	4 00	71 35·7

If we combine these ten results by the method of least squares, we obtain the following values: $x = + \cdot 250$; $y = - \cdot 484$; $u = -62° 39'$; $r = 0' \cdot 545$; and $\delta = 72° 40' \cdot 8$ at the mean geographical position 57° 20′ N., and 3° 08′ W., and at the mean epoch August 18, 1838.

Major Sabine's Observations.—These observations were made at twenty-seven stations in the summer of 1836, with a circle by Nairne and Blunt, and the needle S 2 of Robinson. The details have been already published in the 5th vol. of the Reports of the British Association, and need not therefore be repeated in this place. When that Report was published, the correction of S 2 was provisionally taken as $-12'$; it has since been more correctly ascertained to be $-9' \cdot 5$ by a much more extensive series of comparative observations; (Table XIX.) The subjoined table (XXIV.) contains the latitudes and longitudes of the twenty-seven stations, and the dips, to which the new correction of $-9' \cdot 5$ has been applied. As the whole of these observations were comprised within an interval of six weeks, no reduction to a mean epoch has been thought necessary.

Table XXIV.

Station.	Lat.	Long.	Dip.	Station.	Lat.	Long.	Dip.
	° ′	° ′	° ′		° ′	° ′	° ′
Tobermorie ...	56 38	6 01	73 07·6	Loch Ridan ...	55 57	5 10	72 16·6
Loch Scavig...	57 14	6 07	73 05·2	Castle Duart ...	56 31	5 45	72 15·2
Loch Slapin...	57 14	6 02	73 02·1	Braemar	57 01	3 25	72 14·1
Golspie.........	57 58	3 57	72 55·5	Kirkaldy.........	56 07	3 09	72 10·9
Inverness......	57 27	4 11	72 46·4	Loch Gilphead .	56 04	5 28	72 07·6
Artornish......	56 33	5 48	72 42·8	Glasgow	55 51	4 14	72 01·6
Gordon Castle	57 37	3 09	72 40·8	Great Cumbray	55 48	4 52	72 01·1
Fort Augustus	57 08	4 40	72 40·3	Campbeltown...	55 23	5 38	71 55·9
Rhynie.........	57 20	2 50	72 25·6	Blairgowrie......	56 36	3 18	71 54·7
Loch Ranza...	55 42	5 17	72 22·9	Edinburgh	55 57	3 11	71 50·3
Alford	57 13	2 45	72 21·9	Loch Ryan......	54 55	4 59	71 43·3
Newport	56 25	2 55	72 17·4	Melrose	55 35	2 44	71 36·8
Glencoe	56 39	5 07	72 17·1	Dryburgh	55 34	2 39	71 33·6
Helensburg ...	56 0	4 41	72 16·7				

If we combine these twenty-seven results by the method of least squares, we obtain the following values: $x = +{\cdot}337$; $y = -{\cdot}461$; $u = -53° \ 47'$; $r = 0{\cdot}571$; $\delta = 72° \ 18'{\cdot}7$ at the mean geographical position 56° 28′ N., and 4° 19′ W., and at the mean epoch September 1, 1836.

Mr. Fox's Observations.—These observations were made with Mr. Jordan's 7-inch circle and needle, and are as follows:

Table XXV.

Mr. Fox's Observations of the Dip in Scotland.

Station.	Date.	Hour.	Lat.	Long.	Dip.	Place of Observation.
	1837.		° ′	° ′	° ′	
Melrose	Aug. 26	11½ A.M.	55 35	2 44	71 38	East of the Abbey.
Edinburgh ...	— 28	8 A.M.	55 57	3 11	71 47	Gard. opposite Princes St.
Edinburgh ...	— 28	6 P.M.	55 57	3 11	71 53	Botanic Garden.
Linlithgow ...	— 30	1½ P.M.	55 59	3 37	71 59	Near ruins of the Palace.
Inverary	— 31	6 P.M.	56 15	5 04	72 7	In the Park.
Loch Lomond	Sept. 1	5 P.M.	56 13	4 40	72 15	Lakeside near Tarbet.
Glasgow	— 4	4½ P.M.	55 51	4 14	72 5	Botanic Garden.
Moffat	— 6	8 A.M.	55 20	3 27	71 40	Near the Inn.
Gretna Green	— 6	2 P.M.	55 01	3 04	71 29	Behind the Inn.

The portion of country over which these observations extend is too limited to afford an advantageous combination for the deduction of the values of u and r; I have therefore combined them with my own twenty-seven results in Table XXIV., forming an united series of thirty-six stations towards the final deduction of the values of u and r in Scotland, Mr. Fox's observations having been previously reduced to September 1836. From this combination we obtain the following values; $x = +{\cdot}320$; $y = -{\cdot}447$; $u = -54° 20'$; $r = 0{\cdot}550$; $\delta = 72° 13{\cdot}2$ at the mean geographical position 56° 18′ N., and 4° 10′ W.

If we collect in one view the values of u and r which have been thus obtained from the observations in Scotland, we have as follows:

TABLE XXVI.

Observer.	No. of Station.	Mean Geographical Position.		Values of	
		Lat.	Long.	*u*.	*r*.
		° ′	° ′	° ′	
Ross	10	57 20	3 08	−62 39	0·545
Sabine and Fox......	36	56 18	4 10	−54 20	0·550

Regarding the values of u and r as entitled to weight proportioned to the number of stations of which each is the representative, we obtain $u = -56° 06'$, and $r = 0{\cdot}549$, as the mean values derived from the observations in Scotland, and corresponding to the central geographical position of 56° 49′ N., and 3° 39′ W.

Section III.—Ireland.

(*This Section is by the Rev.* H. Lloyd.)

Before entering into the details connected with this division of our memoir, it will be necessary to make a few remarks upon the principles of the calculation which has been employed in deducing the position of the isoclinal lines from the scattered observations.

If z denote the dip (or intensity) at any station of observation; z_0 that at some *near* station, which is taken as the origin of co-ordinates; and x and y the actual distances (in geographical miles) between the stations, estimated on the parallel of latitude and on the meridian, respectively,—or the co-ordinates of position of the first station referred to the latter as an origin; then I have shown*, (Fifth Report, p. 151) that the relation of these quantities is expressed approximately by the equation

$$z - z_0 = \mathrm{M}x + \mathrm{N}y; \qquad (1)$$

in which M and N represent the increase of the dip (or intensity), corresponding to each geographical mile of distance in the two directions.

In employing this equation in the calculation of the isoclinal and isodynamic lines, I had taken one of the stations of observation—namely, Dublin—as the origin of co-ordinates: observation, therefore, gave the values of z and z_0, and the equations of condition thus obtained were combined, by the method of least squares, so as to give the most probable values of M and N. In a subsequent application of this method, (Sixth Report, p. 99) Major Sabine adopted a better course, and took an *arbitrary station*, with an *unknown* dip and intensity, as the origin. z_0 was thus unknown, as well as M and N; and the resulting equations gave not only the most probable values of the increase of the dip (or intensity) in the two directions, but likewise that of its absolute amount at some one station.

Let this latter quantity be denoted by L, i. e. let $z_0 = \mathrm{L}$ in the preceding equation; then each observation will furnish an equation of condition of the form

$$\mathrm{L} + \mathrm{M}x + \mathrm{N}y = z. \qquad (2)$$

Combining these equations by the method of least squares, we have the three following final equations:

* The notation here used is somewhat different from that employed in the Report. The variation can cause no embarrassment to the reader.

$$
\begin{aligned}
&\mathrm{L}\,\Sigma\,(w) + \mathrm{M}\,\Sigma\,(w\,x) + \mathrm{N}\,\Sigma\,(w\,y) = \Sigma\,(w\,z),\\
&\mathrm{L}\,\Sigma\,(w\,x) + \mathrm{M}\,\Sigma\,(w\,x^2) + \mathrm{N}\,\Sigma\,(w\,x\,y) = \Sigma\,(w\,x\,z),\\
&\mathrm{L}\,\Sigma\,(w\,y) + \mathrm{M}\,\Sigma\,(w\,x\,y) + \mathrm{N}\,\Sigma\,(w\,y^2) = \Sigma\,(w\,y\,z);
\end{aligned}
\tag{3}
$$

in which w denotes the *weight* of the determination, and the symbol Σ the *sum* of the n values of the quantities within the brackets, n being the number of separate determinations. From these equations, the most probable values of the three unknown quantities, L, M, N, are obtained by elimination.

If the point taken for the origin of the co-ordinates be that for which

$$\Sigma\,(w\,x) = 0, \quad \Sigma\,(w\,y) = 0;$$

or be, as it were, the *centre of gravity* of the stations, the final equations are reduced to

$$
\begin{aligned}
\mathrm{L}\,\Sigma\,(w) &= \Sigma\,(w\,z),\\
\mathrm{M}\,\Sigma\,(w\,x^2) + \mathrm{N}\,\Sigma\,(w\,x\,y) &= \Sigma\,(w\,x\,z),\\
\mathrm{M}\,\Sigma\,(w\,x\,y) + \mathrm{N}\,\Sigma\,(w\,y^2) &= \Sigma\,(w\,y\,z).
\end{aligned}
$$

The values of L, M, N being obtained, we may apply the equation (2) either to determine the value of z, when x and y are given, i.e., to deduce the *most probable value of the dip* for a given place,—or, conversely, to infer the relation of x and y when z is given, i. e. to determine the *equation of the line* passing through all the points of *given dip*. In this latter application let $z - \mathrm{L} = \mathrm{K}$; the equation of the line then is

$$\mathrm{M}\,x + \mathrm{N}\,y = \mathrm{K}, \tag{4}$$

x and y being the co-ordinates, measured along the parallel of latitude and the meridian respectively. On this supposition, then, the isoclinal line is a *right line*; the angle which it makes with the meridian is

$$ang\left(tan = -\frac{\mathrm{N}}{\mathrm{M}}\right); \tag{5}$$

and the increase of the dip corresponding to each geographical mile of distance, in a direction perpendicular to the line, is

$$\sqrt{\mathrm{M}^2 + \mathrm{N}^2}. \tag{6}$$

In this mode of computation it is assumed, not only that the portion of the earth over which the observations extend may be treated as a plane surface, but also that the differences of dip (or intensity) are *linear* functions of the differences of latitude and longitude,—in other words, that the isoclinal and isodynamic lines are *straight*. This supposition may be safely made, where the district of observation, itself inconsiderable in extent, is remote from the poles of dip or of intensity;

for in such cases the curvature of the lines not being rapid, the curve itself may, for a small portion of its extent, be confounded with its tangent. It suggests perhaps the best mode of determining with precision the empirical laws of the distribution of terrestrial magnetism; namely, by means of *small groups* of observations, each of which will give, by this method, not a *point* in the curve merely, but a portion of its *tangent*.

The extent of the district in which this method is available will, of course, vary with the curvature of the lines on the earth's surface, becoming more and more limited as we approach the poles. Where the flexure of the lines is rapid, and we seek, nevertheless, to combine the observations scattered over a moderately extensive tract of country, it becomes necessary to obtain some means of pushing the approximation further.

Such means readily present themselves. Whatever be the *laws* of distribution of magnetism on the surface of the earth, it is manifest that the dip (or intensity) at any station is a function of its co-ordinates of position; or that

$$z = \mathrm{F}(\alpha, \beta),$$

α and β denoting the co-ordinates of the station (in parts of radius) referred to some neighbouring station as an origin. Accordingly,

$$z = (z) + \left(\frac{dz}{d\alpha}\right)\alpha + \left(\frac{dz}{d\beta}\right)\beta + \tfrac{1}{2}\left(\frac{d^2 z}{d\alpha^2}\right)\alpha^2 + \left(\frac{d^2 z}{d\alpha\, d\beta}\right)\alpha\beta$$
$$+ \tfrac{1}{2}\left(\frac{d^2 z}{d\beta^2}\right)\beta^2 + \&c.$$

the brackets denoting the particular values of the derived functions, when $\alpha = 0$, $\beta = 0$. The quantities α and β, in the preceding equation, being small, we may push the approximation as far as we please, by including a greater number of terms in the development.

Let the co-ordinates of linear distance be denoted, as before, by x and y,

$$\alpha = \frac{x}{r},\ \beta = \frac{y}{r};$$

r being the radius of the earth. Substituting these values in the preceding equation, and making

$$\mathrm{L} = (z),\ \mathrm{M} = \frac{1}{r}\left(\frac{dz}{d\alpha}\right),\ \mathrm{N} = \frac{1}{r}\left(\frac{dz}{d\beta}\right),\ \mathrm{P} = \frac{1}{2r^2}\left(\frac{d^2 z}{d\alpha^2}\right),$$
$$\mathrm{Q} = \frac{1}{r^2}\left(\frac{d^2 z}{d\alpha\, d\beta}\right),\ \mathrm{R} = \frac{1}{2r^2}\left(\frac{d^2 z}{d\beta^2}\right),\ \&c.$$

we have

$$z = \mathrm{L} + \mathrm{M}x + \mathrm{N}y + \mathrm{P}x^2 + \mathrm{Q}xy + \mathrm{R}y^2 + \&c. \qquad (7)$$

If we retain only the terms of this equation in which x and y are of the first dimension, we have the equation (2) already obtained.

To advance another step in the approximation, we should include the terms in which x and y are of the *second* dimension; and we shall thus have six unknown coefficients L, M, N, P, Q, R, to be determined. For this purpose, the equations (in number the same as the stations of observation) are to be combined by the method of least squares; and the six resulting equations will give, by elimination, the quantities sought.

The coefficients L, M, N, &c. being known, the line of *given dip* is

$$\mathrm{R}\, y^2 + \mathrm{Q}\, x\, y + \mathrm{P}\, x^2 + \mathrm{N}\, y + \mathrm{M}\, x = \mathrm{K}, \tag{8}$$

in which K denotes, as before, the particular value of $z - (z)$. Here, then, the isoclinal line is of the *second order*; and its species is determined by the relation of the first three coefficients, P, Q, R. The equation of the curve being found, it is easy to construct it graphically by points.

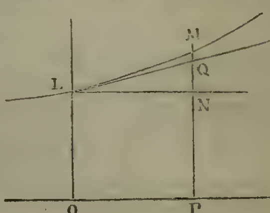
The preceding solution of the problem is probably sufficient for all purposes; but the determination of six unknown quantities by the method of least squares, when the equations of condition are numerous, is a formidable labour; and it is therefore important to consider whether we can safely stop short at any step of less generality. Now it is easily seen that in most cases to which we have to apply this method, the isoclinal line may be represented by the equation

$$\mathrm{P}\, x^2 + \mathrm{N}\, y + \mathrm{M}\, x = \mathrm{K}, \tag{9}$$

in which there are only four coefficients to be determined*. This equation (considered as belonging to a plane curve) is that of a *parabola*.

The equation, being *linear* in *one* of the co-ordinates, is very easily constructed by points.

* This is evident from geometrical considerations.



Let L M be a portion of the curve, referred to the axes of co-ordinates O P, O L; and let L Q be its tangent at the point L, making with the axis of abscissæ an angle whose tangent is a. The ordinate of the curve P M, is equal to P Q + Q M. But P Q, the ordinate of the tangent, is equal to $a\,x + b$, b denoting the ordinate at the origin, O L. And the sagitta Q M, is proportional to Q L^2, the arc being small in proportion to the radius of curvature; i. e. $\mathrm{Q\,M} = k \times \mathrm{Q\,L}^2 = k\,(1 + a^2)\,x^2 = c\,x^2$. Hence

$$y = b + a\,x + c\,x^2.$$

The object proposed in the preceding method has been attained by Major Sabine by a different process, which will be applied by him in the sequel. It is therefore unnecessary to make any application of that here laid down.

In combining the equations of condition by the method of least squares, it is manifest that we cannot, in general, allow *equal weight* to all. The result obtained at one station may be derived from a single observation only; while, at another, it may be the *mean* of several observations, made at different times, and with different instruments. In a former discussion of the observations in Ireland, weights were assigned to the results at each station, but on arbitrary and uncertain principles. I now proceed to remedy this defect; and I do so the more willingly, both on account of the great importance of this branch of the theory of probabilities in Physical science, and because the results to be referred to are connected with researches not as well known as they deserve.

Let x_1, x_2, x_3, &c., x_n, be n values of the quantity x, obtained by separate and independent observations; and let a denote their arithmetical mean, so that

$$a = \frac{1}{n}\left(x_1 + x_2 + x_3 + \text{\&c.} + x_n\right);$$

then the *probable error* of this mean, i. e. the limit on either side of which there are *equal chances* of the actual error lying, is given by the formula

$$E^2 = \frac{2\rho^2 \Sigma (x - a)^2}{n(n-1)}, \tag{10}$$

in which $\Sigma (x - a)^2$ denotes the sum of the squares of the differences of the several partial results and the mean, or the value of

$$(x_1 - a)^2 + (x_2 - a)^2 + \text{\&c.} + (x_n - a)^2;$$

and in which, also, ρ is the number which satisfies the equation

$$\int_0^\rho e^{-t^2} dt = \tfrac{1}{4}\sqrt{\pi}.$$

Numerically, $\rho = 0{\cdot}4769$; and substituting in (10)

$$E^2 = \frac{{\cdot}4549\, \Sigma (x - a)^2}{n(n-1)} \tag{11}$$

The probable error of a *single* result, as deduced from comparison with the rest, is in like manner given by the formula

$$\epsilon^2 = \frac{\cdot 4549 \, \Sigma \, (x - a)^2}{n - 1} \tag{12}$$

so that $\epsilon^2 = n \, E^2$. The *weights*, in both cases, are measured by the inverse of the squares of the probable errors; that is

$$W \, E^2 = 1, \; w \, \epsilon^2 = 1, \tag{13}$$

w and W denoting the weights of the single result, and of the mean, respectively*.

When the quantity sought is a *linear* function of two or more unknown quantities, which latter are obtained immediately by observation, its probable error is connected with those of the quantities on which it depends by a very simple relation.

Let x and y be the quantities sought by *immediate* observation, and let the quantity actually sought, z, be a linear function of these, expressed by the equation

$$z = p \, x + q \, y.$$

Let a denote the arithmetical mean of m observations of the unknown quantity x; b the mean of n observations of y; and let E_x and E_y be their *probable errors*, or the limits on either side of which there are *equal chances* of the actual errors, $x - a$, $y - b$, being found. Then the probable error of z, E_z, is expressed by the formula†

$$E_z^2 = p^2 \, E_x^2 + q_2 \, E_y^2. \tag{14}$$

The case of a linear function includes every case in which the quantities sought are already approximately known. We have only to substitute for these quantities their approximate values *plus* the unknown corrections, and to neglect the squares and higher powers of the latter.

To apply these principles to an important case,—let it be required to determine the probable error (or the weight) of the mean dip at a given station, as deduced from n_o observations, with n_i instruments.

The true dip being equal to the observed dip *plus* the instrumental correction, it is manifest that, in this case,

$$E^2 = E^2_{\,o} + E^2_{\,i};$$

* For the demonstration of these theorems, the reader is referred to a paper by Prof. Encke, in the *Astronomisches Jahrbuch* for the year 1834. See also a paper by M. Poisson on the same subject in the *Connoissance des Temps*, 1827.

† See a paper by M. Poisson in the *Bulletin Universel des Sciences*, tome xiii. p. 266. See also the Memoir by Prof. Encke, already referred to.

E_o denoting the error of observation, and E that due to the imperfection of instruments. But

$$E_o^2 = \frac{\epsilon_o^2}{n_o}, \; E_i = \frac{\epsilon_i^2}{n_i},$$

ϵ_o denoting the probable error of a single observation, and ϵ_i that of a single instrument. Hence

$$E^2 = \frac{\epsilon_o^2}{n_o} + \frac{\epsilon_i^2}{n_i}. \quad (15)$$

We have here taken no *separate* account of the error arising from the *variations* of the dip, that error being inseparably combined with the error of observation; the symbol ϵ_o, therefore, in the preceding, denotes the probable error resulting from the two conjoint sources.

In order to estimate the value of ϵ_o, I have taken the following series of observations, made with the needles, L. 1, L. 4, in Dublin, the longest series of observations made with the same instrument at a single station in Ireland. The 1st column of the table contains the *dates* of observation; the 2nd the *observed dips* (uncorrected); the 3rd the *reduced* dips, referred to the 1st of January, 1836. In the 4th column are the differences between the partial results and the mean; and in the 5th, the squares of these differences.

Table XXVII.

Needle L. 1.

Date.	Observed Dip.	Reduced Dip. $=x$	$x-a$	$(x-a)^2$
Oct. 21, 1833	70° 56′·4	70° 51′·2	− 0·4	0·16
Aug. 7, 1834	70 51·6	70 48·2	− 3·4	11·56
— 8,	70 57·6	70 54·2	+ 2·6	6·76
— 9,	70 54·3	70 50·9	− 0·7	·49
— 19,	70 49·5	70 46·1	− 5·5	30·25
Sept. 22,	70 56·0	70 53·0	+ 1·4	1·96
— 23,	70 53·8	70 50·8	− 0·8	·64
Sept. 4, 1835	70 46·7	70 45·9	− 5·7	32·49
— 5,	70 55·6	70 54·8	+ 3·2	10·24
— 7,	70 54·2	70 53·4	+ 1·8	3·24
— 9,	70 54·4	70 53·6	+ 2·0	4·00
— 14,	70 56·7	70 55·9	+ 4·3	18·49
— 15,	70 53·3	70 52·5	+ 0·9	·81

Table XXVIII. Needle L. 4.

Date.	Observed Dip.	Reduced Dip.	$x-a$	$(x-a)^2$
Sept. 22, 1834.	71° 2′·2	70° 59′·2	+ 9·1	82·81
— 23,	70 53·8	70 50·8	+ 0·7	0·49
— 29,	70 44·8	70 41·8	− 8·3	68·89
Oct. 25,	70 54·1	70 51·3	+ 1·2	1·44
Aug. 19, 1835.	70 51·6	70 50·8	+ 0·7	0·49
Sept. 4,	70 43·6	70 42·8	− 7·3	53·29
— 5,	70 52·8	70 52·0	+ 1·9	3·61
— 7,	70 52·2	70 51·4	+ 1·3	1·69
— 9,	70 46·2	70 45·4	− 4·7	22·09
— 14,	70 53·4	70 52·6	+ 2·5	6·25
— 15,	70 55·0	70 54·2	+ 4·1	16·81
Nov. 5,	70 49·6	70 49·2	− 0·9	0·81
— 5,	70 45·8	70 45·4	− 4·7	22·09
— 6,	70 53·9	70 53·5	+ 3·4	11·56
Apr. 11, 1836.	70 48·1	70 48·9	− 1·2	1·44
— 15,	70 47·1	70 47·9	− 2·2	4·84
May 7,	70 50·9	70 51·7	+ 1·6	2·56
— 9,	70 56·4	70 57·2	+ 7·1	50·41
Aug. 5,	70 43·1	70 44·5	− 5·6	31·36
— 6,	70 51·3	70 52·7	+ 2·6	6·76

From the former of these tables we find

$$n = 13,\ a = 70°\ 51'{\cdot}6,\ \Sigma\,(x-a)^2 = 121{\cdot}09\,;$$

and from the latter

$$n = 20,\ a = 70°\ 50'{\cdot}1,\ \Sigma\,(x-a)^2 = 389{\cdot}69.$$

Substituting these numbers in (12), the probable error of observation in the former series is found to be $2'{\cdot}1$; and in the latter $3'{\cdot}0$.

It is remarkable that the squares of these errors (the inverse of which are the measures of the weights) are, almost exactly, in the ratio of 1 to 2; that is, in the inverse ratio of the number of readings with each needle. This is a curious confirmation of the accuracy of the conclusion.

From the preceding it follows, that in combining the results of the two needles, L. 1 and L. 4, (when used together) *double* weight must be allowed to the former. It appears from (14) that the probable error of the mean, thus deduced, is $1'{\cdot}8$. We may therefore consider *two minutes* as the probable error of observation in the present series, whether the result be that of a single needle with the usual number of readings, or the mean of the two needles L. 1 and L. 4.

The *probable instrumental error*, ϵ_i, varies, of course, within very wide limits, depending on the perfection of workmanship. In a former part of this memoir, Major Sabine has pointed out the very great improvement which our English dipping needles have undergone in this respect, subsequently to the year 1835*. The mean error, for any set of needles, may be obtained from (15), when we have made a series of observations with these needles at any one station. Let ϵ denote the probable error of the result given by any set of observations with a single needle, as inferred from comparison with the others; Then $\epsilon^2 = n_i \mathrm{E}^2$, and substituting in (15), we have

$$\epsilon^2_i = \epsilon^2 - \frac{n_i}{n_o} \epsilon^2_o,$$

in which the value of ϵ^2 is deduced from the observations by means of (12).

To deduce, according to these principles, the value of ϵ_i for the needles employed in the Irish survey, we must compare the results obtained at Limerick,—that being the only station where all the needles were employed. These results are contained in the following table. The first column contains the names of the needles employed; the second, the dips obtained, reduced to the 1st of January, 1837, of which the mean value is 71° 0′·5; in the 3rd column are the differences of the partial results and the mean; and in the 4th, the squares of these differences.

TABLE XXIX.

Needle.	Dip $= x$.	$x - a$	$(x - a)^2$
S. 2	71° 2′·6	+ 2·1	4·41
M	71 1·4	+ 0·9	0·81
S. 1 †	70 57·6	− 2·9	8·41
S. 1 †	70 59·1	− 1·4	1·96
L. 1	71 4·7	+ 4·2	17·64
L. 4	70 57·7	− 2·8	7·84

From the last column of the preceding table we find
$\Sigma (x - a)^2 = 41{\cdot}07$; and substituting in (12), $\epsilon^2 = 3{\cdot}70$.

Again, $n_i = 6$, $n_o = 26$, and, assuming $\epsilon_o = 2$, $\frac{n_i}{n_o} \epsilon^2_o = 0{\cdot}92$.

* The probable instrumental error of the needles employed at Westbourne Green in 1835, as deduced from the observations recorded in the Irish Report (Fifth Report, p. 142), amounts to 8′·3. The mean probable error of the needles employed at the same place in 1837 and 1838, as deduced from the observations contained in Table III. of the present memoir, is about *one minute* only.

† The needle S. 1 had undergone a change in the disposition of its axle in the interval between the two observations recorded in this table. These observations must therefore (as far at least as the axle is concerned) be regarded as the results of *different* instruments.

We have, therefore, from the preceding formula, $\epsilon^2_i = 2{\cdot}78$, and

$$\epsilon_i = 1'{\cdot}7.$$

It appears, then, that the instrumental error is somewhat less than the error of observation. The difference, however, is probably less than the error of our result; and we shall assume, in round numbers, *two minutes* as the amount of each error in the Irish series.

Taking, then, $\epsilon_i = \epsilon_o = 2$, we have (15) (13)

$$E^2 = \frac{1}{W} = 4\left(\frac{1}{n_o} + \frac{1}{n_i}\right). \qquad (16)$$

From this formula we learn how useless it is to multiply observations with the *same* instrument, in order to obtain the dip at a given station: When $n_i = 1$, we have

$$\frac{1}{W} = 4\left(\frac{1}{n} + 1\right), \ \frac{1}{w} = 4 \times 2;$$

w denoting the weight of a single observation; so that

$$\frac{W}{w} = \frac{2\,n_o}{n_o + 1};$$

and, however the observations be multiplied, the weight of the result can never amount to *double* the weight of a single observation.

In what precedes, we have considered only the *actual* dip at a given station. But in deducing the position of the isoclinal lines from observations of dip made at several stations, it is necessary to consider likewise the probable difference between this dip and that due to the geographical position of the station: or, in other words, the probable mean *local error*.

Let ϵ_l denote this error; then it is manifest, from what has been already said, that the actual resulting error will be expressed by the formula

$$\epsilon^2 = \frac{\epsilon_o^2}{n_o} + \frac{\epsilon_i^2}{n_i} + \epsilon_l^2. \qquad (17)$$

The mean local error will, of course, be very different in different countries, the differences depending chiefly on the relative proportion of the igneous and sedimentary rocks. In Scotland, as appears from Major Sabine's excellent report (Sixth Report, p. 102), the local error is considerable; in England it is probably small. We may estimate its amount in any district, by *computing* the dip due to the geographical position of each station, by the formula (2), and taking the sum of the squares of the differences between the computed and observed results. This, substituted in (12), will give the *total* mean probable error, or the value of ϵ in the equation (17) (n_o and n_i *now* denoting the *mean* number of observations, and of instruments, at each station); and, ϵ_o and ϵ_i being already known, we deduce the value of ϵ_l.

In addition to the observations of dip already printed in the Irish Magnetic Report, the following pages contain, 1st, a series of observations made by Robert W. Fox, Esq., at nine stations, chiefly in the West of Ireland; 2nd, observations made by Major Sabine, chiefly in Limerick; 3rd, my own observations in Dublin; and 4th, a series of observations made by Captain James Ross, at twelve stations, distributed uniformly over the whole island.

Mr Fox's observations are contained in Table XXX. They were made in the autumn of the year 1835, at a time when the other parts of the Irish survey were in progress; but, Mr. Fox not being at that time associated in our labours, his results were separately published*. They are now, with his permission, republished in the present memoir. The instrument employed in these observations has been already described†.

Table XXX.

Mr. Fox's Observations in 1835.

Station.	Date.	Hour.	Dip.	Place of Observation.
Dublin	Aug. 17	11 A.M.	70° 59′	Garden of Trinity College.
Galway	— 19	9½ A.M.	71 26	Hotel Garden.
Gallhorick.........	— 19	3½ P.M.	71 41	Island in Lough Corrib.
Clifden	— 22	2 P.M.	71 52	Hotel Garden.
Westport	— 24	11¾ A.M.	72 3	Garden of Hotel (Sligo Arms).
Puntoon	— 24	6 P.M.	72 8	West side of Lough Conn.
Ballina	— 25	10 A.M.	72 7	Hotel Garden.
Giant's Causeway	— 27	4½ P.M.	73 15	East side.
Cushendall	— 28	9½ A.M.	72 0	Hotel Garden.

Major Sabine's additional observations, contained in Table XXXI, were made at Limerick, Dublin, and Bangor, in the year 1836‡. These observations have been already printed

* Proceedings of the Cornwall Polytechnic Society.

† Page 3.

‡ With the exception of one set of observations made with Mayer's needle in the year 1833. These observations, though referred to in the Irish Report, were overlooked in the compilation of the tables.

in the Scotch Magnetic Report, and are reprinted here, so as to have all the data connected with Ireland present in one view. The needles employed, (Mayer's needle and needles S. 1, S. 2,) have been already described.

Table XXXI.

Major Sabine's Observations.

Station.	Date.	Hour.	Needle.	Dip.
Limerick...	Nov. 1, 1833.	1 P.M.	Mayer's.	71° 11′·0
—— ...	— 2 & 4, 1833.	1 P.M.	——	71 11·9
—— ...	Mean...		——	71 11·5
Limerick...	May 1836.		Mayer's.	
—— ...	June		——	
—— ...	Mean...			71 0·0
—— ...	May 1836.		S 1	71 0·6
Limerick...	Feb. 20, 1836.	1 P.M.	S 2	71 13·4
Limerick...	May 5	11 A.M.	——	71 13·0
—— ...	— 5	1 P.M.	——	71 11·0
—— ...	Mean...		——	71 12·0
Dublin ...	July 22, 1836.	Noon	——	71 14·1
—— ...	— 22	1 P.M.	——	71 11·6
—— ...	— 23	Noon	——	71 13·7
—— ...	Mean...	7½	——	71 13·1
Bangor ...	Sept. 21, 1836.	10 A.M.	——	71 48·7
Dublin ...	Oct. 4	1 P.M.	——	71 12·7

My own additional observations were confined to Dublin, and were made in the years 1836 and 1838. The observations of the former year, contained in Table XXXIII, were made with the statical needles, L. 3 and L. 4, already described. Those of the latter, (Table XXXII), with the dip circle, and needle G. 2 made by Gambey *; and with another circle of the same size, and two needles, made by the same distinguished artist for the Dublin Observatory. All these latter observations were made according to the method of *arbitrary azimuths*. In conjunction with the observations of Captain Ross in Dublin, they are taken as the basis on which the determination of the corrections of my other needles, L. 1, L. 3 and L. 4, is made to rest.

* Page 50.

Table XXXII.

Mr Lloyd's Observations in Dublin in 1838.

Gambey's Needles.—Method of Arbitrary Azimuths.

Needle. Date.	Azim.*	Angle.	Azim.	Angle.	Mean Angle.	Dip.
G 2. Aug. 3, 4, 6, 7.	0°	70° 49′·8	180°	70° 56′·0	70° 52′·9	70° 52′·8
	90	89 43·4	270	89 49·4	89 46·4	
	10	71 4·1	190	71 11·6	71 7·8	70 52·6
	100	86 42·2	280	86 37·2	86 39·7	
	20	71 54·0	200	71 59·1	71 56·5	70 56·0
	110	83 33·0	290	83 21·9	83 27·5	
	30	73 11·4	210	73 19·4	73 15·4	70 56·0
	120	80 27·4	300	80 13·9	80 20·6	
	40	75 0·9	220	75 4·0	75 2·5	70 55·1
	130	77 40·2	310	77 32·4	77 36·3	
	50	77 20·4	230	77 24·0	77 22·2	70 55·3
	140	75 17·9	320	75 11·1	75 14·5	
	60	80 1·0	240	80 8·6	80 4·8	70 56·0
	150	73 28·0	330	73 20·0	73 24·0	
	70	83 4·4	250	83 8·5	83 6·5	70 55·4
	160	72 6·4	340	71 59·1	72 2·8	
	80	86 16·5	260	86 23·0	86 19·8	70 52·6
	170	71 14·8	350	71 7·1	71 11·0	
D 1. Sept. 25, 26.	0	70 59·1	180	71 1·9	71 0·5	71 0·5
	90	89 53·5	270	89 52·3	89 52·9	
	30	73 11·1	210	73 19·5	73 15·3	70 52·7
	120	80 16·2	300	80 11·0	80 13·6	
	60	80 9·6	240	80 16·9	80 13·3	70 55·6
	150	73 20·7	330	73 17·1	73 18·9	
D. 2 Sept. 27, 28.	0	70 55·9	180	71 1·7	70 58·8	70 58·8
	90	89 51·2	270	89 53·0	89 52·1	
	30	73 16·1	210	73 29·1	73 22·6	70 59·3
	120	80 22·0	300	80 6·8	80 14·4	
	60	80 16·0	240	80 24·9	80 20·5	71 2·5
	150	73 26·7	330	73 19·5	73 23·1	
D. 2. Oct. 1, 2.	15†	71 26·0	195	71 32·6	71 29·3	70 58·3
	105	85 19·4	285	85 13·4	85 16·4	
	45	75 53·9	225	75 59·4	75 56·6	70 53·5
	135	76 33·6	315	76 30·4	76 32·0	
	75	84 28·3	255	84 31·1	84 29·7	70 59·9
	165	71 46·7	345	71 38·1	71 42·4	

* The Azimuth 0° is the magnetic meridian, the face of the instrument being to the east. The azimuths increase in the order N., E., S., W.

† The azimuths in this last observation are set down in a round number of degrees. They were (exactly) 14° 15′, 44° 15′, 74° 15′, &c.

Table XXXIII.

Mr. Lloyd's Observations in Dublin in 1836.

Date.	Needle L. 3.		Needle L. 4.	
	Hour.	Dip.	Hour.	Dip.
	h m	° ′	h m	° ′
April, 11.	12 18	70 53·4	12 43	70 48·1
— 15.	12 30	71 0·0	12 8	70 47·1
Mean...	12 24	70 56·7	12 25	70 47·6
May 7.	1 32	70 56·5	1 10	70 50·9
— 9.	1 25	71 0·9	12 50	70 56·4
Mean...	1 28	70 58·7	1 0	70 53·6
Aug. 5.	3 50	70 54·7	3 28	70 43·1
— 6.	2 35	70 58·4	2 10	70 51·3
Mean...	3 12	70 56·5	2 49	70 47·2

The observations of Captain Ross were made in October and November, 1838, with the needles designated as R. 4, R. 5, R. 6, R. 7, L. 3, L. 4, in the preceding pages. The stations of observation being sufficiently numerous, as well as uniformly distributed, it has been thought advisable to combine them in a separate determination. The observations are contained in Tables XXXIX. and XL.

We have now to consider the actual errors of the instruments employed in the preceding observations.

The errors of dipping needles may be ascribed to one or other of the three following causes: namely, 1, the *friction* of the axle on its supports; 2, the *imperfect curvature* of the axle itself; 3, *magnetism* in the limb.

It is owing to the first-mentioned cause that a dipping needle assumes, in general, a new position of equilibrium after it has been disturbed, the limit of error being the angle at which the directive force, increasing as the sine of the deviation, becomes equal to the friction. This limit varies, for a given state of polish of the axle and of its supports, with the *radius* of the cylindrical axle, the *weight* of the needle, and its *directive force* *. In all the earlier dipping needles constructed in this country, this limit of error is considerable, owing to the unnecessary size of the axle.

The errors arising from the two latter causes are, however, of a very different nature. The *positive* and *negative* errors due to friction are *equally probable*, and the effect of the dis-

* *Trans. Royal Irish Academy.* Vol. xvii. p. 166.

turbing cause is merely to widen the limits of probable error. The imperfect curvature of the axle, and the magnetism of the limb, act however very differently. Either of these sources of error must, at a given place, affect all the results in the *same manner*; and, consequently, no repetition of observation, with an instrument so circumstanced, can afford even an approximation to the true dip. At different places the error will be different, and will vary according to no assignable law.

The course to be pursued by the observer with reference to these errors is manifest. Their existence or non-existence should be ascertained at the outset by one or other of the means pointed out by Major Sabine in the commencement of this memoir; and if found to surpass certain limits, the instrument should be rejected. The case is different, however, when the instrument has been actually employed for some time previously to the detection of the error. Here we must seek, if possible, to determine the probable amount of the error, and apply it, with an opposite sign, as a correction to the results. Where the district of observation is limited, this is practicable. It will be easily understood, that the imperfect curvature of the axle, or the disturbing action of the limb, must, within a moderate range of dip, affect all the results in the *same manner*, so that they will all require a correction having the *same sign*; and that when the range of dip is *very small*, the *amount* of the disturbance will be nearly the same throughout, and consequently the correction required will be *nearly constant*. In such a case then we have only to determine the amount of the error at some one station, by a comparison of the results with those of proved needles obtained at the same place, and, if possible, at the same time.

Again, in needles whose poles are unchanged, gravity acts with a certain moment *with* or *against* the directive force; the coincidence of the centre of gravity with the axle being rarely attained. The observed inclination, therefore, deviates from the true dip, and the amount of this deviation varies in different places, according to a known law*. To obtain its actual value, however, at *any* station, it must be known at some *one*; and this knowledge is to be obtained, as before, by a comparison of the results with those of other needles at that sta-

* Fifth Report, p. 144. With needles whose poles are inverted in each observation, the true dip may be inferred from the observed angles of inclination, however considerably they may deviate from it. In such needles, therefore, the non-coincidence of the centre of gravity with the axle cannot properly be ranked among the sources of error.

tion. When the district of observation is limited, the *variation* of this quantity may be disregarded.

The importance of an exact determination of these needle-corrections is very great in the present instance. When, indeed, the *same* needle is employed throughout an entire series of observations (as was done by Major Sabine in Scotland), it is manifest that any error in the amount of its correction will have the effect only of displacing the isoclinal lines in *absolute position*, leaving their *direction* and *interval* unaltered. For the direction and interval of the lines depend solely on the *differences* of dip; and these are manifestly independent of the correction, which alters all the dips by the same amount. The case is different, however, when (as in the present instance) *different* needles requiring correction are employed in the same series. Here the differences of dip cannot be known, unless we know the differences of the corrections of the needles employed; and it is manifest that any error in the amount of that difference will displace one entire group of results relatively to the rest, and thus (when the mean geographical position of these groups is different) induce a grave error in the direction of the lines.

Before we proceed to determine the amount of these errors in the needles employed in the Irish survey, it may be desirable to make a few remarks on *their* particular causes.

Of the two sources of error above mentioned, the *imperfection of axle* appears to be the most common; and it is to it we are to ascribe (as Major Sabine has already remarked*) the chief part of the discordances in the results obtained at Westbourne Green in 1835. The same series, however, affords likewise a remarkable instance of the other error. Having purposely destroyed the balance in two of my dipping needles, so that they rested nearly in the horizontal position in Dublin, I proceeded to use them exclusively for observations of intensity. The results thus obtained were, however, so anomalous, that I was compelled to reject them altogether. After some tedious and vain attempts to discover the source of the anomaly, I was at length satisfied, by a careful inspection of the results, that the needles were under the influence of some other force besides the earth's magnetism and gravity, and I concluded that this disturbing force could be no other than magnetism in the dip circle itself. Trial soon verified this conjecture, and I had the mortification to find that the apparatus which I had been so long using was *throughout* magnetic, and that the magnetism†

* Page 46.

† Magnetism *induced* in ferruginous matter, not permanent.

was greatest in the graduated limb, the very part in which, from its proximity to the needle, it must operate most powerfully.

I had next to consider the painful question,—How far the numerous results obtained with this instrument were vitiated by this newly-discovered source of error? Whether they were entitled to any confidence; and if so, what were the probable limits of error? It is manifest that if the ferruginous matter were *uniformly* distributed throughout the limb, it could produce no disturbance in the position of a needle which (like the dipping needle) divides the limb symmetrically. It is only by an irregularity in its distribution that the magnetic matter of the limb can operate as a disturbing cause; and then it is manifestly only by the *difference of the attractions*, on the two sides of each pole, that the needle is actually disturbed. Hence, though the magnetism of the limb may produce very decided effects upon a test needle, in a position at right angles to its plane, the effect upon a dipping needle may be comparatively trifling.

In order to estimate the amount of these effects, I separated the divided circle from the apparatus, and placed it on a horizontal support of wood. Three strong pins in contact with the inner edge of the limb, and dividing it equally, were then driven into the support, so as to prevent the limb from having any motion, except one of rotation in its own plane. A magnetic bar, whose length was nearly equal to the diameter of the circle, was then supported delicately within it, and the deviation of the bar from its undisturbed position was observed in the different positions of the limb with respect to it. It was thus found that most parts of the limb exerted a sensible disturbing effect upon the needle; and that this effect was not only considerable in the neighbourhood of the two zero points of the limb (the part where the anomalies had been first observed), but that it also varied there *very rapidly*. A detailed examination of the effects in this position showed that there was a disturbing centre of ferruginous matter in the neighbourhood of each of these points, and that it was to the action of these centres that the anomalies in the observations above alluded to were owing.

In the neighbourhood of the divisions of 70° the disturbance of the needle was likewise considerable, and its direction was such as to diminish the apparent dip. Here, then, we have the cause of the large *negative error* of the results obtained with this instrument. But this deflection did not vary rapidly on either side of these positions, so that for small changes of dip the error may be regarded as nearly constant*. Defective, therefore, as the apparatus is in this respect, there is reason to conclude that the *differences of dip* obtained with it in Ireland may be relied on within the usual limits of probable error, and that to obtain the true dip from the observed results, we have only to apply a *positive* correction, which may be regarded as *constant* throughout the series.

The instrument referred to in the preceding pages having been much employed in Dublin, and with very consistent results, we shall take, as the basis of its correction, the dip in Dublin as deduced from the observations with Gambey's needles, Table XXXII. In these observations, made according to the method of arbitrary azimuths, the bearing points of the axle, and the position of the needle with respect to the limb, are different in each azimuth; so that the results may be regarded as, virtually, the results of *different* instruments. Their accordance is sufficient to show that the errors of axle and of limb are inconsiderable. For the convenience of reference, the observations are put together in the following Table; the dips being reduced to the 1st of January, 1838.

Table XXXIV.

Needle.	Azimuth.	Dip.	Needle.	Azimuth.	Dip.
Gambey's Needle, G. 2, belonging to Capt. FitzRoy.	0° & 90°	70° 54′·2	Gambey's Needles, belonging to the Dublin Observatory. D (1)	0° & 90°	71° 2′·3
	10 & 100	70 54·0		30 & 120	70 54·5
	20 & 110	70 57·4		60 & 150	70 57·4
	30 & 120	70 57·4	D (2)	0 & 90	71 0·6
	40 & 130	70 56·5		15 & 105	71 0·1
	50 & 140	70 56·7		30 & 120	71 1·1
	60 & 150	70 57·4		45 & 135	70 55·3
	70 & 160	70 56·8		60 & 150	71 4·3
	80 & 170	70 54·0		75 & 165	71 1·7

The mean of these results is 70° 57′·9. If we combine with this the mean result obtained by Captain Ross at the same place, as deduced from six observations with four needles, and reduced to the same epoch, (namely, 71° 1′·7,) we have, for the mean dip in Dublin, on the 1st of January, 1838,

70° 58′·8.

* A comparison of the results with those of other instruments seems to point to the conclusion that this error diminishes with the dip, and is somewhat less in England than in Ireland.

To compare with this, we have the following observations with the needles L. 1, L. 3, L. 4, in Dublin.

TABLE XXXV.

Needle.	No.	Date.	Observed Dip.	Reduced Dip.	Mean Dip.
L 1	1	Oct. 21, 1833	70° 56′·4	70° 46′·4	
—	6	Aug. 25, 1834	70 53·8	70 45·8	70° 46′·8
—	6	Sept. 9, 1835	70 53·5	70 47·9	
L 3	4	Apr. 25, 1836	70 57·7	70 53·7	70 53·5
—	2	Aug. 5, 1836	70 56·5	70 53·1	
L 4	4	Oct. 2, 1834	70 53·7	70 45·9	
—	7	Sept. 6, 1835	70 50·7	70 45·1	
—	3	Nov. 5, 1835	70 49·8	70 44·6	70 45·4
—	4	Apr. 25, 1836	70 50·6	70 46·6	
—	2	Aug. 5, 1836	70 47·2	70 43·8	

Hence we obtain the following corrections:

Needle L. 1, correction $= + 12'\cdot 0$
,, L. 3 ,, $= + 5'\cdot 3$
,, L. 4 ,, $= + 13'\cdot 4$

In L. 3 and L. 4, needles whose poles are unchanged, the errors here deduced are, of course, those which result from the moment of the needles' weight, combined with that arising from the disturbing action of the limb.

The *weights* due to these corrections are at once deduced from the principles of the preceding pages. When the results of one needle, at a given station, are compared with those of others, and that we seek their *difference*, it is manifest that $p = 1$, $q = 1$, (14), and that, consequently,

$$E^2 = E_1^2 + E_2^2;$$

E_1 denoting the probable error of the *mean* result of the given needle, and E_2 that of those with which it is compared. When we look no further than the *actual* difference of the results at the one station, it is manifest that

$$E_1^2 = \frac{\epsilon_1^2}{n_1}, \quad E_2^2 = \frac{\epsilon_2^2}{n_2};$$

ϵ_1 and ϵ_2 denoting the probable errors of a single observation, in the needles compared, and n_1 and n_2 the number of observations. Hence, if $\epsilon_1 = \epsilon_2$, that is, if the reading power be the same in the two cases, and the same pains be bestowed on the observations,

$$\frac{1}{n} = \frac{1}{n_1} + \frac{1}{n_2}; \tag{18}$$

n denoting the value of the ratio $\frac{\epsilon^2}{E^2}$, or the *equivalent* number of observations of the difference sought, supposing it to be the immediate subject of observation.

But when we desire to compare the result of the uncorrected needle with the *actual dip*, we must also take into account the probable *instrumental error* of the results with which it has been compared; and we have (15)

$$E^2_2 = \frac{\epsilon^2_2}{n_2} + \frac{\epsilon^2_i}{n_i}.$$

And in place of equation (18), we have the following:

$$\frac{1}{n} = \frac{1}{n_1} + \frac{1}{n_2} + \frac{\epsilon^2_i}{\epsilon^2_o}\frac{1}{n_i}. \tag{19}$$

To apply this, we shall assume, as before, the instrumental error to be equal to the error of observation, the latter including the error of epoch; and we obtain

Needle L. 1, $n_1 = 13$, $n = 6{\cdot}1$,
— L. 3, — — 6, — — 3·9,
— L. 4, — — 20, — — 7·3.

We shall adopt the nearest whole numbers, 6, 4, 7.

The correction of needle S. 2 has been determined with great care by Major Sabine *, by a comparison, at various stations, of its results with those of the needles M and G. 2, needles which may be regarded as almost free from all instrumental error. The amount of this correction is $-9'{\cdot}6$; and its weight 16. This amount is almost identical with that previously employed in the calculation of the Irish observations.

The other needle employed by Major Sabine in Ireland, S. 1, is constructed on a plan suggested by Mr. Dollond. The middle of the needle has the form of a cube, and is perforated so as to receive the axle in different directions, the intention being, that the position of the axle should be varied in the

* Table XIX.

course of every observation. From some defect of workmanship, however, the balance of the needle was much deranged in some positions of the axle; and it was accordingly employed by Major Sabine as an ordinary dipping needle, the axle being permanently fixed in one position in which the needle was tolerably balanced. This was the case during the observations made with it in August, September, and October, 1834 (Fifth Report, p. 139); the axle being undisturbed during the whole of the series. In 1835, when Captain Ross used this needle at Westbourne Green, the axle had been repolished, and was, moreover, fixed by the artist in a different position from that which it had occupied during the observations of the preceding year. So far, therefore, as axle error is concerned, the needle must, then and thenceforward, be regarded as a different needle.

In order to deduce the amount of the axle error, previously to the alteration just alluded to, we may compare the result obtained with this needle at Limerick, in August 1834, with the mean dip of the place as given by other needles. The difference (4′·2) is probably not greater than the probable error of observation, which, owing to the imperfect polish of the axle, was in this needle considerable. Under these circumstances, we are not justified in assigning to it any correction.

The needles employed by Mr. Fox appear to give results extremely consistent with one another, and with those of other needles. In their case, therefore, no correction is required.

We are now prepared to exhibit in one view the mean* values of the dip, as deduced from these various needles. The following table contains the results of observations arranged chronologically, and *corrected* as has been above explained.

* Where the needles L. 1 and L. 4 have been employed together, double weight has been allowed to the results of the former in taking the mean, in accordance with the conclusion of page 98.

Table XXXVI.

Corrected Dip.

Station.	Date.		Needle.	No.	Dip.	Mean Dip.
Dublin	Oct. 21,	1833	L. 1	1	71 8·4	71 8·4
Limerick	Nov.	1833	M	4	71 11·7	71 11·7
Limerick	July,	1834	L. 1	5	71 11·5	71 11·5
Dublin	Aug. Sept.		L. 1	6	71 5·8	71 6·1
———	Sept. Oct.		L. 4	4	71 7·1	
Limerick	Aug. 1, 16		S. 1	2	71 3·5	71 3·5
Glengariff.........	Sept. 27, 28		S. 1	2	71 1·5	71 1·5
Killarney	Oct. 4		S. 1	1	71 4·5	71 4·5
Tulla	— 12		S. 1	1	71 15·8	71 15·8
Carlingford	— 13		L. 1	1	71 28·3	71 30·2
———	— 13		L. 4	1	71 34·0	
Armagh	— 14, 15		L. 1	2	71 43·5	71 42·2
———	— 14, 15		L. 4	2	71 39·7	
Colerain	— 20		L. 1	1	71 27·6	71 26·9
———	— 20		L. 4	1	71 25·6	
Carn	— 21		L. 1	1	71 59·8	72 0·9
———	— 21		L. 4	1	72 3·0	
Strabane	— 23		L. 1	1	72 3·6	72 0·0
———	— 23		L. 4	1	71 52·8	
Enniskillen	Oct. 24	1834	L. 1	1	72 0·0	72 0·0
Fermoy............	Dec. 2		L. 1	1	70 48·3	70 48·3
Limerick	July,	1835	S. 2	4	71 7·3	71 7·3
Dublin	Aug. 17		F		70 59·0	70 59·0
Galway	— 19		F		71 26·0	71 26·0
Gallhorick.........	— 19		F		71 41·0	71 41·0
Clifden	— 22		F		71 52·0	71 52·0
Westport	— 24		F		72 3·0	72 3·0
Puntoon			F		72 8·0	72 8·0
Ballina	— 25		F		72 7·0	72 7·0
Giants Causeway	— 27		F		73 15·0	73 15·0
Cushendall	— 28		F		72 0·0	72 0·0
Markree	— 21		L. 1	2	72 5·6	72 6·3
———	— 21		L. 4	1	72 9·0	
Ballina	— 22		L. 1	1	72 13·9	72 11·0
———	— 22		L. 4	1	72 5·2	
Belmullet	— 24		L. 1	1	72 14·7	72 13·4
———	— 24		L. 4	1	72 10·9	
Achill	— 25		L. 1	1	72 6·4	72 6·5
———	— 25		L. 4	1	72 6·6	
Galway	— 28		L. 1	1	71 33·9	71 32·9
———	— 28		L. 4	1	71 30·8	

Station.	Date.	Needle.	No.	Dip.	Mean Dip.
Ennis	Aug. 28	L. 1	1	71 13·5	71 13·2
——	— 28	L. 4	1	71 12·5	
Limerick	— 29	L. 1	1	71 3·9	71 2·9
——	— 29	L. 4	1	71 0·9	
Cork	— 31	L. 1	1	70 41·3	70 43·1
——	— 31	L. 4	1	70 46·6	
Waterford	Sept. 1	L. 1	1	70 49·6	70 50·5
——	— 1	L. 4	1	70 52·2	
Broadway	— 2	L. 1	1	70 31·4	70 35·9
——	— 2	L. 4	1	70 45·0	
Gorey	— 3	L. 1	1	70 55·4	70 55·8
——	— 3	L. 4	1	70 56·5	
Rathdrum.........	— 3	L. 1	1	70 53·1	70 53·5
——	— 3	L. 4	1	70 54·2	
Dublin	Sept. 4—15	L. 1	6	71 5·5	71 5·0
——	Aug. Sept.	L. 4	7	71 4·1	
——	Nov. 5, 6	L. 4	3	71 3·2	71 3·2
Ballybunan	— 8	S. 2	1	71 19·5	71 19·5
Valentia	— 12	S. 2	1	71 5·4	71 5·4
Dingle	— 18	S. 2	1	71 8·1	71 8·1
Tulla................	Dec. 10	S. 2	1	71 26·9	71 26·9
Limerick	— 26, 27	S. 2	3	71 5·0	71 5·0
Youghal	— 29	S. 2	2	70 39·4	70 39·4
Limerick	Feb. 1836	S. 2	1	71 3·8	71 3·8
——	May	S. 2	2	71 2·4	71 1·1
——	May	S. 1	1	71 0·6	
——	May, June	M	2	71 0·0	
Dublin	April, May	L. 3	4	71 3·0	71 3·5
——	April, May	L. 4	4	71 4·0	
——	July 22, 23	S. 2	3	71 3·5	71 3·5
——	Aug. 5, 6	L. 3	2	71 1·8	71 1·2
——	Aug. 5, 6	L. 4	2	71 0·6	
Bangor	Sept. 21	S. 2	1	71 39·1	71 39·1
Dublin	Oct. 4	S. 2	1	71 3·1	71 3·1
——	Aug. 3—7, 1838	G. 2	9	70 54·6	70 54·6
——	Sept. 25, 26	D. 1	3	70 56·3	79 57·9
——	Sept. 27—Oct. 2	D. 2	6	70 58·7	

The following table contains the final mean dip at each station, reduced to a common epoch, (the 1st January, 1837,); and the latitudes and longitudes of the stations:

TABLE XXXVII.

Station.	Lat.	Long.	Dip.	Station.	Lat.	Long.	Dip.
	° ′	° ′	° ′		° ′	° ′	° ′
Causeway .	55 15	6 31	73 11·8	Colerain ...	55 8	6 40	71 21·6
Belmullet .	54 13	9 57	72 10·2	Tulla	52 52	8 43	71 17·5
Ballina ...	54 7	9 7	72 5·8	Ballybunan	52 30	9 41	71 16·8
Puntoon ...	53 58	9 10	72 4·8	Ennis	52 51	8 58	71 10·0
Markree ...	54 12	8 26	72 3·1	Dingle......	52 8	10 17	71 5·4
Achill	53 56	9 52	72 3·3	Valentia ...	51 56	10 17	71 2·7
Westport...	53 48	9 29	71 59·8	Limerick...	52 40	8 35	71 1·8
Cushendall	55 4	6 5	71 56·8	Dublin ...	53 21	6 16	71 1·2
Carn	55 15	7 15	71 55·6	Killarney .	52 3	9 31	70 59·1
Enniskillen	54 21	7 38	71 54·8	Glengariff .	51 45	9 31	70 56·1
Strabane ...	54 49	7 28	71 54·8	Gorey	52 40	6 17	70 52·6
Clifden ...	53 29	9 59	71 48·8	Rathdrum .	52 55	6 14	70 50·3
Bangor ...	54 39	5 42	71 38·5	Waterford .	52 16	7 8	70 47·3
Gallhorich.	53 25	9 5	71 37·8	Fermoy ...	52 7	8 16	70 43·3
Armagh ...	54 21	6 39	71 36·9	Cork	51 54	8 26	70 39·9
Galway ...	53 17	9 4	71 26·3	Youghal ...	51 57	7 50	70 37·0
Carlingford	54 2	6 11	71 25·0	Broadway .	52 13	6 24	70 32·7

Of the foregoing results, those obtained at the Giants' Causeway and at Colerain are manifestly affected, to a very considerable extent, by the disturbing action of the basaltic rocks. The effect of the basaltic pillars of the Causeway upon the magnetic needle has been long since observed; and on comparing the dip recorded in the preceding table, with that due to the geographical position of the station, we find it in excess to the amount of 50′. At Colerain, on the other hand, the effect of the disturbing action has been to diminish the dip, but in a less amount. The cause of these irregularities being apparent, we have no hesitation in rejecting the results, in the computation of the the isoclinal lines.

Before we proceed to this computation, we must estimate the *weights* of the observed results; and for this purpose it is necessary to know the amount of the probable error of station. This is obtained by computing (with assumed approximate values of L,M,N,) the probable dip at each station, due to its geographical position, and comparing it with that observed. The sum of the squares of the differences of the computed and observed results, substituted in (12), will give the *total* mean probable error; from which (the errors of *observation* and of *instrument* being already known) the *local* error is deduced by means of the equation (17).

Now assuming the approximate values

$$L = 71° \; 22'\cdot5, \quad M = +\cdot30, \quad N = +\cdot51;$$

the probable dip at each station will be given by the formula

$$z = 71° \; 22'\cdot5 + \cdot30 \, x + \cdot51 \, y;$$

and the computation gives for the sum of the squares of the differences of the computed and observed results, at the 32 stations,

$$\Sigma \, (x - a)^2 = 1192\cdot09;$$

from which we find (12)

$$E^2 = 17\cdot48, \quad E = 4\cdot2,$$

E denoting the total probable error at any one station. But if E_o and E_i denote the *mean* probable errors of observation and of instrument at each station, and E_l the probable local error,

$$E^2 = E_o^2 + E_i^2 + E_l^2.$$

For the observations of this series, $E_o = E_i = 2\cdot0$*; wherefore

$$E_l = 3'\cdot1.$$

To deduce the weight of the result of n_o observations, with n_i instruments, at any station, we substitute the values thus obtained in (17), and we obtain

$$\frac{1}{w} = 4\left(\frac{1}{n_o} + \frac{1}{n_i}\right) + 9.$$

When the local error, therefore, bears so great a proportion to the errors of observation and of instrument, as it does in the present instance, it is manifestly waste of labour (as far as regards the determination of the position of the isoclinal lines) to multiply observations at any one station. In the case under consideration, the weight due to the result at any station (however the observations be multiplied, and whatever the number of instruments employed) can never amount to double the weight of a single observation.

Substituting the values of n_o and n_i in the preceding formula, we find the weight of the mean dip, in Dublin and Limerick, equal to 1·8, the weight of a single observation being unity: in no other case throughout this series does the weight amount to more than 1·3. Taking the nearest whole numbers for the value of this ratio, we shall assign a weight of 2 to Dublin and

* Throughout a considerable portion of the series, two needles, L. 1 and L. 4, were used together. The probable error of *observation* of the mean is nearly 2′; the *instrumental* error is little less than that of a single needle, being, in this case, due chiefly to the magnetism of the limb.

to Limerick, the weight of each of the other stations being unity. The results of the calculation are the following:

$$L = 71° \, 22'{\cdot}74, \quad M = +{\cdot}300, \quad N = +{\cdot}505.$$
$$u = -\,59° \, 16', \; r = {\cdot}587.$$

Accordingly, the dip at the central station (latitude = 53° 21′, longitude = 8° 0′) is 71° 22′·7; the epoch being the 1st January, 1837.

Captain Ross's Observations of Dip in Ireland.

These observations were made at 12 stations, with the needles already designated as R. 4, R. 5, R. 6, R. 7. They are contained in the following table.

TABLE XXXVIII.

Station.	Date.	Hour.	Needle.	Poles. α direct, β reversed.	Observed Dip.	Mean Dip.	Place of Observation.
Waterford	1838. Oct. 4	1·0 P.M.	R 6	α 70° 43′·4			
				β 70 50·4	70° 46′·9		
		3·15 P.M.	R 4	α 70 44·3		70° 45′·8	In an Orchard,
				β 70 45·1	70 44·7		½ mile mag. S.
Cork	— 6	1·45 P.M.	R 6	α 70 36·6			of the Church.
				β 70 42	70 39·3		
		3·20 P.M.	R 4	α 70 34·5			
				β 70 38·3	70 36·4		
	— 7	2·15 P.M.	R 7	α 70 41·7		70 39·4	In Mr. Jones's
				β 70 41·7	70 41·7		nursery grounds.
		3·30 P.M.	R 5	α 70 36·6			
				β 70 43·4	70 40		
Valentia Island.	— 12	2·30 P.M.	R 6	α 70 50·2			
				β 70 54·4	70 52·3		
	— 13	1·0 P.M.	R 4	α 70 51·5			
				β 70 52·1	70 51·8	70 52	Near the N. W.
		2·30 P.M.	R 5	α 70 50·2			point of the
				β 70 53·6	70 51·9		Island.
Killarney......	— 17	1·30 P.M.	R 6	α 70 49·3			
				β 70 55·9	70 52·6		
	— 19	11·20 A.M.	R 4	α 70 49·6			
				β 70 49·6	70 49·6	70 51·1	In the grounds
		3·0 P.M.	R 5	α 70 50·2			of Mucruss, near
				β 70 53·6	70 51·9		the Abbey, the
Limerick......	— 22	2·30 P.M.	R 6	α 70 58·2			demesne of H.
				β 71 1·8	71 0		Arthur Herbert,
		4·0 P.M.	R 4	α 70 58·4			Esq.
				β 70 58·4	70 58·4		
	— 23	1·0 P.M.	R 5	α 71 1		70 59·6	In the garden of
				β 70 59·7	71 0·4		Somerville, the
		2·45 P.M.	R 7	α 70 59·7			seat of James
				β 70 59·6	70 59·6		Hervey, Esq.
Shannon Harbour.	— 26	11·20 A.M.	R 6	α 71 19			
				β 71 25·4	71 22·2		
		1·0 P.M.	R 4	α 71 25·5		71 23·2	In the garden of
				β 71 22·7	71 24·1		Faulkner's Inn.

Station.	Date.	Hour.	Needle.	Poles. α direct, β reversed.	Observed Dip.	Mean Dip.	Place of Observation.
Dublin	1838. Oct. 29	1·0 P.M.	R 6	α 70° 58′·2			
				β 71 4·2	71° 1′·2		
		2·0 P.M.	R 7	α 70 59·4			
				β 71 1·4	71 0·4		
		2·45 P.M.	R 4	α 70 59·8			
				β 71 0·2	71 0		
		4·0 P.M.	R 5	α 70 56·6		70° 59′·8	Near the Magnetic Observatory in the Gardens of Trinity College.
				β 71 0·8	70 58·7		
	— 30	Noon	R 6	α 70 57			
				β 71 1·3	70 59·1		
		1·30 P.M.	R 4	α 70 57·8			
				β 71 0·6	70 59·2		
Armagh	Nov. 2	11·0 A.M.	R 6	α 71 38·4			
				β 71 42·4	71 40·4		
		0·30 P.M.	R 4	α 71 39·7			
				β 71 40·3	71 40	71 40·5	In the garden North of the Observatory.
		2·20 P.M.	R 5	α 71 41·1			
				β 71 41	71 41·1		
Londonderry	— 5	1·30 P.M.	R 4	α 72 4·7			
				β 72 0·5	72 2·6		
		4·0 P.M.	R 6	α 72 2·6			
				β 72 3·7	72 3·2	72 2·3	In an orchard, S.W. by S. true 1½ a mile from the Cathedral.
	— 6	2·30 P.M.	R 5	α 72 1·7			
				β 72 0·7	72 1·2		
Sligo	— 10	0·40 P.M.	R 6	α 72 0			
				β 72 0·6	72 0·3		
		2·30 P.M.	R 4	α 72 2·2		72 0·2	In the grounds of Markree Castle, the demesne of E. J. Cooper, Esq., M.P.
				β 71 58·2	72 0·2		
Westport	— 13	1·30 P.M.	R 6	α 71 57·8			
				β 71 58·8	71 58·3		
		3·10 P.M.	R 4	α 72 1·0		71 59	In the garden of the Hotel.
				β 71 58·4	71 59·7		
Edgeworthstown.	— 19	1·15 P.M.	R 6	α 71 27·6			
				β 71 32·6	71 30·1		
		3·15 P.M.	R 4	α 71 29·2		71 29·8	In the garden of the residence of the Edgeworth family.
				β 71 29·9	71 29·6		

The next table contains the latitudes and longitudes of Captain Ross's Irish stations, and the mean dip at each station. The observations were made in such quick succession that the reduction to a mean epoch is unnecessary.

Table XXXIX.

Station.	Lat.	Long.	Dip.	Station.	Lat.	Long.	Dip.
Londonderry	55° 0′	7° 20′	72° 02′·3	Dublin	53° 21′	6° 16′	70° 59′·8
Markree	54 12	8 26	72 00·2	Limerick	52 40	8 35	70 59·6
Westport	53 48	9 29	71 59	Valentia	51 56	10 17	70 52
Armagh	54 21	6 39	71 40·5	Killarney	52 03	9 31	70 51·1
Edgeworth's-town	53 42	7 33	71 29·8	Waterford	52 16	7 08	70 45·8
Shannon Harbour	53 14	7 52	71 23·2	Cork	51 54	8 28	70 39·4

The foregoing observations having been made with different needles in the same circle, it becomes necessary, in estimating the probable error, to separate those due to the limb from those which arise from irregularities in the axle. From the mode in which the observations were taken,—namely (in all but one instance) a *single* observation with each needle,—the *axle error* and the error of observation are combined; and the beautiful accordance of the partial observations shows that their combined result is inconsiderable. There seems reason, however, for believing that the circle itself is not free from error. The mean result obtained with these needles, in this circle, at Westbourne Green, is $2'\cdot0$ *less* than the mean of the other needles employed at the same place (see Table III.); while on the other hand, they give a result $3'\cdot8$ *in excess* of the mean dip, as shown by Gambey's needles in Dublin,—the latter being observed by the method of arbitrary azimuths.

Now the total probable error at each station, in this series, (as deduced from a comparison of the computed and observed results) is found to be $4'\cdot0$,—a result scarcely differing from that of the former series. Of this, the part which is reduced by repetition is (as has been already stated) exceedingly small; and, consequently, the remainder (the combined result of the station and circle errors) is considerable. Under these circumstances, it will be readily seen, no disproportion in the number of observations can materially alter the weights; and as, in addition to this, the observations have been distributed with some attention to uniformity, it is manifest that we must regard the weights of all the stations as equal.

The results of calculation are

$$L = 71^\circ\ 22'\cdot0,\quad M = +\cdot270,\quad N = +\cdot550$$
$$u = -63^\circ\ 49',\quad r = \cdot613.$$

Hence the dip at the central station, on the 1st November, 1838, was $71^\circ\ 22'\cdot0$, the central station being the same as before; consequently, the probable dip at that station, on the 1st January, 1837, was $71^\circ\ 26'\cdot4$.

Finally, if we combine these results with those of the former series, allowing weights in proportion to the number of stations, we find

$$L = 71^\circ\ 23'\cdot7,\quad M = +\cdot292,\quad N = +\cdot517$$
$$u = -60^\circ\ 32',\ r = \cdot594;$$

L denoting the mean dip at the central station, on the 1st January, 1837.

Report resumed by Major Sabine.

To the observations in Ireland I have to add a very careful determination of the dip at Lissadel in the county of Sligo, the seat of Sir Robert Gore Booth, Bart., made at my request with Captain Fitz Roy's Gambey by Archibald Smith, Esq., of Jordan Hill.

Table XL.

Lat.	Long.	Date.	Hour.	Needle.	Poles. α direct, β reversed.	Mean.	Mean Dip.
° ′ 54 23	° ′ 8 33	1838. Sept. 19	Noon	2	α ° ′ 71 57·5	° ′	° ′
					β 71 57·6	71 57·6	
		— 22	2 P.M.	2	α 71 54·4		
					β 71 55·3	71 55	
		— 24	9½ A.M.	2	α 71 54·5		71 56
					β 71 56	71 55·2	
		— 25	9½ A.M.	2	α 71 54·5		
					β 71 57·8	71 56·2	

Collecting in one view the values of u and r obtained from the observations in Ireland, we have as follows:—

Table XLI.

Observer.	No. Stations.	Cent. Geog. Posit.		Values of	
		Lat.	Long.	u	r
Lloyd, Fox, and Sabine	34	° ′ 53 21	° ′ 8 0	—59 16	0·578
Ross..........................	12	53 21	8 0	—63 49	0·613

Regarding the values of u and r as entitled to weight, proportioned to the number of stations, of which each is the representative, we obtain — 60° 32′ and 0′·594 as the mean values derived from the Irish series, and corresponding to the mean geographical position, 53° 21′ N. and 8° 00′ W.

Collecting in one view the values of u and r at the central geographical positions in England, Scotland and Ireland, as they have been derived from the several series in each country, we have as follows :

England,	Lat. 52° 38′.	Long. 2° 07′;	$u = -65° 05'$;	$r = 0{\cdot}575'$
Scotland,	— 56° 49′.	— 3° 39′;	$u = -56° 06'$;	$r = 0{\cdot}549'$
Ireland,	— 53° 21′.	— 8° 00′;	$u = -60° 32'$;	$r = 0{\cdot}594'$

Whence it appears that the isoclinal lines do not intersect the geographical meridian at the same angle in the three countries; that they form a greater angle with the meridians in England than in either of the other two countries; and that the angle is also greater in Ireland than in Scotland.

It also appears that the distance between the lines is greatest in Scotland, less in England, and least in Ireland; the number of geographical miles, measured on the perpendicular, corresponding to differences of a degree of dip,—being

109·2 in Scotland;
104·4 in England;
101·0 in Ireland.

It follows, from the different values of r, that the assumption, upon which we have hitherto proceeded in these combinations, of parallelism of the lines and their equidistance apart, does not hold good when applied to an area of the extent of the British islands, and not *strictly* so for any of its three portions; and that it is desirable to find a method of more exactly representing the observations, by tracing each isoclinal line separately from observations nearly of its own value, and consequently but little removed from it in geographical distance. If we have the approximate values of u and r at any station where the dip has been observed, we may readily compute the latitude and longitude of a point furnished by that observation for the position of the next adjacent isoclinal line. If the isoclinal lines sought are those of complete degrees (*i.e.* the lines of 69° 00′, 70° 00′, 71° 00′, &c.), and if the observation be also without fractional minutes—say, for example, 69° 00′—the point furnished by that observation for the line of 69° 00′ is at the station itself. If the observation exceeds or falls short of 69° 00′ by a few minutes, the point furnished by it for the isoclinal line must be distant from the station a geographical space, equivalent to the value in distance of the fractional minutes, as computed by the value of r, and in the direction of $u + 90°$. Thus, if D be the degree of dip represented by the isoclinal line, δ the dip observed at a station, of which the latitude is λ, then is $(D-\delta)\frac{\sin u}{r}$ the difference of latitude, and $(D-\delta)$ $\frac{\cos u}{r}$ sec λ the difference of longitude, between the station and the point which it furnishes for the isoclinal line.

We have the values of u and r at the central geographical positions in England, Ireland, and Scotland, as derived from observation. If, for a general central station in the British Islands, we take the mean of the central stations in the three countries, viz. lat. 54° 16′ N., long. 4° 35′ W., we may deduce the values of u and r for that station from equations of the form

$$u_{,} = u + a_{,}x + b_{,}y$$
$$r_{,} = r + a_{,}x + b_{,}y,$$

where $u_{,}$ is the angle and $r_{,}$ the rate of increase at one of the three central geographical positions; $a_{,}$ and $b_{,}$ co-ordinates of distance in longitude and latitude from the general central station, expressed in geographical miles; and x and y coefficients of the change in the values of u and r in each geographical mile, y in the direction of the meridian, and x in that of the perpendicular thereto. The mean results in the three countries will then furnish respectively the three following equations for the value of u;

$$\begin{aligned} &\text{England, } 3905' = u - 89\,x - 98\,y; \\ &\text{Scotland, } 3366' = u - 34\,x + 153\,y; \\ &\text{Ireland, } 3632' = u + 123\,x - 55\,y; \end{aligned}$$

The number of stations from which the mean results were obtained was,

$$\left.\begin{array}{ll}\text{In England,} & 122 \\ \text{In Scotland,} & 46 \\ \text{In Ireland,} & 39\end{array}\right\} \text{ or nearly in the proportion of } \left\{\begin{array}{l}3 \\ 1 \\ 1\end{array}\right.$$

In combining these equations therefore by the method of least squares, to obtain the most probable values of u, x, and y, we may give the weight of 3 to the English result, and that of unity to each of the two others.

Pursuing the usual process, we derive $u = -60°\ 42'$; $x = +0{\cdot}6$; $y = +2{\cdot}0$: and we may compute the approximate value of u at any geographical position in the British Islands, by the formula

$$u = -60°\ 42' + 0{\cdot}6\,a + 2\,b,$$

the origin of the coordinates, a and b being the general central station in 4° 35′ W. longitude, and 54° 16′ N. latitude.

Proceeding in the same manner for r, we have the 3 equations:

$$\begin{aligned}
&\text{England,} & +0{\cdot}575 &= r - 89\,x - 98\,y\,; \\
&\text{Scotland,} & +0{\cdot}549 &= r - 34\,x + 153\,y\,; \\
&\text{Ireland,} & +0{\cdot}594 &= r + 123\,x - 55\,y.
\end{aligned}$$

Giving the English result the weight of 3, and each of the others that of unity, and deducing by the method of least squares the most probable values of r, x, and y, we obtain $x = +{\cdot}00007$; $y = -{\cdot}00013$; and $r = 0{\cdot}571$, at the central general station in lat. 54° 16′ and long. 4° 35′ W.

Whence the approximate value of r is found at any other geographical position in the British Islands by the formula

$$r = +0{\cdot}571 + {\cdot}00007\,a - {\cdot}00013\,b\,;$$

the longitude and latitude of the general central station being the origin of the coordinates a and b.

The points furnished by the several observations for the nearest adjacent isoclinal line, computed in the manner above described, are inserted in the general table which closes this division of the report. The table is in two parts; the one containing the observations, the other the deductions. In the first part are shown the observed dip, the latitude and longitude of the station, the date, the observer, and a reference to the particular table in which all the details connected with the observations may be examined. In the division which contains the deductions, are shown the dip reduced to the mean epoch of the 1st January, 1837; the differences of latitude and longitude between the station and the point furnished by it for the nearest isoclinal line; the latitude and longitude of the points, and the values of u and r, employed in their deduction.

By the method thus described, the transfer of the observation to the isoclinal line involves no other material inaccuracy than such as may be occasioned by incorrectness in the employed values of u and r. We may, therefore, examine the probable limit of the inaccuracy which may be thus incurred;—30 minutes of dip is the extreme fractional amount in any case for which a deduction is required: if we suppose an error in the assumed value of r equal to 0·01, which is nearly a fourth of the extreme difference found for England, Ireland and Scotland,—the corresponding error in the geographical distance of the point from the station will be less than one mile. An error of 1° in the value of u, in the same extreme case of a fractional amount of 30′ of dip, would cause an error in the position assigned to the point of less than one mile in latitude, and half a mile in longitude. We may hence estimate the probable limits of inaccuracy in the extreme cases alluded to. It is obvious that when the fractional minutes in the observation are less than thirty, these limits are proportionally reduced; and it is further plain that errors thus occasioned will be of a contrary nature to each other, according as the fractional minutes are in excess or in defect of the degree which the line represents. When, therefore, the observations are numerous, and fall on both sides of the lines, as is the case in this survey, a mutual compensation is afforded, and whatever small inaccuracies there may be in the values of u and r, their ultimate effect on the lines may be regarded as wholly insensible.

If the observations at each station were free from instrumental defect and local influence,—and if they were continued sufficiently long at each station to furnish its mean dip independent of diurnal and irregular fluctuations,—the points computed from them and transferred to a map would require merely to be connected in order to form the isoclinal line. As might be expected, however, the results of the observations are far from presenting this perfect accordance, especially in Scotland, where the prevalence of igneous rocks produces much disturbing action. An examination of the map, however, in which the points, and the stations they are derived from, are inserted, will show that, notwithstanding the disturbing causes referred to, they do arrange themselves in such manner as to leave very little uncertainty in any quarter in tracing the position and direction of each isoclinal line. Each line thus becomes an independent determination, derived from observations which belong to itself alone, and uninfluenced by those which differ more than thirty minutes from the degree which the line represents*.

By this method of combination, any departure from systematic arrangement which might exist in any one of the lines passing across the British Islands, would become manifest at once to the eye. Individual stations there are, particularly in Scotland and the north of Ireland, which throw their points to some distance from their respective lines. In some very few cases, a group of neighbouring stations appears to be similarly affected. The most prominent instance of this is in North Wales, where there appears a decided disposition of the majority of the

* This has been strictly adhered to in the table everywhere; and in the map everywhere over the surface of the land. The lines are extended in the map a short distance *beyond* the land; and as the observations which justify this extension are few in comparison with those in other parts of the map, the determinations which fall nearly midway between two lines have, in these few cases, been given a bearing on the lines on either side of them.

points to fall to the south of the line of 71°, contrasted with and counterbalanced by an opposite tendency of the points furnished for the same line on the east of Ireland*. A more extensive research is necessary to determine whether, by multiplying the number of stations in these localities, this apparent irregularity would disappear, or whether the observations referred to truly represent what may be termed a district anomaly. Whilst, however, on minute examination the eye may rest on single stations, or on groups, which present examples of the slight irregularities here referred to, it cannot fail, on the general aspect of the map, to be struck by the absence of any important unsymmetrical inflections, and by the obvious general systematic arrangement of the terrestrial magnetism indicated by the lines. Here, as elsewhere, they present the features of the general magnetic system; the effects of local and partial disturbance being indeed discernible on close examination, but not being found of sufficient comparative magnitude to influence the general representation.

The lines of dip as they appear on the map are slightly curved, being convex towards the S.E. If the extreme points of each line were connected by an arc of a great circle, the curvature of the arc, on the projection which is here employed, would be in the opposite direction to that of the isoclinal lines, or the convexity would be towards the N.W. Their departure from such a straight line on the surface of the globe (or their difference from great circles) is greater therefore than appears in this projection.

* This apparent dislocation of the line of 71° between England and Ireland was noticed by Mr. Fox in the Report of the Royal Cornwall Polytechnic Society for 1835. No trace of a corresponding irregularity occurs in the continuity of the line 72° in crossing the Irish Channel.

GENERAL TABLE. DIP.

OBSERVATIONS. Dips from 73° 30′ to 72° 30′.							DEDUCTIONS.						
									Corresponding points in the Isoclinal line of 73°.				
											The line of 73° in.		
Station.	Lat.	Long.	Date.	Observer.	Table.	Dip.	Reduction to Epoch.	Dip at the Mean Epoch, 1 Jan. 1837.	Δ Lat.	Δ Long.	Lat.	Long.	Values employed of u and r.
	° ′	° ′				° ′		° ′	′	′	° ′	° ′	
Lerwick*	60 09	1 07	July 24–27, 1838	R	XXII.	73 44·9	+3·8	73 48·7					
Aberdeen	57 09	2 05	July 18, 1838	R	XXII.	72 27·6	+3·7	72 31·3	+44	+55	57 53	3 00	$u = -55°·75$, $r = 0'·543$
Kirkwall	59 00	2 58	July 31, 1838	R	XXII.	73 20·4	+3·8	73 24·2	−36	−55	58 24	2 03	$u = -51°·75$, $r = 0'·531$
Wick	58 24	3 05	Aug. 8, 1838	R	XXII.	73 19·9	+3·9	73 23·8	−35	−49	57 49	2 16	$u = -53°·5$, $r = 0'·540$
Gordon Castle	57 37	3 09	— 25, 1836	S	XXIV.	72 40·7	−0·8	72 40·0	+31	+40	58 08	3 49	
Golspie	57 58	3 57	— 23, 1836	S	XXIV.	72 55·5	−0·8	72 54·7	+ 8	+10	58 06	4 07	
Golspie	57 58	3 57	— 10, 1838	R	XXII.	73 04·3	+3·9	73 08·2	−12	−17	57 46	3 40	
Inverness	57 28	4 11	— 20 & 24, 1836	S	XXIV.	72 46·4	−0·9	72 45·5	+21	+28	57 49	4 39	$u = -54°·5$, $r = 0'·546$
Inverness	57 28	4 11	— 13–14, 1838	R	XXII.	72 46·2	+3·9	72 50·1	+14	+19	57 42	4 30	
Fort Augustus	57 08	4 40	— 19, 1836	S	XXIV.	72 40·3	−0·9	72 39·4	+31	+38	57 39	5 18	$u = -55°·75$, $r = 0'·550.$
Artornish	56 33	5 48	— 16, 1836	S	XXIV.	72 42·8	−0·9	72 41·9	+27	+34	57 00	6 22	$u = -54°·75$, $r = 0'·553'$
Tobermorie	56 38	6 01	— 10, 1836	S	XXIV.	73 07·6	−0·9	73 06·7	−10	−13	56 28	5 48	
Loch Slapin	57 14	6 02	— 14, 1386	S	XXIV.	73 02·1	−0·9	73 01·2	− 2	− 3	57 12	5 59	
Loch Scavig	57 14	6 07	— 12, 1836	S	XXIV.	73 05·2	−0·9	73 04·4	− 7	−10	57 07	5 57	

* The Dip at Lerwick 73° 48′·7 belongs to the Isoclinal line of 74°; and is the only dip exceeding 73° 30′.

	OBSERVATIONS.						DEDUCTIONS.						
	Dips from 72° 30′ to 71° 30′.								Corresponding points in the Isoclinal line of 72°.				
Station.	Lat.	Long.	Date.	Observer.	Table.	Dip.	Reduction to Epoch.	Dip at the the Mean Epoch, 1 Jan. 1837.	Δ Lat.	Δ Long.	The line of 72° in. Lat.	The line of 72° in. Long.	Values employed of u and r.
	° ′	° ′				° ′	′	° ′	′	′	° ′	° ′	
Berwick	55 45	2 00	Sept. 17 & 18, 1838	R	XXII.	71 41·9	+4·1	71 46·0	+22	+24	56 07	2 24	
Dryburgh	55 34	2 39	— 7, 1836	S	XXIV.	71 33·6	−0·8	71 32·8	+42	+45	56 16	3 24	$u=-58°·75$
Melrose	55 35	2 44	— 6, 1836	S	XXIV.	71 36·8	−0·8	71 36·0	+37	+40	56 12	3 24	$r=0′·555$
Melrose	55 35	2 44	Aug. 26, 1837	F	XXV.	71 38·0	+1·6	71 39·6	+31	+34	56 06	3 18	
Alford..................	57 13	2 45	— 27 & 29, 1836	S	XXIV.	72 21·9	−0·8	72 21·1	−32	−41	56 41	2 04	$u=-55°·25$
Rhynie	57 20	2 50	— 26, 1836	S	XXIV.	72 25·6	−0·8	72 24·8	−38	−48	56 42	2 02	$r=0′·544$
Carlisle	54 54	2 54	Sept. 29, 1837	P	XIII.	71 28·5	+1·8	71 30·3	+46	+46	55 40	3 40	$u=-59°·75$ $r=0′·562$
Newport	56 25	2 55	— 1, 1836	S	XXIV.	72 17·4	−0·8	72 10·1	−15	−17	56 10	2 38	$u=-57°·0$ $r=0′·550$
Gretna Green.........	55 01	3 04	— 6, 1837	F	XXV.	71 29·0	+1·4	71 30·4	+46	+46	55 47	3 50	$u=-59°·75$ $r=0′·562$
Kirkaldy	56 07	3 09	— 3, 1836	S	XXIV.	72 10·9	−0·8	72 10·1	−15	−17	55 52	2 52	
Edinburgh	55 57	3 11	— 8, 1836	S	XXIV.	71 50·3	−0·9	71 49·4	+16	+18	56 13	3 29	$u=-57°·75$
Edinburgh	55 57	3 11	Aug. 28, 1837	F	XXV.	71 50·0	+1·6	71 51·6	+13	+15	56 10	3 26	$r=0′·555$
Blairgowrie............	56 36	3 18	— 31, 1836	S	XXIV.	71 54·7	−0·8	71 53·5	+10	+12	56 46	3 30	
Braemar	57 01	3 25	— 30, 1836	S	XXIV.	72 14·1	−0·8	72 13·3	−20	−25	56 41	3 00	$u=-55°·5$ $r=0′·547$
Moffat..................	55 20	3 27	— 6, 1837	F	XXV.	71 40·0	+1·6	71 41·6	+28	+30	55 48	3 57	$u=-59°·0$ $r=0′·560$
Dunkeld	56 35	3 33	Sept. 20 & 21, 1838	R	XXII.	72 23·1	+4·1	72 27·2	−41	−49	55 54	2 44	$u=-56°·5$
Linlithgow	55 59	3 37	— 30, 1837	F	XXV.	71 59·0	+1·6	72 00·6	− 1	− 1	55 58	3 36	$r=0′·551$
Culgruff	54 58	4 00	— 8, 1838	R	XXII.	71 35·7	+4·1	71 39·8	+31	+32	55 29	4 32	$u=-59°·5$ $r=0′·564$
Glasgow	55 51	4 14	— 9, 1836	S	XXIV.	72 01·6	−0·7	72 00·9	− 1	− 1	55 50	4 13	
Glasgow	55 51	4 14	— 4, 1837	F	XXV.	72 05·0	+1·6	72 06·6	− 9	−10	55 42	4 02	$u=-57°·5$
Jordan Hill............	55 54	4 21	— 13 & 14, 1838	R	XXIV.	72 20·0	+4·1	72 24·1	−36	−41	55 18	3 40	$r=0′·558$
Jordan Hill............	55 54	4 21	— 11–14 1838	S	XVII.	72 14·3	+4·1	72 18·4	−28	−32	55 26	3 49	

Loch Lomond	° ′ 56 13	° ′ 4 40	Sept. 1,	1837	F	XXV.	72 15·0	+1·6	72 16·6	−25	−29	55 48	4 11	u=−57°·0, r=0′·557
Helensburgh	56 00	4 41	July 2&8, Sept. 13,	1836	S	XXIV.	72 16·7	−0·9	72 15·8	−24	−28	55 36	4 13	
Helensburgh	56 00	4 41	Sept. 10,	1838	S	XVII.	72 17·0	+4·1	72 21·1	−32	−37	55 28	4 04	
Cumbray......	55 48	4 52	July 30,	1836	S	XXIV.	72 01·1	−1·0	72 00·1	0	0	55 48	4 52	u=−59°·0
Loch Ryan.....	54 55	4 59	Sept. 18,	1836	S	XXIV.	71 43·3	−0·7	71 42·6	+26	+27	55 21	5 26	
Inverary	56 15	5 04	Aug. 31,	1837	F	XXV.	72 07·0	+1·6	72 08·6	−14	−15	56 01	4 49	u=−58°·5, r=0′·557
Glencoe	56 39	5 07	— 17,	1836	S	XXIV.	72 17·1	−0·9	71 16·2	−25	−28	56 14	4 39	
Loch Ridan	55 57	5 10	— 5,	1836	S	XXIV.	72 16·6	−1·0	72 15·6	−24	−27	55 33	4 43	
Loch Ranza	55 42	5 17	Sept. 16,	1836	S	XXIV.	72 22·9	−0·7	72 22·2	−33	−38	55 09	4 39	u=−57°·5, r=0′·562
Loch Gilphead	56 04	5 28	Aug. 7,	1836	S	XXIV.	72 07·6	−1·0	72 06·6	−10	−12	55 54	5 16	
Campbeltown	55 23	5 38	Sept. 16,	1836	S	XXIV.	71 55·9	−0·7	71 55·2	+ 6	+ 7	55 29	4 45	
Bangor	54 39	5 42	Sept. 21,	1836	S	XXXVI.	71 39·2	−0·7	71 38·5	+32	+33	55 11	6 15	u=−59°·5, r=0′·571
Castle Duart	56 31	5 45	Aug. 9,	1836	S	XXIV.	72 15·2	−0·9	72 14·3	−23	−27	56 08	5 18	u=−56°·0, r=0′·558
Cushendal	55 04	6 05	Aug. 28,	1835	F	XXXVI.	72 00·0	−3·2	71 56·8	+ 4	+ 5	55 08	6 10	
Armagh	54 21	6 39	Oct. 14 & 15,	1834	L	XXXVI.	71 42·2	−5·3	71 36·9	+37	+35	54 58	7 14	u=−60°·0, r=0′·576
Armagh	54 21	6 39	Nov. 2,	1838	R	XXXVIII.	71 40·5	+4·4	71 44·9	+23	+22	54 44	7 01	
Carn	55 15	7 15	Oct. 21,	1834	L	XXXVI.	72 00·9	−5·2	71 55·7	+ 6	+ 7	55 21	7 22	
Londonderry	54 59	7 19	Nov. 5 & 6,	1838	R	XXXVIII.	72 02·3	+4·4	72 06·7	−10	−10	54 49	7 09	u=−58°·25, r=0′·572
Strabane	54 49	7 28	Oct. 23,	1834	L	XXXVI.	73 00·0	−5·2	71 54·8	+ 7	+ 7	54 56	7 35	
Edgeworthstown	53 42	7 33	Nov. 19,	1838	R	XXXVIII.	71 29·8	+4·5	71 34·3	+38	+36	54 20	8 09	u=−60°·75, r=0′·583
Enniskillen...............	54 21	7 38	Oct. 24,	1834	L	XXXVI.	72 00·0	−5·2	71 54·8	+ 8	+ 9	54 29	7 47	u=−59°·25, r=−0′·581
Markree	54 12	8 26	Aug. 21,	1835	L	XXXVI.	72 06·3	−3·2	72 03·1	− 5	− 5	54 09	8 21	
Markree	54 12	8 26	Nov. 10,	1838	R	XXXVIII.	72 02·0	+4·5	72 06·5	−10	−11	54 04	8 17	
Lissadel	54 23	8 33	Sept. 19–25,	1838	S	XL.	71 56·0	+4·2	72 00·2	0	0	54 23	8 33	
Gallhorich	53 25	9 05	Aug.19,	1835	F	XXXVI.	71 41·0	−3·2	71 37·8	+33	+31	53 58	9 36	u= -60°·75, r=0′·589
Ballina....................	54 07	9 07	— 22,	1835	L	XXXVI.	72 11·0	−3·3	72 07·7	−10	−11	53 57	8 56	u=−59°·0, r=−0′·583
Ballina....................	54 07	9 07	— 25,	1835	F	XXXVI.	72 07·0	−3·2	72 03·8	− 5	− 6	54 02	9 01	
Loch Conn...............	53 58	9 10	— 24,	1835	F	XXXVI.	72 08·0	−3·2	72 04·8	− 7	− 7	53 51	9 03	
Westport.....	53 48	9 29	— 24,	1835	F	XXXVI.	72 03·0	−3·2	71 59·8	0	0	53 48	9 29	
Westport..................	53 48	9 29	Nov. 13,	1838	R	XXXVIII.	71 59·0	+4·5	72 03·5	− 5	− 5	53 43	9 24	
Belmullet..................	54 13	9 57	Aug.24,	1835	L	XXXVI.	72 13·4	−3·2	72 10·2	−13	−15	54 00	9 42	
Achil Ferry........... ...	53 56	9 52	— 25,	1835	L	XXXVI.	72 06·5	−3·2	72 03·3	− 5	− 5	53 51	9 47	
Clifden....................	53 29	9 59	— 22,	1835	F	XXXVI.	71 52·0	−3·2	71 48·8	+17	+16	53 46	10 15	u=−60°·2, r=0′·590

Station.	OBSERVATIONS. Dips from 71° 30′ to 70° 30′. Lat.	Long.	Date.	Observer.	Table.	Dip.	DEDUCTIONS. Reduction to Epoch.	Dip at the Mean Epoch, Jan. 1, 1837.	Corresponding points in the Isoclinal Line of 71°. Δ Lat.	Δ Long.	The line of 71° in. Lat.	Long.	Values employed of u and r.
	° ′	° ′				° ′	′	° ′	′	′	° ′	° ′	
Flamborough	54 08	0 08	June 11, 1837	P	XIII.	70 36·9	+1·1	70 38·0	+35	+31	54 43	0 39	$u=-62°·25$
Bridlington...............	54 08	0 14	May 3, 1838	R	XV.	70 38·8	+3·2	70 42·0	+27	+24	54 35	0 38	$r=0'·560$
Scarborough	54 17	0 24	June 13, 1837	P	XIII.	70 41·8	+1·1	70 42·9	+25	+23	54 42	0 47	
Scarborough	54 17	0 24	May 1, 1838	R	XV.	70 43·0	+3·2	70 46·2	+21	+18	54 38	0 42	
Whitby	54 29	0 37	June 9, 1837	P	XIII.	70 57·9	+1·1	70 59·0	+ 2	+ 1	54 31	0 38	
York	53 58	1 05	July 1837	P	XIII.	70 48·8	+1·3	70 50·1	+16	+14	54 14	1 19	$u=-62°·5$
York	53 57	1 06	April 28, 1838	R	XV.	70 45·2	+3·2	70 48·4	+18	+16	54 15	1 22	$r=0'·565$
Wadworth	53 28	1 07	May 9, 1838	R	XV.	70 27·5	+3·3	70 30·8	+46	+38	54 14	1 45	$n=-63°·5$
Doncaster	53 31	1 07	June 2, 1837	P	XIII.	70 30·2	+1·0	70 31·2	+46	+37	54 17	1 44	$r=0'·568$
Hambleton...............	54 20	1 15	July 7, 1837	P	XIII.	71 04·0	+1·2	71 05·2	− 8	− 7	54 12	1 08	$u=-62°·0$
Osmotherly...............	54 22	1 18	June 6, 1837	P	XIII.	71 03·2	+1·0	71 04·2	− 6	− 6	54 16	1 12	$r=0'·563$
Thirsk	54 14	1 21	June 5, 1837	P	XIII.	70 59·2	+1·0	71 00·2	0	0	54 14	1 21	
Sheffield	53 22	1 31	June 17, 1837	P	XIII.	70 29·7	+1·1	70 30·8	+46	+38	54 08	2 09	$u=-63°·5$ $r=0'·568$
Darlington	54 32	1 33	Aug. 21, 1837	F	XI.	71 07	+1·5	71 08·5	−13	−12	54 19	1 21	$u=-61°·25$
Studley Park	54 08	1 34	Aug. 14, 1837	F	XI.	70 56	+1·5	70 57·5	+ 4	+ 4	54 12	1 38	$r=0'·562$
Newcastle	54 58	1 38	Sept. 30, 1837	P	XIII.	71 18·1	+1·8	71 19·9	−31	−31	54 27	1 07	$u=-60°·25$
Newcastle	54 58	1 36	Aug. 28, 1838	S	XVII.	71 09	+4·0	71 13·0	−20	−20	54 38	1 16	$r=0'·558$
Newcastle	54 58	1 36	Aug. 28, 1838	R	XV.	71 13	+4·0	71 17·0	−24	−24	54 34	1 12	
Alnwick Castle	55 25	1 42	Aug. 31, 1838	S	XVII.	71 22·6	+4·0	71 26·6	−41	−43	54 44	0 59	$u=-59°·5$ $r=0'·556$
Belsay.....................	55 07	1 53	Aug. 25, 1837	F	XI.	71 17	+1·6	71 18·6	−29	−29	54 38	1 24	$u=-60°·25$
Shull	54 43	2 00	Aug. 19, 1837	F	XI.	71 14	+1·5	71 15·5	−24	−24	54 19	1 36	$r=0'·558$
Manchester...............	53 28	2 14	Oct. 14, 1836	L	XII.	70 47·7	−0·5	70 47·2	+20	+17	53 48	2 31	$u=-63°·25$ $r=0'·571$
Stonehouse..............	54 55	2 44	Sept. 1, 1838	S	XVII.	71 19·5	+4·0	71 23·5	−36	−36	54 19	2 08	$u=-60°·25$
Stonehouse...............	54 55	2 44	Sept. 1, 1838	R	XV.	71 24·1	+4·0	71 28·1	−43	−43	54 12	2 01	$r=0'·558$
Penrith	54 40	2 45	Sept. 28, 1837	P	XIII.	71 23·4	+1·8	71 25·2	−39	−39	54 01	2 06	
Garstang..................	53 54	47	Sept. 12, 1837	F	XI.	70 59	+1·7	71 00·7	− 1	− 1	53 53	2 46	

Busco Bridge............	53° 39′	2° 50′	Sept. 12,	1837	F	XI.	70° 45′	+1′·7	70° 46′·7	+20′	+18′	53° 59′	3° 08′	$u = -62°·75$
Calderstone................	53 23	2 53	Aug. 12,	1837	P	XIII.	70 43·5	+1·5	70 45·0	+22	+20	53 45	3 13	$r - 0'·572$
Near Liverpool	53 25	2 55	Sept. 23,	1837	F	XI.	70 44	+1·8	70 45·8	+21	+19	53 46	3 14	$u = -60°·75$
Bowness	54 22	2 55	Sept. 25,	1837	P	XIII.	71 18·4	+1·8	71 20·2	−31	−30	53 51	2 25	$r = 0'·562$
Patterdale	54 32	2 56	Sept. 27,	1837	P	XIII.	71 19·6	+1·8	71 21·4	−33	−32	53 59	2 24	
Liverpool	53 25	2 58	Sept. 19,	1837	F	XI.	70 39	+1·7	70 40·7	+29	+26	53 54	3 24	$u = -62°·75$
Birkenhead...............	53 24	3 00	Aug. 8,	1836	L	XII.	70 49·1	−0·9	70 48·2	+18	+16	53 42	3 16	$r = 0'·572$
Birkenhead...............	53 24	3 00	Aug. 26,	1837	P	XIII.	70 39·4	+1·6	70 41·0	+29	+25	53 53	3 25	
Birkenhead...............	53 24	3 00	Sept. 18,	1837	S	XVII.	70 35·1	+1·7	70 36·8	+36	+31	54 00	3 31	
Birkenhead...............	53 24	3 00	Oct. 1,	1837	R	XV.	70 35·7	+1·8	70 37·5	+35	+30	53 59	3 30	
Grassmere	54 27	3 01	Sept. 9,	1837	F	XI.	71 13	+1·7	71 14·7	−22	−21	54 05	2 40	$u = -63°·75$
Coniston	54 22	3 05	Sept. 25,	1837	P	XIII.	71 19·5	+1·8	71 21·3	−33	−32	53 49	2 33	$r = 0'·564$
Keswick	54 37	3 09	Sept. 7,	1837	F	XI.	71 14	+1·6	71 15·6	−24	−23	54 13	2 46	
Skiddaw	54 40	3 09	Sept. 7,	1837	F	XI.	71 15	+1·6	71 16·6	−26	−25	54 14	2 44	
Coed	53 11	3 12	Sept. 20,	1837	P	XIII.	70 40·9	+1·7	70 42·6	+27	+23	53 38	3 35	$u = -60°·75$, $r = 0'·579$
Whitehaven...............	54 33	3 33	Aug. 16,	1838	S	XVII.	70 10·9	+3·9	71 14·8	−22	−21	54 11	3 12	$u = -60°·75$, $r = 0'·564$
Capelcarig	53 06	3 53	Sept. 3,	1835	F	XI.	70 48	−3·2	70 41·8	+23	+20	53 29	4 13	$u = -63°·0$
Llanberris	53 07	4 03	Sept. 1,	1835	F	XI.	70 57	−3·2	70 53·8	+ 9	+ 8	53 16	4 11	$r = 0'·579$
Bangor	53 14	4 06	Sept. 1,	1835	F	XI.	71 02	−3·2	70 58·8	+ 1	+ 1	53 15	4 07	
Carnarvon	53 09	4 14	Sept. 1,	1835	F	XI.	70 58	−3·2	70 54·8	+ 8	+ 7	53 17	4 21	
Pwllheli	52 55	4 23	Oct. 14,	1837	R	XV.	70 32·5	+1·9	70 34·4	+36	+30	53 31	4 53	$u = -63°·5$, $r = 0'·581$
Douglas	54 10	4 28	Aug. 17,	1837	P	XIII.	71 22·2	+1·5	71 23·7	−37	−32	53 33	3 56	$u = -62°·75$
Douglas	54 10	4 28	Sept. 21,	1837	R	XV.	71 20·3	+1·7	71 22·0	−33	−29	53 37	3 59	$r = 0'·571$
Holyhead	53 19	4 37	Sept. 1,	1835	F	XI.	71 04	−3·2	71 00·8	− 1	− 1	53 18	4 36	$u = -64°·5$
Holyhead	53 19	4 37	April 27,	1836	L	XII.	71 08·5	−1·6	71 06·9	−11	− 9	53 08	4 28	$r = 0'579$
Castleton.................	54 04	4 40	Aug. 18,	1837	P	XIII.	71 22·5	+1·5	71 24·0	−37	−32	53 27	4 08	$u = -62°·75$
Peel town	54 13	4 43	Aug. 18,	1837	P	XIII.	71 23·9	+1·5	71 25·4	−40	−35	53 33	4 08	$r = 0'·571$
Carlingford..	54 02	6 11	Oct. 13,	1834	L	XXXVI.	71 30·2	−5·3	71 24·9	−39	−36	53 23	5 35	$u = -60°·75$, $r = 0'·577$
Rathdrum	52 55	6 14	Sept. 3,	1835	L	XXXVI.	70 53·5	−3·2	70 50·3	+15	+13	53 10	6 27	
Dublin	53 21	6 16	Aug.,Sep.,Oct	1834	L	XXXVI.	71 06·1	−5·3	71 00·8	− 1	− 1	53 20	6 15	$u = -60°·75$
Dublin	53 21	6 16	Sep. & Nov.	1835	L	XXXVI.	71 04·8	−3·0	71 01·8	− 3	− 2	53 18	6 14	$r = 0'·577$
Dublin.....................	53 21	6 16	Aug. 17,	1835	F	XXXVI.	70 59·0	−3·3	70 55·7	+ 7	+ 6	53 28	6 22	

OBSERVATIONS.							DEDUCTIONS.						
	Dips from 71° 30′ to 70° 30′.								Corresponding points in the Isoclinal Line of 71°.				
											The line of 71° in.		
Station.	Lat.	Long.	Date.	Observer.	Table.	Dip.	Reduction to Epoch.	Dip at the Mean Epoch, Jan. 1, 1837.	Δ Lat.	Δ Long.	Lat.	Long.	Values employed of u and r.
Dublin	53° 21′	6° 16′	Apr. & Aug. 1836	L	XXXVI.	71° 02′·3	−1′·3	71° 01′·0	− 2′	− 1′	53° 19′	6° 15′	$u=-62°·25$ $r=0'·582$
Dublin	53 21	6 16	July & Oct. 1836	S	*	71 03·3	−0·8	71 02·5	− 3	− 3	53 18	6 13	
Dublin	53 21	6 16	Oct. 29 & 30, 1838	R	XXXVII	70 59·8	+4·4	71 04·2	− 6	− 6	53 15	6 10	
Dublin	53 21	6 16	Aug. & Sept. 1838	L	XXXVI.	70 56·3	+4·0	71 00·3	0	0	53 21	6 16	
Gorey	52 40	6 17	Sept. 3, 1835	L	XXXVI.	70 55·8	−3·2	70 52·6	+11	+10	52 51	6 27	
Broadway	52 13	6 24	Sept. 2, 1835	L	XXXVI.	70 35·9	−3·2	70 32·7	+40	+31	52 53	6 55	$u=-64°·25$ $r=0'·596$
Waterford	52 15	7 08	Sept. 1, 1835	L	XXXVI.	70 50·5	−3·2	70 47·3	+18	+14	52 33	7 22	
Waterford	52 15	7 08	Oct. 4, 1838	R	XXXVIII.	70 45·8	+4·2	70 50·0	+15	+12	52 30	7 20	
Youghal	51 57	7 50	Dec. 29. 1835	S	XXXVI.	70 39·4	−2·4	70 37·0	+35	+27	52 32	8 15	
Shannon Harbour	53 14	7 53	Oct. 26, 1838	R	XXXVIII.	71 23·2	+4·4	71 27·6	−41	−37	52 33	7 16	$u=-61°·5$ $r=0'·587$
Fermoy	52 07	8 16	Dec. 2, 1834	S	XXXVI.	70 48·2	−5·4	70 43·2	+26	+20	52 33	8 36	$u=-64°·0$ $r=0'·598$
Cork	51 54	8 26	Aug. 31, 1835	L	XXXVI.	70 43·1	−3·2	70 39·9	+30	+23	52 24	8 49	
Cork	51 54	8 26	Oct. 6–7, 1838	R	XXXVIII.	70 39·4	+4·2	70 43·6	+25	+19	52 19	8 45	
Limerick	52 40	8 36	July & Aug. 1834	S	XXXVI.	71 03·5	−5·8	70 57·7	+ 3	+ 3	52 43	8 39	$u=-62°·25$ $r=0'·593$
Limerick	52 40	8 36	July, & Dec. 1835	S	XXXVI.	71 06·1	−3·1	71 03·0	− 5	− 4	52 35	8 32	
Limerick	52 40	8 36	Aug. 29, 1835	L	XXXVI.	71 02·9	−3·2	70 59·7	+ 0	+ 0	52 40	8 36	
Limerick	52 40	8 36	Feb. & May, 1836	S	XXXVI.	71 01·1	−1·8	70 59·3	+ 1	+ 1	52 41	8 37	
Limerick	52 40	8 36	Oct. 22 & 23, 1838	R	XXXVIII.	70 59·6	+4·3	71 03·9	− 6	− 5	52 34	8 31	
Kiltanon	52 52	8 43	Oct., 1834 / Dec., 1835	S	XXXVI.	71 21·3	−3·8	71 17·5	−25	−21	52 27	8 22	
Ennis	52 51	8 58	Aug. 28, 1835	L	XXXVI.	71 13·2	−3·2	71 10·0	−15	−13	52 36	8 45	
Galway	53 17	9 04	Aug. 19, 1835	F	XXX.	71 26·0	−3·3	71 22·7	−34	−31	52 43	8 33	$u=-61°·0$ $r=0'·598$
Galway	53 17	9 04	Aug. 28, 1835	L	XXXVI.	71 32·9	−3·2	71 29·7	−44	−40	52 31	8 24	
Killarney	52 03	9 31	Oct. 4, 1834	S	XXXVI.	71 04·5	−5·4	70 59·1	+ 1	+ 1	52 04	9 32	$u=-63°·5$ $r=0'·601$
Killarney	52 03	9 31	Oct. 17 & 19, 1838	R	XXXVIII.	70 51·1	+4·3	70 55·4	+ 7	+ 6	52 10	9 38	
Glengariff	51 45	9 31	Sept. 27 & 28, 1834	S	XXXVI.	71 01·5	−5·4	70 56·1	+ 6	+ 5	51 51	9 36	
Ballybunian	52 30	9 41	Nov. 8, 1835		XXXVI.	71 19·5	−2·7	71 16·8	−25	−21	52 05	9 20	$u=-62°·5$ $r=0'·598$
Valencia	51 56	10 17	Nov. 12, 1835	S	XXXVI.	71 05·4	−2·7	71 02·7	− 4	− 4	51 52	10 13	$u=-63°·25$ $r=0'·603$
Valencia	51 56	10 17	Oct. 12 & 13, 1838	R	XXXVIII.	70 52·0	+4·4	70 56·4	+ 5	+ 5	52 01	10 22	
Dingle	52 08	10 17	Nov. 18, 1835	S	XXXVI.	71 08·1	−2·7	71 05·4	− 8	− 7	52 00	10 10	

	OBSERVATIONS.						DEDUCTIONS.						
	Dips from 70° 30′ to 69° 30′.								Corresponding points in the Isoclinal Line of 70°.				
											The line of 70° in.		
Station.	Lat.	Long.	Date.	Observer.	Table.	Dip.	Reduction to Epoch.	Dip at the Mean Epoch, Jan. 1, 1837.	Δ Lat.	Δ Long.	Lat.	Long.	Values employed of u and r.
Lowestoffe	52° 28′	−1° 50′	May 24, 1838	R	XV.	69° 29′·2	+3′·4	69° 32′·6	+44′	+31′	53° 12′	−1° 19′	$u=-66°·5$, $r=0'·575$
Cromer	52 56	−1 19	May 21, 1838	R	XV.	69 46·1	+3·3	69 49·4	+17	+12	53 13	−1 07	$u=-65°·5$, $r=0'·571$
Lynn	52 45	−0 25	Oct. 10, 1836	L	XII.	69 53·2	−0·5	69 52·7	+11	+ 8	52 56	−0 17	
Cambridge	52 13	−0 07	Oct. 8, 1836	L	XII.	69 41·5	−0·5	69 41·0	+30	+24	52 43	0 17	$u=-64°·5$, $r=0'·568$
Louth	53 19	0 0	May 16, 1838	R	XV.	70 19·4	+3·3	70 22·7	−36	−29	52 43	−0 29	$u=-64°·25$, $r=0'·567$
Nottingham	52 57	1 08	May 12, 1838	R	XV.	70 16·3	+3·3	70 19·6	−31	−24	52 26	0 44	$u=-65°·25$, $r=0'·576$
Daventry	52 16	1 08	Sept. 1, 1837	R	XV.	69 41·1	+1·6	69 42·7	+27	+21	52 43	1 29	
Matlock	53 08	1 32	Oct. 12, 1836	L	XII.	70 29·2	−0·5	70 28·7	−45	−37	52 23	0 55	$u=-61°·0$, $r=0'·572$
Matlock	53 08	1 32	Aug. 9, 1837	F	XI.	70 19·0	+1·5	70 20·5	−32	−26	52 36	1 06	
Birmingham	52 28	1 53	July 3, 1837	P	XIII.	70 07·2	+1·3	70 08·5	−13	−10	52 15	1 43	$u=-65°·0$, $r=0'·577$
Birmingham	52 28	1 53	Sept. 4, 1837	R	XV.	70 00·5	+1·6	70 01·1	− 2	− 1	52 26	1 52	
Stafford	52 48	2 06	Sept. 7, 1837	R	XV.	70 09·7	+1·6	70 11·3	−18	−14	52 30	1 52	
Malvern	52 07	2 19	Sept. 5, 1835	F	XI.	70 11·0	−3·2	70 07·8	−12	− 9	51 55	2 10	$u=-66°·0$, $r=0'·583$
Combe House	51 31	2 34	July 2, 1838	F	XI.	69 32·0	+3·6	69 35·6	+38	+26	52 9	3 00	$u=-67°·0$, $r=0'·587$
Ross	51 55	2 35	Sept. 8, 1835	F	XI.	70 0·0	−3·1	69 56·9	+ 5	+ 3	52 00	2 38	
Clifton	51 27	2 35	Aug. 29, 1836	L	XII.	69 42·6	−0·8	69 41·8	+29	+20	51 56	2 55	
Clifton	51 27	2 35	Oct. 22, 1837	R	XV.	69 34·0	+2·0	69 36·0	+38	+26	52 05	3 01	
Chepstow	51 38	2 40	Sept. 9, 1835	F	XI.	69 48·0	−3·1	69 44·9	+24	+16	52 02	2 56	
Chepstow	51 38	2 40	Aug. 12, 1836	L	XII.	69 47·9	−0·9	69 47·0	+20	+14	51 58	2 54	
Hereford	52 04	2 44	Aug. 10, 1836	L	XII.	70 07·1	−0·9	70 06·2	−10	− 7	51 54	2 37	$u=-66°·0$, $r=0'·583$
Shrewsbury	52 43	2 45	April 25, 1836	L	XII.	70 27·6	−1·6	70 26.0	−41	−31	52 02	2 14	$u=-64°·5$, $r=0'·577$
Shrewsbury	52 43	2 45	Sept. 19, 1837	S	XVII.	70 24·5	+1·7	70 26·2	−41	−31	52 02	2 14	
Merthyr	51 43	3 21	Sept. 23, 1837	S	XVIII.	70 03·9	+1·7	70 05·6	− 8	− 6	51 35	3 15	$u=-66°·5$, $r=0'·590$
Brecon	51 57	3 21	Sept. 23, 1837	S	XVIII.	70 03·2	+1·7	70 04·9	− 7	− 5	51 50	3 16	

OBSERVATIONS. Dips between 70° 30′ and 69° 30′. Station.	Lat.	Long.	Date.	Observer.	Table.	Dip.	DEDUCTIONS. Reduction to Epoch.	Dip at the Mean Epoch, Jan. 1, 1837.	Corresponding points in the Isoclinal Line of 70°. Δ Lat.	Δ Long.	The line of 70° in: Lat.	The line of 70° in: Long.	Values employed of u and r.
Dunraven Castle.........	51 28	3 37	Sept. 28, 1837	S	XVII.	69 45·7	+1·8	69 47·5	+19	+14	51 47	3 51	$u=-66°·5$, $r=0'·590$
Neath	51 40	3 46	Sept. 11, 1835	F	XI.	69 57	−3·1	69 53·9	+ 9	+ 7	51 49	3 53	
Swansea	51 36	3 55	Oct. 27, 1837	R	XV.	69 46·7	+2·0	69 48·7	+17	+14	51 53	4 09	$u=-64°·5$, $r=0'·585$
Aberysthwith	52 24	4 05	Sept. 21, 1837	S	XVII.	70 23·5	+1·7	70 25·2	−39	−30	51 45	3 35	
Ilfracombe	51 12	4 06	Nov. 3, 1837	R	XV.	69 36·9	+2·0	69 38·9	+33	+22	51 45	4 28	$u=-67°·0$, $r=0'·594$
Pembroke	51 39	4 54	Oct. 26, 1837	R	XV.	69 55·9	+2·0	69 57·9	+ 3	+ 2	51 42	4 56	$u=-67°·5$, $r=0'·608$
Trescow	49 57	6 18	Aug. 30, 1838	F	XI.	69 27·0	+4·0	69 31·0	+44	+28	50 41	6 46	

OBSERVATIONS. Dips between 69° 30′ and 68° 30′. Station.	Lat.	Long.	Date.	Observer.	Table.	Dip.	DEDUCTIONS. Reduction to Epoch.	Dip at the Mean Epoch, Jan. 1, 1837.	Corresponding points in the Isoclinal Line of 69°. Δ Lat.	Δ Long.	The line of 69° in. Lat.	The line of 69° in. Long.	Values employed of u and r.
Margate	51 23	−1 23	Nov. 9 & 11, 1837	S	XVII.	69 02·9	+2·1	69 05·0	− 8	− 5	51 15	−1 28	$u=-67°·5$, $r=0·575$
Margate	51 23	−1 23	April 17, 1838	R	XV.	68 57·2	+3·1	69 00·3	0	0	51 23	−1 23	
Dover	51 08	−1 19	Nov. 2–7, 1837	S	XVII.	68 52·3	+2·0	68 54·3	+ 9	+ 6	51 17	−1 13	
Harwich	51 56	−1 13	May 28 & 29, 1838	R	XV.	69 15·4	+3·4	69 18·8	−30	−20	51 26	−1 33	
Eastbourne	50 47	−0 16	June 20, 1838	F	XI.	68 45·0	+3·5	68 48·5	+18	+11	51 05	−0 05	$u=-69°·25$, $r=0·586$
London	51 31	0 07	July 27, 1837	S	XVIII.	69 18·6	+1·2	69 19·8	−31	−20	51 00	−0 13	$u=-67°·75$, $r=0·581$
London	51 32	0 07	Nov. 15 & 16, 1837	S	XVII.	69 23·8	+2·1	69 25·9	−41	−27	50 51	−0 20	
London	51 32	0 07	May & June, 1838	F	XI.	69 18·0	+3·4	69 21·4	−34	−22	50 58	−0 15	
Brighton	50 50	0 08	Sept. 27, 1836	L	XII.	68 49·7	−0·6	68 49·1	+17	+10	51 07	0 18	$u=-69°·25$, $r=0·586$
Tooting	51 26	0 10	June 14, 1838	F	XI.	69 14·5	+3·5	69 18·0	−29	−19	50 57	−0 09	

London	51 32	0 11	Ap. & Oct.	1836	L	XII.	69 22·7	−1·0	69 21·7	−35	−23	50 57	−0 12	$u=-67°·75$ $r=0·581$
London	51 32	0 11	May 30,	1837	P	XIII.	69 20·2	+1·0	69 21·2	−34	−22	50 58	−0 11	
London	51 32	0 11	August 10,	1837	R	X.	69 20·2	+1·5	69 21·7	−35	−23	50 57	−0 12	
London	51 32	0 11	March 28,	1838	P	XIII.	69 18·2	+3·1	69 21·3	−34	−22	50 58	−0 11	
London	51 32	0 11	June & July,	1838	R	X.	69 14·3	+3·6	69 17·9	−29	−19	51 03	−0 08	
London	51 32	0 11	Dec. 4–10,	1838	R	X.	69 14·6	+4·7	69 18·3	−30	−20	51 02	−0 09	
Worcester Park	51 23	0 17	October 8,	1838	S	XVII.	69 06·8	+4·3	69 11·1	−18	−12	51 05	0 05	
Kew	51 29	0 18	October 13,	1838	S	XVII.	69 16·5	+4·3	69 20·8	−33	−21	50 56	−0 03	
Eastwick Park	51 17	0 19	June 16,	1838	F	XI.	69 08·0	+3·5	69 11·5	−19	−12	50 58	0 07	
Bushey	51 38	0 22	July & Aug.,	1837	R	XV.	69 24·5	+1·4	69 25·9	−42	−28	50 56	−0 06	
Guilford	51 14	0 34	Dec. 12 & 13,	1837	R	XV.	69 05·1	+2·3	69 07·4	−12	−7	51 02	0 27	
Tortington	50 50	0 34	August 15,	1837	R	XV.	68 55·9	+1·5	68 57·4	+4	+3	50 54	0 37	$u=-68°·75$ $r=0·588$
Tortington	50 50	0 34	Aug. & Oct.,	1837	S	XVII.	68 57·2	+1·6	68 58·8	+2	+1	50 52	0 35	
Southsea..................	50 48	0 58	December 8,	1837	R	XV.	69 00·4	+2·3	69 02·7	−4	−2	50 44	0 56	
St. Clairs.............. ...	50 44	1 08	July 19–22,	1837	P	XIII.	69 01·2	+1·3	69 02·5	−4	−2	50 40	1 06	
Ryde	50 44	1 10	Aug. & Sept.,	1836	L	XII.	69 01·3	−0·8	69 00·5	−1	0	50 43	1 10	
Marlborough	51 25	1 43	October 17,	1837	R	XV.	69 25·4	+1·9	69 27·3	−43	−29	50 42	1 14	$u=-67°·5$ $r=0·586$
Salisbury..................	51 04	1 48	August 13,	1836	L	XII.	69 23·1	−0·5	69 22·6	−36	−23	50 28	1 25	$u=-68°·0$ $r=0·589$
Salisbury..................	51 04	1 48	December 5,	1837	R	XV.	69 14·5	+2·2	69 16·7	−26	−17	50 38	1 31	
Weymouth	50 37	2 27	Dec 2 & 4,	1837	R	XV.	69 06·7	+2·2	69 08·9	−14	−9	50 23	2 18	$u=-68°·25$ $r=0'·596$
Exeter.....................	50 43	3 31	Nov. 30,	1837	R	XV.	69 17·3	+2·2	69 19·5	−30	−19	50 13	3 12	
Plymouth	50 23	4 07	Nov. 28 & 29,	1837	R	XV.	69 06·2	+2·2	69 08·4	−13	−9	50 10	3 58	
Lew Trenchard	50 40	4 10	July 19 & 21,	1838	S	XVII.	69 19·0	+3·7	69 22·7	−35	−22	50 05	3 48	$u=-68°·0$ $r=0'·600$
Padstow	50 33	4 56	Nov. 14 & 15,	1837	R	XV.	69 25·1	+2·1	69 27·2	−42	−27	49 51	4 29	
Falmouth	50 09	5 06	Nov. 18,	1837	R	XV.	69 16·1	+2·1	69 18·2	−28	−17	49 41	4 49	$u=-68°·75$ $r=0'·604$
Falmouth	50 09	5 06	July 25,	1838	S	XVII.	69 11·9	+3·8	69 15·7	−25	−15	49 44	4 51	
Falmouth	50 09	5 06	July 31,	1838	F	XI.	69 13·5	+3·8	69 17·3	−27	−16	49 42	4 50	
Land's-end	50 05	5 40	Nov. 21 & 22,	1837	R	XV.	69 18·5	+2·1	69 20·6	−31	−21	49 34	5 19	$=-67°·5$ $r=0'·608$
St. Mary's	49 55	6 17	Aug. 31,	1838	F	XI.	69 26·0	+4·0	69 30·0	−47	−30	49 08	5 47	

DIVISION II.—INTENSITY.

The observations of the Intensity are arranged in three sections, in the same manner as those of the Dip.

Section I.—England.

§ 1. *Statical Method.*

Mr. Lloyd's Observations. These were made with the needles L. 3, L. 4, (page 82), in a 4½ inch circle, made by Robinson. Table XLII contains the detailed statement of the observations.

Table XLII.

[θ is the angle which the needle makes with the horizon, the southern arm being loaded with a weight. The negation sign indicates that the north pole of the needle is *above* the horizontal line.

Station.	Date.	Needle L. 3. Hour.	Ther.	θ	Needle L. 4. Hour.	Ther.	θ
	1836.	h m	°	° ′	h m	°	° ′
Dublin ...	April 11	0 18 P.M.	57·5	– 15 23·4	0 43 P.M.	57·8	– 13 26·4
	— 15	0 30	53·	– 15 3·6	0 08	53·5	– 13 21·0
London ...	— 19	1 0	55·8	– 18 43·5	1 28	56·8	– 16 31·9
	— 21	2 58	58·5	– 18 47·6	2 37	58·5	– 16 59·9
	— 22	0 30	59·2	– 19 6·0	0 14	60·5	– 16 57·6
Shrewsbury	— 25	3 10	55·2	– 17 31·1	2 45	55·0	– 14 56·8
Holyhead .	— 27	1 20	53·	– 16 19·4	0 40	54·0	– 13 55·6
Dublin ...	May 7	1 32	57·2	– 15 52·5	1 10	56·5	– 13 22·5
	— 9	1 25	60·	– 15 52·5	0 50	60·5	– 13 18·4
Dublin ...	Aug. 5	3 50	61·8	– 15 53·8	3 28	61·8	– 13 43·6
	— 6	2 35	67·8	– 16 9·2	2 10	66·5	– 13 34·4
Birkenhead	— 8	10 0 A.M.	68·9	– 18 14·9	9 0 A.M.	68·8	– 15 07·2
		10 50	66·8	– 18 07·5	10 20	67·5	– 14 58·4
Shrewsbury	— 9	11 40	67·2	– 19 4·8	11 15	65·5	– 15 56·8
		0 07 P.M.	66·5	– 19 2·5	0 20 P.M.	66·4	– 16 20·8
Hereford...	— 10	11 20 A.M.	64·5	– 19 13·5	10 50 A.M.	64·5	– 16 24·5
		0 05 P.M.	66·4	– 19 11·2	11 45	66·2	– 16 14·9
Chepstow ..	— 12	0 10	63·2	– 19 14·0	11 40	61·8	– 16 47·2
Salisbury...	— 13	11 10 A.M.	71·2	– 19 58·8	10 45	69·5	– 17 36·0
Ryde	— 15	Noon.	71·5	– 20 33·2	11 30	72·5	– 18 24·2
	— 16	0 45 P.M.	72·2	– 20 36·1	0 20 P.M.	70·0	– 18 20·8
Clifton......	— 29	11 40 A.M.	62·5	– 19 27·3	11 15 A.M.	62·5	– 16 44·2
		0 30 P.M.	63·5	– 19 21·9	0 5 P.M.	63·0	– 17 09·6
Ryde	Sept. 24	0 15	66·4	– 20 22·6	11 45 A.M.	65·8	– 22 53·5
		1 10	64·6	– 20 16·6	0 40 P.M.	65·0	– 22 45·8
Brighton...	— 27	11 15 A.M.	61·5	– 20 41·4	11 40 A.M.	61·5	– 23 25·8
		Noon.	61·0	– 20 21·9	0 30 P.M.	61·2	– 23 11·8
London ...	Oct. 4	0 45 P.M.	56·0	– 19 45·0	1 20	57·0	– 22 54·3
		1 40	57·0	– 19 42·4	2 0	56·4	– 22 32·8
Cambridge	— 8	0 20	59·5	– 19 49·0	0 40	58·5	– 22 34·8
		1 10	56·2	– 19 39·0	1 35	55·8	– 22 29·1
Lynn	— 10	0 55	57·8	– 19 16·5	1 25	57·5	– 21 48·6

Tabular view of the variations of the angle θ, for the purpose of ascertaining the loss of force undergone by the needles, and the period of the change. The angles are reduced to the standard temperature, 60°*.

TABLE XLIII.

Station.	Date.	Needle L. 3.	Needle L. 4.
		° ′	° ′
Dublin	April 11 &c.	−15 21·2	−13 30·7
London	— 19 &c.	−18 55·9	−16 52·0
Shrewsbury......	— 25	−17 38·8	−15 4·8
Holyhead	— 27	−16 30·6	−14 5·2
Dublin	May 7 &c.	−15 54·7	−13 22·9
Dublin	August 5 &c.	−15 53·8	−13 32·3
Birkenhead......	— 8	−17 58·1	−14 49·7
Shrewsbury......	— 9	−18 52·7	−15 59·2
Hereford.........	— 10	−19 3·6	−16 11·1
Chepstow	— 12	−19 8·9	−16 44·3
Salisbury.........	— 13	−19 40·9	−17 20·8
Ryde	— 15 &c.	−20 15·7	−18 4·6
Clifton............	— 29	−19 19·8	−16 52·4
Ryde	Sept. 24	−20 10·8	−22 41·0
Brighton	— 27	−20 29·7	−23 16·6
London	Oct. 4	−19 49·3	−22 48·8
Cambridge	— 8	−19 50·1	−22 36·5
Lynn	— 10	−19 19·9	−21 52·1

Note by Mr. Lloyd.—It appears from this table that Needle L. 3 sustained a loss of force in the interval of time which elapsed between the two observations at Shrewsbury. Now the observations at Dublin in April and May prove that the loss sustained by the needle during the series of observations in spring was comparatively trifling; while, from the results obtained at the same place in May and August, it appears that the magnetism of the needle remained perfectly steady in the interval between the two series. We are consequently conducted to the conclusion, that the change occurred in the short interval between the observations at Dublin on the 5th of August and those at Shrewsbury on the 9th; and we have every reason to believe that it was *previous* to the observation at Birkenhead, and probably due to some accident in the passage across the channel. The magnetism of the needle appears to have been steady during the remainder of the autumn series. This, we think, will appear from the difference of the angles at Shrews-

* For the mode of effecting this reduction see Fifth Report British Association, page 147.

bury and London (near the commencement and end of the series, respectively), as compared with the difference observed at the same places in spring.

With respect to Needle L. 4, the observations at Dublin in April and May show that its magnetism was perfectly steady during the spring series. This needle, however, sustained a very great loss of force between the two sets of observations with it at Ryde; and this loss appears to have been, in a great measure, a sudden one. But that the magnetism of the needle was not stationary during the remainder of the autumn series, will appear at once from a comparison of the observations at Shrewsbury in April and August. As we have no satisfactory means of determining the amount of this loss, and of interpolating a correction, we are forced to reject all the results obtained with this needle in autumn.

Table XLIV.

Computed Intensity.

Station.	Date.	Needle.	Dip.	London = 1·0000.
	1836.		° ′	
London	April 19	L. 3	69 26·4	1·0000
	— 21, 22	L. 4		1·0000
Shrewsbury	— 25	L. 3	70 27·6	1·0076
	— —	L. 4		1·0099
Holyhead	— 27	L. 3	71 08·5	1·0140
	— —	L. 4		1·0149
Birkenhead	August 8	L. 3	70 49·1	1·0112
Shrewsbury	— 9	L. 3	70 27·7	1·0056
Hereford	— 10	L. 3	70 07·1	1·0046
Chepstow	— 12	L. 3	69 47·9	1·0041
Salisbury	— 13	L. 3	69 23·1	1·0006
Ryde	— 15, 16	L. 3	69 02·6	0·9969
Clifton	— 29	L. 3	69 42·6	1·0030
Ryde	Sept. 24	L. 3	69 00·1	0·9975
Brighton	— 27	L. 3	68 49·7	0·9955
London	October 4	L. 3	69 19·0	1·0000
Cambridge	— 8	L. 3	69 41·5	1·0001
Lynn	— 10	L. 3	69 53·2	1·0030

Means.

Shrewsbury .... 1·0077
Holyhead .... 1·0144
Ryde .... 0·9972

We have here eighteen results at the twelve stations, which being combined by the method of least squares, give the following values: $x = + \cdot000047$; $y = - \cdot000067$; $u = - 54° 49'$; $r = \cdot000082$; the mean geographical position being lat. 52° 0′, and long. 1° 50′ W., at which the probable value of the intensity is 1·0048.

Major Sabine's Observations.—The needle S 2 with which these observations were made, has been already described in the Reports of the British Association, vol. v. pages 141—149. It is $11\frac{1}{2}$ inches long, on Professor Lloyd's statical principle, and is used in a circle made by Nairne and Blunt. The observations, together with the deduced values of the intensity, are contained in the subjoined table, No. XLV. Each observation is a mean of forty readings, taken in four positions of the needle. The thermometer by which the temperature was registered was always enclosed with the needle in the dip circle. The values of $\frac{\cos\theta}{\sin(\delta-\theta)}$ are reduced to a standard temperature of 60°, in the manner described by Mr. Lloyd in the Transactions of the Royal Irish Academy for 1836; the coefficient of $\tau - \tau'$ in the reduction, or the value of M a experimentally determined, is ·000024. (See 6th Report, British Association, pp. 11, 12.)

The observations with this needle at Tortington, in Sussex, in the summer of 1837, repeated in the autumn of 1837 and summer of 1838, and lastly in the autumn of 1838, produced on each occasion an almost identical result, and afford most satisfactory evidence of the unaltered state of its magnetism during the whole of the present series: the values of $\frac{\cos\theta}{\sin(\delta-\theta)}$ resulting from the observations at Tortington at the three epochs alluded to are as follows:

	Mean.
May to September, 1837 . . .	0·95390
October 1837 to July 1838 . .	0·95361
October 1838	0·95375

To obtain the value of $\frac{\cos\theta}{\sin(\delta-\theta)}$ in London, to serve as the unity of the series, observations were made on three several occasions, and in three different localities; namely, in the gardens of the Little Cloisters, Westminster; in the nursery garden in the Regent's Park; and in the gardens of the palace at Kew. The results were as follows:

Little Cloisters . . .	0·95245
Regent's Park	0·95684
Kew Gardens	0·95479
Mean . . .	0·95469

The mean of these values, 0·95469, has therefore been taken as the equivalent to unity, and the relative values of the intensity at the other stations have been computed thereby, and are inserted in the final column of the table.

Table XLV.

Station.	Date.	Hour.	Therm.	θ	δ	$\frac{\cos\theta}{\sin(\delta-\theta)}$	Intensity. London =1·0000.
	1837.						
Little Cloisters, Westminster .	June 1	......	58°	−17° 52′·1	69° 18′·5	·95245	0·9977
	— 1	......	58	−17 56·6			
	July 25	9½ A.M.	70	−18 07·4			
	— 25	10½ A.M.	73	−18 00·3			
Tortington	May 17	......	55	−17 38	68 59·6	·95390	0·9992
	— 29	......	56	−17 41·4			
	June 5	11½ A.M.	65	−17 55·6			
	— 5	½ P.M.	65	−17 50·2			
	July 20	4½ P.M.	69	−17 29·5			
	— 20	5½ P.M.	69	−17 37·1			
	Aug. 5	2 P.M.	70	−17 51·2			
	— 5	2¾ P.M.	70	−17 49·7			
	— 31	Noon.	60	−18 03·1			
	— 31	1 P.M.	60	−18 04·6			
	Sept. 1	1½ P.M.	57	−17 42·1			
	— 1	2½ P.M.	57	−17 38·5			
Shrewsbury......	— 19	3½ P.M.	68·5	−16 34·8	70 24·9	·96009	1·0057
	— 19	4½ P.M.	68·5	−16 37·0			
Aberystwith ...	— 21	10 A.M.	66·5	−16 01·8	70 25·9	·96430	1·0100
	— 21	10½ A.M.	66·5	−16 01·8			
	— 21	3½ P.M.	66	−15 41·5			
	— 21	4½ P.M.	66	−15 40·8			
Brecon............	— 22	5¾ A.M.	54	−16 26·3	70 03·2	·96041	1·0060
	— 22	6¼ A.M.	54	−16 30·3			
Merthyr	— 22	1½ P.M.	62	−16 09·8	70 04·0	·96346	1·0081
	— 22	2½ P.M.	62	−16 13·0			
Dunraven Castle	— 25	4½ P.M.	59	−16 30·5	69 45·7	·96215	1·0078
	— 25	5¼ P.M.	59	−16 30·3			
	Oct. 2	5 P.M.	62	−16 13·9			
	— 2	6 P.M.	62	−16 22·0			
	— 3	11 A.M.	65	−16 26·5			
	— 3	Noon.	65	−16 25·9			
	— 5	11½ A.M.	65	−16 27·6			
	— 5	Noon.	65	−16 26·9			
	— 5	5 P.M.	60	−16 31·1			
	— 5	5½ P.M.	60	−16 31·7			
	— 6	11½ A.M.	62	−16 40·6			
	— 6	Noon.	62	−16 39·0			
Dover	Nov. 2	½ P.M.	48	−18 24·7	68 52·3	·94948	0·9945
	— 2	1½ P.M.	52	−18 29·2			
	— 3	2½ P.M.	50	−18 18·8			
	— 3	3 P.M.	50	−18 25·0			
	— 6	½ P.M.	50	−18 21·7			
	— 6	1½ P.M.	50	−18 21·4			

TABLE XLV.—(*continued*).

Station.	Date.	Hour.	Therm.	θ	δ	cos θ / sin (δ − θ)	Intensity. London =1·0000.
	1837.						
Margate	Nov. 9	11½ A.M.	50°	−17° 58′	69° 02′·9	·95180	0·9970
	— 9	½ P.M.	50	−17 56·6			
	— 10	11 A.M.	48	−18 01·6			
	— 10	Noon.	48	−18 01·9			
London (Regent's Park)	— 14	Noon.	50	−17 12·8	69 23·8	·95684	1·0022
	— 14	1 P.M.	50	−17 14·7			
	— 16	3 P.M.	37	−16 53·7			
	— 16	4 P.M.	37	−16 52·6			
	— 16	4½ P.M.	37	−17 00·6			
Tortington	Oct. 15	2½ P.M.	56·5	−17 20·8	68 54·0	·95361	0·9989
	— 15	3 P.M.	56·5	−17 24·2			
	— 19	11 A.M.	58	−17 47·3			
	— 19	12	58	−17 58·4			
	— 19	5 P.M.	54·5	−17 51·3			
	Nov. 24	11¾ A.M.	60	−17 27·5			
	— 24	¾ P.M.	60	−17 25·2			
	1838.						
	June 18	3½ P.M.	63	−17 46·2			
	— 18	4¼ P.M.	63	−17 45·7			
	— 19	8½ A.M.	61	−17 48·9			
	— 19	9 A.M.	61	−17 48·8			
	— 19	1½ P.M.	66	−17 39·7			
	— 19	2 P.M.	66	−17 37·5			
	— 23	4 P.M.	64	−17 53·9			
	— 23	5 P.M.	64	−17 49·6			
	July 9	3 P.M.	71	−18 12·9			
	— 9	3½ P.M.	71	−18 11·8			
Lew Trenchard	— 19	7½ A.M.	64	−17 02·5	69 19·0	·95901	1·0045
	— 19	9 A.M.	64	−16 59·7			
	— 19	Noon.	72	−16 56·8			
	— 19	2½ P.M.	72	−16 52·2			
	— 20	7½ A.M.	58·5	−16 42·8			
	— 21	8 A.M.	58·5	−16 57·1			
	— 21	½ P.M.	65	−16 57·6			
Falmouth	— 24	4 P.M.	58·5	−17 36·6	69 11·9	·95607	1·0015
	— 25	7¾ A.M.	59	−17 07·5			
	— 25	8 A.M.	59	−17 06·6			
	— 25	1 P.M.	63	−17 30·8			
	— 26	4 P.M.	65	−17 18·8			
Dublin	— 31	1¾ P.M.	65	−14 29·1	70 54·6	·97200	1·0182
	— 31	2¼ P.M.	65	−14 29·7			
	Aug. 2	2½ P.M.	66	−14 19·8			
	— 3	3 P.M.	67	−14 29·5			
	— 3	4 P.M.	67	−14 26·0			
Whitehaven	— 16	10½ A.M.	56	−14 24·8	71 10·9	·97144	1·0176
	— 16	11½ A.M.	56	−14 19·5			
Newcastle	— 28	4½ P.M.	69	−15 00·8	71 09	·96870	1·0147
	— 29	7½ A.M.	53	−14 48			
Alnwick Castle.	— 31	4½ P.M.	62·5	−14 39·9	71 22·6	·96987	1·0159
Stonehouse	Sept. 2	1½ P.M.	59	−14 24·9	71 19·6	·97150	1·0176
	— 2	2½ P.M.	59	−14 22·9			
	— 4	10½ A.M.	60	−14 19·1			

TABLE XLV.—(*continued*).

Station.	Date.	Hour.	Therm.	θ	δ	$\frac{\cos\theta}{\sin(\delta-\theta)}$	Intensity. London =1·0000.
	1838.						
Helensburgh ...	Sept. 8	8 A.M.	53·5	−12° 54′·1	72° 17′·0	·97870	1·0252
	— 9	$7\frac{1}{2}$ A.M.	48	−12 33·6			
	— 9	8 A.M.	48	−12 35·2			
	— 9	5 P.M.	53	−12 43·4			
	— 9	6 P.M.	53	−12 40·8			
Jordan Hill......	— 11	$3\frac{1}{4}$ P.M.	60	−12 54·0	72 14·3	·97722	1·0236
	— 11	$3\frac{3}{4}$ P.M.	60	−12 53·0			
	— 13	$11\frac{1}{2}$ A.M.	61	−13 19·3			
	— 13	Noon.	61	−13 18·4			
Worcester Park.	Oct. 8	11 A.M.	55	−17 33·6	69 06·7	·95524	1·0006
	— 8	$\frac{1}{2}$ P.M.	55	−17 36·1			
	— 9	$11\frac{1}{2}$ A.M.	57	−17 19·8			
	— 9	$\frac{1}{2}$ P.M.	57	−17 19·5			
	— 10	$11\frac{1}{2}$ A.M.	54·5	−17 33·2			
	— 10	$\frac{1}{2}$ P.M.	54·5	−17 31·5			
London (Kew Gardens)......	— 12	$2\frac{1}{2}$ P.M.	48	−17 32·1	69 16·4	·95479	1·0001
	— 12	$3\frac{1}{2}$ P.M.	48	−17 33·9			
	— 13	$10\frac{1}{2}$ A.M.	46·5	−17 18·3			
	— 13	$11\frac{1}{2}$ A.M.	46·5	−17 26·8			
Tortington	— 17	$11\frac{1}{2}$ A.M.	61	−17 45·9	68 52·4	·95375	0·9990
	— 17	$\frac{1}{2}$ P.M.	61	−17 45·8			
	— 18	$2\frac{1}{2}$ P.M.	54·6	−17 49·5			
	— 18	3 P.M.	54·6	−17 45·8			

Omitting Dublin, which has been transferred to the Irish section, and taking a mean of the three results at Tortington for the intensity at that station, we have here twenty stations in Britain to be combined by the method of least squares: whence $x = +\cdot000048$; $y = -\cdot000062$; $u = -52° 27'$; $r = \cdot000078$; and $f = 1\cdot0075$, the probable value of the intensity at the mean geographical position, of which the latitude is 52° 36′, and the longitude 2° 11′.

Professor Phillips's observations.—These were made with a needle on Mr. Lloyd's statical principle, employed in Mr. Phillips's six-inch circle. The needle had been recently received from the maker (Robinson), when it was first used at York in June 1837; and the results obtained with it on the 3rd and 5th June, compared with those on the 15th June, indicated that its magnetism had not become steady. To obviate this inconvenience as far as might be possible, Mr. Phillips repeatedly, during the series of his determinations, brought the needle back to York, and re-examined its magnetic state. We are thus furnished with observations at that station in June, August, September, October, 1837, and in February, 1838, which are arranged in Table XLVI., and show the proportion of magnetic force lost by the needle in the several intervals. It will be seen that the loss, on the daily average, progressively diminished; and, excepting in the first interval, namely, between the 4th and 15th June, was not of sufficient amount to create much uncertainty in the results, after the application of a correction assigned in the usual manner, viz. a daily rate for each interval, obtained by dividing the whole loss in an interval by the number of days which it contains. In regard to the first interval, when the loss was considerable, and where a correction applied on the above principle can scarcely be supposed an exact representation of the facts, it fortunately happens that the six included stations are all in Yorkshire; and thus, though an equable correction in this interval may make the values of the intensity at these stations appear more discrepant with each other than they otherwise would do, yet their *collective bearing* on the position and direction of the isodynamic lines is scarcely affected.

By experiments with this needle in different temperatures, Mr. Phillips found ·000090 the coefficient (α) of ($\tau - \tau'$) in the reduction for temperature; which has been employed in reducing the values in the column $\frac{\cos\theta}{\sin\delta-\theta}$ to a mean temperature of 60°.

Table XLVI.

Observations at York, collected in one view, to show the loss of magnetism sustained by Mr. Phillips's needle. $\delta = 70° 48'·8$.

Date.	Therm.	θ	$\frac{\cos\theta}{\sin(\delta-\theta)}$	Interval, Days.	Loss.	Average daily loss.
June 3 & 5, 1837 ...	62°·2	− 15° 24′·9	0·96632			
				11	·00378	00034
June 15	68·2	− 16 10·0	0·96254			
				46	·00357	00008
Aug. 1	67·5	− 16 46·2	0·95897			
				38	·00150	00004
Sept. 7	65·0	− 17 00·0	0·95747			
				25	·00082	00003
Oct. 2	63·5	− 17 06·9	0·95665			
				140	·00135	00001
Feb. 19 & 20, 1838 ...	35·5	− 16 55·0	0·95530			

Mr. Phillips's observations at twenty-four stations in England are comprised in Table XLVII.; the values of $\frac{\cos\theta}{\sin\delta-\theta}$ are reduced to a mean temperature of 60°: the two last columns contain the relative values of the intensity, in the first column to York, and in the second to London. The frequent repetition of the observations at York, at different dates, renders that station the proper base of Mr. Phillips's series. The observations at York and London in February and March 1838, furnish a direct comparison of the force at those stations, and by means of that comparison, a determination of its value at all the other stations relatively to the London unity.

Table XLVII.

Station.	Date.	Hour.	Therm.	θ	δ	$\frac{\cos\theta}{\sin(\delta-\theta)}$	Intensity. York =1·0000.	Intensity. London =1·0000.
	1837.		°	° ′	° ′			
Doncaster ...	June 3	7 A.M.	58·0	−15 50·1	70 30·2	·96383	0·9971	1·0096
York	June 3	2½ P.M.	56·7	−15 17·3	70 48·8	·96632	1·0000	1·0126
York	June 5	9½ A.M.	60	−15 32·3				
York	June 5	12	70	−15 25·2				
York	June 15	4 P.M.	73	−16 18·7				
York	June 15	8 P.M.	63·5	−16 01·3				
Thirsk	June 6	3 P.M.	53	−14 51·3	70 59·2	·96848	1·0029	1·0155
Osmotherley .	June 6	8 P.M.	42·5	−15 08·7	71 03·2	·96583	1·0002	1·0128
Hambletonend	June 7	9½ A.M.	56	−15 19·1	71 04·0	·96606	1·0008	1·0134
Whitby	June 9	7½ A.M.	52	−15 22·0	70 57·9	·96553	1·0009	1·0135
Flamborough .	June 11	8 P.M.	57	−16 29·1	70 36·9	·95988	0·9958	1·0083
Scarborough. .	June 13	1½ P.M.	71	−16 28·3	70 41·8	·96111	0·9978	1·0103
Sheffield	June 17	6½ P.M.	70	−16 18·0	70 29·6	·96220	0·9998	1·0124
Birmingham. .	July 3	3 P.M.	73	−17 04·6	70 07·2	·95897	0·9980	1·0105
Birmingham. .	July 8	6½ P.M.	70	−16 47·1				
St. Clair's.......	July 19	9 A.M.	68	−18 52·7	69 01·2	·94786	0·9878	1·0002
St. Clair's.......	July 22	3¼ P.M.	76	−19 0·1				
St. Clair's.......	July 25	6¼ P.M	66·5	−18 42·1				
York	Aug. 1	4 P.M.	67·5	−16 46·2	70 48·8	·95897	1·0000	1·0126
Calderstone...	Aug. 12	12	69·5	−17 27·7	70 43·5	·95668	0·9981	1·0106
Douglas	Aug. 17	3 P.M.	68·5	−15 27·1	71 22·2	·96610	1·0081	1·0208
Castletown ...	Aug. 18	9 A.M.	66·2	−15 29·8	71 22·5	·96564	1·0077	1·0203
PeelCastleInn	Aug. 18	2 P.M.	70	−15 49·0	71 24·0	·96454	1·0065	1·0192
PeelCastleInn	Aug. 18	3¾ P.M.	69	−15 39·7				
Birkenhead ...	Aug. 26	1½ P.M.	62	−16 33·8	70 39·4	·95980	1·0019	1·0145
York...........	Sept. 7	4¼ P.M.	65	−17 0·0	70 48·8	·95747	1·0000	1·0126
Coed	Sept. 20	12	68	−17 22·8	70 40·9	·95560	0·9985	1·0110
Bowness	Sept. 25	9½ A.M.	54	−15 54·7	71 18·4	·96229	1·0056	1·0182
Coniston	Sept. 27	8½ A.M.	51·5	−15 39·4	71 19·5	·96346	1·0070	1·0196
Patterdale.	Sept. 27	1¾ P.M.	52	−15 55·5	71 19·6	·96202	1·0054	1·0181
Penrith.	Sept. 28	10½ A.M.	50	−15 51·0	71 23·4	·96222	1·0057	1·0184
Carlisle.	Sept. 29	10¾ A.M.	56·5	−15 42·1	71 28·5	·96357	1·0072	1·0198
Newcastle......	Sept. 30	7¼ A.M.	53	−16 06·9	71 18·1	·96120	1·0047	1·0173
York	Oct. 2	10 A.M.	63	−17 10·4	70 48·8	·95665	1·0000	1·0126
York	Oct. 2	4 P.M.	64	−17 3·5				
	1838.							
London	Mar. 28	4½ P.M.	58	−19 22·2	69 19·6	·94346	0·9876	1·0000
York.	Feb. 19	9 A.M.	33	−16 54·8	70 48·8	·95530	1·0000	1·0126
York.	Feb. 20	4½ P.M.	38	−16 55·2				

If we combine the mean results at the twenty-four stations in this table by the method of least squares, we obtain the following values: $x = +\cdot000061$; $y = -\cdot000066$; $u = -47° \ 37'$; $r = \cdot000090$; and $f = 1\cdot0136$, at the mean geographical position in lat. 53° 49′, and long. 2° 08′.

Mr. Fox's observations.—These were made with a $4\frac{1}{2}$ inch needle, on the principle described by its maker, Mr. T. B. Jordan, of Falmouth, in the third volume of the "Annals of Electricity," &c. The needle has a small grooved wheel on its axle, which receives a thread of unspun silk, furnished with hooks, to which weights may be attached. The weights employed were successively 2·0 grains, 2·1 grains, 2·2 grains; and with each weight the intensities are in the inverse ratio of the angle of deflection produced, corrections being applied for differences of temperature at the different stations. The following table exhibits the angles of deflection occasioned by the respective weights, and the values of the intensity deduced therefrom. The angles are reduced to a common temperature; 1° of the centigrade scale having been found by experiment to be equivalent to 2′, or 2′·4 in the angle.

TABLE XLVIII.

Station.	Date.	Weight.	Angle of Deflection.	Intensity.	Mean.	Place of Observation.
	1838.	Grains.	° ′			
London	May 22; June 4 & 8	2·0 2·1 2·2	48 36·7 51 55·3 55 33·0	1·0000 1·0000 1·0000	1.0000	Mean of results in a field N. of Maiden Lane; in the Regent's Park; and at Westbourne Green.
Eastbourne	June 20	2·0 2·1 2·2	48 57 52 19 55 57	0·9938 0·9921 0·9952	0·9937	In the grounds of Davies Gilbert, Esq.
Eastwick Park	June 16	2·0 2·1 2·2	48 35 51 57 55 40	0·9997 0·9996 0·9986	0·9993	In the grounds.
Combe House	July 2	2·0 2·1 2·2	48 25 51 45 55 18	1·0023 1·0024 1·0031	1·0026	In the grounds.
Falmouth	July 5 & 7	2·0 2·1 2·2	48 29 51 48 55 20	1·0013 1·0017 1·0026	1·0018	In Mr. Fox's grounds.

§ 2. *By the Method of Vibrations.*

The observations by this method include twenty-seven stations; i. e. 18 by Captain Ross; 7 by Major Sabine; and 2 by Mr. Lloyd.

1st. Captain Ross's determinations were made with a cylinder (X) vibrated in an apparatus on the well-known plan of M. Hansteen. The loss of magnetism sustained by the cylinder during the time of its employment, from July 1837 to June 1838, was very considerable, and was occasionally so irregular as to prevent any satisfactory conclusion whatsoever being drawn from the observations. On a careful examination, there appeared two intervals, viz. from the middle of September to the middle of November 1837,—and from April 24 to June 5, 1838,—during which there was reason to infer that the loss of magnetism, though considerable, had been tolerably uniform and regular. During the second interval, viz. from April 24 to June 5, 1838, on both which days the cylinder was vibrated in London, the increase in the time of vibration at the *same* station affords a *direct* measure of the diminution in its magnetic intensity; and being divided by the number of days comprised in the interval, furnishes the amount of the daily correction. But in the first interval we have the additional disadvantages of having no *direct* observation showing the amount of the loss of magnetism, and no *direct* comparison with the force in London: and it is necessary, consequently, to have recourse to indirect means for the purpose of determining these particulars. On the 19th of September, 1837, Captain Ross vibrated cylinder X at Birkenhead; and on the 21st of September, at Douglas, in the Isle of Man. In Table XLVII. we have the value of the intensity at both these stations relatively to the London unity, determined by Mr. Phillips; and in Table XLIV. we have Mr. Lloyd's determination of the force at Birkenhead. We may employ these determinations to supply the time of vibration in London corresponding to the observations with the cylinder at Douglas and Birkenhead. In like manner we may accomplish a second indirect comparison with London by means of Captain Ross's observations at Falmouth on the 18th of November, 1837, combined with the values of the intensity at that station determined by Mr. Fox, (Table XLVIII.), and Major Sabine, (Table XLV.). The several observations and processes by which the times of vibration of the cylinder in London have been derived at different epochs, are comprised in Table XLIX.; and in its final column is shown the average daily loss of magnetism experienced in each of the two intervals; which is subsequently applied in Table L., in assigning the corresponding times of vibration in London, on days when the cylinder was employed elsewhere.

Table XLIX.

Station.	Date.	Time of vibration at 60°.	Observed dip.	Intensity of London = 1·0000	Corresponding times of vibration of Cylinder X in London.	Daily loss of force in the respective intervals.
	1837.		° ′			
Birkenhead...	Sept. 19	275·22	70 35·0	{ 1·0145 Phillips 1·0112 Lloyd }	268·45 } 268·37	s 0·06
Douglas	Sept. 22	279·27	71 20·3	1·0208 Phillips	268·30 }	
Falmouth......	Nov. 18	271·48	69 16·1	{ 1·0015 Sabine 1·0018 Fox }	............271·70	
	1838.					
London........	April 24	275·84	69 15·0	1·0000...............	............275·84	0·015
London........	June 2&5	280·06	69 15·0	1·0000...............	............280·06	

Table L. contains the observations made by Captain Ross with cylinder X, and the values of the intensity derived from them. The coefficient in the formula for the reduction to a mean temperature, is ·00017: the reduction has been applied in the column entitled "corrected time."

Table L.

Station.	Date.	Hour.	Temp.	Time of 100 vibrations.	Corrected Time.	Observed Dip.	Corresponding time of vibration in London.	Intensity. London = 1·0000.
	1837.	h m	°	s	s	° ′	s	
Birkenhead...	Sept. 19	1 48 P.M.	70	275·81	275·22	70 35·0	268·45	1·0128
		2 17	70	275·58				
Douglas, (Isle of Man).	Sept. 22	9 47 A.M.	60	279·2	279·27	71 20·3	268·30	1·0208
		10 36	60	279·35				
Pwllheli	Oct. 14	5 11 P.M.	47	274·62	275·71	70 32·5	269·81	1·0173
	— 15	8 55 A.M.	47	275·30				
		10 57	60	275·98				
Marlbro'	— 18	10 50	58	270·53	270·68	69 25·4	270·00	1·0018
		11 14	60	270·75				
Clifton.........	— 22	2 30 P.M.	56	271·4	271·66	69 34·0	270·24	1·0031
		2 55	56	271·57				
Pembroke.....	— 26	1 17	56	272·75	272·95	69 55·9	270·48	1·0128
		1 48	56	272·80				

TABLE L. (*continued.*)

Station.	Date.	Hour.	Temp.	Time of 100 vibrations.	Corrected Time.	Observed Dip.	Corresponding time of vibration in London.	Intensity. London. =1·0000.
	1837.	m	°	s	s	° ′	s	
Swansea	Oct. 27	5 1 P.M.	45	273·38				
	— 28	11 46 A.M.	54	273·12	273·66	69 46·7	270·60	1·0010
	— 29	6 59	54	273·23				
Ilfracombe....	Nov. 3	5 2 P.M.	46	271·42	272·06	69 36·9	270·80	1·0071
Padstow	— 14	1 12	56	270·85	271·10	69 25·1	271·49	1·0096
		1 35	56	271·00				
Falmouth......	— 18	3 42	50	271·02	271·48	69 16·1	271·70	1·0017
Land's End...	— 23	11 6 A.M.	56	270·98	271·14	69 18·5	272·00	1·0082
		11 29	56	270·93				
	1838.							
London........	April 24	3 41 P.M.	53	275·52	275·84	69 15·0	275·84	1·0000
			53	275·51				
York	— 28	1 40	48	284·43	285·16	70 45·2	276·24	1·0094
		2 7	50	284·85				
Scarbro'	May 1	10 56 A.M.	47	285·7				
		11 23	48	286·43	286·85	70 43·0	276·54	0·9975
		11 49	49	286·68				
Bridlington...	— 2	3 44 P.M.	60	285·82				
		5 50	56	285·0	285·46	70 38·8	276·66	1·0048
		6 35	54	285·08				
Wadworth	— 10	3 39	60	284·07	284·07	70 27·5	277·50	1·0117
Nottingham. .	— 12	7 46 A.M.	58	283·02	283·12	70 16·3	277·70	1·0104
Louth	— 16	11 40	57	283·7	283·81	70 19·5	278·10	1·0111
		1 7 P.M.	58	283·67				
Cromer	— 21	0 7	60	281·05	281·21	69 46·1	278·65	1·0065
	— 22	0 6	60	281·38				
Lowestoffe....	— 23	11 31 A.M.	61	279·90				
		0 16 P.M.	62	279·73				
	— 24	10 14 A.M.	61	280·30				
		10 58	64	280·68				
		1 51 P.M.	60	280·65	280·66	69 29·2	278·90	0·9990
	— 25	10 28 A.M.	52	281·0				
		0 37 P.M.	53	281·10				
		1 12	54	281·17				
Harwich	— 29	11 46 A.M.	63	260·13				
		1 40 P.M.	66	279·93				
	— 30	10 23 A.M.	65	280·20	280·00	69 15·4	279·50	0·9974
		0 3 P.M.	66	280·43				
		11 14 A.M.	66	280·53				
London........	June 2	11 52	65	280·27				
		0 21 P.M.	65	280·32	280·06	69 15·0	280·06	1·0000
	— 5	10 58 A.M.	65	280·25				
		0 5 P.M.	68	280·53				

2. Mr. Lloyd's observations were made with two cylinders, L (*a*) and L (*b*), vibrated in Hansteen's apparatus. The agreement of their times of vibration in Dublin, in April and May 1836, is an evidence that their magnetic state remained unaltered in the interval. The values of the intensity at Shrewsbury and Holyhead are deduced, in relation to the London unity, by means of the force in Dublin; which, in a subsequent part of this Report, will be shown to be 1·0195. The coefficient in the formula of reduction to a mean temperature, is ·00025 for both cylinders. (5th Report, B. A., pp. 119 and 120.)

Table LI.

Station.	Date.	Cyl.	Temp.	Time of 100 vibrations.	Corrected Time.		Observed Dip.	Intensity. London = 1·0000.	
	1836.		°	s	s		° ′		
Dublin...	April 11	L (*a*)	56·2	243·56	243·76	243·78	71 03·5	1·0195	1·0195
	— 12		61·0	243·96	243·88				
	— 15		56·5	243·50	243·69				
	April 11	L (*b*)	56·5	292·93	293·16	293·33		1·0195	
	— 12		59·2	293·50	293·53				
	— 15		56·8	293·09	293·29				
Shrewsbury...	April 25	L (*a*)	62·0	241·64	241·51	241·53	70 27·6	1·0095	1·0080
			62·0	241·68	241·55				
	— 25	L (*b*)	70·0	291·58		290·83		1·0066	
Holyhead...	April 27	L (*a*)	54·2	244·08	244·42	244·42	71 08·5	1·0192	1·0195
			53·2	244·02	244·42				
	— 27	L (*b*)	59·0	293·87		293·92		1·0198	
Dublin...	May 7	L (*a*)	57·6	243·96	244·10	243·96	71 03·5	1·0195	1·0195
	— 9		61·0	243·90	243·83				
	May 7	L (*b*)	58·0	292·95	293·08	293·21		1·0195	
	— 9		61·0	293·43	293·34				

3. Major Sabine's observations were made with Mr. Lloyd's cylinders L (*a*) and L (*b*), and with a pair, in all respects similar, designated as L (3) and L (4). The results are comprised in the two following Tables, LII. and LIII. Table LII. contains observations made to determine the value of the intensity at Tortington, in Sussex; and Table LIII. the values at six other stations in Great Britain: in Table LIII. the value of the force in Dublin = 1·0195, has supplied the means of checking the magnetism of the cylinders.

Table LII.

Deduction of the Intensity at Tortington.

1. By comparison with Dublin. The observations at Dublin are by Professor Lloyd; those at Tortington by Major Sabine. The intensity at Dublin = 1·0195. The co-efficient in the formula for the reduction to a mean temperature of L (3) = ·00027; of L (4) = ·00022.

Cyl.	Station.	Date.	Hour.	Therm.	Time of 100 Vibrations.	Corrected Time.	Dip.	Intensity. London = 1·0000.
		1838.	h m	°	s.	s.	° ′	
L (3).	Tortington ...	Feb. 9	4 40 P.M.	42	295·67	297·05	68 55·1	
		— 10	1 31	36	295·09			
	Dublin	March 3	1 37	46·2	307·79	308·81	70 58·4	0·9963
		— 3	2 02	47	308·00			
		— 5	3 03	46·5	307·35			
	Tortington ...	March 10	1 44	46	296·74	297·75	68 55·1	
		— 10	2 38	45·5	296·53			
L (4).	Tortington ...	Feb. 9	5 09 P.M.	41	271·22	272·28	68 55·1	
		— 10	0 34	36	270·78			
	Dublin	March 3	2 46	46·8	282·58	283·40	70 58·4	0·9985
		— 3	3 08	44·2	282·58			
		— 5	2 40	47·2	282·57			
	Tortington ...	March 10	10 43 A.M.	49	272·53	272·99	68 55·1	
		— 10		46	271·98			

2. By direct comparison with London. The London observations were made in the Palace Gardens at Kew.

Cyl.	Station.	Date.	Hour.	Therm.	Time of 100 Vibrations.		Dip.	Intensity. London = 1·0000
		1838.	h m					
L (*a*).	London.........	Oct. 13	3 0	39	237·77	238·98	69 16·4	0·9986
		— 13	3 16	40	237·81			
	Tortington ...	Oct. 18	0 08	52	236·65	237·16	69 53·5	
		— 18	0 27	53	236·80			
L (*b*).	London.........	Oct. 13	11 45	44	303·92	305·21	69 16·4	0·9965
		— 13	0 15	44	304·06			
	Tortington ...	Oct. 17	1 28	58·5	303·26	303·20	68 53·5	
		— 18	10 17	48·0	302·18			
		— 18	11 07	50·5	302·46			

The values of the intensity at Tortington, relatively to unity in London thus deduced, are as follows:

L (3), 0·9963; L (*a*), 0·9986;
L (4), 0·9985; L (*b*), 0·9965;
Mean, 0·9975.

Table LIII.

Deduction of the Intensity at Six Stations in Britain.

Station.	Date.		Hour.	Therm.	Time of 100 Vibrations.	Corrected Time.	Observed Dip.	Corresponding Time of Vibration in London.	Intensity. London =1·0000.
L (*b*).									
	1838.		h m	°	s	s	° ′	s	
ondon	June	1	10 21 A.M.	62	284·17	284·01	69 17·4	284·01	1·0000
	—	1	11 04	62	284·17				
almouth......	July	7	1 10 P.M.	67	283·73*	283·40	69 12·0	283·94	0·9997
	—	25	0 18 P.M.	62	283·66				
ublin	Aug.	6	3 42	67	292·88	292·32	70 54·6	283·87	1·0195
	—	8	1 42	63	292·48				
hitehaven...	Aug.	16	4 25	57·5	294·46	294·64	71 10·7	283·94	1·0180
ewcastle ...	Aug.	28	2 28	71·5	295·22	294·38	71 09·0	283·94	1·0183
L (*a*).									
ublin	Aug.	6	4 12 P.M.	66	246·30	245·77	70 54·6	238·60	1·0195
	—	8	3 44	63	245·81				
hitehaven...	Aug.	16	4 55	57	247·81	248·00	71 10·7	238·80	1·0169
ewcastle ...	Aug.	28	2 57	73·5	248·40	247·72	71 09·0	238·80	1·0177
	—	30	1 00	63·0	248·07†				
tonehouse ...	Sept.	3	11 30	59	248·86	248·92	71 19·6	238·80	1·0171
elensburgh..	Sept.	9	2 50	55	253·22	253·58	72 17	238·80	1·0310
	—	9	3 20	57	253·45				
ordan Hill...	Sept.	13	3 24	59	253·66	253·72	72 14	238·80	1·0273
ondon	Oct.	13	3 00	39	237·77	238·98	69 16·4	238·98	1·0000
	—	13	3 16	40	237·81				

The results in Table LIII., collected in one view, are as follows:

Station.	Intensity, London = 1·0000.			Station.	Intensity, London = 1·0000.
	L (*b*).	L (*a*).	Mean.		L (*a*).
Whitehaven.........	1·0180	1·0169	1·0175	Helensburgh........	1·0310
Newcastle	1·0183	1·0177	1·0180	Jordan Hill.........	1·0273
Falmouth........	0·9997	....	0·9997	Stonehouse	1·0171

If we combine, by the method of least squares, the results at the twenty-seven stations at which the intensity was thus de-

* Observed by Mr. Fox. † Observed by Captain Ross.

termined by horizontal vibrations,—namely, eighteen stations by Captain Ross, exclusive of those which have served to examine the magnetism of the cylinder; two stations by Mr. Lloyd; and seven by Major Sabine,—we obtain the following values: $x = + \cdot 000064$; $y = - \cdot 000069$; $u = -47° 14'$; $r = \cdot 000094$. The mean geographical position is 52° 43′ N., and 2° 18′ W.

If we now collect in one view the several values of u and r which have been obtained from the intensity observations in England, we have as follows:

Table LIV.

Observer.	Method.	No. of Stations.	Mean Geographical Position.		Values of	
			Lat.	Long.	u	r
			° ′	° ′	° ′	
Lloyd	Statical	12	52 01	1 50	−54 49	·000082
Phillips	Statical	24	53 49	2 08	−47 37	·000090
Sabine	Statical	20	52 36	2 11	−52 27	·000078
Ross, Sabine, Lloyd	Horizontal vibrations	27	52 43	2 18	−47 14	·000094

If we regarded the several values of u and r in Table LIV., as entitled to weight proportioned to the number of stations of which each is the representative, we should assign a preponderance to the values obtained by the horizontal vibrations, which the circumstances of the observations from which they are derived would scarcely justify. To give them exactly their just weight, would require a lengthened investigation of the respective probable errors, not only of the two methods, but of the horizontal method under some disadvantages, as shown in page 143. The occasion would not justify the expenditure of the necessary time and labour; and I have assigned the arbitrary value of 18 to the horizontal deductions from the twenty-seven stations; making, in this particular instance, three horizontal determinations equivalent to two statical. Thus weighted, we obtain —50° 48′ and ·000086 as the mean values of u and r derived from the English series, corresponding to the central geographical position in 52° 48′ N. lat., and 2° 07 W. long.

Section II.—Scotland.

§ 1. *Observations by the Statical Method.*

Major Sabine's Observations.—These were made in the summer of 1836, with the statical needle S (2); an account of them is contained in the report on the Scotch Magnetical Lines, in the 6th vol. of the Reports of the British Association. Between the 30th of July and the 4th of October, in which interval the magnetism of the needle was shown to have sustained no change, twenty-two stations were observed at, including two in Ireland, viz. Bangor and Dublin. These are now transferred to the Irish Series, and being thus included in their more appropriate place, will be omitted here. At the time of the publication of the Scotch report, no *direct* comparison had been made of the intensity in Scotland with that in London; but its values at the several Scottish stations relatively to London were given provisionally, by means of the observations in Dublin, and by adopting 1·0208 as the ratio of the force in Dublin to unity in London, according to a determination of Mr. Lloyd's, published in the Transactions of the Royal Irish Academy, in 1836. The values at the Scottish stations were consequently subject to be altered by any modification which Mr. Lloyd's determination in Dublin might subsequently receive. In the present report Mr. Lloyd has given a corrected value for the force in Dublin, resulting from a much larger number of determinations. The corrected value is 1·0195. With this value, therefore, and the comparative observations at Dublin and Helensburgh, published in the Sixth Report of the British Association, we may now derive a more correct expression, relatively to London for the intensity at Helensburgh as the base of the Scottish determinations.

The observations contained in the Scotch report presented a double comparison between Dublin and Helensburgh: one by the observations of the 22nd July, in Dublin, and the 27th July, at Helensburgh; the other by those of August 2, and September 13 and 14, at Helensburgh, and October 4, at Dublin. They are presented in the following table.

Note.—Between the first and second comparisons the needle sustained an accident, which is related in the Scottish Report, and which accounts for the angles of deflection being different in the two comparisons.

Table LV.

Station.	Date.	Ther.	θ	δ	$\frac{\cos\theta}{\sin(\delta-\theta)}$	Intensity.	
						Dublin=1	London=
	1836.						
Dublin	July 22	56°	−18° 27′·2	71° 03′·6	·94843	1·0000	1·0195
Helensburgh ...	July 27	60	−17 17·9	72 16·8	·95478	1·0067	1·0263
Helensburgh ...	Aug. 2	65	−18 59·7	72 16·8	·94572	1·0062	1·0258
Helensburgh ...	Sept. 13&14	64	−19 06·1	72 16·8			
Dublin	Oct. 4	49	−19 53·3	71 03·2	·93993	1·0000	1·0195

Whence it results that $\left(\frac{1·0263+1·0258}{2}\right)=1·0261$ expresses the force at Helensburgh relatively to unity in London, as derived through the medium of Dublin.

In 1838 I visited Helensburgh for the purpose of obtaining a *direct* comparison with London. The observations which I then made are included with the series already given in Table XLV; their result is 1·0252. The near agreement of this result, with that obtained in 1836 through the medium of Dublin, is satisfactory, both in confirming the relation of the Scottish intensities to London, and in showing the confidence to which this mode of experiment is entitled. I have taken 1·0258 as the force at Helensburgh, considering the determination through Dublin as entitled to rather the most weight; and have computed from it the value of the intensity at the other stations, as inserted in the final column of Table LVI.

Table LVI.

Station.	Date.	Ther.	θ	δ	$\frac{\cos\theta}{\sin(\delta-\theta)}$ Temp. 60°	Intensity.	
						Helensburgh = 1·0000	London = 1·0000.
	1836.						
Helensburgh	Aug. 2	65°	−18° 59′·7	72° 16′·8	·94572	1·0000	1·0258
	Sept. 13&14	64	−19 06·1				
Cumbray	July 30	64	−18 31·9	72 01·2	·94839	1·0028	1·0287
Tobermorie ...	Aug. 10	70	−15 29·3	73 07·7	·96452	1·0199	1·0462
Loch Slapin ...	— 14	56	−15 59	73 02·2	·96130	1·0165	1·0427
Glencoe.........	— 17	57	−17 50·8	72 17·2	·95173	1·0064	1·0324
Inverness ...	— 20	59	−16 44·2	72 46·5	·95718	1·0121	1·0382
	— 24	58	−16 53·7				
Golspie	— 23	51	−17 08·4	72 55·6	·95510	1·0099	1·0360
Gordon Castle	— 25	60	−16 52·4	72 40·9	·95693	1·0119	1·0380
Alford	— 27	57	−18 22	72 22	·94900	1·0035	1·0290
Braemar.........	— 30	44	−18 40·1	72 14·2	·94668	1·0010	1·0269
Blairgowrie ...	— 31	59	−18 06·1	71 54·7	·95052	1·0051	1·0310
Newport.........	Sept. 1	60	−18 40·8	72 17·5	·94745	1·0018	1·0277
Kirkaldy	— 3	60	−18 37·7	72 11	·94769	1·0021	1·0279
Melrose	— 6	51	−19 43·7	71 37	·94111	0·9951	1·0208
Dryburgh	— 7	56	−19 56·1	71 33·7	·94023	0·9942	1·0199
Edinburgh......	— 8	55	−19 24	71 49·4	·94320	0·9973	1·0231
Glasgow.........	— 9	56	−19 24	72 01·7	·94330	0·9974	1·0232
Loch Ranza ...	— 16	57	−18 55·9	72 23·0	·94600	1·0003	1·0261
Cambleton......	— 16	53	−18 16·1	71 56	·94925	1·0037	1·0296
Loch Ryan ...	— 18	52	−19 31·8	71 43·5	·94230	0·9964	1·0221

In the discussion of these observations in the 6th Report of the British Association, I have adverted to the frequent influence of the igneous rocks in Scotland in producing what may be termed *station error*. In the table in page 20 of that Report, the intensities observed at Tobermorie, both by the statical and horizontal methods, are shown to have been affected, apparently by an error of this nature, to a degree much exceeding that of the results at any other station. In combining the results of both methods, therefore, for the values of x, y, &c., I have thought it right to omit altogether the intensities at Tobermorie. We have, therefore, the statical results at nineteen stations to combine by the method of least squares, whence we obtain the following values: $x = + \cdot 000083$; $y = - \cdot 000107$; $u = - 52° 15'$; $r = \cdot 000136$. The mean geographical position is in latitude 56° 22′ N. and longitude 4° 01′ W.

Captain Ross's Observations.—These were made with two needles, R L (3) and R L (4), on Professor Lloyd's principle, used in Captain Ross's six-inch circle. One of these needles (R L 3) appears to possess the peculiar property of preserving its magnetism unchanged in different temperatures, requiring no reduction to a mean temperature. Table LVII. contains a series of experiments with it, made by Captain Ross, by which it will be seen that in differences of temperature, including the whole range of natural temperatures to which it is likely to be exposed, the time of vibration of the needle remained unaltered.

Table LVII.

Observations to investigate the influence of differences of temperature on the time of vibration of Captain Ross's statical needle R L (3).

Date.	Hour.	Ther.	Time of 100 Horizontal Vibrations.	Date.	Hour.	Ther.	Time of 100 Horizontal Vibrations.
1839.	h m	°	s	1839.	h m	°	s
Jan. 17.	10 39 A.M.	32	428·4	Jan. 17.	2 29 P.M.	60	428·0
	10 54	32	428·4		2 45	56	428·2
	11 42	98	428·8		3 01	53	428·2
	11 58	92	428·4		3 16	51	428·0
	0 17 P.M.	83	428·2		3 32	50	428·4
	0 56	92	428·4	Jan. 18.	10 32 A.M.	28	428·4
	1 11	87	428·4		10 47	29	427·8
	1 27	81	428·8		11 08	30	428·4
	1 44	75	428·6		11 34	30	428·4
	1 58	70	428·2		11 49	30	428·6
	2 14	64	428·4		0 05 P.M.	30	428·5

The following table, No. LVIII. contains two series of experiments of a similar nature, with R L (4), one made by Major

Sabine, and the other by Captain Ross, the results according extremely well in the value of the coefficient deduced.

Table LVIII.

Observations to ascertain the coefficient in the formula for reduction to a mean temperature of Captain Ross's statical needle R L (4).

Major Sabine, Tortington, December 18, 1838.

Hour.	Temp.	Time of 100 Vibrations.	Means.	
h m	°	s		
10 38 A.M.	47	480·4	480·4 at 46·75	Here, in the formula, $a = \frac{2(T-T')}{T'(\tau-\tau')}$, $T = 480·25$; $T - T' = 0^s·75$; $\tau - \tau' = 55°·8$: whence $a = ·000056$.
11 34	46·5	480·4		
1 57 P.M.	109	480·8	481·0 at 103·3	
2 22	103	481·4		
2 42	98	480·8		
5 02	49	479·4	480·1 at 48·3	
6 43	48	480·8		
7 44	48	480·0		

Captain Ross, London, February 21, 1839.

Hour.	Temp.	Time of 100 Vibrations.	Means.	
h m	°	s		
1 14 P.M.	38	474·2	474·22 at 38°	Here $T = 474·2$; $T - T' = 1^s·16$ $\tau - \tau' = 45°·1$; whence, $a = ·000054$.
1 45	38	474·23		
2 19	104	474·96	474·90 at 102·5	
2 41	103	474·93		
3 7	103	474·61		
3 30	100	475·01		
3 51	88	474·36	474·51 at 75·7	
4 34	77	474·72		
4 53	71	474·66		
5 15	67	474·30		
8 7	50	473·98	474·03 at 50	
8 32	50	474·08		

By the mean of the two determinations $a = ·000055$; which being multiplied by ·43429, the modulus of the common system of logarithms $= ·000024$, the coefficient of $(\tau - \tau')$ in the correction for temperature.

In the following table are collected the observations made with these needles in London, in July 1838, and in December of the same year, for the double purpose of examining the steadiness of their magnetism in the interval,—during which they had been employed in the observations in Scotland now under notice, and in a similar series in Ireland,—and of determining the angle of deflection in London as the base station of both series.

Table LIX.

Needle.	Date.	Hour.	Ther.	θ	δ	$\frac{\cos\theta}{\sin(\delta-\theta)}$	Intensity.	
	1838.	h m	°	° ′	° ′			
R L (3).	July 7	5 30 P.M.	70	−26 38·4	69 14·2	·89683	0·9986	1·0000
	July 12	0 30	70	−27 1·9				
	——	1 30	70	−27 48·3				
	Dec. 5	3 0	45	−26 32·9	69 14·7	·89932	1·0014	
	——	3 40	45	−26 30·3				
						Mean. ·89807		
R L (4).	July 10	5 0	68	−13 33·2	69 14·2	·98061	1·0005	1·0000
	July 12	4 0	72	−13 32·7				
	——	5 0	72	−13 30·7				
	Dec. 4	3 0	47	−13 28·8	69 14·7	·97970	0·9995	
	——	4 30	47	−13 28·0				
						Mean. ·98015		

On comparing the observations with both needles in July and in December, we may conclude that the magnetism of both had remained unchanged during the interval; the small differences are only such as frequently occur on different days; they are, moreover, in different directions, and so far will compensate each other in the final deduction.

In Table LX. are comprised Captain Ross's observations with these needles at nine stations in Scotland and the north of England.

Table LX. Needle, R L (3).

Station.	Date.	Hour.	Ther.	θ	δ	$\frac{\cos\theta}{\sin(\delta-\theta)}$	Intensity. London = 1·0000.
	1838.	h m				Therm. 60°	
Aberdeen ...	July 19	3 0 P.M.	63°	− 23° 32′·3	72° 27′·6	·92185	1·0266
Lerwick	— 25	3 0 P.M.	54	− 22 29·1	73 44·9	·92947	1·0351
	— 26	0 30	53	− 22 33·1			
		1 10	53	− 22 21·8			
Kirkwall......	Aug. 1	11 30 A.M.	60	− 22 4·3	73 20·4	·93081	1·0366
		0 30 P.M.	60	− 22 5·5			
Inverness ...	— 14	2 30 P.M.	59	− 21 49·6	72 46·2	·93118	1·0370
		3 15	59	− 21 53·1			
Newcastle ...	— 29	4 0 P.M.	60	− 24 55·9	71 13·0	·91202	1·0156
		5 0	60	− 24 57·1			

Needle, R L (4).

Station.	Date.	Hour.	Ther.	θ	δ	$\frac{\cos\theta}{\sin(\delta-\theta)}$	Intensity. London = 1·0000.
Aberdeen ...	July 19	4 0 P.M.	63	− 7 35·2	72 27·6	1·0056	1·0270
Lerwick	— 25	3 30 P.M.	54	− 4 52·2	73 44·9	1·0159	1·0365
	— 26	2 0	55	− 4 51·0			
		3 0	55	− 5 0·0			
Kirkwall......	Aug. 1	1 30 P.M.	61	− 4 55·6	73 20·4	1·0175	1·0381
		2 10	61	− 5 0·			
Inverness ...	— 14	3 30 P.M.	60	− 5 14·7	72 46·2	1·0180	1·0386
		4 30	60	− 5 14·1			
Newcastle ...	— 29	3 00 P.M.	61	− 9 51·9	71 13·0	·99727	1·0175
		3 30	61	− 9 52·9			
Stonehouse...	Sept. 3	10 40 A.M.	61	− 9 48·5	71 24·1	·99713	1·0173
		Noon.	63	− 9 50·7			
Jordan Hill..	— 11	2 0 P.M.	64	− 7 36·7	72 20·0	1·0055	1·0259
		3 0	62	− 7 41·5			
	— 13	10 0 A.M.	63	− 8 2·8			
		11 0	63	− 8 3·5			
Berwick	— 17	11 30 A.M.	60	− 8 19·9	71 41·9	1·0050	1·0254
		Noon.	60	− 8 12·0			
Dunkeld......	— 20	0 15 P.M.	57	− 7 37·2	72 23·1	1·0063	1·0267
		2 0	57	− 7 37·4			

Collecting the results in one view, we have as follows:

TABLE LXI.

Station.	R L (3.)	R L (4).	Mean.	Station.	R L (3).
Aberdeen ...	1·0266	1·0270	1·0268	Dunkeld......	1·0267
Lerwick......	1·0351	1·0365	1·0358	Jordan Hill..	1·0259
Kirkwall ...	1·0366	1·0381	1·0373	Berwick......	1·0254
Inverness ...	1·0370	1·0386	1·0378	Stonehouse...	1·0173
Newcastle ...	1·0156	1·0175	1·0165		

If we combine the results at these nine stations by the method of least squares, we obtain the following values: $x = + \cdot 000080$; $y = - \cdot 000069$; $u = - 40° 38'$; $r = \cdot 000106$. The mean geographical position is in latitude 56° 52′ and longitude 2° 45′ W.

§ 2. *By the Method of Vibrations.*

Major Sabine's Observations.—These observations were made in the summer of 1836; a detailed account of them is given in the Sixth Report of the British Association. Two cylinders, L *a* and L *b*, were vibrated at twenty-two stations in Scotland, between the 28th July and 18th September, during which interval the magnetism of the cylinders was proved to have been steady. The times of vibration at the several stations, reduced to a temperature of 60°, are inserted in Table LXII., being taken from the Sixth Report of the Association. The values of the horizontal intensity are given in the table in relation to unity at Helensburgh; and those of the total force to unity in London: the intensity at Helensburgh having been already shown to be as 1·0258 to 1·0000 in London.

Table LXII.

Station.	Date.	Needle.	Time of Vibration, Therm. 60.	Horizontal Intensity. Helensburgh = 1·0000.		Observed Dip.	Total Intensity. London = 1·0000.
	1836.		s				
Helensburgh	July 28—Aug. 2	L a	251·05	1·0000	1·0000	72° 16′·8	1·0258
	Sept. 13–14......	L a	251·27				
	July 28—Aug. 2	L b	302·08	1·0000			
	Sept. 13–14......	L b	301·33				
Great Cumbray	July 30	L a	249·82	1·0108	1·0087	72 01·2	1·0203
	—	L b	300·71	1·0066			
Loch Gilphead..	Aug. 7...........	L a	249·75	1·0113	1·0106	72 07·7	1·0279
	—	L b	300·22	1·0099			
Tobermorie......	Aug. 10	L a	254·34	0·9755	0·9753	73 07·7	1·0492
	—	L b	305·46	0·9752			
Loch Slapin	Aug. 14	L a	254·50	0·9740	0·9761	73 02·2	1·0446
	—	L b	305·04	0·9782			
Artornish.........	Aug. 16	L a	252·57	0·9866	0·9878	72 42·9	1·0380
	—	L b	303·75	0·9889			
Glencoe	Aug. 17	L a	250·32	1·0035	1·0051	72 17·2	1·0315
	—	L b	301·17	1·0067			
Fort Augustus...	Aug. 19	L a	253·34	0·9829	0·9839	72 40·4	1·0315
	—	L b	304·00	0·9849			
Inverness.........	Aug. 21	L a	253·11	0·9831	0·9850	72 46·5	1·0385
	Aug. 24	L a	253·53				
	Aug. 21	L b	303·16	0·9869			
	Aug. 24	L b	304·25				
Golspie	Aug. 23	L a	254·48	0·9741	0·9735	72 55·6	1·0353
	—	L b	305·87	0·9729			
Gordon Castle...	Aug. 25	L a	252·72	0·9877	0·9886	72 40·9	1·0371
	—	L b	303·29	0·9895			
Rhynie	Aug. 26	L a	251·09	1·0006	1·0018	72 25·7	1·0358
	—	L b	301·24	1·0031			
Alford	Aug. 28–29......	L a	252·23	0·9916	0·9926	72 22·0	1·0231
	— 28	L b	302·67	0·9936			
Braemer	Aug. 30	L a	250·96	1·0016	1·0035	72 14·2	1·0270
	—	L b	300·88	1·0055			
Blairgowrie......	Aug. 31	L a	248·10	1·0249	1·0260	71 54·8	1·0319
	—	L b	297·69	1·0272			
Newport	Sept. 1...........	L a	251·26	0·9992	0·9995	72 17·5	1·0260
	—	L b	301·72	0·9998			
Kirkaldy.........	Sept. 3	L a	250·79	1·0030	1·0042	72 11·0	1·0247
	—	L b	300·87	1·0055			
Melrose	Sept. 6	L a	247·56	1·0293	1·0311	71 37	1·0208
	— 6 and 7 ...	L b	296·85	1·0330			
Dryburgh	Sept. 7...........	L a	247·20	1·0323		71 33·7	1·0191
Loch Ranza......	Sept. 16	L a	252·57	0·9889	0·9897	72 23	1·0210
	—	L b	303·15	0·9905			
Campbelton......	Sept. 17	L a	249·33	1·0148	1·0163	71 56·0	1·0232
	—	L b	299·05	1·0178			
Loch Ryan	Sept. 18	L a	247·68	1·0283	1·0299	71 43·4	1·0254
	—	L b	297·06	1·0315			

Omitting Tobermorie, for the reasons assigned in page 157, and combining the results at the other twenty-one stations by the method of least squares, we obtain the following values: $x = +\cdot000080$; $y = -\cdot000118$; $u = -55°\ 46'$; $r = \cdot000143$. The mean geographical position is latitude 56° 35′, and longitude 4° 15′ W.

Captain Ross's Observations.—These were made in the summer of 1838 with a cylinder (X) described in page 148. It was vibrated at Westbourne Green, near London, in June and July 1838, and again in December of the same year, having been used in the interval both in Scotland and in Ireland. The observations at Westbourne Green, showing that its magnetism underwent no change in this interval, are contained in the following table.

TABLE LXIII.

Date.	Hour.	Therm.	Time of 100 Vibrations.	Mean Time of 100 Vibrations at 60°.		Observed Dip.
1838.	h m	°	s			
June 2...	11 52 A.M.	65	280·27			
—	0 21 P.M.	65	280·32			
June 5...	10 59 A.M.	65	280·25	279·99		
—	0 4 P.M.	68	280·53			
June 8...	11 49 A.M.	57	279·78			
—	0 12 P.M.	57	279·63			
July 6...	11 05 A.M.	68	280·38		280·06	69° 14′·5
—	11 27 A.M.	70	280·40	280·24		
July 12...	10 50 A.M.	68	280·83			
—	11 12 A.M.	68	280·98			
Nov. 30...	11 0 A.M.	50	279·48	279·95		
—	11 27 A.M.	51	279·52			

The coefficient in the formula for the reduction to a mean temperature is ·00017.

Table LXIV. contains the observations with cylinder (X) at ten stations in Scotland, and at two stations in the north of England, viz. Newcastle and Stonehouse. The values of the total intensity in the final column, relatively to unity in London, have been computed by means of the time of vibration of this cylinder in London shown in the preceding table.

TABLE LXIV.

Station.	Date.		Hour.	Therm.	Time of 100 Vibrations.	Corrected Time.	Observed Dip.	Intensity. London = 1·0000.
	1838.		h m	°	s	s	° ′	
Aberdeen	July	18	2 10 P.M.	64	299·57	299·37	72 27·6	1·0292
			3 1	61	299·42			
Lerwick	July	23	2 52 P.M.	50	307·82			
		24	11 12 A.M.	54	309·35			
		26	11 0	52	308·82	309·27	73 44·9	1·0386
		27	11 12	60	309·90			
		28	0 40 P.M.	54	308·98			
Kirkwall.........	July	31	11 50 A.M.	56	304·92			
	Aug.	1	10 50	59	305·12			
		3	11 44	58	305·12	305·31	73 20·4	1·0403
		4	11 21	60	305·82			
		6	11 28	57	305·08			
Wick	Aug.	8	11 12 A.M.	58	305·32	305·43	73 19·9	1·0390
Golspie	Aug.	10	11 42 A.M.	66	303·28			
		11	11 27	63	303·48	303·26	73 04·4	1·0382
		12	10 28	62	303·58			
Inverness	Aug.	13	1 32 P.M.	58	300·38	300·51	72 46·0	1·0395
		14	Noon.	59	300·48			
Newcastle	Aug.	29	8 3 A.M.	52	290·9			
		30	10 40	59	291·28	291·41	71 13·0	1·0167
			11 15	60	291·6			
Stonehouse	Sept.	1	0 46 P.M.	57	292·87	292·86	71 24·0	1·0163
		3	11 11 A.M.	60	292·70			
Culgruff	Sept.	6	0 45 P.M.	58	293·0			
		7	10 18 A.M.	47	293·05	293·50	71 35·7	1·0219
		8	0 43 P.M.	52	293·35			
		9	9 47 A.M.	51	293·00			
Jordan Hill......	Sept.	11	5 27 P.M.	60	298·18			
		12	11 0 A.M.	56	298·22	298·39	72 20·3	1·0289
		13	8 54	60	298·55			
Berwick	Sept.	17	9 4 A.M.	56	293·83	294·02	71 41·9	1·0241
		18	9 2	52	293·62			
Dunkeld	Sept.	20	10 22 A.M.	58	298·82	298·92	72 23·1	1·0281
		21	10 19	48	298·32			

If we combine these twelve results by the method of least squares, we obtain the following values, viz.: $x = +\cdot000091$; $y = -\cdot000086$; $u = -43° 32'$; $r = \cdot000125$. The mean geographical position is 56° 56′ N. lat., and 2° 58′ W. long.

If we collect in one view the values of u and r which have been obtained from the several series in Scotland, we have as as follows:

TABLE LXV.

Observer.	Method.	No. of Stations.	Mean Geographical Position.		Values of	
			Lat.	Long.	u	r
Sabine...	Statical	19	56° 22′	4° 01′	−52° 15′	·000136
Ross......	Statical	9	56 52	2 45	−40 38	·000106
Sabine ...	Hor. Vibrations....	21	56 35	4 15	−55 46	·000143
Ross......	Hor. Vibrations....	12	56 56	2 58	−43 32	·000125

Regarding the values of u and r as entitled to weight proportioned to the number of stations of which each is the representative, and giving equal weight to a result by each method, we obtain −50° 02′ and ·000132 as the mean values of u and r derived from the Scottish series, and corresponding to the central geographical position in 56° 40′ N. lat., and 3° 30′ W. longitude.

SECTION III.—IRELAND.

(*By the* REV. H. LLOYD.)

1. *Method of Vibration.*

The body of results obtained by this method in Ireland has received some valuable accessions, and undergone other important alterations, since the publication of the Irish Magnetic Report. We shall consider these under the following heads. 1. Additional observations; 2. Corrections of the results previously obtained; 3. New determinations of the intensity at the base stations.

Additional Observations.—These consist in a comparison of the intensity at London and Dublin, made by myself in the year 1836; a comparison of Dublin and Bangor, made by Major Sabine in the latter part of the same year; a comparison of London and Dublin, by the same observer, in the year 1838; and a complete series of observations made by Captain James

Ross, in the year 1838, at twelve distinct stations throughout the island. This latter series, forming in themselves a complete body of results, will be considered separately. The additional observations made by Major Sabine and myself are contained in the following table*.

Table LXVI.

Cylinder L (*a*).

Station.	Date.	Hour.		Time.	Temp.	Corr. Time.
		h	m	s	° ′	s
Dublin	April 11, 1836.	11	14	243·56	56·2	243·76
	— 12	11	8	243·96	61·0	243·88
	— 15	11	14	243·50	56·5	243·69
	Mean.			243·67	57·9	243·78
London	April 19	12	11	236·02	59·5	236·04
	— 21	2	3	235·94	60·0	235·93
	— 22	11	51	236·23	61·5	236·13
	Mean.			236·06	60·3	236·03
Dublin	May 7	12	30	234·96	57·6	244·10
	— 9	12	6	243·90	61·0	243·83
	Mean.			243·93	59·3	243·96
Dublin	July 24, 1836.	9	0	243·47	59·0	243·53
	— 25	7	30	243·11	55·0	243·41
	Mean.			243·29	57·0	243·47
Bangor	Sept. 21	9	45	246·53	48·6	247·20
	— 21	10	15	246·72	49·0	247·39
	Mean.			246·62	48·8	247·30
Dublin	Oct. 3	10	10	243·25	45·0	244·16
	— 3	2	8	243·22	47·0	244·01
	— 3	2	30	243·09	48·0	243·82
	— 4	1	45	243·18	51·5	243·70
	Mean.			243·18	47·9	243·92
London	June 1, 1838.	11	37	236·27	62·0	236·15
	— 1	11	56	236·15	62·0	236·03
	Mean.			236·21	62·0	236·09
Dublin	Aug. 6	4	12	246·30	66·0	245·93
	— 8	3	44	245·81	63·0	245·63
	Mean.			246·05	64·5	245·78
London	Oct. 13	3	0	237·77	39·0	239·01
	— 13	3	16	237·81	40·0	238·99
	Mean.			237·79	39·5	239·00

* The details of the comparison of Bangor and Dublin have been already printed in the Scotch Magnetic Report: they are reprinted here, so that all the results obtained in Ireland may be seen in connexion.

Table LXVII.

Cylinder L (b).

Station.	Date.	Hour.		Time.	Temp.	Corr. Time.
	1836.	h	m	s	° ′	s
Dublin	April 11	10	48	292·93	56·5	293·16
	— 12	10	44	293·50	59·2	293·53
	— 15	10	48	293·09	56·8	293·29
	Mean.			293·17	57·5	293·33
London	April 19	11	36	284·17	61·0	284·08
	— 21	1	38	284·44	60·5	284·38
	— 22	11	22	284·27	60·5	284·21
	Mean.			284·29	60·7	284·22
Dublin	May 7	12	5	292·95	58·0	293·08
	— 9	11	40	293·43	61·0	293·34
	Mean.			293·19	59·5	293·21
Dublin	July 24	8	30	293·22	59·0	293·29
	— 25	8	0	292·25	54·0	292·69
	— 25	8	40	292·57	55·5	292·90
	Mean.			292·68	56·2	292·96
Bangor	Sept. 21	11	10	295·28	49·6	296·04
Dublin	Oct. 3	9	25	291·02	44·5	292·15
	— 3	9	45	291·24	44·5	292·37
	— 3	2	55	291·37	49·6	292·13
	— 4	1	15	291·73	53·5	292·20
	Mean.			291·34	48·0	292·21
London	June 1, 1838.	10	21	284·17	62·0	284·00
	— —	11	4	284·17	62·0	284·00
	Mean.			284·17	62·0	284·00
Dublin	Aug. 6	3	42	292·88	67·0	292·37
	— 8	1	42	292·48	63·0	292·26
	Mean.			292·68	65·0	292·31

Correction of the Results.—The first correction that seems to be required is in the series of results obtained in the North of Ireland, in the autumn of the year 1834. On a comparison of the times of vibration of cylinder L (b) in Dublin, at the commencement and end of that series, it will be seen that the magnet sustained a loss of force; and an attentive examination of the other parts of the series shows that this loss occurred *immediately* previous to the final observation in Dublin. This fact will be seen very evidently by means of the following table, which contains the corrected rates of the *two* cylinders, and the deduced values of the intensity compared with the intensity in Dublin *at the time of the initial observation.* The results obtained with the two cylinders present a very close agreement, except in the final observation.

TABLE LXVIII.

Station.	L (*a*).		L (*b*).	
	Time.	Intensity.	Time.	Intensity.
	s		s	
Dublin	243·90	1·000	292·74	1·000
Armagh	246·88	·976	296·40	·975
Carn	248·10	·966	297·71	·967
Strabane	248·51	·963	298·22	·964
Enniskillen	248·42	·964	297·83	·966
Dublin	243·92	1·000	293·62	·994

Hence, instead of comparing the other results of cylinder L (*b*) with the *mean* of the initial and final observations in Dublin, they are to be compared with the *initial* observations *alone*; the final observations not being comparative with the rest of the series. The loss of force sustained by the cylinder L (*b*) being ·006, the amount of the correction is

$$\delta h = - \cdot 003 \times h;$$

h denoting the horizontal intensity, as originally deduced, and δh its correction.

A correction of a similar kind (that is, depending on the rate of vibration at the base station) seems to be required also in the series of results obtained in the west and south of Ireland in the summer of 1835. In reducing the observations of this series, I had taken as the Dublin time, the mean of the initial and final times, without regarding the number of separate observations; but, if we suppose the difference between these times to be owing to errors of observation, or to any *fluctuating* source, it is manifest that we should take, as the Dublin time, the mean of the separate results themselves. This seems to be the proper course in the present instance. The initial time is the result of a single observation only, and that taken under the disadvantage of an unusually high temperature; so that the difference between it and the final time (which difference is nearly the same for the two cylinders) is probably due either to the irregular fluctuations of the horizontal intensity, or to error in the coefficient of the temperature correction.

It is easy to determine the amount of the required correction. If T denote the time of vibration at any station, T′ that at the base station, and h the ratio of the horizontal intensities,

$$h = \frac{T'^2}{T^2}.$$

Hence if $\delta T'$ denote the small correction in the value of T', and δh the corresponding correction of h,

$$\frac{\delta h}{h} = \frac{2\,\delta T'}{T'}.$$

To apply this in the present instance, we have

	L (*a*)	L (*b*)
	s	s
Mean of separate observations . . .	243·43	293·18
Mean of initial and final results . .	243·29	293·06
Correction of T', or $\delta T'$	+0·14	+0·12
Resulting value of $\frac{\delta h}{h}$	+·0011	+·0008

The corrections here obtained are applied to all the results of the series (Aug. 19, to Sept. 15, 1835) in Table LXVIII.

Values of the Intensity at the Base stations.—The following is a summary of the comparisons of the horizontal intensity in London, Dublin, and Limerick, as contained in Table LXIX.

Horizontal intensity in Dublin, referred to London :

July, August,	1835.	Cyl. R *c*	Int.	= ·9456
— —	—	— R *d*	—	= ·9421
Sept. Oct. Nov.	1835.	— L *a*	—	= ·9354
— — —	—	— L *b*	—	= ·9348
April, May,	1836.	— L *a*	—	= ·9367
— —	—	— L *b*	—	= ·9392
June, Aug. Oct.	1838.	— L *a*	—	= ·9340
— — —	—	— L *b*	—	= ·9440
		Mean		= ·9390

Horizontal intensity in Limerick, referred to London :

July, Aug. Sept.	1834.	Cyl. S *b*	Int.	= ·9396
July,	1835.	— S *b*	—	= ·9470
July, August,	1835.	— R *c*	—	= ·9461
July, August,	1835.	— R *d*	—	= ·9513
		Mean		= ·9460

Horizontal intensity in Limerick, referred to Dublin:

October,	1834.	Cyl.	L *a*	Int.	= 1·0075
—	—	—	L *b*	—	= 1·0015
July, Aug.	1835.	—	R *c*	—	= 1·0005
— —	—	—	R *d*	—	= 1·0098
Aug. Sept.	1835.	—	L *a*	—	= 1·0039
— —	—	—	L *b*	—	= 1·0055
Nov. Dec.	1835.	—	L *a*	—	= 1·0001
— —	—	—	L *b*	—	= 1·0021
			Mean	....	= 1·0039

Now, the comparison of Dublin with London and with Limerick being each the mean of eight separate comparisons, while that of Limerick and London is deduced from four only, we have (see Fifth Report, p. 133.)

$$A = 2B = C.$$

Hence the formulæ of page 134 become

$$\delta x = \frac{\frac{1}{2} b (c_{\prime} - c)}{\frac{1}{2}(a^2 + c_{\prime}^2) + 1}, \quad \delta y = -\frac{a (c_{\prime} - c)}{\frac{1}{2}(a^2 + c^2) + 1};$$

but $\quad a = \cdot 9390, \quad b = \cdot 9460, \quad c = 1\cdot 0039;$

$$c_{\prime} = \frac{b}{a} = 1\cdot 0075, \quad c_{\prime} - c = \cdot 0036:$$

and, substituting these values,

$$\delta x = + \cdot 0009, \qquad \delta y = - \cdot 0017;$$
$$x = a + \delta x = \cdot 9399;$$
$$y = b + \delta y = \cdot 9443.$$

The numbers in the 6th column of the following table are deduced from those of the 5th, by multiplying by one or other of these numbers, according as the station has been compared, in the first instance, with Dublin or with Limerick.

It will readily appear, from the principles laid down in pages 95 *et seq.*, that the *weights* of these determinations are expressed by the formulæ

$$X = A + \frac{B C c^2}{B a^2 + C}, \quad Y = B + \frac{A C}{A a^2 + C c^2},$$

Now, A=C=8, B=4; substituting these values, and those of *a*, *b*, *c*, given above, we have

$$X=10{\cdot}8, \quad Y=8{\cdot}2;$$

the weight of a single comparison being unity.

Table LXIX.

Intensity of the Horizontal Force.

Station.	Date.	Cyl.	No.	Hor. Int.	Hor. Int. (London=1.)
	1834.				
Limerick	July, Sept.	S *b*	5	. . .	·9396
London	Aug. 20–27	S *b*	20		1·0000
Limerick	Sept. Oct.	S *b*	3	1·0000	
	— 9, Oct. 8	L *a*	2	1·0000	·9443
	— 9, — 8	L *b*	3	1·0000	
Ballybunian	— 16	S *b*	1	1·0010	
	— 17	L *a*	1	·9954	·9441
	— 17	L *b*	1	1·0029	
Glengariff............	— 27	S *b*	1	1·0110	
	— 27	L *a*	1	1·0110	·9511
	— 27	L *b*	1	·9997	
Killarney	Oct. 4	S *b*	1	1·0039	
	— 4	L *a*	1	1·0086	·9503
	— 4	L *b*	1	1·0066	
Kiltanon	— 12	S *b*	1	·9983	·9427
Templemore	— 17	S *b*	1	1·0404	·9824
Clonmel	— 19	S *b*	1	1·0092	·9530
Fermoy	Dec. 2	S *b*	1	1·0157	·9591
Limerick	— 10	S *b*	1	1·0000	·9443
Dublin	Oct. 10–28	L *a*	6	1·0000	·9399
	— 11	L *b*	2	1·0000	
Limerick	— 8	L *a*	1	1·0075	·9441
	— 8	L *b*	2	1·0015	
Carlingford	— 13	L *b*	1	·9868	·9275
Armagh...............	— 14, 15	L *a*	2	·9761	·9172
	— 14, 15	L *b*	2	·9754	
Colerain...............	— 18, 20	L *b*	2	·9870	·9277
Carn	— 21	L *a*	1	·9665	·9086
	— 21	L *b*	1	·9669	
Strabane	— 23	L *a*	1	·9633	·9056
	— 23	L *b*	1	·9636	
Enniskillen	— 24	L *a*	1	·9640	·9070
	— 24	L *b*	1	·9661	

Station.	Date.	Cyl.	No.	Hor. Int.	Hor. Int. London=1.
	1835.				
London	July 4–7	S *b*	12		1·0000
	July 8–20 }	R *c*	25		1·0000
	Aug. 28–31 }	R *d*	14		1·0000
Limerick	July 27, 28	S *b*	2		·9470
	— 27–29	R *c*	10	1·0005	·9461
	— 29–31	R *d*	11	1·0098	·9513
Dublin	Aug. 16	R *c*	3	1·0000	·9456
	— 14	R *d*	3	1·0000	·9421
Markree	— 19	R *c*	3	·9531	·9012
	— 19, 20	R *d*	3	·9558	·9005
Dublin	Aug. 19 }	L *a*	5	1·0000 }	·9399
	Sept. 12–15 }	L *b*	4	1·0000 }	
Markree	Aug. 21	L *a*	1	·9580 }	·8998
	— 21	L *b*	2	·9566 }	
Ballina	— 22	L *a*	1	·9545 }	·8959
	— 22	L *b*	1	·9517 }	
Belmullet	— 24	L *a*	1	·9497 }	·8906
	— 24	L *b*	2	·9454 }	
Achill	— 25	L *a*	1	·9576 }	·8990
	— 25	L *b*	1	·9552 }	
Leenan	— 26	L *a*	1	·9621 }	·9051
	— 26	L *b*	1	·9636 }	
Oughterard	— 27	L *a*	1	·9777 }	·9191
	— 27	L *b*	1	·9781 }	
Ennis..................	— 28	L *a*	1	·9995 }	·9386
	— 28	L *b*	1	·9977 }	
Limerick	— 29	L *a*	1	1·0039 }	·9443
	— 29	L *b*	1	1·0055 }	
Cork	— 31	L *a*	1	1·0211	·9597
	— 31	L *b*	1	1·0294*	
Waterford............	Sept. 1	L *a*	1	1·0125 }	·9512
	— 1	L *b*	1	1·0115 }	
Broadway	— 2	L *a*	2	1·0215 }	·9615
	— 2	L *b*	1	1·0246 }	
Rathdrum............	— 3	L *a*	1	1·0013 }	·9422
	— 3	L *b*	1	1·0035 }	
London	Sept. 19–22 }	L *a*	6		1·0000
	Oct. 23, 24 }	L *b*	7		1·0000
Dublin	Sept. 12–15 }	L *a*	7		·9354
	Nov. 5, 6 }	L *b*	6		·9348
Dublin	Nov. Dec. Jan.	L *a*	8	1·0000	
	Nov. Dec. Jan.	L *b*	7	1·0000	
Limerick	Dec. 19–23	L *a*	3	1·0001	
	— 19–23	L *b*	3	1·0021	
London	Apr. 19–22 }	L *a*	3		1·0000
	1836. }	L *b*	3		1·0000
Dublin	Apr. 11–15 }	L *a*	5		·9367
	May 7–9 }	L *b*	5		·9392

* Disturbing influence suspected in this observation: the result has been accordingly omitted in deducing the number in the last column.

Station.	Date.	Cyl.	No.	Hor. Int.	Hor. Int. (London=1.)
	1836.				
Dublin	July 24, 25	L *a*	6	1·0000	·9399
	Oct. 3, 4	L *b*	7	1·0000	
Bangor	Sept. 21	L *a*	2	·9710	·9154
	— 21	L *b*	1	·9768	
London............ ..	June 1, Oct. 13, 1838	L *a*	4		1·0000
	— 1	L *b*	2		1·0000
Dublin	Aug. 6–8	L *a*	2		·9340
	— 6–8	L *b*	2		·9440

The following table contains the resulting values of the horizontal intensity, those of the total intensity thence deduced, and the latitudes and longitudes of the stations. The values of the dip employed, in deducing the total from the horizontal intensities, will be found in Table XXXVI.

Table LXX.

Station.	Lat.	Long.	Hor. Int.	Total Int.
	° ′	° ′		
Dublin	53 21	6 16	·9399	1·0203
Limerick	52 40	8 35	·9443	1·0260
Ballybunian	52 30	9 41	·9441	
Glengariff................	51 45	9 31	·9511	1·0283
Killarney	52 3	9 31	·9503	1·0300
Kiltanon	52 52	8 43	·9427	1·0318
Templemore	52 47	7 48	·9824	
Clonmel	52 20	7 41	·9530	
Fermoy	52 7	8 16	·9591	1·0259
Carlingford	54 2	6 11	·9275	1·0279
Armagh....................	54 21	6 39	·9172	1·0272
Colerain	55 8	6 40	·9277	1·0250
Carn	55 15	7 15	·9086	1·0346
Strabane	54 49	7 28	·9056	1·0303
Enniskillen	54 21	7 38	·9070	1·0321
Markree	54 12	8 26	·8998	1·0316
Ballina	54 7	9 7	·8959	1·0313
Belmullet	54 13	9 57	·8906	1·0274
Achill	53 56	9 52	·8990	1·0308
Leenan	53 36	9 40	·9051	
Oughterard	53 26	9 18	·9191	
Ennis	52 51	8 58	·9386	1·0270
Cork	51 54	8 26	·9597	1·0236
Waterford................	52 16	7 8	·9512	1·0209
Broadway	52 13	6 24	·9615	1·0194
Rathdrum................	52 55	6 14	·9422	1·0137
Bangor	54 39	5 42	·9154	1·0266

Of these results, those obtained at Templemore, Carlingford, and Colerain, are not included in the computation of the lines, being manifestly affected by disturbing action. The disturbance at the two latter stations is obviously due to the presence of trap rocks.

In deducing the lines of total intensity, I have been guided by the principles laid down in page 95 and *seq.*, and have accordingly assigned *double* weight to the results in Dublin and Limerick, the weight of each of the other comparisons being taken as unity. The results of the computation are as follows:

$$L=1{\cdot}0268, \quad M=+{\cdot}0000748, \quad N=+{\cdot}0000501;$$
$$u=-33^\circ\ 48', \quad r={\cdot}0000900;$$

L denoting the intensity at the central station (Lat. $=53^\circ\ 21'$, Long. $=8^\circ\ 0'$), the intensity at London being unity; M and N the increase of the intensity, corresponding to each geographical mile of distance in the direction of the two coordinates; u the angle which the isodynamic line, passing through the central station, makes with the meridian; and r the increase of the intensity in the direction perpendicular to that line.

The lines of *horizontal intensity* rest upon a somewhat broader basis, there being four stations where the horizontal force was observed without the dip. In deducing them, I have given a weight of *two* to the results obtained at Dublin, Limerick, and Markree, the weight of each of the other determinations being unity. We find, accordingly,

$$L={\cdot}9290, \quad M=-{\cdot}000190, \quad N=-{\cdot}000368;$$
$$u=-62^\circ\ 40', \quad r={\cdot}000414.$$

Captain Ross's observations are contained in the following table. They were made in the autumn of the year 1838, with a single cylinder, designated as R (X) in the following pages. The stations are twelve in number, and are distributed uniformly over the island. The permanency of the magnetism of the cylinder during this series, and its time of vibration at Westbourn Green, near London, have been already shown in Table LXIII.

Table LXXI.

Station.	Date.	Hour.	Therm.	Time of 100 Vibrations.	Mean reduced to Temperature 60.
	1838.	h. m.	°	s.	s.
Waterford	Oct. 3	42 3 P.M.	56	287·18	287·45
	— 4	10 17 A.M.	56	287·35	
Cork	— 6	5 32 P.M.	54	285·43	286·20
	— 7	11 40 A.M.	63	286·47	
	— 8	0 38 P.M.	54	286·08	
Valencia Island	— 12	11 26 A.M.	54	286·83	287·07
	— 13	10 26 A.M.	53	286·68	
Killarney	— 17	11 2 A.M.	52	286·75	287·17
		11 40 A.M.	52	286·96	
	— 18	2 3 A.M.	52	286·65	
	— 19	10 22 A.M.	58	286·87	
Limerick	— 22	11 8 A.M.	60	288·33	288·33
		Noon	62	288·47	
	— 23	8 41 A.M.	54	287·98	
		11 6 A.M.	57	288·18	
Shannon Harbour	— 26	9 35 A.M.	50	290·35	290·88
		10 14 A.M.	52	290·52	
Dublin	— 29	11 28 A.M.	50	288·82	289·12
		11 51 A.M.	50	288·78	
	— 30	7 59 A.M.	40	287·95	
		8 30 A.M.	41	288·13	
Armagh	Nov. 1	4 32 P.M.	42	292·	292·95
	— 2	8 44 A.M.	43	292·17	
Londonderry	— 5	11 41 A.M.	52	295·18	295·55
	— 6	11 13 A.M.	51	295·07	
Markree	— 10	11 17 A.M.	44	294·33	295·16
	— 11	4 13 P.M.	43	294·33	
Westport	— 13	0 32 P.M.	45	293·97	294·66
	— 14	11 15 A.M.	42	293·70	
Edgeworth's Town	— 19	0 15 P.M.	45	291·00	292·04
	— 20	10 59 A.M.	44	291·28	

The following table contains the resulting values of the horizontal intensity; those of the total intensity thence deduced, and the latitudes and longitudes of the stations. The dips employed in deducing the total from the horizontal intensities, are given in Table XXXIX; the London dip used in the computation is the mean dip at Westbourn Green (Table III.), reduced to the mean epoch of the present series.

TABLE LXXII.

Station.	Lat.	Long.	Hor. Int.	Total Int.
	° ′	° ′		
Waterford	52 16	7 8	·9493	1·0205
Cork	51 54	8 26	·9576	1·0239
Valencia	51 56	10 17	·9517	1·0285
Killarney	52 3	9 31	·9511	1·0271
Limerick	52 40	8 35	·9435	1·0262
Shannon Harbour	53 14	7 52	·9270	1·0287
Dublin	53 21	6 16	·9383	1·0205
Armagh	54 21	6 39	·9140	1·0296
Londonderry	55 0	7 20	·8979	1·0314
Markree	54 12	8 26	·9003	1·0321
Westport	53 48	9 29	·9034	1·0345
Edgeworth's Town	53 42	7 33	·9196	1·0264

In deducing the values of L, M, N, equal weights have been assigned to all the results. The following are the values obtained for the lines of total intensity.

$$L=1\cdot0276,\quad M=+\cdot0000858,\quad N=-\cdot0000671;$$
$$u=-38^\circ\ 0',\quad r=\cdot000109.$$

For the lines of *horizontal* intensity, we find

$$L=\cdot9269,\quad M=-\cdot000138,\quad N=-\cdot000379;$$
$$u=-70^\circ\ 0',\quad r=\cdot000403.$$

2. *Satical Method.*

Additional Observations.—The observations made according to the statical method since the printing of the Irish Magnetic Report, consist of my own observations in London and Dublin, in the year 1836; Major Sabine's observations in Limerick, Dublin, and Bangor, in the autumn of the same year; a comparison of London and Dublin, by the same observer, in the year 1838; and a series of observations, at eight distinct stations, made by Captain James Ross, towards the close of the latter year. The details of my own observations, and of those of Major Sabine, are given in the following tables. Captain Ross's observations, as before, will be considered separately.

Table LXXIII.

Mr. Lloyd's Observations, Needles L 3 and L 4.

Needle.	Station.	Date.	Hour.		Temp.	Angle.	
			h	m	°	°	′
Needle L 3.	Dublin	April 11, 1836.	12	18	57·5	−15	23·4
		— 15	12	30	53·0	−15	3·6
		Mean.	12	24	55·2	−15	13·5
	London	April 19	1	0	55·8	−18	43·5
		— 21	2	58	58·5	−18	47·6
		— 22	12	30	59·2	−19	6·0
		Mean.	1	29	57·8	−18	52·4
	Dublin	May 7	1	32	57·2	−15	52·5
		— 9	1	25	60·0	−15	52·5
		Mean.	1	28	58·6	−15	52·5
		Aug. 5	3	50	61·8	−15	53·8
		— 6	2	35	67·8	−16	9·2
		Mean.	3	12	64·8	−16	1·5
Needle L 4.	Dublin	April 11, 1836.	12	43	57·8	−13	26·4
		— 15	12	8	53·5	−13	21·0
		Mean.	12	25	55·6	−13	23·7
	London	April 19	1	28	56·8	−16	31·9
		— 21	2	37	58·5	−16	59·9
		— 22	12	14	60·5	−16	57·6
		Mean.	1	26	58·6	−16	49·8
	Dublin	May 7	1	10	56·5	−13	22·5
		— 9	12	50	60·5	−13	18·4
		Mean.	1	0	58·5	−13	20·5
		Aug. 5	3	28	61·8	−13	43·6
		— 6	2	10	66·5	−13	34·4
		Mean.	2	49	64·2	−13	39·0

TABLE LXXIV.

Major Sabine's Observations, Needle S 2.

Station.	Date.	Temp.	Angle.
		° ′	° ′
Limerick	July 15, 1836.	58·0	−17 32·8
	— 15	57·0	−17 25·9
	— 16	59·8	−17 21·5
	Mean.	58·3	−17 26·7
Dublin	July 22	54·0	−18 31·6
	— 22	56·0	−18 28·1
	— 23	57·5	−18 22·7
	Mean.	55·8	−18 27·5
Bangor	Sept. 21	50·0	−18 55·9
Dublin	Oct. 4	49·0	−19 53·3
London	June 1, 1837.	58·0	−17 52·1
	— 1	58·0	−17 56·6
	July 25	70·0	−18 7·4
	— 25	73·0	−18 0·5
	Mean.	64·8	−17 59·2
	Nov. 14	50·0	−17 12·8
	— 14	50·0	−17 14·7
	— 16	37·0	−16 53·7
	— 16	37·0	−16 52·6
	— 16	37·0	−17 0·6
	Mean.	42·2	−17 2·9
Dublin	July 31, 1838.	65·0	−14 29·1
	— 31	65·0	−14 29·7
	Aug. 2	66·0	−14 19·8
	— 3	67·0	−14 29·5
	— 3	67·0	−14 26·0
	Mean.	66·0	−14 26·8
London	Oct. 12, 1838.	48·0	−17 32·1
	— 12	48·0	−17 33·9
	— 13	46·5	−17 18·3
	— 13	46·5	−17 26·8
	Mean.	47·2	−17 27·8

Correction of the Results.—The only correction which seems necessary in the results already recorded is that due to the effect of temperature upon the needle S 2, the temperature-correction of that needle having been obtained by Major Sabine subsequently to the publication of the Irish Magnetic Report. This correction is small, the coefficient in the logarithmic formula being only ·000024*. The corrected results are given in Table LXXV.

As the expression of the intensity deduced by the statical method is a function of the dip, as well as of the inclination of the needle when loaded, it may be necessary to show that the changes in the dip-corrections of the needles (page 104 and *seq.*)

* Sixth Report, p. 108.

can have no sensible effect upon the deduced values of the intensity.

The ratio of the intensity at any station to that at the base-station being denoted by ϕ, we have (Fifth Report, p. 147,)

$$\phi = \frac{\cos\theta \sin(\delta_{\prime} - \theta_{\prime})}{\cos\theta_{\prime} \sin(\delta - \theta)}.$$

Hence, supposing δ and $\delta_{\prime}$ to vary by any small and equal amount, $\Delta\,\delta$, the corresponding variation of ϕ will be expressed by the formula

$$\frac{\Delta\,\phi}{\phi} = \{\text{cotan}(\delta_{\prime} - \theta_{\prime}) - \text{cotan}(\delta - \theta)\}\,\Delta\,\delta.$$

Now the quantity, $\Delta\,\delta$, is very small, and (where the stations are not widely separate) the coefficient by which it is multiplied is likewise small; for such stations, then, the resulting value of $\frac{\Delta\,\phi}{\phi}$ is inconsiderable. On substituting the numerical values of δ, $\delta_{\prime}$, θ, $\theta_{\prime}$, for the extreme stations of the present series, it will be seen that the correction does not affect the fourth place of decimals.

Values of the Intensity at the Base stations.—The following is a summary of the comparisons of the intensity at London, Dublin, and Limerick, as contained in Table LXXV.

Intensity at Dublin, referred to London:

Aug. Sept. 1834	Needle L 4	Int.	= 1·0194
Sept. Oct. Nov. 1835 .	— L 4	—	= 1·0212
April, May, 1836 . . .	— L 3	—	= 1·0194
April, May, 1836 . . .	— L 4	—	= 1·0189
June 1837, Oct. 1838 .	— S 2	—	= 1·0183
	Mean . .		= 1·0194

Intensity at Limerick, referred to London:

June, July, Aug. 1834 .	Needle L 4	Int.	= 1·0262

Intensity at Limerick, referred to Dublin:

Aug. Sept. 1835	— L 4	Int.	= 1·0030
July 1836	— S 2	—	= 1·0062
	Mean . .		= 1·0046

We have therefore (Fifth Report, p. 148),

$$a = 1{\cdot}0194,\ b = 1{\cdot}0262,\ c = 1{\cdot}0046;$$

$$c_{\prime} = \frac{b}{a} = 1{\cdot}0067, \qquad c_{\prime} - c = {\cdot}0021;$$

$$A = 5, \qquad B = 1, \qquad C = 2.$$

Substituting these values in the formulæ of page 134 (Fifth Report), we find

$$\delta x = + \cdot 0003, \qquad \delta y = - \cdot 0012;$$
$$x = a + \delta x = 1 \cdot 0197;$$
$$y = b + \delta y = 1 \cdot 0250.$$

The results in the 6th column of the following table are deduced from those of the 5th, by multiplying by one or other of these numbers, according as the station has been originally compared with Dublin or with Limerick.

The *weights* due to the preceding determinations are given by the formulæ of page 170. Substituting the numerical values of A, B, C, &c., we find

$$X = 5 \cdot 7, \qquad Y = 2 \cdot 4;$$

the weight of a single comparison being unity. Adopting the nearest whole numbers, we may consider the deduced value of the intensity in Dublin as equivalent to the result of *six* separate comparisons; and that of the intensity in Limerick as equivalent to *two*.

TABLE LXXV.

Intensity of the Total Force.

Station.	Date.	Needle.	No.	Intensity.	Intensity. (London=1.)
London	August, 1834.	L 4	3		1·0000
Limerick	June, July.	—	3		1·0262
Dublin	Sept. 22—29.	—	4		1·0194
Dublin	Sept. Oct.	L 4	5	1·0000	1·0197
*Carlingford	Oct. 13.	—	1	1·0166	1·0366
Armagh............	— 14, 15.	—	2	1·0044	1·0242
*Colerain	— 20.	—	1	·9997	1·0194
Carn	— 21.	—	1	1·0151	1·0351
Strabane	— 23.	—	1	1·0100	1·0299
Dublin	Aug. Sept. 1835.	L 4	5	1·0000	1·0197
Markree............	Aug. 21.	—	1	1·0091	1·0290
Ballina	— 22.	—	1	1·0077	1·0276
Belmullet	— 24.	—	1	1·0093	1·0292
Achill	— 25.	—	1	1·0096	1·0295
Galway	— 28.	—	1	1·0086	1·0285
Ennis...............	— 28.	—	1	1·0055	1·0253
Limerick	— 29.	—	1	1·0030	1·0229
Cork	— 31.	—	1	·9992	1·0189
Waterford	Sept. 1.	—	1	·9966	1·0162
Broadway	— 2.	—	1	·9976	1·0173
Gorey...............	— 3.	—	1	·9933	1·0129
Rathdrum	— 3.	—	1	·9944	1·0140
London	Sept. Oct. 1835.	L. 4	6		1·0000
Dublin	Sept. Nov.	—	7		1·0212

* Evident local disturbance at these two stations. The district about Carlingford is intersected with trap dykes; Colerain lies within the basaltic field of the North East of Ireland.

Station.	Date.	Needle.	No.	Intensity.	Intensity. (London=1.)
Limerick	July, Dec. 1835.	S. 2	5	1·0000	1·0250
Ballybunian	Nov. 8.	—	1	1·0083	1·0335
Valencia............	— 12.	—	1	1·0043	1·0294
Dingle	— 18.	—	1	1·0091	1·0343
Kiltanon	Dec. 10.	—	1	1·0031	1·0282
Limerick	Dec. Jan. 1836.	S. 2	3	1·0000	1·0250
Youghal............	Dec. 29.	—	2	·9970	1·0219
London	April, 1836.	L. 3	3		1·0000
		L. 4	3		1·0000
Dublin	April, May.	L. 3	4		1·0194
	April, May.	L. 4	4		1·0189
Limerick	July 15, 16.	S. 2	3	1·0062	
Dublin	— 22, 23.	—	3	1·0000	
Dublin	Oct. 4.	S. 2	1	1·0000	1·0197
Bangor	Sept. 21.	—	1	1·0059	1·0257
London	June1837,Oct.1838	S. 2	13		1·0000
Dublin	July, Aug. 1838.	—	5		1·0183

The following Table contains the resulting values of the intensity at each station, with the latitudes and longitudes of the stations.

Table LXXVI.

Station.	Lat.	Long.	Intensity.	Station.	Lat.	Long.	Intensity.
Dublin.........	53° 21′	6° 16′	1·0197	Ennis	52° 51′	8° 58′	1·0253
Limerick.......	52 40	8 35	1·0250	Cork	51 54	8 26	1·0189
Carlingford...	54 2	6 11	1·0366	Waterford ...	52 16	7 8	1·0162
Armagh	54 21	6 39	1·0242	Broadway ...	52 13	6 24	1·0173
Colerain	55 8	6 40	1·0194	Gorey	52 40	6 17	1·0129
Carn.	55 15	7 15	1·0351	Rathdrum ...	52 55	6 14	1·0140
Strabane	54 49	7 28	1·0299	Ballybunian..	52 30	9 41	1·0335
Markree	54 12	8 26	1·0290	Valentia	51 56	10 17	1·0294
Ballina.........	54 7	9 7	1·0276	Dingle..........	52 8	10 17	1·0343
Belmullet.....	54 13	9 57	1·0292	Kiltanon......	52 52	8 43	1·0282
Achill	53 56	9 52	1·0295	Youghal	51 57	7 50	1·0219
Galway........	53 17	9 4	1·0285	Bangor........	54 39	5 42	1·0257

Of the foregoing results, those obtained at Carlingford and Colerain are not included in the deduction of the isodynamic lines, on the grounds already stated. To all the others equal weights have been assigned; the local error bearing so large a proportion to the error of observation, that the resulting probable error is but slightly diminished by the multiplication of the observations.

The following are the results of the calculation:

$$L = 1 \cdot 0252,\ M = + \cdot 000095,\ N = + \cdot 000058;$$
$$u = - 31^\circ\ 20',\ r = \cdot 000111;$$

the central station being the same as before.

Captain Ross's observations of intensity (according to the statical method) were made in the autumn of the year 1838, with two needles designated as R L 3 and R L 4. They are contained in the following Table.

TABLE LXXVII.

	Station.	Date.	Hour.	Temp.	Angle.
			h m	°	° ′
Needle R L 4.	London	July 10 1838	5 0	68	− 13 33·2
		— 12	4 0	72	− 13 32·7
		— 12	5 0	72	− 13 30·7
		Mean...	4 40	70·7	− 13 32·2
	Waterford	Oct. 4	11 0	57	− 9 42·2
		— 4	12 0	57	− 9 39·0
		Mean...	11 30	57·0	− 9 40·6
	Cork	Oct. 6	4 20	58	− 9 22·9
		— 6	5 40	56	− 9 24·9
		— 7	0 15	60	− 9 36·0
		— 7	1 35	60	− 9 39·1
		Mean...	2 58	58·5	− 9 30·7
	Valencia	Oct. 12.........	0 30	53	− 8 36·2
		— 12.........	1 20	53	− 8 21·0
		— 13.........	11 0	51	− 8 12·1
		— 13.........	0 0	51	− 8 13·8
		Mean...	0 12	52·0	− 8 20·8
	Killarney	Oct. 19.........	0 15	59	− 8 44·4
		— 19.........	2 0	59	− 8 44·0
		Mean...	1 8	59·0	− 8 44·2
	Limerick.........	Oct. 22.........	0 40	61	− 8 48·5
		— 22.........	2 0	61	− 8 51·4
		Mean...	1 20	61·0	− 8 50·0
	Dublin	Oct. 30.........	10 10	52	− 9 34·3
		— 30.........	11 0	52	− 9 33·7
		Mean...	10 35	52·0	− 9 34·0
	Londonderry ...	Nov. 6	0 45	49	− 5 27·6
		— 6	2 0	49	− 5 34·0
		Mean...	1 22	49·0	− 5 30·8
	London	Dec. 4	3 0	47	− 13 28·8
		— 4	4 30	47	− 13 28·0
		Mean...	3 45	47·0	− 13 28·4

	Station.	Date.	Hour.	Temp.	Angle.
Needle R L 3.			h m	°	° ′
	London	July 7.........	5 30	70	− 26 38·4
		— 12.........	0 30	70	− 27 1·9
		— 12.........	1 30	70	− 27 4·8
		Mean...	2 30	70·0	− 26 55·0
	Dublin...........	Oct. 30.........	11 30	52	− 24 27·3
		— 30.........	0 0	52	− 24 22·3
		Mean...	11 45	52·0	− 24 24·8
	Londonderry ...	Nov. 6.........	11 20	51	− 22 47·6
		— 6.........	0 40	51	− 22 49·8
		Mean...	0 0	51·0	− 22 48·7
	Westport.........	Nov. 14	0 40	45	− 22 19·7
		— 14	2 0	45	− 22 20·9
		Mean...	1 20	45·0	− 22 20·3
	London	Dec. 5	3 0	45	− 26 32·9
		— 5	3 40	45	− 26 30·3
		Mean...	3 20	45·0	− 26 31·6

With respect to these observations, Captain Ross observes: "The readings of R L 4 at Dublin, with the letters on the needle to the face of the instrument, gave 5° greater when facing the east, and 5° less when facing the west, than the mean of similar facings with the needle reversed on its axle. I therefore thought that the axle had got some bend, and was totally ruined; and accordingly used R L 3 always in future. But at Londonderry I had some spare time, and thought I would try and find out the cause of this error, for I was sure it had sustained an injury.

"At Londonderry the mean of the readings E. and W., with the letters to the face of the instrument, was $2\frac{1}{2}$ degrees less than the mean of similar readings with the needle reversed on its axle. I therefore believe that some considerable irregularity of the axle, about the point where the needle (with its letters to face of instrument) should rest at about $-7°$, has occasioned this error; and the circumstance of the Dublin observation coming out right, is merely accidental. In all other parts of the axle that I have tried, its readings agree very nearly with each other."

Under the circumstances above detailed, it seems necessary to reject the observations with R L 4 at Dublin and Londonderry.

The following Table contains the computed results of the foregoing observations, and the latitudes and longitudes of the stations. In making the computation, no correction for temperature has been applied to the results of R L 3; the logarithmic correction of R L 4 is ·000024.*

* See pages 157 and 158.

TABLE LXXVIII.

Station.	Lat.	Long.	Intensity.	Station.	Lat.	Long.	Intensity.
Waterford ...	52° 16′	7° 8′	1·0197	Limerick	52° 40′	8° 35′	1·0243
Cork...........	51 54	8 26	1·0211	Dublin.........	53 21	6 16	1·0186
Valentia	51 56	10 17	1·0272	Londonderry..	55 0	7 20	1·0301
Killarney......	52 3	9 31	1·0253	Westport	53 48	9 29	1·0329

In deducing the position of the isodynamic lines from these results, equal weights have been assigned to all, for the reason already given. The following are the results of the computation:

$$L = 1{\cdot}0256,\ M = +\ {\cdot}000091,\ N = +\ {\cdot}000067;$$
$$u = -\ 36°\ 29',\ r = {\cdot}000113.$$

The results which have been above obtained respecting the position of the isodynamic lines in Ireland, are combined in the following Table:

TABLE LXXIX.

Observers.	Method.	No. of Stations.	L.	M.	N.
Lloyd, Sabine, Ross..	Hor. Vibr. ...	20	1·0268	·000075	·000050
Lloyd, Sabine.........	Statical.........	22	1·0252	·000095	·000058
Ross	Hor. Vibr. ...	12	1·0276	·000086	·000067
Ross	Statical.........	8	1·0256	·000091	·000067

In deducing the mean values from the preceding results, we cannot, consistently with the character of the observations, assign to each a weight in proportion to the number of stations from which it is derived. If we compute the probable value of the intensity at each station, and compare it with that observed, we shall find that the differences are in general smaller in Captain Ross's observations than in those of the two earlier series; so that the individual results are entitled to a greater weight. This superiority is due, in great measure, to the circumstance that, in the latter series, all the observations were taken by the same observer, with the same instrument, and about the same time. On instituting a similar comparison between the results of the *two methods*, it will be found that, in Captain Ross's two series, the weight due to the results of the statical method is very nearly *double* of that in the method of vibration; the probable errors being, nearly, in the ratio of 1 to $\sqrt{2}$. The same disparity between the methods is not found in the results of the two earlier series, a fact which seems to be fully accounted for by the imperfection of the instrument used by me in the statical observations, the effect of the magnetism of the limb (page 106 *et seq.*) being in this case uncorrected.

The equations of condition afford the means of deducing the weights of the preceding results, on the supposition that there is no constant error. But as this cannot be supposed, we are left to a certain extent unguided. On the whole, we shall probably be not far from the truth in assigning *equal* weights to each of the former results, notwithstanding the disparity in the number of stations. The following are the mean values thus deduced:

$$L = 1 \cdot 0263, \quad M = + \cdot 000087, \quad N = + \cdot 000061.$$

Accordingly, the probable value of the intensity at the central station (lat. = 53° 21′, long. = 8° 0′) is

$$1 \cdot 0263.$$

And from the mean values of M and N we obtain, for the direction of the isodynamic line passing through that station,

$$u = - 35° \, 0';$$

and for the rate of increase of the intensity in the direction perpendicular to that line,

$$r = \cdot 000106.$$

In order to reduce the intensity results of the present survey to *absolute* measures, it is only necessary to determine the absolute intensity of the magnetic force at some one of the base stations, according to the method of Professor Gauss. This will be done, ere long, in Dublin; and it is therefore important that the ratio of the intensities in Dublin and London (with which latter station all the others are compared) should be accurately known.

For the determination of this ratio we have abundant materials in the present memoir. The ratio of the horizontal intensities in Dublin and London, as deduced from the first series, was found to be ·9399; the result being equivalent to the mean of eleven distinct comparisons. If we combine with this the result obtained by Captain James Ross, namely, ·9383, the mean value of the horizontal intensity in Dublin is found to be

$$\cdot 9398;$$

the horizontal intensity in London being unity. But the dip in London corresponding to the mean epoch of these observations (the 1st of January, 1837) is 69° 19′·6; and that in Dublin is 71° 1′·2; wherefore the total intensity in Dublin is

$$1 \cdot 0201,$$

the total intensity in London being unity.

Again, we have found that the intensity in Dublin, as deduced by the statical method from the observations made by Major Sabine and myself, is expressed by the number 1·0197, the intensity in London being unity. The value of this ratio obtained by Captain Ross in 1838 is 1·0186; and the former result being equivalent to the mean of six distinct comparisons, the final mean is 1·0195.

Of these results, deduced by the two methods, the difference is only ·0006; and we should therefore err very little from the truth in taking their arithmetical mean. But the probable error of a single comparison in the latter method is so much less than in the former, that we shall certainly be nearer to the truth in adopting the latter result. We shall accordingly consider the number 1·0195 as expressing the ratio of the intensities of the magnetic force in Dublin and London.

Report resumed by Major Sabine.

Collecting in one view the values of u and r resulting from the several series of intensity observations, we have as follows:

TABLE LXXX.

	Method.	Observer.	No. of Stations.	Mean Geog. Posit. Lat.	Long.	u.	r.
				° ′	° ′	° ′	
England	Statical ...	Lloyd	12	52 01	1 50	−54 49	·000082
	Statical ...	Phillips ...	24	53 49	2 08	−47 37	·000090
	Statical ...	Sabine	20	52 36	2 11	−52 27	·000079
	Hor. Vibr.	Ross ..... Sabine ... Lloyd ...	27	52 43	2 18	−47 14	·000094
		Mean...	74	52 48	2 07	−50 48	·000086
Scotland	Statical ...	Sabine	19	56 22	4 01	−52 15	·000136
	Statical ...	Ross	9	56 52	2 45	−40 38	·000106
	Hor. Vibr.	Sabine	21	56 30	4 10	−55 46	·000143
	Hor. Vibr.	Ross	12	56 56	2 58	−43 32	·000125
		Mean...	61	56 40	3 30	−50 02	·000132
Ireland	Statical ...	Lloyd .... Sabine ...	22	53 21	8 00	−31 20	·000111
	Statical ...	Ross	8	....	......	−36 29	·000113
	Hor. Vibr.	Lloyd ... Sabine ... Ross......	20	....	.. ...	−33 48	·000090
	Hor. Vibr.	Ross.........	12	....	......	−38 00	·000109
		Mean...	62	53 21	8 00	−34 06	·000104

The values of u in England and Scotland, or the angle which the isodynamic lines in those countries make with the meridian, appear to be very nearly the same; the difference in the mean values is much within the order of the differences of the partial results. But the values of r, or the rate of increase of the intensity corresponding to equal geographical spaces, differ considerably, and give a decided indication that the spaces between the isodynamic lines are less in Scotland than in England. If we examine the partial results obtained in the two countries by the different observers, and by the different methods of observation, we perceive that all the series are consistent in this indication. The lines which are selected for representation in the map are those of unity (passing through London), of 1·01, 1·02, and 1·03: the mean distance between the lines, which thus differ ·01 in the values of the intensity they represent, is in England 116, and in Scotland 75 geographical miles; the partial results vary in England from 106 to 126 miles, and in Scotland from 69 to 94 miles.

Whatever may be the cause of this difference in the value of r in the northern and southern portions of the island, it is obviously much too great to be taken as a regular part of a general progression; as in its extension towards the N.W. and S.E., the separation between the lines would in the one case be soon rendered extravagantly small, and in the other extravagantly great.

In order to deduce the position of the several isodynamic lines in best conformity with the observations, it is particularly necessary, under such circumstances, to derive each line from those observations only which are in its immediate vicinity; and thus to reduce within very small limits the effect on each of the rapidly-changing and somewhat uncertain values of r. We require, for this purpose, only its approximate values in the vicinities of the respective lines; and without entering into nice calculations where we have not a sufficiently satisfactory basis, we may provisionally assume these values as follows; always remembering, that any inaccuracy in the assumption will produce an opposite effect on the deductions from the observations which are on either side of each isodynamic line, and that such opposite effects will counterbalance each other in the mean position assigned to the line.

Approximate values of r in England and Scotland, in the vicinity of the several isodynamic lines:

Line of 1·0 ; $r =$ ·00008
. . . . 1·01; $r =$ ·00009
. . . . 1·02; $r =$ ·00011
. . . . 1·03; $r =$ ·000135

The mean value of r in Ireland, derived from the several series in that country, is ·000104 or ·000106, (page 185,) which corresponds so nearly with the value which might be interpolated from the results in England and Scotland for the latitude of the central geographical position in Ireland, that we may safely take ·00010 as a general value for the Irish deductions.

If we compare the mean value of u derived from the Irish series, $-34° 6'$ (varying in the several partial results from $-31° 20'$ to $-38° 00'$), with its mean values in England and Scotland $-50°$, (the partial results varying from $-40° 38'$ to $-55° 46'$), we find, notwithstanding the amount of the partial differences, a general and consistent indication that the isodynamic lines are less inclined to the meridian in Ireland than in Great Britain. The two Irish series which give the least values for this angle, are those which were the earliest obtained,—which had consequently the disadvantages of less experience in the observers, and less perfection in the instruments; and of combining in one series observations at different epochs, and results by different observers, and with different instruments. The two series of Captain Ross were, on the other hand, obtained by one observer with the same instruments; were well distributed over the country; and were made in immediate and rapid succession. We may therefore safely infer, as Mr. Lloyd has done (pages 184, 185), that the values of u derived from Captain Ross's series are entitled to weight beyond the proportion which the number of the stations which they represent bears to the number of stations in the other Irish series. Still the difference in the angle with the meridian in Ireland and in Great Britain cannot, in any consistency with the observations, be less than several degrees. I have employed $-35°$, the value deduced by Mr. Lloyd, pages 184 and 185, as the general mean value of u in the Irish deductions.

If we compare generally the mean results of the horizontal with those of the statical series, we are not able to discover any apparent systematic differences whatever in regard to the values of u and r. The individual observations by the horizontal method do indeed exhibit much greater discordances with each other than is the case in the statical method. This has been already shown in detail in the analysis of the observations by the two methods in Scotland, in pages 20, 21, of the Sixth Report of the British Association: and Mr. Lloyd has elsewhere pointed out the causes of the advantage in this respect of the method for which we are indebted to him. Although, therefore, the accordance of the two methods, when the observations are grouped, is a satisfactory confirmation of the conclusions which they unite in establishing, the horizontal observations are less fitted than the statical to be employed in a graphical representation of the particular nature adopted in this report, in which the discordances of individual observations are brought strongly into notice, and if exceeding a certain limit might produce inconvenience, by in some degree perplexing the judgment. In extreme cases they might entirely mislead it; as, for example, if the point furnished by an observation for a particular line should fall nearer to an adjacent line than to the one to which it really belongs; and this will occur whenever, from accidental causes of any kind, the discordance exceeds in amount half the interval between the lines which are represented. Such extreme cases are frequent in the horizontal observations; but are of very rare occurrence in the statical. Of the 114 statical results, there are only five which have been omitted in the graphical representation; (though of course included in the table). Four of these are, Ballybunian, Dingle, Gorey, and Rathdrum, all in the south of Ireland, and amongst our earliest observations. The two first named were my stations, and the intensity is in excess;—the two others were Mr. Lloyd's stations, and the intensity is in defect of the general body of the results; the omission of the four should consequently have no effect on the position of the lines.

The fifth observation omitted in the map is Captain Ross's at Berwick, which would furnish a point for the line of 1·03 in a geographical position which is nearer the line of 1·02.

The evidence supplied by the collective horizontal observations is, however, too valuable to be dispensed with in the representation. I have collected in the following Table the values of the intensity derived, for the respective mean geographical positions, from the combined observations of each series, both horizontal and statical. In the map the central stations are designated thus, +, with the initial of the observer annexed; and the points furnished by the respective intensities for the nearest adjacent line thus, ∗, with H or S[1], according as the series was horizontal or statical, and a figure is added expressing the number of stations contributing to the result. In Ireland the one central station has been taken by Mr. Lloyd as common to all the series, and the initials of the observer, therefore, are transferred to the points.

Table LXXXI.

Statical.				Horizontal.			
Observer.	Mean Geographical Position. Lat.	Mean Geographical Position. Long.	Intensity.	Observer.	Mean Geographical Position. Lat.	Mean Geographical Position. Long.	Intensity.
	° ′	° ′			° ′	° ′	
P	53 49	2 08	1·0136				
S	52 36	2 11	1·0075	R, S, L	52 43	2 18	1·0087
L	52 01	1 50	1·0048				
S	56 22	4 01	1·0290	S	56 30	4 10	1·0285
R	56 52	2 45	1·0277	R	56 56	2 58	1·0302
L, S	53 21	8 00	1·0252	L, S, R	53 21	8 00	1·0268
R	53 21	8 00	1·0256				
				R	53 21	8 00	1·0276

The General Table of the intensity results by the statical method is analogous to the General Table of the Dip observations: it appears, therefore, to require no separate explanation. The intensities which exceed 1·035 belong to the line of 1·04, of which no representation has been attempted, because the results on which it would rest are all, with a single exception, on one side of the line. The stations to which these results belong are, however, retained in the map, and are accompanied in each case by the numerical value of the observed intensity.

General Table.

Intensity. Statical Method.

OBSERVATIONS.					DEDUCTIONS.				
Station.	Lat.	Long.	Observer.	Intensity.	Δ Lat.	Δ Long.	Isodynamic Line of 1·03 in Lat.	Isodynamic Line of 1·03 in Long.	Values of u and r.
Lerwick	60 09	1 07	R	1·0358	These stations belong to the isodynamic line of 1·04, which is not drawn in the map.				
Kirkwall	59 00	2 58	R	1·0373					
Gordon Castle	57 37	3 09	S	1·0380					
Golspie	57 58	3 57	S	1·0360					
Inverness	57 28	4 11	S R	1·0382 1·0378					
Loch Slapin	57 14	6 02	S	1·0427					
Carn	55 15	7 15	L	1·0351					
Berwick	55 45	2 00	R	1·0254	+25	+39	56 10	2 39	u = − 50°; r = ·000135.
Aberdeen	57 09	2 05	R	1·0268	+18	+28	57 27	2 33	
Alford	57 13	2 45	S	1·0294	+ 4	+ 5	57 17	2 50	
Newport	56 25	2 55	S	1·0277	+13	+20	56 38	3 15	
Kirkaldy	56 07	3 09	S	1·0279	+11	+17	56 18	3 26	
Blairgowrie	56 36	3 18	S	1·0310	− 6	− 9	56 30	3 09	
Braemar	57 01	3 25	S	1·0269	+17	+26	57 18	3 51	
Dunkeld	56 35	3 33	R	1·0267	+19	+28	56 54	4 01	
Helensburgh	56 00	4 41	S	1·0258	+23	+36	56 23	5 17	
Cumbray	55 48	4 52	S	1·0287	+ 8	+12	55 56	5 04	
Glencoe	56 39	5 07	S	1·0324	−13	−20	56 26	4 47	
Loch Ranza	55 42	5 17	S	1·0261	+22	+33	56 04	5 50	
Campbelton	55 23	5 38	S	1·0296	+ 2	+ 3	55 25	5 41	
Bangor	54 40	5 40	S	1·0257	+25	+57	55 05	6 37	u = − 35°; r = ·00010.
Londonderry	54 59	7 19	R	1·0301	− 1	− 1	54 58	7 18	
Strabane	54 49	7 28	L	1·0299	+ 1	+ 1	54 50	7 29	
Markree	54 12	8 26	L	1·0290	+ 6	+13	54 18	8 39	
Kiltanon	52 52	8 43	S	1·0282	+10	+23	53 02	9 06	
Ennis	52 51	8 57	L	1·0253	+28	+64	53 19	10 01	
Galway	53 17	9 04	L	1·0285	+ 9	+20	53 26	9 24	
Ballina	54 07	9 07	L	1·0276	+13	+32	54 20	9 39	
Westport	53 48	9 29	R	1·0329	−17	−39	53 31	8 50	
Killarney	52 02	9 30	R	1·0253	+28	+64	52 30	10 34	
Ballybunian	52 30	9 41	S	1·0335	−21	−48	52 09	8 53	
Belmullet	54 13	9 57	L	1·0292	+ 5	+11	54 18	10 08	
Achill	53 56	9 52	L	1·0295	+ 3	+ 7	53 59	9 59	
Valencia	51 56	10 17	S R	1·0294 1·0272	+ 9	+22	52 05	10 39	
Dingle	52 08	10 17	S	1·0343	−25	−58	51 43	9 19	

General Table—(*continued*).

OBSERVATIONS.					DEDUCTIONS.				
Station.	Lat.	Long.	Observer.	Intensity.	Δ Lat.	Δ Long.	Isodynamic line of 1·02 in Lat.	Isodynamic line of 1·02 in Long.	Values of u and r.
Thirsk	54° 14′	1° 21′	P	1·0155	+31′	+45′	54° 45′	2° 06′	$u = -50°$; $r = ·000110$.
Newcastle	54 58	1 37	P R S	1·0173 1·0165 1·0147	+26	+38	55 24	2 15	
Alnwick Castle	55 25	1 42	S	1·0159	+28	+41	55 53	2 23	
Dryburgh	55 34	2 39	S	1·0199	+ 1	+ 1	55 35	2 40	
Melrose	55 35	2 44	S	1·0208	− 6	− 8	55 29	2 36	
Stonehouse	54 55	2 44	R S	1·0173 1·0176	+17	+25	55 12	3 09	
Penrith	54 40	2 45	P	1·0184	+11	+16	54 51	3 01	
Carlisle	54 54	2 54	P	1·0198	+ 1	+ 2	54 55	2 56	
Bowness	54 22	2 55	P	1·0182	+13	+18	54 35	3 13	
Patterdale	54 32	2 56	P	1·0181	+14	+19	54 46	3 15	
Coniston	54 22	3 05	P	1·0196	+ 3	+ 4	54 25	3 09	
Edinburgh	55 57	3 11	S	1·0231	−22	−31	55 35	2 40	
Whitehaven	54 33	3 33	S	1·0176	+16	+24	54 49	3 57	
Glasgow	55 51	4 14	S	1·0232	−23	−32	55 28	3 42	
Jordan Hill	55 54	4 21	R S	1·0236 1·0259	−34	−48	55 20	3 33	
Douglas	54 10	4 27	P	1·0208	− 6	− 8	54 04	4 19	
Castleton	54 04	4 40	P	1·0203	− 2	− 3	54 02	4 37	
Peelton	54 13	4 43	P	1·0192	+ 6	+ 8	54 19	4 51	
Loch Ryan	54 55	4 58	S	1·0221	−15	−21	54 40	4 37	
Dublin	53 21	6 16	L R S	1·0195	+ 3	+ 7	53 24	6 23	$u = -35°$; $r = ·00010$.
Broadway	52 13	6 24	L	1·0173	+15	+36	52 28	7 00	
Armagh	54 21	6 39	L	1·0242	−24	−57	53 55	5 42	
Waterford	52 16	7 08	L R	1·0162 1·0197	+12	+26	52 28	7 34	
Youghal	51 57	7 50	S	1·0219	−11	−25	51 46	7 25	
Cork	51 54	8 26	L R	1·0189 1·0211	0	0	51 54	8 26	
Limerick	52 40	8 36	S R	1·0250 1·0243	−27	−63	52 13	7 33	

General Table—(*continued*).

OBSERVATIONS.					DEDUCTIONS.				
Station.	Lat.	Long.	Observer.	Intensity.	Δ Lat.	Δ Long.	Isodynamic line of 1·01 in Lat.	Isodynamic line of 1·01 in Long.	Values of u and r.
Flamborough	54 08	0 08	P	1·0083	+14	+19	54 22	0 27	u = − 50°; r = ·000090.
Scarborough	54 17	0 24	P	1·0103	− 3	− 4	54 14	0 20	
Whitby	54 29	0 37	P	1·0135	−29	−43	54 00	−0 04	
York	53 58	1 05	P	1·0126	−22	−31	53 36	0 34	
Doncaster	53 31	1 07	P	1·0096	+ 3	+ 5	53 34	0 12	
Hambleton	54 20	1 15	P	1·0134	−28	−41	53 52	0 34	
Osmotherley	54 22	1 18	P	1·0128	−24	−34	53 58	0 44	
Sheffield	53 22	1 31	P	1·0124	−20	−28	53 02	1 03	
Birmingham	52 28	1 53	P	1·0105	− 4	− 6	52 24	1 47	
Shrewsbury	52 43	2 45	L S	1·0077 1·0057	+27	+38	53 10	3 23	
Calderstone	53 23	2 53	P	1·0106	− 5	− 7	53 18	2 46	
Birkenhead	53 24	3 00	L P	1·0112 1·0145	−24	−33	53 00	2 27	
Coed	53 11	3 12	P	1·0110	− 8	−12	53 03	3 00	
Brecon	51 57	3 21	S	1·0060	+35	+46	52 32	4 07	
Merthyr	51 43	3 21	S	1·0081	+17	+23	52 00	3 44	
Dunraven Castle	51 28	3 37	S	1·0078	+20	+26	51 48	4 03	
Aberysthwith	52 24	4 05	S	1·0100	0	0	52 24	4 05	
Holyhead	53 19	4 37	L	1·0144	−38	−53	52 41	3 44	
Rathdrum	52 55	6 12	L	1·0140	−34	−48	52 21	5 24	
Gorey	52 41	6 15	L	1·0129	−25	−34	52 16	5 41	

Station.	Lat.	Long.	Observer.	Intensity.	Δ Lat.	Δ Long.	Isodynamic line of 1·00 in Lat.	Isodynamic line of 1·00 in Long.	Values of u and r.
Margate	51 23	−1 23	S	0·9970	+29	+39	51 52	−0 44	u = − 50°; r = ·000080.
Dover	51 08	−1 19	S	0·9945	+53	+70	52 01	−0 09	
Lynn	52 47	−0 25	L	1·0030	−29	−39	52 18	−1 04	
Eastbourne	50 47	−0 16	F	0·9937	+51	+68	51 38	0 52	
Cambridge	52 13	−0 07	L	1·0001	− 1	− 1	52 12	−0 08	
Brighton	50 50	0 08	L	0·9955	+44	+58	51 34	1 06	
Worcester Park	51 23	0 17	S	1·0006	− 6	− 7	51 17	0 10	
Eastwick Park	51 17	0 19	F	0·9993	+ 7	+ 8	51 24	0 27	
Tortington	50 50	0 34	S	0·9990	+10	+13	51 00	0 47	
St. Clair's	50 44	1 08	P	1·0002	− 2	− 2	50 42	1 06	
Ryde	50 44	1 10	L	0·9972	+27	+35	51 11	1 45	
Salisbury	51 04	1 47	L	1·0006	− 6	− 7	50 58	1 40	
Combe House	51 31	2 34	F	1·0026	−26	−34	51 05	2 00	
Clifton	51 27	2 36	L	1·0030	−29	−39	50 58	1 57	
Chepstow	51 38	2 41	L	1·0041	−40	−53	50 58	1 48	
Hereford	52 04	2 44	L	1·0046	−44	−59	51 20	1 45	
Lew Trenchard	50 40	4 10	S	1·0045	−43	−56	49 57	3 14	
Falmouth	50 09	5 06	F S	1·0018 1·0015	−17	−22	49 52	4 44	

Extension of the Isoclinal and Isodynamic Lines into Meridians East and West of the British Islands.

Having thus completed the representation of the principal lines of dip and intensity passing across the British Islands, it appears desirable to trace their prolongation on either side, until they are brought in connexion with the lines of the same value in adjacent meridians to the east and west, as determined by recent and satisfactory observations. As a single line of each of the phenomena will suffice to exhibit this connexion, I have selected for that purpose the isoclinal line of 70°, and the isodynamic line of 1·03.

In Plate III. the portion of the isoclinal line, which is represented by an unbroken line, has been determined by the observations contained in this report. In its eastern prolongation it passes through countries where its position is well assured by observations of higher amount on the one side, and of lower amount on the other, too numerous for insertion in a map on so small a scale, and too well known to need a recapitulation here. Towards the north-eastern extremity of the map, the position of Gros Novgorod is marked in lat. 58° 31′ and long. 31° 19′, where M. Erman observed the dip 70° 26′·1 on the 13th of July, 1828. This observation, reduced to January 1837, by allowing an annual diminution of 3′, becomes 70° 00′·6: the line of 70° is therefore made to pass through this station. To the west of the British Islands, the line is prolonged until it is brought in connexion with M. Erman's observations on his homeward passage, in August 1830. For this purpose I have formed M. Erman's observations into two groups, each of three stations, as follows:

1830.	Lat.	Long.	Dip.	
	° ′	° ′	° ′	° ′
Aug. 19	41 27	327 25	70 03·6	70 19·4
— 20	42 29	328 34	69 47·6	
— 21	44 22	330 55	71 07·1	
— 22	46 46	335 42	70 18·5	70 06·3
— 24	47 47	343 58	69 46·0	
— 25	47 46	344 25	70 14·9	

Allowing, as in Britain, an annual decrease of 2′·4, the dips in January 1837, corresponding to the mean positions of these groups, are as follows:

Lat.	Long.	Dip.
42° 46′	328° 58′	70° 04′
47 26	341 22	69 51

These positions are marked in the Map, and the isoclinal line of 70° is prolonged to the westward in correspondence with the mean of M. Erman's observations thus corrected for epoch.

To connect the isodynamic line of 1·03 with intensities of the same value in the adjacent meridians, it is necessary to express the value of this line in terms of the arbitrary scale employed by Continental observers, in which the force in London = 1·372. In this scale the line of 1·03 corresponds in value to (1·03 × 1·372 =) 1·413. The portion of this line which is represented in the Map by an unbroken line has been determined by the observations contained in this report. Its prolongation to the eastward is traced in conformity with M. Hansteen's observations in Norway, and with MM. Hansteen's and Erman's in Russia. The station marked in lat. 60° 11′ and long. 10° 20′ is the mean geographical position of a group of six stations in Norway, not far removed from each other, for which M. Hansteen's observations in 1821, 1823, and 1825, gave a mean intensity of 1·414 (7th Report, British Association, page 49). At Gros Novgorod (lat. 58° 31′, long. 31° 19′) the determinations of MM. Hansteen and Erman accorded in assigning 1·412 as the value of the force (7th Report, British Association, page 51), and the line has been still further extended, in conformity with the observations of the same gentlemen at Moscow, in lat. 55° 46′, and long. 37° 36′, their mean determination being 1·405. The position of the line in its western prolongation has been drawn in conformity with the values of the intensity at the islands of Terceira and Madeira, contained in the general table of the memoir on the magnetic intensity already referred to, viz.

Terceira	Fitz Roy	1836	1·457	
Madeira	Sabine	1822	1·373	1·375.
	King	1826	1·377	

Both stations are included in the Map. The values of the force at M. Erman's dip stations in the same quarter, determined by the same excellent observer, are also inserted in the map, as affording corroborative evidence of the correct position of the isodynamic line in this its western extension.

In order to render the view in this Map of the magnetic phenomena in the British Islands more complete, I have added the direction, shown by arrows, of the horizontal or compass needle at three extreme stations, determined by Captain James Clark Ross, viz. Lerwick, in the Shetland Islands; Valencia, at the S.W. extremity of Ireland; and Bushey, near London. The geographical positions of these stations, and the variations observed at them, are as follows, the latter being the *mean* variation at the epoch named, obtained by observations repeated every fifteen minutes from 7 A.M. to 7 P.M. for several successive days.

Station.	Date.	Lat.	Long.	Variation.
Lerwick .	July 26, 1838	60° 09′	1° 07′ W.	27° 08′ 35″ W.
Valencia .	Oct. 13, —	51 56	10 17 W.	28 41 52 W.
Bushey .	April 3, —	51 38	0 22 W.	23 59 24 W.

REPORT ON THE MAGNETIC ISOCLINAL AND ISODYNAMIC LINES IN THE BRITISH ISLANDS.

TABLE OF CONTENTS.

First Report on the Determination of the Mean Numerical Values of Railway Constants. *By* Dionysius Lardner, LL.D. F.R.S., *&c.*

It will be in the recollection of the Members of the Mechanical Section of the British Association, that the circumstance out of which this inquiry arose, was the discordance of opinion which prevailed among the members of the Section, including several engineers and other practical men, on the subject of the amount of the resistance to the tractive power offered by trains on railways; this resistance being variously estimated at six, seven, eight, nine, and even ten or twelve pounds per ton of the gross load.

The resistance to the motion of a train of wagons or coaches on a level and straight line of rails arises from the following causes:

1°. The friction of the axles with their bearings.

2°. The resistance to the rolling motion of the tires on the rails.

3°. The friction of the flanges with the rails brought into occasional contact with the latter by the lateral oscillation of the carriages.

4°. The resistance of the air.

If the line of rails be curved, another source of resistance arises from the pressure and consequent friction of the flanges of the outer wheels on the rails, which combined with the effects of the conical form of the tires, is in fact the force by which the direction of the motion of the train is continually changed.

If the line be inclined at any given angle to the horizon, the resistance will be modified by the gravitation of the load in a manner which is easily inferred from the elementary principles of mechanics.

The practical importance of ascertaining the proportion in which the whole resistance is distributed among these several sources is evident. It is only by determining this that the engineer can be guided in the selection of means for reducing that resistance; and the importance of reducing it will be understood, when it is considered how large an item in the expenditure of railway companies is locomotive power, and that the amount of this power is, *ceteris paribus*, in the exact proportion of the resistance of the loads which it draws.

The first question to which the present inquiry has been directed was, to determine what the *total* resistance which is produced on a straight and level railway by the combination of all the above-mentioned causes. It is a matter of regret that the obstacles to experiment which are produced by the great amount of traffic on the principal railways are such that, notwithstanding the lapse of time which has taken place since the commencement of this inquiry, means have not been obtained for making such an extensive and various course of experiments as would be sufficient to solve this question. Besides the obstacles produced by the traffic on the different lines of railway, difficulties also arose in obtaining the means of experimenting, owing in some cases to the inability of railway companies to spare the necessary engines, carriages, and wagons. It is nevertheless due to these companies to state their general willingness to facilitate the investigation, and this acknowledgement is especially due to the Boards of Directors of the Grand Junction and the Liverpool and Manchester Railway Companies.

Three methods for discovering the amount of resistance opposed by a train to the tractive power have been proposed:

1°. By a dynamometer, interposed between the tractive power and the load, which should measure and record the force exerted by the tractive power in drawing the load along a level and straight line of railway.

2°. By observing the motion of a load down an inclined plane sufficiently steep to give it accelerated motion, and comparing the rate of its acceleration with that which it ought to receive from gravity, if it were subject to no resistance.

3°. By putting a load in motion on a straight and level line of railway, so as to impart to it a certain known velocity, and then permitting it to run until it is brought to rest by the resistance gradually destroying the velocity imparted to it.

Each of these methods of experimenting was attended with difficulties and objections. In practice, a line of rails is never truly level. That which is commonly called level is a line which, being examined from point to point at intervals—say of quarter miles, is found neither to rise nor fall upon the whole. But the surface of the rails along the intermediate parts is subject to considerable departures from an uniform level. However accurately they may have been laid when the line is first constructed, the traffic upon them soon impairs their evenness, and the inequalities of level become so considerable, that it frequently happens that a wagon will not rest in certain positions upon them, but will roll until its wheels get at the lowest point of a part of the rail which has sunk. In the use of any form of a dynamometer this circumstance produces extreme variations in the index, so much so that in most of those which have been tried, the index oscillates between zero and the extreme limit of its play.

Besides this difficulty, which, from the nature of the resistance, would appear to be inseparable from every form of dynamometer, another will arise if it be admitted that the atmosphere have any considerable share in producing the resistance which the tractive power has to overcome. The dynamometer must be interposed between the engine and tender, or between the latter and the first coach or wagon in the train; or, to speak more generally, it must be immediately before the coach or wagon whose resistance it is used to measure, and must be *behind* the engine, tender, or carriage which precedes that load. It is evident that, under such circumstances, the atmospheric resistance will produce only a modified and partial effect on the dynamometer; nor will this instrument, under such circumstances, exhibit a true estimate of the resistance arising from friction alone, independently of the atmosphere, since the effect of the atmosphere is only partially intercepted by the preceding part of the train.

The only manner in which the dynamometer could be used with any prospect of obtaining a tolerably correct and satisfactory result, would be to construct it in such a manner as to register its own indications, by describing a curve on paper with a pencil moved by the index of the instrument, so that the ordinate of this curve would represent the resistance, and the corresponding abscissa the point of the road where that resistance was produced. If the instrument thus constructed were applied with so very slow a motion as to render the atmospheric resistance so small that it might be practically disregarded, then the mean value of the ordinate of the curve, or, what is the same, the area of the curve divided by its abscissa, would express the mean amount of the resistance.

The second and third methods of experimenting are those which have been hitherto generally used for the practical determination of the resistance of railway trains to the tractive power. The combination of both has been resorted to by M. de Pambour in the following manner:—He placed the carriages whose resistance was to be determined upon a steep inclined plane, having a line nearly level at its foot, and allowing them to move by gravitation from a state of rest, they attained a certain velocity at the foot of the plane; with this velocity the carriages moved along the level until they were reduced to a state of rest. It was then assumed that the resistance was represented by the ratio of the difference of absolute levels of the point from which they started and the point at which they stopped, to the distance, measured along the rails, between the same points. This method would be unobjectionable if the resistance was, as M. de Pambour and most others at that time supposed it to be, independent of the velocity. But we shall show presently that, so far from this being the case, it has a dependence on the velocity which renders this method of experimenting altogether fallacious.

The following method of experimenting, with a view to the determination of the amount of the resistance due to friction, occurred to the reporter as being subject to fewer objections than any of the methods above mentioned.

Let two inclined planes of different acclivities be selected. Let $h =$ the gradient of the steeper plane, expressed by the sine of its inclination, or the numerical ratio of its height to its length.

Let $h' =$ the gradient of the other plane, similarly expressed. Let $L =$ a load which an engine, with an observed pressure of steam in the boiler, and the regulator open to an observed point, is capable of moving up the steeper plane at a slow uniform rate; and let $L' =$ the load which the same engine, in precisely the same state, is capable of moving up the other plane at a slow uniform rate.

The resistance on each plane will be the sum of the gravity of the load down the plane and the friction. Now if F represent the friction on the former, and F' on the latter, the resistance on the former will be

$$L\,h + F,$$

and the resistance on the latter will be

$$L'\,h' + F'.$$

Since these two resistances are balanced respectively by the tractive power of the same engine in the same state, they must be equal. Hence we have

$$L\,h + F = L'\,h' + F'$$

$$\therefore F' - F = L\,h - L'\,h'$$

$$\therefore \frac{F' - F}{L' - L} = \frac{L\,h - L'h}{L' - L}.$$

But $F' - F$ being the difference between the friction of L' and L, the first member of this equality will be the ratio of the friction to the load. If this be expressed by f, we shall therefore have

$$f = \frac{L\,h - L'h'}{L' - L} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

To reduce this to experiment it is only necessary to attach to an engine a train of loaded wagons, and so to adjust the load and the engine that the latter shall be just capable of drawing the former up the less steep plane with a slow uniform motion. Let it then be taken to the steeper plane, and let such a number of wagons be detached as will enable the engine, all things being as before, to draw the remainder slowly and uniformly up the steeper plane. If then for L' in (1) be substituted the former load, for L the latter, and for h and h' the gradients of the two planes, the numerical value of f will be obtained by the formula (1), and this will be the ratio of the friction to the load for the wagons or carriages, which were detached to enable the engine to draw the load up the steeper plane.

It is evident that this experiment may be varied by altering the tractive power of the engine, which may be done within practical limits, by varying the pressure of steam in the boiler, and the extent to which the regulator is opened. This will produce a corresponding variety in the values of L and L', and in this way various experiments may be made on the same pair of planes.

In this mode of experimenting it is not necessary that the actual pressure of steam on the pistons be known. All that is indispensable is, that on both planes the tractive power of the engine be the same.

The equality of the tractive power would be more satisfactorily insured if the pressure of steam in the cylinders could be measured and recorded, but no means have yet been contrived for accomplishing this in locomotive engines. The pressure of steam in the cylinders, however, depends on, 1°, the pressure of steam in the boiler; 2°, the extent of the opening of the regulator or steam valve; 3°, on the velocity of the piston. These will be the same, if in both cases the motion of the engine be slow and uniform, the regulator be equally open, and the steam-guage show the same pressure.

The limits of error depend on the practicability of trimming the load so as to accommodate it to the same tractive power of the engine. If the train on the less steep plane consist of a great number of wagons, this may be done very nearly by casting off a certain number of them on the steeper plane; but, if necessary, the load of the wagons remaining may be trimmed by the addition or subtraction of weights.

On the Grand Junction Railway, between Madeley and Crewe, there is a succession of three planes which are well adapted for this method of experimenting: proceeding from Crewe, the first ascends at the mean inclination of 1 in 330, the second at 1 in 260, and the third 1 in 178.

As it is necessary to observe with some precision the pressure of steam in the boiler, during experiments made according to this method, the writer of this report constructed a self registering steam-guage for the purpose.

A smaller cylinder in which a piston is accurately fitted, similar to the cylinder and piston of common "indicators," is let into the boiler at a place near the position of the engineer. The piston-rod is carried through a tube outside the boiler, in which it is made to act on a spiral spring, the force of which is opposed to the motion of the piston, when driven upwards by the pressure of the steam. The position of the piston being determined by this spring becomes an indication of the pressure of steam, and so far the instrument is a mere steam-guage.

Attached to a part of the piston-rod is a pencil, the point of which is lightly pressed against the surface of a drum or cylinder which stands over the boiler and near the steam-guage. This drum is covered with paper rolled repeatedly round it, and gradually discharged from it to a small roller placed beside it, and pressed by a spring against it. On the axis of the drum is fixed a worm-wheel, which is driven by an endless screw. The latter receives its motion from a ratchet-wheel, in which a claw or catch acts. This claw is alternately raised and drawn down by some part of the machinery which has a reciprocating motion, so that for each stroke of either piston the ratchet-wheel is pulled through a space equal to one, two, three, or more of its teeth, according to adjustments which are provided in the apparatus. In this manner the drum receives a slow motion of rotation, bearing a known relation to the revolution of the driving-wheel, and therefore to the speed of the engine. By such means the drum may be made to revolve once in a quarter of a mile, or any other given distance.

If the pressure of steam in the boiler remain unvaried, the pencil will continue in the same position, and the paper moving under it will receive the mark of a straight and horizontal line at a certain height, which, by a scale previously adjusted, will express the pressure of steam in lbs. per square inch. If, however, the pressure of the steam vary, the pencil will have a corresponding variation of height, and a curve will be traced the ordinate of which will express the pressure; and since the absciss will represent the motion of the pistons, it will represent according to a known scale the motion of the engine along the road, and therefore the absciss corresponding to any ordinate will register the exact part of the road where the pressure of the steam was expressed by that ordinate.

The method of investigating the amount of resistance from friction above explained is attended with the further advantage, that the result is very slightly, perhaps insensibly, affected by the resistance of air. The wagons whose friction is here observed, being those thrown off in passing from the less to the more steep plane, are preceded by others, before which the air is driven. Besides, the motion being slow, the resistance of the air to the motion of the wheels must be quite insensible, and the motion on both plains being at nearly the same rate, the same resistance, or nearly so, from the air is encountered. For all these reasons the quantity $F - F'$ in the formula (1.) may be taken to represent the actual resistance from friction of the wagons detached.

In the course of the limited number of experiments which this Committee have been enabled to make, however, they have not yet obtained an opportunity of instituting any by this method, the provisions for which are not easily obtained in the midst of the busy traffic constantly carried on upon the railways; and this difficulty has been increased by the circumstance that there are very few gradients on railways which fulfil the conditions here required, and these few not always accessible.

The method of determining the resistance by observing the accelerated motion of carriages down inclined planes, and by observing the gradual retardation of their motion on a line where the inclination is not such as to render gravity greater than the friction, next demands attention.

An extensive series of experiments having been formerly made by M. de Pambour by this method, it will be convenient, in the first instance, to notice the principles adopted by him and the chief results at which he arrived.

Let g = the accelerating force of gravity.
θ = the angle which the plane makes with the horizon.
ϕ = the accelerating force of the load moving down the plane.
T = the time of the motion counted from the moment at which the load commences to move by gravity from a state of rest.
V = the velocity it has acquired in the time T.

Then we shall have $\phi = \dfrac{dV}{dT}$.

M. de Pambour then infers that if the load which descends the plane were free from friction, we should have

$$g \sin \theta = \frac{dV}{dT} \quad \ldots\ldots\ldots\ldots \quad (2.)$$

and that if x express the space moved over in the time T,

$$V = \frac{d\,x}{d\,T} \therefore V\,d\,V = g \sin\theta\,d\,x,$$

which being integrated, supposing that when $x = o$, $V = o$, gives

$$V^2 = 2\,g\,x \sin\theta.$$

But if the load be subject, as it always is in practice, to friction, then let the retarding force of friction be f, and the above equation will become

$$V^2 = 2\,(g \sin\theta - f)\,x\,;$$

and if the load descend a succession of planes of different gradients, passing from one to the other without any shock by which it will lose velocity, let x', x'', &c. represent the spaces over which it moves on each plane. Its motion will be then represented by the equation,

$$V^2 = 2\,(g \sin\theta - f)\,x + 2\,(g \sin\theta' - f)\,x' + 2\,(g \sin\theta'' - f'')\,x'' + \text{\&c.},$$

or, $$V^2 = 2\,\Sigma\,\{(g \sin\theta - f)\,x\} \quad \ldots\ldots\ldots \quad (3.)$$

Such is the equation obtained by M. de Pambour for the motion of a train down one or more inclined planes.

But this is manifestly erroneous and does not really express that which it professes to express:

1st. Because the condition (2.), from which all the others are deduced, would be only true on the supposition that all the particles of the load moved in lines parallel to the inclined plane with a common velocity V, which in fact is not the case, since the wheels and axles of the wagons or carriages have a motion compounded of a progressive and rotatory motion; and the mass of these bears a considerable proportion to the whole weight of the load.

2nd. Admitting that the error just mentioned were corrected, it is assumed that the excess of the gravity down the plane over the resistance opposed to the motion is independent of the velocity. Now, if any resistance be produced by the air, that resistance will increase, according to some law, with the velocity. It is therefore implicitly assumed in the reasoning of M. de Pambour, either that the resistance of the air in his experiments, or any other resistance depending on the velocity, is so inconsiderable that it may be disregarded, or that even at the greatest velocity it bears so small a ratio to the friction, that it may be confounded with the friction, and that the result will exhibit the mean resistance with sufficient accuracy for practical purposes.

Let us, in the first place, see to what extent the error arising from the omission of the consideration of the wheels operated on the result of M. de Pambour's experiments. To accomplish this we must obtain the correct solution of the problem of a train of wheeled carriages moving down an inclined plane, subject only to a resistance which is independent of the velocity, that being the condition on which M. de Pambour's investigation proceeds.

Let $M =$ the gross load in tons.

$g =$ the velocity produced by gravity in a falling body in one second.

$f =$ the ratio of friction to gravity.

$\because fg =$ the velocity destroyed by friction in one second.

Let $h =$ the gradient or the ratio of the height of the plane to its length.

$\because g\,h =$ the velocity which would be imparted to a body in one second moving down the plane without the friction.

$\because g\,(h-f) =$ the velocity which would be imparted to a body descending the plane by the excess of the gravity over friction.

Let $T =$ the time in seconds.

$\because M g\,(h-f)\,d\,T =$ the moving force which would be imparted to the descending load in the time $d\,T$.

Let $V' =$ the velocity of the train when started down the plane in feet per second.

$V =$ its velocity after T seconds.

$\because d\,V =$ the velocity it acquires in $d\,T$.

Let $m =$ the weight of a pair of wheels and their axles.

$d\,m =$ a particle of this mass.

$z =$ the distance of that particle from the centre of the wheel.

$r =$ the semi-diameter of the wheel.

$\omega =$ its angular velocity round its centre.

$\because z\,\omega =$ the linear velocity of $d\,m$.

$z\,d\,\omega =$ the increment of its velocity in $d\,T$.

$z\,d\,\omega\,d\,m =$ the increment of its moving force in $d\,T$.

$\dfrac{z^2\,d\,\omega\,d\,m}{r} =$ this increment reduced to the point of contact of the wheel with the rail.

$\because \displaystyle\int \frac{z^2\,d\,\omega\,d\,m}{r} =$ the increment of moving force received by the entire mass of the wheels and axle in the time T; and this being applied to each pair of wheels in the train,

$\Sigma\left(\int \frac{z^2\, d\omega\, dm}{r}\right)$ = the increment of moving force received by the mass of all the wheels and axles.

But since $r\, d\omega = d\mathrm{V}$, if $d\omega$ be eliminated we have

$$\Sigma\left(\int \frac{z^2\, d\omega\, dm}{r}\right) = d\mathrm{V}\,\Sigma\left(\int \frac{z^2\, dm}{r^2}\right).$$

By the principle of D'Alembert the moving forces which act upon the train must be in equilibrium with the moving forces received by it. Therefore the forces $\mathrm{M}g\,(h-f)\,d\mathrm{T}$ must fulfil the conditions of equilibrium with $\mathrm{M}\,d\mathrm{V}$, the progressive momentum of the whole train, and $d\mathrm{V}\,\Sigma\left(\int \frac{z^2\, dm}{r^2}\right)$ the revolving momentum of all the wheels and axles.

Hence we have

$$\mathrm{M}g\,(h-f)\,d\mathrm{T} - \left\{\mathrm{M} + \Sigma\left(\int \frac{z^2\, dm}{r^2}\right)\right\} d\mathrm{V} = 0,$$

which being integrated gives

$$\mathrm{M}g\,(h-f)\,\mathrm{T} = \left\{\mathrm{M} + \Sigma\left(\int \frac{z^2\, dm}{r^2}\right)\right\}(\mathrm{V}-\mathrm{V}') \ldots (4.)$$

The quantity $\int z^2\, dm$ being the moment of inertia of the wheels round their centres is equal to $m\,k^2$, where k is the distance of the principal centre of gyration from the centre of gravity; and this quantity $m\,k^2$ may be determined by observing the vibration of the wheels on any point of suspension, and thence determining the corresponding centre of oscillation.

Let d = the distance of the point of suspension from the centre of gravity.

l = the distance of the centre of oscillation from the point of suspension.

Then by known principles we have

$$d\,(l-d) = k^2,$$

and hence $m\,k^2$ may be found for each pair of wheels.

We shall therefore consider the quantity

$$\Sigma\left(\int \frac{z^2\, dm}{r^2}\right)$$

as thus determined, and for brevity shall call it M′, so that the equation (4.) shall be reduced to the form

$$\mathrm{M}g\,(h-f)\,\mathrm{T} = (\mathrm{M}+\mathrm{M}')\,(\mathrm{V}-\mathrm{V}') \quad . \quad . \quad . \quad (5.)$$

If S express the space over which the train moves in the time T, then $\mathrm{V}\,d\mathrm{T} = d\mathrm{S}$, and we obtain the relation between V and

S by eliminating T. Hence we have

$$2\,Mg\,(h-f)\,dS - 2\,(M + M')\,V\,dV = 0.$$

$$2\,Mg\,(h-f)\,S = (M + M')\,(V^2 - V'^2) \quad \ldots\ldots \quad (6.)$$

$$2\,(M + M')\,S = Mg\,(h-f)\,T^2 + 2\,V'\,(M + M')\,T \quad . \quad (7.)$$

It is evident that from the formulæ (5.), (6.), and (7.), the value of f may be found if the initial velocity V' of the train and the time of passing the posts by which the plane is staked out be observed.

If the train be allowed to move from a state of rest by gravity alone, the formulæ will be simplified by the condition $V' = 0$. They then become

$$Mg\,(h-f)\,T = (M + M')\,V \quad \ldots\ldots \quad (8.)$$

$$2\,Mg\,(h-f)\,S = (M + M')\,V^2 \quad \ldots\ldots \quad (9.)$$

$$2\,(M + M')\,S = Mg\,(h-f)\,T^2 \quad \ldots\ldots \quad (10.)$$

In the preceding formulæ the load is considered as descending the gradient. If it ascend, gravity will become a retarding force, and the sign of h must be changed; also the sign of dV will become negative. The formulæ (5.), (6.), and (7.), will then become

$$Mg\,(f+h)\,T = (M + M')\,(V' - V) \quad \ldots\ldots \quad (11.)$$

$$2\,Mg\,(f+h)\,S = (M + M')\,(V'^2 - V^2) \quad \ldots\ldots \quad (12.)$$

$$2\,(M + M')\,S = -\,Mg\,(f+h)\,T^2 + 2\,V'\,(M + M')\,T \quad (13.)$$

If in this case the load having the initial velocity V' be allowed to run until it stop, we shall have $V = 0$ ∴ (11.), and (12.) become

$$Mg\,(f+h)\,T = (M + M')\,V' \quad \ldots\ldots \quad (14.)$$

$$2\,Mg\,(f+h)\,S = (M + M')\,V'^2 \quad \ldots\ldots \quad (15.)$$

In the case of retarded motion in descending a gradient less steep than the angle of friction, h in these formulæ must be taken negatively.

If, therefore, the train move down an inclined plane from a state of rest, we shall have (9.)

$$V^2 = \frac{M}{M + M'} \times 2\,g\,(h-f)\,S,$$

instead of

$$V^2 = 2\,g\,(h-f)\,S,$$

according to M. de Pambour. The value of V^2, therefore, obtained by him (neglecting all resistances which depend on the velocity) is greater than the truth in the ratio of $M + M'$ to M.

If the train move successively on two planes whose gradients are h and h', we shall have

$$V^2 = \frac{M}{M + M'} \times 2g\{(h - f)S + (h' - f)S'\} \quad . \quad . \quad (16.)$$

If we suppose that on the second plane $f > h'$ the motion will be retarded, and still more if h' be negative, or, which is the same, if the second plane be an ascending gradient. If the train in such case be allowed to move until it come to rest, we should have $V^2 = 0$, which would give

$$(h - f)S + (h' - f)S' = 0. \quad . \quad . \quad . \quad (17.)$$

Now, it is remarkable that this conclusion will follow equally from the correct formulæ which include the effect of the wheels, and from the erroneous formulæ in which that effect is omitted. This takes place by a compensation of two contrary errors. So long as the motion of the train is accelerated, the error produced on V^2 by neglecting the wheels is in *excess*, and while it is retarded, the error produced on V^2 is in *defect*; and in M. de Pambour's formulæ this *excess* and *defect* are equal. They therefore neutralize each other, and the final result, so far as respects the effect of the wheels, is thus accidentally correct.

From the condition (17.) we obtain

$$f = \frac{hS + h'S'}{S + S'} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18.)$$

The quantities hS and $h'S'$ are the differences between the levels of the extremities of the spaces S and S', and if h' be taken negatively when the gradient rises, the quantity $hS + h'S'$ will be the actual difference between the level of the point from which the train commences its motion and that of the point where it stops. Thus the resistance would, according to this reasoning, be found by dividing the difference of levels of these two points by the entire space run over by the train.

The Sutton inclined plane on the Liverpool and Manchester Railway, falling toward Manchester, was staked out in distances of 110 yards, commencing from a point 1100 yards from the foot of the plane. The level of the tenth stake, which marked the foot of the plane, is stated by M. de Pambour to be 34·61 feet below the level of the first stake. The line extending from the foot of the plane towards Manchester, which continued to fall, but in a very slight degree, was also staked out through a distance of more than a mile from the foot of the plane.

Five wagons loaded with bricks, and weighing gross 31·31 tons, were allowed to descend by gravity from the point 1100 yards from the foot of the plane; and they continued to move along the gradient at the foot of the plane until they had traversed 9933 feet and had attained a level 38·55 feet below the point from which they started. Hence, by the formulæ already given, the ratio of the friction to the load would be $\frac{3855}{993300} = \frac{1}{258}$, being at the rate of 8·69 lbs. per ton of the gross load.

By throwing off a quantity of the bricks the load was then reduced to 25·58 tons, and the experiment was repeated in the same manner, when the proportion of the resistance to the load was found to be $\frac{1}{244}$ or 9·17 lbs. per ton.

Three loaded wagons and an empty one were next allowed to run separately down the plane, and the following were the results.

Gross Load.	Distance run.	Difference of Level.	Ratio of Friction to Load.	Friction in lbs. per Ton.
Tons.	Feet.	Feet.	One to	
4·65	7326	37·16	197	11·36
5·15	6663	36·95	180	12·42
5·20	7455	37·19	200	11·17
1·85	6204	36·78	169	13·28

Without pursuing the experiments of M. de Pambour further, it will be easily perceived that the atmosphere must have exercised upon them an influence much greater than he suspected, and certainly greater than he has taken any account of in the computations which he has founded on them.

When the five wagons were charged with a load amounting to 31·31 tons gross, the resistance computed by the formula (18.) was 8·69 lbs. per ton, and the total resistance was consequently 272 lbs. When the gross load of the same wagons was reduced to 25·58 tons, the resistance per ton, computed in the same way, was 9·17 lbs., and the total resistance was 235 lbs.

If the resistance were, like friction, proportional only to the load, the resistance *per ton* would have been the same in both cases. But we find, on the contrary, that by diminishing the gross load, the gross resistance is not diminished in so great a proportion and the resistance per ton is increased*.

* All the experiments which have been made to develope the laws of friction, go to prove that, except in extreme cases, friction bears an invariable ratio to the pressure on the rubbing surfaces. When pressures, however, bearing a very high ratio to one another are compared, the corresponding quantities of friction are not found to be in this ratio, the friction corresponding to the greater pressure bearing a ratio to that corresponding to the lesser pressure, less than that of the pressures. This exception to the law of the constant ratio between the friction and pressure has, however, no application in a case like the above.

This is just the effect which the resistance of the air would produce. If the velocity were the same in both experiments, that part of the total resistance due to the air would be the same, because the same wagons being used in each case, the same surfaces were exposed to the air. In that case, therefore, the same amount of atmospheric resistance being divided amongst a less number of tons, there would necessarily be a greater resistance per ton in the second experiment, and the gross resistance of the train would be diminished in a less proportion than the load.

But it is evident that the mean velocity must have been less with the lesser than with the greater load, because a less amount of atmospheric resistance would be sufficient, combined with friction, to balance the diminished effect of gravitation. M. de Pambour, however, did not observe, or at least has not recorded, the *time* which the train took in any case to move down the plane, or to come to rest, and has not, therefore, supplied any data by which the mean speed can be computed.

If it be admitted that the resistance due to friction is independent of the velocity, it will follow that the difference between the resistance *per ton* in the one experiment and the other must be altogether ascribed to the air. Now, if A express the whole resistance due to the air in the first experiment, and A' in the second, we shall therefore have

$$\frac{A'}{25{\cdot}58} - \frac{A}{31{\cdot}31}$$

equal to the difference of the resistance per ton in the one case and the other. Hence we shall have

$$\frac{A'}{25{\cdot}58} - \frac{A}{31{\cdot}31} = 9{\cdot}17 - 8{\cdot}69 = 0{\cdot}48.$$

Let $A' = A - \Delta$. Hence we obtain

$$A\left(\frac{1}{25{\cdot}58} - \frac{1}{31{\cdot}31}\right) = 0{\cdot}48 + \frac{\Delta}{25{\cdot}58}.$$

Whence we find

$$A = 67 + 5{\cdot}5\,\Delta.$$

In the absence of the necessary data for determining Δ, we can only infer from this that $A > 67$. Now let f' be the resistance due to friction, properly so called, in lbs. per ton. We have then

$$31{\cdot}31 f' + A = 272,$$

$$\therefore 31{\cdot}31 f' = 272 - A.$$

But since A $>$ 67,

$$31{\cdot}31 f' < 272 - 67 = 205,$$

$$\therefore f' < 6{\cdot}6.$$

Thus it follows that the resistance from friction, properly so called, in these experiments was *less than* $6\frac{6}{10}$lbs. per ton, and the angle of friction would therefore be less than 1 in 340.

These results are not as definite as could be desired, but they seem to be the only ones to which the data supplied by the experiments are sufficient to conduct us. Had the moment of the train commencing to move, and the moment it came to rest, been observed, its mean velocity would in each case have been known; and although that would not have been sufficient to establish the amount of resistance at any given speed, it would at least have supplied the means of better approximation. Had the experiment, however, been satisfactorily conducted with a view to develop the effect of the resistance of the air, the time of passing each successive stake should have been observed, and thus the rate of the variation of the speed would have been discoverable, as we shall presently perceive.

Since the preceding paragraphs were set in type, the writer of this report has been favoured by Mr. Edward Woods, engineer to the Liverpool and Manchester Railway Company, (to whose intelligent aid the Committee has been throughout its proceedings much indebted,) with an account of the times of passing the successive stakes in the experiment made with the five wagons loaded with the reduced weight of 25·58 tons. Mr. Woods, however, wishes it to be understood that for this observation of the times M. de Pambour is not responsible, it having been taken on the occasion by Mr. Woods himself for his individual satisfaction.

Stake.	Distance run.	Time.			Difference.	Levels.		Stake.	Distance run.	Time.			Difference.	Levels.		Stake.	Distance run.	Time.			Difference.	Levels.	
	yards.	h	m	s	s	ft.	in.		yards.	h	m	s	s	ft.	in.		yards.	h	m	s	s	ft.	in.
29	0	7	5	0		0	0	18	1210	7	8	2	10	35	0·3	7	2420	7	10	38	20	37	1·7
28	110			52	52	3	5·7	17	1320			13	11	35	2·3	6	2530		11	1	23	37	2·6
27	220		6	14	22	7	0·8	16	1430			23	10	35	2·8	5	2640			26	25	37	4·5
26	330			31	17	10	7·5	15	1540			$34\frac{1}{2}$	$11\frac{1}{2}$	35	4·4	4	2750			56	30	37	4·1
25	440			46	15	14	4·3	14	1650			47	$12\frac{1}{2}$	35	8·5	3	2860		12	34	38	37	11·1
24	550			59	13	18	2·1	13	1760		9	0	13	36	2·1	2	2970		13	25	51		
23	660		7	11	12	21	9·3	12	1870			14	14	36	5·3	1	3080		14	42	77	38	4·2
22	770			22	11	25	6·4	11	1980			28	14	36	7·9	+28yds.	3108		15	20	38		
21	880			32	10	28	11·8	10	2090			44	16	36	9·6								
20	990			42	10	32	0·8	9	2200		10	0	16	36	11·0								
19	1100			52	10	34	7·3	8	2310			18	18	37	0·7								

The foot of the inclined plane corresponded with the stake No. 19; and it will be observed, that the time of descending

1100 yards was 172 seconds, and that therefore the mean velocity of the descent was 19·18 feet per second. But by comparing the times of descending each successive interval of 110 yards, it will be observed that the rate of acceleration, instead of being uniform, as it would be independently of the resistance of the air, is gradually less; and the last 330 yards of the plane was descended at an uniform velocity of 33 feet per second.

Mr. Woods has computed the value of f, determined by the formula 10, page 207, for the first 110 yards, the first 220 yards, the first 330 yards, and the first 440 yards, and the following are the results.

1. From 0 to 110 yards $f = \cdot 00228 = 5\cdot 107$ pounds per ton.
2. From 0 to 220 yards $f = \cdot 00255 = 5\cdot 712$ pounds per ton.
3. From 0 to 330 yards $f = \cdot 00265 = 5\cdot 936$ pounds per ton.
4. From 0 to 440 yards $f = \cdot 00293 = 6\cdot 563$ pounds per ton.

The increasing value of f shows the increase of the resistance with the velocity. In the first 110 yards, the mean velocity being only 6·34 feet per second, the resistance of the atmosphere was trifling, and the value of f may be considered as a close approximation to the friction, properly so called.

Since in the first 110 yards there must have been *some* atmospheric resistance, however small, it follows that the friction, properly so called, must have been less than the value of f obtained by Mr. Woods' calculation. We shall therefore assume that f was in this case less than 5·11 pounds per ton. The total amount of friction, therefore, for the load of 25·58 tons would be less than 130·7 pounds. If we take the mean resistance of this load at 9·17 pounds per ton, as determined by M. de Pambour's method, we shall find the total mean resistance to be 234·55 pounds. The mean atmospheric resistance would therefore be greater than 104 pounds.

It will be observed that this result is in accordance with that already obtained for the load of 31·31 tons, by a different process of reasoning. The determination of the limit of f, by the formula (10.), may, however, be regarded as a closer approximation.

The angle of friction corresponding to 5·11 pounds per ton, would be 1 in 438. Therefore $f < \frac{1}{438}$.

We shall not pursue these experiments of M. de Pambour further than to observe, that the computed resistances of the single wagons, as given in p. 209, rendered the effects of the resistance of the air still more apparent. While the mean computed resistance of the train of five wagons was only 8·69 lbs. per ton, their gross weight being 31·31 tons, that of the single wagons was about 11·3 lbs. per ton. This difference M. de Pambour ascribed to the atmosphere, yet it does not appear to have occurred to him to direct his experiments or calculations to the determination of the share which friction and the air had respectively in resisting the motion. Having disregarded in all cases the effect of the velocity in modifying the resistance, and having based all his calculations on suppositions which are only applicable to friction, we must conclude, that he regarded the effects of the air as so inconsiderable, that, without any error of practical importance, the mean retardation due to them might be considered as part of the friction.

It has been thought right to bestow some attention on the experiments and calculations of M. de Pambour in this place, because they are not only the most extensive series of which we have any knowledge, but because much stress is usually laid on them by engineers and others who are interested in these questions. We shall, however, presently demonstrate that the resistance of railway trains has so important a dependence on the velocity, that no principle of calculation can be admitted which proceeds, like those of M. de Pambour, upon the supposition of a constant amount of resistance. But we shall also be enabled to give a conclusive proof, founded on direct experiments, that the method of determining the resistance by the formula (18.) adopted by M. de Pambour is altogether fallacious, and that by such a method any value, however great, of the resistance might have been obtained.

Having noticed these erroneous conclusions to which M. de Pambour has arrived, it is but justice to that gentleman at the same time to acknowledge the activity and zeal with which he pursued his inquiries, and the quantity of valuable results of his extensive experiments by which he has enriched the practical science of this country. That M. de Pambour should have overlooked or underrated a source of resistance to locomotive power, which, however obvious, had eluded the attention of the whole engineering profession in Great Britain, as well as of his own country, will not, we are sure, be felt by him to be any serious disparagement to his sagacity.

In commencing this inquiry, it was not suspected that that part of the resistance which increases with the velocity, the chief part of which, if not the whole, is probably due to the atmosphere, formed so important an agent in opposition to the moving power on railways worked at high speeds as the results of experiments which were subsequently made have proved it to be; and in the acknowledgment of this oversight the writer of this report very willingly joins. This source of resistance had been however to an equal extent neglected, so far as we are informed, by engineers generally, and indeed by all who had directed their attention to the practical working of railways and to the experimental investigation of their effects. Some scientific men had called the attention of engineers to the subject, and Mr. Herapath more especially insisted on its importance, made various calculations of its probable effects, and predicted that on railways worked at high speeds it would prove to be the chief source of resistance to the moving power. As, however, no direct experiments had been made to demonstrate its amount, and as it was known that the theory of the resistance of elastic fluids had not been based on experiments with sufficient certainty and precision to render its principles capable of being applied for practical purposes in operations of the kind now considered, these suggestions were disregarded, and the effects of the resistance of the air continued to be considered as sufficiently allowed for by estimating them in combination with friction at mean speeds, no attempt whatever having been made to ascertain experimentally the variation of resistance of the same loads at different speeds.

It was determined, in the first instance, to repeat and vary the experiments on the accelerated motion of trains down inclined planes, and their retarded motion in running to rest where the resistance exceeded the moving power.

The first experiments made with this view were tried on the same inclined plane on which the experiments of M. de Pambour were made, viz. the Sutton plane on the Liverpool and Manchester Railway. This plane and the level at its foot were staked out as in M. de Pambour's experiments, but in the present case the *time* of passing each successive stake was observed and recorded, so that the variation of speed, during the motion, might be rendered apparent.

In these experiments it became manifest, that the rate of acceleration in the descent and the subsequent retardation could not be represented by the formulæ for uniformly accelerating and retarding forces, and that therefore some force was in operation which, unlike friction, had *a dependence on the velocity*.

To decide this, it was determined to try the effect of gravity on a train of loaded wagons descending an inclined plane less steep than those which occur upon the Liverpool and Manchester Railway, and for that purpose the Madeley plane on the Grand Junction Railway, already mentioned, was selected, and, as a first trial, a train of wagons loaded with iron rails and chairs was prepared. This train was placed near the summit of the plane, and was allowed to move down by gravity. The plane was staked out in distances of a hundred yards by 58 stakes, commencing from the lowest point and numbered upwards, and the inclination was ascertained to be at the rate of 1 in 178, with great uniformity, throughout the whole length of 5800 yards.

The time of passing the stakes successively being observed, it was found that the motion of the wagons was accelerated rapidly at first, but gradually less and less, until at length all acceleration ceased and a perfectly uniform motion was maintained to the foot of the plane.

The unfavourable state of the weather prevented the circumstances of these earlier experiments from being observed and recorded with sufficient accuracy to render them fit to be taken as the basis of any exact calculation of resistance, but more than sufficient evidence was obtained from them that no principles of calculation could be applied to the motion of trains on railways with any view to accurate results, or even to a rough approximation in which the increase of resistance due to the increase of velocity is not allowed for.

The problem which now presented itself for solution was the motion of a train of wheeled carriages subject to resistances which have some dependence on the velocity. All the investigations which have been hitherto made respecting friction are in accordance in showing that the amount of this resistance is independent of the velocity; and unless it be maintained that the friction of carriages on railways differs from all the varieties of friction to which experimental inquiry has been directed, it must be admitted that the part of the resistance to railway carriages which depends on friction is independent of the velocity of the motion.

The problem of the resistance opposed by fluids to solids moving through them has been investigated by Newton, and by the most eminent of his successors, Bernoulli, Euler, and the principal mathematicians of the last century. Their researches, however, so far as regards the resistance of elastic fluids, are more remarkable for profound mathematical skill than for practical usefulness, most of them being founded on conditions inapplicable to the actual motion of bodies through the air, and leading to results more or less in discordance with experience.

The earliest experiments on the resistance of the air to bodies moving through it which are entitled to attention, are those of Robins, made about the middle of the last century. These were subsequently repeated and to some extent varied by Borda, who published the results of his inquiry in the Memoirs of the Academy of Sciences of Paris, in 1763.

The object of the experiments of Robins was to obtain grounds for a practical treatise on gunnery, and they were accordingly limited for the most part to the motion of cannon balls at high velocities. The result of these experiments was to prove, that the law of the resistance being proportional to the square of the velocity was not true in comparing slow with very high speeds. It was found, for example, to give a resistance in some cases three times less than the actual resistance, showing, that when extended to such limits, the resistance must vary in a much higher proportion.

Dr. Hutton was, so far as we are informed, the latest inquirer who undertook a course of experiments with the view of determining the amount and the law of the atmospheric resistance. Besides directing his inquiries to more varied velocities, he also endeavoured to investigate the effects which the form of the moving body produces upon the resistance. The experiments were made with hemispheres moved alternately with the convex and flat sides foremost, with cones moved alternately with the point and base foremost, with cylinders moved with the end foremost, and with spheres.

It was found that at moderate velocities the resistance did not sensibly vary from the law of the squares of the velocities; but in comparing slow speeds with high speeds, a gradual departure from that law took place, the resistance increasing in a higher ratio.

In comparing together bodies exposing a frontage of different magnitudes with the same speed, it was found that the resistance was not proportional to the magnitude of the frontage, but in some higher unascertained ratio.

It was also found that the resistance did not depend alone on the magnitude of the transverse section, for that with the same transverse section different resistances were encountered according to the form of the body. Thus, in general, a flat front produced more resistance than a round or pointed one. But on the other hand, the resistance was not found to diminish in proportion to the sharpness of the foremost end of the moving body; but that, on the contrary, a body presenting a hemispherical end was less resisted than one presenting a conical end, the transverse section of both being the same.

It was also found that the resistance did not depend alone on the magnitude or form of the foremost end, but had some dependence on the hinder part. Thus, a cone, hemisphere, and cylinder, having equal bases, moved base foremost, with the same velocity, suffered different resistances.

No law was obtained from these experiments by which the resistance of a body could be calculated from its form and magnitude. The results obtained were merely negative, showing that the resistance could *not* be calculated on such or such data, but that it depended on some principle not yet discovered, and which the experiments themselves of Dr. Hutton did not develop.

These experiments also were made on bodies of very limited magnitude: the bases of the cones, cylinders and hemispheres were less than a quarter of a square foot. It will therefore be apparent, that they furnish no just grounds by which the resistance to bodies of the form and magnitude of railway trains can be computed independently of experiment. How strongly Dr. Hutton himself was impressed with the imperfect nature of his results, and with the necessity for further experimental inquiry before any real or satisfactory determination of the atmospheric resistance could be obtained, will be collected from the following observations, with which he closes this part of the inquiry:

"On a review of the whole of the premises, we find that the resistance of the air, as determined from the foregoing experiments, differs very widely, both in respect to its quantity on all figures, and in regard to the proportion of its action on oblique surfaces, from the same actions and resistances, as assigned by the most plausible and imposing theories which have been hitherto delivered and confided in by philosophers. Hence it may be concluded that all the speculative theories on the resistance of the air hitherto laid down are very erroneous, and that it is from experiments only, carefully and skilfully executed, that a rational hope can be grounded of deducing and establishing a true and useful theory of the action of forces so intimately connected with the numerous and important concerns of human life."

Since the only two sources of resistance to moving bodies with which we are acquainted, are the friction of the parts moving upon and against one another, and the resistance of the atmosphere through which the body moves; and since all scientific experiments which have been directed to ascertain the law of the former agree in showing it to be proportional to the weight or pressure and independent of the velocity, and that the latter, within moderate limits of speed, varies in a proportion, *cæteris paribus*, not much departing from that of the square of the velocity; the form which may with most probability be assigned to the expression for the resistance of a railway train will be one consisting of two terms, one of which is proportional to the load and the same at all velocities, while the other for the same train will vary as the square of the velocity. If, then, R express the whole resistance of a train moving with a velocity V, we shall have

$$R = aV^2 + B.$$

Now, since B is proportional to the load, if M express the load in tons, and f the resistance for a load of one ton with an indefinitely slow motion, we shall have

$$B = Mf,$$

and therefore

$$R = aV^2 + fM.$$

The coefficient a being the constant number which, being multiplied by the square of the velocity, gives that portion of the resistance which varies with the velocity, will depend on the form and magnitude of the train, on the number, form, and magnitude of the wheels, and in general on any circumstances by which the resistance of the air to the moving parts of the train may be affected. But it should be observed, also, that there is nothing in the mere mathematical formula which limits the term $a\,V^2$ to represent the effect of the air; that term in fact represents any effect which would be attended with a resistance proportional to the square of the velocity.

If any means were devised by which the total resistance of the same train at two different velocities could be found, the value of the coefficient a might then be determined; for let R and R′ be the two resistances of the same train at the velocities V and V′, then we have

$$R = aV^2 + Mf$$

$$R' = aV'^2 + Mf$$

$$\therefore a = \frac{R - R'}{V^2 - V'^2}.$$

Hence it appears that the difference between the two observed resistances, divided by the difference of the squares of the corresponding velocities, would be the value of a.

But as the estimation of the resistance of trains by any direct means is attended with difficulty, it may be useful to seek in the circumstances of accelerated and retarded motion on inclined planes which are straight, other means for the solution of this problem.

If R, as already explained, express the ratio of the retarding force produced by the whole resistance to the retarding force of gravity, expressed as usual by g, then the velocity which gravity would destroy in the time $d\,T$ being $g\,d\,T$, the velocity which the resistance would detroy in the same time will be $R\,g\,d\,T$.

If we suppose the train to move down an inclined plane whose gradient is h, the effective moving force will then be the excess of the gravitation of the train down the plane above this resisting force: this excess will be

$$M h g - R g;$$

and the moving force which that will impart in the time $d\,T$ will be

$$(M h - R) g\, d\,T.$$

This, by the principle of D'Alembert, must be in equilibrium with the moving force which in the same time shall be received by the train; and since this moving force, including as before that which is absorbed by the revolution of the wheels, will be

$$(M + M')\, d\,V,$$

we shall have

$$(M h - R) g\, d\,T = (M + M')\, d\,V;$$

and substituting for R its value already found, this will become

$$\{M (h - f) - a V^2\} g\, d\,T = (M + M')\, d\,V.$$

To integrate this, let

$$x^2 = \frac{a V^2}{M (h - f)} \therefore d\,V = \sqrt{\frac{M (h - f)}{a}} \,.\, d\,x.$$

Hence we have

$$\sqrt{M a (h - f)}\, (1 - x^2)\, g\, d\,T = (M + M')\, d\,x$$

$$\therefore \frac{\sqrt{M a (h - f)}}{M + M'} g\, d\,T = \frac{d\,x}{1 - x^2},$$

which being integrated gives

$$\frac{\sqrt{M a (h - f)}}{M + M'} g\,T = \frac{1}{2}\, l' \frac{1 + x}{1 - x} + C,$$

the logarithm being hyperbolic.

If x' be the value of x, which corresponds to $T = 0$, the above integral will become

$$\frac{\sqrt{M a (h - f)}}{M + M'} g\,T = \frac{1}{2}\, l' \frac{(1 + x)(1 - x')}{(1 - x)(1 + x')}. \quad . \quad . \quad (19.)$$

The relation between V and S may be found by eliminating T by $V\, d\,T = d\,S$, by which we obtain

$$\{M (h - f) - a V^2\} g\, d\,S = (M + M')\, V\, d\,V$$

$$\therefore \frac{g\, d\,S}{M + M'} = \frac{V\, d\,V}{M (h - f) - a V^2},$$

which being integrated gives

$$\frac{2\,a\,g\,S}{M+M'} = l'\,\frac{M\,(h-f) - a\,V'^2}{M\,(h-f) - a\,V^2}, \quad . \quad . \quad . \quad . \quad . \quad (20.)$$

where V' is the value of V corresponding to $S = 0$ and $T = 0$, and is therefore the initial velocity.

Hitherto the train has been assumed to move with accelerated motion down an inclined plane. If it ascend, having received any initial velocity, V', the motion will be retarded, and the equation will be

$$\{M\,(h+f) + a\,V^2\}\,g\,d\,T = -\,(M + M')\,d\,V.$$

Substituting as before, let

$$x^2 = \frac{a\,V^2}{M\,(h+f)} \quad \therefore d\,V = \sqrt{\frac{M\,(h+f)}{a}}\cdot d\,x.$$

Hence we have

$$\sqrt{M\,a\,(h+f)}\,(1 + x^2)\,g\,d\,T = -\,(M + M')\,d\,x$$

$$\therefore \frac{\sqrt{M\,a\,(h+f)}}{M+M'}\,g\,d\,T = -\,\frac{d\,x}{1+x^2},$$

which being integrated between the limits x and x', the value x' corresponding to $T = 0$, we have

$$\tan\left\{\frac{\sqrt{M\,a\,(h+f)}}{M+M'}\,g\,T\right\} = \frac{x'-x}{1+x\,x'}.$$

And substituting for x and x' their values, we find

$$\tan\frac{\sqrt{M\,a\,(h+f)}}{M+M'}\,g\,T = \frac{\sqrt{M\,a\,(h+f)}\,.\,(V'-V)}{M\,(h+f) + a\,V\,V'} \quad . \quad . \quad (21.)$$

The relation between V and S will be found as before:

$$\frac{2\,a\,g\,S}{M+M'} = l'\left(\frac{M\,(h+f) + a\,V'^2}{M\,(h+f) + a\,V^2}\right) \quad . \quad . \quad . \quad . \quad (22.)$$

If the train move down a plane, of which the gradient is such that $h < f$, the motion will be retarded, and in that case the equations may be put under the forms

$$\tan\left\{\frac{\sqrt{M\,a\,(f-h)}}{M+M'}\,g\,T\right\} = \frac{\sqrt{M\,a\,(f-h)}\,(V'-V)}{M\,(f-h) + a\,V\,V'} \quad (23.)$$

$$\frac{2\,a\,g\,S}{M+M'} = l'\left(\frac{M\,(f-h) + a\,V'^2}{M\,(f-h) + a\,V^2}\right). \quad . \quad . \quad . \quad . \quad . \quad (24.)$$

Such are, then, the equations of the motion of a train of wheeled carriages which are submitted to the action of accelerating and retarding forces, or retarding forces only which are independent of the velocity, combined with a retarding force which is proportional to the square of the velocity; and such will be the actual equations of the motion of a train of railway carriages if the friction be independent of the speed, and the resistance of the air, and any other retarding forces which act upon it, be as the square of the speed.

It will now be a matter for consideration, in what manner experiments may be devised so as to enable us to determine the values of the constants f and a.

If a train descend an inclined plane by gravity with accelerated motion, that part of the resistance which increases with the speed will be continually augmented, while the accelerating force of gravity will remain unaltered. At length, therefore, a velocity will be attained which will render the resistance so great that it will be equal to the accelerating force of gravity, and then all acceleration will cease, and the train will move with an uniform velocity. The condition under which this will take place will be expressed by putting the value of $d\,V = 0$, which gives

$$M\,(h - f) - a\,V^2 = 0$$

$$\therefore M f + a\,V^2 = M\,h \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25.)$$

where V is the uniform velocity attained in moving down the gradient h.

If the same train be moved down another gradient, h', another uniform velocity, V′, will be attained, and we shall have the condition

$$M f + a\,V'^2 = M\,h'.$$

From these two equations the values of a and f may be obtained.

$$a = \frac{M\,(h - h')}{V^2 - V'^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (26.)$$

$$f = \frac{V^2\,h' - V'^2\,h}{V^2 - V'^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (27.)$$

If, therefore, two inclined planes be selected sufficiently steep to produce accelerated motion in the train, and if the same train be allowed to descend them until it acquire an uniform velocity, this will give values for V and V′; the inclinations of the planes will determine h and h', and the weight of the train will determine M. The values of a and f may then be computed by the above formulæ.

In the practical application of this method there are some circumstances which will demand attention. It may happen that the inclined plane selected for the experiment may not have sufficient length to allow the acceleration of the train by gravity to continue till the velocity become uniform. It will therefore be more convenient to dismiss the train with a considerable speed from the top of the plane, which may be done by impelling it by means of a locomotive engine towards the top of the plane, and detaching the engine so that the train shall be started down the plane with the velocity given to it by the engine. If this velocity be less than that which balances the accelerating force down the plane, the train will be accelerated until it attain the limiting speed. If it be greater, then it will be retarded by the air until it be reduced to the limiting speed.

In the preceding investigation we have proceeded upon the supposition that the air through which the train is moved is quiescent. The effects of a wind of any considerable force would generally be so complicated, that it would be difficult indeed to introduce them into the calculation in such a manner as to give results of any practical value. If the wind blow in the direction of the motion, the velocity of the train through the air will be the difference between the velocity of the train and the velocity of the wind; and if this was all the effect to be considered, the investigation would not be attended with much difficulty; for it would only be necessary to consider in that case the velocities expressed by V and V′ in the preceding formula to be the excess of the velocity of the train above that of the air. But it should be remembered, that besides the progressive motion of the train, a part of the resistance which is assumed to vary in proportion to the square of the velocity, is produced by the revolution of the wheels. Now this part of the resistance is not affected by the wind, and will be the same whatever be the state of the atmosphere. Thus it is possible to suppose the velocity of the train equal to the velocity of the wind, and therefore no resistance whatever to be produced by the progressive motion of the train. Nevertheless, in such a case, it is evident that the revolution of the wheels would produce by the action of their spokes the same resistance as if the atmosphere were calm. These considerations appear to lead to the conclusion, that the diminution of resistance to be expected from a wind blowing in favour of a train, and the increase of resistance from a wind blowing against it, will not be so great as it might be expected to be, if no effect but the progressive motion were taken into account.

But if a correct investigation of the effects of a wind either directly favourable or directly adverse to the motion of a train be attended with doubt and difficulty, the effects of every side or oblique wind are still more so. An oblique wind would be resolved into components parallel and perpendicular to the motion of the train. The component parallel to the motion would then be treated as a wind directly favourable or adverse. The lateral component acting against the extensive surface usually presented by the side of the train, would have the effect of pressing the flanges of the opposite wheels against the rails. This, combined with the effect of the conical form of the tires, would have a tendency to impart to the carriages an oscillating motion between the rails, causing the flanges alternately to strike the rails, and thereby to produce a resistance the amount of which it would be difficult indeed to reduce to general methods of calculation.

It appears, therefore, most desirable that experiments for the exact determination of the mean amount of resistance to railway trains should be made when the atmosphere is calm, but it is rarely that this condition can be obtained. In its absence, the results of the experiments can only be regarded as approximations, more or less precise as the disturbing causes exist in a less or greater degree.

It was not easy to find on the railways which have been completed inclined planes in convenient situations in all respects suited for the plan of investigation which was contemplated. On the whole, however, it seemed that the most eligible were the Whiston and Sutton inclines on the Liverpool and Manchester Railway, and a series of inclines between Madeley and Crewe on the Grand Junction Railway.

The summit of the Whiston plane is at about nine miles from Liverpool, and the plane falls at nearly an uniform rate of 1 in 96 towards Liverpool for a distance of 2700 yards. From the foot of the plane the line rises at the average rate of 1 in 936 for a distance exceeding the range of the experiments.

A stake marked 0 was placed at the summit of the plane, and twenty-seven other stakes, marked successively 1, 2, 3, &c., divided the whole length of the plane into spaces of 100 yards. The distance from the 27th stake, which marked the foot of the plane, to the 24th mile post was 150 yards, and the line from that point towards Liverpool was divided by quarter-mile posts, the levels of which were taken.

The inclined plane thus divided by the twenty-seven stakes was perfectly straight from the summit to the 24th stake. At that stake curves having a radius of 3300 yards commenced, which terminated at the $24\frac{1}{2}$ mile post, a point about 900 yards from the foot of the plane. From that point to a point 220 yards beyond the $24\frac{3}{4}$ mile post from Manchester the line was straight, and from the latter point to 370 yards beyond the 25th mile post it was curved with a radius of 2700 yards. Beyond the last point the line was straight.

The following experiments, which were conducted by the reporter, assisted by Mr. Edward Woods, were made on the south line of rails of this incline. The line was laid with parallel rails on stone blocks. The weight of the rails was 50 pounds per yard, and they had been three years laid. The experiments were made with four first-class carriages, weighing each, when unloaded, 3 tons 16 cwt. Each carriage was supported on two pair of 3-feet wheels. Each pair of wheels with their axle weighed 8 cwt.

During the experiments a wind of moderate force blew down the plane. The velocity of the wind was not ascertained. The weather was fair, and the rails clean and dry.

The gross weight of the four carriages in the first and second experiments was 15·6 tons. After the second experiment a weight was added by placing iron chains in the carriages, which rendered the gross weight of the train 18·05 tons, which was estimated to be equivalent to their weight when transporting 42 passengers.

The total frontage presented by the foremost carriage was 62 square feet, including the vertical cross section of the wheels. The cross section of all the carriages was the same, and the distance between the carriages when coupled was 3 feet 10 inches. They were coupled by the patent couplings of Mr. Booth.

The train was placed on the summit level of the Liverpool and Manchester Railway, at about half a mile from the post 0, which marked the commencement of the plane. It was drawn by an engine so as to give it a considerable speed. On approaching the stake 0 the engine was detached, and the train was allowed to descend by gravity only, the engine proceeding down the plane so much faster as to be considerably in advance of the train.

The results of the experiments are given in the following table. The first column in each experiment gives the time of passing each successive stake, as taken down, without any reduction for errors of observation, however apparent. In the second column the differences, or the times of passing over each successive hundred yards, are given. In the third column these differences are averaged, so as in some degree to obliterate the errors of the observed times of passing the successive stakes. At the foot of the table the mean time of moving over a hundred yards taken from the entire time of descending the plane is given.

Stakes.	Experiment I. Time.	Experiment I. Diff.	Experiment I. Mean time of moving 100 yards.	Experiment II. Time.	Experiment II. Diff.	Experiment II. Mean time of moving 100 yards.	Experiment III. Time.	Experiment III. Diff.	Experiment III. Mean time of moving 100 yards.	Experiment IV. Time.	Experiment IV. Diff.	Experiment IV. Mean time of moving 100 yards.	Experiment V. Time.	Experiment V. Diff.	Experiment V. Mean time of moving 100 yards.
	m s	s	s	m s	s	s	m s	s	s	m s	s	s	m s	s	s
0	7 27			22 41			2 32·5			34 44			10 51·5		
1	34	7		47·5	6·5		39	6·5		51	7		58·5	7	
2	41	7		54	6·5		46	7		58·5	7·5		11 4·5	6	
3	47	6	6·66	23 1	7	6·66	53	7	6·83	35 3·5	5		11·5	7	6·66
4	53·5	6·5		7	6		59	6		10·5	7	6·63	18·5	7	
5	8 0	6·5		14	7		3 5·5	6·5		17	6·5		25	6·5	
6	6·5	6·5		21	7	6·66	12·5	7	6·50	24	7		32	7	6·83
7	13	6·5	6·50	28·5	7·5		19	6·5		31	7		38·5	6·5	
8	20	7		33·5	5		26·5	7·5		36·5	5·5		45	6·5	
9	27	7		42	8·5	7·00	33·5	7	7·00	43·5	7	6·60	51·5	6·5	6·50
10	34	7	7·00	49	7		39·5	6		50	6·5		58	6·5	
11	40	6		55	6		46	6·5		56	6		12 4	6	
12	47	7		24 1·5	6·5	6·50	52	6	6·17	36 2	6	6·17	10	6	6·17
13	54	7	6·66	8	6·5		58·5	6·5		13	11		16	6	
14	9 0	6		15	7		4 5	6·5		14·5	1·5		23	7	
15	5	5		21·5	6·5	6·66	11	6	6·33	21	6·5	6·33	30	7	
16	11	6		28·5	7		17	6		29	8		36	6	6·50
17	19	8	6·25	35·5	7	7·00	24	7		34	5		42	6	
18	25	6		42	6·5		30	6	6·33	39	5		48	6	
19	31	6		50	8		36·5	6·5		45·5	6·5	6·12	55	7	
20	38	7	6·33	54·5	4·5	6·33	42·5	6		51·5	6		13 2	7	6·50
21	44	6		25 0·5	6		49	6·5	6·33	59	7·5		8	6	
22	50	6		7	6·5		55	6		37 3·5	4·5	6·00	13	5	
23	56	6		14	7	6·50	5 1	6		9	5·5		19	6	5·66
24*	10 2	6	6·00	20	6		5·5	4·5		15	6		25	6	
25*				27	7		12·5	7		21·5	6·5	6·00	31	6	
26*	15	13		33·5	6·5		19	6·5		27	5·5		37	6	
27*	22	7	6·66	39·5	6	6·38	25·5	6·5	6·08	34	7	6·25	43	6	6·00
Mean			6·48			6·61			6·41			6·30			6·35

* Curved with a radius of 3300 yards.

After passing the 27th stake the train was in each experiment allowed to run until it came to rest. The distances which it ran beyond the foot of the plane and the times were as follows.

	Distance. Feet.	Time. Seconds.
Experiment I.	6270	372
Experiment II.	6870	360
Experiment III.	7530	384
Experiment IV.	7620	393
Experiment V.	7410	382

The time of passing each mile post from the 27th stake, at the foot of the plane, until the train came to rest was also observed, and is given in the following table, as well as the levels of the successive posts.

				Experiment I.			Experiment II.			Experiment III.			Experiment IV.			Experiment V.		
Miles.	Quarters.	Distances.	Difference of level.	Time.	Diff.	Mean time of moving 100 yards.	Time.	Diff.	Mean time of moving 100 yards.	Time.	Diff.	Mean time of moving 100 yards.	Time.	Diff.	Mean time of moving 100 yards.	Time.	Diff.	Mean time of moving 106 yards.
	s 27	feet.	feet.	m s 25 39·5	s	s	m s 10 22	s	s	m s 5 25·5	s	s	m s 37 34	s	s	m s 13 43	s	s
24		450		50	10·5	7	32	10	6·7	35	9·5	6·3	43	9	6	53	10	6·7
	1	1320	1·45	26 23·5	33·5	7·6	11 2·5	30·5	6·9	6 6	31	7	38 13	30	6·8	14 24	31	7
	2	1320	1·71	27 4·5	41	9·3	42	39·5	9	43·5	37·5	8·5	50	37	8·4	15 0	36	8·2
	3	1320	1·16	59	54·5	12·4	12 30	48	10·9	7 29	45·5	10·3	39 34	44	10	46	46	10·5
25		1320	1·64	29 28	89	20·2	14	70	15·9	8 29	60	13·6	40 34	60	13·6	16 47	61	13·9
	1	1320	1·09							10 5	96	21·8	42 8	94	21·4	18 30	10·3	23·4

In the first two experiments, with the gross load of 15·6 tons, no acceleration is apparent in the descent. In the first the velocity fluctuated between 100 yards in 6 and 100 yards in 7 seconds, the mean being 100 yards in 6·48 seconds. In the second experiment the limits of the varying observed speed are more narrow, being 100 yards in $6\frac{1}{3}$ seconds and 100 yards in 7 seconds, the mean being 100 yards in 6·61 seconds. The mean result of these two experiments is 100 yards in 6·55 seconds, or 45·8 feet per second.

In the last three experiments a slight acceleration is apparent in the first thousand yards of the descent, but the subsequent variations of speed are too minute and irregular to be ascribed to anything save the casual inequalities of the rails and the inevitable errors of observation.

The time of moving down the last thousand yards in the third experiment was 61 seconds, in the fourth 60 seconds, and in the fifth 61 seconds. We cannot, therefore, be far from the truth if we assume that the train loaded, as in these experiments, with 18·05 tons gross would continue to descend a plane falling 1 in 96 with the velocity of 1000 yards in $60\frac{2}{3}$ seconds, or 49·45 feet per second.

Thus, then, it appears that the uniform velocities attained by the same train of coaches loaded with 15·6 tons and 18·05 tons was 45·8 and 49·45 feet per second respectively.

It will be evident from the formula (25.) that if the resistance of the air vary in the ratio of the square of the velocity within the limits of the experiments, the gross weights of the same train differently loaded ought to be in the ratio of the squares of the uniform velocities attained by it in descending the same plane, the effect of the wind being supposed to bear an inconsiderable proportion to the whole resistance at this speed. For let M and M′ be the two loads and V and V′ the two uniform velocities, we shall then have

$$M f + a V^2 = M h$$

$$M' f + a V'^2 = M' h$$

$$\therefore \frac{M}{M'} = \frac{V^2}{V'^2}.$$

In the present case

$$\frac{M}{M'} = \frac{15{\cdot}60}{18{\cdot}05} = 0{\cdot}864$$

$$\frac{V^2}{V'^2} = \frac{(45{\cdot}8)^2}{(49{\cdot}45)^2} = 0{\cdot}856.$$

From which it appears that the resistance in this case is very nearly proportional to the squares of the velocities.

By substituting for M and h in (25.) their values in these experiments, we find that M h = 364 lbs. in the first two experiments, and M h = 421 lbs. in the last three experiments.

Hence it follows, that at the speed of 45·8 feet per second, or 31·2 miles per hour the resistance of this train of four first-class carriages, weighing 15·6 tons gross, was 364 lbs., and at the speed of 49·45 feet per second, or 33·72 miles per hour, the resistance of the same carriages loaded so as to amount to 18·05 tons gross was 421 lbs.; being in each case at the rate of $23\frac{1}{3}$ lbs. per ton.

Since the effect of the wind must, in these experiments, have rendered the resistance less than it would have been had the atmosphere been calm, it may be inferred with certainty, that the resistance of a train of four first-class carriages, carrying the weight of their usual complement of passengers at $33\frac{3}{4}$ miles an hour on a level and straight railway in calm weather, must be *greater than* 421 pounds, or $23\frac{1}{3}$ pounds per ton.

Consequently, for such a load moved at such a speed, the angle of resistance, or the inclination which in its ascent would double the resistance, and in its descent require no moving power, is greater than $\frac{1}{96}$.

If the weather had been calm when these experiments were made, the distance which the train ran in each case before it came to rest, after leaving the foot of the plane, would have supplied means of obtaining a tolerable approximation to the proportion in which the whole resistance ought to be assigned to each of the two causes—that which is independent of the velocity, and that which is proportional to its square.

As it is intended to repeat these experiments in calm weather, it may be worth while at present to investigate the formulæ by which such an approximation may be obtained.

The symbols in (22.) and (25.) retaining their signification, and h' expressing the gradient of the line extending from the foot of the plane down which the train has been supposed to have descended with a velocity rendered uniform by the resistance, we shall suppose this uniform velocity to be expressed by V'; and since the train is allowed to run until it is brought to rest by the resisting forces, we shall have $V = 0$, and S = the distance from the foot of the plane to the point where the train stops. Making the reductions consequent on these conditions the equations (22.) and (25.) become

$$\frac{2\,a\,g\,S}{M + M'} = l'\left(\frac{h+h'}{h+f}\right)$$

$$\mathrm{M} f + a \mathrm{V}'^2 = \mathrm{M} h'$$

Eliminating a, we obtain

$$\frac{2 \mathrm{M S} g\, (h' - f)}{(\mathrm{M} + \mathrm{M}')\, \mathrm{V}'^2} = l' \left(\frac{1 + \frac{h'}{h}}{1 + \frac{f}{h}} \right)$$

For brevity let $p = \dfrac{(\mathrm{M} + \mathrm{M}')\, \mathrm{V}'^2}{2 \mathrm{M S} g\, h}$. Hence

$$\frac{h' - f}{p\, h} = l' \left(\frac{1 + \frac{h'}{h}}{1 + \frac{f}{h}} \right)$$

Let

$$u = \left(1 + \frac{h'}{h}\right) - p\, l' \left(1 + \frac{h'}{h}\right)$$

$$x = 1 + \frac{f}{h},$$

and we have

$$u = x - p\, l' x. \quad . \quad . \quad . \quad . \quad . \quad . \quad (28.)$$

This equation would be satisfied by $f = h'$; but that would involve the condition $\mathrm{V}' = 0$, and therefore cannot be admitted.

The data necessary for the calculation of p will be obtained by the experiments and by the levels of the line beyond the foot of the gradient h'. There are also practical limits between which it is certain that the mean value of f must be included. Thus it is certain that f is not greater than 0·0050, and it is equally certain that it is not less than 0·0015. If, then, the equation (28.) be tabulated between these limits, taking differences sufficiently small to give the necessary approximation, the values of f may be obtained corresponding to those values of the several quantities, M, M′, V′, &c. which are given by the experiments.

In the case of the Whiston plane, the line rises from the foot of the plane at the mean rate of 1 in 936. We shall have, therefore, the following values for the quantities on which p depends in the first two experiments, the value of M′ having been determined by experiments made on the oscillation of the wheels;

$$\mathrm{M} = 15{\cdot}6 \quad \mathrm{M}' = 1{\cdot}86 \quad g = 32{\cdot}16 \quad h = \frac{1}{936} \quad \mathrm{V}' = 45{\cdot}8.$$

The mean of the distances from the foot of the inclined plane to the points where the train stopped in the first two experiments is 6570 feet. If a quarter of a mile be deducted for the increase of this distance produced by the effects of the wind, the reduced value of S would be 5250 feet, which, combined with the above values of the other quantities, would give $p = 6{\cdot}508$, and the corresponding value of f would be 0·00274.

In the last three experiments we have M′ as before, and $M = 18{\cdot}05$. The mean value of V′ is 49·45, and the mean value of $S = 7520$ feet, from which, if a quarter of a mile be deducted as the effect of the wind, we shall have $S = 6200$. Hence $p = 6{\cdot}331$, and $f = 0{\cdot}00249$.

If a mean be taken between the two values of f thus found, we shall have

$$f = 0{\cdot}00261 = \frac{1}{383}.$$

This value of f is in accordance with the approximation obtained from the experiments of M. de Pambour.

The next set of experiments which demand attention were made upon the Grand Junction Railway.

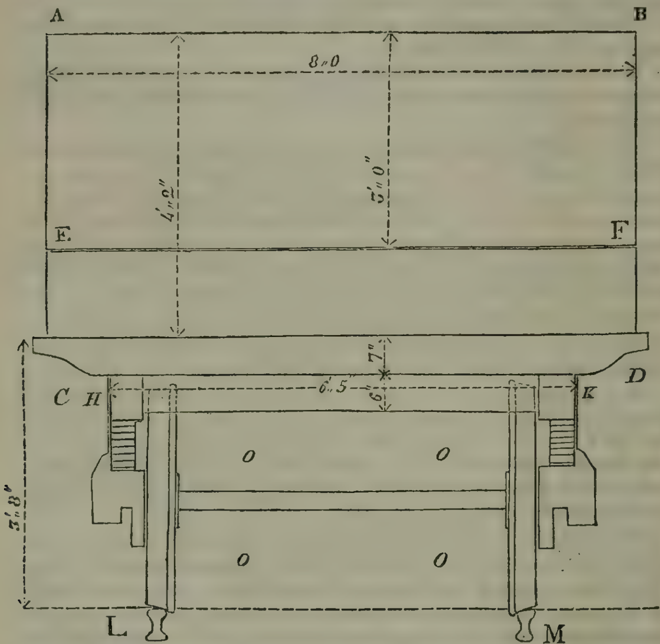
The section of the Grand Junction Railway from Madeley to Crewe is as follows:

Station.	Length of Plane.			Fall.	Rate per mile.	Gradient.
	M.	C.	L.	F.	F.	One in
Madeley ...						
Charlton ...	3	20	90	97·28	29·83	178
Basford ...	3	3	72	60·68	19·92	265
Crewe	1	31	31	22·26	16·00	330

This series of planes was staked out in the following manner: a stake marked 0 was placed at the foot of the plane at Charlton, at the point where the gradients of 1 in 178 and 1 in 265 meet. The plane ascending towards Madeley was divided into spaces of 100 yards by 57 stakes, numbered 1, 2, 3, &c., upwards from the stake 0; and the plane falling 1 in 265 from Charlton towards Basford was also divided into like spaces by 17 stakes, numbered 1, 2, 3, &c., to 17, commencing from the stake 0, the remainder of the line to Crewe being divided by quarter-mile-posts.

Five merchandise wagons were loaded with iron chairs, so as to weigh precisely six tons each gross. The empty wagons weighed two tons each.

These wagons were constructed with high sides and ends, capable of being removed and laid flat upon the platforms of the wagons, so as to expose a greater or less bulk of carriage alternately to the air. The transverse section of these wagons is represented in the annexed figure. The rectangle A B F E represents the moveable end, which, when the frontage of the wagon is required to be diminished, is laid flat upon the platform. The



whole frontage, composed of the rectangle A B D C, and the transverse section of the framing, wheels, springs, and axle, amounts to 47·8 square feet, which, when the high sides are lowered, is diminished by the magnitude of the rectangle A B F E. This latter being twenty-four square feet, it follows that the transverse section with the high sides has very nearly double the magnitude of the transverse section when the sides were lowered.

Immediately before the experiments, the wagons had been taken a distance of thirty miles, from Warrington to the Madeley summit, so that the axles might be expected to be in good running order, and the grease properly melted and supplied.

The weather was fair and quite dry, with a breeze from the north blowing almost directly up the planes, and therefore increasing the resistance of the air. The rails were clean, and the line had previously been accurately levelled from stake to stake.

The other members of the Committee being absent, the following experiments were made by Mr. Hardman Earle, Mr. Edward Woods, engineer of the Liverpool and Manchester Railway, and Mr. Alfred King.

The line changes its direction by curves, all of which have a radius of a mile at the parts marked in the following table with an asterisk.

In conducting the experiments, the train of wagons was allowed, in each case, to pass without interruption from gradient to gradient, the time of passing each successive stake being observed and recorded. But as the motion on the different gradients are essentially distinct experiments, they have been separately tabulated and reduced. In experiment I. the train was brought to the stake No. 33 on the plane falling 1 in 178, and allowed to descend by gravity from a state of rest. It was allowed to move on the next gradient until it reached the seventeenth stake, where it was stopped by the brake.

In experiment II. the same train, in the same state, was placed at the fifty-seventh stake, at the summit of the plane, falling 1 in 178, and was allowed, as before, to descend by gravity from a state of rest. It moved along the successive gradients, and finally stopped 364 yards beyond the 51½ mile-post, on the gradient falling 1 in 330.

In experiment III. the high sides of the wagons were taken down, and laid on the platforms of the wagons, so as to reduce the surface exposed to the air without altering the gross weight of the train. The train was then started again, as in the second experiment, from the fifty-seventh post, and it descended the successive gradients, and finally came to rest on the level at three yards beyond the fifty-fourth mile-post.

In the second and third experiments the train was started from the same point; in the one case it came to rest at 10,019 yards from the point of its departure, and descended 139 feet, and in the other case it came to rest at 14,058 yards from the point of its departure, and descended 175 feet.

In the following table the results of the three experiments on the gradient of 1 in 178 are exhibited in the same manner as in the table of the experiments on the Whiston plane already given.

No. of Stake.	Total fall in feet.	Fall per 100 yards.	Experiment I.			Experiment II.			Experiment III.		
			Time of Passing.	Diff.	Mean.	Time of Passing.	Diff.	Mean.	Time of Passing.	Diff.	Mean.
	feet.	feet.	m s	s	s	m s	s	s	m s	s	s
57	0					49 0			37 0		
56*	1·63	1·63				50 37·5	97·5		39 7·5	127·5	
55*	3·36	1·73				51 19	41·5		40 5	57·5	
54*	5·06	1·70				52	32		51	46	
53*	6·68	1·62				52 19·5	28·5		41 32	41	
52*	8·48	1·80				45	25·5		42 10·5	38·5	
51*	10·21	1·73				53 8·5	23·5		45	34·5	
50*	12·06	1·75				31	22·5		43 17	32	
49*	13·57	1·51				52	21		48	31	
48*	15·30	1·73				54 12	20		44 17·5	29·5	
47*	16·98	1·68				31	19		46	28·5	
46*	18·70	1·72				49	18		45 13	26	
45*	20·38	1·68				55 6·5	17·5		40·5	27·5	
44*	22·04	1·66				23·5	17		46 6	25·5	
43*	23·49	1·65				40	16·5		31	25	
42*	25·40	1·91				56	16		54·5	22·5	
41*	27·14	1·64				56 11	15		47 17	22·5	
40*	28·79	1·65				26	15		38	21	
39*	30·53	1·74				41	15		58	20	
38*	32·11	1·58				55	14		48 17	19	
37*	33·78	1·67				57 9	14		35·5	18·5	
36*	35·50	1·72				23	14		53	17·5	
35	37·09	1·59				36·5	13·5		49 10·5	17·5	
34	38·82	1·73				50	13·5		27·5	17	
33	40·54	1·72	53 0			58 3	13		43	15·5	
32	42·24	1·70	55 2	122·		16	13		58	15	
31	43·87	1·63	47·5	45·5		29	13	13	50 13	15	
30	45·81	1·94	56 22	34·5		41·5	12·5		27·5	14·5	
29	47·49	1·68	51	29		54	12·5		41	13·5	
28	49·23	1·74	57 17·5	26·5		59 6	12	12·03	54	13	
27	50·87	1·64	42·5	25		18·5	12·5		51 7	13	
26	52·51	1·64	58 6	23·5		31	12·5		20	13	
25	54·12	1·61	28·5	21·5		43·5	12·5		32·5	12·5	
24	55·89	1·77	49·5	21		56	12·5	12·5	44·5	12	
23	57·58	1·69	59 10	20·5		0 8	12		56	11·5	
22	59·25	1·67	30·5	20·5		20	12		52 8	12	
21	60·93	1·67	50·5	20		32·5	12·5		19·5	11·5	
20	62·76	1·83	0 8·5	18·5		44·5	12		30·5	10·5	
19	64·28	1·52	27	18·5		57	12·5	12·02	42	11·5	
18	66·01	1·73	44·5	17·5		1 9	12		52·5	10·5	
17*	67·61	1·60				21	12		53 3·5	11	
16*	69·31	1·70	1 18	33·5		33	12		14	10·5	
15*	71·03	1·72	34	16		45	12		24·5	10·5	10·66
14*	72·71	1·68	50	16		57	12		34·5	10	
13*	74·41	1·70	2 5·5	15·5		2 9	12	12	44·5	10	
12*	75·98	1·57	21	15·5		21·5	12·5		55	10·5	10·16
11*	77·67	1·69	36	15		34	12·5		54 5	10	
10*	79·31	1·64	50·5	13·5	14·25	45	11		15	10	
9*	80·94	1·63	3 4·5	14		57	12		25	10	
8*	82·92	1·98	18·5	14	14	3 9	12		35	10	
7*	84·68	1·76	33	14·5		21	12		44·5	9·5	
6*	86·21	1·53	46·5	13·5	14·25	33	12		54	9·5	9·83
5*	87·89	1·68	4 0	13·5		45	12	12	55 4	10	
4	89·50	1·61	14	14		57	12		13	9	
3	91·13	1·63	27	13	13·5	4 9	12	12	23	10	
2	92·83	1·70	41	14		20	11		32·5	9·5	
1	94·48	1·65	54	13	13·5	32·5	12·5		42	9·5	
0	95·96	1·48	5 7	13		44	11·5	12	51	9	9·5

* The divisions marked with an asterisk are curved with a radius of a mile.

In the first experiment the motion was continually accelerated, until the train passed to the succeeding gradient. The acceleration was rapid at first, but gradually lessened as the speed increased, proving a continual augmentation of the resistance. For the last thousand yards of the plane, the acceleration became very small in amount, showing a tendency to an uniform speed, and therefore to an equality between the moving force and the resistance.

In the second experiment, the train being started from the fifty-seventh stake, a more extensive space was allowed for the action of the gravity of the inclined plane. Throughout the first 3300 yards the motion, as nearly as possible, corresponded with the motion of the train in the first experiment, the velocity at corresponding posts being nearly the same. The rate of acceleration, as before, gradually diminished, until the train arrived at the twenty-eighth stake, from which to the foot of the plane the motion was sensibly uniform. From the twenty-eighth to the eighteenth, the rate of motion is 100 yards in a small fraction above 12 seconds, and from the eighteenth stake to the foot of the plane the motion is uniformly 100 yards in 12 seconds, being at the rate of 25 feet per second, or 17 miles an hour.

Hence it follows, that with this train of five wagons, weighing 30 tons gross, with high sides, and presenting a frontage of 47·8 square feet, the whole resistance, at a speed of 17 miles an hour, was equal to $\frac{1}{178}$th part of its weight, or 377 lbs., being at the rate of 12·6 lbs. per ton.

In the third experiment, in which the high sides of the wagons were taken down so as to reduce the frontage or end surface of the train to 23·8 square feet, the motion continued to be accelerated to the foot of the plane; but for the last 1000 yards the acceleration is so little as to be barely sensible. There is a tendency to an uniform velocity of 100 yards in 9 seconds, or 33·3 feet per second, being at the rate of $22\frac{3}{4}$ miles per hour.

If this be assumed as the uniform velocity which the train would have attained had the plane preserved an uniform inclination for a sufficient distance, it will follow that its resistance at this speed, with the reduced frontage, was equal to its resistance at 17 miles an hour with the larger frontage.

Thus, with the same expenditure of tractive power, a diminution of frontage in the ratio of 2 to 1 nearly gives, in this case, an increase of speed in the ratio of only 25 to 33·3.

After descending the plane of 1 in 178, the train in each experiment moved along the next plane, the average descent of which is 1 in 266. The first 1700 yards of this inclination was staked at intervals of 100 yards, and the 50¼ mile-post from Birmingham was 55 yards beyond the 17th stake. The remainder of the plane was divided by quarter-mile posts. In the following table the times of passing the successive posts in each experiment and their differences are given. In the column of mean differences the mean time of traversing a hundred yards is given, the means being taken at intervals as in the former tables.

No. of Stake.	Total Fall.	Fall from Stake to Stake.	Experiment I.			Experiment II.			Experiment III.		
			Time of Passing.	Diff.	Mean.	Time of Passing.	Diff.	Mean.	Time of Passing.	Diff.	Mean.
	feet.	feet.	m s	s	s	m s	s	s	m s	s	s
0						4 44			55 51		
1	1·37	1·37	5 20·5			55	11		56 0	9	
2	2·59	1·22	34·5	14		5 7	12		9	9	9
3	3·64	1·05	47	12·5		19	12	11·7	18·5	9·5	
4	4·72	1·08	6 1	14	13·5	32	13		28·5	10	9·75
5	5·79	1·07	15	14		44	12		38	9·5	
6	6·89	1·10	30	15		56·5	12·5	12·5	48	10	9·75
7	7·95	1·06	45	15	14·7	6 9	12·5		58	10	
8	9·00	1·05	7 0	15		23	14	12·2	57 7·5	9·5	9·75
9	10·14	1·14	15·5	15·5		37	14		18	10·5	
10	11·24	1·10	31·5	16	15·5	51	14		28	10	10·25
11	12·38	1·14	47	15·5		7 4·5	13·5	13·7	38·5	10·5	
12	13·40	1·02	8 3·5	16·5		18·5	14		49	10·5	10·5
13	14·65	1·25	20	16·5	16·2	33	14·5		59·5	10·5	
14	15·79	1·14	36·5	16·5		47·5	14·5	14·3	95	10	10·5
15	16·95	1·16	52·5	16		8 2	14·5		20	10·5	
16	18·25	1·30	9 9	16·5	16·6	17	15		31	11	
17	19·22	0·97	27	18		32	15	14·8	41	10	10·5
50¼	19·28	0·06	44	17		40	8		47	6	
½*	24·36	5·08				9 50·5	70·5	16	59 34	47	10·6
¾*	29·44	5·08				11 6	75·5	17·2	0 22	58	13·2
51	34·52	5·08				12 45	99	22·6	1 17	55	
¼	39·60	5·08				14 31	10·6	24·1	2 0	43	11·8

In the first experiment there is a gradual retardation, which continues until the train is stopped by the brake. At all the velocities, therefore, which it attained, the resistance to its motion was greater than its gravity down the plane.

In the second experiment, where a greater extent of the plane is given for the motion, the retardation is also continued until the train passes to the succeeding gradient. The average speed of the train for the last quarter of a mile is 100 yards in 24·1 seconds, or 12·4 feet per second, being at the rate of 8½ miles an hour. Hence we infer that the resistance to the train at this speed was greater than its gravity down, 1 in 266, which is equivalent to 8·5 lbs. per ton. The total resistance of this train of 30 tons, was therefore greater than 255 lbs. at 8½ miles an hour.

In the third experiment, in which the end surface was diminished, the train attained an uniform velocity at the 10th stake of 100 yards in $10\frac{1}{2}$ seconds, or 28·6 feet per second, or $19\frac{1}{2}$ miles an hour, which it preserved to the foot of the plane. The resistance, therefore, at this speed with the diminished end surface was 8·5 lbs. per ton, and the total resistance was 255 lbs.

It appears, therefore, that with the frontage of 47·8 square feet this train suffered a greater resistance at $8\frac{1}{2}$ miles an hour, than that which it sustained with the lesser frontage of 23·8 square feet at $19\frac{1}{2}$ miles an hour.

In the second and third experiments the train continued to move on the succeeding gradients, and the circumstances of its motion are exhibited in the following table. The gradient of 1 in 330 and the succeeding level are straight.

No. of Posts.	Average Gradient, one in	Experiment II.		Experiment III.	
		Time of Passing.	Diff.	Time of Passing.	Diff.
		m s	s	m s	s
$51\frac{1}{4}$	330	14 31		2 0	
$\frac{1}{2}$	330	17 9	15·8	2 50	50
$\frac{3}{4}$	330	21 24	25·5	3 40	50
52	330			4 31·5	51·5
$\frac{1}{4}$	330			5 24	52·5
$\frac{1}{2}$	330			6 18	54
$\frac{3}{4}$	330			7 15	57
53	330			8 16	61
$\frac{1}{4}$	330			9 22·5	66·5
$\frac{1}{2}$	330			10 35	72·5
$\frac{3}{4}$	level.			11 52	77
54	level.			14 40	16·8
+ 3 yards.	level.			11 55	15

It appears, therefore, that with the greater frontage the train came to rest after having proceeded half a mile on the gradient of 1 in 330. With the diminished surface the motion was gradually reduced to the foot of the gradient of 1 in 330, the average speed on the last quarter of a mile of that gradient being 109 yards in 72·5, or 4·14 feet per second, being nearly three miles an hour. It may therefore be inferred that with the lesser surface, at very slow rates of motion, the resistance was somewhat greater than the gravity down an inclination of 1 in 330. This resistance is at the rate of 6·8 lbs per ton, and the total resistance for the train of 30 tons was therefore greater than 204 lbs.

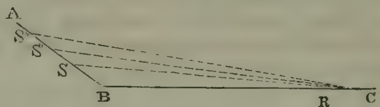
It may be observed that this result is quite in accordance with those already obtained in p. 211 and p. 231, from the experiments down the Whiston plane, and from those of M. de Pambour on the Sutton plane.

In considering these experiments, and in deducing from them any inferences of a general nature, it is of importance to remember that in all of them a wind of unascertained force blew up the planes, and therefore against the motion of the train.

It may be observed that in these experiments no perceptible effect is produced by the curves. The uniform velocity of the train in the second experiment is the same before entering on the curve which commences at the 17th stake on the gradient of 1 in 178, and after passing the 5th stake, where the line becomes straight.

From all these experiments it is apparent that a train of railway carriages in descending an inclined plane is subject to a resistance which is continually augmented as its motion is accelerated, and that if the plane have sufficient length, this resistance will at some certain speed become equal to the gravitation down the plane, and then all further acceleration must cease. This conclusion will be corroborated, and indeed put beyond all doubt by other experiments which are still to be reported. It will be evident, therefore, that if the train on which the experiments were made be started from a point at a sufficient distance from the foot of the plane, the velocity which it will have when it leaves the foot of the plane and commences to move along the next gradient will be the same, whatever may be the point from which it may have been started.

Let A B be the inclined plane down which the train is moved, and let B C be the succeeding gradient. Let S be a point



from which the train being started it will acquire the uniform speed before it arrives at B. Then, if it be successively started from S′, S″, or any other point still more distant from B, it will have, on arriving at B, the same velocity. It will, therefore, in all cases move on B C to the same point before it be brought to rest. Let this point be R.

According, then, to the method of determining the resistance adopted by M. de Pambour (p. 199.), the amount of resistance obtained when the train is started from S would be equal to the gravity on the inclination S R; if started from S′, it would be equal to the gravity on the steeper plane S′ R; if started from

S'', it would be equal to the gravity on the plane S'' R; and, in a word, the value of the resistance according to this method might be found to be of any amount whatever.

The experiments were next directed to the trial of the movement of trains of coaches down the series of planes extending from Madeley to Crewe, already described, and were conducted by Dr. Lardner. A train consisting of one first-class and three second-class close coaches, were loaded in the same manner as the train of first-class coaches used in the experiments already described upon the Whiston plane, the gross weight being 18 tons. The second-class coaches differed in nothing but the structure of their body from the first class, their transverse section being nearly the same. In addition to the fifty-seven stakes by which the plane falling 1 in 178 had been divided, a fifty-eighth stake was placed at the top of the plane, the inclination being found to extend 100 yards higher than fifty-seventh stake. The direction of the line was nearly due north and south, and the wind was from the south, and therefore blowing directly down the plane. No means of ascertaining its velocity could be procured at the time. The train was in each case pushed by an engine to the fifty-eighth stake, and there dismissed to descend the plane by gravity. The time of passing the successive stakes was observed as in the former experiments. In the first two experiments, given in the following table, the entire train was dismissed down the plane, the carriages being coupled by Mr. Booth's patent couplings. In the third experiment the first-class carriage and one of the second-class carriages, coupled, were used; and in the fourth experiment the other two second-class carriages. The entire transverse section of the carriages, including the frame, wheels, and axles, was 61 square feet, and the distance between carriage and carriage, when coupled by the patent couplings, was 3 feet 10 inches.

No. of Stake.	Total Fall.	Fall from Stake to Stake.	Mean Gradient, one in	Experiment I.			Experiment II.			Experiment III.			Experiment IV.		
				Time of Passing Stakes.	Time from Stake to Stake.	Mean Time per 100 yards.	Time of Passing Stakes.	Time from Stake to Stake.	Mean Time per 100 yards.	Time of Passing Stakes.	Time from Stake to Stake.	Mean Time per 100 yards.	Time of Passing Stakes.	Time from Stake to Stake.	Mean Time per 100 yards.
	feet.	feet.		m s			m s			m s			m s		
58				49 58						11 36			30 10		
57				50 20	22					50	14		22	12	
56*	1·63	1·63		36	16		37 13			12 4	14		35	13	
55*	3·36	1·73		48	13		31	18		18	14		48	13	
54*	5·06	1·70		51 1	12		44	13		33	15		31 4	16	
53*	6·68	1·62		12	11		54	10		48	15		20	16	
52*	8·48	1·80	176·9	22	10		38 4	10		13 3	15		34	14	
51*	10·21	1·73		32	10		12	8		18	15		49	15	
50*	12·06	1·85		41	9	10·4	22	10	10·2	34	16	15·2	32 4	15	15·2
49*	13·57	1·51		50	9		31	9		49	15		19	15	
48*	15·30	1·73		59	9		40	9		14 3	14		33	14	
47*	16·98	1·68	176·5	52 7	8		50	10		18	15		49	16	
46*	18·70	1·72		12	5		59	9		34	16		33 4	15	
45*	20·38	1·68		26	14	9	39 7	8	9	50	16	15·2	19	15	15
44*	22·04	1·66		36	10		17	10		15 4	14		37	18	
43*	23·49	1·45		45	9		27	10		20	16		54	17	
42*	25·40	1·91	178·1	54	9		37	10		36	16		34 10	16	
41*	27·14	1·74		53 3·5	9·5		45	8		51	15		28	18	
40*	28·79	1·65		13	9·5	9·4	55	10	9·6	16 5	14	15	43	15	16·8
39*	30·53	1·74		23	10		40 4	9		21	16		58	15	
38*	32·11	1·58		33	10		13·5	9·5		38	17		35 14	16	
37*	33·78	1·67	179·0	43	10		23	9·5		53	15		29	15	
36*	35·50	1·72		53	10		33	10		17 8	15		44	15	
35	37·09	1·59		54 3	10	10	43	10	9·6	24	16	15·3	36 3	19	16
34	38·82	1·73		13	10		52	9		40	16		23	20	
33	40·54	1·72		23	10		41 1	9		56	16		40	17	
32	42·24	1·70	177·3	33	10		10	9		18 12	16		58	18	
31	43·87	1·63		43	10		20	10		28	16		37 15	17	
30	45·81	1·94		53	10	10	30	10	9·4	34	16	16	31	16	17·6
29	47·49	1·68		55 3	10		40	10		19 0	16		47	16	
28	49·23	1·74		13	10		50	10		14	14		38 2	15	
27	50·87	1·64	173·8	23	10		42 0	10		30	16		17	15	
26	52·51	1·64		34	11		9	9		45	15		32	15	
25	54·12	1·61		44	10	10·2	19	10	9·8	20 0	15	15·2	47	15	15·2
24	55·89	1·77		54	10		29	10		15	15		39 5	18	
23	57·58	1·69		56 4	10		39	10		30	15		18	13	
22	59·25	1·67	179·0	24	10		48	9		45	15		32	14	
21	60·93	1·68		24	10		58	10		21 0	15		48	16	
20	62·76	1·83		34	10	10	43 7	9	9·6	15	15	15	40 0	12	14·6
19	64·28	1·52		44	10		16	9		30	15		15	15	
18	66·01	1·73		54	10		26	10		45	15		30	15	
17*	67·61	1·60	179·4	57 3	9		36	10		22 0	15		30	14	
16*	69·31	1·70		13	10		45	9		14	14		58	14	
15*	71·03	1·72		23	10	9·8	55	10	9·6	28	14	14·6	41 10	12	14
14*	72·71	1·68		33	10		44 4	9		42	14		26	16	
13*	74·41	1·70		42	9		13	9		56	14		40	14	
12*	75·98	1·57	179·2	52	10		23	10		23 10	14		53	13	
11*	77·67	1·69		58 2	10		33	10		24	14		42 8	15	
10*	79·31	1·64		11	9	9·6	43	10	9·6	38	14	14	20	12	14
9*	80·94	1·63		21	10		52	9		51	13		34	14	
8*	82·92	1·98		31	10		45 1	9		24 4	13		47	13	
7*	84·68	1·76	172·4	40	9		10	9		17	13		43 0	13	
6*	86·21	1·53		50	10		20	10		30	13		14	14	
5*	87·89	1·68		59 0	10	9·8	30	10	9·4	43	13	13	26	12	15·2
4	89·50	1·61		9	9		39	9		43			38	12	
3	91·13	1·63		19	10		49	10		43			52	14	
2	92·83	1·70	184·0	29	10		58	9		43			44 4	12	
1	94·48	1·65		39	10		46 7	9		43			18	14	
0	95·96	1·48	191·7	48	9	9·6	17	10	9·4	25 49	66	13·2	32	14	13·2

In the first and second experiments made with the train of four coaches, it will be observed that after having descended 800 yards, a velocity of 100 yards in nine seconds was acquired, which underwent a very slight diminution throughout the middle

* The divisions thus marked are curves having a radius of a mile.

of the plane, and subsequently a very slight increase. These fluctuations were, however, so small that they may fairly be attributed to the varying effects of the wind arising from the different exposure of the train in cuttings and on embankments. The velocity, therefore, may be regarded for nearly 5000 yards as practically uniform, the mean rate in the first experiment being 100 yards in 9·74 seconds, or 30·8 feet per second, which is equivalent to twenty-one miles per hour; and in the second experiment 100 yards in 9·95 seconds, or 31·6 feet per second, being 21½ miles per hour. The mean of the two will give a velocity of 51·2 feet per second, or 21¼ miles per hour. Hence it appears that the resistance of this coach train, at a velocity of 21¼ miles an hour, is the 178th part of its weight, or 226·8 pounds, being at the rate of 12·6 pounds per ton of the gross weight.

In the third experiment made with the first-class carriage and one second-class carriage coupled, the speed commencing from the summit was 100 yards in fourteen seconds, which was gradually diminished to a point beyond the middle of the plane, where it was reduced to 100 yards in sixteen seconds. It then slightly increased to 100 yards in thirteen seconds, which was maintained uniform for the last 1000 yards. In the fourth experiment with the second-class carriages, the initial velocity at the top of the plane was 100 yards in twelve seconds, which was gradually diminished, till at the middle of the plane the velocity was 100 yards in 17½ seconds, after which it was gradually but slightly increased to the foot of the plane, where the final velocity was the same as in the third experiment. Considering the lightness of the trains and the consequently increased effect of the wind, these fluctuations of speed probably arose from the varying shelter and exposure of cuttings and embankments in the descent. In the third experiment the mean velocity through 5000 yards was 100 yards in 14·7 seconds, or 20·4 feet per second, being at the rate of 13·91 miles per hour. In the fourth experiment it was at the rate of 100 yards in 15·16 seconds, or 19·8 feet per second, being at the rate of 13·5 miles per hour. The mean of these two is 20·1 feet per second, or 13·7 miles per hour.

Now it follows that, the wind being with it, this train of two coaches suffered a resistance, the total amount of which, at 13·7 miles per hour, amounted to the 178th part of their gross weight, or to 113·4 pounds.

The train of four coaches after having descended the plane falling 1 in 178, as described in the first two of the preceding experiments, was allowed to continue its motion on the succeeding plane, falling at the mean rate of 1 in 266, and extending to a distance of 5360 yards from the foot of the former plane. The particulars of these experiments are given in the following table.

No. of Stake.	Total Fall.	Fall from Stake to Stake.	Mean Gradient, one in	Experiment I.			Experiment II.		
				Time of Passing Stakes.	Time from Stake to Stake.	Mean Time per 100 yards.	Time of Passing Stakes.	Time from Stake to Stake.	Mean Time per 100 yards.
	feet.	feet.		m s			m s		
0			191·7	59 48			46 17		
1	97·33	1·37		57	9		27	10	
2	98·55	1·22		0 6	9		36	9	
3	99·60	1·05		16	10		46	10	
4	100·68	1·08		26	10		56	10	
5	101·75	1·07	259·1	36	10		47 6	10	
6	102·85	1·10		46	10		16	10	
7	103·91	1·06		56	10	10	26	10	10
8	104·96	1·05		1 6	10		37	11	
9	106·10	1·14		17	11		47	10	
10	107·20	1·10	275·2	29	12		58	11	
11	108·34	1·14		39	10		48 9	11	
12	109·46	1·12		50	11	10·8	20	11	10·8
13	110·61	1·15		2 0	10		32	12	
14	111·75	1·14		11	11		43	11	
15	112·91	1·16	262·7	23	12		54	11	
16	114·21	1·30		35	12		49 6	12	
17	115·18	1·97	264·3	47	12	11·4	17	11	11·4
18			266	58	11		29	12	
19			266	58	11		29		
20			266	58	11		29		
21			266	58	11		29		
22*			266	3 45	47	11·6	50 14	45	10·23
50¾			266	4 40	55	12·5	51 7	43	9·77
51			266	5 43	63	14·32	52 6	59	13·41
51¼			266	6 36	53	12·05	56	50	11·36
51½			266	7 38	62	14·09	53 54	58	13·18
51¾			266	8 43	65	14·77	54 54	60	13·64
52			266	9 51	68	15·45	55 55	61	13·86
52½			266	11 3	72	16·36	56 58	63	14·32
+ 80 yards.			266	17	14				

* Stake No. 22 coincides with the 50½ mile post.

In the first experiment the train moved over this gradient with a gradual retarded motion, commencing with the velocity with which it left the former plane, and gradually diminishing in speed until it attained the rate of 100 yards in 16·36 seconds, with which speed it passed on to the succeeding gradient. In the second experiment the train, in like manner, was retarded till it attained the velocity of 100 yards in 14·32 seconds, with which it passed to the succeeding gradient. The difference between the final velocities in descending this gradient in the two experiments must be ascribed to the varying force of the wind, since the train in both was the same, and the initial velocity was not materially different. The mean of the two final velocities is 100 yards in 15·34 seconds, or 19·55 feet per second, being at the rate of 13·3 miles per hour.

Since, then, this train in descending the gradient 1 in 266 had not yet ceased to be retarded, having attained the velocity of 19·55 feet per second, it follows that at this velocity the resistance to the train must have exceeded its gravity down that gradient.

It was now determined to try the effects of four carriages moved separately down the inclined plane falling 1 in 178. The carriages were therefore separately pushed to the summit of the plane and dismissed from it at a high speed, the times of passing the successive posts being observed as in the former case. The results of these four experiments are exhibited in the following table.

No. of Stake.	Total Fall.	Fall from Stake to Stake.	Mean Gradient, one in	Experiment I.			Experiment II.			Experiment III.			Experiment IV.		
				Time of Passing Stakes.	Time from Stake to Stake.	Mean Time per 100 yards.	Time of Passing Stakes.	Time from Stake to Stake.	Mean Time per 100 yards.	Time of Passing Stakes.	Time from Stake to Stake.	Mean Time per 100 yards.	Time of Passing Stakes.	Time from Stake to Stake.	Mean Time per 100 yards.
	feet.	feet.		m s			m s			m s			m s		
58				29 54			40 3			21 43			31 50		
57				30 3	9		11	8		50	7		57	7	
56*	1·63	1·63		13	10		19	8					32 3·5	6·5	
55*	3·36	1·73		21	9		29	10		22 7	17		11	7·5	
54*	5·06	1·70		30	9		37·5	8·5	9·0	16	9		19	8	
53*	6·68	1·62		41	11		46	8·5		25	9		28	9	
52*	8·43	1·80	176·9	50	9		55	9		33	8		36·5	8·5	
51*	10·21	1·73		31 1	11		41 4	9		42	9		45	8·5	
50*	12·06	1·85		12	11	10·2	14	10		52	10	9·0	53·5	8·5	8·5
49*	13·57	1·51		24	12		25	11		23 3	11		33 3	9·5	
48*	15·30	1·73		35	11		36	11		13	10		12	9	
47*	16·98	1·68	176·5	48	13		47	11		21	11		23	11	
46*	18·70	1·72		32 1	13		58	11		31	11		34	11	
45*	20·38	1·68		15	14	12·6	42 10	12	11·2	45	12	10·6	45	11	10·3
44*	22·04	1·66		28	13		22	12		57	12		55	10	
43*	23·49	1·45		42	14		35	13		24 10	13		34 7	12	
42*	25·40	1·91	178·1	57	15		47	12		21	11		18	11	
41*	27·14	1·74		33 12	15		59	12		31	13		30	12	
40*	28·79	1·65		26	14	14·2	43 11·5	12·5	12·3	46	12	12·2	43	13	11·6
39*	30·53	1·74		41	15		25	13·5		25 0	14		55	12	
38*	32·11	1·58		58	17		39	14		14	14		35 10	15	
37*	33·78	1·67	179·0	34 13	15		52	13		26	12		23	13	
36*	35·50	1·72		30	17		44 5	13		41	15		38·5	15·5	
35	37·09	1·59		45	15	15·8	19·5	14·5	13·6			11·0	53	14·5	14·0
34	38·82	1·73		35 2	17		33·5	14		26 8	27		36 6·5	13·5	
33	40·54	1·72		18	16		46	12·5		21	13		22	15·5	
32	42·24	1·70	177·3	34	16		45 0	14		35	14		37·5	15·5	
31	43·87	1·63		49	15		13	13		48	13		52	14·5	
30	45·81	1·94		36 6	17	16·2	27	14	13·5	27 1	13	15·8			11·8
29	47·49	1·68		22	16		41	14					37 23	31	
28	49·23	1·74		39	17		55	14		27	26		39	16	
27	50·87	1·64	173·8	55	16		46 8	13		41	14		55	16	
26	52·51	1·64		37 12	17		22	14					38 10	15	
25	54·12	1·61		29	17	16·2	36	14	13·8	28 5	24	12·8	25	15	18·6
24	55·89	1·77		45	16		50	14		17	12		40	15	
23	57·58	1·69		38 3	18		47 3	13		25	8		55	15	
22	59·25	1·67	179·0	19	16		17	14		40	15		39 20	15	
21	60·93	1·68		35	16		31	14		50	10		25	15	
20	62·76	1·83		52	17	16·6	45	14	13·8	29 4	14	11·8	40	15	15·0
19	64·28	1·52		39 10	18		59	14		16	12		55	15	
18	66·01	1·73		27	17		48 13·5	14·5		28	12		40 10	15	
17*	67·61	1·60	179·4	45	18		29	15·5		42	14		25	15	
16*	69·31	1·70		40 3	18		44·5	15·5		54	12		40	15	
15*	71·03	1·72		21	18	17·8			14·76	30 7	13	12·6	55	15	15·0
14*	72·71	1·68		40	19		49 13	28·5		20	13		41 10	15	
13*	74·41	1·70		41 0	20		29	16		33	13		27	17	
12*	75·98	1·57	179·2	18	18		44	15		46	13		43	16	
11*	77·67	1·69		37	19		59	15		59	13		59	16	
10*	79·31	1·64		57	20	19·2	50 14	15	15·05	31 12	13	13·0	42 14	15	15·8
9*	80·94	1·63		42 17	20		23	9		25	13		29	15	
8*	82·92	1·98		36	19		47	14		38	13		45	16	
7*	84·68	1·76	172·4	57	21		51 3	16		50	12		43 0	15	
6*	86·21	1·53		43 17	20		20	17		32 4	14		16	16	
5*	87·89	1·68		37	20	18·0	36	16	14·4	16	12	12·8	33	17	15·8
4	89·50	1·61		44 0	23		52	16		29	13		50	17	
3	91·13	1·63		23	23		52 8·5	15·5		42	13		44 6	16	
2	92·83	1·70	184·0	46	23		25	16·5		54	12		23	17	
1	94·48	1·65		45 11	25		41	16		33 8	14		41	18	
0	95·96	1·48	191·7	36	25	23·8	57	16	16·0	21	13	13·0	59	18	17·2

* The divisions thus marked are curves having a radius of a mile.

In the first experiment the velocity, commencing with 100 yards in nine seconds, was continually diminished to the foot of the plane, where it was reduced to 100 yards in 238 seconds, being at the rate of 12·6 feet per second, or 8·6 miles per hour. At this speed, therefore, with a favourable wind, the resistance of the carriage used in this experiment was greater than its gravity down 1 in 178. In the second experiment the speed, commencing at 100 yards in eight seconds, was gradually retarded to the bottom of the plane, where it was reduced to 100 yards in sixteen seconds, or 12·75 feet per second, being at the rate of 12·8 miles per hour. Since the retardation had not ceased, the resistance of this carriage at the velocity of 18·8 miles per hour must be greater than its gravity down 1 in 178. In the fourth experiment, likewise, there is a continual retardation, commencing at 100 yards in seven seconds. The velocity was gradually diminished until, at the foot of the plane, it was reduced to 100 yards in 17·2 seconds, being at the rate of 17·44 feet per second, or 11·9 miles per hour, at which speed, therefore, the resistance of the carriage used in this experiment was greater than its gravity down 1 in 178. In the third experiment the speed, commencing at 100 yards in seven seconds, was gradually reduced, about the middle of the plane, to 100 yards in 12·8 seconds. Throughout the last 3000 yards the speed varied between 100 yards in 11·8 seconds, and 100 yards in thirteen seconds, alternately increasing and decreasing, probably from variations of the wind and the varying exposure on cuttings and embankments, accompanied probably with slight changes in the gradient. The speed may therefore be regarded as practically uniform throughout this distance of 3000 yards, and its mean value was 100 yards in 12·7 seconds, or 23·62 feet per second, being at the rate of 16·6 miles per hour.

The coach used for this third experiment was now taken to the top of the plane and there dismissed with the speed of 100 yards in seven seconds, and it was determined to allow it to move along the successive gradients until it should come to rest. To observe the motion, the gradients were staked out in intervals of 100 yards to a distance of 7200 yards beyond the foot of the plane falling 1 in 178. The seventy-second stake was 275 yards short of the 53½ mile post, and the line beyond that was divided by quarter-mile posts. The particulars of this experiment are given in the following table.

No. of Stake.	Mean Gradient, one in	Time of Passing Stakes.	Time from Stake to Stake.	Mean Time per 100 yards.	No. of Stake.	Mean Gradient, one in	Time of Passing Stakes.	Time from Stake to Stake.	Mean Time per 100 yards.	No. of Stake.	Mean Gradient, one in	Time of Passing Stakes.	Time from Stake to Stake.	Mean Time per 100 yards.
		m s					m s					m s		
58	178	12 41			11	178	34	13		36	266	52	16	
57	178	48	7		10	178	46	12	13·2	37	266	34 10	18	
56	178	55	7		9	178	22 1	15		38	266	25	15	16·2
55	178	13 2	7		8	178	15	14		39	266	41	16	
54	178	10	8		7	178	27	12		40	266	57	16	
53	178	17	7		6	178	41	14		41	266	35 13	16	
52	178	25	8		5	178	54	13	13·6	42	266	28	15	
51	178	32	7		4	178	23 9	15		43	266	43	15	15·6
50	178	40	8	7·6	3	178	23	14		44	266	59	16	
49	178	49	9		2	178	36	13		45	266	36 15	16	
48	178	59	10		1	178	50	14		46	266	31	16	
47	178	14 9	10		0	178	24 4	14	14	47	266	45	14	
46	178	18	9		1	266	19	15		48	266			14·6
45	178	28	10	9·6	2	266	32	13		49	266	37 7	22	
44	178	39	11		3	266	46	14		50	266	20	13	
43	178	49	10		4	266	25 2	16		51	266	38	18	
42	178	15 1	12		5	266	17	15		52	266	53	15	
41	178	12	11		6	266	23	16		53	266	38 8	15	14·4
40	178	23	11	11	7	266	49	16		54	330	23	15	
39	178	35	12		8	266	26 6	17	16	55	330	39	16	
38	178	46	11		9	266	23	17		56	330	54	15	
37	178	59	13		10	266	39	16		57	330	39 10	16	
36	178	16 11	12		11	266	56	17		58	330	26	16	15·6
35	178	23	12	12	12	266	27 13	17		59	330	42	16	
34	178	35	12		13	266	29	16	16·6	60	330	59	17	
33	178	47	12		14	266	40	11		61	330	40 16	17	
32	178	17 0	13		15	266	28 3	23		62	330	32	16	
31	178	12	12		16	266	19	16		63	330	49	17	16·6
30	178	25	13	12·4	17	266	36	17		64	330	41 8	19	
29	178	37	12		18	266	53	17	16·8	65	330	24	16	
28	178	50	13		19	266	29 10	17		66	330	42	18	
27	178	18 3	13		20	266	26	16		67	330	59	17	
26	178	16	13		21	266	42	16		68	330	42 17	18	17·6
25	178	28	12	12·6	22	266	59	17		69	330	34	17	
24	178	41	13		23	266	30 16	17	16·6	70	330	51	17	
23	178	54	13		24	266	33	17		71	330	43 10	19	
22	178	19 7	13		25	266	49	16		72*	330	27	17	17·5
21	178	20	13		26	266	31 7	18		53½	330	44 20	53	19·3
20	178	33	13	13	27	266	24	17		53¾	L	45 42	82	18·6
19	178	46	13		28	266	41	17	17·0	54	L	47 52	130	
18	178	20 0	14		29	266	57	16						
17	178	14	14		30	266	32 14	17						
16	178	27	13		31	266	31	17						
15	178	40	13	13·4	32	266	47	16						
14	178	54	14		33	266	33 4	17	16·6					
13	178	21 7	13		34	266	20	16						
12	178	21	14		35	266	36	16						

The coach being dismissed from the summit with a velocity of 100 yards in seven seconds, was gradually retarded until it reached the twentieth stake, when it attained a speed of 100 yards in 13½ seconds. It fluctuated between 100 yards in 13·2 seconds and fourteen seconds to the foot of the plane. The velocity, therefore, for the last 2000 yards may be considered as uniform, its mean amount being 100 yards in 13½ seconds, being the same with the result of the former experiment. At this speed, therefore, with a favourable wind, the resistance of

* Distance from this stake to 53½ mile post = 275 yards.

the coach was equal to the gravity on the plane. As the coach descended the next gradient its motion was again gradually retarded until the speed became 100 yards in seventeen seconds, but was again accelerated until it became 100 yards in fourteen seconds, these fluctuations being probably due to the varying exposure to the wind. The uniform motion down this gradient may therefore, perhaps, be taken as a mean of the varying motion of the train in descending it. This mean would be 100 yards in sixteen seconds, or 12·8 miles per hour.

If it be assumed that the train of four coaches used in the experiments down the Madeley plane, falling 1 in 178, had the same friction as the train of four first-class coaches used in the experiments down the Whiston plane (page 224), the proportion in which the whole resistance is in each case due to friction and the air, may be obtained by an easy calculation derived from the formulæ 26 and 27; making in these the following substitutions,

$$M = 18, h = \frac{1}{96}, h' = \frac{1}{178}, V = 49{\cdot}45, V' = 31{\cdot}2,$$

we shall find

$$f = \frac{1}{409}, a = \frac{1}{17043}.$$

In both these experiments the wind was favourable, but its force unascertained, and in the formula from which these values of f and a have been deduced no allowance has been made for its effect. By comparing the value of f thus obtained with the value of f in page 231, it will be seen that the present value is less in amount, as might be expected, from the effect of the wind.

The resistance per ton due to friction according to this calculation would be 5·48 pounds, and the total resistance from friction for the load of eighteen tons would therefore be 98·64 pounds.

Since the entire resistance of this load at twenty-one miles per hour was found by the experiments to be 226·8 pounds per ton, it follows that the total resistance due to the atmosphere was 128·16 pounds.

Two objections have been advanced against the method of determining the resistance by moving down inclined planes until a velocity be obtained which renders the resistance equal to the gravitation on the plane. The first is, that the engine not being in front of the train, the flat surface of the foremost carriage is exposed to the air, and that a greater atmospheric resistance is thereby produced than would be produced if the engine were in front of the carriage inasmuch as the engine would have, in a greater or less degree, the same effect upon the air as the bow of a ship has upon the water through which it is carried.

The second is, that under such circumstances the moving power acts from behind against the resistance, in the same manner as an engine acts when used to push the train from behind instead of drawing it; that thereby the coaches composing the train are thrown out of square, and the resistance from flange friction, and other causes consequent on such derangement, is increased, and is the main cause of the excessive amount of resistance which has been found in these experiments.

To the first of these objections it may be answered, that the engine and tender placed in front of the train increase the amount of the transverse section by which the air is displaced in the motion of the train; the fire-box and ash-pit extend nearly to the ground, and fill a space which is left almost open in the absence of the engine; the chimney rises above the roof of the carriage and produces a resistance which has no existence in the absence of the engine; the head of the engine is usually flat, and so far as it is concerned produces as much resistance as an equal extent of flat surface upon the foremost end of the coach. The tender which follows the engine presents a concave form to the air, a form considerably more adapted to produce a resistance than the flat end of the carriage which it intercepts.

Up to the period of writing this report no opportunity has been presented to the committee of ascertaining the force due to this objection by direct experiment; but it is intended to place an engine and tender in front of a train, disconnecting the working machinery, so that the engine shall have no other resistance than a coach of equal weight and similar construction, and to repeat the experiment with the engine and tender so placed. It will then appear how far the resistance will be modified by the form of the engine dividing the air in front.

To the second objection it may be answered, that the case of the train moving by gravity down an inclined plane is not analogous to that of an engine pushing a train behind. In the latter case the whole power of the impelling force acts against the end of the last carriage, while the resistances which it has to overcome have their position in the moving parts of each individual carriage, and in the frontage exposed to the resistance of the air. But in the case of a train descending an inclined plane by gravity neither the whole moving force nor any part of it acts against the back of the hindmost carriage: the moving force, being the gravitation of the matter composing the several carriages, will necessarily act at their respective centres of gravity. Thus the force which moves the first carriage will act at its centre, and that portion of it which is expended on the friction of that particular carriage will act in a manner as favourable as a drawing force would act. The same may be said of the force of gravitation of the several carriages; but that portion of the force of gravitation which balances the resistance of the air is subject in a modified sense to the objection. Thus, that part of the gravitation of the second coach which is over and above the resistance from friction, is transmitted to the first coach, and through it to the air which it drives before it; and the like may be said of the gravitation of each succeeding coach. But it should also be remembered that the resistance of the air to a train of coaches does not act exclusively on the front of the first coach. The coaches of the train are nearly four feet asunder, and the air probably acts more or less on the foremost end of each coach. This portion of the resistance is not acted upon with the same disadvantage by the gravitation of the coaches as that resistance which is produced by the end of the first coach.

It is intended to test the force of this objection by moving a train of coaches with an engine along a level, or up an inclination, first placing the engine in front and afterwards behind, and comparing the time taken by the engine to drive the train a given distance under both circumstances: but at the time of making this report the committee had not had an opportunity of making such an experiment.

Whatever importance may be attached to this objection, it is presumed that it cannot for a moment be supposed that the difference between the resistance in pushing a train from behind and drawing it in front can account for the enormous disproportion between the common estimate of resistance, and that which results from the experiments here given, the common estimate being about nine pounds per ton, while that which the trains exhibited moved down the Whiston plane at thirty-two miles an hour, amounted to more than twenty-three pounds a ton, and that even with the advantage of a favourable wind.

A further objection, however, has been made to the effect, that the trains on which the various experiments have been made, especially those with which the greatest velocities were attained, were lighter than trains generally are in railway practice, and that therefore the proportion which the atmospheric resistance would bear to the whole resistance would be greater than in practice it is, for that if the magnitude of the train were increased the resistance from the air would not be proportionately increased.

To a certain extent the force of this objection may be admitted. The fastest trains, however, on the Liverpool and Manchester railway, viz. the 11 o'clock first-class train consists invariably of four coaches and no more. The trains of passengers, however, generally consist of from seven to nine coaches, and it is intended before the next meeting of the Association to extend the experiments to trains of this magnitude. It will then be seen whether the lightness of the train increases the proportionate resistance at a given speed, and if so, to what extent.

A summary of the results of the experiments contained in this Report is exhibited in the annexed table. It is, however, to be regretted that the effects of the wind were such as to render these results not so exact as could be wished. Still they may be regarded as tolerable approximations, all circumstances considered. The experiments which appear to be entitled to most attention and likely to give the most accurate results, were those made with the train of four first-class coaches on the Whiston plane.

In reviewing the results of these experiments, the near agreement of the several values obtained for the friction proper from different experiments by different principles and processes of calculation, is sufficiently striking, and affords a presumption of truth. Before, however, conclusions apparently so much in discordance with all previous estimates of resistance on railways can be accepted with confidence, it will be necessary to multiply and vary the experiments, and more especially to do so with a view to meet the objections which have been brought against some of those detailed in the present Report. Meanwhile, whatever be the source of the resistance to the tractive power, and whatever may be its exact amount, it does appear to be established by tolerably conclusive evidence that the resistance of railway trains at high speeds is considerably greater than the common estimate; and, on the other hand, that at low speeds it is probably less. Should it appear upon further investigation that the motive power necessary on railways has a material dependence on the speed, and that at high speeds, such as velocities from thirty to forty miles an hour, its amount is as considerable as the experiments here detailed would indicate, some important changes must be admitted in the principles which have hitherto guided those who have projected and constructed railways.

If it be admitted that the power engaged in opposing friction forms but a small part of the whole power used in working railways at high speeds, it will become a matter of comparatively small importance to contrive means of diminishing an obstruc-

Description of Train.	Weight.	Frontage.	Wind.	Speed.	Total Resistance.	Resistance per ton.	Friction by Computation.	Friction per ton.	Resistance of air.	Observations.
	tons.	square ft.		miles per hour.	lbs.	lbs.	lbs.	lbs.	lbs.	
I. Five wagons loaded with bricks	31·31	24	Not observed.	12·5	272	8·69	$<$ 205	$<$ 6·6	$>$ 67	Approximation from experiments by M. de Pambour.
II. Same train	25·58	24	Not observed.	13	234·5	9·17	$<$ 130·7	$<$ 5·11	$<$ 104	
III. Four first-class coaches	18·05	61	Favourable.	33·72	421	23·33	100·68	5·58	321·68	
IV. Same train	15·6	61	Favourable.	31·2	364	23·33	95·75	6·14	268·25	The same carriages as in last experiment, but having been lightened by throwing out a quantity of iron chairs with which they had been loaded.
V. Five merchandise wagons constructed with high sides so as to present an enlarged frontage.........	30	47·8	Adverse.	17	377	12·6				
VI. Same train with sides lowered	30	23·8	Adverse.	$22\frac{3}{4}$	377	12·6				
VII. Same train with high sides ...	30	47·8	Adverse.	8·5	255	8·5				
VIII. Same train with sides lowered	30	23·8	Adverse.	19·5	255	8·5				
IX. Same train with high sides ...	30	47·8	Adverse.	0	204	$>$ 6·8	....	....	....	Train came to rest while descending a gradient of 1 in 330.
X. Same train with sides lowered	30	23·8	Adverse.	3	204	$>$ 6·8				
XI. One first-class coach A, and three second-class coaches B, C, D, loaded............	18	61	Favourable.	21	226·8	12·6	98·64	5·48	128·16	Friction deduced from a comparison of this with Experiment III.
Same train	18	61	Favourable.	21·5	226·8	12·6				
XII. One first-class coach A, and one second-class coach B	9	61	Favourable.	13·9	113·4	12·6				
XIII. Two second class-coaches C, D	9	61	Favourable.	13·5	113·4	12·6				
XIV. One first-class coach A, and three second class coaches B, C, D	18	61	Favourable.	19·5	$>$151·6	$>$ 8·4				
XV. One first-class coach A.........	4·5	61	Favourable.	8·5	$>$ 56·7	$>$12·6	....	....	....	The variations of speed in the experiments with single coaches showed sensible effects from the wind. The small amount of the resistance of the coach C probably arose from this cause.
XVI. One second-class coach B......	4·5	61	Favourable.	12·8	$>$ 56·7	$>$12·6				
XVII. One second-class coach C......	4·5	61	Favourable.	16·6	= 56·7	=12·6				
XVIII. One second-class coach D......	4·5	61	Favourable.	11·9	$>$ 56·7	$>$12·6				
XIX. One second-class coach C......	4·5	61	Favourable.	12·8	= 38	= 8·4				

tion already of such trifling amount. The adoption, therefore, of large wheels, of expensive lubrication, of friction rollers, and of other similar contrivances for reducing the amount of friction, will be clearly unadvisable, since such expedients would be attended with much more expense and inconvenience than would be adequate to any effects they could produce in diminishing a resistance already so small.

The importance of low gradients will be diminished. The advantages supposed to attend these are founded on the supposition that the tractive power upon a level requires so great an increase when a moderate gradient is ascended, that either a superfluous moving power must be provided on the level, or that the moving power adapted to the level will be overstrained in ascending the gradient. So long as the resistance on a level is estimated at eight or nine pounds a ton, a gradient rising at the rate of eighteen or twenty feet a mile will require the power to be doubled; but if the whole resistance on the level be considerably greater, and the proportion of it due to friction be small, then a much steeper inclination would be necessary to double the resistance to the tractive power; and, on the other hand, a small diminution in the velocity of the train would compensate for the increased effect from gravitation. In laying out lines of railway, therefore, intended exclusively or chiefly for rapid passenger-traffic, instead of obtaining by a large outlay of capital a road nearly level, steeper gradients would be adopted, and the resistance to the moving power rendered sufficiently uniform by variation of speed. That this has been in fact practically accomplished on some of the more extensive railways now in operation in this country, is within the knowledge of some of the Members of this Committee; and it is hoped that in a subsequent Report they will be enabled to prove it by producing the actual results of such experience.

If it shall appear, as now seems at least probable, that in railway traffic conducted at high speeds the chief part of the moving power is engrossed by the atmospheric resistance, it will be a matter for serious consideration how this resistance can be diminished; and it is evident that, *ceteris paribus*, wide frontage, and therefore increased gauge is disadvantageous.

These are points to the investigation of which the Committee will hereafter devote attention, and it is hoped they will be enabled to lay before the Association such experiments and such results of the practical traffic on railways, as will justify distinct and satisfactory conclusions upon them.

First Report upon Experiments, instituted at the request of the British Association, upon the Action of Sea and River Water, whether clear or foul, and at various temperatures, upon Cast and Wrought Iron. By Robert Mallet, M.R.I.A. Ass. Ins. C.E.

1. The subject of the present report, for the furtherance of which the Association, at its last meeting, made a grant of money, is one of great interest in a scientific point of view, and of paramount importance as an inquiry of civil engineering. Scarcely half a century has elapsed since the adaptation of iron in its various forms to the many purposes of the engineer, upon a scale before unknown, and as forming parts of public structures whose limit of duration was to be measured, not by years, but by centuries, first made it necessary to inquire—What was the durability of the apparently hard and intractable material employed? What were the forces likely to occasion its destruction? How would they act? What would be their results? And what were the means of arresting their progress?

Yet important as a full answer to these inquiries would be, and though the application of iron in construction to harbours and ships, bridges and railways, and the innumerable other contrivances by which the engineer subdues and administers the forces supplied by the Creator to the social wants of man, yet our information upon this fundamental subject is scarcely more advanced than it was twenty years ago; and while the chemist is not precisely informed as to the nature of the changes which air and water (our most universal elements), separate or together, produce on iron, the engineer is without data to determine what limit their corroding action sets to the duration of his aspiring and apparently unyielding structures. The investigation, therefore, is one full of importance to science and to the arts; and although the commands of the British Association, as respects it, have not been neglected, yet the conditions of the subject were such, and the difficulties and delays in procuring the requisite specimens of iron so great, that the following report consists chiefly of a general survey of the present aspect of this field of knowledge, and of the operations commenced or intended by us for extending its boundaries, than of acquisitions already made.

2. It comprises, therefore,—1st, a very brief "précis" of the actual state of chemical knowledge of the subject at large, viz.

of the chemical actions, under various modifications, of air and water upon iron; 2nd, a statement of the experiments upon the large scale which have already been instituted at the request of the British Association; 3rd, a refutation of some fallacies as to supposed methods of protection of iron from the action of air and water; 4th, the suggestion of a proposed new method of protection of cast and wrought iron from these actions, now in progress of experiment; and, lastly, the statement and consideration of such questions upon this subject as still stand in need of experimental answers, and are desiderata to chemical science and to civil engineering.

3. There has been much discussion as to the number and composition of the oxides of iron, arising partly from the difficulty of procuring iron free from foreign matter for experiment, and partly from its oxides combining with each other. They are now reduced to two, viz.

The Protoxide = $Fe\,O$
The Sesqui-oxide = $Fe_2\,O_3$.

The hydrate of the first is not permanent, and its water has not been precisely determined; it is highly probable from analogy that it has the composition $Fe\,O + 2\,H\,O$.

There are two hydrates of the sesqui-oxide, one of which occurs native $= 2\,Fe_2\,O_3 + 3\,H\,O$, and the other formed artificially $= Fe_2\,O_3 + 3\,H\,O$, and others probably exist. These two oxides are capable of combining and forming

Magnetic oxide (native) $= Fe_2\,O_3 + Fe\,O$,
Forge scales (battitures) $= Fe_2\,O_3 + 6\,Fe\,O$.

Other less distinctly ascertained combinations have been described. It is dubious whether forge scales are a chemical combination at all, but rather a mixture of the protoxide and sesqui-oxide in progress of change by cementation into the latter.

4. And first of our chemical knowledge of the action of air and water upon iron.

Of the Action of Pure Water upon Iron.

"Pure water deprived of air does not act on iron at any temperature below 80° Reaum.=212° Fahr., and at that but slowly. The water was freed from any air by Hall, by boiling, and by the action of the iron itself."—(Marshall Hall, *Phil. Trans.* 1818; Karsten, *Chim. du Fer.*)

"At a red heat, and above it, iron is instantly oxidized by decomposition of vapour of water, producing, according to Robiquet and others, $(Fe_2\,O_3 + Fe\,O)$."—(*Journal de Pharm.* 1818.)

The resulting combination, the oxidum, ferroso-ferrique of Berzelius, is, according to him, by long continued watering, converted all into a hydrated peroxide ($Fe_2O_3 + 2HO$). Gay Lussac, however, states, that it is impossible to oxidate iron to its maximum by the action of water, which seems most probable. He states the composition of the oxide produced at 37·8 per cent. of oxygen, which approaches that of the native magnetic oxide. Neither iron nor its oxides are at all soluble in pure water according to Westrumb.

Of the Action of Dry Air upon Iron.

5. Perfectly dry air has no action whatever on iron, nor has dry oxygen below ignition, unless we consider the "*blueing*" of steel as a state of oxidation. Both air and oxygen rapidly decompose iron at and above the temperature of ignition, producing, according to Berthier, sesqui-oxide of iron, quadri-protoxidated $= Fe_2O_3 + 4FeO$.

Mosander's results, however, do not agree with these; he found, that iron oxidated by dry air, at a red heat, produced an outer coat of sesqui- or peroxide, and beneath this, one having the composition ($Fe_2O_3 + 6FeO$).

The extreme slowness with which moderately dry air acts on iron is evidenced by an experiment of M. Zumstein, who fixed a polished iron cross on the summit of Monte Rosa, in the Alps, in August in 1820: on visiting it again, in August, 1821, it was found neither rusted nor corroded, but had merely acquired a tarnish the colour of bronze. The temperature of the air was 21° Fahr. Barometer, 16 inches 42 lines, and height above the level of the sea, 14,086 feet.—(*Bib. Univer.* xxxiii. p. 65.)

6. *Of the Action of Air and Water combined on Iron.*

While at common temperatures, both air and water are separately strictly neutral bodies in respect of iron; yet when acting conjointly, the case is widely different. In general it may be stated that any neutral body, however slight its own electro-positive or negative relations may be in presence of iron and oxygen, will modify the action of these bodies on each other in proportion as it tends to render the oxygen more negative and the metal more positive.

7. It must be confessed that there are many points in the action of air and water combined still in need of being experimentally cleared up. We are enabled, however, to discern the general nature of the phenomena. We are to be understood as speaking, in the first instance, of wrought and malleable iron,

or iron as nearly pure as possible. Air or oxygen dissolved in water is in a condensed state, and hence in a condition peculiarly appropriate to combination. Rain water frequently contains, when fresh fallen, one-fifth of its volume of oxygen.

8. When a piece of iron is immersed in such water, the whole becomes electrically excited. The water, rendered more negative by contact with the iron, repels its dissolved oxygen, while the iron, become more positive by the contact of water, exercises an unusual affinity for the oxygen. Supposing the surface of the metal everywhere uniform, a film of oxide is soon produced over it, and this once effected, decomposition proceeds with increased rapidity; for as every metal is positive with regard to its own oxides, it follows that the film of rust and the iron beneath now form a voltaic couple of greater energy than the last; and whereas the electric energies were before only sufficient to bring the dissolved oxygen of the water into combination with the iron, they now become sufficient to decompose the water itself, and hydrogen commences to be evolved. At this epoch, if the volume of water be not too great in proportion to the iron, and the latter present a large surface, as in the preparation of Æthiop's Martial, considerable heat even is evolved, but the water is previously decomposed in the cold. (Guibourt, *Jour. de Pharm.* 1818.) It is a most remarkable fact, however, that while iron has this vigorous action on water holding air in solution, neither the metal nor its peroxide have any on the *eau oxygené* of Thénard.—(Thénard, *Traité.*)

9. Should the surface of the iron not be uniform in the first instance, as when patches of rust pre-exist upon it, or when one part is much harder or denser than another, these form voltaic elements from the beginning and aid the progress of oxidation. In nearly all specimens of wrought iron, when exposed to the action of water holding air in solution, in addition to the first coat of rust, one of carbon and sometimes, in minute quantity, of oxides higher in the electro-negative scale, are deposited upon its surface, which still further exalt the conditions favouring corrosion.

10. When iron is freely exposed to air and water in a shallow vessel, the result of their reaction is a hydrated peroxide; if, however, the surfaces of the iron are placed near, but not in contact with neutral solids, as glass or porcelain, or the depth of water be considerable, there is also formed a large proportion of magnetic oxide. Becquerel considers this difference to be owing to the increased slowness of action in the latter case, from the greater depth to which the water has to carry the oxygen.

11. It has been doubted by Marshall Hall and others, who assert that nothing but nitrogen is evolved, whether water is ever decomposed by iron at common temperatures, though in presence of air; but independent of the conclusive and simple experiment of Guibourt, of mixing in large bulk iron turnings and water, and collecting the hydrogen, Becquerel is of opinion, that the existence of ammonia in the oxides produced, which was first detected by Vauquelin, is corroborative of the fact, inasmuch asthe water must suffer decomposition as well as the air, in order that the hydrogen and nitrogen may combine to form ammonia. Chevallier and Bousingault also found ammonia in the native oxides of iron; and Austin states, that it is always present when iron is oxidated by air and water. (*Ann. de Chim.* vol. xxxiv. p. 109.) Too much stress, however, cannot be laid upon this argument, as it has been found that rust, in common with other porous bodies, greedily absorbs ammonia and many other gaseous substances.

12. When the action of air and water on iron has taken place with sufficient slowness, the resulting oxides are found crystallized in the form of the native octohedral iron ore. Becquerel describes a case of crystals, of both hydrated and anhydrous peroxides, found united in one specimen of corroded wrought iron from an old chateau of the ninth century. The hydrated oxide would seem here to have been formed first, and afterwards decomposed by the action of the still unchanged iron upon its water.

13. When water contains foreign admixture, the composition of the rust resulting from its action varies accordingly, together with the rapidity of its corrosion; thus, when it contains carbonic acid, the rust contains water and subcarbonate of iron, according to Dr. Thomson; and Soubeiran found rust under such circumstances, formed of the sesquioxide, combined with 3 atoms of water, and containing variable quantities of the sesqui-basic carbonate of iron, and occasionally the carbonate of the protoxide. Carbon is always, silica occasionally, deposited from the iron dissolved.

14. With the exception of those bodies which are occasionally met with in mineralized waters, and of carbonic acid, and the constituents of sea water, that rendered foul by decaying organic matter, and that from mines, all others are rather beside our present object, as modifying the action of air and water on iron.

We proceed, then, to consider the nature and results

Of the Action of Sea Water on Iron

at ordinary temperatures; and although the results of the careful observations of Marcet, Scoresby, and others, show that the water of the ocean is rather denser, or contains more saline matter in torrid and temperate zones than in high latitudes, yet from $3\frac{1}{2}$ to 4 per cent. of solid salts may be taken as the general average. And as these have a complex constitution, so the results of their action on iron and water containing air are complex; and experiments are yet wanting to enable a perfect rationale of the process to be given and its results precisely stated.

15. In the first instance the actions already described, in the case of air and pure water at low temperatures, take place and give rise to the oxides of iron; and as the sea water almost always contains carbonic acid, a portion of these is resolved into carbonate of iron.

16. As in pure so in sea water, when iron is deeply immersed the oxide produced is the magnetic in the first instance, the iron becomes covered with a light buff coat of rust; but if the vessel be shallow, the sesqui-oxide is formed gradually from it. In each case, the first appearance of action which the fluid presents is the formation of numerous slight green streaks in it. These form usually in about thirty minutes from immersion of the metal, and appear to be protoxide in progress of transition into magnetic oxide and sesqui-oxide, of which latter oxides a large precipitate soon forms at the bottom of the vessel.

17. But as sea water likewise contains chlorides of sodium and magnesium, the carbonate of iron is, in part at least, decomposed, and a subchloride of iron is formed which unites with a part of the sesqui-oxide of iron, having previously assumed the state of sesquichloride, and forms with it an insoluble compound.

18. It happens hence, after a mass of iron has lain for a considerable time in a *limited* quantity of sea water, that the latter holds carbonate of soda in solution, and the further action becomes very slow, and that a hydrated carbonate of magnesia has deposited on the iron.

19. It would appear also, that the sulphates are in part decomposed, the sulphuric acid passing to the iron and forming a basic insoluble sulphate, and the lime an insoluble carbonate, with the carbonic acid of the water. But sulphuric acid is by no means *uniformly* to be detected in the ochreous deposit formed by the action of sea water on iron, nor indeed chlorine either. But besides chlorides, sea water contains bromides and iodides, and of the part which these play in the decompositions consequent on the action of iron, it must be confessed we are as yet wholly ignorant. Analogy, however, gives reason to presume they play similar parts to the chlorides.

20. If malleable iron or steel have been subjected to the solvency of sea water, carbon and sometimes silicon are deposited in small quantities; but when cast iron is acted on more remarkable results follow. After it has remained for a length of time immersed, the metal is found wholly removed, and in its place a pseudomorph of its original size remains, as first observed by Priestley, consisting of a carbonaceous substance, analogous to plumbago, mixed with oxides of iron, and which frequently, but not invariably, possesses the property of heating or inflaming spontaneously when exposed to air. There have been unfortunately, as yet, but few cases of this remarkable change, which requires the lapse of time to take place, carefully observed; and it is as yet by no means clear how it is produced, what is its precise composition, or to what is owing the rise in its temperature on exposure to air.

21. It is remarkable, that not cast iron alone is subject to this change; under circumstances but little understood as yet, the purest malleable iron is alike converted into what we shall for brevity call *plumbago.*

Karsten mentions, that when iron, whether wrought or cast, has been long exposed to water holding in solution alkaline or earthy salts, it is at length dissolved; that when hard bar iron had remained some centuries in sea water it was altogether dissolved, and a mass of carbonaceous matter remained, as though it had been submitted to the prolonged action of a diluted acid. This change, he says, is generally attributed to the decomposition of the carbonic acid contained in the sea water; but it is much more likely that, in the long run, the sulphates and chlorides contained in the sea water are decomposed likewise by the iron.

The writer possesses a portion of an ancient anchor taken up in the port of Liverpool, the iron that remained of which was of remarkable purity, and which was converted into plumbago of unusual hardness and brilliancy to the depth of half an inch. This plumbago did not heat on exposure. Its specific gravity is 1·773. This fact militates against an observation made by Hatchet, and repeated by Becquerel, that anchors and other objects of forged iron sustain no alteration in sea water but oxidation, from which we must suppose that the contact of iron and plumbago in the cast iron produces a voltaic current, which accelerates the action of the latter.

Berzelius' opinion is, that the carbonic acid contained in the water dissolves and removes the iron. He quotes an instance of the guns of a vessel which had foundered off Carlscrona, which, when taken up fifty years afterwards, were found nearly wholly converted into plumbago, and which heated to such an extent in a quarter of an hour after exposure as to evaporate the water contained in its pores. He adds, "We know not precisely what passes under these circumstances."—(*Traité de Chim.* vol. iii.) Dr. M'Culloch states, that plumbago thus formed always possesses the property of spontaneous heating. This, however, from the writer's own observation, is certainly erroneous.—(*Edin. Phil. Jour.* No. 14.)

Hatchet examined a specimen of plumbago which remained long immersed in sea water at Plymouth: he found it contained a little chloride of iron, and that it was composed of

Oxide of iron . .	0·81
Plumbago . . .	0·16
	0·97

Dr. M'Culloch made several experiments upon the artificial formation of this plumbago by the action of diluted acids; he found it bore in quantity no definite relation to the species of cast iron from which it was obtained.

Pig iron produced more than that cast into guns or shot; of the latter the blackest varieties, as might be expected, produced the most.

This author mentions a case of its production from the action of London porter on iron, and also of the recovering of some of the iron guns of the Armada off the coast of Mull, which became so hot on being weighed, that they could not be touched. He found that the produce of plumbago from the blackest cast iron, dissolved in dilute acetic acid, equalled the bulk of the iron, and was not pulverulent, but coherent, so as to be cut with a knife.

In some cases the plumbago heated, and in some it did not; in the latter he presumes oxygenation to have taken place during solution.

From his experiments Dr. M'Culloch drew the rather singular conclusion, that the plumbago was the oxide of a peculiar metal, the oxygenation of which produced the heating.

Dr. Thompson, in commenting upon this paper, observes, that M'Culloch appears ignorant of the existence of silicon in cast iron, and of Daniell's experiments upon the subject.

22. Mr. Daniell has given some interesting experiments on this subject in a paper, on the structure of iron developed by solution, in the Journal of the Royal Institution. In this, after describing the formation of this plumbago by the action of dilute acids, and its properties, he gives an analysis of the sub-

stance, and a theory of the cause of its heating on exposure to air.

Hydrochloric and sulphuric acids both produced it. Nitric acid produced it, but in a state incapable of heating in air. It did not lose this property by long exposure in a solution of a salt of iron, or in water. It absorbed oxygen from the air with evolution of heat. In pure oxygen or chlorine it became much hotter, absorbing either: the residue, after absorption of oxygen, was found to contain silex; and Mr. Daniell considers that the plumbaginous compound consists of carburet of iron and silicon, and that, by absorption of oxygen, these became protoxides without separation from the carbon.

The experiments of Berzelius and Strömeyer, however, adduced by Mr. Daniell in support of this view, appear rather to militate against its truth; and however it may be a "*vera causa*" that the presence of silicon may occasionally produce the spontaneous heating of this plumbago, the result of my own experiments prove that it can be produced from many specimens of cast iron which do not contain a particle of silicon.

23. Dr. William Henry has given, in Thomson's Annals for January 1815, an interesting account of his examination of this substance produced from cast iron in a coal-pit shaft near Newcastle-on-Tyne. The cast iron was part of a pipe used to convey the water, and evolved gases from a bed of quick sand; its external characters were the same as those previously described. The specific gravity of the specimen was from 2·008 to 2·155. He states its composition "as iron, plumbago, and the other impurities usually present in cast iron;" his examination, however, was cursory and rather imperfect. The water from the shaft contained 64 grains in a wine-pint of chlorides of sodium, calcium, and magnesium, and of the sulphate and carbonate of lime. He ascribes the removal of the metal to decomposition of the chlorides, and instances their capability of removing the iron from ink. He also adds a case of conversion of cast iron into plumbago by the action of steam and powdered charcoal on it.

24. Dr. Thomson gives in his Annals for 1817 a case of like change, produced, with unusual rapidity, by the action of sour paste, or weavers' "dressing" to cast-iron rollers. The change was so rapid as to oblige the substitution of wood for iron. It is not stated whether the rollers were heated by steam or otherwise, or were at the atmospheric temperature. In the Annals for 1825, a very interesting case is given in a letter

from Charles Horsfall, Esq. to Dr. Traill, in which bars of cast iron of 3 inches broad by 1 inch thick, which formed protectors to the copper of a vessel, to the amount of about $\frac{1}{100}$ of its surface, were, in a voyage of not quite five months, to Jamaica and back, converted into plumbago to the depth of half an inch; it heated on being scraped and exposed to the air when the ship first went into dock. Mr. Brande, in the Quarterly Journal, vol. xii., describes an iron gun which had long lain in water as converted into plumbago to the depth of an inch. I have also been favoured by my friend, Mr. Firmston of Glasgow, with a piece of similarly changed cast iron from the false keel of the John Bull, East Indiaman. In four years this piece of $1\frac{1}{2}$ inch by 4 inches was completely altered through. Its specific gravity is 1·259. I have not yet been enabled to determine the composition of this specimen, or that from the wrought iron before alluded to.

25. Mr. Pepys found cast iron similarly changed by the action of pyroligneous acid (Gill's *Tech. Rep.*, vol. iii.); and I have myself obtained specimens so produced by this acid in a state of vapour. The same change is produced by the vapour disengaged in the roasting of coffee; and a curious case of similar action of sherry wine on wrought iron and steel is to be found in Thomson's Annals. It is also well known that the cast-iron plates at first used in the interior of Coffey's Patent Still, were rapidly converted into plumbago by the action of the low wines and proof spirits. Much more lately cannon shot have been found immersed in the sea, near the site of the battle of La Hogue, converted to the depth of an inch into plumbago, or, according to another statement, all through. The battle of La Hogue took place in May 1692; hence these shots have lain in the sea for a period of about 145 years; it is probable they were thirty-two pound shot, and, if converted into plumbago *all through*, this fact shows that some cast irons may be wholly destroyed in the above period by sea water, to the depth of $3\frac{1}{8}$ inches,—a 32 lb. shot being about $6\frac{1}{4}$ in. in diameter.

26. I have thus collected and given at a tedious length nearly all the cases of this singular change published; they serve as an index for future experiments, and they show how very little we know of the real nature of the phenomena. It is equally obvious that, from the want of precision and of data as to time and surface, &c. in most of the statements, no information is afforded of any use to the engineer.

It strikes one at once, that every author hitherto who has studied this subject has wholly omitted any consideration of a most important result which the carbon in iron, especially cast iron, plays during its solution.

It is established that carbon exists in cast iron, steel, &c. in two states: as graphite or crystallized carbon, disseminated in its mass, giving it brilliancy of fracture, softness, porosity, and fluidity in fusion; and as a definite carburet, combined with a portion of the iron chemically, and mixed mechanically with the remainder.

Now, in the decomposition of iron so circumstanced by air and water, whether in presence of an acid or not, besides the combination of oxygen furnished both by the air and water with the metal, other reactions take place. Nitrogen and hydrogen are both set free, but they may or may not be both evolved. The nitrogen combines with part of the hydrogen to form ammonia, which, according to circumstances, is evolved, or combines with the oxides of iron produced.

But it is probable, from the experiments of Thénard and Despretz, that an azoturet of the undissolved metal may also be formed. Iron at a higher temperature is unquestionably capable of decomposing ammonia and combining with azote, so as to augment its weight by 0·12. But in addition, as the combined carbon is set free from the iron in a nascent state, it seizes upon a portion of the evolved hydrogen, and forms a highly volatile and odorous oily hydrocarbon, while some of the uncombined or suspended graphite, also set free in a highly divided state, combines with another portion of the hydrogen and with oxygen, and produces an extractive matter—apothème of Berzelius, and which differs little from ulmic acid in its reactions. This latter deposits as a brown substance, soluble in alkalis, &c., and *combined with all the magnesia and silica* due to the amount of their bases, which the iron may have contained, if any. The volatile oily hydrocarbon is partly dissipated with the hydrogen evolved, partly swims upon the surface of the fluid, rendering it irridescent, and is partly held absorbed by the porous mass of oxides, carbon, and ulmic acid resulting from the whole reaction. Hence we see that the simple decomposition of cast iron by air and water may give rise to no less complex a result than the following formula indicates:—

$$(F\,e\,C_x + C + S_2 i + M_2 g) + (H\,O + N_2O) = (F\,e_2O_3 + F\,e\,O) + (Fe_2O_3 + 3HO) + (H + C_x.H_y.) + (C_{60}H_{30}O_{15}? + S i O_3 + M g O) + C.$$

And here several of the substances commonly present in cast iron are omitted; if these be included, or an acid present, the result will of course be still further complicated.

27. Now to the presence of these hydrocarbons I conceive we are to look for the phenomenon of the spontaneous heating of the plumbaginous matter, so many cases of the production of which have been adduced. It appears to be owing to their further oxidation, on exposure to air, presenting a great surface to absorption as existing in the porous mass of plumbago, and to be a strict analogue of the cases of spontaneous combustion produced by various fat oils, &c. exposed to air, in contact with cotton, linen, &c. &c., or other carbonaceous bodies exposing a large surface to absorption. The fact that cast iron, which *will* produce spontaneously-heating plumbago, when decomposed by air and water, or by hydrochloric acid, when dissolved in nitric acid, gives a plumbago which will *not* heat spontaneously, favours this view of the subject; the nitric acid supplying the oxygen in the first instance.

This is at present but an hypothesis used in directing experiments now in progress to determine the ultimate constitution of these hydrocarbonous compounds, which have not yet been analyzed, or collected even in sufficient quantity to admit of analysis by others, and to discover the nature of the changes which they suffer in presence of air or oxygen.

28. The analogy of this substance with the carburets produced by the destructive distillation of the iron salts of the organic oxacids and cyanogene compounds, is obvious. These, Berzelius is of opinion, are true carburets, while other chemists conclude them to be mere mixtures of finely-divided carbon with the base of the salt.

I cannot, however, but coincide in the view of Boucharlat, whose experiments lead him to believe them mixtures of carbon, with one or more definite carburets of iron whose properties he has described. If thus, they are analogous to the conditions in which carbon exists in cast iron itself.

29. It also bears a striking resemblance to the powders described by Messrs. Stodart and Faraday, as obtained by the solution of some of their alloys of steel in sulphuric and hydrochloric acids: these were not acted on by water, but oxidated in air, and burnt like pyrophorus when heated to 300° or 400° Fahr., leaving protoxide of iron and the alloying metal. They conclude, that during the action of the acid, hydrogen entered into combination with the metal and charcoal, and formed an inflammable compound, as they found these powders sometimes burnt with flame. By the action of nitric acid on these powders they obtained some fulminating compounds.

I have found that when borate of lead is decomposed by the joint action of charcoal and platina with heat, a boruret of platina is formed, which, on subsequent digestion in nitric acid, becomes powerfully explosive; boron here apparently playing the part of the carbon in Mr. Faraday's compounds.

30. The inflammable powders produced by Magnus, by reduction of the difficultly fusible metals by hydrogen, also connect themselves with the subject. To complete our knowledge of all these remarkable substances, with reference to the immediate subject of this report, will need a careful and extensive series of experiments.

31. There have been but few observations made as to the variations in composition of cast and wrought iron as regards their acceleration or retardation of the action of solvent agents upon it. It is not known at this moment with certainty what properties should be chosen, in either cast or wrought iron, that its corrosion may be the least possible, under given circumstances, when used in construction.

32. Faraday found the alloys of most of the metals he tried with steel much less acted on by moist air than steel unalloyed; but he also discovered the remarkable circumstance, that a very minute quantity of an alloying metal produced an increased action of sulphuric acid on steel, within certain limits; thus, $\frac{1}{400}$ of platina greatly increased the action of the acid on the steel with which it was alloyed; with from $\frac{1}{200}$ to $\frac{1}{100}$ it was powerful; with 10 per cent. of platina there was a feeble action; with 50 per cent. of platina the action was the same as with unalloyed steel; and an alloy of 90 platina and 20 steel was not touched by the acid. In these cases even acids of very weak combining power, as oxalic, tartaric, and acetic, rapidly dissolved the steel. Of three possible modes of accounting for this suggested by Sir H. Davy, Mr. Faraday justly chooses that which supposes the platina in part forming a definite alloy, and the remainder diffused through the substance of the steel; thus forming an indefinite number of voltaic elements. On the first action of the acid some of the particles of platina are denuded, and being strongly negative with respect to the rest of the compound, aid its solution. Upon this ground the action of the excess of platina, in reducing the action, is obviously dependent upon the whole alloy becoming definite again. Solutions of chloride of sodium did not act more rapidly on these alloys than on steel alone.

33. It has been long observed how little liable to tarnish or rust native and meteoric iron are, which contain often as much as 9·5 per cent. of nickel, and variable proportions of chrome and cobalt. The following tables embody perhaps the whole of

our present knowledge on this interesting point, in which the alloying metals are grouped according to their producing an alloy more or less corrodible by oxidizing agents than iron alone.

Alloys more Corrodible than Iron.	Authorities.	Alloys less Corrodible than Iron.	Authorities.
Potassium. . . .	Serullas	Nickel	Berzelius
Sodium.	,,	Cobalt	,,
Barium	Lampadius	Tin	Rinmann
Glucinum. . . .	Davy	Copper	Karsten
Aluminum . . .	,,	Copper and Zinc	Vazie
Manganese. . .	Berthier	Mercury	Berzelius
Silver	Berzelius	Iridium	Karsten
Platina	Faraday	Osmium , ...	,,
Silicium	Berthier	Columbium	Berthier
Antimony. . . .	Serullas	Chrome.	,,
Arsenic.	Berthier		

The metals are here arranged according to their electrical order, beginning with the most positive. In the first column all above silver are positive to iron, and all below it, inclusive, and in the second column, negative to it. It is obvious, however, that this gives us very defective information, as Faraday's case of the platina alloy shows that mere difference in proportion may wholly change the properties of the alloy in this respect. An analogue to the peculiar action of the platina in this case is found in the epigene crystals of native oxide of iron, which are generally auriferous, and have the form of the bisulphuret of iron, whose decomposition the electric agency of the noble metal seems to have facilitated, though present in such minute quantity in an uncombined state.

34. Dr. Faraday also found that the alloys of pure iron were less acted on by moist air than those of steel. It is also exceedingly remarkable, that in respect of corrodibility, the alloys of steel follow a totally different order to those of wrought iron. In the following table the first column shows the order of corrodibility of various alloys with steel, compared with steel alone, commencing with the less corrodible; and the second column shows the electric order of the metals with reference to iron, beginning with the most positive.

Order of Corrodibility.	Electric Order. E +.
Unalloyed Steel.	Iron or Steel.
Steel and Chrome	Nickel
.. .. Silver	Silver
.. .. Gold	Palladium
.. .. Nickel	Platina
.. .. Rhodium	Rhodium
.. .. Iridium	Iridium
.. .. Osmium	Gold
.. .. Palladium	Osmium
.. .. Platina	Chrome
	E –

It is obvious that each of these must be considered, not as a binary alloy, but as a ternary compound of two metals with carbon, or of one metal with a carburet of iron. These tables point out a wide field for experiment of great interest.

We may from these results conclude, that the alloy of iron with any metal in a negative relation to it, unless the alloy be definite, will probably be attended with an increased corrosion of the metal; and that its alloy with a metal positive to it, though it may possibly initially protect the iron from action, will, by its own removal, be likely to render its texture open and porous, and hence more fitted for subsequent solution and removal.

35. M. Vazie has recommended an alloy of brass and cast iron, in other words, a quadruple compound of carbon, copper, iron, and zinc, as a suitable metal for various large works where capability of resisting rusting or corrosion is important. It is stated that experiments made with it on a large scale, at Gleiwitz, in Silesia, were attended with satisfactory results. These require repetition, and much may possibly yet be done in improving the durability of cast iron by minutely alloying it. It will be recollected, that a minute quantity of iridium alloyed with iron confers on it the same power of being hardened by rapid cooling that carbon, boron, and silicon do.

36. The porousness of the crystalline grain of cast iron is frequently very remarkable, and is such as to permit many fluids to enter its pores, and actually saturate the metal like a sponge. A very remarkable case of this is recorded in the Quarterly Journal of Science, vol. ii. p. 385. M. Clement formed a large cylinder of copper, within which he placed a turned cylinder of cast-iron, also bored out; a space intervened between the two, into which he poured melted tin. To his surprise, on becoming cold, much of the tin was squeezed through the cast-iron cylinder, and appeared as a fine filamentous wool, lining

the internal part of the cast-iron cylinder. It was of such tenuity as to take fire and burn at a candle like tinder. It is here obvious that the iron and copper cylinders were heated alike, but the latter expanded much more than the former, and hence, on cooling, compressed the tin (still fluid, probably, about the centre of the length of the cylinder, although cold at its ends), and forced it out through the pores of the iron. The limit of force here was only that of the cohesion of the copper cylinder.

37. It is usually considered an ignorant prejudice of workmen, that a "*hard skin*," as it is technically called, is given to cast iron, after planing or turning, by coating it with oil, and, until a short time since, I was myself of that opinion; but, on examining some broken castings, whose surfaces had been turned and exposed to oil for several years, I found that the oil had penetrated the pores of the iron to a considerable depth.

38. I also possess a piece of cast iron, of considerable thickness, which formed, I believe, part of a furnace for decomposition of sea salt; it contains throughout its mass a minute quantity of chloride of sodium, and a great deal of sulphur. This has been produced by cementation, in the same way as Herapath describes an alloy of

Zinc . . . . . 92·6
Iron . . . . . 7·4=100·0

as being produced in the Bristol Zinc Works; and as alloys of cast iron with arsenic, antimony, and lead have been formed. These observations are intended to show the importance of selecting close-grained cast irons for works designed to resist longest the action of air and water.

39. Of the relative rates of corrosion of the various commercial "makes," or specimens of wrought and cast iron, scarcely anything is known, and that of a very general character. It is certain that the blackest cast irons, viz. those which contain the largest quantity of uncombined carbon or graphite in a mere state of mixture, are acted on by air and water the most rapidly. This has probably partly an electro-chemical cause, and partly a mechanical one, from defects of hardness, and open and porous grain. There can be little doubt but that the suspended graphite in this kind of iron forms the negative element of innumerable voltaic couples which aid the process of oxidation.

The gray or mottled iron most used for castings of machinery and engineering purposes in general, as containing a less quantity of uncombined carbon, and having a denser structure, is less acted upon; and the varieties of iron which present scarcely any symptoms of a crystalline texture at all, but still are *grained* or mottled, and can barely be touched by the file, turned, or bored, are those which, while they are still capable of being used for almost every purpose to which cast iron is applicable, are the least susceptible of alteration or decay.

40. The officers of the French artillery, amongst whom M. Born has been most conspicuous, have made a number of experiments on this branch of our subject. They have found that the corrosion of iron by air and water is greater in proportion to the purity or goodness of the coke with which the iron is made, and that it is altered less when made with charcoal than with coke. In the former case, it is probable this arises from the iron containing the largest dose of uncombined carbon or graphite; and in the latter, namely, in that made with charcoal, it seems to arise from the less quantity of silicium contained in this cast iron. Various careful analyses made by Berzelius, Karsten, Berthier, and others, show that, while coke-made iron contains from 0·025 to 0·045 of silicium, that made with charcoal only contains from 0·002 to 0·013; and it is certain that the presence of silicium disposes iron to corrode, although in dissolving in menstrua it may sometimes act as a mechanical protector, covering it with a coat of silex.

41. M. Born has also observed that iron cast in "*dry sand*," or "*in loam*" moulds faced with charcoal, oxidates much less speedily than when cast in green sand; and that "*chilled*" cast iron, or that cast in iron moulds, is the least of all susceptible of this change.—(*Comptes Rendus*, 1837.)

42. Becquerel, in remarking upon these statements, observes, that cannon which are cast of close gray iron, and in "dry sand," sustain little alteration further than a single coat of rust, or browning like a gun-barrel, which seems to suspend further action. This he attributes to the charcoal facing of the mould; and adds, that if it were possible to carburate the surfaces of objects cast in iron in the operation of moulding, this alone would preserve them from further oxidation. There appears, however, here to be a serious mistake; the presence of a carbonaceous coat on the surface of cast iron, unless impervious to air and water, cannot preserve it from rust, however uniformly spread. That it should do so, would be at variance not only with the observed facts, and with the circumstance that the coat of plumbago formed by the action of sea-water on iron *does not preserve* the remainder, but is at variance with an experiment of Becquerel himself, in which he shows that the application of a piece of common charcoal to the surface of iron in a solution

of sea salt and sub-carbonate of soda greatly promotes its oxidation. Unless, then, it were impervious to the fluid agent, it could never prevent the oxidation, however uniform.

43. It cannot have escaped the notice of any one who has had an opportunity of observing castings, with what rapidity the water of a fresh-fallen shower of rain, which is highly charged with oxygen, attacks fresh-made castings of iron; and this, according to my observation, *more* rapidly in "dry sand" or "loam" castings than those made in damp or "green sand," contrary to the opinion of the French engineers: which I attribute to the circumstance, that in "loam" or "dry sand" moulds, moisture not being present, but little hydrogen is generated by the fluid metal to burn off the "facing" of charcoal, which remains "parsemé" on the surface of the casting, producing innumerable voltaic couples in contact with water; while, in the case of "green sand" castings, most of the charcoal facing is removed in a gaseous form from the casting before it leaves the sand.

44. "Chilled" cast iron, or that whose substance, to a greater or less depth, has suffered an alteration of crystalline arrangement by having been cast in a cold iron mould, is unquestionably that which suffers least change, in a given time, in water charged with air, whether fresh or salt; and this from two distinct causes: first, from its greatly increased density and hardness; and, secondly, from the fact that a very large portion of uncombined carbon is pressed or squeezed out by the expansion of the crystals of iron at the moment of consolidation.

45. I have presented to the Chemical Section two specimens of chilled cast iron, in which, by a little management, this phenomenon has been rendered very apparent. On these the suspended or uncombined carbon is seen exuded in the form of a metalline dew, and adherent to the surface in drops of various sizes.

These specimens are interesting in another point of view, as affording decisive instances of the expansion of iron in consolidating.

I have also in my possession a piece of an unusually dense and white "chilled" iron, of large dimensions, whose entire substance is filled with interspersed octohedral crystals of apparently pure carbon. They are nearly all of equal size, the principal axis of the crystal being about one twentieth of an inch in length. They are hard enough to scratch quartz, and are exceedingly obvious and striking from their dark colour, compared with the iron in which they are imbedded, the grain of which also is brilliant and highly crystalline.

46. It has been considered by most authors that "chilled" or white cast iron contains less combined carbon than the black or gray varieties; this, however, appears to be a mistake; yet it is undoubtedly true that it does contain much less carbon or graphite in a suspended or uncombined state, and that the latter is mechanically expressed and locally deposited in much the same way, as the author has endeavoured to show in another place,—that the contemporaneous quartz veins in granite have been formed from the residual quartz existing in that rock, over and above that which was necessary to the atomic composition of its constituent minerals. It is also analogous to the observation made by Pelletier respecting the combination of phosphorus and silver, viz. that this metal holds more phosphorus in combination, or rather in suspension, while in fusion than when solid, for at the moment of congelation of fused phosphuret of silver it exudes a quantity of phosphorus, which takes fire, unless the whole be suddenly cooled in water.—(*Ann. de Chim.* vol. xiii. p. 110.)

47. By two remarkable properties may the whole of the metals be divided into as many classes, namely, those which pass from the fluid state of fusion instantaneously to the solid; and those which assume the latter state by passing through an intermediate condition of pastiness.

The former property is exemplified in all the metals which crystallize best, as bismuth, zinc, arsenic, &c.; and the latter in potassium, sodium, iron, platina, &c. The power of being welded is entirely due to this latter condition of intermediate pastiness between fluidity and solidity, and hence it is properly not confined merely to metals, for wax, tallow, resins, camphor, caoutchouc, glass, and most vitrifactions, have strictly the welding property.

But it results from this that those bodies which can be welded can scarcely ever be crystallized by fusion and slow cooling, because that pasty and viscid condition they assume before solidification forbids the freedom of motion to their molecules, which is essential to their crystalline arrangement.

But in "chilling" cast iron by sudden cooling, time is not given it to assume the viscid or pasty state; its particles are compelled to pass *per saltum* from a liquid to a crystalline solid, and, *e converso*, by continued cementation at a temperature approaching its fusing point, "chilled" cast iron may be brought back again to the ordinary grain of common cast iron. By this instantaneous change, the crystals of iron in forming, in obedience to the general law, reject and throw out the uncombined carbon as heterogeneous, which itself also assumes the crystalline form. We see here, also, the close analogy to the phenomena and properties of unannealed glass, that is, of silicates which have been compelled suddenly to pass from the semi-fluid state to that of solidity, without passing through the pasty condition. In these the crystalline arrangement is distinctly produced, as Sir David Brewster has determined by the optical examination of Prince Rupert's Drops, and yet these, by being annealed, are brought back to the state of ordinary glass.

48. Hence, then, the superior hardness and greater specific gravity of chilled cast iron. But while it accrues, from the chilling of cast iron, that it corrodes less rapidly, a singular and in some cases disadvantageous circumstance occurs in the manner of its corrosion, which needs further consideration, as it may sometimes happen to be of much greater practical importance than any amount of mere decay of substance.

49. When a piece of cast iron moulded in sand is exposed to corrosion, this takes place with somewhat variable, but yet with very considerable uniformity over its entire exposed surface. Not so, however, with chilled castings; in these, each nucleus of exudation of its uncombined carbon forms with the iron in its immediate vicinity a voltaic couple; and its results are, that, in place of the uniform action as before, the largest portion of the surface remains unchanged, and corrosion is nearly wholly confined to these spots, as so many local centres of action. The oxides of iron are formed as usual; but, from the texture of the casting, and its constitutional carbon being all in a state of combination, little or no carbonaceous or plumbaginous matter is produced; hence, as the spots of carbon form, with the rest of the casting, so many voltaic couples, the oxides formed are rapidly transferred to these points, and gradually produce large tubercular concretions, which, M. Payen states, always consist of $(Fe\,O) + (Fe_2\,O_3 + Fe\,O) + (Fe_2\,O_3)$, and which in course of time contain crystals of the octohedral iron ore thus artificially produced.

50. M. Payen has presented several memoirs to the Academy of Sciences on the subject of these local actions on iron, and has applied to their explanation the facts he had previously discovered, as to the effects of alkaline and saline solutions in retarding or accelerating the corrosive action of water on iron. The principal facts he has established are the following, as given by the Commission of the Institute, in reporting on his memoir.—(*Comptes Rendus*, Feb. 1837, No. VI.)

"He has found solutions, containing an alkali and sea salt in such proportions, that, in place of being at all preserved, iron placed therein was rapidly attacked.

"A cylinder of iron, filed bright, is preserved for a long time from all alteration when plunged into a solution of pure potass diluted with one thousand times its weight of water; but if the solution be left in contact with the air, the alkali, absorbing carbonic acid, by degrees loses its preservative power.

"When the water contains $\frac{2}{100}$ of its volume of a saturated solution of carbonate of soda, it forms conical concretions of oxide," &c.

What is more remarkable in this mode of alteration is, that all points of the surface of the metal are not equally attacked; the action commences where there are breaks of continuity, or where foreign bodies are deposited, which constitute, by their contact with the iron and the liquid, a voltaic couple: all the rest of the surface preserves its metallic lustre.

"A saturated solution of sea salt preserved from contact of air only produces some excrescences of oxide of iron; while, if exposed to air, oxidation goes on as usual.

"When this solution is saturated with carbonate of soda it possesses the property of preserving iron from all alteration, even though exposed to air, but it loses it when the solution is diluted.

"It might be presumed that this difference arose from the saturated solutions containing less air than the dilute, but this cannot be so, since M. Payen has proved that the proportions of alkaline bases capable of arresting all oxidation eliminate but a very small portion of the air contained in the water.

"M. Payen has determined the proportions of sea salt and sub-carbonate of soda which *accelerate* the formation of tubercles (or local corrosion) most. A solution of these two salts, diluted with 75 times its volume of Seine water, produced, in less than a minute, on cast and wrought iron, a commencement of oxidation, indicated by light green points, which, in less than ten minutes, formed visible projections."

The effect is increased by applying to the surface a fragment of charcoal, in which case a voltaic circle is formed;—"Hence," says the Report, "in similar circumstances, cast iron will alter more rapidly than pure iron."

We see, then, that solutions which have a feeble alkaline reaction possess the property, in presence of air and sea salt, of producing on cast and wrought iron, which they moisten, local concretions which preserve the remainder of the surface from all change; and that these effects vary according to the proportion of the different salts, breaks of continuity, and the foreign bodies adherent to the surface of the metals.

"M. Payen thinks that the concretions formed in the pipes which supply Grenoble with water (presently to be described) are formed in this way, the waters which pass in them having a feeble alkaline reaction, owing to the presence of carbonate of lime, and being slightly saline.

"At the suggestion of the Commission, M. Payen incrusted pieces of wrought iron in cast iron, and fragments of cast iron in plates of cast iron of another sort: in all these cases he found the tubercular oxidations adhered to the points of contact.

"It may be concluded (they continue), from the observed facts, that whenever there is a want of homogeneity in cast-iron pipes, which convey water slightly alkaline and saline, tubercles will be formed at the points where heterogeneity exists. M. Payen has studied the circumstances in which, as regards the formation of tubercles, white cast iron produces the same effect as gray or black;" having diluted one volume of a solution of carbonate of soda and of chloride of sodium, saturated at a temperature of 15° Cent., with from 100 to 200 volumes of distilled water, he has found that all the solutions between these limits produce on white cast iron oxidations evidently more tubercular and better localized than on the other kinds of cast iron. These last afford more points of easy attack, and produce more numerous tubercles, and hence less distinct.

"We see, then, that white cast iron, as being less oxidizable by certain mineral waters, appears to merit the preference over gray iron for water-pipes."

The reporters further add, "We are not obliged to say that the constitution and composition of the *artificial* tubercles are the same as those of the pipes at Grenoble, which would tend to prove that both depend on like causes."

51. This local action, in many cases, is of small importance, and indeed might be more advantageous than that deep removal of metal which takes place in softer irons; but in the case of pipes or similar receptacles for the containing or conveying of water, the accumulation of these tubercular excrescences gradually chokes the passages at numberless places, and obliges the removal of the whole conduit. Nor does this evil solely apply to chilled cast iron; all hard cast iron which has been rapidly or unequally cooled is *pro tanto* liable to the same.

52. M. Vicat, in the *Comptes Rendus*, vol. iii. 1836, p. 181, gives an account of the formation of these tubercles on the pipes which supply Grenoble with water, in which they had increased to such an extent as seriously to reduce the delivery of water, and engaged the attention of the authorities. He proposed as a remedy the coating of the pipes inside with hydraulic mortar to the thickness of $2\frac{1}{2}$ millimeters,—in fact, to brush them over

inside with Roman cement. This mode would no doubt for a time diminish corrosive action, but it is much to be feared that it could have but little permanence when the current was rapid; and, should the water contain much earthy matter, the tendency of *this* to deposit and adhere to the pipes must be fatally increased.

53. The Academy appends a note to M. Vicat's communication, in which an opinion is expressed, that the tubercles of Grenoble have attained their largest size, and are stationary; and it is questioned, Will they always remain so? It must be obvious, indeed, that the rate of increment of these must be a decreasing one; but I do not perceive anything to set a limit to their accretion, except the stoppage of corrosive action.

54. In the same volume of the *Comptes Rendus*, p. 462, a letter is published, from M. Prunelle, stating a case in which tubercles had formed in conduit pipes where the water passing was found not to contain a trace of iron. He did not chemically examine the tubercles, which were friable, and as large as eggs. From this it would seem to be the author's view that these masses originate from iron contained in the water; that this, however, is not the nature of their formation, has been already shown, and is evidenced by the fact, that no tubercles are found in any of the pipes conveying the waters of Grenoble except those made of iron.—(Payen, *Ann de Chim.* vol. lxiii. p. 409.)

55. The explanation of the phenomena of tubercular corrosion given by M. Payen and the reporters to the Academy, seems to lose clearness in proving too much. Mere want of homogeneity of structure or of surface is alone sufficient ground to explain the results; and that the peculiar preservative action of alkaline solutions is not a necessary adjunct, I have lately had an opportunity of proving. In experimenting on the action of very dilute hydrochloric acid, on wrought and cast iron partially coated with zinc, I found that, after a time, local concretions or tubercles were formed at the points of contact of the zinc and iron; thus the effect is produced indifferently in acid and in alkaline solutions. And I have since found tubercles formed in iron pipes at Curraghmore, the seat of the Marquis of Waterford, by water, which appears free from alkaline or earthy matter. The peculiar effect, too, is confined to chilled or unequally-cooled cast iron, to mottled cast iron, and to damasked wrought iron, or that of mixed constitution, and in all appears to result from heterogeneity of composition; it is therefore unnecessary to call in to the aid of the explanation the distinct and curious phenomena of the preservative action of alkaline solutions.

In further corroboration of which may be again noticed M. Payen's own experiment,—that wrought iron locally encrusted with cast iron, by dipping into the latter while fluid, or pieces of cast iron cast into and surrounded by plates of a different sort of cast iron, are liable to like tubercles, which attach themselves to the points of contact; reasoning from which, Becquerel rightly concludes, that *want of homogeneity* is the cause of this peculiar action.

56. M. Payen, as we have seen from his experiments to determine the conditions in which white or chilled cast iron would be acted on like the darker-coloured varieties, concludes very erroneously on the practical maxim, that it is much "better to make conduit pipes of white cast iron than any other, as various mineral waters will oxidate it less"—and truly they will so; but as the peculiar nature of the oxidation in this case stops up the pipes at intervals, any amount of uniform corrosion which merely sets a limit to their duration is to be preferred to this, which renders their entire object nugatory.

57. A letter is found in the *Comptes Rendus* for 1836, p. 506, from Sir John Herschel, stating that the pipes supplying Cape Town with water had become tubercular; and that, at his recommendation, the engineer, Mr. Chisholm, had remedied this by coating the pipes internally with Roman cement. The ancient pipes in the streets of Dublin are likewise much affected in this way; and some fragments of the tubercles, and a piece of the cast iron, which is of the white variety, were presented to the chemical section at Newcastle.

58. Another method for preventing tubercular concretions has been employed successfully by M. Juncker, at Huelgoat mine, in France, viz. that of impregnating the cast iron of which the pipes are made with linseed oil, rendered drying by litharge, and caused to penetrate the pores of the iron by great pressure. This fact is confirmatory of a preceding observation I have made, as to the permeability of cast iron to many substances, and seems to offer a new field of investigation, as a method of protecting cast iron from the action of corroding agents. We know that it has been long usefully applied to other hard and crystallized substances, as stone, marble, &c.

59. Another method, wholly mechanical, is described in the Mining Review as in use in Cornwall for the preservation of iron pipes, and must be eminently useful in many cases.

Each length of pipe is lined with a thin tube of wood, consisting of staves of pine, of equal length with the pipe, driven in from the end, and to which the iron pipe forms, as it were, one elongated hoop. The pine staves are driven in when very dry. On being wet, they expand, and force themselves up so close to the interior of the pipe, and at their joints, that the whole cast-iron pipe becomes staunchly lined with a casing of wood, which cuts off all communication with the corroding agents.

60. The difference in the rate of action of air and water, but still more of acids, on different specimens of cast iron, containing variable minute quantities of foreign matter, is very remarkable. The iron obtained by the remelting of old coal-gas retorts is of a quality closely approaching what is called "Refinery Pig, or No. 4;" in fact, by a little management, it may be forged at once on the anvil into a bar. It is found to contain a large quantity of sulphur, and an unusual amount of silex, in some cases as much as 18 per cent., with very little carbon.

A fragment of this iron placed in hydrochloric acid, diluted with 15 volumes of water, will not be dissolved for months, a coat of silex being apparently formed; while a fragment of the iron before mentioned as containing chloride of sodium in minute quantity, with a large proportion of sulphur and carbon, will dissolve in the same acid almost as readily as sugar in water.

Sulphur, also, which acts on wrought iron with such intensity at a bright red heat, acts on black cast iron, according to Colonel Evans, at the same temperature with comparative feebleness.—(*Ann. de Chim.* vol. xxv. p. 107.)

61. Both these highly sulphuretted irons seem to coordinate with the highly carburetted ones, or black cast irons, the sulphur appearing to form a definite compound, intermixed with the rest of the substance mechanically.

62. Of the relations in respect of corrodibility subsisting between the different known varieties or "makes" of bar iron and of steel, but very little is established with any certainty. The hardest kinds of malleable iron generally appear to oxidize slowest, but this is not universally true, as Swedish iron is acted on by air and water with great rapidity.

63. Karsten states that cold short iron is rapidly corroded. Steel appears less corrodible than any variety of wrought iron; but of none of these is there any precise knowledge on our subject, or that approaches numerical results, which alone are of practical use in directing the engineer, or indeed the *physicien.* Neither is our actual knowledge more advanced as to the variable effects of corrosive action on the same iron, of different waters, such as are commonly met with, containing their usual mineral ingredients in solution. We know not whether foul water, or

clear water, strongly saline, or merely brackish, calcareous, alkaline, or chalybeate, holding much or little carbonic acid, acts most powerfully in producing rust, that is, produces the largest quantity in a given time from any one specimen, or in what relative degree. This statement is made exclusively, of course, of the better-understood cases of mine-waters, such as solutions of sulphate of copper, &c., whose action on iron belongs to another branch of our subject.

64. There are some cases of local action on wrought iron, however, which appear remarkable, and need investigation. The very purest and finest specimens of wrought iron, when exposed, with turned or bored surfaces, to fresh water, frequently corrode entirely locally, by deep and destructive *pitting*. A portion of a turned wrought iron valve was presented, acted on in this way, in about two months, by a remarkably pure stream of fresh water, holding nothing but air and a trace of carbonate of lime and iron in solution. This sort of reaction appears the converse of M. Payen's tubercles, and its explanation would seem to lie in the iron so affected possessing a *damasked* structure, that is, in fact, being composed of two sorts of iron chemically different, and united by welding. This is unavoidably the case with all "scrapped" wrought iron, or that forged or rolled from scraps of various sorts. Now these differently-constituted irons, being in different electrical relations, give rise to a voltaic circuit, in which the most positive is corroded fastest; but as all the surface is in some degree affected, the oxides formed cannot adhere, and hence, while pitting goes on in some places, tubercles are formed in none. This view suggests the importance of having large bars, which require to be formed by rolling several smaller ones together, formed from iron all of the same "make," if for any important purpose as regards durability.

It shows that rolled bars, as being more uniform, are preferable, for the same reason, to "scrapped" or hammered iron; and it points out that the practice adopted in some cases of making railway bars by rolling together two bars of different sorts of iron, the one hard and rigid, to give a durable upper or working face to the bar, and the other tough and soft, to resist extension, is highly objectionable as regards the duration of the rail, under the influence of air and moisture—eminently so, because the lower segment of the rail, viz. the extended part, will corrode the fastest, thus losing strength where it is most wanted.

65. There were also presented the two extremities of a bar of wrought iron found at the bottom of a very deep well in a

brewery in Dublin*, where it was supposed to have lain in about 20 feet of water for nearly eleven years.

The bar lay approximately due north and south, horizontally. When taken up it was found powerfully magnetic, with polarity; and though the bar was throughout equally exposed, corrosion had alone taken place at its two extremities. The ends were injudiciously cut off, by which its magnetism was almost destroyed. This curious subject needs further elucidation. It is doubtful if any authentic instance has yet been given in which magnetism appeared *directly* to play a chemical part. And the question is one of considerable importance as regards our subject; for, as it is well known that bars of iron, by long standing in the vertical or certain other positions, acquire magnetic properties; if it should be found that magnetic polarity exercises any direct effect upon chemical agencies, then it may result that the duration of a structure in iron may in some degree depend upon the position of its parts with respect to the magnetic poles of the earth.

Long since, Professor Maschmann, of Christiana, published some curious experiments, which were confirmed by Hansteen, on the effects of a magnet on the crystallization of the Arbor Dianæ, in a U-shaped tube of glass, beneath or above which it was placed. (Quarterly Journal of Science, vol. xvii. p. 158.) His results, however, were denied by some British chemists at the time.

66. Since that time, the investigation of this influence, which has been repeatedly asserted and denied, has been undertaken in a very careful and particular manner by Professor Erdmann. He first points out the number of delicate perturbing causes which may, and have occasionally led to mistakes, pointing out the effects produced by irregularity in the wires, handling them with the uncovered fingers, &c. &c.; and especially states that many repetitions of each experiment should be made. The bars and magnets which he had occasion to use were very powerful, some of them competent to lift 80 pounds.

I. By experiments made to ascertain the oxidation of the iron wire, when under the influence of terrestrial magnetism, it was ultimately proved,—1st. That the oxidation of iron placed under water is not at all influenced by terrestrial magnetism. There is no point of the horizon towards which it is more strongly or more quickly produced than towards another. 2nd. The oxidation arising from unequal contexture of the iron *always begins at the point where the wire is in contact with*

* Messrs. Guinness, James' Gate.

other bodies, not only metals, but even wax or baked earth. 3rd. Diffuse daylight, or the weakened rays of a winter sun, neither retard nor assist oxidation, provided they are accompanied by no change of temperature.

II. In experiments made with magnetized wires, the results were the same; no difference of oxidation occurred at the poles or other parts.

III. In experiments on the reduction of metals by the humid process, as in the Arbor Dianæ, no influence of terrestrial magnetism could be observed. The crystallization took place in both branches of the syphon tube, and without reference to their direction.

IV. In repeating the experiments with the additional power of a very large magnet, its poles proved not to have the slightest power over the formation or disposition of the crystal within.

V. Numerous salts were made to crystallize slowly in vessels placed over the poles of magnets, with every care that their power as conductors of heat should not interfere. The magnetism exerted not the slightest influence over the crystallization. In chemical actions, where gas was evolved, no difference in the rapidity of evolution, or quantity of gas produced, occurred, when magnets were present or absent.

VI. No evidence of the influence of the magnetic poles over the colours of vegetable solutions could be obtained. — (***Bib.*** *Univ.*, xlii. p. 96.)

67. These apparently satisfactory experiments of Professor Erdmann are, however, again laid open to discussion by some curious results stated by M. Levol (*Annal. de Chim.* vol. lxv. p. 285), in a paper "On the phenomena which accompany the precipitation of a metal in the metallic state by another, in presence of a third metal, exercising no chemical action; and on the circumstances which may modify the results." In this the following statement occurs:—

"I found a circumstance which appeared to me very curious. It is, that the position of the iron, during the precipitation (all other things being equal), is by no means indifferent as regards the separation of the copper (viz. from its solution).

"In varying in different ways the experiments which were requisite, I have observed that the results, which were accordant when I plunged the iron horizontally, ceased to be so when (making a double experiment) I placed in one the iron horizontally, and vertically, or nearly so, in the other.

"In the first case I have constantly more copper on the platina (*i. e.* the passive metal), less iron dissolved, and delay of the complete precipitation.

"Insulation, or free communication with the substance of one of the two metals, having scarcely any influence on these sorts of reactions, as I have assured myself, and which also conforms to the properties (*propriétés*) of electric currents, I have thought that these variations might result from magnetism, acquired by the iron placed vertically; and, to try if in fact it had any influence on the decomposition of the salt, I plunged, in this position (viz. vertically), a bar of soft iron in a tube of glass containing a neutral solution of sulphate of copper, and corked it. As scarcely any disengagement of gas agitated the liquid, I saw that decomposition began towards the two extremities of the bar, advancing progressively towards its middle point; the two extremities and this point acting meanwhile on a magnetic needle, as the two poles and the neutral point of a magnet, and the poles changing by reversal in the usual manner. The effect of this magnetization appeared then to augment chemical action, and hence to diminish the quantity of copper deposited on the platina."

Are we to conclude from this that magnetism modifies chemical action, or that chemical action is capable, under certain conditions, of conferring magnetism on iron?

The subject is obviously in need of much further experiment, and is one of interest in a general, as also in our particular view.

68. It will be now necessary to state the nature and extent of the experiments upon the large scale which have been instituted at the desire of the Association, and aided by its funds. On the very first consideration, it appeared important that those experiments in which time was an element should be first of all put in progress; and of these the most important seemed to be,

1st. To obtain experimentally a set of numerical results of the relative rates of corrosion of all the different most important makes of British cast iron, exposed under the same circumstances to sea water, and unprotected, except from mechanical abrasion; and a like set for fresh water, varying the conditions in both cases as far as might occur in practice.

With this view complete sets of authenticated specimens of cast iron from most of the principal iron-works in Britain have been written for and obtained. A very considerable delay necessarily occurred in procuring these, and it was not until about two months since that it was found practicable to complete the collection, which numbers between eighty and ninety specimens.

These specimens were then all fused, at the works with which the writer is connected, separately in crucibles, to avoid change of composition by contact with the fuel, and cast in green-sand moulds into the form of parallelopipeds, of 5 inches by 5 inches × 1 inch thick, and of 5 inches by 5 inches × $\frac{1}{4}$ inch thick, respectively; and at the same time a bar of 1 inch square and 12 inches long was cast of each description of iron. The whole of these specimens, whose surfaces are as nearly equal as possible, were then weighed each to a single grain, or within about $\frac{1}{10,000}$ of the weight of the piece, and inclosed in the external frame of the box (fig. 1, as shown in Plate XVII.), No. . This box is so contrived as to permit free access of air and sea water at all sides, and while the specimens of iron are held fast at four of their angles, they are freely exposed to the action of these agents; but each in a separate cell, for a reason to be hereafter mentioned. As any mode of numbering these specimens would be inadmissible, if not impracticable, they are to be recognised when reexamined solely by their place in the box. The series commences at A. fig. 1, and reads from left to right, going upwards, as more particularly described in the notes attached to the tables. No iron used in the construction of the box enters its interior, and the specimens, with the frame in which they are arranged, can be lifted out at any time for inspection, without disturbance of their position or touching their surfaces. This box, like the others to be described, is of stout oak kyanized, which, although the researches of Lassaigne upon this subject, showing that the combination of albumine and bichloride of mercury is soluble in alkaline chlorides, renders it probably of less service in sea water, yet is not likely to interfere in any way with the results of these experiments.

69. The object of casting the parallelopipeds of two thicknesses, viz. 1 inch and $\frac{1}{4}$ inch, is, that the "grain" or crystalline arrangement, and proportion to the *metal* of "skin," as it is technically called, varies with the scantling of the casting: hence these thin castings will give results discovering what variety of British metal produces a skin best calculated to resist corrosion, and what amount of variation of skin each sustains by difference of thickness in casting.

70. The castings were all poured, as nearly as possible, at the same temperature, (the crucibles having been heated in draught furnaces,) and all permitted to cool at the same rate.

Their forms being all regular, and their dimension and weight known on again weighing after taking up and cleansing from adherent matter, a set of numerical results will be obtained, giving the relative rates of degradation in sea and fresh water, of most of the British cast irons per unit of surface; thus enabling the engineer to choose that which will be most durable in his structures, and enabling us, by analysis of the least and most corrodible, to see on what these properties depend. Hot and cold blast iron are included in this box, and also specimens of the same cast iron chilled, and cast in loam, dry sand and green sand, cooled rapidly and slowly, and some with protected surfaces, (not electro-chemically protected however,) as will be again alluded to. This box was sunk and moored in Kingstown harbour in 3½ fathom water, at half tide, at the second mooring buoy in from the western pier head, at one o'clock, on the 3rd of August (1838), on a bottom of clean sharp sand; temperature of the water, 58° Fahr. It is proposed being weighed and examined once every six months if possible, and at the expiration of a year the specimens weighed accurately and again put down, and so weighed year by year for at least four years. By this, not only the actual amount, but the rate of progress of corrosion on every specimen will be determined.

71. The object of casting the inch-square bars 12 in. long, of each sort of iron, at the same time with those exposed to the sea and fresh water, is in order to have specimens, comparable in all respects with these as to constitution and texture, whose specific gravities have been, or are in progress of being, taken, and whose chemical or physical properties can be in future determined, should the progress of the experiments on the exposed pieces render such desirable.

There is the utmost variability of structure and composition amongst these specimens, as will be observed by referring to the column of observations in the table, but can only be fully perceived by inspection of the castings, when fresh broken. As an accurate knowledge of the specific gravities of these specimens was of some importance in several respects, and chiefly as being a check upon the weighings, and upon the soundness and dimension of the rectangular pieces of iron, submitted to experiment, some pains were taken in arriving at the best and most expeditious mode of proceeding.

The common mode of taking the specific gravity of solids by weighing in air, and then suspending from a silk fibre or hair in water, is subject to many inconveniences and sources of error, as the variable quantity of the suspending thread wetted and immersed, its capillarity and the resistance of the fluid to free vibration of the beam, &c. &c., a modification was therefore adopted of the plan of weighing the solid in air and immersed in a given volume of water, which is weighed along with it.

But as the cast irons, if broken into fragments sufficiently small to go into a common specific-gravity bottle, would be likely to involve air bubbles hard to be extricated, each specimen of cast iron whose specific gravity was required was filed accurately by a steel gauge to a cube of 0·75 of an inch; this cube was weighed in air at a temperature of 60° Fahr. A small glass cylinder was provided capable of being closed air tight at its open end by a circular disk of thin Bohemian plate glass, equal in diameter to its exterior. The size of the cylinder was such as just to admit, without contact at the angles, a cube of the above size, and its weight with the plate-glass cover, was under 100 grains when empty. Its weight was then accurately determined from the mean of a number of weighings, when quite full of distilled water, free from air, at 60° Fahr. The filling is easily accomplished by pouring in the water after having been boiled in vacuo and cooled, until its surface rose a little above the edge or top of the cylinder, and then sliding on the glass plate. A number of the iron cubes having now been weighed in air were thrown into a considerable volume of distilled water at 60°, and placed under the exhausted receiver of the air pump, and agitated until all air bubbles had escaped. The glass cylinder being filled as before, each cube was taken out of the water by forceps and placed in the cylinder, from which it of course expelled its own bulk of water; the cylinder was now closed, dried rapidly in bibulous paper with gloved hands, and weighed, the temperature of the apartment being preserved carefully at 60° Fahr.

It is sufficiently obvious from these data that we get the specific gravity from the formula

$$S = \frac{W s}{w},$$

—where S is the specific gravity of cast iron, w = the weight of a cube of distilled water = 0·75 of an inch, s = the specific gravity of water, and W the weight of the cube of cast iron, equal in volume to the cube of water.

This method possesses several advantages in rapidity and ease of execution, and in precision of result, besides involving a check upon any serious error of experiment in every instance; for as each cube is weighed in air, and the weight of a cubic inch of distilled water at 60° Fahr. is well determined, the specific gravity of each cube is at once known within the limit of error in the gauged dimensions to which the cast iron cube is filed. There was found to be no difficulty in drying the outside of the cylinder, so that it did not change its weight in the balance, i. e. perfectly, and no sensible evaporation took place from under the plate-glass disk, after remaining in the balance for forty-eight hours.

I have entered rather at length upon the mode of taking these specific gravities, because the method in its details will be found useful in other researches, as in taking the specific gravity of small mineral specimens, &c., and because the precise determination of the maximum and minimum specific gravity of cast and wrought iron is of importance to the iron founder and engineer, as giving the data upon which the weight of castings are estimated, and which, as usually stated by authors, are an unsafe guide, inasmuch as the specific gravity of cast iron varies with its composition, the way in which it is cast, the rate of its cooling, and the depth of the mould, to an extent not generally known.

72. I was favoured by my friend Mr. William Fairbairn, of Manchester, with a few specimens of the same hot and cold-blast irons, on which he and Mr. E. Hodgkinson experimented as to their cohesion, so that not only the physical properties of these will be known from the experiments of these gentlemen, but their durability from the present.

73. Some specimens of Irish iron from the Arigna works, county Leitrim, are also included, and some experiments, made by Messrs. Bramah, of London, upon its strength are given, on the authority of the agent of the works, as an appendix to the tables, by which it will appear, that in point of cohesion this iron ranks with almost any in Britain, while its fluidity in casting recommends it as equal to the best Scotch iron. It is sold on the terms of the latter in the market.—This iron being scarcely known out of Ireland, these experiments and remarks will not be deemed irrelevant.

74. Four other similar boxes have been prepared, which all contain a selection of specimens coordinating with those in No. 1. The second box, No. 2, is sunk and moored in the foul and putrid sea water at the mouth of the Kingstown town sewer, where it debouches into the sea.

The water here is 2 feet deep at ebb, and from 8 to 12 feet at flood tide, and a constant succession of bubbles of sulphuretted hydrogen and marsh gas pass through it from the deep deposit of mud which forms the bottom.

Precautions have been taken to prevent the box of specimens sinking in the mud. The temperature of the water is 58° Fahr.; it contains as much saline matter, except during heavy rains, as the clear water of the harbour—its specific gravity, when filtered, being the same.

The results of this experiment will determine the relative actions of clear and foul sea water, when examined in the same way and at the same periods as No. 1.

75. The box, No. 3, containing a similar set of specimens with No. 2, has been deposited, by permission of the Dublin and Kingstown Railway Company, in the hot-water cistern of their baths at Salt Hill, in clear sea water, maintained constantly at a temperature varying from 110° to 125° Fahr. The object of this experiment is to determine the change of corrosive action produced by increased temperature in sea water containing but little combined air, and the differences of this action on various kinds of iron.

76. Van Beek and Dr. John Davy appear to be of opinion, that sea water, after it has been boiled, is incapable of decomposing iron from containing no air; at the temperature of 125° Fahr. decomposition is however most rapid, as the action of the sea water on the iron cisterns of these baths demonstrate; and yet the water contains little air at that temperature.

The results of my own experiments also show me, that after all the air is expelled that can be from sea water by boiling, it is still capable, at its boiling temperature, of decomposing iron, and that with a rapidity as great as at ordinary temperatures, however highly charged with air. In addition to which, there is no longer any reason to doubt the fact, that under such circumstances the alkaline chlorides are in part decomposed by the iron as well as the other salts contained in sea water.

77. Indeed Scoresby's experiments appear to prove that it is impossible to deprive water, whether salt or fresh, of all its air, by any amount of even alternate boiling and freezing. He found that, on boiling briskly some sea water in a phial, and then corking the latter and exposing it to cold, as the water froze air bubbles began to appear moving upwards in the fluid, and the ice produced was full of microscopic air bubbles. Hence he concludes it probable, either that water is not entirely freed from air by boiling, or that some of the water is decomposed during the progress of the freezing process: of the latter there is no likelihood.

Boussingault also states that he found 16 per cent. of oxygen in snow collected from the summit of Chimborazo in South America.—(*Annal. de Chim.*) It is to be remarked, however, that, as has been observed to be the case with lead and some other metals, so iron seems to be corroded much more rapidly by air and *distilled* water at a high temperature, than by water holding any alkaline or earthy salts in solution. The destructive effects of a small leakage of steam producing a trickle of distilled water to steam boilers have often been observed by engineers.

78. The experiments of Dr. Faraday on the order of deposi-

tion by boiling of the saline contents of sea water, and the respective temperatures at which each salt deposits, showing that they fall in the order of their respective insolubilities, indicate that important differences in the corrosive action of sea water, when boiling, may result from its degree of saline concentration, and to this, the resulting boiling point, the electro-conducting power of the fluid, as well as the nature of the salts deposited and remaining in solution, are conditions. And, further, as means have been devised (although with increased expenditure of fuel) of preserving sea water in marine steam boilers, (or others using salt water,) at a constant degree of saturation, it becomes important to discover when this is such as to produce a minimum corrosion, whether before or after the deposition of the sulphate lime, or of the chloride, sodium or magnesium.

79. The next box, No. 4, has been moored by permission and assistance of the Ballast Corporation of the port of Dublin, in the foulest water of the river Liffey, in the mid stream, opposite the mouth of the Poddle river, at this place a tributary of corrupted water. It lies in water 4 feet deep at ebb, and from 15 to 20 feet at flood tides. The water is very brackish at full tide, and at the other periods fresh; its temperature, when the box was sunk, was 61° Fahr. The specimens in this are the same as in Nos. 2 and 3, as may be seen by the tables. Its object will be to determine the relative effects of foul river water, alternately brackish and fresh, and this will again compare with the results to be obtained from the last box, No. 5.

80. It has been sunk in the clear, unpolluted fresh water of the Liffey above Island-bridge, and within the premises of the Royal Military Hospital. It lies in water varying at times from 3 feet to 6 feet in depth; its temperature varies with the season.

Specimens of water have been taken from these five localities for examination, and will be again taken and examined from time to time. The highest and lowest temperatures of each will also be observed.

81. In each of these boxes have been included a number of specimens, coated with various protecting varnishes and paints. —This was originally suggested by a fact of importance communicated to the writer by Thomas Rhodes, Esq., civil engineer, whose experience in the construction of great works in iron is well known. He mentioned, that when engaged on the locks of the Caledonian canal, certain cast-iron sluices were put down and exposed to the ocean water, having been coated over with common Swedish tar, with the exception of their faces, which were ground together, and were removed in about four

years afterwards: every part of the iron still covered with the tar was found sound and untouched as when put down; but the ground faces, which had not been tarred, were softened and converted into plumbago to the depth of $\frac{3}{4}$ of an inch.

This interesting and important observation shows that, where abrasion does not interfere, if we could get any coating to adhere to the iron which would be impervious to air and water, the preservation of the metal would be effected in the best and simplest manner. Unfortunately, many difficulties oppose this, and few, if any, varnishes can be obtained which will spread over the iron without leaving uncovered spaces or microscopic pores.

Professor Lampadius long ago directed his attention to this point, and, in the *Annales des Arts et Manufactures*, published the composition of a paint or varnish for the preservation of iron from rust, the basis of which is sulphate of lead and sulphate of zinc ground with plumbago and oil. It is difficult, however, to see the precise point aimed at by this composition.

82. The paints and varnishes which have been placed in process of experiment in the above five various conditions are several of those most ordinarily in use, with the view, that as nothing certain is known upon this branch of the subject, the fate of these coverings, many of whose other properties are well known, may afford leading indications as to the direction in which improvement may be sought.

83. As yet it has been impossible to arrange any experiments upon a large scale upon wrought iron, nor indeed to collect sufficient specimens; but there has been included in each of those five boxes a single parallelopiped, all of equal size, and cut from the same bar; it is of what is called common Welsh bar iron, or No. 1, and was made at Dowlais Iron-works, South Wales.

This bar I have called "The Standard," and the remainder of it, which is some feet in length, is proposed being deposited with some learned body to be appointed by the British Association.

Now this standard being placed in each box in circumstances precisely similar to the rest of the specimens, it is intended to take the action of the sea and fresh water upon *it* as unity, and refer their action upon all the other specimens to this, by which means not only will this whole series of present and projected experiments on wrought and cast irons be numerically comparable most conveniently by the engineer, but any future experimenters upon novel makes of iron, or upon foreign ones, can, by reference to the standard bar in possession of the Association, make their experiments comparable with these.

Without this precaution the present experiments, although correct, would stand isolated, and be scarcely capable of being even brought into comparison with future ones. Nor could it be hereafter determined what change as to corrodibility, future, and now perhaps unthought of, revolutions in the manufacture of iron may produce in the metal to be made in years yet to come.

84. The writer's experiments also lead to the expectation, that with the same bar of iron, or the same casting, a simple and closely approximate estimate may be formed of its destructibility in water or in solutions of the alkaline or earthy chlorides; by the rate of its solution in other agents; and with this view experiments are in progress upon the standard bar and other iron, and in the event of their results being found as here stated, it is obvious that upon the basis of the present prolonged experiments in sea water, the durability, under similar circumstances, of all other or future irons may be determined in a few hours by the aid of this new method of examination.

85. The subject now leads us to consider briefly the various modes of protection which have been proposed for the purpose of preventing, as far as possible, those actions of water and air on iron, the rate and nature of which our experiments have been directed to determine; and these, with the exception of mere superficial coverings, as already alluded to, have all been of the electro-chemical class, and more or less directly derived from Sir Humphry Davy's original discovery and proposal of the protection of the copper sheathing of vessels. In that paper the great principle was developed of counteracting chemical by electrical forces; his successors have only, with greater or less perfection, developed and applied his brilliant idea to particular cases, while in doing so, it must be confessed, they have corrected some small errors into which this great philosopher fell. In Sir H. Davy's original papers on the preservation of copper sheathing, he distinctly states, that it follows from his principles then developed, that cast or wrought iron may be preserved from chemical action by suitable protectors of zinc *or tin*.

But my friend Professor Edmund Davy has unquestionably the merit of having been the first to conduct a series of well-devised and careful experiments upon the subject on the large scale, which he did partly in connexion with the preservation of the iron work of the mooring chains and buoys in Kingstown harbour, under the auspices of the Board of Public Works. The results of these have been already communicated by him to the Association, at its meeting in Dublin, and published in its reports.

The results of these investigations show that zinc is fully capable of protecting cast or wrought iron in sea or fresh water, when applied in a massive form, at least for a time. They also put in a forcible point of view the important part which the contact of air plays in the corrosion of iron.

86. It would seem, however, to be doubtful how far this protecting power even of zinc is completely permanent, for as a portion of the oxide of zinc is transferred to the surface of the iron, as Professor Edmund Davy has observed, it would seem that the preserving power of the zinc is diminished.

A forelock key, now presented, with which I have been favoured by Professor Davy, and which has been immersed in sea water for about three years, though protected by zinc in form of a ring loosely connected with it, is yet somewhat acted on, a crust of magnetic oxide being formed all over it, spotted over with the oxide of zinc; yet the action is incomparably less than it would have been in the same time and circumstances if wholly unprotected. My attention has also been drawn by Professor Miller, of Cambridge, to the curious fact, that the surface of the iron is covered in places with microscopic crystals of calc spar; these he was kind enough to examine for me with the goniometer, and although under very disadvantageous circumstances, succeeded in verifying their form as that of the common calc spar rhomb. This fact is interesting, as a new instance of the production of an insoluble crystallized mineral by galvanic currents of low tension.

87. Pepys long since proposed to preserve polished instruments of iron and steel from rust in air by zinc protectors. This seems to have been unsuccessful, and was found to be so by Professor Edmund Davy.

88. Very lately a company has arisen in London, under the name of the "British Galvanization of Metals Company," based upon a patent for the protection of iron by coating its surface with fluid zinc, obtained by a French engineer, M. Sorel. I lately wrote to the secretary of this company, and have obtained specimens of the so-called galvanized iron, which are now presented. I also wrote to another company, styled the "Zincked or Galvanized Iron Company": my letter was returned unopened by the secretary. Having only received the specimens a very few days before the present meeting, I have been unable as yet to make many experiments upon them; some, however, are detailed in the prospectus of the company, of Professor Graham, Mr. Children, Mr. Garden, and Mr. Brand, which amount to this, that, as was to be expected, the zinc preserved the iron, in dilute acids, until the whole of it was dissolved. In the speci-

mens furnished us, the iron, which is all wrought, (and its application to the more carbonaceous cast irons must be more difficult,) is zincked or, if the expression may be used, tinned with zinc; the coating is excessively thin, and from its peculiar greasy feel, leads to the presumption that it has been slightly amalgamated also.

89. I was enabled to detach from one spot a few grains of zinc, which, on examination, appeared to be as pure as it is usually found in commerce. I expected to have found it alloyed with lead; of this it contains a trace, and a good deal of iron, probably taken up in part from the bar. No mercury could be detected in it.

90. A very few minutes are sufficient to dissolve off the whole of the zinc from the surface of the iron when immersed in hydrochloric acid, diluted with 40 volumes of water.

91. Oxide of zinc is rapidly deposited in sea water or a solution of common salt, when acting on it.

92. When a bar of the zincked iron is placed in hydrochloric acid, diluted with 20 volumes of water, the zinc having been completely removed by the file from one half of its surface, hydrogen is given off both from the zinc and iron surfaces from the first moment; and after the whole of the zinc is dissolved, this gas is much more copiously evolved from the surface that had been zincked, than from that from which it was filed off. This circumstance appears to be connected with the strength of the acid; it does not occur in that which is very dilute.

93. There can be no doubt of the power of this combination to protect iron for a time, or while the thin coat of zinc lasts perhaps, and in some practical points of view it would seem to offer advantages over zinc protectors, as proposed being applied by Edmund Davy. But it seems to be forgotten by the advocates of this attenuated application of the preserving metal, that for every particle of iron protected, an equivalent of zinc must be destroyed, and that hence, unless a sufficient mass of the electro-positive metal is provided to allow for degradation, its efficacy must soon be null.

94. It is not intended, however, to pronounce any decisive opinion as to the advantages or disadvantages of this peculiar mode of applying zinc protectors until we have had time to make other and careful experiments upon it; meanwhile, in justice to my friend Professor Edmund Davy, I must remark upon the arrogation of original discovery to M. Sorel, the patentee of this process, which some of the French scientific journals make. It does seem strange how any pretension to originality of discovery can be now set up on this score, after the previous

publications of Sir H. Davy, Pepys, and Edmund Davy; and still more, how a French patent is to be maintained for a process which, although its principle was doubtless then not understood, was, with little variation, before patented on the 26th of September, 1791, by Madame Leroi de Jaucourt, for preserving metals from rust by covering with an alloy of zinc, bismuth, and tin. I may add, that Professor Davy informs me he used the method of zincking over the surface of iron as a preserver so far back as 1834.

95. M. Sorel's patent is described as capable of being applied in three ways, viz. 1st, by covering the surface with fluid zinc; 2nd, by the application of a paint made from zinc; 3rd, by covering with a powder made from zinc. Unless the second mean a paint made from ground metallic zinc, it is similar to Lapadius' varnish, before described; and if the former, then it does not differ from the third mode described, apparently. We have, however, not been furnished with specimens of either of these modes, which would seem beforehand not likely to answer their intended purpose, from a want of that continuity of metallic connexion which appears essential to preservation in this way.

96. Sir H. Davy erroneously supposed that tin also possessed the property of preserving iron in sea water. This opinion has been controverted by the experiments of M. Van Beek, of Utrecht, and of M. Mulder, of Rotterdam, and more recently by Professor E. Davy, in a paper communicated to this Association, in which he shows that iron, on the contrary, will preserve tin, but that zinc will preserve both.

Sir H. Davy, and his brother Dr. John Davy, who has defended his opinion, appear both to have been led astray by merely considering and experimenting upon the galvanometrical relations of tin to iron *when first placed in contact.* But Van Beek, in the paper alluded to (*Edin. New Phil. Journal* for October 1837,) has cleared this up by the discovery of the remarkable and anomalous fact, that although it is certain that tin is to iron in a positive relation in atmospheric air, yet when both are plunged into sea water, after a period, never greater than half an hour, has elapsed, the astatic needles of the galvanometer, which had before indicated the above relation, gradually return to zero, and pass through it to the opposite side, and indicate that the iron has become positive with respect to the tin, thus showing the singular fact apparently, that metals retain for a longer or shorter time the electrical condition they have once acquired.

97. By decisive and direct experiments also, M. Mulder, of

Rotterdam, determines the corrosion of iron in presence of tin, and its amount:—1st. A plate of iron weighing 32·907 grains was placed in a glass vessel containing one *litre* (= 61·028 cub. in.) of sea water, during 20 days, at the temperature of the month of November 1836 (at Rotterdam namely). After the experiment the weight of the iron was found to be = 32·726 grains, loss by oxidation = 0·181 grain.

2nd. A similar plate of iron, exactly of the same weight of 32·907 grains, but on whose surface was fixed a small piece of tin weighing 8·140 grains, was in the same manner exposed for 20 days in one *litre* of sea water: the weight of the iron, after the experiment, was found to be = 32·674, that of the tin 8·139 grains; hence loss by oxidation of the iron = 0·233 grain, and loss by oxidation of the tin = 0·001 grain. These results show that the iron, when exposed to sea water as above, alone lost by oxidation 0·052 grain less than when in contact with the tin.

Van Beek, in recording these experiments, observes, that the action on the tin must have taken place at the first moment of immersion of the metals, and before it had become negative with respect to the iron.—(*New Edin. Phil. Journ.*, Oct. 1837.)

98. De la Rive has observed an analogous change of electrical state in these metals in a different research, and the fact is a very important one as regards our subject: it may possibly be hereafter found that the diminished preservative power of zinc to iron, after a length of time, has an analogous cause, as may the following like phenomenon. It sometimes happens that when one of Schœnbein's inactive wires, and another rendered inactive by it, have remained together in a tube of nitric acid for a very considerable time perfectly passive, they at length suddenly, and without any assignable cause, both become active, and the reaction on the iron is so unusually violent, that most of the acid is instantly driven out of the tube with a sort of explosion.

99. We have now to consider the subject of a communication made at the last meeting of this Association, at Liverpool, by Mr. John B. Hartley of that town, upon the power of brass to preserve cast and wrought iron in sea water. Mr. Hartley is reported to have stated in the Chemical Section, that certain iron sluices having brass in connexion with some of their parts, had on examination been found perfectly sound and uncorroded in the neighbourhood of the brass, after an exposure of twenty-five years, but were corroded elsewhere; and that in consequence of this discovery, all the iron work below the tidal level employed in the Liverpool Docks had been placed in connexion

with brass in some way, and that its preservation had followed.

100. The statement created considerable discussion and attention at the time, and at first seemed to Professor Davy and myself an important element in the subject of investigation with which we had been entrusted by the Association. Accordingly, very soon after the meeting, Professor Davy addressed Mr. Hartley upon the subject, detailing the results of his previous experiments, and expressing his conviction of the non-protective power of brass to iron, and assigning another and sufficient cause wholly unconnected with electro-chemical protection to the phenomena described by Mr. Hartley. A copy of his letter is annexed, as published in Saunders' News-letter of Oct. 24, 1837.

"*To John B. Hartley, Esq., Liverpool.*

"Royal Dublin Society's Laboratory.

"SIR,

"You will I am sure excuse the liberty I take in addressing you, on an interesting and important subject on which you have recently been engaged, namely, preventing the corrosion of cast and wrought iron in salt water: I also have made many experiments with a view to the same object. I have to express my regret that the state of my health prevented me from taking an active part in the proceedings of the Chemical Section at the late meeting of the British Association for the Advancement of Science in Liverpool. I was not present when your paper "On preventing the corrosion of cast and wrought iron in salt water" was read and discussed. The object of it, as reported in the only two public prints I have seen, namely, Saunders' News-letter of 15th Sept., and the Athenæum of the same date (the former of which I only saw yesterday), was to prove that brass protects cast and wrought iron from corrosion in salt water, without being itself corroded. It was also stated, that the iron so protected remained in excellent preservation after a period of twenty-five years. I must confess that these statements appeared to me to be not only anomalous, but in direct opposition to my own experiments. I have no hesitation in stating, as the result of my experience, that brass will not protect cast or wrought iron or steel from corrosion, either in salt or fresh water; but, on the contrary, these metals will protect brass from corrosion under such circumstances, at least for a limited time.

"I need not tell you that if brass were found to protect cast and wrought iron in salt water, suppose for ten days, the presumption would be, that it would do so for twenty-five years; but if, on the contrary, brass will not protect iron for ten days, nor for a single day, which is the fact, then it would seem absurd to expect that it will protect them for twenty-five years!

"If I mistake not there is little difficulty in accounting for the preservation of the iron under the circumstances noticed by you, without having recourse to any fancied power of protection in brass, which it really does not possess.

"In Saunders' News-letter already referred to, which contains the fullest report of your paper which I have seen, the iron is stated to be 'an iron pin working in a brass socket, which was again inclosed in an iron case; all the iron in connexion with the brass was in excellent preservation, whilst that removed from it was corroded.'

"Now it seems clear to me that the preservation of the iron, under the circumstances here enumerated, was an effect due to the mere condition in which the metal was placed, which was such as precluded (almost entirely) the access of air, on which its corrosion, both in salt and fresh water, depends. Under similar conditions I entertain no doubt but that iron will preserve iron, and brass, brass, and each of these metals the other respectively; and glass, porcelain, &c., will equally preserve both brass and iron from corrosion in salt and fresh water. But the preservation of metals under such circumstances is not protection in the sense in which it has been commonly understood, since the first just views on the subject were advanced by the late Sir Humphry Davy.

"As the protection of cast and wrought iron in salt water by brass is not only spoken of as a discovery, but has already been acted upon as such in some of the great public works in Liverpool, and may soon be extended to other seaports, to our shipping, and to innumerable cases where iron is exposed to salt water, I lose no time in making you acquainted with my experience and views on the subject.

"I beg, in conclusion, to remark, that my statements proceed on the ground that the brass spoken of, without any qualification, is no other than the common brass of commerce. If you have used a different alloy containing more zinc or other material, allow me to suggest to you the propriety of setting the public right on such a matter, as well as your humble servant,

"Edmund Davy."

Professor Davy has since favoured me with the following additional note containing the results of his more recent experiments on the subject. He proceeds:—

101. "As the protection of cast and wrought iron in salt

water by brass was not only spoken of as a discovery, but also acted upon as such in some of the great public works in Liverpool," Professor Davy (who was not present when Mr. Hartley's paper was read and discussed) lost no time in making Mr. Hartley acquainted with his experiments and views on the subject, which he did in a letter inserted in "Saunders' News-letter, 24th October, 1837." In this communication Professor Davy stated, as the result of his experience, "that brass will not protect cast or wrought iron either in salt or fresh water, but that, on the contrary, these metals will protect brass from corrosion under such circumstances at least for a limited time.

"Professor Davy refers the preservation of the iron under the circumstances enumerated to the mere condition in which it was placed, being such as almost entirely precluded the access of air, on which its corrosion, both in salt and fresh water, depends.

"Professor Davy was at first led to suppose that Mr. Hartley's brass, which was spoken of without any qualification, was the common brass of commerce; but on learning that its composition was different, he instituted experiments with Mr. Hartley's brass, for specimens of which he was indebted to Mr. Robert Mallet. On trying the effects of this brass on iron in salt water, it had no more *protecting power* than the glass vessel in which the experiments were made. When the two metals were in close contact, the iron preserved its original brightness, as was also the case where the iron was in contact with the bottom of the glass vessel; but all the other exposed surfaces of the iron were corroded just as readily as if common brass were used with the iron."

102. In April last I wrote to Mr. Hartley requesting specimens of his brass, and of the iron preserved by it. I received a very minute portion of brass, and a piece of iron stated to have been in contact with it, together with a piece of plumbaginated iron, part of a sluice or paddle, through Mr. Gilbert Cummins, with the following letter:—

"Dock Yard, Liverpool, 23rd April, 1838.

"Sir,—In Mr. J. B. Hartley's absence from England (he being at present on the continent, and not expected back for some time) I have to acknowledge the receipt of your letter of the 21st instant, and in accordance with your request have forwarded to your address, by the City of Dublin Company's packet, a small parcel, containing a specimen of the brass composition referred to, and also of the cast iron preserved by it; the latter is part of the hinge of a large cylinder used as a valve to admit the ingress of sea water into a mill-dam or reservoir; the brass is a part of the bush with which the interior surface of the hinge was lined. The bolt or pin for connecting the valve to the cylinder is of wrought iron, which, as well as the cast iron, was found in a perfect state. I have also sent a piece of a cast-iron clough paddle, taken out of one of the dock sluices. When first taken up it was quite in a soft state, capable of being easily cut with a knife; but by exposure to the atmosphere has again become hard.

"I am, Sir, your obedient servant,

"GILBERT CUMMINS.

"*To Mr. Robert Mallet.*"

With this fragment, weighing only about 500 grains, we made a few experiments, and shortly wrote again to Mr. Cummins, requesting a larger supply of the brass, and replies to certain questions respecting its influence, as in annexed copy:—

"MR. GILBERT CUMMINS,

"Sir,—In reply to yours of the 23rd instant, Professor Davy and myself return you our thanks for your attention, and for the specimens of altered cast iron and the brass, &c. just received. The specimen of brass is quite sufficient to enable us to determine its composition, but insufficient to enable us to institute some comparative experiments as to the precise conditions of its preservative power. For this purpose it would be necessary to have five or six pounds of the brass, the value of which, should that stand in the way, we are quite ready to pay; we therefore hope to receive it by the same conveyance which brought the former specimens. We would also desire replies to the following questions in your next—

"1st. Is the brass—brass proper or gun metal, viz.—made with zinc or tin, and what, *about*, are its proportions?

"2nd. How long has it been in use as a preserver of cast iron, and to what purposes chiefly applied?

"3rd. How has the brass been chiefly applied? has it been *cast into or round* the cast iron preserved, at a temperature of fusion, or merely placed in contact at a common temperature?

"4th. Have its preservative effects been uniform, or have there been exceptions, and if so, under what conditions?

"5th. Has its preservative influence been found as effective when the iron was exposed to '*wet and dry*,' or about the level of ordinary spring tides, as when always immersed in sea water?

"6th. Has cast iron in the neighbourhood of the brass, but not actually *shielded* or *covered up* from the sea water, been as well protected as when covered; for instance, would the pin of a hinge in a brass socket be better protected than the parts of the iron hinge outside the socket?

"7th. Are cast and wrought iron equally well protected?

"8th. Has it been tried in fresh water?

"The favour of your replies as early as convenient to these queries will be esteemed by us."

To which we received the following reply:—

"Dock Yard, Liverpool, 27th April, 1838.

"Sir,—I am in receipt of yours of the 25th instant, the contents of which I have communicated to Mr. Hartley, sen., who has directed me to inform you, that he has only a small portion of the brass left that was attached to the cylinder that first caused his attention to the preservative properties of that metal; and with regard to the series of questions put by you, I am desired to say, that during his son's absence his other avocations are such as not to afford time or opportunity of properly attending thereto.

"I am, Sir, your obedient servant,

"Gilbert Cummins.

"*To Mr. Robert Mallet.*"

We hence were precluded from any information or assistance from Mr. Hartley, and were about giving up all hope of experimenting on the identical brass stated to have been used at Liverpool for protection, when we were unexpectedly favoured by Professor Kane with a piece of this brass weighing about two pounds, which he stated had been personally handed to him by Mr. Jesse Hartley; with these the following selection of experiments made by the writer, from amongst many others made by Professor Davy and himself, may be stated with their results.

103. When a piece of cast iron was placed in a glass vessel of sea water with a piece of this brass laid in close contact with its upper side, the iron was rapidly attacked, the brass remaining bright, and rust soon deposited in large quantity.

104. An equal sized piece broken from the same specimen of cast iron, and exposed in similar circumstances to sea water alone, was much less acted upon by it.

105. Two pieces of wrought iron similarly treated produced similar results.

106. Specimens of cast iron and of wrought iron similarly treated, with and without the presence of the brass, produced similar results, as above, *in fresh water*, but more slowly.

107. Where the surfaces of the brass and iron were in close contact, the iron remained nearly bright; but it did so likewise when a piece of plate-glass was substituted for the brass, or

when wood, mica, paper, or another piece of the same iron took its place.

108. The larger was the proportion of the brass present to the quantity of iron exposed, the faster the latter corroded.

109. When the brass was attached by solder to the iron, whether cast or wrought, the action was the same, with increased energy, provided the solder (composed of lead and tin) was not immersed in the fluid. When it was, so the results were anomalous, corrosion being retarded at first, and afterwards accelerated, apparently from a change of electric relation between the metals, as in Van Beek's experiments before noticed.

110. When a cylinder of brass, in composition the same as Mr. Hartley's, was cast round a turned cylinder of wrought iron at its fusing temperature, the iron on exposure to sea water was rapidly acted upon, and carbonates of lime and magnesia were deposited upon the brass, which remained bright.

111. Corrosion in all cases commenced at the moment of immersion, and continued without change for periods of nearly two months.

112. When cast or wrought iron was exposed to sea water or fresh in the same vessel with a surface of this brass, but without contact, but each communicating by a gold-soldered platina wire outside the fluid, corrosion took place of the iron more rapidly than when similar pieces were exposed without the presence of the brass.

113. No modification of alloy in the brass within the limits of brass or gun metal seemed to produce any very remarkable change in the increased rate of corrosion of iron by its presence, nor did the results differ materially whether brass proper, viz. zinc and copper, were used, or Mr. Hartley's brass, which is, in fact, impure gun metal, or copper and tin.

114. As the proportion of zinc, however, in the brass increased, a tendency to preservation should be manifested, and conversely as the copper predominated, increased corrosion would be expected. This view has suggested a very curious branch of investigation now in progress, as to the changes of electrical relations to a third metal of definite atomic alloys of two other metals, whereof one is in a positive, and the other in a negative electrical relation to the former.

115. These results are sufficient to prove incontestably, that brass or gun metal have no protective power over iron whatever, but, on the contrary, greatly promote its corrosion in sea or fresh water, and, as we also found, in diluted acids.

116. But as practical instances often come more home to the

practical man than any experiments made on a small scale, it so happens that I am enabled to present an actual instance from the Dublin Docks of cast iron deeply acted on and corroded in a period of eighteen years, though in close contact with brass. This is a portion of a sluice, situated between high and low water, made eighteen years since by the firm to which I belong, and lately obliged to be removed and replaced with a new one, in company with several others, from the deep corrosion and softening it had undergone.

The brass was here a facing riveted to the cast-iron sluice all round, to make it water tight. The composition of this brass differs from Mr. Hartley's only in containing some more zinc, of which his contains but a very small quantity, which by analogy and according to Mr. Hartley's own view is all in its favour. Here, then, is an experiment of eighteen years' duration, which results in showing that brass has had no protective power in the tidal water of the River Liffey.

117. It appeared worth while to make a quantitative analysis of Mr. Hartley's brass and of this from the Dublin Docks, both for the purpose of comparison, and to see if they were atomic compounds or mere accidental mixtures with approximations to atomic constitution, as is generally the case in brass used for engineering purposes, which is produced by remelting. I accordingly analyzed a fragment of the first specimen of Mr. Hartley's brass, sent us direct from Liverpool, and also of the Dublin Dock brass, and lastly, that given us by Dr. Kane. The method adopted with all was the following, which differs in some respects from the modes usually recommended for the analysis of brass, and which are incapable of giving results approaching correctness.

1st. A given weight of brass was dissolved with heat, continually agitating in strong nitric acid, which was boiled nearly to dryness, diluted with water, and the stannic acid separated, washed, ignited, and weighed.

2nd. The solution and washings evaporated nearly to dryness, sulphuric acid added, evaporation continued to dryness, water added, and sulphate lead, separated, ignited, and weighed.

3rd. The solution being acid, treated with sulphuretted hydrogen, and precipitate washed in water impregnated with the same; the CuS—redissolved in aqua regia, with heat, again precipitated hot with caustic potass washed with hot water, ignited, burning filter and weighed.

4th. The solution and washings concentrated, and pure ammonia added in excess, and the Fe_2O_3 separated and weighed.

5th. Bicarbonate potass added to the filtered solution, boiled briskly to dryness, avoiding spattering, redissolved in water, and precipitate of ZnO. separated, ignited, and weighed warm.

6th. The solution tested for remains of zinc by bihydro-sulphuret ammonia.

118. 24·80 grains of the brass received from Liverpool through Mr. Gilbert Cummins, analyzed in this way, gave the following results, reduced to per cent.:

Tin	=	12·012
Lead	=	0·266
Copper	=	79·750
Iron	=	3·137
Zinc	=	4·786
Loss	=	0·049
		100·000

119. 42·085 grains of the brass from the Dublin Docks gave the following composition, also reduced to per cent.:

Tin	=	0·807	
Lead	=	4·062	
Iron	=	0·879	
Copper	=	65·890	= 2 atoms Cu
Zinc	=	28·288	= 1 atom Zn
Loss	=	00·074	
		100·000	

120. I analyzed 41·705 grains of the specimen of Mr. Hartley's brass given us by Dr. Kane, with the following results, which present a larger amount of loss than I could have wished, arising from my having been several times delayed in completing the process by unavoidable business.—It gave, reduced to per cent.,

Tin	=	4·524
Lead	=	13·051
Iron	=	1·743
Zinc	=	8·639
Copper	=	67·233
Loss	=	4·810
		100·000

It is hence obvious that all these brasses are chance mixtures, and that the Dublin Dock brass contains the most zinc, and comes nearest to atomic constitution; and hence might have been expected, on Mr. Hartley's hypothesis, to have most effectually preserved the iron it was in contact with. It is still more remarkable, however, how great the discrepancy in constitution of the two specimens of Liverpool brass is, the latter containing an enormous proportion of lead.

121. It is not difficult to imagine that the mistake of Mr. Hartley has arisen from neglect in observing that which Professor E. Davy has first pointed out, namely, that iron, or other metals, in its relation to water are preserved from corrosion by *covering surfaces, although the water insinuates itself between.* Nor does it appear difficult to account for this result. It would appear to be a case of slow or retarded chemical action by the opposition of capillary forces of the same class as slow action, through Becquerel's clay plugs or diaphragms, in which, when once the first portion of air, combined with the fluid between the surfaces in contact, is decomposed and taken up, the chemical affinity of the iron, or similar metals, for it, is counteracted by the capillarity of the flat tube formed by the opposed surfaces which it is unable to overcome, so as to draw in fresh air to the fluid within, already exhausted of it.

122. Indeed, if Mr. Hartley be reported rightly, he is stated to have mentioned that "soil," *mud* I suppose, lay on the sluices said to have been preserved, and which had to be removed prior to their examination; if so, there is little wonder they should not have sustained corrosion.—*Liverpool Journal.*

123. Another, and an extremely probable reason for the mistake, may have been the supposed preservative surfaces being often smeared with oil or grease, which, for a considerable time, resists the action of sea or fresh water, and protects the metal on which it lies. Indeed, if we could get an air and water-tight covering which would remain so, no further protector for immersed or moistened metals need be sought for.

124. Accordingly, Mr. Arthur Aikin suggested the application of melted caoutchouc, with or without admixture of oil of turpentine, as a varnish to preserve iron and steel, &c., from corrosion, so far back as 1821.—Gill's *Tech. Rep.* vol. i. p. 55. And Dumas has proposed the employment of caoutchouc in solution as a varnish to the shot and shells in the French arsenals (*Comptes Rendus,* 1836. p. 373); but Payen states that this had been tried by the municipality of Grenoble in the year 1834, and found useless after a short period.

125. During the experiments already detailed it seemed just possible that Mr. Hartley's might be yet a concealed case of Professor Schœnbein's anomaly of passive iron, or of Dr. Andrew's inactive bismuth, and the writer was just about commencing some experiments with a view of elucidating this, when

he received the *Bibliothèque Universelle*, published in February last, containing an article by Professor Schœnbein on the very subject. In this he shows, as indeed he had previously done in a letter to Dr. Faraday in the Lond. & Edinb. Philosophical Magazine for December last, that he, Schœnbein, "had already demonstrated, 1st. That iron only comports itself passively as the anode in relation to the oxygen disengaged by the current in aqueous solutions, which contain alone oxygenized compounds, as oxacids, oxide, oxysalts, &c.; 2nd. That the state of chemical indifference of iron can only be obtained with respect to oxygen; and, 3rd. That this metal acts in its ordinary way when it is plunged as an anode into aqueous solutions of the hydracids of the chlorides, bromides, iodides, fluorides, or sulphurets; in fact, in solutions of combinations whose negative element has a great affinity to iron. In these cases the oxygen resulting from the electro-chemical decomposition of the water combines with the iron in the same way as the chlorine or iodine disengaged under like circumstances. Hence," continues Schœnbein, "as the substances which are in solution in sea water are for the most part electrolytes which do not contain oxygen as a constituent, it is impossible, after the facts above stated, that iron as an anode can be indifferent chemically, in relation to sea water; but, on the contrary, this metal must combine with the oxygen, chlorine, &c. disengaged by the current."

Schœnbein then states the result of an experiment he made directly with sea water, by plunging an iron wire connected with the positive pole of a pile into it, thereby closing the circuit; no oxygen was evolved at the iron, which was oxidized, in strict accordance with his general principle. He then proceeds to show, that assuming Mr. Hartley's view to be right, it involves an initial absurdity or contradiction in principle; and finally concludes, that the observation of Mr. Hartley must be considered as doubly anomalous, namely, in relation to common and acknowledged electro-chemical laws, and also to those special ones developed by himself.

126. The anomaly, however, may now be considered simply as an error, but one of a very serious character, apparently from the extent to which its consequences seem to have been wrought out in the application stated to have been made of brass as protectors to all the work of the Liverpool Docks, which, unless removed, must be attended with the rapid decay and destruction of all the iron it is connected with. I have also understood that, acting on this presumptive protection, the Liverpool chain-cable makers now supply gun-metal pins to their cable shackles, at intervals of a few fathoms, by way and under the name of

"preservers;" a more destructive practice can scarcely be conceived, or one more fatally applied. From these circumstances, and lest these mischievous results should be extended elsewhere, it has been deemed right thus at length to refute it, which I conceive is fully done by Schœnbein's, Professor Davy's, and my own experiments.

127. To recur again for a moment to the subject of the boxes of specimens of cast iron sunk for experiment, it was stated that they were divided into separate cells for each kind of iron by veneers of varnished oak. The reason of this arrangement, and a deduction which has grown out of it, and is likely to prove important as affording a mode of protecting cast and wrought iron, remain to be stated. It having been early remarked that the harder irons, whether cast or wrought, were acted on much more slowly than the softer and more carbonaceous ones, it appeared not impossible that if several different sorts were inclosed in electrical continuity in the same box, grave errors might be introduced into our results by the iron least acted on standing in a negative relation to those more rapidly corroded, and increasing the action of the sea or other water upon them, and at the same time being themselves preserved to a certain extent.

128. By a few preliminary experiments with the galvanometer, this was found to be a correct view,—it was found that of any two different irons, the harder was always in a negative relation to the softer, which was positive to it, and hence the separation of every specimen became necessary in order to eliminate this source of error.

129. This at once suggested to the writer the possibility of preserving the hard gray cast iron and the wrought iron, &c., in common use, by the application of protectors formed of the softest and most highly carburetted cast iron attainable; and as the conversion of this latter into plumbago, to a great extent, did not seem materially to alter its electrical relation to gray or white cast iron, or to wrought iron, it seemed probable that it might afford an electro-chemical protector superior in many respects even to zinc. With this view experiments are now in progress, and so far are decisively in favour of the method.

130. The intensity of current produced by soft and hard cast iron is much greater than would have been anticipated. When two small bars, each 4 in. long, by 0·5 in. wide, by 0·25 thick, one of soft black and the other of hard gray cast iron, were both broken in two, and an iron wire soldered to each half, on immersing the two halves of either one piece in common water the needle of a Melloni's galvanometer was scarcely disturbed;

it oscillated about 2°; but when one half of each of the two original pieces, i e. the black and the gray, were immersed, the needles deviated at once from 78° to 80°, and on adding a single drop of hydrochloric acid, flew round.

131. This action in common water or sea water always showed the softer iron positive to the other, and continued constant and unchanged in an experiment continued for some days.

132. When two pieces of cast iron, such as the above, were immersed in dilute hydrochloric acid, tied together and in contact, the harder one remained quite bright and untouched before and after the acid was saturated, while the softer was rapidly blackened and dissolved; neither was any gas evolved from the negative piece, unless the acid was concentrated.

133. The subject is not in a state to do more now than state the principle in view, and that so far as experiments have yet gone, it is likely to add a new if not a better mode of protecting iron to those already known; if successful, its application to engineering structures will afford many facilities of execution, and be attended with much greater economy than any process in which zinc is used possibly can.

134. It already suggests to the engineer the importance of preserving uniformity of texture and of chemical composition in all parts of his structures of iron, in order that one part may not accelerate the destruction of the other. It also shows the necessity, when wrought iron (which is negative to all but *chilled* cast iron) is applied in contact with cast iron, of allowing extra substance in the latter to meet the increased corrosion produced by the wrought iron at the points of contact, and it explains why the vital injury is so often sustained in works in cast iron of this metal being first eaten away round the bolt holes, as for instance, in the air-pumps and condensers of marine engines, where the parts of the work are secured together; and it further suggests, that where the sides of these bolt holes in the cast iron can be chilled or cast on an iron core, as is often practicable in such cases, while the bolt will slightly suffer, the cast iron round it will be preserved. On a future occasion I hope to lay the results of this branch of the investigation before the Association.

135. M. Payen's observations as to the power of alkaline waters to protect iron from rust, though seldom applicable, are worthy of further investigation, and an attempt to discover their rationale. It has very long been known that lime in powder, or limewater, possesses a decided power of this sort, and both are in use amongst workmen.

Cases may be found in practice where solutions of an alkali or alkaline earth would be admissible and valuable if found effective preservers of iron; for instance, lime-water might readily replace the bilge-water in steamers, whose action is at present so destructive to the holding-down bolts, blow-off pipes and cocks, boiler bottoms, coal bunkers, &c., and to the decomposition of which in a great degree the peculiarly offensive smell of the bilge-water of steamers is owing. There is no reason to assume that dilute lime-water would have any injurious action on the timbers of the ship.

136. Dr. Andrews' and Schœnbein's experiments, however, in which a metal becomes capable of rendering passive or of protecting in certain cases another, *although itself not acted on*, give hope that protectors of this kind may yet be found and practically applied to iron; and hence it is in this direction that our efforts should be bent with most energy in seeking to preserve metals from oxidation, namely, *to obtain a mode of electro-chemical protection, such that, while the metal shall be preserved, the protector shall not be chemically acted on, and whose protection shall be invariable.*

137. In illustration of this I may adduce a very interesting experiment of Becquerel and Dumas (*Comptes Rendus*, 6th Feb. 1837): "Having taken a flask half filled with distilled water, in which was dissolved $\frac{1}{100}$ of potass, they plunged into it a slip of perfectly polished iron, and another of gold; to each was fixed a wire of the same metal passing through the cork. The flask was sealed with all possible care to prevent the access of air. Seventeen months afterwards the iron preserved all its brilliancy, no tubercles had formed on it, and every thing indicated that it had undergone no appreciable alteration."

When the gold and iron wires were placed in communication with a multiplier with a short coil, an immediate deviation of 35° was produced, and the magnetic needle having oscillated awhile, came to rest again at zero. On interrupting and again re-establishing the communications *it* remained motionless, but on leaving the circuit open for a quarter of an hour, and again closing it, the needle deviated 25°, and after remaining interrupted for half an hour, the deviation amounted to 35° again.

The experiment was repeated, and always with accordant results. The current produced is then the result of a discharge like that of a Leyden phial.

Thus, when the iron is in contact with the alkaline water, the metal takes by degrees a charge of negative electricity, and the water a charge of positive electricity, *as if* there had been a chemical reaction between them. (De la Rive in fact considers that it is due to a chemical action, though excessively slow.)

These two electricities, notwithstanding their reciprocal attraction, remain in equilibrium at the surface of contact, which they are not able to break, and they only recombine when we establish the communication between the iron and the solution by means of a wire of gold or platina. Hence it results that the iron rendered constantly negative is found in the least favourable state for combining with the oxygen of the air present in the solution.

It is unnecessary here to pursue the extract into the rationale and objections thereto discussed by the authors, as I merely wish to indicate the class of experiments which are the most valuable as regards our subject. Others very analogous, in which anthracite, plumbago, and sesquioxide of manganese are the agents, are to be found in Becquerel's fifth volume of his *Traité de l'Electricité*.

138. The subject, of which I have thus given I fear a very imperfect sketch, is a wide and important one, and many careful experiments are wanting to complete our knowledge of it. The following especially are desiderata immediately applicable to the engineer and also to the chemist.

1st. A series of experiments to determine the rate of progression of corrosion in sea water and fresh, at increasing depths, from 0 to say 10 fathoms.

2nd. A comparative series for this reaction at the various temperatures of the sea and of rivers, &c. known to be found within the the range of our inhabited climates.

3rd. A determination of the nature and amount of air contained in sea water at various depths, as recommended by M. Biot to the officers of the "Bonite."

4th. A set of comparative experiments on the action of sea water, diluted with various known proportions of fresh, as at the mouths of tidal rivers.

5th. Experiments are wanting as to the effects of the presence of animal matters in a state of putrid fermentation in sea and river water, in modifying their action on iron, as in rivers, &c. receiving the sewerage of cities.

6th. Determinations of the amount and nature of the plumbago produced from various makes of iron, its precise composition, and the conditions of its heating or not spontaneously, with the results of this action.

A careful repetition of many of the experiments on the action of pure water, and of air and water on iron, is also needed, the results of former experiments being neither satisfactory nor uniform.

A paper of some novelty on this subject has just appeared in the *Bibliothèque Universelle* for June and July, 1838, by Professor Bonsdorf of Helsingfors. In this the author studies in general the action of various metals on air and water, under the following conditions:—

1st. In air perfectly dry and free from carbonic acid.

2nd. In air saturated with vapour of water, but free from carbonic acid.

3rd. In air containing both the latter.

4th. In contact with liquid water and air, both free from, and also containing carbonic acid.

He states that in the first condition no metal oxidates but potassium and sodium. That in the second case no metal oxidates but arsenic and lead; in particular, that zinc, iron, and bismuth do not oxidate, and that a gentle heat increases the action on the first two. The author also brings forward some new views on the subject of the deposit of moisture on metallic surfaces in certain conditions, which, however, do not seem quite correct.

7th. Experiments would also be desirable as to whether magnetism affects the rate or form of corrosion of iron, and hence, whether position as to meridian has any thing to do with the durability of engineering works in iron.

8th. Experiments upon the suitability of various protecting paints and varnishes, and the modes of their application to works exposed to air and moisture, would be very valuable, giving preference to those which, with other obvious properties, dry soonest after rain, and, under given circumstances, cause the least deposit of dew. Upon this point Bonsdorf's paper above alluded to may be consulted.

9th. A comparative set of experiments would be useful also showing, under like circumstances, the effect of corrosion of sea water, and of its mechanical abrasion by this fluid in motion, or of the difference of action on iron in still sea water and in a tideway.

10th. It would be exceedingly important also, as an element of this investigation, as well as useful to the mechanic, to obtain a correct measure of the resistance to abrasion of various makes of iron. This has been attempted by Mr. Fairbairn, unsuccessfully, by means of grinding on a stone a piece of weighed iron, of given surface and under a given pressure, for a known time, and noting the loss of weight. The most likely means of arriving at this will probably be by making wheels of the various irons to be tried, turned on the face and fitted on to steel axles, suspending these in a swinging frame, and causing them to revolve for a length of time against the faces of other turned wheels, all of one sort of iron, say tyred with No. 1 Welsh bar iron (our standard), and pressed together by known weights. The loss of weight sustained by the wheels would then indicate their respective ratios of abrasion, the loose axles being, previously to weighing, taken out so as to eliminate their wear from that of the face of the wheel.

This method and the probability of its giving correct results have been suggested to me by the uniformity with which the wheels of railway carriages wear when tyred with the same iron. Railway experience also shows that the resistance to abrasion in rails of wrought iron is to that of cast iron as 15 : 4. (Wood *on Railways.*)

139. In conclusion, I have to regret that my friend Professor Davy's public avocations have hitherto prevented his devoting more of his attention and great experimental skill to this subject, which, while it has been entrusted to us conjointly, would, I am certain, in his hands have found an abler reporter.

Our thanks also are due to the several public bodies and private individuals to whose assistance we are indebted in making our experiments.—To the Board of Public Works and the Ballast Corporation of the Port of Dublin we are under obligation, not only for personal assistance, but for freely placing their stores in Kingstown Harbour and the Port of Dublin at our disposal.

Note.

Since the foregoing report was sent to press it has been considered unnecessary to print the tables of experiments on the great scale referred to therein at length in this volume, in as much as at present they necessarily consist more of data than of results, the latter demanding the lapse of time for their collection. It has been therefore determined at present merely to give a synoptic view of those tables of experiments in progress.

These experimental tables at present contain the following data respecting the specimens of iron submitted to trial, viz.

No. of Experiments.	Original mark of the Specimen.	Place or No. of Specimen in Box.	Weight of the Specimen in Grains.	Dimensions of the Specimen.	How Cast.	Specific Gravity. Water = 1000.	Commercial character of the Iron.	Hot or Cold Blast.	Work where made and Iron Masters.	Physical character of the Iron.

The whole series of experiments in progress on the great scale is contained in five separate boxes, each containing several classes, each of which again consists of numerous individual specimens of iron, and are arranged as follows:

Box, No. I.—Sunk in the clear sea water of Kingstown Harbour, and containing:—

Class, No. 1.—Welsh cast iron. Experiments, No. 1 to 13 inclusive, *i. e.* 13 specimens, different.

Class, No. 2.—Irish cast irons. Experiments, 14 to 17.

Class, No. 3.—Staffordshire and Shropshire cast irons. Experiments, 18 to 25.

Class, No. 4.—Scotch cast irons. Experiments, 26 to 57.

Class, No. 5.—The standard bar of wrought iron.

Class, No. 6.—Scotch cast iron, cast in green sand, and also chilled.

Class, No. 7.—Welsh cast iron, cast in green sand, and also chilled.

Class, No. 8.—Staffordshire cast iron, cast in green sand, and also chilled.

Class, No. 9.—Irish cast iron, cast in green sand, and also chilled.

Class, No. 10.—Mixed cast irons, various—Scotch and Welsh, Irish and Welsh, &c. &c. &c.

Class, No. 11.—Cast iron used by Messrs. Hodgkinson and Fairbairn in their experiments, viz. Scotch, Welsh, Derbyshire, Yorkshire, &c. &c. &c.

Class, No. 12.—Mixed cast iron, suitable for fine finishing in machinery, with the "skin" removed entirely by the planing machine.

Class, No. 13.—Hard gray mixed irons, protected by various known paints and varnishes, viz. caoutchouc varnish, copal, mastic, turpentine, asphaltum, white-lead paint, soft cement (wax and tallow), Swedish tar, coal tar laid on hot, drying oil.

Box, No. II.—Sunk in the foul sea water at the mouth of the Kingstown sewer.

Box, No. III.—Sunk in clear sea water, at temperatures varying from 115° to 125° Fahr., in the Dublin and Kingstown Railway Company's hot baths at Salt Hill.

Box, No. IV.—Sunk in the foul water of the river Liffey, within the tidal limits, opposite the Poddle river.

Box, No. V.—Sunk in the fresh water of the river Liffey, within the premises of the Royal Hospital, Kilmainham.

All these boxes, viz. II., III., IV., and V., contain classes of specimens co-ordinating with those before stated as contained in No. I., consisting in all of about one hundred and sixty separate and different specimens.

It is intended to take up these boxes at determinate intervals and examine the reaction which may have taken place; when this has been done, it is purposed to give, in addition to the foregoing data, the following information, with such other in addition as may hereafter appear desirable.

1st. The weight of each specimen when taken up after an interval of twelve months, again after two years, and again, perhaps, after a longer period.

2nd. Loss of weight when cleared from adherent plumbago, and weight of the latter when dry.

3rd. Loss of weight per unit of surface.

4th. Loss of weight per unit of surface as referred to the standard bar as unity.

5th. Uniformity or otherwise of corrosion and its depth.

6th. Amount of water *absorbed* by the iron, if any.

7th. Chemical properties of the plumbago, *e. g.* if it ignite spontaneously, &c. &c.

8th. Physical properties of the iron if altered.

9th. Relative preservative effects of the various varnishes or coverings, if any.

As the giving the information under the second head above will obviously render it possible that the reaction on the specimens may be greater the second and subsequent years than it would have been if the plumbago were not removed, means are taken to compare the effects of water, &c. on iron in the same times and circumstances as above, when the coat of plumbago is periodically removed, and when it remains untouched for the whole period of experiment.

The following Notice of Experiments on the ultimate Transverse Strength of Cast Iron made at Arigna Works, Co. Leitrim, Ireland, at Messrs. Bramah and Robinson's, 29th May, 1837, *is appended to the preceding Report.*

No. of Exper.	Mark or Mixture of Iron used.	Deflection in ins. 1000.	Comparative Breaking wt.	Observations.
1	No. 1, Arigna pig	0·472	220	
2	No. 2, ditto	0·555	240	Maximum strength.
3	$\frac{1}{2}$ of No. 1, pig.. $\frac{1}{2}$ of No. 2, pig..	0·460	164	
4	$\frac{1}{2}$ of No. 1, pig.. $\frac{1}{2}$ of old scraps..	0·5605	236	
5	$\frac{1}{2}$ of No. 2, pig.. $\frac{1}{2}$ of old scraps..	0·617	236	
6	$\frac{1}{3}$ of No. 1, pig.. $\frac{2}{3}$ of old scraps..	0·4767	252	
7	$\frac{1}{3}$ of No. 2, pig.. $\frac{2}{3}$ of old scraps..	0·547	256	
8	$\frac{1}{4}$ of No. 1, pig.. $\frac{3}{4}$ of old scraps..	0·384	172	
9	$\frac{1}{4}$ of No. 2, pig.. $\frac{3}{4}$ of old scraps..	0·440	192	

Note.—The size of the bars used was 1·5 inch square by 3 ft. 6 in. long. The distance between the supports was 3 ft. 1 inch.

The comparative breaking weights in column the fourth, multiplied by 12, will give the absolute weight in pounds which broke the bars.

Notice of Experiments in Progress, at the desire of the British Association, on the Action of a Heat of 212° Fahr. when long continued, on Inorganic and Organic Substances. By Robert Mallet, *M.R.I.A.*

The original object of these experiments has been to try how far it is possible to form many mineral substances which we either have not formed in the laboratory, or which have only hitherto been produced by the dry method.

Many circumstances concur with the few scattered experiments which have been made on this subject in causing us to suppose that this may frequently be effected by the long-continued action of boiling water or steam, or both, on the constituents of the mineral to be formed.

These may be presented to the action of these agents either in a nascent state, as in silex and earths, or oxides recently precipitated, without being dried and mixed in the atomic proportions required to constitute a given mineral if combined; or the mineral may be formed by the mutual decomposition and recombination of other bodies, more or less difficultly soluble in boiling water. Attention has been directed to both methods, and indications are not wanting to give hopes of success in both. It has been, for instance, some time known, that chalcedony may be formed by the prolonged action of a boiling temperature upon gelatinized silica; while, on the other hand, it can be equally well formed by the decomposition of certain kinds of glass by the same means as remarked by the late Dr. Turner. Indeed, the extreme facility with which almost every kind of glass decomposes, under the continued action of boiling water, has greatly retarded these experiments. None has been found to answer the purpose but the hard Bohemian glass, (which is objectionable in point of expense,) and green bottle glass; the latter imperfectly.

The minerals as yet chosen for experiment have been chiefly hydrates, of which the following may serve as a type; the production of these has been attempted by direct combination of their constituents with excess of water.

	Formula adopted.
Chalcedony or Opal	$S_6 + HO$
Lenzinite	$A + S + HO$
Triklasite	$A + S_2 + HO$
Cymolite	$A + S_3 + HO$
Terre de Reigate	$A + S_4 + 3HO$

	Formula adopted.
Mesotype	$3\,A\,S + Na\,S_3 + 2\,H\,O$
Prehnite	$3\,A\,S + Ca_2\,S_3 + 2\,H\,O$
Steatite	$A\,S_2 + 2\,Ma\,S_2 + 4\,H\,O$
Chabasie	$3\,A\,S_2 + Ca\,S_3 + 6\,H\,O$
Analcime	$3\,A\,S_2 + Na\,S_2 + 2\,H\,O$
Harmotome	$4\,A\,S_2 + Ba\,S_4 + 6\,H\,O$
Killinite	$8\,A\,S_2 + K\,S_4 + 3\,H\,O$

Various uniaxal and biaxal micas, and some metallic sulphurets and sulpho-salts, have also been attempted by way of double decomposition.

The substances to be tried, when mixed and covered with water, are sealed in Bohemian glass tubes, numbered and exposed to steam in a box between two low-pressure boilers, in one or the other of which steam is always up.

Specimens of peat, of lignin, of coal, and other analogous organic bodies, have also been exposed in various ways, in the expectation that some light may be thrown upon the formation of coal and bitumens; and various supposed insoluble crystallized native minerals have also been exposed immersed in boiling water, in order to determine what its action, if any, may be on their crystals, and what effect may result from the dissolved matter. Various woods have been placed in contact with gelatinized silex and its solutions, in the hope of slowly forming silicifications similar to those from Antigua, &c.

These experiments, it is conceived, will connect themselves in a very interesting point of view with those in progress under Mr. Vernon Harcourt's superintendence upon the action of a much higher but indefinite temperature upon mineral bodies. There is every probability that very many of the minerals in the crust of the earth, especially the crystallized ones, have been formed at a comparatively low temperature. Quartz is daily deposited from the water of the Geysers, and has been found in a soft and pasty state elsewhere.—(*Beudant Traité.*) Malachite has been found in a similar state. Vauquelin found stalactitic quartz, (*Ann. de Chim.* xxi.). Crystals of quartz have been found in the United States, containing anthracite, and one containing a liquid with a piece of coal floating in it; and Mr. Haig found hard crystals of quartz in a bottle of Saratoga water, which had stood many years, (*Quart. Jour.* xv.). The globules of fluid found in amethyst, chrysoberyl, topaz, fluor spar, &c. &c., a fluid found by Dr. Brewster to be volatile at 75° Fahr., (*Edin. Phil. Jour.*); the existence of bitumen in basalt, serpentine, greenstone, mica, and many other minerals discovered by Mr. Knox (*Phil. Jour.*), and of fire damp in the vesicles of sal gem by Dumas; all these indicate the comparative low temperature at which the formation of many minerals has probably proceeded.

Coal too has been found in Scotland converted into plumbago by the proximity of a dyke, yet at such a distance that its communicated heat must have been extremely low. On the other hand, facts are not wanting to indicate the powerful effects of water and a moderate heat in decomposing and changing organic or organized bodies, as, for instance, the changes remarked by Perkins in the oil of his high-pressure steam engine, and very many similar known to the organic chemist.

Again the analytical chemist is familiar with abundant cases of the direct combination under favourable circumstances—of oxides with oxides, earths with earths, salts with salts, &c., to prove the likelihood of minerals being formed by synthesis without further decomposition resulting, than loss of constitutional water; as the combination of alumina and magnesia when precipitated together, giving a compound when ignited of $\mathrm{A}l_6 + \mathrm{M}g$, or colourless spinell, as remarked by Chenevix.

These scattered facts are sufficient to show that the experiments here indicated, while they belong to chemistry and mineralogy, abound in interest to the geologist. The experiments have not been sufficiently long in operation to yield definite results.

Provisional Reports and Notices of Progress in Special Researches, entrusted to Committees and Individuals.

Physical Section.

Professor Forbes contributed a notice of the experiments he has been some time prosecuting into the Temperature of the Earth at different depths, and in different sorts of rock. The results will be laid before a future meeting of the Association.

Sir J. Robison and Mr. J. S. Russell reported the progress of their investigations on Waves. As this subject has been again entrusted to the further examination of the Committee, it is thought proper to defer the publication of the results already obtained till the Committee shall present their complete report.

Mr. Baily reported that the Committee appointed to represent to government the importance of reducing the Greenwich observations of the moon, had waited on the Chancellor of the Exchequer, and that the sum of 2000*l.* had been appropriated for that purpose, which was placed at the disposal of the Astronomer Royal, who had undertaken to superintend the reductions.

Mr. Baily reported that the reduction of the stars intended to form the enlarged Catalogue of the Royal Astronomical Society was in progress; that a small portion only of the original sum appropriated had been expended; but that, in all probability, the whole would be required in the course of the ensuing year.

Mr. Baily reported that the reduction of the stars in the *Histoire Céleste*, &c., was in progress; that a small portion only of the sum appropriated had at present been expended; but that about half the amount would be required.

Mr. Baily added, that he had made repeated application to the Secretary of the Bureau des Longitudes for the corrected copy of the *Histoire Céleste* gratuitously offered by that Board for the use of the computers; but that he had not yet received any answer to such applications.

Report of the Committee for the Liverpool Observatory.

The Committee, after carefully examining the local circumstances of the port of Liverpool, and arranging the plan which seemed most expedient for the establishment of an Observatory at Liverpool, laid it before the local authorities, who approved of the proposed arrangement, and expressed their readiness to carry it into effect as soon as the necessary powers could be obtained from Parliament.

Chemical Section.

Apparatus for the Detection and Measurement of Gases present in minute quantity in Atmospheric Air. By Wm. West.

Mr. West produced and reported verbally upon his apparatus for the above purpose, for the construction of which the Association had, in a former year, voted the sum of 20*l.* By the action of a spiral spring in front of the drum or cylinder of Crosley's gasometer, a partial vacuum is produced, to fill which the air presses from without, and in its passage is conducted through several two-necked bottles filled with liquids fitted to combine with and detain the gases sought, as lime-water for carbonic acid, &c. The same gasometer registers the quantity of air thus deprived of the accidental and variable gas, while the quantity of gas separated is found by calculation from the precipitate formed in the bottles. The apparatus had been constructed too recently to admit of any results being obtained beyond preliminary trials, which promised well, both as to efficiency and accuracy.

Information was furnished of the progress made by Professor Liebig in the preparation of his report on Organic Analysis.

Professor Johnston read a preliminary report on Inorganic Analysis.

Geology and Geography.

The progress made by M. Agassiz in developing the Fossil Ichthyology of Great Britain was stated.

Natural History.

Sir W. Jardine, Bart., presented a report on the Salmonidæ of Scotland, and expressed his desire to continue the investigation. The full report will consequently appear hereafter.

Mr. Gray communicated a preliminary notice on the subject of the Perforation of Rocks by Mollusca.

Mr. Jenyns stated, that the Committee on preserving animal and vegetable substances in a moist state were in operation.

The commencement of Mr. Gould's Essay on the Caprimulgidæ was communicated.

Mr. Vigors stated, that considerable progress had been made by the Committee on the Irish Fauna.

Medical Science.

The London Committee on the Sounds of the Heart stated the circumstances which prevented the preparation of their report, and announced that it will be ready at the meeting in 1839.

"The Committee have been engaged almost daily for several hours during the months of June and July, in prosecuting the researches for which they were appointed, and have obtained many interesting results, particularly in relation to the sounds of the heart and arteries as signs of disease. These results were to be presented at the present Meeting of the Association; but the death of a colleague having prevented the member who was to draw up the report from completing it, the Committee are obliged to postpone it to the next meeting, when they trust that it will be made still more worthy of the attention of the Medical Section of the British Association."

Dr. Williams expressed his hope of being able to present a report on the Physiology of the Lungs and Bronchi in 1839.

Dr. Carson stated, that owing to the small number of cases of lung disease in animals which had occurred in-the Zoological Gardens of Liverpool in the winter of 1837–8, the Committee on that subject had not been enabled to make a report to the Newcastle Meeting, but intended to do so at the Birmingham Meeting.

Appendix to a Report on the Variations of the Magnetic Intensity (printed in vol. vi.). *By Major* E. SABINE, *F.R.S., &c.*

IN reference to the report on the Variations of the Magnetic Intensity, which the British Association have done me the honour to print in their last volume, I wish to communicate the results of the observations made by Captain Duperrey in his voyage of circumnavigation in the *Coquille*, in 1822—1825, which I have received in a private communication from that distinguished officer and magnetic observer. The Section will learn with pleasure the satisfactory accord of these observations with those of Captains De Freycinet and Fitz Roy, published in my report. When in compliance with the wishes of the Association, I first entertained the purpose of collecting in one body the observations of intensity made by different observers in all parts of the globe, so far as they are comparable with each other, one of my first steps was to write to Captain Duperrey to solicit the communication of any intensity results which he might have obtained. I find, by the letter which I have received, that Captain Duperrey did kindly comply with my request; but, unfortunately, the packet which must have contained the particulars of his observations has never reached me. The letter which I have received contains a notice, both of the results he obtained, and of the mode in which they were observed. Had I possessed this information at the time my report was printed, I should on every account have rejoiced to have embodied it in the report: and I am anxious to avail myself of this opportunity of doing what may yet be done to supply the omission. Captain Duperrey's observations were made with a horizontal needle, which, from accidental circumstances, was not observed with prior to his departure from France. The usual test of the permanency of the magnetism of the needle, viz. its vibration at the same station, at the commencement and at the close of the series, was, therefore, omitted in this case. In the absence of this, which is the most conclusive test, Captain Duperrey has estimated the loss which his needle may have sustained, by comparing its rate of vibration at Paris on his return, with its rate at a station in Peru, in the line of no dip, in which comparison he has assumed the relation of the force at that station to the force at Paris to be as 1 to 1·3482. The loss of magnetism sustained by the needle on this estimation was altogether inconsiderable. The times of vibration at four other stations at which this needle was employed, corrected for temperature and arc, give the following values of the total intensity.

Payta	5° 6′S...	278·50E...	1·024
Offak	0 2 ..	130·44 ..	1·079
Port Jackson ..	33 52 ..	151·12 ..	1·617
Isle of France ..	29 9 ..	57·31 ..	1·181

These determinations are inserted in a map engraved in 1832, referred to in a paper read by M. Duperrey to the Academy of Sciences at Paris in 1833, entitled, "Considérations sur le Magnetisme Terrestre." Captain Duperrey notices, that at two other stations, viz. Talcahuano and St. Catherine's, he observed the times of vibration of a dipping needle, the poles of which were reversed at each station, in the usual manner, for the observation of the dip; and that the results derived from the vibration of this needle, presuming it to have received, on every occasion when the poles were changed, an equal magnetic charge, correspond in a remarkable manner—as indeed they do,—with the subsequent observations of Captains King and Lutke; but Captain Duperrey, of course, attaches to these determinations no independent value, and therefore I need not notice them further. Captain Duperrey has also communicated to me three results obtained at stations in France in 1834, with one of M. Hansteen's needles, made, as it appears, with very great care, and with every necessary precaution. These results are, for

	Lat.	Long. W. Paris.	
Brest	48·24 ..	6·50 ..	1·365
Landevence ..	48·18 ..	6·35 ..	1·363
Orleans	47·54 ..	0·26 ..	1·341

I may take this opportunity also of adverting to the observations of Professor Bache and other gentlemen of the United States, which were not included in my report. These observations were made at New York, and in the adjoining states; and Mr. Bache is now engaged in connecting them with Europe, and, consequently, with the general body of the intensity observations. Until this comparison is complete, which it will not be until Mr. Bache returns to the United States, the observations referred to serve to determine the value of the magnetic force at the stations at which they are made, *relatively to each other*, but not *relatively to other parts of the globe*; and they were not, therefore, available for my report. The American observations were made with magnetic needles inclosed in a vacuum apparatus, which Mr. Bache had devised, with the view of avoiding some of the anomalies occasionally experienced by other observers. They were made with extreme care, and were remarkable for minute attention to all those circumstances which conduce to the accuracy of the results.

END OF THE REPORTS.

NOTICES

AND

ABSTRACTS OF COMMUNICATIONS

TO THE

BRITISH ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE,

AT THE

NEWCASTLE MEETING, AUGUST, 1838.

ADVERTISEMENT.

The Editors of the following Notices consider themselves responsible only for the fidelity with which the views of the Authors are abstracted.

CONTENTS.

NOTICES AND ABSTRACTS OF MISCELLANEOUS COMMUNICATIONS TO THE SECTIONS.

MATHEMATICS AND PHYSICS.

CHEMISTRY.

GEOLOGY.

GEOGRAPHY.

ZOOLOGY.

BOTANY.

MEDICAL SCIENCE.

MECHANICAL SCIENCE.

STATISTICS.

NOTICES AND ABSTRACTS

OF

MISCELLANEOUS COMMUNICATIONS

TO THE SECTIONS.

MATHEMATICS AND PHYSICS.

On a General Geometric Method. By CHARLES GRAVES, *F.T.C.D.*

MR. GRAVES was led to the views he was about to explain, from observing the use in the doctrine of Conic Sections of a theorem given by M. Chasles, in his "Histoire de la Géométrie," viz., that "*the enharmonic relation of four lines drawn from four fixed points in a conic section, to any fifth point in the curve, will remain invariable.*" Mr. Graves explained the term "*enharmonic relation,*" as employed by M. Chasles, to mean the ratio of Sin. (a, d), Sin. (b, c) to Sin. (a, b) Sin. (c, d); a, b, c, and d, being right lines diverging from the same point. He insisted on the importance of M. Chasles's theorem, as a kind of geometrical characteristic of the conic sections, defining them like an equation; and showed how it might be advantageously applied in the determination of loci, and also in the invention, proof, and generalization of theorems relating to the conic sections. In ascertaining whether the plane curve described by a point, subject to a certain condition, is a curve of the second degree or not, the general method that suggests itself is, to find four particular positions of the point, and to draw from these points right lines to any fifth point in the locus. If the enharmonic relation of these four lines be invariable, the curve will be a conic section, and not otherwise. Among several exemplifications of this method, Mr. Graves discussed the problem of finding the locus of the centres of all the conic sections passing through four given points. The middle points of the sides of the quadrilateral, at whose angles are the given points, being evidently situated on the locus, it was sufficient to show that the enharmonic relation of lines drawn from them to any other point in it was constant; and this follows immediately from a theorem announced by Mr. Graves, viz., that "*the enharmonic relation of four diameters of a central conic section, is the same as that of their four conjugates.*" In order to connect this mode of investigation with the ordinary algebraic method, Mr. Graves formed the equation of a conic section passing through the four points (x', o), $(-x'', o)$, (o, y'), $(o, -y'')$, (the axes of the co-ordinates being made to pass through the points,) and finding only the co-efficient of xy to remain indeterminate, he establishes the following equation between this co-efficient B, and (r) the enharmonic relation of four lines drawn from any point in the locus to the four given points, $r = \frac{x'y'' + x''y' - B}{x'y' + x''y'' + B}$. From this Mr. Graves deduced some elegant consequences, and pointed out the readiness with which M. Chasles's theorem serves to group together, and to prove other very general ones; such, for instance, as that of Pascal, relating to irregular hexagons, inscribed in conic sections, of which it furnishes by far the shortest and most elegant proof yet obtained. He concluded with the expression of a wish, that mathematicians would not disdain to employ the resources of geometry combined with analytic methods in the treatment of conic sections, many valuable properties of which have been lost sight of by those who seem to consider the study useful only as an exercise in the application of algebra to geometry.

A paper was read by Charles Ball, Esq., of Christ's College, Cambridge, "On the meaning of the Arithmetical Symbols for Zero and Unity, when used in General Symbolical Algebra."

On the Propagation of Light in vacuo. *By Professor Sir* W. R. HAMILTON, *F.R.S.*

The object of this communication was to advance the state of our knowledge respecting the law which regulates the attractions or repulsions of the particles of the ether on each other. The general differential equations of motion of any system of attracting or repelling points being reducible to the form

$$\frac{d^2 x}{dt^2} = \text{S}.\, m_, \Delta\, x\, f(r), \qquad (1.)$$

the equations of minute vibration are of the form

$$\frac{d^2\, \delta x}{dt^2} = \text{S}.\, m_, \Big(\Delta \delta x . f(r) + \Delta x . \delta f(r)\Big), \qquad (2.)$$

in which

$$\delta f(r) = f'(r)\, \delta r, \qquad (3.)$$

and

$$\delta r = \frac{\Delta x}{r} \Delta\, \delta x + \frac{\Delta y}{r} \Delta\, \delta y + \frac{\Delta z}{r} \Delta\, \delta z. \qquad (4.)$$

A mode of satisfying the differential equations (2), and at the same time of representing a large class of the phenomena of light, is to assume,

$$\frac{\delta x}{\xi'} = \frac{\delta y}{\eta'} = \frac{\delta z}{\zeta'} = \text{const.} + \cos. \frac{2\pi (v t - a x - b y - c z)}{\lambda}, \quad (5.)$$

in which ξ', η', ζ' are constants, depending on the extent and direction of vibration: a, b, c, are the cosines of the inclinations of the direction of propagation of a plane wave to the positive semi-axes of x, y, z; v is the velocity of propagation of that wave, and λ is the length of an undulation; and π is the semicircumference of a circle, of which the radius is unity. With this assumption (5.), and with a natural and obvious supposition respecting a certain symmetry of arrangement in the ether, causing the sums of odd powers to vanish, it is permitted to substitute in (2.) the expressions

$$\frac{d^2 \delta x}{d t^2} = -\left(\frac{2\pi v}{\lambda}\right)^2 . \delta x, \quad (6.)$$

$$\Delta \delta x = - \text{vers.} \Delta \theta . \delta x, \quad (7.)$$

in which $\Delta \theta = - \frac{2\pi}{\lambda}(a \Delta x + b \Delta y + c \Delta z)$; (8.)

and thus arises a system of conditions of the form

$$\xi' \left(\frac{2\pi v}{\lambda}\right)^2 = \xi' m \, \text{S.} \left\{ f(r) + \frac{\Delta x^2}{r} f'(r) \right\} \text{vers.} \Delta \theta$$
$$+ \eta' m \, \text{S.} \frac{\Delta x \Delta y}{r} f'(r) \, \text{vers.} \Delta \theta$$
$$+ \zeta' m \, \text{S.} \frac{\Delta x \Delta z}{r} f'(r) \, \text{vers.} \Delta \theta \quad (9.)$$

the masses m' of the etherial particles, being supposed each $= m$. Three conditions of this form (9.) exist for every particle, and determine, in general, for any given values of a, b, c, λ, that is, for any given direction of propagation, and any given length of wave, the value of v, and the ratios of ξ', η', ζ', that is, the velocity of propagation of the wave, and the direction of vibration of the particle. Accordingly, with some slight differences of notation, they have been proposed for this purpose by Cauchy, and adopted by other mathematicians. Suppose now, for simplicity, that the plane wave is vertical, so that $c = o$; and let, at first, the direction of its propagation coincide with the positive semi-axis of x, so that b also vanishes, and a is $= 1$. Then, for transversal vibrations, the expression for the square of the velocity of propagation is

$$v^2 = \left(\frac{\lambda}{2\pi}\right)^2 m \, \text{S} \left\{ f(r) + \frac{r^2 - \Delta x^2}{2 r} f'(r) \right\} \text{vers.} \frac{2\pi \Delta x}{\lambda}; \quad (10.)$$

which appears to extend not only to the interplanetary spaces, but also to all ordinary transparent media, and contains, for them, the theoretical law of dispersion, which was first discovered by Cauchy, namely, the expression

$$v^2 = A_0 - A_1 \lambda^{-2} + A_2 \lambda^{-4} \&c. \qquad (11.)$$

in which

$$A_i = \frac{(2\pi)^{2i} m}{1.2.3.4\ldots(2i+2)} S \left\{ f(r) + \frac{r^2 - \Delta x^2}{2r} f'(r) \right\} \Delta x^{2i+2}. \qquad (12.)$$

But, in order that this law may agree with the phenomena, it is essential that the series (11.) should be convergent, even in its earliest terms; and this consideration enables us to exclude the supposition which has occurred to some mathematicians, that the particles of the ether attract each other with forces which are inversely as the squares of the distances between them. For if we suppose $r f(r) = r^{-2}$, and therefore $f(r) = r^{-3}$, $f'(r) = -3 r^{-4}$, we shall have

$$A_i = \tfrac{1}{2} \frac{(2\pi)^{2i} m}{1.2.3.4\ldots(2i+2)} S \left\{ -r^{-3} + 3 r^{-5} \Delta x^2 \right\} \Delta x^{2i+2}; \qquad (13.)$$

and by extending the summation to particles, distant by several times the length of an undulation from the particle which they are supposed to attract, these sums (13.) become extremely large, and the terms of the series (11.) diverge very rapidly at first, though they always finish by converging. In fact, if we conceive a sphere, whose radius $= n\lambda$ $= n$ times the length of an undulation (n being a large multiplier), and whose centre is at the attracted particle; and if we consider only the combined effect of the actions of all the particles within this sphere, we may, as a good approximation, convert each sum (13.) into a triple definite integral, and thus obtain, for the general term of the series (11.), the expression

$$(-1)^i A_i \lambda^{-2i} = \frac{(-1)^i 4\pi m n^2 \lambda^2}{(2i+5)\epsilon^3} \cdot \frac{(2\pi n)^{2i}}{1.2.3\ldots(2i+3)}, \qquad (14.)$$

ϵ being the mean interval between any two adjacent particles of the ether, so that the number of such particles contained in any sphere of radius r, is nearly $= \frac{4\pi r^3}{3\epsilon^3}$, if r be a large multiple of ϵ.

And hence we find, by taking the sum of all these terms (14.), the expression

$$v^2 = \frac{\lambda^2 m}{\pi \epsilon^3} \left\{ \frac{1}{3} + \frac{\cos. 2\pi n}{(2\pi n)^2} - \frac{\sin. 2\pi n}{(2\pi n)^3} \right\}; \qquad (15.)$$

so that, by taking the limit to which v^2 tends, when n is taken greater and greater, we get at last as a near approximation

$$v^2 = \frac{\lambda^2 m}{3\pi\epsilon^3}, \qquad (16.)$$

and

$$\frac{\lambda}{v} = \sqrt{\frac{3\pi\epsilon^3}{m}}. \qquad (17.)$$

But $\frac{\lambda}{v}$ expresses the time of oscillation of any one vibrating particle; this time would therefore be nearly constant, if the particles attracted each other according to the law of the inverse square of the distance; and consequently this law is inadmissible, as being incompatible with the law of dispersion. It had appeared to Sir William Hamilton important to reproduce these results, though he remarked that they agree substantially with those of Cauchy, because the law of the inverse square was one which naturally offered itself to the mind, and had, in fact, been proposed by at least one mathematician of high talent. There was, however, another law which had great claims on the attention of mathematicians, as having been proposed by Cauchy to represent the phenomena of the propagation of the light *in vacuo*, namely, the law of a repulsive action, proportional inversely to the fourth power, or to the square of the square of the distance. M. Cauchy had, indeed, supposed that this law might hold good only for small distances, but in examining into its admissibility, it appeared fair to treat it as extending to all the neighbouring particles which act on any one. But against this law also, Sir William Hamilton brought forward objections, which were founded partly on algebraical, and partly on numerical calculations, and which appeared to him decisive.

The spirit of these objections consisted in showing that the law in question would give too great a preponderance to the effect of the immediately adjacent particles, and would thereby produce irregularities which are not observed to exist. In particular, if it be supposed that

$$\mathrm{S}.r^i \Delta x^2 = \mathrm{S}.r^i \Delta y^2 = \mathrm{S}.r^i \Delta z^2,$$
$$\mathrm{S}.r^i \Delta x^4 = \mathrm{S}.r^i \Delta y^4 = \mathrm{S}.r^i \Delta z^4,$$
$$\mathrm{S}.r^i \Delta x^2 \Delta y^2 = \mathrm{S}.r^i \Delta y^2 \Delta z^2 = \mathrm{S}.r^i \Delta z^2 \Delta x^2,$$

and also, in (5.), that $c = o$, $a = b$, and that λ is much greater than ϵ, it is found that the two values v^2 and $v_{,}^2$ of the square of the velocity v, corresponding to vertical and to horizontal but transversal vibrations, are connected by the relation

$$v_{,}^2 = -\frac{2}{3 v^2},$$

being expressed as follows:

$$v^2 = \frac{m}{4} \mathrm{S} \left(5 r^{-7} \Delta x^4 - r^{-3}\right),$$

$$v^2 = \frac{3m}{8} \mathrm{S} \left(r^{-3} - 5 r^{-7} \Delta x^4\right);$$

In conclusion, he offered reasons for believing that the law of action of the particles of the ether on each other resembles more the law which Poisson has in one of his memoirs proposed as likely to express the mutual action of the particles of ordinary and solid bodies, being perhaps of some such form as the following:—

$$rf(r) = -a \cdot b^{-\left(\frac{r}{g\epsilon}\right)^h} + a_{,} \cdot b_{,}^{-\left(\frac{r}{g_{,}\epsilon}\right)^{h_{,}}}; \qquad (18.)$$

b and $b_{,}$ being each greater than unity, and $g, g_{,}, h, h_{,}$ being some large positive numbers, while a and $a_{,}$ are constant and positive multipliers, and ϵ is, as before, the mean or average interval between two adjacent particles. With such a law there would be a nearly constant repulsion, if a be greater than $a_{,}$, and if g be less than $g_{,}$, as long as $\frac{r}{g\epsilon}$ is sensibly less than unity; but the force would rapidly change, as the distance r approached to $g\epsilon$, and would then become a nearly constant attraction, until r became nearly $= g_{,}\epsilon$; it would then diminish rapidly, and soon become insensible. Sir William Hamilton did not, however, intend to exclude the hypothesis, that the function $rf(r)$ may contain several alternations of such repulsive and attractive terms,—much less did he deny that at great distances it may reduce itself to the law of the inverse square.

On the Propagation of Light in Crystals. By Prof. Sir W. R. HAMILTON, *F.R.S.*

By continuing to modify the analysis of M. Cauchy in the manner already explained, he had succeeded in deducing, more satisfactorily than had in his opinion been done before, from dynamical principles, a large and important class of the phenomena of light in crystals; though much still remained to be done before it could be said that a perfect theory of light was obtained. He had employed, for the purposes of calculation, the supposition that the arrangement of the particles of the ether in a crystal differs from an exactly cubical arrangement only by very small displacements, caused by the action of the particles of the crystalline body; and had attended only to those indirect or reflex effects of the latter particles which are owing to the disturbances which they produce in the arrangement of the former particles: but he did not mean to assert that he had established any strong physical probability for this being the true *modus operandi* in crystals, though he thought the hypothesis had explained so much already that it deserved to be still further developed.

On some Points connected with the Theory of Light. By Professor POWELL, *F.R.S.*

At the last meeting, the author dwelt on the importance of extending observations on the refractive indices for the standard rays to more highly dispersive media. In prosecuting these inquiries, he has to report that a prism of chromate of lead, owing to the nature of the substance, will not enable him to determine the indices, as the whole spectrum is confused, no lines visible, and the violet end totally absorbed.

In the identification of certain of the standard rays of Fraunhofer some discrepancy appeared to exist between different representations. The author's attention is now directed to this point, among others connected with a more accurate repetition of his former approximate determinations of refractive indices, on which he is now engaged.

He wishes also to draw attention to questions connected with the application of photometry to the theory; especially to that referring to the power of the eye to judge of the equalization of lights, and the influence which the illumination of one space has upon that of another in juxtaposition. To show how great the uncertainty is, the following very simple experiment may be referred to. On receiving the rays of a candle on a white screen, and intercepting a portion of them by a clear plate of glass, the eye can recognise no difference in the illumination of the covered part. Yet, from both the first and second surfaces of the glass, there is a copious reflection.

With regard to the mathematical theory, he alludes to the important researches of Mr. Tovey, especially those on elliptic polarization. All the preceding investigations for integrating the differential equations for waves, including the dispersion, have proceeded on the supposition that certain terms vanish. This appears essential to the *general* solution. Mr. Tovey has, however, shown, that if those terms do not vanish, we have still a *particular* solution: and this applies to the case of light elliptically polarized. This case is absolutely excluded in the former investigations, which are therefore imperfect. The author has endeavoured to clear up some points connected with this inquiry. Upon the evanescence or non-evanescence of these terms simply depends the elliptic, circular, or rectilinear character of the vibrations. Corresponding to these mathematical conditions, are those of the arrangement of the ætherial molecules in the medium, or part of the medium, where the polarization is communicated. He has pointed out the connexion between these views and the investigations of Prof. Maccullagh, in which that gentleman connects with certain equations of motion the elliptic polarization in quartz, by which Mr. Airy had explained the results and laws of M. Biot.

On an Ocular Parallax in Vision, and on the Law of Visible Direction. By Sir D. BREWSTER, *K.H., F.R.S.*

The honour of suggesting or illustrating the law of visible direction belongs, said Sir David Brewster, to Dechales, Porterfield, and Reid. D'Alembert, in his "Doutes sur différentes questions d'Optique*," maintains that the action of light upon the retina is conformable to the

* Opuscules Mathématiques, tom. i. p. 266, 268.

laws of mechanics; and he adds, that it is difficult to conceive how the object could be seen in any other direction than that of a line perpendicular to the curvature of the retina, at the point where it is really excited. He then proceeds to investigate mathematically how the apparent magnitudes of objects would be affected, on the two suppositions, that the line of visible direction coincided with the refracted ray, or with a line perpendicular to the retina, at the point where the refracted ray fell upon it. On the first supposition, he finds that the apparent magnitude of small objects would be increased about 1–13th or 1–16th, if the anterior surface of the crystalline is supposed to have a radius of six lines in place of four. On the second supposition, namely, that of Porterfield and Reid, he finds that the apparent magnitude of objects would be increased nearly one-third, which, as he remarks, being contrary to experience, we cannot suppose that vision is thus performed, however natural the supposition may appear. "According to what line then," he continues, "do we perceive objects or visible points, which are not placed in the optic axis? This is a point which it appears very difficult to determine exactly and rigorously. However, as experience proves that objects of small extent, which are within the range of our eyes, do not appear sensibly greater than they are in reality, it follows, that the visible point, which sends a ray to the cornea, is seen sensibly in its place, and, consequently, this visible point is seen sensibly in the direction of a line joining the point itself and its image on the retina. But why is this the case? It is a fact which I will not undertake to explain*." This abandonment of the inquiry will appear the more remarkable, when we consider the assumptions from which D'Alembert has deduced the preceding results. He takes for granted the dimensions of the eye as given by Petit and Jurin; and he assumes Jurin's Index of Refraction for the human crystalline lens, though it is almost exactly the same as that of an ox, as given by Hawksbee. These, indeed, were the best data he could procure; but he should have inquired if the most probable law of visible direction was compatible with any other dimensions of the eye, and any other refractive powers of the humours, which were within the limits of probability; and, above all, he ought to have examined experimentally the truth of his fundamental assumption, that visible points are really seen in their true places when they are not in the axis of vision. In submitting this assumption to experiment, I had no difficulty in ascertaining that there exists an ocular parallax, and that this parallax is the measure of the deviation of the visible from the real direction of objects. It is nothing in the axis of the eye, and increases as the visible point is more and more distant from that axis; and hence it follows, that during the motion of the eye, when the head is immoveable, visible objects do not appear absolutely fixed, and have an apparent magnitude greater than their real magnitude. We are, consequently, not entitled to reject any law of visible direction, on the ground of its giving a position to visible points, and a magnitude

* Opuscules Mathématiques, tom. i. p. 27.

to visible objects, different from their true position and magnitude. Having removed this difficulty, I proceeded to examine the other data upon which D'Alembert reasoned. According to the anatomy of the eye which he adopted, the centre of curvature of the retina, which he supposes to be spherical, (as he does the eye-ball,) is equidistant from the extremity of the axis, or the *foramen ovale*, and the centre of the crystalline lens. This, however, is far from being the case. M. Dutour, M. Maurice, a recent and able writer on vision, and, which is of more consequence, Dr. Thomas Young, have all made the centre of curvature of the retina, at the bottom of the eye, coincident with the centre of the spherical surface of the cornea; and this centre, in place of being almost half way between the apex of the posterior surface of the lens and the *foramen ovale*, is actually almost in contact with that apex. The dissections of Dr. Knox, and of Mr. Clay Wallace, of New York, give results conformable with those of Dr. Young; and almost all these authors regard the human eye as a spheroid. When we add to these considerations the fact that the refractive power of the crystalline lens assumed by D'Alembert is nearly triple of what it really is, we have no scruple in concluding that the results of his calculations are inadmissible. Assuming, then, the most correct anatomy of the eye, namely, that according to which the cornea and the bottom of the retina have the same centre of curvature, it is very clear that if there was no crystalline lens, pencils incident perpendicularly upon the cornea will pass through this common centre, and fall perpendicularly upon the retina. Hence, in this case, the line of visible direction will coincide with the line of real direction, and also with the incident and refracted ray, and will likewise pass through the centre of curvature of the retina. Now, the refractions at the surfaces of the crystalline are exceedingly small, and at moderate inclinations to the axis the deviations from the preceding law are very minute. At an inclination of 30°, a line perpendicular to the point of impression on the retina passes through the common centre already referred to, and does not deviate from the line of real visible direction more than half a degree, a quantity too small to interfere with the purposes of vision. At greater inclinations to the axis of the eye, the deviation of course increases; but as there is no such thing as distinct vision out of the axis, and as the indistinctness increases with the inclination of the incident ray, it is impossible to ascertain by ordinary observation that such a deviation exists. Hence, the mechanical principle of D'Alembert, and the law of Dr. Reid, are substantially true. If the retina is spheroidal, the centre of visible direction will shift its place along the axis of vision, and will correspond to the points where lines perpendicular to the surface of the spheroid cut its lesser axis. As the Almighty has not made the eye achromatic, because it was unnecessary, so he has, in the same wise economy of his power, not given it the property of seeing visible points in their real directions.

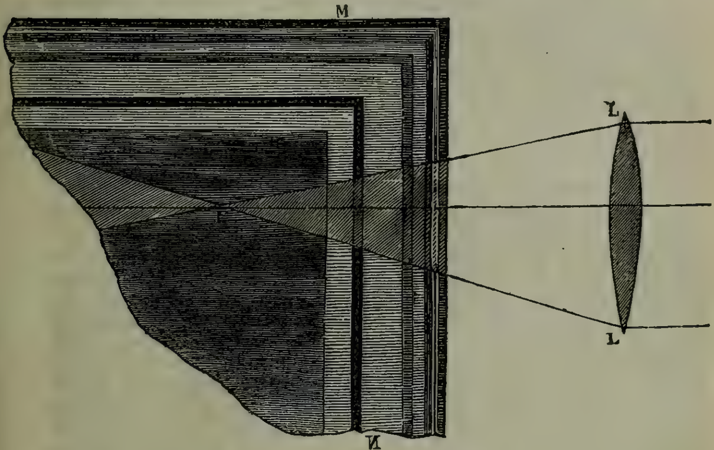
On a New Phenomenon of Colour in certain specimens of Fluor Spar. By Sir D. BREWSTER.

Mineralogists have long ago observed, in certain varieties of fluor spar, a beautiful *blue colour*, different from that which is seen by transmitted light. Haüy noticed this property in some of the fluor spars from Derbyshire. Succeeding mineralogists, however, have confounded this colour with the ordinary tints of the spar, and, so far as the author knows, its nature and origin have not been successfully investigated. In describing a species of dichroism, noticed by Dr. Prout* in the purpurates of ammonia and potash, Sir John Herschel† ascribes the reflected green light to "some peculiar conformation of the green surfaces, producing what may be best termed a *superficial colour*, or one analogous to the colour of thin plates, and striated or dotted surfaces." And he adds—"A remarkable example of such superficial colour, differing from the transmitted tints, is met with in the green fluor of Alston Moor, which on its surfaces, whether natural or artificial, exhibits, in certain lights, a *deep blue* tint, not to be removed by any polishing." As the phenomenon which Sir D. Brewster had studied in the Derbyshire fluors was clearly one of internal structure, he was led to suppose that the superficial colour seen by Sir John Herschel on the Alston Moor specimens, belonged to another class of phenomena; but having attempted in vain to communicate the blue colour of the Alston Moor crystals to *wax* or *isinglass*, he is disposed to believe that the two phenomena are identical. In the fluors from Derbyshire, which consist of differently coloured strata parallel to the faces of the cube, the blue colour is most powerfully developed in the purplish brown or bluish brown strata, in a less degree in the greenish strata, and scarcely, if at all, in those layers which are colourless by transmitted light. In the first of these cases, the blue colour may be distinctly seen emanating from the interior of the crystal, when it is held in the common light of day. In the sun's light the colour is still more brilliant; but the effect may be greatly increased by covering the greater part of the crystal with black wax, or by immersing it in a trough of glass covered externally with wax, and containing an oil of nearly the same refractive power as the spar. If there are fissures within the crystal, they will greatly influence the effect of the experiment, by reflecting to the eye the transmitted light. In order, however, to witness this experiment in all its beauty, and to have ocular evidence of its nature and character, a beam of condensed solar light should be transmitted through the crystal, as shown in the annexed figure, where L L is the condensing lens, F its focus, and M N the system of differently coloured layers, traversed by the cone of refracted rays. The first layer of spar reflects in all directions an *intensely blue* light; the two adjacent layers (separated by a thin layer which reflects blue light) reflect a light nearly white; the next layer gives a blue of exceeding brilliancy; and so on with the other layers, till the cone reaches the brown central nucleus, which also reflects a rich blue tint, though inferior to that of one of

* Phil. Trans. 1818, p. 424.

† Treatise on Light, sec. 1076.

the preceding layers. In the green fluor of Alston Moor there are also different layers, some of which are *pink*, and some of different shades of *green*; but the different shades of blue which they give out under exposure to strong light, are not so strikingly contrasted as in the Derbyshire specimens. As the blue colour now described is reflected from surfaces within the spar, and as it does not occur in all



specimens, nor in every part of the same crystal, it must be produced by extraneous matter of a different refractive power from the spar, introduced between the molecules of the crystal during its formation. That the blue colour is not produced by shallow cavities or minute pores, as in some of the opals, is inferred from the perfect transparency of the specimens in which it occurs, and from the fact that the same reflected tints are found in fluids, particularly the juices of plants extracted by alcohol, and in several artificial glasses, particularly in those of a *pink* and *orange* colour, the former of which give a *blue* and the latter a *green* colour. Having found that some of the dichroitic colours in doubly refracting crystals were discharged by heat, it occurred to the author that the blue tints in fluor spar might suffer a similar change, and might even be connected with the phosphorescence of the mineral. He therefore exposed two pieces, one of the Derbyshire and one of the Alston Moor fluor, to a considerable heat. Both of them gave out a blue phosphorescence, similar to that of the reflected tint, and much of the natural colour of the fragments was discharged by the heat. In both specimens the blue reflected tint was greatly diminished. In another specimen of the Alston Moor fluor, it appeared to be wholly removed; but in a third, taken from the solid angle of the cube, the blue tint still appeared, though with an impaired brilliancy. It is possible, that a very intense heat might discharge the blue tint altogether, but it is difficult to obtain satisfactory results with a mineral which decrepitates by the action of heat, and thus prevents the observer from comparing the tints under circumstances exactly the same.

An Account of certain New Phenomena of Diffraction. By Sir D. Brewster.

The phenomena of the inflexion or the diffraction of light observed by Sir Isaac Newton, Fresnel, and others, were those which are visible at a greater or less distance behind the diffracting body, and according to the undulatory theory they are produced by the secondary waves which fall converging on the points where the fringes appear within and without the geometrical shadow. These fringes are all calculable by a formula given by Fresnel, depending on the relation of the two quantities a and b, a being the distance of the place where the fringes are formed from the diffracting body, and b the distance of the diffracting body from the point from which the beam of light diverges. In the phenomena hitherto studied, the quantity a is always *positive*. The new phenomena discovered and described by Sir David Brewster are those in which a is negative; and they may be represented by a formula differing from Fresnel's only in the sign of a. These new phenomena are rendered visible by bringing lenses of different foci in contact with the diffracting body, and the fringes seen in any case are those belonging to a value of $-a$ equal to the focal distance of the lens. The fringes are in this case produced by the secondary waves, which proceed *diverging* from the main wave, from a point between the diffracting body and the luminous centre, whose distance from the former is a. When $-a$ is equal to b, the fringes are formed in *parallel* rays; and when the diffracting body is placed between the lens and the eye, they are formed in converging rays. Hence, in studying these phenomena, we may use a telescope with a micrometer, and obtain accurate measures. These phenomena were illustrated by diagrams.

An Account of an Analogous Series of New Phenomena of Diffraction when produced by a Transparent Diffracting Body. By Sir D. Brewster.

These phenomena, when carefully produced by the various methods which he explained, exhibited a series of splendidly coloured bands of light, sometimes perfectly symmetrical and sometimes unsymmetrical, accordingly as the diffracting body was regular or irregular in its section; and the author remarked, that an instrument could thus be constructed for giving new patterns of ribands of all forms and colours. The theory of the phenomena he considered quite simple and obvious, but he stated that a comparison of the results of theory and experiment would be difficult, from the difficulty of ascertaining the exact form of the diffracting body.

On the Combined Action of Grooved Metallic and Transparent Surfaces upon Light. By Sir D. Brewster.

The phenomena described in this paper, discovered by the author, were altogether new and of a very remarkable description. The spectra, produced by the methods which were explained to the meeting, were covered with bands like those produced by the action of *nitrous gas* upon the spectrum, and the phenomena varied with the distance of the grooves, with the relation of the dark and luminous intervals, and with the inclination of the incident ray. Sir David Brewster described analogous phenomena and others of a remarkable character when the grooves were made in *transparent* surfaces; and he explained to the Section the manner in which he conceived the phenomena were produced, on the principles of interference.

On a New Kind of Polarity in Homogeneous Light. By Sir D. Brewster.

At the last meeting of the Association Sir D. Brewster communicated an account of a *new property of light*, which did not admit of any explanation. Since that time he has had occasion to repeat and vary the experiments; and having found the same property exhibited in a series of analogous though different phenomena, he has no hesitation in considering this property of light as indicating a new species of *polarity* in the simple elements of light, whether *polarized* or *unpolarized*. In the original experiment, two pencils of perfectly homogeneous light, emanating from the same part of a well-formed spectrum, interfered after one of them had been retarded by transmission through a thin plate of glass. The fringes were exceedingly black, but no phenomena of colour were visible. He was anxious to observe what would take place when the retarded pencil passed through the edges of various plates differing very little in thickness, so that different parts of it suffered different degrees of retardation, for the preceding experiment entitled him to expect a series of overlapping bands and lines of different sizes. In making such an experiment, however, he encountered great difficulties, and he failed in every attempt to combine such a series of thin edges. He had recourse therefore to laminated crystals, and in an accidental cleavage of *sulphate of lime* he obtained the desired combination of edges. "Upon looking through this plate at a perfect spectrum, in the manner described in my former communication, I was surprised to observe a splendid series of *bands* and *lines* crossing the whole spectrum, and shifting their place and changing their character by the slightest inclinations of the plate. But what surprised me most was to perceive that the spectrum exhibited the same phenomena as if it had been acted upon by absorbing media, so that we have here *dark lines* and the effects of local absorptions produced by the interference of an unretarded pencil with other pencils, proceeding in the same path with different degrees of retardation. The bearing of this unexpected result upon some of the most obscure questions in physical optics, I may have another opportunity of explaining. At present, I beg the attention of the meeting to another part of the experiment. We have seen that the effects of interference are distinctly developed in a certain position of the retarding plates. This position, when the effects are most distinct, is that in which the edges of the plates are turned *towards* the red end of the spectrum and are parallel to its fixed lines. If we give the plates a motion of rotation in their own plane, the bands and lines and the phenomena of absorption become less and less distinct as the angle between the edges of the plates and the lines of the spectrum increases. When this angle is 90° the bands disappear altogether, and during the next 90° of rotation they continue invisible. At 270° of azimuth they begin to reappear, and attain their maximum distinctness at 360°, when they have returned to their original position. Here then we have certain phenomena of interference, and also of absorption, distinctly exhibited when the least refrangible side of the retarded ray is towards the *most* refrangible side of the spectrum, or towards the most refrangible side of the unretarded ray; while the same phenomena disappear altogether when the *most* refrangible side of the retarded ray is towards the *least* refrangible side of the unretarded ray; and between these two opposite positions we have phenomena of an intermediate character. Hence I conclude, that the different sides of the rays of homogeneous light have different properties when they are separated by prismatic refraction or by the diffraction of grooved surfaces or gratings,—that is, *these rays have polarity*. When light is rendered as homogeneous as possible by absorption, or when it is emitted in the most homogeneous state by certain coloured flames, it exhibits none of the indications of polarity above mentioned. The reason of this is, that the more or less refrangible sides of the rays lie in every direction, but as soon as these sides are arranged in the same direction by prismatic refraction or by diffraction, the light displays the same properties as if it had originally formed part of a spectrum."

On some Preparations of the Eye by Mr. Clay Wallace, of New York. By Sir D. Brewster.

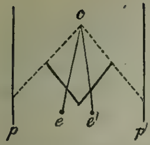
Sir David Brewster laid before the Section a series of beautiful preparations of the eye made by Mr. Clay Wallace, an able oculist in New York, calculated to establish some important points in the theory of vision. Mr. Clay Wallace, he stated, considers that he has discovered the apparatus by which the eye is adjusted to different distances. This adjustment is, he conceives, effected in two ways. In eyes which have *spherical lenses* it is produced by a *falciform*, or hook-shaped muscle, attached only to one side of the lens, which by its contraction brings the crystalline lens nearer the retina. In this case, it is obvious that the lens will have a slight motion of rotation, and that the diameter, which was in the axis of vision previous to the contraction of the muscle, will be moved out of that axis after the adjustment, so that at different distances of the lens from the retina different diameters of it will be placed in the axis of vision. As the diameters of a sphere are all equal and similar, Mr. Clay Wallace considered that vision would be equally perfect along the different diameters of the lens, brought by rotation into the axis of vision. Sir David Brewster, however, remarked that he had never found among his numerous examinations of the lenses of fishes any which are perfectly spherical, as they were all either *oblate* or *prolate* spheroids, so that along the different diameters of the solid lens the vision would not be similarly performed. But, independent of this circumstance, he stated that in every solid lens there was only one line or axis in which vision could be perfectly distinct, namely, the axis of the optical figure, or series of *positive* and *negative* luminous sectors, which are seen by the analysis of polarized light. Along every other diameter the optical action of the lens is not symmetrical. When the lens is not a *sphere*, but *lenticular*, as in the human eye or in the eyes of most quadrupeds, Mr. Clay Wallace considers that the apparatus for adjustment is the ciliary processes, to which this office had been previously ascribed, though not on the same scientific grounds as those discovered by Mr. Wallace. One of the most important results of Mr. Wallace's dissections is the discovery of *fibres in the retina*. These fibres may be rendered distinctly visible. They diverge from the base of the optic nerve, and surround the *foramen ovale* of Soemmerring at the extremity of the eye. Sir John Herschel had supposed such fibres to be requisite in the explanation of the theory of vision, and it is therefore doubly interesting to find that they have been actually discovered.

On the Structure of the Vitreous Humour of the Eye of a Shark. By Sir J. W. F. HERSCHEL, *Bart.*

Sir J. Herschel states, that while crossing the Atlantic on his return from the Cape, a shark was caught in lat. 2° N. and long. about 20° W. Having procured the eyes, which were very large, and extracted the crystalline lenses, the vitreous humour of each, in its capsule, presented the usual appearance of a very clear, transparent, gelatinous mass, of little consistency, but yet forming, very distinctly, a connected and continuous body, easily separable from every other part. Wishing to examine it more narrowly, it was laid to drain on blotting-paper; and, as this grew saturated, more was applied, till it became apparent that the supply of watery liquid was much too great to be accounted for by adhering water or aqueous humour. "Becoming curious to know to what extent the drainage might go, and expecting to find that, by carrying it to its limit, a gelatinous principle of much higher consistency might be insulated, I pierced it in various directions with a pointed instrument. At every thrust a flow of liquid, somewhat ropy, but decidedly not gelatinous, emanated; and, by suspending it on a fork, and stabbing it in all directions with another, this liquid flowed so abundantly, as to lead me to conclude that the gelatinous appearance of this humour, in its natural state, is a mere illusion, and that, in fact, it consisted of a liquid no way gelatinous, inclosed in a structure of transparent and, consequently, invisible cells. The vitreous humour of the other eye, insulated as far as possible, was therefore placed in a saucer, and beaten up with a fork, in the manner of an egg beaten up for culinary purposes. By this operation, the whole was resolved into a clear watery liquid, in which delicate membranous flocks could be perceived, and drawn out from the water in thready filaments, on the end of the fork. From this experiment, it is clear that the vitreous humour (so called) of this fish is no jelly, but simply a clear liquid, inclosed in some close cellular structure of transparent membranous bags, which, by their obstruction to the free movements of the contained liquid, imitate the gelatinous state."

On Binocular Vision; and on the Stereoscope, an instrument for illustrating its phenomena. By Professor WHEATSTONE.

Professor Wheatstone stated that, at the last meeting of the Royal Society, he had presented the first of a series of papers on the phenomena of vision, in the investigation of which subject he had been for some years engaged. On the present occasion he proposed merely to state so much as would enable him to explain the experiments which the apparatus on the table was intended to exhibit. This apparatus he called a Stereoscope, from its property of presenting to the mind the perfect resemblances of solid objects. To understand the principles on which it was constructed, he explained the circumstances which enable us to distinguish an object in relief from its representation on a plane surface; he showed that when a solid object, a cube for instance, was placed at a short distance before the eyes, its projections on the two retinæ form two dissimilar pictures, which in some cases are so different, that even the eye of an artist would with difficulty recognise them as representations of the same object; notwithstanding this dissimilarity of the two pictures, the object is seen single; and hence it is evident that the mind perceives the object in relief, in consequence of the simultaneous perception of the two monocular pictures. He next showed, that if the object were thus drawn, first as it appears to the right eye, and then as it appears to the left eye, and those two pictures be presented one to each retina, in such manner that they fall on the parts as the projections from the object itself would, the mind perceives a form in relief, which is the perfect counterpart of the object from which the drawings have been taken: the illusion is so perfect, that no effort of the imagination can induce the observer to suppose it to be a picture on a plane surface. Professor Wheatstone described various modes by which the two monocular pictures might be made to fall on similar parts of the two retinæ; but he gave the preference to a method which may be understood by the annexed diagram.



e é are the two eyes of the observer placed before two plane mirrors, inclined to each other at an angle of 90°; the axes of the eyes converge to a point *c*; the pictures *p p'* are so placed on sliding panels, that their reflected images may be adjusted to appear at the place of convergence of the optic axis; it is obvious, then, that the pictures on the retinæ will be precisely the same as if they proceeded from a real object placed at *c*. In this manner may solid geometrical forms, crystals, flowers, busts, architectural models, &c. be represented with perfect fidelity, as if the objects themselves were before the eyes. The law of visible direction, which is universally true, for all cases of monocular vision, may, Professor Wheatstone stated, be extended to binocular vision, by the following rule: That every point of an object of three dimensions is seen at the intersection of the two lines of visible direction, in which that point is seen by each eye singly.

Observations on Stars and Nebulæ at the Cape of Good Hope. By Sir John F. W. Herschel, *Bart. F.R.S., &c.*

The notice of these observations laid before the Section reduced itself to the following heads:

Reduced Observations of 1232 Nebulæ and clusters of Stars, made in the years 1834, 5, 6, 7, 8, at the Cape of Good Hope, with the twenty-feet Reflector.

Reduced Observations of 1192 Double Stars of the Southern Hemisphere.

Micrometrical Measures of 407 principal Double Stars of the Southern Hemisphere, made at the Cape of Good Hope, with a seven-feet Achromatic Equatorial Telescope.

A List of the Approximate Places of fifteen Planetary and Annular Nebulæ of the Southern Hemisphere, discovered with the twenty-feet Reflector.

Drawings illustrative of the Appearance and Structure of three principal Nebulæ in the Southern Hemisphere.

The observations in the first two of these communications form parts of two catalogues of southern nebulæ and double stars respectively, which comprehend the chief results of the author's astronomical observations at the Cape. They are complete only as far as the first nine hours in right ascension. In the other hours, only a few of the objects which occur are added, being the results of a partial and very incomplete reduction of the observations in those hours. Sir John Herschel considered it probable, that when the reduction of his observations shall enable him to complete these catalogues, the total number of objects contained in them will be nearly doubled. The first catalogue contains all the nebulæ and clusters comprised in the two Magellanic clouds, which are very numerous. Each reduced observation expresses the mean right ascension and north polar distance of the object for the beginning of 1830, together with a description (in abbreviated language), more or less detailed, of its appearance and physical peculiarities—as to size, degree of brightness, condensation, &c. The observations of double stars in the second catalogue express the mean place for the epoch above named—the angle of position of the stars with the meridian, as micrometrically measured at the time of observation—the estimated distance, and the magnitude assigned to each star, together with a column of remarks, in which peculiarities of colour or other phenomena are noted. The micrometrical measures in the third paper were taken with the same achromatic and micrometer, and are arranged in precisely the same manner as the former similar observations made by the author, which have been printed in the Transactions of the Astronomical Society. Among the principal double stars in this work occur, α Centauri, α Crucis, γ Centauri, γ Lupi, μ Lupi, π Lupi, β Hydræ, ϵ Chameleontis, γ Piscis Volantis, γ Coronæ Australis, &c. Of these, the measures therein stated afford unequivocal evidence of rotation in several of the double stars, among which may be particularized α Centauri, β Hydræ, γ Coronæ, and π Lupi. In the case of α Centauri, the diminution of distance, even within the comparatively short period of observation, is remarkable; and the author stated verbally, that on examining the catalogue of the Astronomical Society, that of Captain Johnson, and the Paramatta Catalogue, in all which the places of the two stars are given separately, he finds this diminution of distance fully borne out, and regularly progressive; from which he is led to conclude, that in no great number of years from the present time (fifteen or twenty), the stars may be expected to appear in contact, or to be actually occulted one by the other, as has recently been observed to happen to γ Virginis. The fourth of these communications is a list of the planetary and annular nebulæ of the Southern Hemisphere, which have been detected by Sir J. Herschel in his sweeps. They are arranged in order of R.A., and numbered. Among these, several are somewhat elongated, and offer the appearance of being double. One of them (No. 7) is of a fine blue colour, and being particularly well defined, has exactly the aspect of a blue planet. No. 4 is a very bright and considerably large elliptic disc of uniform light, on which, but excentric, is placed a pretty large star. Several are very small; No. 15, in particular, is not more than $3''$ or $4''$ in diameter. Many of them occur in crowded parts of the Milky Way, with not fewer than 80 or 100 stars in the field of view at the same time.—The drawings above mentioned were copies of much more elaborate originals, and were produced merely as specimens selected from a greater collection, illustrative of three of the most singularly constituted nebulæ in the Southern Hemisphere, viz. θ Orionis, η Argus, and 30 Doradus. Sir John Herschel gave several examples from the voluminous tables of the manner of registering the observations respecting each star, double star, clusters, and nebulæ; he also explained how, by the contrivance of a small achromatic collimator placed inside of his great sweeping telescope, he was able to obtain nearly the same precision in his observations as was to be had in fixed observatories: although, from the ropes and wooden frame with which it was mounted, it was subjected to great hygrometic and pyrometric changes of form and position. These changes, however, by equally affecting the cross of the collimator, and the object itself, were readily detected and corrected.

On Halley's Comet. *By Sir* John F. W. Herschel, *Bart. F.R.S., &c.*

"One of the most interesting series of observations of a miscellaneous kind I had to make at the Cape of Good Hope, was that of Halley's comet.—I saw the comet for the first time after its perihelion passage on the night of the 25th of January. Mr. Maclear saw it on the 24th. From this time we both observed it regularly. Its appearance was that of a round, well-defined disk, having near its centre a very small bright object exactly like a small comet, and surrounded by a faint nebula. This nebula in two or three more nights was absorbed into the disc, and disappeared entirely. Meanwhile, the disc itself dilated with extraordinary rapidity; and by examining its diameter at every favourable opportunity, and laying down the measures by a projected curve, I found the curve to be very nearly a straight line, indicating a uniform rate of increase; and by tracing back this line to its intersection with the axis, I was led, at the time, to this very singular conclusion, viz. that on the 21st of January, at 2h. P.M., the disc must have been a point—or ought to have had no magnitude at all! in other words, at that precise epoch some very remarkable change in the physical condition of the comet must have commenced. So far all was speculation. But in entire harmony with it is the following fact communicated to me no longer ago than last month by the venerable Olbers, whom I visited in my passage through Bremen, and who was so good as to show me a letter he had just received from M. Boguslawski, Professor of Astronomy at Breslau, in which he states that he had actually procured an observation of that comet on the night of the 21st of January. In that observation it appeared as a star of the sixth magnitude—a bright concentrated point, which showed no disc, with a magnifying power of 140! And that it actually *was* the comet, and no star, he satisfied himself, by turning his telescope the next night on that point where he had seen it. It was gone! Moreover, he had taken care to secure, by actual observation, the place of the star he observed; that place agreed to exact precision with his computation; that star *was* the comet, in short. Now, I think this observation every way remarkable. First, it is remarkable for the fact, that M. Boguslawski was *able* to observe it at all on the 21st. This could not have been done, had he not been able to direct his telescope point blank on the spot, by calculation, since it would have been impossible in any other way to have known it from a star. And, in fact, it was this very thing which caused Mr. Maclear and myself to miss procuring earlier observations. I am sure that I must often have swept, with a night-glass, over the very spot where it stood in the mornings before sunrise; and never was astonishment greater than mine at seeing it riding high in the sky, broadly visible to the naked eye, when pointed out to me by a notice from Mr. Maclear, who saw it with no less amazement on the 24th. The next remarkable feature is the enormously rapid rate of dilatation of the disc and the absorption into it of all trace of the surrounding nebula. Another, is the interior cometic nucleus. All these phenomena, while they contradict every other hypothesis that has ever been advanced, so far as I can see, are quite in accordance with a theory on the subject which I suggested on the occasion of some observations of Biela's comet,—a theory which sets out from the analogy of the precipitation of mists and dews from a state of transparent vapour on the abstraction of heat. It appears to me that the nucleus and grosser parts of the comet must have been entirely evaporated during its perihelion, and reprecipitated during its recess from the sun, as it came into a colder region; and that the first moment of this precipitation was precisely that which I have pointed out as the limit of the existence of the disc, viz. on the 21st of January, at 2h. P.M., or perhaps an hour or two later."

On the Difference of Longitude between London and Edinburgh. By Sir THOMAS M. BRISBANE, *F.R.S.*

Having observed the surprising accuracy with which the difference of longitudes of London and Paris had been obtained by Mr. Dent's chronometers, Sir Thomas Brisbane applied to that gentleman, who, with great liberality, furnished for the purpose of the experiments twelve of his valuable chronometers. With these, the differences of longitude of London, Edinburgh, and Mukerstoun were taken; and by a mean of all the observations taken in going to the latter station and in returning, they were found to differ only by five one-hundredths of a second. He exhibited to the Section the following table.

Chronometers.	Difference of Longitudes.			
	Going.	Returning.	Mean of Going and Returning.	
	m. s.	m. s.	m. s.	
A	2 40.14	2 39.10	2 39.62	−0.16 Minimum difference.
B	... 39.68	... 39.66	... 39.67	
C	... 39.85	... 39.52	... 39.68	
D	... 39.68	... 39.96	... 39.82	
E	... 39.66	... 39.89	... 39.78	
F	... 40.33	... 39.92	... 40.12	
G	... 39.48	... 39.95	... 39.72	+0.34 Maximum difference.
H	... 39.79	... 40.13	... 39.96	
I	... 39.99	... 39.59	... 39.79	
K	... 40.03	... 39.68	... 39.86	
L	... 39.76	... 39.73	... 39.74	
M	... 39.52	... 39.75	... 39.64	
Means	2 39.83	2 39.74	2 39.78	

On the means adopted for correcting the Local Magnetic Action of the Compass in Iron Steam-ships By G. B. Airy, *F.R.S., Astronomer Royal. (In a letter to Rev. Prof. Whewell.)*

In this communication, the author states some of the principal results of a series of observations and experiments (made at the request of the Admiralty) for correcting the local magnetic action on the compass in the steam-ship the Rainbow.

" The compass was placed in four different stations near the deck, and in four stations about 13 feet above the deck; and for each of these the ship was turned round, and the disturbance observed in many positions. The disturbances even at the upper stations were great, but at all the lower stations they were very great, and at the station next the stern they were enormous. The whole amount there was 100° (from − 50° to + 50°); and on one occasion, in turning the vessel about 24°, the needle moved 74° in the opposite direction. I should have perhaps found some difficulty in reducing these to laws if I had not made some observations of the horizontal intensity at the four lower stations in different positions of the ship. From these I was able to infer the separate amounts of disturbance due to the permanent magnetism of the ship and to the induced magnetism, and to construct correctors. These correctors I tried yesterday, completely at the sternmost station, and imperfectly at two others. The correction at the sternmost station was (speaking generally) complete; the extreme of deviation, which formerly exceeded 100°, did not, with the corrector, exceed 1°. At the other stations I had not leisure to adjust the apparatus: but I fully expect to-morrow to produce the same accordance at them. This result is, I should think, important in a practical sense. Some theoretical results which I did not anticipate are also obtained. At the stern position, the disturbance is produced almost entirely by the permanent magnetism, the inductive magnetism producing only $\frac{1}{25}$ of the whole effect. Going towards the head, the effect of the permanent magnetism diminishes, and that of the inductive magnetism increases, till the latter produces about $\frac{1}{3}$ of the whole effect. The resolved part of the permanent magnetism transverse to the ship varies little (increasing somewhat towards the head): the part longitudinal to the ship decreases rapidly from the stern to the head (where it is less than the transverse part)."* "G. B. Airy."

A Statement of the Progress made towards developing the Law of Storms; and of what seems further desirable to be done, to advance our knowledge of the subject. By Lieut.-Colonel Reid, *Royal Engineers.*

Having been ordered, in the course of military duty, to the West Indies in 1831, the author arrived at Barbadoes immediately after the

* A memoir containing the full investigation of this subject has been presented to the Royal Society, and is expected to appear in the forthcoming volume of the Philosophical Transactions.

great hurricane of that year, which, in the short space of seven hours, killed upwards of 1400 persons on that island alone. He was for two years and a half daily employed as an engineer officer, amidst the ruined buildings, and was thus naturally led to the consideration of the phenomena of hurricanes, and earnestly sought for every species of information which could give a clue to explain them.

The first reasonable explanation met with was given in a small pamphlet, extracted from the American Journal of Science, written by W. C. Redfield, of New York.

The gradual progress made in our acquaintance with the subject of storms is not uninteresting. The north-east storms on the coast of America had attracted the attention of Franklin. One of these storms preventing his observing an eclipse of the moon in Philadelphia, he was much surprised to find that the eclipse had been visible at Boston, which town is north-east of Philadelphia: this was a circumstance not to be lost on such an inquiring mind as Franklin's. By examination he ascertained that this north-east storm came from the south-west; but he died before he had made the next step in this investigation.

Colonel Capper, of the East India Company's Service, after having studied meteorological subjects for twenty years in the Madras territory, wrote a work on the winds and monsoons in 1801. He states his belief that hurricanes will be found to be great whirlwinds, and that the place of a ship in these whirlwinds may be ascertained; for, the nearer to the vortex, the faster will the wind veer; and subsequent inquiries prove that Colonel Capper was right in this opinion.

Mr. Redfield, following up the observations of Franklin, probably without knowing those of Colonel Capper, ascertained that whilst the north-east storms were blowing on the shore of America, the wind, with equal violence, was blowing a south-west storm in the Atlantic. Tracking Franklin's storms from the southward, he found throughout their course that the wind on opposite sides blew in opposite directions; and that, in fact, they were progressive whirlwinds, their manner of revolving being always in the same direction. By combining observations on the barometer with the progressive movement of storms, Mr. Redfield appears to have given the first satisfactory explanation of its rise and fall in stormy weather, and Colonel Reid's observations confirm his views.

The first step taken by the author, in furtherance of this inquiry, was to project maps on a large scale, in order to lay down Mr. Redfield's observations, and thus to be better able to form a judgment on the mode of action of the atmosphere.

These maps, which have now been engraved for publication in a separate work, were laid before the Association. The wind is marked on them by arrows. On the right-hand side of the circles the arrows will be observed to be flying from the south; on the left-hand, coming back from the north.

The field of inquiry which this opens can be but merely indicated here; to proceed in a satisfactory manner with the inquiry, the study being a new one, requires that the proofs be exhibited step by step.

The inferences drawn from the facts appear very important, and the further pursuit of the investigation well deserving attention.

The manner in which Colonel Reid has followed it up has been by procuring the actual log-books of ships, and combining their information with what could be obtained on land, so as to compare simultaneous observations over extended tracts. On Chart VII. were represented thirty-five ships in the same storm, the tracks of several crossing the storm's path, and the wind as reported by the ships corroborated by the reports from the land.

The observations of ships possess this great advantage for meteorological research, that merchant log-books report the weather every two hours, and ships of war have hourly observations always kept up.

After tracing a variety of storms in north latitudes, the author was struck with the apparent regularity with which they appear to pass to the North Pole; and was thence led to suppose, from analogy, that storms in south latitude would be found to revolve in a precisely contrary direction to that which they take in the northern hemisphere. Earnestly seeking for facts to ascertain if this were really the case, he had obtained much information to confirm the truth of the opinion before he was at all aware that Mr. Redfield had conjectured the same thing, without, however, having himself traced any storms in south latitude. Chart VIII. represents the course of a storm productive of very disastrous consequences, encountered by the East India fleet, under convoy, in 1809, and it is strikingly illustrative of this important fact.

If storms obey fixed laws, and we can ascertain what those laws are, the knowledge of them must be highly useful to navigation; but to apply the principles practically, requires that seamen should study and understand them. The problem so long desired to be solved, viz. on which side to lay to a ship in a storm, Colonel Reid trusts is now explained.

By watching the mode of veering of the wind, the portion of a storm into which a ship is falling may be ascertained. The object required is, that the wind, in veering, shall veer *aft* instead of *ahead*; and that a vessel shall come up instead of having to break off. To accomplish this the ship must be laid on opposite tacks, on opposite sides of a storm; but the limits of this notice render it impossible to attempt an explanation in detail.

The researches which have been carried into the southern hemisphere afford a very interesting explanation of the observations of Capt. King, in his sailing directions for the southern extremity of America, namely, that the rise and fall of the barometer in storms correspond with the rise and fall in high northern latitudes; east and west remaining the same, but north and south changing places.

Five connected storms which occurred in 1837, and followed each other in close succession, possess an interest altogether new, for they give us a clue to explain the variable winds. Since these whirlwinds revolve by an invariable law, and always in the same direction, every new storm changes the wind. Thus the hurricane of the middle of August 1837, traced on Chart VII., had hardly passed towards the

Azores, with the wind in the southern portion of it blowing violently at the west, when another storm, coming from the south, and bringing up the ship Castries with it, at the rate of seven or eight knots an hour, reversed the wind to east.

The storms expanding in size, and diminishing in force, as they proceed towards the poles, and the meridians at the same time approaching each other, gales become huddled together; and hence, apparently, the true cause of the very complicated nature of the winds in the latitude of our own country.

Since great storms in high latitudes often extend over a circular space of 1000 miles, the length and breadth of the British Islands afford far too limited a sphere for their study. Nations should unite to study the laws of atmospheric changes. By exchanging the observations made at the light-houses of different countries, reports would be obtained along the coasts of the whole civilized world. If the merchant log-books, instead of being destroyed, which is often the case at present, were preserved in depôts, each great commercial port keeping its own, they would greatly assist in giving information, by simultaneous observations on the sea and along the coast. The meteorological reports within the interior of different countries should, after the same manner, be exchanged, and we should then soon be enabled to trace the tracks of storms over almost the entire surface of the globe.

(The author then alluded to certain electro-magnetic phenomena, which offer close analogies to the phenomena of revolving storms.)

During his investigation of the law of storms Colonel Reid endeavoured also to ascertain the laws by which water-spouts revolve. After many fruitless researches, he obtained at length two satisfactory instances, one of which is from Captain Beechey. It is remarkable, that in these two instances, which occur in opposite hemispheres, the revolutions are in opposite directions, but both in the contrary direction to great storms. The double cones in water-spouts, one pointing upwards from the sea, the other downwards from the clouds, peculiarly mark these phenomena, and we ought to observe whether the cloud above, and the sea below, revolve in the same directions with each other. To ascertain their electrical state would be also highly interesting, and this perhaps may not be impracticable, for the great hydrographer and navigator Horsburgh actually put his ship through small phenomena of this description, in order to examine them.

Colonel Reid notices the apparent accordance of the force of storms with the law of magnetic intensity, as exhibited by Major Sabine's report to the Association. It is frequently remarked, with astonishment, that no storms occur at St. Helena; the degree of magnetic intensity there is nearly the lowest yet ascertained on the globe. Major Sabine's isodynamic lines to express less than unity are only marked there, and they appear, as it were, to mark the true Pacific Ocean of the world. The lines of greatest intensity, on the contrary, seem to correspond with the localities of typhoons and hurricanes, for we find the meridian of the American Magnetic Pole passing not far from the Caribbean Sea, and that of the Siberian pole through the China Sea.

The author then notices the performance of Mr. Whewell's and Mr. Osler's anemometers, and observes, "It is very desirable that these beautiful instruments should be placed beyond the limits of our own island, particularly in the West Indies and at the Cape of Good Hope, where they may measure the force of such a gale as no canvass can withstand; that which forces a ship to bare poles.

"It is not only to measure the wind's greatest force that it is desirable these anemometers should be multiplied and placed in different localities, but that we may try, through their means, to learn something more of the gusts and squalls which always occur during storms."

Note on the Effect of Deflected Currents of Air on the Quantity of Rain collected by a Rain-gauge. By Professor A. D. Bache, *of Philadelphia.*

The experiments referred to grew out of a report made at the request of the British Association, on the quantity of rain collected at different heights, which was presented at the Cambridge meeting of the Association by Professor Phillips and Mr. William Gray, jun. Professor Rogers was then present, and at his instance the author commenced a series of observations about the close of the year 1833. Philadelphia, from the extent of the plain on which it stands, is a good locality for such a purpose. The observations were at first made by gauges placed at three different heights. One of these stations was the top of a tower formerly used for making shot. The height of the tower is 162 feet. A second was near the ground within the inclosure about the tower, and the intermediate one was the roof of the university. The author's attention was ultimately fixed upon the fact that the effect of eddy winds upon the phenomena observed, was by no means a secondary one in amount, and that he could not hope to deduce a law, nor to throw any light on the nature of the phenomena, until this disturbing action was got rid of. He has therefore thought that it might be useful to those who may undertake similar experiments, to submit some of the evidence of the effects which he attributes to deflected currents of air. The observations on this point were chiefly made at the upper station, on the top of the tower. The tower is square in its section, and the alternate sides are nearly parallel and perpendicular to the meridian. At the roof the horizontal section is about twelve feet on a side, and a parapet wall, cut like a battlement, surrounds it. At first, one gauge was placed at the N.W. angle of the tower, rising about six inches above the parapet wall; subsequently, a gauge for collecting snow was placed at the S.W. angle; and ultimately, four gauges, besides the original one, were placed at the four corners of the tower, upon the parapet wall, above which they rose about ten inches. The rain gauges consisted of an inverted cone, with a cylindrical rim, about five inches in diameter, attached to the base, and a small aperture near the vertex; this fastened tightly upon a vessel serving as a reservoir. The snow gauges were frustums of upright cones, the upper section being nearly four inches in diameter. The water was measured in a glass tube, in which one-thousandth of an inch of rain fallen was measurable. When the snow gauges became useless, they were used as rain gauges, by attaching a funnel to them, or were finally replaced by rain gauges similar to those described. The quantity of water collected was measured after each rain, and the direction of the wind during the rain was frequently noted. To illustrate the effects which are attributed to currents of air deflected by the tower, Professor Bache has taken from the journal of the latter months of observation the records of the quantities of rain collected by four similar gauges, placed at the four angles of the tower, under different circumstances as to the direction of the wind. These are selected so as to present, as far as possible, a case of rain with each principal direction of the wind.

Date.	Wind.	Angle of the Tower at which the Gauge was placed.				Relative Quantities at different Angles.			
		N.E.	S.E.	S.W.	N.W.	N.E.	S.E.	S.W.	N.W.
		Rain in Inches.							
July 26	N.	0·552	0·760	0·749	0·583	1·00	1·37	1·35	1·05
Aug. 6	N.E.	0·311	0·378	0·607	0·491	1·00	1·21	2·08	1·58
July 15	E. & N. by E.	0·912	1·398	1·868	1·715	1·00	1·53	2·04	1·88
April 13	N.E., S.E., S.W.	1·316	1·186	1·568	1·670	1·10	1·00	1·31	1·40
Aug. 26	S. & S.S.E.	0·407	0·253	0·241	0·391	1·68	1 04	1·00	1·62
June 19	W.S.W. & S.S.W.	0·389	0·285	0·252	0·198	1·96	1·43	1·26	1·00
Sept. 1	W.	0·302	0·328	0·202	0·141	2·14	2·32	1·43	1·00
Sept. 5	W.N.W., N.	0·638	0·731	0·429	0 679	1·48	1·70	1·00	1·58

On this table the author remarks,—1. That it illustrates the very great differences between the quantities of rain collected at the different angles of the tower. In one extreme case the quantity collected at the S.E. angle was $2\frac{1}{3}$ times that at the N.W. angle. 2. That, in general, the gauges to leeward received more rain than those to windward. Thus, with a north wind, the gauges at the S.E. and S.W. angles received more rain than those at the N.E. and N.W. angles. With a N.E. wind the gauge at the S.W. corner of the tower received the most rain. In the case given in the table, the ratio of the quantities is nearly 2·1 to 1. With an easterly wind the N.E. and S.E. gauges received less than the N.W. and S.W. With a south-easterly wind the S.E. gauge received the least, and the N.W. the greatest quantity of rain, and so on, nearly in the order stated in the general remark. 3. As the more considerable rains accompany certain winds, it is not to be expected that averages of any number of observations exposed to such errors will lead to an accurate result of the quantity of rain falling at a certain height above the surface. In fact, the averages from a period of nine months do not agree nearly so well as those from the selected specimens in the table. These give ratios of 1, 1·19, 1·24, and 1·20, for the quantities at the different angles; while the former-mentioned averages at the N.E. and S.W. angles are nearly as one to one and a half.

4. The connexion between the direction of the wind and these effects is easily made out; but without an anemometer this is not possible for that of the force. "I have found, however," observes the author, "in the case of the N.E. wind, which most frequently attends our greatest rains, considerable differences, even with a moderate wind amounting, for example, as high as a ratio of one and a half to one. Having seen that I could not hope for accurate results by these arrangements, I next tried the effect of elevating the gauge upon a high pole, as was done by Professor Phillips and Mr. Gray with the gauge on the top of York Minster. The differences that appeared in this case were very trifling indeed: thus, on the 26th of August, when the N.E. and S.W. gauges upon the parapet wall gave quantities in the ratio of 1 to 1·68, those six feet above the parapet gave 1 to 1·08; with a more moderate wind the quanties were more nearly the same."

The author proposes to resume this inquiry with reference to the general question on his return to America. (See Reports of the Association, Vols. II. III. IV. for the researches conducted during three years at York.)

On the Variations in the Quantity of Rain which falls in different Parts of the Earth. By WILLIAM SMITH, *LL.D.*

Effects so very local, as shown by rain-gauges, at short distances apart in our own island, must arise, Dr. Smith imagines, from local causes. The general remark, that much less rain falls on the eastern than on the western side of England, stands confirmed by the tables; and as at Edinburgh there fall only 22 inches, and in Dublin 22·2, it seems likely to hold in other parts. The local variations in the quantities of rain in England are, however, very great; and in a short table of sixteen local averages, from 67 inches at Keswick, down to 22·7 at South Lambeth, the half of them on the western side of England are by far the highest; but the comparatively small quantities of rain at Bristol and Chatsworth not according with this generalization, and one place on the eastern coast being even higher than these, and higher than Liverpool, this generalization, Dr. Smith conceived, required to be modified. More local causes seemed requisite to account for the observed facts; and Dr. Smith imagined that they are to be found in the nature of the surrounding country, that is, in the physical differences of the vicinity of each place, and not altogether in the track of the most rainy winds.

In confirmation of this opinion, meteorological registers were quoted to show that, although westerly and southerly winds blow at London 101 days more than the drier easterly or northerly winds, yet London is the least rainy place in Britain, except Edinburgh, which averages seven tenths lower than South Lambeth. The cause of much rain depends not, therefore, he conceived, wholly on the prevalence of westerly winds; but, in part at least, on the humidity of the neighbouring regions. The westerly winds, before they reach London, pass over a great extent of high and dry land; and, consequently, there is, at the level of the chalk hills (600 to 800 feet high), a dry atmosphere over London. Bristol, having 5·2 inches less rain than Liverpool, and nearly seven inches less than Manchester, is surrounded by high and dry hills of limestone; and Chatsworth, having 4 inches less than Bristol, is also, except on the eastern side, surrounded by very high and dry hills. Dr. Smith illustrated this part of his argument by reference to other places. Manchester, having 36·1 inches, is not far west of the high and damp hills of millstone grit, which here form the summit ridge of England; but about Lancaster, the width of wet-topped hills increases, so that vapour from these, the southern swampy shore, and of the tides and sands of Morecambe bay, may account for 39·7 at Lancaster. Townly, high, and in the vicinity of bog-topped hills, has 41·5; Grisdale, Westmorland, 52·3; and Kendal 53·9; the latter, perhaps, partly from the hills, and partly from Lancaster sands and adjacent marshes; but Keswick averages 67 inches. This, perhaps, is the greatest quantity of rain which falls at any one place in England, and is perhaps to be accounted for by the peculiar situation of Keswick, at the meeting of four valleys, which intersect a group of very high mountains, and near to swampy ground, and large pieces of water, on which the winds have great influence in raising vapour, which the cold sides of the mountains rapidly condense. These hill tops, as well as those of millstone grit, have a covering of peat, which holds water like a sponge.

Notice of a Brine Spring emitting Carbonic Acid Gas. By Professor Forbes, *F.R.S.* (*In a Letter to Prof. Phillips.*)

The letter of Prof. Forbes noticed a remarkable spring, about a mile from Kissingen, Bavaria, which had occupied much of his attention, and of which he will probably at a future time draw up a more detailed account*. It is a *brine spring*, having 3 per cent. of salt, rising in a bore, 325 Bavarian feet deep, in red sandstone; but the author understands that the water flows at about 200 feet in depth. Its temperature is never less than 65°; the mean temperature of springs near being only 50° to 52°. It discharges carbonic acid gas in volumes almost unexampled, keeping the water, in a shaft eight feet diameter, in a state resembling turbulent ebullition. The enormous supply of gas has led to its use in gas baths, for which purpose it is carried off by a tube connected with a huge inverted funnel, which rests upon the water. *It contains scarcely a trace of nitrogen.* It is conducted into chambers properly prepared, and thence into baths, in which it lies by its weight, and is used as water would be. But the most remarkable feature still remains to be noticed. About five or six times a day the discharge of gas suddenly stops; in a few seconds the surface of the well is calm. The flow of water, amount-

* This account has been published in the Edinburgh Philosophical Journal, 1839.

ing to *forty cubic feet* per minute, also stops, or rather becomes *negative*, for the water recedes in the shaft even when the pumps commonly used to extract the brine do not work, and the water subsides during fifteen or twenty minutes. It then flows again, the water appearing first and suddenly, the gas gradually increasing in quantity, till, after three quarters of an hour, the shaft is full as at first. The state of greatest discharge continues with little variation for three or four hours, but by no means with absolute regularity. It is also affected by various circumstances, apparently extraneous; this has gone on with little variation since the bore was made in 1822. Within a short distance is a bore 554 Bavarian feet deep, which exhibits somewhat similar phenomena. Altogether, Prof. Forbes considers that the salt spring at Kissingen is the most singular phenomenon of its kind in Europe except the Geysers.

On the Climate of North America. By Dr. DAUBENY, *Professor of Chemistry and Botany, Oxford.*

The principal object of this communication was to invite the attention of meteorologists to the present state of our knowledge with respect to the climate of the North American continent. With this view the professor laid before the Section the following general table, which comprehends all the observations on this subject that he had been able to collect during his late visit to the United States and Canada.

The best observations made in Canada are those of Mr. M'Cord, of Montreal, who has procured from England excellent instruments, and has spared no pains in arriving at accurate results.

From his statement, it would seem as if there had been a sensible deterioration in the climate of that part of Canada since 1830, for the mean of that year was .... 47·8

of 1831	46·8
1832	44·7
1833	44·8
1834	45·0
1835	42·9
1836	40·43
1837	41·22

And a tendency in the same direction may perhaps be detected in the observations recorded at Fort Diamond, above Quebec.

	Temp.	
For in 1830	41·00	Fahr.
„ 1831	39·00	„
„ 1832	35·50	„
„ 1833	36·91	„
„ 1834	36·87	„
„ 1835	33·41	„
„ 1836	35·82	„

It would be interesting to find out, whether this reduction of temperature indicated a permanent change in the climate of Canada, or whether the years noticed constitute the coldest portion of a cycle of longer duration, and consquently give a result below the actual mean. Dr. Daubeny remarks, that the position selected for meteorological observations at Quebec is so elevated and exposed that it does not fairly represent the mean temperature of the neighbourhood. In the United States the best observations made are those carried on at the several academies in the state of New York, under the direction of the state government. The author has quoted a sufficient number of these to convey a notion of the climate of that portion of the Union, and the "Annual Report of the Regents of the University of the State of New York" for the remainder. The mean temperature of Philadelphia cannot yet be regarded as settled, though good observations have been carried on for the last three years by Captain Mordecai. These have been quoted in preference to others of longer date reported by Mr. J. Young, stated by him to have been deduced from twenty years observations, as the mean obtained by the latter (58·4) is so much above that of places lying to the south (Washington and Richmond for instance), that we are driven to suppose that the spot selected must have been an unsuitable one.

Table of the comparative Temperature of various places in the continent of North America, from N. Lat. 46° to 24°, compiled from various sources.

Parallel.	Position with reference to the Alleghany mountains.	Locality and Geographical situation.	Latitude.	Longitude.	Elevation in feet above the sea.	Mean Temperature of the year.	As deduced from observations continued.	Authority for the annexed statement.
46th	E.	Cape Diamond, Quebec, Lower Canada	° ′ 46·50	° ′ 71·10	330	° ′ 37·66	8 years, viz. 1829 to 1836	Mr. Watt.
	W.	Fort Brady, near the Falls of St. Mary, Michigan State	46·39	84· 5	595	41·37	Not stated	Lovell's Register, quoted by Darby (View of the Unit. States).
45th	E.	Montreal, Lower Canada	45·30	73·35	.....	44·20	8 years, viz. 1830 to 1837	Mr. M'Cord.
44th	W.	Fort Howard, S. extremity of Green Bay, Michigan	44·40	87· 2	600	44·50	Not stated	Lovell's Register, quoted by Darby.
	W.	Fort Snelling, near the junction of the St. Peter's and Mississippi rivers	44·53	93·15	780	45· 0	Not stated	Ditto.
	E.	Fort Sullivan, Eastport, Maine	44·44	67·11	......	42·44	Not stated	Ditto.
43rd	E.	Dartmouth Coll. Hanover, New Hampshire	43·45	72·22	......	40· 6	2 years, viz. 1835 & 1836	Vermont Chronicle.
	E.	Dover, New Hampshire	43·20	69·12	......	42· 8	1 year, viz. 1836	A. Tufts.
	E.	Concord, New Hampshire	43·20	70·22	......	42· 4	1 year, viz. 1836	J. Farmer.
	E.	Portsmouth, New Hampshire	43· 5	70·46	......	45· 8	Not stated	Mellish's description.
	W.	Rochester, New York State	43·08	77·10	506	47·26	5 years, viz. 1830, 33, 34, 35, 36	Regents of the University of New York.
42nd	W.	Lewiston near Buffalo, New York State	42·50	79·20	......	48·35	6 years, viz. 1831 to 1836	Ditto.
	W.	Albany, New York State	42·39	73·20	......	48·56	11 years, viz. 1826 to 1836	Ditto.
	E.	Cambridge, near Boston, State of Massachusetts	42·25	71· 0	0	52·36	2 years	Humboldt on Isothermal lines.
	W.	Detroit, State of Michigan	42·30	82·50	......	47· 4	1 year, viz. 1818	Mellish's description.
41st	E.	Newport Harbour, Rhode Island	41·30	71·25	0	51·02	Not stated	Lovell.
	W.	Council Bluffs', above the mouths of Platte River, Missouri	41·25	95·50	800	50·82	Not stated	Ditto.

Parallel.	Position with reference to the Alleghany mountains.	Locality and Geographical situation.	Latitude.	Longitude.	Elevation in feet above the sea.	Mean Temperature of the year.	As deduced from observations continued.	Authority for the annexed statement.
40th	E.	New York Harbour...	° ′ 40·42	° ′ 73·20	0	52·82	Not stated	Ditto.
	W.	Fort Columbus, near Pittsburgh, Pensylvania	40·82	80·20	......	54· 2	1 year, 1820	Mellish.
	E.	Germantown, near Philadelphia	40·03	75· 0	......	52·37	10 years, viz. 1819 to 1828	R. Haines.
39th	E.	Philadelphia...... ...	39·56	75·10	0	52·52	Not stated	Humboldt, fr. comparing the observations of Rush and Legare.
		Ditto	......	......	......	50·67	1836-7-8	Captain Mordecai, journal of the Franklin Institute.
	E.	Baltimore..............	39·17	77· 0	0	53· 0	8 years, viz. 1817 to 1824	L. Bruntz.
	W.	Cincinnati...	39·07	84·50	500	54· 0	8 years, viz. 1806 to 1813	Drake, View of Cincinnati.
38th	E.	Washington City......	38·50	77· 2	0	57·09	8 years, viz. 1821 to 1827	Rev. R. Little.
	W.	St. Louis, State of Missouri	38·36	90·40	550	55· 2	7 years, viz. 1829 to 1836	Dr. Drake.
	W.	N. Harmony, Indiana	38·11	86·50	340	56·69	3 years, viz. 1826-7-8	Dr. Tront.
37th	E.	Richmond, Virginia...	37·04	77·50	......	56·81	14 years, viz. 1824 to 1837	Chevallier.
34th	E.	Smithville, mouth of Cape Fear River, North Carolina	34· 0	78·	0	58·88	Not stated	D. Lovell.
32nd	E.	Charleston, South Carolina	32·44	80·20	0	58·80	18 years, viz. 1750 to 1759	Drayton, View of South Carolina.
		Ditto...	......	......	......	60·18	Not stated	Dr. Lovell.
31st	W.	Natchez, State of Mississippi	31·28	91·45	180	64·76	4 years	Dunbar.
	W.	Jessup Cantonment, near Sabine River, Louisiana	31·30	94· 0	150	68·31	Not stated	Dr. Lovell.
30th	W.	Baton Rouge, Louisiana	30·26	91·40	......	68·07	Not stated	Ditto.
	W.	Pensacola, W. Florida	30·24	87·40	......	68·77	Not stated	Ditto.
29th	W.	New Orleans, Louisiana	29·57	90· 8	......	66· 0	1 year, viz. 1836	Professor Barton.
	E.	St. Augustine, E. coast of Florida	29·50	81·50	0	72·23	Not stated	Dr. Lovell.
24th	E.	Key, West Florida...	24·33	82·20	0	76· 6	6 years, viz. 1830 to 1835	Whitehead, collector of customs.

On the Helm Wind of Crossfell. By the Rev. J. WATSON.

Helm Wind is a local name of uncertain origin, but generally supposed to be derived from the cloud that, like a cap or helmet, is often seen on the tops of the mountains. It is specially applied to a very violent wind, blowing frequently from some easterly point of the compass, but mostly due east, at the west side of the mountains known by the name of the Crossfell range, and confined both in length and breadth to the space contained between the Helm and Helm Bar, hereafter described. Along the top ridge of the mountains, and extending from three or four to sixteen or eighteen miles each way, north and south, from the highest point, is often seen a large long roll of clouds; the *western* front clearly defined and quite separated from any other cloud on that side; it is at times above the mountain, sometimes rests on its top, but most frequently descends a considerable way down its side; this is called the Helm. In opposition to this, and at a variable distance towards the west, is another cloud with its *eastern* edge as clearly defined as the Helm, and at the same height: this is called the *Bar* or *Burr*; the space between the Helm and the Bar is the limit of the wind. The distance between the Helm and Bar varies as the Bar advances or recedes from the Helm; this is sometimes not more than half a mile, sometimes three or four miles, and occasionally the Bar seems to coincide with the horizon, or it disperses and there is *no* Bar, and then there is a general east wind extending over all the country westward. However violent the wind be between the Helm and the Bar, it extends no farther; on the west side of the Bar there is either no wind or it blows in a contrary direction, that is, from the west, or from various points in sudden and strong gusts, when the Bar advances so far as to unite with the Helm; if the Bar disperses, the wind ceases. Neither the Helm nor Bar are separate or detached clouds, but may be rather said to be the bold, clearly defined fronts of bodies of clouds extending eastward behind the Helm, and westward from the Bar. The clouds forming the Helm and Bar cannot perhaps strictly be said to be parallel; the open space between them may rather be called a very flat ellipse, in which the transverse diameter varies from eight or ten to twenty-five or thirty miles, and the conjugate from half a mile to four or five miles: they appear always united at the ends.

This wind is very irregular, but most frequent from the end of September to the month of May; it seldom occurs in the summer months; there was one this year, 1838, on the 2nd of July, and there have been more in the last two years than in the preceding six. Sometimes, when the atmosphere is quite settled, not a breath of wind stirring, and hardly a cloud to be seen, a small but well-known cloud appears on the summit, extends itself to the north and south—the "Helm is on," and in a few minutes blowing furiously, sufficient to break trees, overthrow stacks of grain, throw a person from his horse, or overturn a horse and cart. The Helm at times seems violently agitated, and on ascending the fell and entering it there is little wind, and this sometimes not in the direction of the wind below: one may, in fact, be in the Helm for a whole day without being aware of the wind on the west. The Helm appears sometimes to run or pour off from the highest part, each way towards the north and south points of the junction of the Helm and Bar, and there to be piled up in great masses; occasionally a Helm forms and goes off without a blast. The open space between the Helm and Bar is clear of clouds, with the exception of small pieces breaking off now and then from the Helm and driving rapidly over to the Bar; through this open space is often seen a higher stratum of clouds quite at rest.

Most mountainous countries, particularly where the mountains terminate abruptly, seem liable to sudden gusts of wind, such as occur at the Cape of Good Hope, in Switzerland, and among the lakes in our own country; but the Helm wind differs from all in respect to the Bar, and that within the space described it blows *continually;* it has been known to blow for nine days together, the Bar advancing or receding, or continuing stationary for a day. When heard and felt for the first time it does not seem so very extraordinary; but when we find it blowing and roaring morning, noon, and night, for days together, it makes a strong impression on the mind, and we are compelled to acknowledge that it is one of the most singular phenomena of meteorology. Its sound is peculiar, and when once known is easily distinguished from that of ordinary winds; it cannot be heard more than three or four miles beyond its limit, but by persons who have stood within the wind or near it, it has been compared to the noise made by the sea in a violent storm, or that of a large cotton mill when all the machinery is going. It is seldom accompanied by rain within the open space, and never continues long after it begins to rain heavily; in spring, it is most frequent after rain. The country subject to it is very healthy, but the wind does great injury to vegetation, as it batters the grain, grass, and the leaves of trees till they are quite black. Various hypotheses have been suggested to account for this phenomenon; one of the most plausible assumes that the air is cooled by its gradual ascent from the east coast, and on reaching the summit of the mountains, rushes with great force down the western escarpment into a lower and warmer region. In opposition to this it is stated, that the valley of the Tyne, where the Helm wind is not felt, is not much higher than that of the Eden; and secondly, the wind does not extend farther west than where the Bar is vertical, and this is not very often so far as the Eden. The cause, Mr. Watson thinks, must be sought for in that region of the atmosphere, extending from 800 to about 5000 feet above the earth's surface.

On the Temperatures observed in certain Mines in Cheshire. By EATON HODGKINSON, *Esq.*

(The results will be given hereafter in combination with the account of other experiments.)

Facts relating to the Effects of Temperature on the Regulators of Timekeepers; and description of some recent improvements in Pendulums, with Observations, and Tabulated Experiments. By Edward John Dent, *F.R.A.S.*

The subjects contained in Mr. Dent's paper may be arranged under three heads.

1st. The continuation of an inquiry into the compound effect of variable temperature upon the regulating machinery of Timekeepers, the commencement of which had been laid before the Association some years before.

2nd. A description of improvements in mercurial pendulums, principally with a view of making them portable.

3rd. Improvements in the suspension of pendulums in general; and incidental remarks connected with the subject.

In all estimates of the effect of variable temperature upon the regulating part of timekeepers, made for the purpose of ascertaining the necessary amount of compensation, it had generally been assumed that the effect was confined to an alteration of length in a part of the regulator. Mr. Dent, having long felt that this view of the subject was insufficient to account for facts which his daily practice brought before him, at length commenced a series of experiments with a view to a more complete and consistent explanation of them; and, in 1833, at the meeting of the Association in Cambridge, he endeavoured to show that the effect of variable temperature upon the balance-springs of chronometers might be resolved into two distinct portions: *viz.* the one, long known, which produces the variation of length; and *another, which had hitherto escaped attention, affecting the elasticity of the spring.*

Mr. Dent afterwards extended the inquiry to the pendulums of clocks, and succeeded in separating and determining the respective amounts of effect which changes of temperature produce upon the elasticity of the spring and the length of the rod. The details of this subsequent inquiry, with descriptions of the apparatus invented and used for the purpose, were described at length. As an illustration, the following experiment is taken from amongst many others:—

A clock provided with a spring-pendulum, and adjusted to keep correct time at an ordinary temperature, was found, when the temperature was maintained at 48° Fahr. higher, to lose twelve seconds in twenty-four hours.

Mr. Dent attempts to demonstrate by experiment that eight parts and a half only of this difference belong to the effect of elongation in the rod, and the remaining one and a half are produced by a decrease of elasticity in the spring.

From the epoch of the introduction of the mercurial cistern to the present time, this admirable modification of the pendulum has remained nearly in the state in which it was left by its inventor.

The mercurial pendulum cannot even now be filled and transported in a state proper to be immediately attached to a timekeeper; consequently in the case of the transport of clocks with this description of pendulum to great distances, either this part of the machine (*which regulates the whole*) must be placed in inexperienced hands, to be filled with mercury and to have the column adjusted to the exact length required for compensation, or a workman must accompany the clock and set it in action. The expense of the latter process constitutes, generally, a very serious objection.

The pendulum cistern being made of glass, is liable to fracture, and, on account of the risk which attends the boiling the mercury within it, to drive off the air, this process is never attempted. It is very possible to give to a glass vessel, externally, a form mathematically correct, but the case is very different when similar accuracy is required for the interior. The glass cistern, therefore, never receives a perfect figure, and the mercurial column it contains cannot be a regular cylinder. This condition, combined with the irregularity of expansion which glass is peculiarly liable to from its compound nature, renders measurement and calculation with regard to the column, so vague and deceptive that they are never employed.

In order to meet these serious obstacles to the satisfactory and extensive use of this valuable instrument Mr. Dent has recommended the substitution of *cast-iron* for *glass* in the cistern.

Mercury and cast-iron are quite as little disposed to amalgamate as mercury and glass; and iron is a material, compared with glass, which is more simple in its nature, and more obedient to the workman. It is susceptible of the most perfect forms, which it will maintain with very little liability to alteration, and is quite proof against numerous accidents that would be fatal to glass. The expansion of iron by heat being also uniform and well known, it is evident that, in the cast-iron cistern, we may have a vessel of a known, regular, and permanent figure, or, if not strictly permanent, one whose changes and their laws we are acquainted with. Calculation may, therefore, be used in anticipating results, without any fear of its widely differing from experiment.

Further,—in a cistern of cast-iron, the mercury may be *boiled at any time*. The clockmaker may do it himself when he first puts the machine together,—he may adjust the column,—he may then hermetically seal it, and despatch the pendulum to the most distant countries with the adjustment so perfect that it may be instantly attached to the wheel-work by any workman capable of setting the clock upon its supports. If, at a subsequent period, minute portions of air have, from any cause, again mingled with the mercury, and rendered the pendulum susceptible of barometric changes, the air may be again driven off with the greatest facility, by repeating the process of boiling, without removing the mercury from the cistern.

Mr. Dent has accompanied the introduction of cast-iron in the cistern with several other alterations which have all the same intention of improving this kind of pendulum. Among others, he has removed entirely the metal stirrup, or frame, which carried the cistern, and has attached the latter to the rod;—he has prolonged the rod, and plunged it into the mercury, nearly to the bottom of the cistern;—a condition evidently favourable to uniformity of temperature in the rod and mercury, &c. &c.

All strain or warp of the spring, in the final suspension of the pendulum, can be avoided. The pendulum being first suspended freely, is left until, by the cessation of its action, it arrives at its own line of rest in every direction, particularly in that which passes through the plane of the spring. The fixing-piece is then brought to it and the whole permanently attached together.

The line of the flexure of the spring can be determined and preserved. Usually, the exact position of this line is, within certain limits, left to accident, and is, from several causes, continually changing its position; consequently, the pendulum is simultaneously varying in length. Errors in rate, often attributed to other causes, are the necessary consequence.

Notice of a cheap and portable Barometrical Instrument proposed for the use of Travellers in Mountainous Districts. By Sir John Robison, *Sec. R.S.E., &c.*

The instrument is a glass tube about 0·25 of an inch in diameter, and about 14 inches long, with a small bulb like that of a thermometer blown on the upper end. The stem of the tube has been graduated by divisions made experimentally by the instrument-maker in the following manner. On a day when the barometer stood at 30 inches, and the temperature of the air was 62°, it was placed in the receiver of an air-pump, and when the rarefaction allowed the barometer of the pump to fall to 29 inches, the instrument was lowered until the open end of the tube became immersed in a cup of water, over which it had been suspended by a thread and wire passing through a stuffing-box. On allowing the atmosphere to enter the receiver, the water was pressed up the tube until the density of the air corresponding to 30 inches was restored, and the height of the fluid was carefully marked. The instrument was a second time suspended in the air-pump receiver, and the exhaustion was repeated until the barometer gauge indicated 28 inches, the immersion in the cup having been made as formerly; the air rushed in, and the graduation of the tube corresponding to 28 inches was accomplished. By continuing this process, the graduation of the stem was carried on as far as was thought requisite, (the intermediate divisions having been made in a similar manner,) when the instrument became ready for use.

It is obvious, that in this manner a traveller arriving at a station in the midst of mountains, and having with him a number of such tubes, would only require to send messengers to their summits with one or more of these tubes and a tin case containing water, for the purpose of giving him the means of determining their heights with considerable accuracy. Each messenger, carrying with him the empty glass tubes, is to be instructed to insert their open ends in his flask of water when he shall have reached the summit, and to bring them down again.

Having done so, the air in the bulb and tube having become rarified to the tension of that on the top of the mountain, is compressed by the water which the increased pressure of the atmosphere as he descends forces into the tube, so that when he returns to the place where the barometer is at 30 inches, the height of the fluid will indicate the height at which the barometer would have stood on the summit of the elevation. If the barometer be not exactly at that height, a correction may be applied.

If the temperature and degree of moisture of the air in the tube on the mountain and at the lower station were alike, no further correction would be requisite: but just as in the case of the barometer so with this instrument; for minute accuracy, a thermometer and hygrometer should accompany it, and be simultaneously observed, so as to permit the application of the usual corrections.

In many cases precisely equal temperatures may be obtained at the upper and lower stations by keeping the tin case supplied with water, melting snow, or ice.

In a general and rapid survey of a country, such instruments would possess value from their portability and cheapness.

Tables intended to facilitate the computation of Heights by the Barometer. By the Rev. Temple Chevallier, *B.D., Professor of Mathematics in the University of Durham.*

In these tables, the correction for the temperature for the air is computed, so that the difference of elevation of two stations, in feet, is at once found by taking the difference of two numbers corresponding with the heights of the column of mercury at the two stations, and the mean temperatures of the air. The table is constructed for differences of one tenth of an inch in the barometer, the proportionate variation for hundredths and thousandths of an inch being readily found by an accompanying table of proportional parts.

A table is given for the correction for the difference of temperature of the mercury.

Mr. J. S. Russell described a magnetic instrument invented by Mr. Watt, of Lasswade, which, according to the experience of the inventor, appeared to take positions corresponding to the direction of the wind.

CHEMISTRY.

Extracts from a Letter addressed by Dr. HARE, *of Philadelphia, to the Chemical Section of the British Association for the Advancement of Science.*

" Since July last, when I had the honour of addressing you through your venerable and distinguished president Dr. Dalton, I have made some additional observations and attained some farther results, of which a brief notice may, I trust, be deemed worthy of attention.

" I have, by improvements in my process for fusing platina, succeeded in reducing twenty-five ounces of that metal to a state so liquid, that the containing cavity not being sufficiently capacious, about two ounces overflowed it, leaving a mass of twenty three ounces. I repeat, that I see no difficulty in extending the power of my apparatus to the fusion of much larger masses.

" When nitric acid (or sulphuric acid and a nitrate) is employed to generate ether by reaction with alcohol, there must be an excess of two atoms of oxygen for each atom of the hyponitrous acid which enters into combination. This excess involves not only the consumption of a large proportion of alcohol, but also gives rise to several acids and to some volatile and acrid liquids.

" It occurred to me, that for the production of pure hyponitrous ether, a hyponitrite should be used. The result has fully realized my expectations.

" By subjecting hyponitrite of potash or soda to alcohol and diluted sulphuric acid, I have obtained a species of ether which differs from that usually known as nitrous, or nitric ether, in being sweeter to the taste, more bland to the smell, and in being more volatile. It boils below 65° F., and produces by its spontaneous evaporation a temperature of 15°. On contact with the finger or tongue, it hisses as water does with red-hot iron. After being made to boil, if allowed to stand for some time at a temperature below its boiling point, ebullition may be renewed in it apparently at a temperature lower than that at which it had ceased. Possibly this apparent ebullition arises from the partial resolution of the liquid into an aëriform etherial fluid, which escapes both during the distillation of the liquid ether and after it has ceased, even at a temperature below freezing. This aëriform product has been found partially condensible by pressure into a yellow liquid, which when allowed to escape into the mouth or nose, produced an impression like that of the liquid ether. I conjecture that it consists of nitric oxide, so directed to a portion of the liquid ether as to prevent the wonted reaction of this gas with atmospheric oxygen. Hence it does not produce red fumes on being mingled with air.

" Towards the close of the ordinary process for the evolution of sweet spirits of nitre, a volatile acrid liquid is created, which affects the eyes and nose like mustard or horse-radish. It is probable, however, that this is in part due to the presence of chloride of sodium, as I have reason to suspect the acrid liquid to be chlorocyanic ether.

"Quick lime, when the new ether, as it first comes over, is distilled from it, becomes imbued with an essential oil, which it yields to hydric ether. This oil may be afterwards isolated by the spontaneous evaporation of its solvent. It has a mixed odour, partly agreeable, partly unpleasant. From the affinity between its odour and that of common nitrous ether, I suspect that it is one of the impurities which exist in that compound.

"The new ether is obtained in the highest degree of purity, though in quantity less, by introducing the materials refrigerated by snow and salt into a strong, well-ground, stoppered bottle. After some time the ether will form a supernatant stratum, which may be separated by decantation. Any acid having a stronger affinity for the alkaline base than the hyponitrous acid, will of course answer to generate this ether. Acetic acid not only extricates, but appears to combine with it, forming apparently a hyponitro-acetic ether.

"I observed some years ago, that when olefiant gas is inflamed with an inadequate supply of oxygen, carbon is deposited, and the resulting gas occupies double the space of the mixture before explosion. Of this I conceive I have discovered the explanation. By a great number of experiments performed with the aid of my barometer-gauge, eudiometer, and other instruments, I have ascertained, that if, during the explosion of the gaseous elements of water, any gaseous or volatile inflammatory matter be present, instead of condensing there will be a permanent gas, formed by the union of the nascent water with the inflammable matter. Thus, two volumes of oxygen with four of hydrogen and one of olefiant gas, give six volumes of permanent gas, which burns like light carburetted hydrogen. The same quantity of the pure hydrogen and oxygen with half a volume of hydric ether, give on the average the same residue. One volume of the new ether, under like circumstances, produced five volumes of gas.

"An analogous product is obtained when the same aqueous elements are inflamed in the presence of an essential oil. With oil of turpentine, a gas was obtained weighing per hundred cubic inches $16\frac{5}{10}$ grains, which is nearly half the gravity of light carburetted hydrogen. The gas obtained from olefiant gas, or from ether, on the average weighed, for the same bulk, $12\frac{5}{10}$ grains: this leaves no doubt of its being chiefly constituted of the water, as the olefiant gas which I used weighed per hundred cubic inches only $30\frac{5}{10}$ grains; if *per se* expanded into six volumes, it could have weighed only one sixth of that weight, or little over five grains per hundred cubic inches.

"With a volume of the new ether, six volumes of the mixture of hydrogen and oxygen gave on the average about five residual volumes.

"The gases thus created do not contain carbonic acid, and, when generated from olefiant gas, appear to yield the same quantity of carbon and hydrogen as the gas affords before expansion.

"These facts point out a source of error in experiments for analyzing gaseous mixtures by ignition with oxygen or hydrogen, in which the consequent condensation is appealed to as a basis for an estimate. It appears that the resulting water may form gaseous products with any volatile matter which may be present. It is in this way, as I conceive, that olefiant gas, when inflamed as above mentioned with a quantity of oxygen inadequate to saturate it, is expanded into a residual gas larger than that of the mixture before ignition.

"I remain, gentlemen, with high esteem, your co-labourer,

"Robert Hare."

Some Observations on the Foreign Substances in Iron. By Thomas Thomson, *M.D. F.R.S., &c., Prof. of Chemistry, Glasgow.*

The great difference which exists between different specimens of iron is generally known. The best Swedish iron when compared with British iron, even of the best quality, in point of strength is as 4 to 3. A Swedish wire of a diameter of about $\frac{1}{13}$th of an inch supports a weight of 450 lbs. without breaking, while the utmost weight that a wire of British iron of the same diameter can bear is 350 lbs. Iron from the mine of Dannemora in Sweden makes excellent steel; while British iron is so ill adapted for the purpose that it is hardly ever converted into steel, and never into good steel. Dr. Thomson thought it likely that their differences were owing to something in the British iron which injured its quality and which was wanting in Swedish iron. The results of some analyses made by Dr. Thomson do not entirely clear up the question; but they present some important information on the peculiarities of iron. The following is the statement of the experiments.

"I selected, as best suited to the object which I had in view, the best Dannemora iron, which is all used for conversion into steel, common Welsh iron, which is hardly capable of being converted into steel, and Lowmoor iron from Yorkshire*. Mr. Buthray, a very intelligent steel-maker and iron-smelter in the neighbourhood of Glasgow, was kind enough to supply the specimens, so as to ensure their coming from the places stated.

"The first remarkable difference in these three specimens is their specific gravity. It was as follows:

Best Dannemora iron	7·9125
Lowmoor iron	7·3519
Welsh iron	7·4059

These differences are much greater than I expected to find: perhaps it will be more intelligible if I state them as follows:

"If the specific gravity of Dannemora iron be reckoned	1000
the Lowmoor iron will be	929
the Welsh iron	939·7

* Dr. Thomson was informed that this iron from Lowmoor was smelted with charcoal.

or Lowmoor iron is about 7 per cent. and Welsh iron 6 per cent. lighter than the best Dannemora iron.

"To analyze Dannemora iron I dissolved 100 grains of it in muriatic acid, evaporated the solution to dryness in a gentle heat, and redissolved the residue in water slightly acidulated with muriatic acid. There remained undissolved a gray-coloured matter, which was thoroughly washed, and dried at a temperature of 300°. It weighed 0·32 gr. or very nearly one-third of a grain. Being ignited in a platinum crucible, the weight was reduced to 0·06 gr. of a gray matter, which, examined before the blow-pipe, proved to be silica very slightly tinged with iron. The 0·26 gr. lost by ignition was probably carbon; for a temperature of 300° was doubtless sufficient to drive off all the water which might at first have been present.

"The muriatic acid solution was mixed with nitric acid and boiled for several hours in a flask, to peroxidize the iron. When cold, the excess of acid was neutralized as exactly as possible by carbonate of soda, taking care that no precipitate fell. It was then raised to the boiling point and thrown upon a filter. The whole peroxide of iron which it contains is retained upon the filter, and must be well washed with hot water. At first the water passes through the filter quite colourless; but when most of the common salt is washed out the oxide of iron begins to pass also. To prevent this we must wash it with water containing sal ammoniac dissolved in it: this salt not only prevents the oxide of iron from passing, but the solution of it speedily replaces the common salt in the oxide, and thus enables us to wash it much more speedily and completely than we otherwise could do. The oxide being washed, dried, and ignited, weighed 142·23 grains, equivalent to 99·56 grains of iron.

"The solution thus freed from iron was evaporated to dryness by a gentle heat: the residue redissolved completely in water, showing the absence of phosphate or arseniate of iron. The solution being mixed with carbonate of soda, a white powder fell, weighing after ignition 0·07 grains. It was brownish red, and being fused with carbonate of soda it exhibited the well-known characters of red oxide of manganese. It was equivalent to 0·05 grain of manganese. According to this analysis, the constituents of Dannemora iron are

Iron	99·56
Carbon	0·26
Manganese	0·05
Silicon	0·03
	99·90

"Thus almost the only foreign matter in Dannemora iron is carbon, which cannot be injurious as far as steel-making is concerned; for the manganese and silicon together amount only to 8 parts in the ten thousand, or not so much as the $\frac{1}{1000}$th part, which could not affect the quality to any great amount.

"In the Lowmoor iron I found no carbon; the only foreign bodies were manganese and silicon; the former to the extent of nearly 2 per cent., and the latter almost to that of $\frac{1}{1000}$th part: the analysis gave

Iron	98·060
Manganese	1·868
Silicon	0·090
	100.018

"In the Welsh iron the quantity of manganese was small, but, owing to an accident, I did not separate it from the iron. The silicon was sensibly the same as in the Lowmoor iron. But in the Welsh iron I found another substance, phosphorus, to the amount of nearly a half per cent.; this substance is entirely wanting in the Dannemora and Lowmoor irons. The constituents of Welsh iron were

Iron, with some manganese .	99·498
Phosphorus	0·417
Silicon	0·085
	100·000

The presence of phosphorus is probably the reason why Welsh iron is too brittle to be converted into steel.

"I hardly think that these analyses enable us to account for the striking difference in the specific gravity of these three irons. I rather ascribe this difference to a mechanical cause; the Dannemora iron has probably been exposed to longer hammering or rolling than either of the British specimens. If this be so, it will in some measure explain its greater strength, for the strength of iron, *cæteris paribus*, is well known to increase with the degree of hammering to which it has been subjected."

On the Sugar in Urine of Diabetes. By THOMAS THOMSON, *M.D. F.R.S., Professor of Chemistry, Glasgow.*

"Though the existence of sugar in the urine of persons labouring under the disease called *diabetes mellitus*, has been known for more than a century and a half, having been discovered by Dr. Willis, who died in the year 1678, and though it has been frequently extracted from such urine and exhibited in a state of purity, the author was not aware that experiments requisite to determine its nature had been made. After noticing the statement of Dr. Prout (Phil. Trans. 1827), that such sugar contained gr. 36 to 40 per cent. of carbon, and gr. 60 to 64 per cent. water; Dr. Thomson described the results of some experiments which he had recently undertaken to remove the uncertainty which appeared to involve the subject."

By evaporation, and subsequent digestion in alcohol, the sugar was obtained *white*, and by re-solution in boiling alcohol and slow cooling, acicular crystals were obtained. Specific gravity, when simply dried in air, 1378; heated to fusion (which takes place at 239°) the specific gravity becomes 1·423, while that of common sugar is 1·56, and that of sugar of grapes is stated by Prout at 1·5. Diabetic sugar dissolves without limit in boiling water, and at 62° in its own bulk of water.

By analysis with oxide of copper, the constitution of this sugar was found to be

Carbon......	37·23	or 12 atoms	= 9·00	or per cent. 38·09
Hydrogen....	7·07	13 ..	1·625	.. 6·88
Oxygen......	55·70	13 ..	13	.. 55·03
	100·00		23·625	100·00

Starch sugar, according to Dr. Prout, is composed of

Carbon......	12 atoms	9·
Hydrogen....	14 ..	1·75
Oxygen......	14 ..	14·0
		24·75

differing from diabetic sugar by an additional atom of water.

Pure crystallized sugar, according to the analysis of Liebig in 1834, is composed of

Carbon......	12 atoms	9·
Hydrogen....	11 ...	1·375
Oxygen......	11 ..	11·
		21·375

By uniting the diabetic sugar with oxide of lead, it was found to have combined with three atoms of oxide of lead, and to have lost three atoms of water, constituting a trisaccharate of lead, composed of

Carbon....	12 atoms	= 9·
Hydrogen..	10 ..	= 1·25
Oxygen....	10 ..	= 10·
		20·25
Oxide of lead 3	..	= 42·00

In this combination the diabetic sugar is therefore exactly isomeric with common sugar, in its combination with two atoms of oxide of lead, as determined by Berzelius.

From the yellowish-brown solution which had yielded the trisaccharate of lead the addition of alcohol caused a flocky precipitate to fall, which appeared, on analysis of a small quantity, to be disaccharate of lead, containing

$$\left.\begin{array}{ll} \text{Sugar........} & 0{\cdot}56 \\ \text{Oxide of lead..} & 0{\cdot}64 \\ \hline & 1{\cdot}20 \end{array}\right\}\text{or nearly two atoms of lead to one of sugar;}$$

and Dr. Thomson supposes from some trials he made that the sugar in this combination had lost two atoms of water, so as to be composed of

Carbon,	12 atoms	=	9·
Hydrogen,	11 ..	=	1·375
Oxygen	11 ..	=	11·
			21·375

but the analysis requires repetition on a larger scale before any definite conclusion on this particular subject can be drawn.

It appears from the facts stated in this paper that there are three species of sugar distinguished from each other by the quantity of water which they retain when heated to as high a temperature as they can bear without decomposition; viz. common sugar, which may be deprived of one atom of water by combining with oxide of lead; diabetic sugar, which may in like manner be deprived of three atoms of water; and starch sugar, which by *analogy* may be presumed to be capable of losing four atoms: so that all the species would, under these conditions, become isomeric with anhydrous common sugar.

If these views have any solidity, it would appear that sugar, like phosphoric acid, has in all cases the same constitution, and that the three states of it depend upon the quantity of water and probably of other bases with which it is disposed to combine.

Common sugar combines with one atom of water, diabetic sugar with three, and starch sugar with four. There is doubtless a fourth variety of sugar, not yet discovered, capable of uniting with two atoms of water.

All the three species are capable of undergoing fermentation and of being resolved into 4 atoms carbonic acid and two atoms alcohol.

4 atoms carbonic acid	$C^4 \quad O^8$
2 atoms alcohol	$C^8 H^{12} O^4$
	$C^{12} H^{12} O^{12}$

Starch sugar has an excess of 2 atoms of water and diabetes sugar of 1 atom; while common sugar requires an atom of water to undergo the decomposition.

Dr. Thomson had previously mentioned that 39·65 grains of diabetes sugar dried in vacuo over sulphuric acid, when exposed for 24 hours to the heat of a steam-bath, lost 3·35 grains of moisture, and were of course reduced to 36·3 grains.

But 36·3 : 3·35 : : 23·625 : 2·18.

It follows from this, that diabetes sugar dried over sulphuric acid is deprived of two atoms of water when exposed to the heat of a steam-bath or to 212°. The diabetes sugar, therefore, when in crystals, is composed of

12 atoms carbon	=	9·
15 atoms hydrogen	=	1·875
15 atoms oxygen	=	15·
		25·875

So that the atomic weight of the crystals of this sugar is 25·875, and it can be deprived of 5 atoms of water by combining it with oxide of lead.

Dr. Thomson then noticed a *crystallized* sugar obtained from diabetic urine by Mr. Macgregor of Glasgow, by Ambrosiané and Maitland from the serum of blood in diabetes, and by himself in urine in a case of diabetes, 1827. This sugar was in 4-sided prisms of 110° and 70°, white, translucent, sweetish, soluble in alcohol. This is believed by Dr. Thomson to be a fourth kind of sugar, but having been interrupted in his experiments upon it he recommends the subject to the attention of chemists.

On Galactin. By THOMAS THOMSON, *M.D., F.R.S., Professor of Chemistry, Glasgow.*

This is a substance which constitutes the principal ingredient in the sap of the Cow-tree, or *Galactodendron utile* of South America, which is used as a substitute for cream. The sap, on standing, throws up a white matter, soluble in boiling alcohol, but deposited as that liquid cools. When well washed and dried, in vacuo, over sulphuric acid, it constitutes galactin. It is yellow, translucent, brittle, has a resinous aspect, and is tasteless. It is insoluble in water, but becomes white and soft by imbibing that liquid. It is soluble in alcohol and ether. This white compound becomes soft and ductile at 60°; at 117° it is still solid, but at 137° it is liquid. Abundance of aqueous vapour is driven off, but the galactin does not become translucent and yellow till kept some time at 170°. The specific gravity of pure galactin is 0·969. It dissolves readily in oil of turpentine and olive oil. It does not combine with potash, nor form a soap. Its constituents are—

6 atoms carbon	= 4·5, or per cent.		72
6 ...... hydrogen	= 0·75	....	12
1 .... oxygen	= 1	....	16
	6·25		100

being isomeric with Brazil wax, which does not, according to Mr. Brande, form a soap with potash.

Notice respecting the native Diarseniate of Lead. By THOMAS THOMSON, *M.D., F.R.S., Professor of Chemistry, Glasgow.*

During the last meeting of the British Association at Liverpool, a mineral dealer from Cumberland exposed a collection of North of England minerals for sale. Among others there was one labeled *vanadiate of lead*, from Caldbeck fell. On examining it, Dr. Thomson thought that it differed too much, both in its colour and lustre, from the true vanadiate of lead, of which he had been in possession for some years, to be that mineral; and, upon comparing it with the vanadiate in his cabinet, this suspicion was confirmed.

The Caldbeck fell mineral was in botryoidal concretions, upon quartz. When examined by the microscope many of the nodules had the aspect of cylinders.

Colour honey yellow, similar to that of the Cornish arseniate of lead, first described and analyzed by Mr. Gregor, but lighter, and much less translucent.

The lustre is resinous, and it has much greater brilliancy than specimens of vanadiate of lead.

The hardness is not easily determined, from the shape of the nodules. Calcareous spar was not scratched by rubbing them against it while selenite was scratched by them with great facility. The specific gravity by two different trials was 7·272. This specific gravity is decisive that the mineral is not *vanadiate of lead*, for the specific gravity of native crystals of vanadiate is only 6·663. The specific gravity of Cornish arseniate of lead is still lower, being 6·41.

When exposed to a red heat on platinum foil it undergoes no alteration, except becoming a shade lighter in colour. Before the blow-pipe on platinum it melts into a transparent globule, which assumes nearly its original appearance on cooling, and does not crystallize like phosphate of lead. On charcoal it gives out abundance of arsenical fumes, when acted on by the blow-pipe, and a globule of metallic lead is obtained.

It was analyzed twice in Dr. Thomson's laboratory, with every attention to accuracy, by Mr. Stenhouse. During the first analysis he suspected the presence of a minute quantity of phosphoric acid; but he did not succeed in separating it from the arsenic, and of course the actual existence of this acid in the mineral is still problematical. The quantity is certainly very minute, and cannot amount to so much as half per cent., otherwise it would have been separable from the arsenic acid.

The two analyses were very similar: the following are the constituents as determined by the second analysis, which Mr. Stenhouse considers as most to be depended on:

Chlorine	2·46
Lead	7·10
Arsenic acid . . .	18·20
Protoxide of lead . .	70·14
Peroxide of iron . .	1·20
Volatile matter . . .	1·00
	100·1

The moisture and peroxide of iron are obviously accidental impurities. The chloride of lead in 100 grains of the mineral amounts to about half an atom, the arsenic acid to 2½ atoms, and the oxide of lead to 5 atoms. If we abstract the chloride of lead, which exists in nearly the same proportion in phosphate of lead, vanadiate of lead, and arseniate of lead, as in this mineral, the constituents are 1 atom arsenic acid, and 2 atoms protoxide of lead. It is therefore a diarseniate of lead, constituting a new species of lead ore, which has been met with the first time in Cumberland at Caldbeck fell. The author's specimen of this mineral exhibits a deposit of yellow phosphate of lead upon another side of the mass of quartz, upon which the diarseniate has been deposited.

On Emulsin. By Dr. T. Thomson *and* T. Richardson.

Some years ago Robiquet and Boutron Charlard showed that volatile oils of bitter almonds and prussic acid, which are obtained by the distillation of bitter almonds, do not exist naturally in almonds but result from the process. They further ascertained that when milk of bitter almonds, formed by triturating almonds with water in a mortar, is treated with strong boiling alcohol, white crystals are deposited, on cooling, which separate in larger quantity by concentration. To this substance they gave the name of *Amygdalin.* Liebig and Wöhler have determined this body to be an amide of amygdalic acid, represented by the following formula:

$$N_2 C_{40} H_{54} O_{22} + H_4 O_2$$

Subsequently, the investigation was continued by Wöhler and Liebig, who observed that when a solution of amygdalin is brought in contact with a milk of sweet almonds, a most remarkable and peculiar action takes place; prussic acid and oil of bitter almonds are formed, as in the instance already mentioned. When milk of bitter almonds is distilled without the artificial addition of amygdalin, besides prussic acid and oil of bitter almonds, there is also formed sugar, which may be decomposed by fermentation. The solution after the termination of the fermenting process affords a strong acid reaction which is not produced by acetic acid or any other volatile acid. When alcohol is added and the solution concentrated, thick white flocks are precipitated which obviously contain no emulsin, because when dissolved in water they have no action upon amygdalin. From these properties the flocks would appear to be gum.

The phenomena exhibited in the reaction described, which have been termed *Catalytic* by Berzelius, resemble in a great measure those which take place in fermentation; and their investigation promises to throw great light upon some of the most important processes of the vegetable and animal œconomy. With the view of assisting in the elucidation of the subject, the authors have commenced with the examination of the essential ingredient of the milk of sweet almonds which has been termed *emulsin.*

The process by which this substance was obtained was as follows. Sweet almonds were triturated in a mortar and small portions of water were gradually added until a milky fluid was obtained. This fluid was mixed with four times its volume of ether and frequently agitated so as to effect an intimate mixture. A clear fluid gradually separated at the bottom of the stoppered bottle in which the experiment was made, which in the course of three weeks was drawn off by means of a syphon. This fluid was passed through a filter, and to one-half of the clear solution a large quantity of alcohol was added; a copious precipitation of white flocks ensued; these were *emulsin.* From the other half the emulsin was separated by bringing the solution to the boiling point, when it precipitated in flocky coagula. The emulsin precipitated by alcohol was carefully washed with alcohol, and then dried over sulphuric acid in the vacuum of an air-pump, to avoid the effects of heat. In this state it possessed the following characters: it is a white powder, destitute of taste and smell, soluble in water, insoluble in alcohol and ether. When submitted to analysis in the usual way, the following results were obtained:

I. ·3485 grns. gave ·6180 grns. CO_2 and ·2445 grns. H_2O.
II. ·3625 grns. gave ·6365 grns. CO_2 and ·2505 grns. H_2O.

The relation of the carbon and azote, as determined by experiment, was 6 CO_2 : 1 N or 3C : 1N. From these data, which the authors would desire to state only with diffidence till better confirmed, the following composition may be deduced:

	I.	II.
C.	49·025	48·555
N.	18·910	18·742
H.	7·788	7·677
O.	24·277	25·026
	100·000	100·000

The fact of the existence of the substance operated on in the almonds appears to be established by its acting on amygdalin in the same manner as the milk of almonds in the case alluded to in the commencement of the paper. After numerous trials with various re-agents, its most distinguished character was elicited by the phenomena exhibited when boiled with barytes. During the whole of the boiling, which was continued for above six hours, ammonia was slowly and continuously disengaged. Through the solution a current of carbonic acid was passed and the whole filtered; the clear solution was evaporated to dryness, and the residual salt, which contained a large quantity of barytes, possessed a strongly bitter taste, leading to the conclusion that emulsin is an amide, and that the salt formed by the action of barytes is a compound of barytes with an acid which it is proposed to term *emulsic* acid. From this fact the authors are inclined to infer that fibrin, gelatin, casein, &c., are all amides.

Examination of Sphene. By THOMAS RICHARDSON.

The author having been supplied with two specimens of sphene by Mr. Hutton, of Newcastle, submitted them to analysis. One of these, from Arendahl, in Norway, possessed a specific gravity of 3·425:—colour, light brownish yellow;—translucent;—brittle;—fracture uneven;—lustre vitreous, inclining to resinous. The mineral was fused with carbonate of soda, and the fused mass digested in the cold with dilute muriatic acid. Caustic ammonia was added, and the precipitate separated by filtration. The precipitate was washed with cold distilled water, and in this state was exposed to the air for about six weeks without the application of heat, when it appeared quite dry. The solution and washings from the precipitate were carefully evaporated to dryness, but on digesting the same with water everything redissolved, showing that all the silica had been precipitated by the ammonia. The solution, after being gently heated, was mixed with oxalate of ammonia, and the precipitated salt of lime thrown on a filter. The dry precipitate obtained by ammonia was digested in the cold with concentrated muriatic acid, and the insoluble portion, after the ordinary washing and ignition, weighed. The titanic acid was precipitated from the filtered solution after being gently warmed by caustic ammonia, of which reagent an excess was carefully avoided. The specific gravity of the second specimen was 3·5128 ; hardness, 6·75;—lustre, resinous; —colour, cinnamon brown ;—cross fracture, granular and uneven;—opaque, but translucent in thin plates. Before the blowpipe alone on charcoal, it became white, but did not fuse. With carbonate of soda in the oxidizing flame it fused into an orange bead. Its locality is not known. The following is the composition of the two specimens :—

Silica	31·05	29·35
Titanic Acid	43·90	42·60
Lime	24·80	24·90
Water..........	1·00	1·15
	100·75	98·00

and the formula,

$$3\,(Ca\,O, 2\,Ti\,O_2) + 2\,(Ca\,O, 2\,Si\,O_3).$$

On a New Process for the Extraction of Silver from Lead. *By* H. L. Pattinson.

The object of this communication was to lay before the Association an account of a discovery made by the author some time ago, the application of which to practice constitutes a new process in the arts, and forms an important improvement in the extraction of silver from lead.

Adopting the estimate of Mr. J. Taylor, in 1828*, that the quantity of lead raised annually in England and Wales amounts to 45,500 tons, the author states that it all contains silver, in variable proportions, but with so much of constancy in the proportion of silver in the lead ore of each vein, that it is easy to arrive at a tolerably accurate knowledge of the quantity of silver contained in the lead of each district.

Of 22,000 tons of lead yielded in the district of Alston Moor, it is believed that 16,000 tons contain silver at the rate of from 6 to 12 oz. per ton, and 6000 from $3\frac{1}{2}$ to 6 oz. per ton—the average being about 5.

* See Records of Mining for that year; also Reports of the British Association, vol. v., for an estimate, by Mr. J. Taylor, of the quantity of lead raised in Great Britain in 1835.

4700 tons from Swaledale, and Wharfdale, Pateley Bridge, &c. yield on an average only 2 oz. per ton. The lead of Derbyshire and Shropshire, 4800 tons, from 1 oz. to $1\frac{1}{2}$ oz. only. The lead of Cornwall and Devon, 2000 tons, is rich in silver, so as to yield on an average 20 to 30 oz. per ton; half of that from Flintshire and Denbighshire, contains from $4\frac{1}{2}$ to $6\frac{1}{2}$ oz., and the other half 9 or 10 oz.

The ordinary process of cupellation or refining*, consists in the oxidation of the lead, kept at a red heat, and traversed by a current of air; the silver remains nearly pure; the oxide of lead is either reduced, or sold as litharge. The cost of this process and the waste of lead are so considerable, that with lead at the price of 20*l.* the ton from 6 to 8 oz. of silver are required to barely cover the whole charge against the operation. About 18,000 tons of the whole quantity raised in England and Wales are supposed to undergo the refining process; and the waste of lead upon the whole quantity would amount to 1000 tons. Where the lead before refining was impure, with admixture of other metals, its quality is improved by the process, sometimes to the value of 10*s.* a ton; but this is only the case in small quantities.

The desirableness of some more economical mode of extracting silver from lead has been long obvious to those conversant with that branch of our national industry; and Mr. Pattinson had for some years been engaged in occasional experiments on the subject. Among these, he describes the attempts which he vainly made to separate the lead from the silver by *distillation* and *long-continued fusion.*

Various other experiments were tried by the author, both in the dry way and by the application of liquid menstrua, all of which were unsuccessful; but during their prosecution in the month of January 1829, he required lead in a state of powder, and to obtain it, adopted the mode of stirring a portion of melted lead in a crucible, until it cooled below its point of fusion, by which the metal is obtained in a state of minute subdivision. In doing this he was struck with the circumstance, that as the lead cooled down to nearly its fusing point, little particles of solid lead made their appearance, like small crystals, among the liquid lead, gradually increasing in quantity as the temperature fell. After observing this phenomenon once or twice, he began to conceive that possibly some difference might be found in the proportions of silver held by the part that crystallized, and the part that remained liquid. Accordingly, he divided a small quantity of lead into two portions, by melting it in a crucible, and allowing it to cool very slowly with constant stirring until a considerable quantity crystallized, as already mentioned; from which the remainder, while still fluid, was poured off: an equal weight of each was then submitted to cupellation, when the button of silver from the liquid lead was found to be very much larger than that from the crystallized lead; proving that argentiferous fluid lead suffers a portion of silver to escape from it, under certain circumstances, in the act of becoming solid.

* See Mr. Sadler's Essay in Nicholson's Journal, vol. xv., and Mr. Pattinson's paper in Newcastle Transactions, vol. xi. pt. I.

The lead used in the original experiment was what is considered rich in silver; it contained 40 oz. 15 dwts. 8 grs. per ton, and was divided into a crystallized portion, found to contain 25 oz. 4 dwts. 21 grs., and a fluid portion, holding 79 oz. 11 dwts. 12 grs. per ton; the latter being necessarily much smaller than the former in quantity. The experiment was repeated a great number of times upon lead of every variety as to proportion of silver, with the same general result; but being always performed in a crucible upon small quantities of lead, which of necessity cooled quickly, the crystallized portion was never entirely deprived of its silver, nor indeed reduced below two or three ounces per ton.

It was not until the spring of the year 1833 that the author was conveniently circumstanced to proceed in applying to practice the principle he had developed; but at that time his attention was again directed to the subject, and he began by providing large pots of cast iron, in each of which he could melt together and crystallize several tons of lead. All the phenomena of crystallization in the large way were speedily observed, which, with the mode of conducting the operation adopted then and since continued without alteration, may be thus briefly described. Four or five tons of lead being melted in one of the pots, the metal was carefully freed, by skimming, from all dirt or oxide, and its surface made quite clean; it was then suffered to cool very slowly, care being taken to break off and mix with the fluid mass, from time to time, any portion that might congeal on the sides of the pot: when the temperature had fallen sufficiently, small solid particles or crystals began to form, principally upon the surface of the melted mass. These, if suffered to remain, would have cohered together and formed a solid crust; but being continually struck, and the whole body of metal kept in motion by constant stirring, they sunk down to the bottom of the pan, and soon appeared in considerable quantity. By means of a perforated iron ladle, the crystals were taken out of the pan from time to time as they formed, and placed in another pot, the liquid lead being drained out of them as much as possible, and suffered to flow back into the original pot. In this way the operation was conducted until two-thirds or three-fourths of the original lead was crystallized and withdrawn from the pot. The author now found, as before, that the crystals always contained much less silver than the lead from which they were formed; but still he did not succeed by one or even by two crystallizations, when operating with lead containing eight ounces of silver per ton, in making them sufficiently poor: for instance, a pot filled with 8 ounce lead would yield at first crystals holding from 1 to 2 ounces of silver; in a little time, as the lead in the pot became richer by receiving silver from the previously formed crystals, it yielded crystals of 2 to 3 ounces; and the crystals became progressively richer, until, in the end, the original lead was divided into three parts of crystalized lead holding about 4 ounces, and one part liquid lead holding about 20 ounces per ton. Upon the lead of 4 ounces, as well as upon the lead of 20 ounces, the operation might evidently be repeated without limit, until the crystals became nearly free from silver on the one hand, and the liquid lead exceedingly rich on the other; but this seemed to involve so much labour and delay, that the author was most desirous of finding a mode by which the object could be accomplished at once. Conceiving that the crystals would be rendered poorer if more thoroughly drained from the liquid lead, he adopted the plan of exposing them, after removal from the pot to a cautiously regulated heat in the chamber of a reverberatory furnace, so as to melt out from among them a further portion of liquid lead; and in this way he succeeded in obtaining at one operation from original lead holding 12 ounces of silver per ton, four parts of poor lead containing not more than $\frac{3}{4}$ of an ounce per ton, and one part of rich lead containing 50 ounces per ton, or thereabouts. This was effected at a moderate expense, and with a very inconsiderable loss of lead; and the new process thus arrived at, which he called the process of separation, was immediately adopted and carried on in this way for some time at different lead-works in the kingdom.

The exposure of the crystals a second time to heat, required, however, a peculiar and rather expensive apparatus, and was found somewhat difficult to get managed properly, for the workmen could not always keep the furnace in which it was performed at the exact temperature necessary for the operation; and it often happened that, by the application of too much heat, the crystals were melted entirely without being drained of their richer lead; besides, the lead exposed to heat in its crystallized state was oxidized rapidly, and the subsequent reduction of the oxide occasioned some loss of metal. These objections to the draining process induced the author to recommend in preference the simple plan of repeated crystallization, which has been everywhere adopted, and now constitutes the process of separation; experience and practice have gradually rendered it easy and perfect, and it has become an established operation among the arts of this country.

The apparatus required for the separating process is exceedingly simple, and consists merely of a number of nearly hemispherical iron pots, each capable of holding about five tons of lead, the size for which is about 4 ft. diameter and 2 ft. 3 in. deep; one or two smaller pots, 18 in. diameter by 2 ft. deep, are required for the purpose of holding melted lead, in which the perforated iron ladles are to be occasionally dipped to keep them hot; and another pot, about 2 ft. 10 in. diameter by 1 ft. 10 in. deep, for melting the ultimate poor lead to be cast into pieces. These, with a few perforated iron ladles 15 in. diameter, and 5 in. deep, and one or two whole ladles of lesser size for casting the melted lead into pigs, are the principal articles required. The large pots are to be placed side by side in a line, each with a separate fire-place, (upon which there must be an ash-pit door as well as a fire door,) and also with a separate flue and damper, so that the draught under each pot can be entirely stopped by closing the flue with its damper, and the heat of the fire-place in some measure retained by shutting the ash-pit door. Above the centre of this line of pots, at the height of six or eight feet, it is convenient to have a small iron railway, with a frame or carriage on four wheels to move backwards and forwards the whole length of the range of pots, from which is to depend a chain, terminated by a hook at the bottom, and reaching to nearly the top of the pots. This is for the purpose of more easily conveying the ladles filled with crystals from pot to pot.

All this being provided, one of the large pots is filled with lead, containing silver, say 10 oz. per ton, and after it is melted and skimmed, the fire is withdrawn, the damper put down, and the ash-pit door closed, when it cools and crystallizes as already described. Crystals, as they are formed, are laded out into the second pot until about three-quarters of the whole have been removed, which will contain about 5 ounces of silver per ton: upon this the operation is repeated, giving lead 2 ounces; and by a third crystallization, there is obtained from this, poor lead, holding not more than 10 to 15 dwts. of silver per ton, which is cast into pieces for sale as separated lead. The rich lead, on the other hand, is collected and repeatedly crystallized, until it is made to contain 200 or 300 ounces per ton, after which the silver is extracted by cupellation. In working, the different pots at each stage are filled up always with lead of the same content of silver before beginning to crystallize, and a greater or less amount of crystals taken out, as the operator may think fit, in which respect the practice differs almost at every establishment; but the process is so very simple and the mode of proceeding so obvious, that it is unnecessary to give a more minute detail.

By operating in the way described, it is evident that but a very small portion of lead is made to undergo the process of cupellation, not more than one twentieth part, when 10 ounces of lead is enriched to 200 ounces by repeated crystallization; and as the loss by separation has not been found to exceed a 250th part of the whole lead, the loss by the joint processes becomes $\frac{1}{12}$ of $\frac{1}{20} + \frac{1}{250}$, or about one part in 120. The expense of separation is something less than that of cupellation, so that by the reduction of expense and the reduction of loss of lead, the extraction of silver is so far economized that 3 ounces per ton will now fully cover the whole charge.

By this reduction of the cost of extracting the silver, *all* the lead of Alston Moor (22,000 tons), Devon, Cornwall, and West Cumberland (2000), and the lead of North Wales (12,000), making a total of 36,000 tons per annum, can now be made to yield up its silver with advantage, so that on the very low average of 6 ounces per ton, at least 54,000 ounces of silver per annum are gained by the arts. There may also be safely estimated a reduction of the loss of lead on the 18,000 tons generally refined by cupellation of at least 300 tons. The lead obtained by separation is much improved in quality, being more soft and ductile than ordinary lead.

It only remains to consider, how it happens that lead in the act of consolidation gives up a portion of its silver to the surrounding and still fluid lead; and the most simple view of the matter is, undoubtedly, that it is an instance of true crystallization, in which the homogeneous particles of lead are drawn together by virtue of their molecular attraction, to the exclusion of the foreign body, silver. On examining the crystals, it is true, no trace of regular form can be perceived; but this could scarcely be expected, from their being so much agitated and broken at the instant of their production: if, however, a pot in the act of crystallizing is suffered to remain at rest a few moments until a crust forms on its surface, on carefully withdrawing a portion of this crust, it is found on its under side to exhibit a distinctly crystalline appearance, proving that the solid particles, which are merely this crust broken to pieces, are the result of a rapid crystallization.

This reasoning the author endeavoured to confirm by illustrations drawn from other chemical processes, and mentioned experiment to ascertain the degree in which, by a cautiously regulated heat, silver may be separated from lead by the process of eliquation. Pieces of lead were most cautiously heated till a few drops of fused metal oozed out from their pores; this was found to be slightly richer in silver than the original mass. In these experiments, as in the draining of the crystals, the separation is effected by the difference of fusibility between pure lead and lead containing silver, aided, no doubt, by the tendency of pure lead, in that state of semi-fluidity, to assume a crystalline form.

Observations on some of the Products obtained by the Action of Nitric Acid on Alcohol. By Golding Bird, *M.D., F.L.S., G.S., &c., Lecturer on Natural Philosophy at Guy's Hospital, London, &c.*

In this paper the author alluded more particularly to the nature of the substances produced simultaneously with hyponitrous ether during the preparation of the spiritus etheris nitrici of the London Pharmacopeia, which products have been usually stated to be malic, oxalic, acetic, and carbonic acids, together with a substance mentioned by Thenard as " très facile à charbonner," in addition to the hyponitrous ether (4 C, 5 H, O + N, 3 O). Taking advantage of the residue left after preparing a large quantity of sp. eth. nit. in the pharmaceutical laboratory of Guy's Hospital, Dr. Bird instituted a series of experiments enumerated in his paper, from which he was induced to believe that oxalic acid was by no means a necessary product, and that it is not generated until aldehyd begins to appear in the distilled fluid. As the paper is published entire in the Philosophical Magazine for this year, it is unnecessary to do more than give the result of Dr. Bird's investigations:

1. That in the preparation of sp. etheris nitrici, as long as the latter, with alcohol only, distils over, no oxalic acid is produced; an acid apparently identical with the oxalhydric alone appearing in the retort.

2. That on continuing the distillation beyond this point, the free nitric acid in the retort acting on the oxalhydric acid, generates oxalic acid.

3. That during the action of nitric acid on alcohol in the cold, as in Dr. Black's process for the preparation of hyponitrous ether, acetic acid is produced, instead of, or in addition to, oxalhydric acid.

4. That aldehyd is, as has been long known, produced by the action of nitric acid on alcohol, but that it is not formed in any quantity, or at

least does not appear in the distilled fluid until the formation of hyponitrous ether has nearly or altogether ceased.

5. That the production of aldehyd and oxalic acids are nearly simultaneous; and that both these appear to result from the secondary action of nitric acid upon products formed in the earlier stages of the operation.

6. That the crystals long known as "les cristaux de Hierne" formed when the distillation is protracted until red fumes appear, are oxalic acid, notwithstanding their remarkable micaceous form; and that the "substance très facile à charbonner" of Thenard is probably aldehyd, which, from its behaviour with alkalies, might apparently merit that character.

Notice respecting the Artificial Formation of a Basic Chloride of Copper by Voltaic Influence. By GOLDING BIRD, *M.D., &c.*

Becquerel has proved that a homogeneous metallic surface exposed to the action of a given fluid will assume a state of electric tension, provided that the fluid in which it is immersed is of different degrees of concentration in two different layers, so that the plate may become unequally acted upon at two different points. The crystallization of protoxide of copper by the immersion of a plate of that metal in a solution of its nitrate, some of the black oxide being placed in the lower part of the vessel, affords a familiar example of this circumstance. But if the fluid remains homogeneous, *quoad* its degree of concentration, no action, so far as electricity is concerned, ensues, unless one portion of the metallic surface immersed is in a condition which enables it to be more readily acted on than the other. This may be effected by partial and superficial oxidation, by roughening or burnishing part of the metallic surface, or by the induction of that peculiar passive state which Schönbein has shown to exist in some metals under certain circumstances.

If a plate of metallic copper is made the negative electrode of a single pair, acting on a solution of copper, crystallization of the latter metal, often in delicate, rosette-like patches, if the current is weak, ensues. This deposition generally takes place at the lower part of the metallic plate, leaving the upper one smooth and free from crystals. A plate thus prepared is in a condition to assume two different electric states on immersion in a homogeneous fluid, in consequence of the delicate crystals of metallic copper undergoing oxidation more readily than the smooth part of the plate. Such a piece of metal was immersed in a solution of common salt during three months in a dark closet; on examining the plate at the end of that time, the smooth portion, on which no metallic crystals had been deposited, was found tarnished and covered with blackish patches, without any perceptible roughening of the surface. The lower portion of the same plate on which the copper had crystallized had undergone a very interesting change, the metallic crystals having become replaced by an infinite number of hemispherical patches from a mere point to the size of a large pin's head, of a rich green colour, of a somewhat velvety or satiny lustre. On breaking some of the crystals they were found to be radiated like zoolite, without any metallic nucleus, and firmly adhering to the copper on which they were deposited. The crystals were insoluble in water, did not effervesce with sulphuric or nitric acids, in which they were with difficulty soluble, the solution being at first brownish, and readily becoming green by exposure to the air: their solution, in dilute nitric acid, precipitated nitrate of silver. These hemispheric-radiated crystals are therefore regarded by Dr. Bird as a basic chloride, probably resembling the native tribasic chloride. This, however, he has not yet had an opportunity of proving by direct analysis.

The theory of the formation of these crystals appears to be very simple. On immersing the copper plate into the brine, its electricity became disturbed, and two states of electric tension were assumed, the smooth and polished part becoming the negative, and the rough and crystalline portion the positive electrode of a simple voltaic circle. The chloride of sodium becoming decomposed, the chlorine uniting with the crystalline surface of the positive electrode, the soda being at first set free, although probably almost immediately after re-acting on the newly-formed copper salt, and thus reducing it to the state of basic chloride, the crystalline deposition resulted from the slowness of the action, as in Becquerel's experiments.

Notice respecting the Deposition of Metallic Copper from its Solutions by slow Voltaic Action at a point equidistant from the Metallic Surfaces. By Golding Bird, *M.D., &c. &c.*

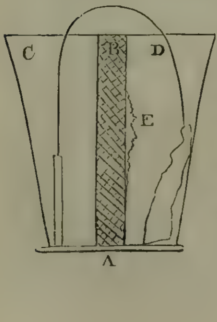
At the last meeting of the Association Dr. Bird presented some remarks on the possibility of reducing metallic bases on surfaces not in contact with the electrode*. During the past year he has varied his experiments, chiefly with a view to the prevention of any source of fallacy connected with accidental metallic contact; and, although he has repeatedly succeeded in reducing metals on, or in the centre of masses of earthy substances, as plaster of Paris or clay, he has never yet obtained metallic deposits in a fluid intermediate between electrodes when these substances were absent. An interesting modification of the apparatus, described at Liverpool, has been contrived by Mr. Sandall, chemical assistant at St. Thomas's hospital, whilst engaged in constructing a voltaic battery on Prof. Daniell's arrangement, but in which the membranes should be replaced by cylinders of sulphate of lime. This gentleman carried on his experiments during this summer, and Dr. Golding Bird requested him to break up the masses of plaster that had been used in his arrangement, for the purpose of ascertaining whether any deposition of metallic copper had taken place at any part not in connexion with the metallic surfaces, as

* See vol. vi., p. 45.

he considered these results would go far to support or refute the opinions he had hazarded on this subject.

The results of these experiments were uniform; they differed from Dr. Bird's in one remarkable circumstance, viz. that the copper, instead of being deposited in a crystalline form, was in nodular or tubercular masses (a specimen was exhibited to the Section). When the plaster of Paris diaphragm, which of course was vertical, was carefully made, and one side of a jar thus divided was filled with water, and the other with a solution of sulphate of copper, no intermixture of fluids was evident even at the end of a month. On placing in the solution of sulphate of copper, at a certain distance from the vertical partition, a mass of copper pyrites, connected by a copper ribbon with a piece of zinc immersed in the water in the other cell, voltaic action slowly commenced. At the end of a few days the water in the zinc cell became acid, bubbles of hydrogen gradually appeared, and were slowly evolved, whilst the copper pyrites slowly assumed the iridescent appearance of peacock-ore described by Mr. Fox: in a month the apparatus was dismounted; no trace of sulphate of copper was found in the water cell, and neither the zinc nor the mass of pyrites had touched the plaster diaphragm. On removing the latter, a copious deposition of firmly adherent metallic copper, in a nodular or almost stalagmitic form, was found on that surface which had been exposed to the metallic solution; and, on breaking the mass of plaster transversely, numerous delicate veins of copper appeared permeating it in every direction. This experiment is considered by Dr. Bird to be less liable to sources of fallacy, and much less exceptionable than those described by him last year; for, not only is all metallic contact with the plaster diaphragm carefully avoided, but the very form of the reduced copper would afford an argument against its being furnished by portions shooting off from the negative electrode, on the beautifully iridescent surface of which, moreover, no trace of reduced copper had appeared.

Arrangement of the Apparatus.



A is a conical earthen vessel, in which the plaster diaphragm B is carefully fitted.

C. The cell filled with water, and containing a piece of zinc metallically connected with a mass of native copper pyrites, placed in the cell D, which is filled with a solution of sulphate of copper. On the surface of B, where the irregularities E are sketched, is the copper deposited in a nodular form, and connected apparently with delicate metallic veins traversing B in every direction.

On a new Compound of Sulphate of Lime with Water. *By Prof.* Johnston, *F.R.S.*

This compound is represented by $2\dot{C}a\,\dddot{S} + \dot{H}$, and occurs in masses of a minute radiated structure, in minute cylindrical crystals, and in large six-sided prisms. It is formed as a sediment in the boiler of a steam engine at the Team Colliery, near Newcastle. The boiler is worked under an average pressure of about two atmospheres. (*See the Lond. and Ed. Phil. Mag. for Nov.* 1838.)

On a new Compound of Bicyanide with Binoxide of Mercury. *By Prof.* Johnston, *F.R.S.*

When dilute hydrocyanic acid is digested on red oxide of mercury in excess, a white nearly insoluble compound is formed, which may be separated from any soluble bicyanide which may be present in the supernatant liquid by collecting it on the filter. Boiling water dissolves the new compound, and leaves the excess of oxide of mercury. On cooling, the salt is, in a great measure, deposited on the sides and bottom of the vessel in minute, pure, white, transparent, prismatic needles. This salt is anhydrous, its solution has an alkaline reaction, and it consists of equal atoms of the two mercurial compounds, or it is ($HyCy_2 + HyO_2$). When heated in a tube, it decomposes with a slight detonation, giving off carbonic acid, nitrogen, cyanogen, and metallic mercury, leaving a black residue (*para-cyanogen*). Neutralized by nitric acid, it gives a beautiful salt in long, delicate, quadrangular prisms, which are represented by $HyCy_2 + (HyO_2 + \frac{1}{2}\,NO_5)$, and are very soluble in water. It gives also with acetic acid, a crystalline compound, in which the quantity of acid appears to exist in a still smaller proportion. With acid nitrate of silver, it gives Wöhler's salt ($HgCy_2 + \dot{A}g\overset{.:.}{N} + 4\dot{H}$), nitrate of mercury remaining in solution. With neutral nitrate of silver and various other salts, it gives crystalline compounds.

On some supposed Exceptions to the Law of Isomorphism. *By Prof.* Johnston, *F.R.S.*

In this paper the author endeavoured to show, that if the chemical analyses and crystalline measurements of certain groups of substances are to be depended on, the law indicated by previous researches, *that like forms indicate like formulæ, is not universally true.* The paper does not admit of abridgement, but may be consulted in the *Lond. and Ed. Phil. Mag. for Dec.*, 1838. *See also Reports of the British Association, Vol. VI., p.* 173, *et seq.*

On the Origin of Petroleum, and on the Nature of the Petroleum from Whitehaven. By Prof. JOHNSTON, *F.R.S.*

The author stated that petroleum was obtained in considerable quantity from the mass overlying the three several seams of coal which are worked in the neighbourhood of Whitehaven. This petroleum he found to agree in nearly all its characters with that of Rangoon. In thickness, colour, smell, and especially in the products of distillation alone, and with water, the two varieties agreed; and as there can be no doubt of the origin of the one variety—that it has been volatilized from the coal into the bed which covers it, and from which it now exudes—the author of the paper considered it to be almost certain that the wells of Rangoon must derive their supplies from subjacent beds of coal, and that deposits of combustible matter are to be looked for wherever similar sources of petroleum are met with.

On Middletonite and some other Mineral Substances of Organic Origin. By Prof. JOHNSTON, *F.R.S.*

The name Middletonite is given by the author to a yellow resinous substance found in the body of the coal at the Middleton Collieries, near Leeds, and in other parts of the Yorkshire and the Staffordshire coal fields. It is represented by the formula $C_{20}\,H_{11}\,O$, and is chiefly interesting as being in all probability the resin of certain trees of the carboniferous epoch, more or less altered.

Of the Resin of Gamboge (Gambodic Acid) and its Compounds. By Prof. JOHNSTON, *F.R.S.*

Prof. Johnston stated that this acid resin is represented approximately by the formula $C_5\,H_3\,O$, and that it forms three classes of salts, represented respectively by

$$RO + 5\,(C_5\,H_3\,O)$$
$$RO + 10\,(C_5\,H_3\,O)$$
$$RO + 15\,(C_5\,H_3\,O)$$

This resin he also stated to be distinguished from all other known resins by dissolving in dilute caustic ammonia, forming a solution which may be diluted with any quantity of water, and which throws down gambodiates from ammoniacal solutions of magnesia, of the oxides of copper, zinc, silver, manganese, and the other metallic oxides which dissolve in dilute ammonia. No other resin is known to be capable of giving salts from aqueous solutions.

On a Blue Pigment. By R. PHILLIPS, *F.R.S., &c.*

During the meeting of the Association at Liverpool, Professor Traill exhibited to the chemical section a fine blue pigment prepared by adding a solution of ferrocyanide of potassium to one of chloride of antimony; at the request of the Professor, Mr. R. Phillips undertook to examine the pigment in question, and has communicated the following observations.

" Having by me some chloride of antimony, which I employ on ordinary occasions, I added to it some ferrocyanide of potassium, and immediately produced the blue precipitate. This solution had, however, a very slight yellow tint, and remembering that Professor Clarke had shown me that hydrochloric acid very much deepens the colour of perchloride of iron, I suspected its presence; I therefore prepared some chloride of antimony from hydrochloric acid, and a protoxide which I believed to be pure. I obtained a perfect colourless solution, and in this no blue precipitate was formed by ferrocyanide of potassium.

" To show that the chloride of antimony, which I first employed, contained peroxide of iron, I decomposed a portion of it by the addition of water; the solution gave a much deeper blue precipitate than before, while the oxychloride of antimony, re-dissolved in hydrochloric acid, gave scarcely any blue tint whatever; and the slight one which it did yield was evidently owing to the adhesion of a small portion of peroxide of iron precipitated with it; for, again precipitating with water and dissolving in hydrochloric acid, this minute portion of peroxide of iron was almost entirely removed.

" It is therefore evident, that the blue pigment is merely Prussian blue, largely diluted and rendered pale by ferrocyanide of antimony."

The author again adverts to the curious fact, already alluded to as pointed out to him by Professor Clarke, of the colour imparted to hydrochloric acid by perchloride of iron. A few drops of the perchloride were rendered perfectly colourless by half an ounce of water, while three ounces of colourless hydrochloric acid acquired a considerable yellow tint by the addition of a similar quantity of the perchloride.

On the Blue Pigment of Dr. Traill. By C. T. Coathupe.

Mr. Coathupe stated other experiments which confirm the conclusion of Mr. R. Phillips, that the colour in question could not be produced from pure chloride of antimony.

A new case of the Chemical Action of Light in the Decoloration of Recent Solutions of Caustic Potass of Commerce. By R. Mallet.

The author of this paper has examined chemically a large number of specimens of commercial caustic potass, with a view to determine the reality of the cause usually assigned to the deep green colour of its aqueous solutions, namely, the presence of manganese, in the state of manganesiate of potass. The colour of these solutions gradually fades in close or open vessels, and in light or darkness, an effect which has been ascribed, as above, to their containing mineral cameleon. On careful analysis, however, the author has assured himself that manganese does not occur as a constituent of caustic potass, and that its aqueous solutions owe their colour, and the change of their colour, to the presence of protoxide and proto-chloride of iron in solution; the former being held so by the presence of chloride of potassium. These, by taking up oxygen (probably from the air combined with the water), become further oxydized and gradually precipitate in combination, leaving the solution colourless, and giving rise to a new compound of sesquioxide and sesquichloride of iron, consisting, by the author's analysis, of 10 atoms of the former and one of the latter body, or

$$(Fe_2 \, Cl_3) + 10\,(Fe_2 \, O_3).$$

It is anhydrous. Having observed that these changes took place at very different rates in bottles of variously coloured glass, the author commenced a series of experiments on the relative effects of light transmitted to the solution through various media, and has found and recorded in ruled sheets, by curves, the results of observations made every two hours. In these curves, the ordinates represent time, and the abscissæ the rate of chemical change, as marked by certain changes of colour.

Means were taken to prevent inequality of temperature, or of intensity of light, in each solution exposed to a coloured ray, and some of the results arrived at are given in the subjoined table.

Mode of exposure to light.	Time of perfect decoloration.
Violet glass, exposed to air	30 hours.
Violet glass, closed	50 ..
Flint glass, colourless, exposed to air	80 ..
.. .. .. closed	115 ..
.. .. yellow „	170 ..
.. .. blue „	185 ..
.. .. orange „	190 ..
.. .. red „	200 ..
Green glass, by oxide copper	wholly unchanged in colour in 200 hours.
Do. Bristol bright metal	wholly unchanged in colour in 200 hours.

The rate of progressive decoloration for different rays at different periods is very various.

The result as to green glass agrees with Mrs. Somerville's experiments. Ink, which Sir John Herchel found transmitted white light unaltered, was used to reduce all the coloured media to the same illuminating power, and immersion in water to preserve the temperature constant.

The author conceives that this is the first attempt that has been made to reduce the phenomena of the chemical action of light to numerical registration, and suggested the importance of the subject, upon which so little was known, and in which the observed cases were so few.

Observations on the Constitution of the Commercial Carbonate of Ammonia. By Mr. Scanlan.

Having occasion some months ago to make a quantity of the "Solution of Sesquicarbonate of Ammonia," of the London Pharmacopœia, the author found, (without knowing that Dr. Dalton had done so before,) by pouring successively small portions of pure water on large quantities of the salt, that saturated solutions were obtained, successively decreasing in specific gravity, and smelling less and less of ammonia, till all the salt was dissolved. He agrees with Dr. Dalton in opinion, that the commercial carbonate of ammonia is not a homogeneous salt, not a sesquicarbonate of ammonia, but a mixture of carbonate and bicarbonate, of which the former is first dissolved by the water. The irregular masses of salt which remain still retain, almost exactly, their original form and dimensions—they are, in point of fact, skeletons of the original mass, but consist solely of a congeries of crystals of bicarbonate of ammonia, from the interstices of which carbonate of ammonia has been removed by the solvent power of the water, if we do not proceed so far as to dissolve all. What takes place here, may be likened, in some measure, to the case in which the gelatin is removed from bone by water, leaving the phosphate of lime. Independently of showing the true nature of the salt, this is of some importance, as it affords us a very ready mode of preparing bicarbonate of ammonia without the waste, which occurs by exposure of the commercial salt in powder to the air, or without the trouble of transmitting a current of carbonic acid gas through its solution, as directed by the Dublin Pharmacopœia. Indeed, the latter method is both troublesome and wasteful, for it is difficult to evaporate a solution of bicarbonate of ammonia without decomposition. Mr. Scanlan has found that water at 90° or 100° decomposes bicarbonate of ammonia, setting carbonic acid at liberty.

On the Blackening of Nitrate of Silver by Light. By Mr. Scanlan.

Nitrate of silver was recommended many years ago, by Dr. John Davy, as a test of the presence of organic matter in distilled waters. He showed, that if nitrate of silver in solution be added to perfectly pure water, it is not altered by exposure to direct sunshine; but if the water contain a trace of organic matter, it will become blackened. Mr. Fergusson, some years ago, when he had the management of the chemical laboratory belonging to the Dublin Apothecaries Company, informed Mr. Scanlan, that perfectly pure nitrate of silver is not blackened by long exposure to direct sunlight, but it is believed that he never gave further publicity to this fact than mentioning it to his chemical friends in Dublin at the time. In consequence of some observations upon the blackening of this salt, made by Dr. Aldridge, of Dublin, in his review of Mr. Phillips's translation of the London Pharmacopœia, Mr. Scanlan was led to make the following experiment upon the subject:—He took two cylinders of perfectly pure fused nitrate of silver, immediately they were cast, from the mould, and wrapped one of them in paper, in the usual way that this substance is found in the shops; the other cylinder was transferred to a glass tube, and sealed up hermetically, by means of the blow-pipe, without being suffered to come in contact with organic bodies: it was pushed from the mould into the tube by means of a glass rod. After a lapse of three days, the paper was removed from the first, and it was then sealed up in a tube, in a similar manner to the other. The two tubes were now exposed to the direct rays of the sun, and in half an hour the nitrate of silver that had lain in contact with paper was blackened, while that in the other tube was not altered by six weeks constant exposure. The whole amount of blackening of the cylinder that had been papered was produced in the half hour. Nitrate of silver, free from organized matter, is sometimes blackened by exposure to the air; but this may be owing to the presence perhaps of sulphuretted hydrogen, accidentally present. Atmospheric air too is, perhaps, seldom free from organic matter.

On the Specific Gravities of Nitrogen, Oxygen, Hydrogen, and Chlorine; and also of the Vapours of Carbon, Sulphur, Arsenic, and Phosphorus. By the Rev. T. Exley, *M.A.*

The author of this communication first shows the bearing of his new theory of physics on questions relating to the combination of atoms, which he classes according to their supposed absolute forces and relative spheres of repulsion. Among the consequences of this theory the author mentions the deduction, that "equal volumes contain equal numbers of atoms," and proceeds to show the application of this to the construction of a general table of specific gravities for chemical compounds in the gaseous form. Taking the specific gravity of air as unity, he shows, from comparison of the best experimental results, that the following specific gravities may be depended on within minute errors:—

Nitrogen . .	35–36ths.	Carbon* . . .	10–12ths.
Oxygen . .	10–9ths.	Sulphur† . . .	60–9ths.
Hydrogen . .	10–144ths.	Arsenic‡ . . .	$10\frac{1}{2}$.
Chlorine . .	$2\frac{1}{2}$.	Phosphorus § . .	$4\frac{4}{9}$.

Now these numbers, being compared with the atomic weights of the substances, lead to the following simple rule for determining specific gravities in all gaseous bodies:—Multiply the sum of the atomic weights of the atoms of a single group by $\frac{10}{144}$ (the specific gravity of hydrogen), the product is the specific gravity required. By this rule the following table was calculated:—

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* The number for carbon is inferred from its gaseous combinations.

† It is by calculation only $\frac{20}{9}$, but the author explains this difference by supposing the sulphur vapour to exist in single groups of three atoms each, and gives reasons for thinking this probable.

‡ By calculation $5\frac{1}{4}$; supposed to consist of single groups of *two* atoms each.

§ By calculation $2\frac{2}{9}$; supposed to consist of single groups of *two* atoms each.

A Table of Chemical Compounds in the Gaseous Form.

Name.	Composition.	Sp. gr. Hyd. = 1.	Sp. gr. By Cal.	Air = 1. By Expt.	Authority.
	I. COHESIVE COMBINATION.				
1. Carbonic Oxide	O | C	14	·9722	·9727	Berzelius.
2. Nitric Oxide	O | N	15	1·0426	1·0409	Davy; Thomson.
3. Muriatic Acid	Cl | H	$18\frac{1}{2}$	1·2847	1·2844	Thomson.
4. Hydrobromic Acid	Br | H	$40\frac{1}{2}$	2·8125	2·731	Turner.
5. Hydriodic Acid	I | H	$63\frac{1}{2}$	4·4097	4·443	Gay Lussac.
6. Fluoric Acid	F | H	$9\frac{1}{2}$	·6597		
7. Sulphuret of Mercury	S | Hg | Hg	$77\frac{1}{3}$	5·3703	5·384	Dumas.
8. Common Air	O | N | N | N | N	$14\frac{2}{5}$	1	1	The unit.
	II. SINGLE GROUPS.				
9. Cyanogen	NC or N (C_2) N	26	1·8055	1·8064	Gay Lussac.
10. Chloride of Sulphur	Cl S or Cl (S_2) Cl	68	4·7222	4·70	Dumas.
11. Chloride of Mercury	Cl Hg or Cl (Hg_2) Cl	136	9·4444	9·439	Mitscherlich.
12. Bromide of Mercury	Br Hg or Br (Hg_2) Br	180	12·5	12·362	Ditto.
13. Iodide of Mercury	IHg, or I (Hg_2) I	226	15·6944	15·67	Ditto.
14. Fluoboric Acid	FB or F (B_2) F	34	2·3611	2·3622	Thomson.
15. Nitrous Acid	O_2 N or N (O_4) N	46	3·1944	3·1764	Gay Lussac.
16. Peroxide of Hydrogen	HO, or H (O_2) H	17	1·1805	these are decomposed by a degree of heat below the boiling point.	
17. Persulphuret of Hydrogen	HS, or H (S_2) H	33	2·2916		
18. E. Davy's Carb. of Hydro.	HC, or H (C_2) H	13	·9027	·9027	E. Davy.
19. Faraday's Carb. of Hydro.	H_3 C_3	39	2·7083	2·776	Faraday.
20. Olefiant Gas	H_2C or H, C, H	14	·9722	·9709	Thomson.
21. Etherine	H_4 C_2	28	1·9444	1·91	Faraday.
22. Faraday's Oil Gas	H_6 C_3 or H_2 C, H_2 C, H_2 C	42	2·9166	3·0555	Ditto.
23. Naphtha	H_5 C_3 or H_2 C, HC, H_2 C	41	2·8472	2·833	Saussure.
24. Naphthaline	H_4 C_5	64	4·4444	4·528	Dumas.
25. Petrolene	H_8 C_{10}	128	8·8888	9·415	Boussingault.
26. Camphene	H_8 C_5	68	4·7222	4·767	Dumas.
27. Oil of Turpentine	H_8 C_6	80	5·5555	5·0130	Gay Lussac.
28. Hydrocarb. of Chlorine	H_2 Cl C, or H, Cl C, H	50	3·4722	3·4434	Ditto.
29. Arsenious Acid	As_2 O_3 or O, OAs_2, O	200	13·8888	13·6695	Mitscherlich.
30. Chloride of Silicon	Cl_2 Si, or Cl, Si, Cl	88	6·1111	5·939	Dumas.
31. Fluosilicic Acid	F_2 Si, or F, Si, F	52	3·6111	3·600	Thomson.
32. Alkarsin	H_6 C_2 As, or H_2 C, H_2 As, H_2 C	106	7·3611	6·516	Bunsen.
	III. DOUBLE GROUPS.				
33. Water	H (O) H	9	6·25	·6235	Gay Lussac.

Table continued.

Name.	Composition.	Sp. gr. Hyd.=1.	Sp. gr. By Cal.	Air = 1. By Expt.	Authority.
34. Sulphuretted Hydrogen...	H (S) H	17	1·1805	1·1912	Thenard.
35. Carburetted Hydrogen ...	H (C) H	7	·4861	·490	Dumas.
36. Telluretted Hydrogen......	H (Te) H	33	2·2916	2·292	Brande.
37. Selenuretted Hydrogen ...	H (Se) H	41	2·8472		
38. Inflble. Phosphtted. Hyd.	H (P) H	17	1·1805	1·1935	Rose.
39. Carbonic Acid...............	O (C) O	22	1·5277	1·523	Thenard.
40. Sulphurous Acid............	O (S) O	32	2·2222	2·221	Thomson.
41. Deutoxide of Chlorine ...	O (Cl) O	34	2·3611	2·346	Thenard.
42. Protoxide of Chlorine ...	Cl (O) Cl	44	3·0555	3·0	Gay Lussac.
43. Bichloride of Sulphur ...	Cl (S) Cl	52	3·6111	3·67	Dumas.
44. Bichloride of Carbon	Cl (C) Cl	42	2·9166		
45. Chloride of Manganese ...	Cl (Mn) Cl	64	4·4444		
46. Chloride of Selenium	Cl (Se) Cl	76	5·2777		
47. Borochloric Acid	Cl (B) Cl	44	3·0555	3·942	Dumas.
48. Chlorocyanic Acid	N (C) Cl	31	2·1527	2·153	Ditto.
49. Hydrocyanic Acid	N (C) H	13½	·9375	·9385	Thenard.
50. Bisulphuret of Carbon ...	S (C) S	38	2·6388	2·6447	Gay Lussac.
51. Nitrous Oxide...............	N (O) N	22	1·5277	1·5269	Thomson.
52. Protochloride of Mercury	Hg (Cl) Hg	118	8·1944	8·204	Mitscherlich.
53. Protobromide of Mercury	Hg (Br) Hg	140	9·7222	9·665	Ditto.
54. Bromide of Sulphur	B (S) B	96	6·6666		
55. Sulphuric Acid	O (SO) O	40	2·7777	3·0	Mitscherlich.
56. Selenic Acid	O (SeO) O	64	4·4444		
57. Phosphuretted Hydrogen	H (PH) H	17½	1·2152	1·214	Dumas.
58. Arsenuretted Hydrogen...	H (AsH) H	39½	2·7430	2·695	Ditto.
59. Ammonia.....................	H (NH) H	8½	·5902	·5967	Biot & Arago.
60. Chloride of Phosphorus...	Cl (PCl) Cl	70	4·8611	4·875	Dumas.
61. Chloride of Arsenic	Cl (As Cl) Cl	92	6·3888	6·295	Ditto.
62. Sesquichloride of Carbon	Cl (CCl) Cl	60	4·1666		
63. Chlorocarbonic Acid	Cl (CO) Cl	50	3·4722	3·472	Henry.
64. Light Carburetted Hydr.	H (CH_2) H	8	·5555	·5550	Thomson.
65. Perchloride of Tin	Cl (Sn Cl_2) Cl	130	9·0277	9·1997	Dumas.
66. Perchloride of Titanium...	Cl (Ti Cl_2) Cl	98	6·8055	6·836	Ditto.
67. Chloral	Cl (Cl C_2 HO) CL	74½	5·1736	5·0	Ditto.
68. Infl. Gas of Dr. Thomson	Cl (Cl CH_2) Cl	61	4·2361	4·1757	Thomson.
69. Chloroform	Cl (Cl CH) Cl	60½	4·2013	4·2	Dumas.
70. Muriatic Methyline	H (Cl CH) H	25½	1·7708	1·736	Ditto.
71. Sesquiodide of Arsenic ...	I (As_3) I	240	16·666	15·64	Mitscherlich.

Table continued.

Name.	Composition.	Sp. gr. Hyd.=1.	Sp. gr. By Cal.	Air = 1. By Expt.	Authority.
72. Nitric Acid	$N\ (O_5)\ N$	54	3·75	3·75	Henry.
73. Hyponitrous Acid	$N\ (O_3)\ N$	38	2·6388	2·6388	Inferred from No. 96.
74. Pyroxylic Spirit	$H\ (H_2\ OC)\ H$	16	1·1111	1·115	Brande.
75. Pyroacetic Spirit	$H_2\ C\ (H_2\ OC)\ H_2\ C$	29	2·0138	2·019	Dumas.
76. Protohydrate of Methyline	$H_2\ C\ (H_2\ C)\ H_2O$	23	1·5972	1·60	Dumas.
77. Sugar Anhydrous	$H_2\ C\ (O_2\ C)\ H_2\ C$	36	2·5		Solid.
78. Alcohol	$H_2\ C\ (H_2\ O)\ H_2\ C$	23	1·5972	1·6133	Gay Lussac.
79. Mercaptan	$H_2\ C\ (H_2\ S)\ H_2\ C$	31	2·1527	2·326	Bunsen.
80. Oil Gas	$H_2\ C\ (H_2\ C)\ H_2\ C$	21	1·4583	1·458	Henry.
81. Muriatic Ether	$H_2\ C\ (Cl\ H)\ H_2\ C$	32½	2·2569	2·219	Gay Lussac.
82. Hydriodic Ether	$H_2\ C\ (IH)\ H_2\ C$	77½	5·3819	5·4749	Ditto.
83. Aldehyd	$H_2\ C\ (O)\ H_2\ C$	22	1·5277	1·532	Liebig.
84. Acetic Acid	$H_2\ C\ (C_2\ O_3)\ H_2\ C$	50	3·4722	3·067	Dumas.
85. Ether	$H_4\ C_2\ (H_2\ O)\ H_4\ C_2$	37	2·5694	2·5860	Gay Lussac.
86. Citrene	$H_4\ C_2\ (C)\ H_4\ C_2$	34	2·3611	2·385	Dumas.
87. Oxalic Ether	$H_4\ C_2\ (H_2\ O_4\ C_2)\ H_4\ C_2$	73	5·0964	5·087	Ditto.
88. Carbonic Ether	$H_4\ C_2\ (H_2\ O_3\ C)\ H_4\ C_2$	59	4·0972	4·243	Ettling.
89. Œnanthic Ether	$H_4\ C_2\ (H_{28}\ O_3\ C_{14})\ H_4\ C_2$	150	10·4166	10·4769	Delachamps.
90. Benzoic Acid	$H_4\ C_5\ (OC_2)\ H_4\ C_5 \mid H_2\ C\ (CO_2)\ H_2\ C$	60	4·1666	4·27	Dumas.
91. Benzoic Acid, concentrat.	$H_4\ C_5\ (H_4\ C_5\ O_3)\ H_4\ C_5$	120	8·3333	8·352	Inferred from 85 and 95.
92. Paranaphthaline	$H_4\ C_5\ (H_4\ C_5)\ H_4\ C_5$	96	6·6666	6·741	Dumas.
93. Camphor	$H_8\ C_5\ (O)\ H_8\ C_5$	76	5·2777	5·29	Ditto.
94. Acetic Ether	$H_4\ C_2\ (H_2\ O)\ H_4\ C_2 \mid H_2\ C\ (C_2\ O_3)\ H_2\ C$	43½	3·0208	3·067	Ditto.
95. Benzoic Ether	$H_4\ C_2\ (H_2\ O)\ H_4\ C_2—H_4\ C_2\ (H_4\ O_3\ C_{11})\ H_4\ C_2$	78½	5·4513	5·419	Ditto.
96. Nitric Ether	$H_4\ C_2\ (H_2\ O)\ H_4\ C_2 \mid N\ (O_3)\ N$	37½	2·6041	2·627	Ditto.
97. Sulphur Vapour	S_3	96	6·6666	6·648	Mitscherlich.
98. Phosphorus Vapour	P_2	64	4·4444	4·327	Ditto.
99. Arsenic Vapour	As_2	152	10·5555	10·362	Ditto.

On Chemical Combinations produced in virtue of the presence of other bodies which still remain. By Rev. T. Exley, M.A.

Mr. Exley points out in this paper the application of his theory to the explanation of those cases in chemistry where powerful affinities between bodies are brought into activity by means of other substances which are present and continue to exercise the same energies. Berzelius attributes this to a peculiar force, which he calls *catalytic*, and Mr. Exley takes four examples to show how these phenomena receive an explanation in conformity with his general view of atomic centres of force.

The first case is that of the combination of hydrogen and oxygen, effected by the presence of clean platinum. "Since metals are simple bodies and dense solids, the theory recognises their atoms as having small spheres of repulsion, and those of platinum must be very small because of the great density of the metal: hence, the atmosphere of ethereal matter belonging to this metal is very dense, being more dense as the square of the radius of the sphere of repulsion is less and as the atomic weight is greater. For this reason the oxygen can approach extremely near to the atoms of platinum, and yet not combine with them by reason of that dense atmosphere; and the hydrogen being drawn close on them the reunion in question occurs, the generated steam escapes, heat is evolved, and the process advances till ignition is produced."

The author examines in succession three other cases of supposed peculiar actions, and explains them on the principles of his theory:

The conversion of starch into sugar by the action of a weak solution of sulphuric acid.

The conversion of sugar into alcohol by the action of barm.

The conversion of alcohol into ether by the action of sulphuric acid.

On an Improvement in the Manufacture of Iron, by the Application of Gas obtained from the decomposition of Water. By John Samuel Dawes, *of Bromford and Oldbury Ironworks, near Birmingham.*

Mr. Dawes has attempted to obtain a heating effect approaching in intensity to that which is produced by the oxy-hydrogen blow-pipe, but upon a scale sufficiently large to be available in the smelting of iron.

Hydrogen gas, in addition to its great inflammability, exhibits, when applied to the above purpose, a purifying property in a high degree, so that the most sulphurous materials may be used with advantage, and metal of good quality be produced. There can be little doubt (Mr. Dawes thinks) as to the value of hydrogen thus applied; the question would rather be, Can the gas be obtained at a sufficiently cheap rate so as to make the use of it profitable? This, he thinks, may now be confidently answered in the affirmative. The method of proceeding is as follows. Jets of steam are made to pass through red-hot cast-iron pipes filled with small coke or charcoal; (the riddlings from the coke hearth answer the purpose, and are of little value;) decomposition immediately takes place. The carbon of the coke combines with the oxygen of the steam, forming, in the first instance, carbonic acid, which, by passing on through a further portion of the red-hot carbon, is converted into carbonic oxide; the hydrogen gas, together with the oxide before-mentioned, is applied to the furnace by means of a jet inserted within the blast-pipe tongue; the pressure upon the gas, of course, being equal to that upon the blast. The pipes require to be replenished with the brays about every twelve hours, which is conveniently effected by means of a plug fitted to the top of each of them. At first some diffficulty arose from destruction of the pipes; but as the melting point of cast-iron is so much higher than the temperature required to decompose water, it was evident that the cause of the mischief lay in the construction of the heating furnace. This has been remedied, and the apparatus seems now to be very durable. The present one at Oldbury has been in operation for some months, and the pipes are apparently little the worse for wear. The quantity of fuel required to keep them hot is from twelve to fifteen cwt. of small coal for twelve hours; and as the steam is obtained from the engine-boilers, and the fireman of the hot-air apparatus has time enough to attend to it, the expense, with the exception of wear and tear, is a mere trifle. The wear and tear, in every probability, will be very moderate, and Mr. Dawes has sufficient reason to conclude that the cost will not be more than three or four shillings for every hundred thousand feet of gas, every foot of which is, of course, equivalent to a certain quantity of fuel. Various experiments have now shown not only that the quality of the iron is very much improved by this process, but that the producing power of the furnace, at the same time, has greatly increased. In conclusion, Mr. Dawes observes, that any advantages it may be found to possess in the smelting of iron must be equally valuable in the reduction of other metals.

On the Influence of Voltaic Combination on Chemical Action. *By Dr.* ANDREWS.

In dilute sulphuric acid, composed of one atom of the dry acid and eight atoms of water, the solution of distilled zinc is permanently accelerated, by connecting it with a plate of platina, immersed in the same liquid, so as to form a voltaic combination. In acid, containing seven atoms of water, the ordinary action is at first increased, and afterwards rather diminished by contact with platina. But when zinc is heated in acid, containing less than this quantity of water, the connexion with platina transfers the evolution of gas, from the surface of the positive to that of the negative metal, and at the same time diminishes its quantity, and consequently retards the rate of solution of the zinc. The formation of a galvanic circle exerts, therefore, a reverse effect on the solution of zinc in sulphuric acid containing more or less than seven atoms of water. The principal circumstances which influence these results are the adhesion of the hydrogen gas to the surface of the zinc; the formation of sulphate of zinc, which is greatly facilitated by the presence of seven atoms of water in union with each atom of acid (that being the number of atoms of water of crystallization contained in it); and, lastly, the proper action of the voltaic circle, which tends to diminish the solution of the zinc. In dilute acid, the first circumstance retards the action on the zinc alone, and the second facilitates its solution; then the platina surface enables the hydrogen to escape. But in the stronger acid, the voltaic association impedes the solution of the zinc, partly from the evolution of gas being transferred to the platina, and thus the saturated liquid being allowed to accumulate around the zinc plate, and partly from the real effect of the galvanic combination. That the proper tendency of a voltaic circle is, to diminish the chemical action of the solution on the electro-positive metal, the author endeavoured to show, from the consideration, that in ordinary solution the electricities thus developed have only an indefinitely small portion of liquid to traverse, while in voltaic solution their reunion can only be effected by passing across a column of variable extent, and composed of an imperfectly conducting substance. And, as the action is greater the nearer the plates are to each other, that action ought to attain a maximum when the distance between the plates vanishes, provided this condition could actually be realized.

On the Construction of Apparatus for solidifying Carbonic Acid, and on the Elastic Force of Carbonic Acid Gas in contact with the liquid form of the Acid, at different Temperatures. By ROBERT ADDAMS.

Mr. Addams prefaced the communication by adverting to the original production of liquid carbonic acid by Dr. Faraday, in 1823, and also to the solidification of the acid by M. Thilorier, and then exhibited three kinds of instruments which he (Mr. Addams) had employed for the reduction of the gas into the liquid and solid forms. The first mode was mechanical, in which powerful hydraulic pumps were used to force gas from one vessel into a second, by filling the first with water, saline solutions, oil, or mercury; and in this apparatus a "*gauge of observation*" was attached, in order to see when the vessel was filled. The second kind of apparatus is a modification of that invented and used by Thilorier. The third includes the mechanical and the chemical methods, and by which, as stated, a saving of a large quantity of acid formed in the generator is effected; whereas by the arrangements of Thilorier's plan, two parts in three are suffered to rush into the atmosphere, and are lost. With this set of instruments are used two gauges of observation,—one to show when the generator is filled with water by the pumps, and consequently all the free carbonic acid forced into the receiver; and the other to determine the quantity of liquid acid in the receiver. Mr. Addams likewise exhibited other instruments for drawing off and distilling liquid carbonic acid from one vessel into another, and mentioned some experiments which were in progress, and especially the action of potassium on liquid carbonic acid,—an action which indicated no decomposition of the real acid, but such as implied the presence of water, or a hydrous acid. A table of the elastic force or tension of the gas, over the liquid carbonic acid, was shown, for each ten degrees of the thermometer, beginning at zero, and terminating with 150 degrees. The following are some of the results:—

Degrees.	lb. per sq. inch.	Atmospheres of 15lb. each.
0	279·9	18·06
10	300·	20·
30	398·1	26·54
32	413·4	27·56
50	520·05	34·67
100	934·8	62·32
150	1495·65	99·71

Mr. Addams announced his intention of examining the pressure at higher temperatures, up to that of boiling water, and above; and asserted his belief that carbonic acid may be profitably employed as an agent of motion—a substitute for steam,—not directly, as had been already tried by Mr. Brunel, but indirectly, and as a means to circulate or reciprocate other fluids. The solidification of the acid was shown, and the freezing of pounds of mercury in a few minutes, by the cooling influence which the solid acid exercises in passing again to the gaseous state.

On a New Process for Tanning. By William Herapath.

The author, after noticing the impediments to a perfect accomplishment of the three great objects of the tanner, viz. to make the skins of animals *insoluble*, *imputrescible*, and *impermeable* to water, describes a new process, by which these objects are accomplished at a less cost than that of the old methods. In the ordinary process, the harder fibres of the skin are perfectly tanned; but the gelatine, which, if retained, would be of the greatest value in rendering leather impermeable to water, disappears from the product, or is seen as an injurious yellow coat on the surface. In reasoning on the difficulties experienced by tanners, Mr. Herapath was brought to the conclusion that they were occasioned by the force of capillary attraction; and finding that the ordinary modes of applying *pressure*, *handling*, &c. were expensive and insufficient, he determined to try the effect of rolling the hides, connected together as an endless band.

"If I wish to tan one hundred hides a week, I should have eight pits, over each of which would be affixed a pair of rollers; the upper ones to be loaded by weights fixed on their levers. For each pit fifty hides or butts would be made into an endless band by ligatures of twine; upon introducing each band between a pair of rollers, each hide would be in succession pulled from the bottom of the pit, squeezed by the rollers, and then returned again into the pit for a fresh supply of tanning liquor; such liquor becomes exhausted, about two degrees of the barktrometer, in twenty-four hours, when it is pumped to the next pit backwards of the series; and, by the time it arrives at the last pit, (eight days,) it will have lost from 16 to 20 degrees by the same instrument. The eight pairs of rollers require one-horse power to work them, and two boys of 2*s.* 6*d.* a week each to superintend them; when two bands or one hundred hides will be taken off weekly, as one month is sufficient for them to be on the rollers. They are now laid by in strong solutions for *another month, when they are found to be completely tanned*; weighing 10 per cent. more than if they had been operated upon in the old way, while the leather has more soft elasticity, and is ten times more impervious to water.

"The levers are loaded according to the state of tannage, and the liquors are changed once a day, making twenty-four changes in the whole, which is about one-fourth of the number formerly used."

The author entered into a minute statement of facts to prove the practical advantages of the process thus briefly described, in respect of cheapness, expedition, quick return of capital, and quality of the product.

On some New Salts of Mercury. By WILLIAM WEST, *Leeds.*

This paper describes the composition and properties of some salts, composed of bicyanide of mercury, with the haloid salts of potassium.

As these salts had, without Mr. West's knowledge, been previously formed by other chemists, it is unnecessary to detail his experiments, which confirm former researches as to their composition. The author observed that the pearly lustre of some of these crystals, especially those from the bromide, was such that they might probably be employed in place of the scales of the Bleak, for the manufacture of artificial pearls.

On a New Compound of Carbon and Hydrogen. By WILLIAM MAUGHAM, *Lecturer on Chemistry at the Royal Gallery of Practical Science, London.*

The compound in question is produced when the electrodes of a voltaic battery are armed with charcoal points, and these points introduced into a vessel of distilled water. The points should be attached to the copper-wire electrodes by means of platinum wires. On bringing the charcoal points together under the water, so as to produce the electrical spark with as little interruption as possible, the water undergoes decomposition, carbonic oxide is produced, and a compound, not previously noticed, consisting of carbon and hydrogen, is at the same time formed. Neither hydrogen nor oxygen gases are obtained as happens when the action is electrolytic.

The compound under consideration is of an oily nature; it imparts a very peculiar and unpleasant odour to the water, which becomes impregnated with it as it is formed; when kept for some time the liquid loses its odour, and there is a precipitate of carbonaceous matter. This spontaneous change takes place whether the liquid be exposed to the air or kept in a stoppered phial.

On a Mode of obtaining an Increase of Atmospheric Pressure, and on an Attempt to liquefy Hydrogen and Oxygen Gases, with accompanying Apparatus. By WILLIAM MAUGHAM, *Lecturer on Chemistry, Royal Gallery of Practical Science, London.*

The apparatus consists of a strong glass tube, bent in the form of the letter U, having a platinum wire attached to a brass cap passing into each leg. The glass tube is ground at each end, and the ground surface of the brass cap is held down by means of screws, a collar of leather being interposed to make the whole air-tight. Water being introduced into the tube, and the tube closed, the wires are to be attached to the electrodes of a sustaining battery. The water then undergoes decomposition, and the oxygen and hydrogen gases evolved are retained in the tube, the pressure on the gases being increased in proportion to the time the action is going on.

The pressure thus obtained may be carried to such an extent as to burst the strongest glass tubes that have yet been employed in the experiment.

By making the experiment with the assistance of cold, the author of the paper anticipates that both hydrogen and oxygen may be liquefied.

The tube is not to be completely filled with water, and it will be necessary to have the water slightly acidulated with sulphuric acid.

Should we not be able to succeed in liquefying hydrogen, oxygen, &c., we have, nevertheless, a mode of obtaining increased gaseous pressure, which, with a slight modification of the apparatus, that will readily suggest itself to those experienced in manipulation, will enable us to liquefy those bodies which pass from the aëriform to the liquid state at comparatively low pressures. The same process may also be rendered available for other purposes, where increased pressure becomes requisite.

On the Water of the Dead Sea. By JOHN MURRAY, *F.L.S. &c.*

Having had an opportunity of examining chemically the waters of the Dead Sea, the author discovered several substances which he supposes to have escaped the attention of those chemists who have already submitted them to analysis. He described the effect of many re-agents in testing the constitution of the water, which exhibited, after repose, strong signs of sulphuretted hydrogen. There was no trace of iron. In addition to lime, magnesia, sulphur, &c., the substances which the author supposes himself to have been the first to detect in this celebrated water are, iodine, boracic acid, ammonia, silica, selenium. He also found bromine. The waters of the Jordan are strongly contrasted with those of the Dead Sea; for they yielded to Mr. Murray only minute traces of lime and magnesia and muriate of soda.

Observations and Experiments made upon an Instrument termed a Magnet-Electrometer. By Lieut. Morrison, *R.N.*

In this communication Lieut. Morrison repeats the assurance that in the instrument of his invention the magnet deflects to the east when the air is *positively* electrified, and to the west when *negatively* electrified.

He also states the results of some examinations of the action of the instrument, made to determine the validity of an opinion which had been advanced, that the deflections in this instrument depend on the hygrometric state of the suspending string. The author declares that the same phenomena happen when a fine silver wire is used instead of the string.

On the Production of Crystals of Silver. By Thos. E. Blackwall.

Having observed nearly two years since in his writing-desk, some small crystals of silver which were firmly fixed upon a green substance like malachite, the author inferred, from seeing also a piece of brass in his desk, which fitted to the green mass, that there had been galvanic action generated by the zinc and copper of the brass, and that by its influence the nitrate of silver had been decomposed, and thus the silver crystals formed.

He describes experiments which he had instituted to test the truth of this view, and mentions the production of very similar crystals by the aid of corresponding metallic combinations.

GEOLOGY.

Observations on the Newcastle Coal-field. By John Buddle, *F.G.S.*

This elaborate memoir, comprising a detailed account of the most interesting phenomena of the coal formation round Newcastle, was divided into several portions, entitled,

Observations on the Newcastle coal-field.
Strata of the Newcastle coal-field.
Description of the sections of the seams of coal.
Foreign substances in coal.
Hitches and troubles.
Dykes and faults.

Numerous coloured sections, drawn to a large scale, and geological maps, were exhibited in illustration of the statements in the memoir. It has been found impracticable, without the aid of engravings from these beautiful and valuable drawings, to convey any proper notion, within moderate limits, of the mass of curious facts made known by this communication. It appears, indeed, the less necessary to attempt an analysis of Mr. Buddle's Essay, since it is understood that the author proposes to communicate it to the Natural History Society of Northumberland, Durham, and Newcastle on-Tyne, to whose Transactions he has previously consigned several important sections of the strata of the Newcastle coal-field. The following passages are extracted from the first section of the memoir.

"On referring to the line of the crop of the coal, and the line of the axis of the general dip of the strata, it would appear that this field of coal, so far as it has hitherto been explored, although traversed by various undulations and large faults, forms only a portion of an immense trough or basin, the south-western, western, and northern margins of which we have yet only been able to trace. But as the strata, so far as they have yet been explored, in the line of the dip under the magnesian limestone, are conformable, there is reason to conclude that the seams of coal extend far under the German Ocean before they rise at the opposite margin of the basin, if that should be their form, or are cut off by the extension of the magnesian limestone.

"A great many seams of coal are found in this extensive district, but they differ in number, character, quality, and thickness, in its several portions, and it is seldom that more than five of workable thickness co-exist, and it frequently happens that not more than one or two occur in the same locality. In Monkwearmouth colliery, for example, we find 31 seams of coal sunk through in a depth of 264 fathoms 4 feet 9 inches, containing an aggregate thickness of 47 feet 2 inches of coal, (including the foreign substances with which the several seams are interstratified,) only one of which has yet been found of workable thickness and merchantable quality. Those seams vary in thickness from an inch and a half to 6 feet 2 inches and a half. In Backworth Colliery 283 beds have been sunk and bored through within a depth of 206 fathoms 0 feet 11 inches from the surface, comprising 45 seams of coal, of the aggregate thickness of 60 feet 1 inch, including the foreign substances with which the coal is interstratified. Of these seams only two or three can be considered of workable thickness at the present era."

It appears that the coal seams of the Newcastle district are very variable in respect of the presence and thickness of interstratified shales and sandstone called "bands." The most remarkable of these lies in the High Main coal, and is called the "Heworth Band," from the place of its first discovery. The direction of its Northern edge is about N. 80° E. by compass. From Felling Colliery, towards the N. E., it traverses Walker, Hebburn, Bewick, Percy Main, and Collingwood collieries, to the outcrop of the seam near North Shields; and in the S.W. direction from Felling Colliery, it passes through Sheriff

Hill, Team, Urpeth, Stanley, South Moor, and Lanchester Common Collieries, probably to the outcrop of the coal in that direction.

"This band first shows itself as a mere parting in the coal; generally at about 10 inches above the Black-band, which is incidental to the seam. By almost imperceptible degrees it goes on increasing till it reaches the thickness of 3 inches, and becomes a confirmed slate band of a dark gray colour. A little before it attains this thickness, a three-inch layer of coarse 'brassy coal,' (coal mixed with iron pyrites), appears at the bottom of the under division of the seam, separating it from the bottom coal; and it is worthy of remark, that this layer of 'brassy' coal almost invariably accompanies the Heworth Band. From 3 inches in thickness the band goes on thickening more rapidly to 12 inches, after which it goes on in a still more rapid ratio to 10 and 12 feet; finally dividing and destroying the seam as it goes southward."

"When the band approaches the thickness of 12 inches, it changes to a much lighter hue, and increases in hardness; and as it goes on thickening it becomes arenaceous, and finally passes into a stratum of sandstone, 7 fathoms thick, in one of the pits of Washington Colliery, while in another of the pits of the same colliery, it forms a variety of beds of sandstone and gray and black metal stone."

Considering the extent of the Newcastle coal-field, but few whin dykes occur, as only three or four of any considerable magnitude have yet been discovered. These are the Coaly Hill, the Hamsterly Common or Hett Dyke, the Cockfield Fell Dyke, and the Acklington Dyke. The first of these dykes is the subject of a notice of Mr. Buddle in the Transactions of the Natural History Society of Northumberland, Durham, and Newcastle on Tyne, (vol. i.,) and it is remarkable for its undulatory character and its limited vertical depth. In fact, levels have been driven across the presumed plane of its fissure, both above and below the really existing vertical mass of whin-rock; of this Mr. Buddle furnishes ample proof from colliery workings, which also disclose the curious vertical divisions which exist in the dyke.

The dislocations of the strata called 'slip dykes,' or 'faults,' are infinitely more numerous than whin dykes in the great coal-fields of the Tyne and Wear. All the principal faults and whin dykes were represented by Mr. Buddle on a map, and minutely described in the paper from documents of the most undoubted accuracy. Accurate sections illustrating these phenomena, were drawn on a magnificent scale, and have been copied in a reduced form for publication along with the original memoir, which is expected to appear in the Newcastle Transactions, already rich in contributions to the geology of the coal formation of the Tyne and Wear, from the stores gathered in Mr. Buddle's extensive mining experience.

On the Berwick and North Durham Coal Fields. *By* D. Milne, *F.R.S.E.*

The strata of the Berwick and North Durham coal field consist of sandstones, limestones, coal, and the strata usually existing in all coal fields. They underlie the millstone grit rocks which crop out at Alemouth, and they overlie thick beds of a red conglomerate, accompanied by slaty red sandstones, which rest on the Lammermuir Hills towards the north, and on the Cheviot towards the west. There are altogether fourteen beds of workable coal, the thickest of which contain about six feet of pure coal. There are seven beds of marine limestone, each on an average fifteen feet thick. This coal field is intersected by four greenstone dykes, all of which run in a direction E. and W., and all of which become thinner towards the west; two of them run severally about eight miles. The Kyloe hills consist of greenstone, which is stratified, and forms part of the whin sill that runs through Northumberland. The strata are in the form of a basin, having been elevated on two sides by the porphyry of Lamberton and Cheviot hills. But the phenomena of the district afford clear evidence that there have been two periods when the porphyry was ejected, one of these periods being before the deposition of the stratified rocks, and the other after the deposition. This evidence is afforded *by the conglomerate* under the coal-measures, which in many places contains fragments of the Lamberton and Cheviot porphyries,—*and by the verticality* of these coal-measures in other places where they are in contact with the porphyry. The slips or dislocations caused by these convulsions were pointed out and described, with reference to a map and sections. The direction of these slips was stated to be generally coincident with the dip and rise of the strata, so that where the strata dip the same way continuously over a great extent of country, they are all parallel; and where they are in the form of a basin, they converge to the trough of it.

It was mentioned, that organic remains of various kinds were found in the strata of the district. Remains of fish, and of the same species that occur in the Lothian coal fields, viz. the Megalichthys and the Gyracanthus, occur in an impure limestone that forms the roof of the lowest workable coal, which limestone contains also terrestrial plants and bivalve shells, resembling the Sanguinolaria. Lower down in the series, and near what was probably the shore of the sea in which these strata were deposited, the shales and sandstones exhibit broken fragments of Coniferæ and other plants having *Serpulæ* and *Modiolæ* attached to them. The workable beds of limestone are filled with all the marine shells usually characteristic of the carboniferous limestones.

The superficial deposits consist of bowlder clay which immediately covers the rocks, and is filled with blocks of grauwacke, basalt, and granite, clearly showing that it has come from the westward. This bowlder clay is covered by sand, which in some places is sixty feet deep and is continuous for many miles. Over this lies fine brick clay; and above the sand is a covering of gravel. It would appear from this, that a sea had probably existed in the district, at the bottom of which the bowlder clay, by some violent cause, had been spread, that a long period of tranquillity thereafter prevailed, when at length the sea retired, whereby gravel was spread over its bottom, and the existing valleys (which are all east and west in direction) were scooped out.

On the Red Sandstone of the Tweed and Carlisle. By NICHOLAS WOOD, *F.G.S.*

The author, referring to his memoir in the Transactions of the Natural History Society of Newcastle, described the inclined beds of red sandstone which rise out from under the mountain-limestone series on the sea coast, about a mile south of the Tweed. He stated his belief that these beds flatten towards the west, so as to form the great deposit of red sandstone of the Tweed, and supported this opinion by a section from Berwick to the porphyritic hills of the Cheviot range near Barmouth. In the line of this section the relation of the red sandstone in question to subjacent coal-beds and overlying shales of the limestone series is clearly seen, and the flattening of the strata previously alluded to is witnessed at many points. The most conclusive evidence on this subject is obtained at the coal-workings on the south side of the Tweed. Near their junction with the porphyritic rocks south of Barmouth, the red sandstones assume inclined, vertical, or even reversed positions.—From all his inquiries Mr. Wood infers that the beds of red sandstone of the Tweed are referable to the series lying immediately below the mountain limestone and reposing upon the old red sandstone.

The second part of the paper was illustrated by a section on the line of the north side of the 'Great Dyke' from the sea-side at Cullercoats near Newcastle to Croglin-fell in Cumberland, showing the position of the detached western coal-fields of Stublick, Hartley Burn, Midgeholme, &c. From Midgeholme the strata rise rapidly west, so that the limestone rocks come to the day, and one included coal seam is worked in Tindal-fell, and at Talkin, and crops out on the escarpment of Croglin-fell. Below this coal-bed appears a series of limestones, the 'whin sill,' a second layer of basalt, limestones and sandstones, and, in some of the deep ravines, beds of red sandstone lying underneath the limestones.

The author compares with this mighty escarpment of the carboniferous limestone, thus based on red sandstone, the analogous and probably contemporaneous section of Tweeddale, and further declares his conviction that the vale of the Eden as well as the vale of the Tweed rests on red sandstones, which rise from beneath the escarpments of limestone. In support of this opinion he states that the red sandstones of Cumberland and the Tweed are very similar, and that they occupy precisely the same relative geographical position to the series of neighbouring mountain limestones.

The coal of Sanquhar and Cannoby was noticed in connexion with this subject; and regarding the latter, the author affirms it to be worked under limestones and red sandstones, and that extensive beds of red sandstone overlie this coal formation, and stretch from thence to the Solway Firth, while the coal strata are cut off on the north against the transition rocks. Mr. Wood entertains little doubt that the coal series of Dumfries-shire is to be placed on the same parallel as that of Berwickshire.

The opinions thus advanced and supported were compared with the statements of other geologists, who have generally adopted with regard to one of the points discussed a different inference, as the sandstone of the Eden and the plain of Carlisle is by most writers ranked with new red sandstone.

An Account of Rolled Stones found in the main Coal Seam of Cockfield Fell Colliery. By H. T. M. WITHAM, *F.G.S.*

Specimens of rolled stones, and a fragment of quartz, were exhibited and Mr. Witham stated them to have been found in the main coal seam of Cockfield-fell colliery, in a portion of which they are of frequent occurrence. This portion is comprised in about 3 acres on the north side of the trap dyke, which does not seem to have influenced the position of the stones, as these are found in many instances at a distance of 400 yards from it, and occasionally at greater distances. Similar specimens have been also met with on the south of the dyke as far as the outcrop of the coal. In the coal which is altered by the dyke for about 25 yards on each side, only one solitary specimen has been found, though they are abundant in the solid coal adjacent. A specimen has been also found at St. Helen's Colliery, two miles to the north-east.

On Sections of the Mountain Limestone Formation in Alston Moor, exhibiting the general uniformity of the several beds. By T. SOPWITH, *F.G.S.*

Mr. Sopwith stated that these sections form a portion of illustrations of the stratification across the island from the German Ocean at Sunderland to the Irish Sea at Whitehaven, which could not be fully completed in time for the present Meeting, but are now in progress for a subsequent meeting of the British Association. This series of sections will comprise the coal district of the county of Durham, by Mr. Buddle; the lead mine district, by Mr. Sopwith; the Cumbrian group of mountains, by Professor Sedgwick; and the Whitehaven coal field, by Mr. Williamson Peile. The sections exhibited showed the succession and relative thickness of the several strata, and comprised comparative sections of the strata in the manor of Alston Moor, and of the workings of several mines.

On the Position of the Rocks along the South Boundary of the Penine Chain. By J. B. JUKES, *F.G.S.*

In this communication the calcareous strata of Derbyshire, which form the lower part of the whole Penine chain of mountains, are described at length; the superincumbent limestone shale and millstone grit are traced; and the coal formation of Derbyshire is noticed both as to its mineral composition and relations to the new red sandstone series above. The memoir includes also a description of the Pottery coal-field, which fills a long trough from Biddulph to Lane End, on a synclinal axis from north to south.

After a minute examination of many cases of disturbed stratification, the author states that, upon the most general view of the position of the carboniferous system, in this district, it is perceived to be one great arched elevation on an axis from north to south, having also a gradual slope to the south. This great elevation is made up of many minor undulations, the higher parts of the middle of the arch being stripped off, and the inferior beds exposed to view. On every side, as soon as the beds descend to a sufficiently low level, they are masked from further observation by beds of the new red sandstone.

Mr. Jukes describes minutely the proofs of the gradual attenuation of the magnesian limestone in its course to the South near Nottingham, till it dwindles to one yard in thickness, and is lost under the westward range of red and white sandstone which borders the Nottinghamshire and Derbyshire coal-field on its south side.

Connected with this superposition of the red sandstones on coal, (themselves covered by the red and white clays which constitute the upper part of the red formation) is a point of great practical importance, viz. the extent to which coal may be reasonably looked for beneath these red rocks.

Among other evidence bearing on this question Mr. Jukes describes the narrow extensions and peculiar aspect of the red sandstone in the vicinity of Ashbourne; notices the physical configuration of the country along the junction line of the red formation and the coal strata, which in places indicates a great depression of the latter, and the production of valleys in it anterior to the deposition of new red sandstone. He states, finally, that it is probable that a large part of South Derbyshire and the adjacent district is composed of the rocks belonging to the lower part of the carboniferous system, covered by the new red sandstone; that the north point of the Leicestershire coal-field must be looked upon as the connecting link between the coal-fields of Derbyshire on the one hand, and of the north of Staffordshire on the other; that the present break between them was caused in part by denuding forces acting before the new red sandstone period; and, consequently, that any mining operations in the south of Derbyshire in search of coal are unlikely to be attended with success.

On the Silurian System of Strata. By R. I. Murchison, *F.R.S., G.S., &c.*

Mr. Murchison exhibited to the Section the finished geological map, plates, and sections, prepared in illustration of his work on the Silurian System, and described the principles on which the map and the other illustrations were constructed and coloured. He also noticed the probable extent of this system of strata in the British Islands, on the continent of Europe, and other foreign localities.

On the Geological Structure of the South of Ireland.
By R. Griffith, *F.G.S.*

Mr. Griffith exhibited his new Geological Map of Ireland, which had been constructed at the Ordnance Survey Office, Dublin, by Lieutenant Larcom, under the direction of Colonel Colby, of the Royal Engineers.

Having briefly alluded to the geology of Ireland generally, and the principle which had been adopted in colouring the map, Mr. Griffith proceeded to illustrate the geological structure of the southern counties by the description of the sections, one of which commenced in the granite district of Mount Leinster in the county of Carlow, and extended in a line nearly parallel to the south-east coast through the counties of Kilkenny, Waterford, and Cork, crossing the valley of the river Suir, at Carrick-on-Suir, passing over the summit of the Monavullagh mountains in the county of Waterford, crossing the valleys of the river Blackwater at Lismore, the Bride at Tallow, and the Lee at Cork Harbour, and terminated at Cork Head on the south coast of the county of Cork.

The second section extended from Brandon Head in the county of Kerry in a south-eastern direction, crossing the summits of Cahirconree and Carrawntoohil mountains, the valleys of Kenmare and Bantry, and terminated at Gally Head, also on the south coast of the county of Cork.

According to Mr. Griffith's views, the structure of the south-east of Ireland, commencing at Mount Leinster in the county of Carlow, and following the order of superposition, consists of a base of granite, on which rest strata of a rock intermediate between mica slate and clay slate. To the south and west of Killeen Hill, clay slate extends to the base of the conglomerate ridge of the county of Kilkenny, where at Coolnahay Hill, in the line of section, beds of coarse-grained red conglomerate, composed of rounded fragments of quartz, cemented by a brown or reddish brown arenaceous paste, are observed to rest *unconformably* on the clay slate, which dips to the south at an angle of about 60° from the horizon, while the conglomerate beds dip nearly to the same point at an angle of about 20° from the horizon. These conglomerate beds are identical in position and composition with those which underlie the carboniferous limestone to the north of Hook Head, and in other parts of the county of Wexford, which are universally admitted to belong to the old red sandstone series, and, like those of Coolnahay Hill, rest unconformably on old clay slate.

The thickness of the conglomerate at Coolnahay may be about 300 feet. It is succeeded by beds of coarse-grained quartzose slate, and occasionally of red quartz rock.

The same succession may be traced as far as the river Suir, where the red beds are succeeded in a conformable position by strata of yellowish white arenaceous quartz rock, and these again by beds of greenish gray imperfect clay slate, which latter rock alternates with the lower beds of the carboniferous limestone of the valley of the river Suir, In this valley the strata of the carboniferous limestone form a synclinal depression, the beds to the north of the river dipping south, and those lying south of it dipping to the north.

On the south side of the Suir, the strata which have already been described are observed resting in the same order of succession on the extensive clay-slate district of the county of Waterford, in which are situated the important slate quarries of Glenpatrick and other localities.

Proceeding to the southward from the clay-slate district of Waterford, the line of section crosses the summit of the Monavullagh mountains, which consist of a vast accumulation of conglomerate similar to that already described, and forming an escarpment nearly perpendicular for an elevation of about 500 feet; the strata dip to the south at an angle of 10° from the horizon. As in the cases mentioned before, this conglomerate is succeeded by coarse red slate and quartz rock, but in descending towards the river Blackwater, near to Lismore, we find beds of roofing slate interstratified with the quartz rock, and it is to be observed generally, that the roofing slate occurs only in the upper portion of the red slate series.

Approaching the Blackwater, the clay slate is succeeded in a conformable position by yellowish white sandstone and sandstone slate, which in many localities is found to contain casts of *Calamites*, and apparently of some varieties of *Stigmaria*, and these again, as in the valley of the river Suir, by the greenish gray imperfect clay slate, which alternates with the limestone of the valley of the river Blackwater. This valley, like that of the Suir, is connected with the admitted carboniferous limestone district of the counties of Cork, Tippêrary, &c.

The whole of the limestone beds of the river Blackwater at Lismore, dip to the south, but not at equal angles from the horizon; on the north side of the valley the angle of inclination does not exceed 20°, while in the middle and southern side it amounts to 80° or 85°, but still the inclination is to the south.

Proceeding to the southward beyond the limestone boundary, we find greenish gray clay slate and yellowish sandstone similar to those already described on the north side, inclining to the south at an angle of about 85° from the horizon. Judging from the position of the strata alone, these schistose and arenaceous beds might be supposed to be superior, instead of inferior to the limestone. But arguing from the analogy afforded by other localities in the south of Ireland, there can be no hesitation in admitting that the strata are here contorted.

Contortions are frequently observable on the sea coast and in many precipices and quarries in the interior of the country, and although, when seen at the surface, the strata everywhere dip towards the south, still these strata present a series of convolutions, frequently on a small scale, both sides of which incline to the southward, though usually at different angles. This peculiarity is general throughout the southern counties, and is alike observable in the transition slate, the limestone series and the culmiferous strata,—a circumstance which shows the necessity of extreme caution in making calculations as to the probable thickness of any formation, founded solely on the persistence of the dip of the strata towards any particular point.

To the south of Lismore a low ridge intervenes between the valleys of the river Blackwater and the river Bride at Tallow. This ridge is composed of coarse red slate, and occasionally rather fine-grained greenish gray clay slate. The strata for the most part dip to the south, but in the centre of the ridge they form an anticlinal axis. Approaching the valley of the Bride at Tallow, we again meet with yellowish white sandstone beds containing *Calamites* similar to those of the valley of the Blackwater, and also greenish gray imperfect slate, which, as before, is succeeded by the limestone; here the calcareous strata form a regular trough, those on the north side dipping to the south, and on the south side to the north, beyond which we have the usual succession of strata which are interposed between the red schistose beds and the limestone.

Proceeding to the southward, the section crosses the barony of Barrymore in the county of Cork, which forms the base of the limestone trough of the valley of Middleton and Youghal, and thence continues to its southern termination at Cork Head. Within this space a succession of strata similar to that already described, is repeated three times; first we have the red quartzose slate ridge of the barony of Barrymore, succeeded by the limestone trough of the valley of Middleton and Youghal; next the low red quartzose ridge of Great Island in the harbour of Cork, succeeded by the limestone of that harbour, Carrigaline, &c.; and lastly, the red quartzose ridge of Hoddersfield, which is succeeded on the south side by the blackish gray carboniferous slate which forms so characteristic a feature along the south coast of the county of Cork. This blackish gray slate appears to be similar to the greenish gray slate of the valleys of the Suir, the Blackwater, and the Bride; it underlies the limestone of the valleys of Middleton and of Cork Harbour, where it contains small *Orthocerata* in great abundance, and in some localities it contains *Calamites*. Approaching the limestone of Cork Harbour at Rosslillan, Renniskeddy, &c. the slate assumes a gray colour, is interstratified with limestone, and contains numerous fossils belonging to the carboniferous series, identical with those which occur in a similar position at Killinamack, in the county of Waterford, close to Knocklofty Bridge, over the river Suir. On the evidence of the sections thus briefly described, Mr. Griffith grounds his conclusion, that the limestones of the valley of the Bride, Cork Harbour, &c. belong to the same geological series as those of the Blackwater and the Suir, which are connected with the great carboniferous limestone field of Ireland, and this inference from the observed position of the rocks is stated to be confirmed by the evidence at present collected from organic remains.

In respect to the section of the strata near the western coast of the counties of Kerry and Cork, already mentioned, which was also exhibited by Mr. Griffith, similar proofs respecting the order of superposition of the strata were brought forward to show that the limestone of Killarney, Kenmare, and Bantry belongs to the carboniferous, and not to the transition series; and also that the red conglomerate beds of Cahirconree, and Carrantoohill mountains, together with the coarse red slate of which Tornies and Glena mountains at Killarney are composed, belong to the old red sandstone series.

It is to be observed, that the schistose strata belonging to the secondary formations of the south of Ireland are much more compact than those of the northern districts, and hence we find the quartzy slates and sandstones of the old red sandstone series have assumed the form of coarse clay slates and quartzy rocks; and also the dark gray carboniferous slates of the south of the county of Cork, which contain *Orthocerata*, *Calamites*, &c., have assumed the character and fissile structure of ordinary roofing clay slate; and several extensive slate quarries have been opened in different parts of the district, but these slates are not found to be of a durable nature.

On a small Tract of Silurian Rocks in the County of Tyrone. By Captain Portlock, *F.G.S.*

Captain Portlock remarked that he first recorded the existence in the County of Tyrone of rocks of the Silurian system, in the 1st volume of the Ordnance Survey Memoir of the County of Derry, and that he considers this to be the first authenticated case of their occurrence in Ireland, though there is little doubt that they also exist in Kerry and other counties. The tract in question is small, extending only over a few miles of surface in the eastern portion of Tyrone; it rests upon granitic and other primary crystalline rocks, and is succeeded by rocks partly belonging to the old red sandstone, and partly to the carboniferous system, by which it is completely detached from other rocks of a similar epoch. Apparently it has been raised up from its original level by the intrusion or eruption of the granitic mass, a movement which must have occurred prior to the deposition of the more recent rocks, as they exhibit no appearance of disturbance. The portion of the Silurian system, here exhibited, appears capable of sub-division. The lower or gritty slate is manifestly a fragmentary rock, and remarkable for containing great numbers of a brachiopodous bivalve, which is either identical with, or very similar to, *Orthis grandis* of Murchison. Above this is a black, smooth schist, occasionally slightly calcareous, and sometimes thinly laminated by calcareous spar. This is the depository of the Graptolites (*Lomatoceras*, Bronn), which are abundant.

The upper part of the Silurian district is a more decided slate, and abounds in Trilobites of the genera, *Calymene*, *Asaphus*, *Cryptolithus*, (Green,) *Trinucleus*, (Llwyd and Murchison). *Illænus? perovalis*, Murchison, and what Captain Portlock is inclined to believe the true *Isotelus*, besides some species of doubtful genera. This district will be fully illustrated in one of the earliest forthcoming parts of the Ordnance Memoir, and these and other fossils, such as Orthocerata, Bellerophon, Lingula, etc. figured.

An Account of the Footsteps of the Cheirotherium and five or six smaller Animals in the Stone Quarries of Storeton Hill, near Liverpool, communicated by the Natural History Society of Liverpool, through Dr. Buckland.

These footsteps were first noticed, in June last, by Mr. Cunningham and Mr. Tomkinson, who have taken means to preserve specimens in the Museum of the Natural History Society of Liverpool. Dr. Buckland having visited the quarries last week, confirmed the accuracy of the statements contained in the present communication. He found nearly all the circumstances identical with those attending the footsteps of similar animals discovered at Hildberghausen in Saxony, three years ago, in a bed of white stone belonging to the new red sandstone formation. The most remarkable of these footsteps are those of the hind-feet of the Cheirotherium, which nearly resemble the form of a large man's hand; the fore-feet of this animal have made much smaller impressions: other footsteps of four or five smaller animals are found on the same slabs with those of the Cheirotherium; they are apparently the tracks of small aquatic and land tortoises. (A further account has been communicated to the Geological Society since the meeting at Newcastle.—See Geological Proceedings, vol. iii. No. 59.)

Dr. Buckland exhibited and explained enlarged sections copied from Cotta's recently published sections, showing granite and syenite overlying strata of the chalk formation at Hohnstein, Oberau, and Weinböhla in Saxony; and laid on the table Mr. Cotta's Memoir in which they are described.

On a Plan of Cementing together Small Coal and Coal Dust for Fuel. By Mr. Oram.

Dr. Buckland stated the object of this plan to be the rendering these substances available for economical purposes, by moulding them into the form of bricks; and stated the results of trials made by Mr. Oram, at Woolwich, to test the efficiency of this substance, when it appeared that in working a 10-horse pumping engine, 750lbs. of this prepared fuel were equivalent to 1128lbs. of Wylam Main coal.
1046lbs. of large Welsh coal.
988lbs. of Pontop coal.
and to 680lbs. of a compound of the small coal, anthracite, and coke. These experiments were made under the inspection of P. Ewart, Esq.

Description of a Cave at Cheddar, Somersetshire, in which Human as well as Animal Bones have been lately found. By Mr. Long.

After noticing the circumstances which led to the discovery of the bones, the author describes the cave.

The cave is situated on the summit of the range of the Mendip, in limestone rock, and the entrance to it is from the flat surface, not from any broken chasm in the declivity of the rocks. This is generally the case with the other bone caves hitherto discovered in these hills, all of which are in the like strata of rock. The fissure of rock by which the cave was entered is about thirty feet in depth, a perpendicular descent; thence bearing to the west, is the opening which leads into the cave; from general appearances, and from what was afterwards discovered, this does not appear to have been the original entrance to the cave, and most likely was made for the purpose of admitting light and air. On entering the cave from this opening, the visitor finds himself in a lofty but not very large chamber, about sixty or seventy feet in height; from this cave there is an arched way into another smaller chamber, and from thence an ascending path leads towards the plain surface of the rock; this passage was undoubtedly the original entrance.

The bones were found in a detritus of soft mud or diluvium, as is the case in all the other ossiferous caves of this district, and so circumstanced as to be defended from the pressure of soil above, and excluded from the air. The human bones were found beneath the animal bones, so far as the cave has hitherto been searched; a few remains of foxes and sheep were found at the head of the cave, but the bones to which attention was particularly drawn, were found in a mass, in quite a separate position, and easily distinguished from those of a more recent date. "In searching in the cave," says Mr. Long, "I found some bones imbedded in stalactite, as also one almost forming part (as it might be termed) of a rocky substance. It was the work of many hours to clear away the soil and rock to obtain any specimens of the bones, but I was successful in finding both human and animal bones, having been accompanied by the individual who had been most active in the former search. In the first instance there were about nine human sculls found together, with a large quantity of human bones, and with them were the bones of bear, deer, ox, and horse. By comparison with the bones in Mr. Beard's extraordinary collection at Banwell, they are exactly similar and apparently of the same era. Some of the bones and sculls fell to pieces and crumbled to dust on being exposed to the air."

On the Discovery of the Northern or Diluvial Drift containing Fragments of Marine Shells covering the remains of Terrestrial Mammalia in Cefn Cave. By JOSHUA TRIMMER, *F.G.S.*

Mr. Trimmer's attention being drawn to the investigation of diluvial phenomena in North Wales by the discovery of marine shells of existing species near the summit of Mael Tryfan in Caernarvonshire, which is 1392 feet above the sea, he has become impressed with a growing conviction that the detrital deposits of North Wales were suddenly spread over pre-existing land, and not gradually accumulated beneath the sea. The evidence on this subject he has presented to the Geological Society, Dublin. In the present communication he describes the occurrence of marine remains covering the bones of land animals in Cefn Cave, in Denbighshire, on which he has also presented a memoir to the same society.

The principal deposit of bones lies *below* the level of the entrance, and beneath one if not more than one crust of stalagmite. The bones in this lower deposit are accompanied by *rounded* pebbles of grauwacke, slate, and limestone. The surface of the *upper* mass of marl, with angular pieces of limestone and bones, is covered by a deposit of sand, divided by a few inches of finely laminated marl, and in this sand, the total thickness of which, including the marl, does not exceed 18 inches, are fragments of marine shells. These fragments are small and not very numerous, but they are not smaller than a considerable portion of the fragments dispersed through the northern drift which covers the surface of the neighbouring country. At the extremity of the excavations in the cave, this sand is covered by a thin film of stalagmite. No marine remains were found in any other part of the cave, nor were any perforations of lithodomous shells seen on the sides.

On the Shells of the Newer Pleiocene Deposits. By James Smith, *F.G.S., of Jordan Hill.*

The author communicated the result of a comparison made by him between the marine shells found in elevated stratified deposits belonging to the newer pleiocene tertiary formation of the British Islands, with those now existing in the adjoining seas. Out of 176 species, 92 per cent. were recent, and 8 per cent. extinct or unknown.

On Vertical Lines of Flint, traversing Horizontal Strata of Chalk, near Norwich. By C. Lyell, *F.R.S., G.S.*

It has long been known that near Norwich the horizontal beds of flint nodules in the white chalk are crossed by perpendicular rows of much larger flints, often several yards in height. These larger and vertical flints are provincially called potstones, and are the same as those which occur in the chalk of Ireland, and are described by Dr. Buckland under the name of *paramoudra*. At a place called the Grove's End House, near Horsted, about six miles from Norwich, an excavation has been made, from 15 to 20 yards wide, and nearly half a mile in length, through 26 feet of white chalk, covered by strata of sand, loam, and shelly gravel, about 20 feet thick. In the chalk thus intersected, the rows of potstones are remarkable for their number and continuity; it is affirmed by those who for more than twenty years have superintended the cutting of the canal, that every row of upright flints has been found to extend from the top to the bottom of the chalk, so far as the excavation has been carried downwards. The rows occur at irregular distances from each other, usually from 20 to

30 feet apart, and they are not portions of continuous siliceous beds in a vertical position, but piles of single flints running through the chalk, like so many wooden stakes driven into mud. Few of the separate flints are symmetrical, but some are pear-shaped. They are very unequal in size—usually from a foot to three feet in their longest diameter. At the point of intersection between a row of potstones and one of the horizontal beds of flints, there is no mutual interruption or shifting, but they are united as if both were formed at one time. Each potstone is not siliceous throughout, like the nodules of flint in the horizontal beds, but contains invariably within it a cylindrical nucleus of chalk, which, when deprived of its siliceous envelope, has the form and smooth surface of a tree when stripped of its bark. This internal mass of chalk is much harder than the ordinary chalk surrounding the flints, and does not fall to pieces when exposed to frost: it penetrates the flinty covering at the top and bottom of each potstone. A ventriculite was observed in the chalky nucleus in one instance. The author concluded by inviting those geologists who resided near Norwich to examine these phenomena more minutely; and adverting to the late discoveries of Ehrenberg, declared his expectation that the origin both of the vertical and horizontal masses of flint would be found to be intimately connected with the fossil remains of Infusoria, sponges, and other organic beings.

On the Stratification of Rocks. By JOHN LEITHART.

The strata in Alston Moor, to which Mr. Leithart's personal observations have been chiefly confined, consist of numerous alternations of limestone, argillaceous shale or 'plate,' and sandstone. The definite order in which these rocks succeed one another, the variety of inclinations which they present, the phenomena of faults which interrupt the continuity of the strata, and other facts, appeared to the author inexplicable as the result of deposition from water, followed by elevatory action or the influence of heat.

Being engaged in the study of galvanism, he remarked that many other substances besides metals would, when piled in alternate layers, develop electrical action, and became impressed with the opinion that the stratified rocks might be likened to a galvanic battery, and that the peculiar appearances above noticed might receive an explanation upon this supposition, provided there was a communication across the enormous 'pile' of rock: such a communication is made, the author thinks, by mineral veins.

Upon this hypothesis he proceeds to show the probability that mixed sediments would be re-arranged by the electrical action into alternating distinct zones or strata, and confirms his reasoning by the result of direct experiments. In the first of these, a battery of 28 cylindrical plates of copper and zinc was used, but the author finds 18 or 20 pairs answer better. The copper plates had about 9 and the zinc plates about 6 square inches of surface. They were placed in jars, containing a mixture of 59 parts water and 1 part muriatic acid. The substances submitted to experiment were limestone and sandstone, mixed and reduced to fine powder and made into a paste with water. This mixture was put in a glass tube half an inch in diameter; the ends of the tubes were closed by metallic discs united to the connecting wires of the battery; and thus the mixed sediment was interposed in the line of the electrical currents. The result was a decided appearance of stratification, and a strong cementation of the mass.

In all the subsequent experiments the author endeavoured to imitate nature by a more slow electrical action, and employed only spring water as an exciting fluid.

In a mixture of limestone and shale the former was invariably rearranged on the zinc or negative end of the battery, and the shale on the other. In a mixture of limestone, sandstone, and shale, the same result occurred, the sandstone grains remaining in the middle, and being of the three the most consolidated. By adding to the small battery by which limestone and shale had been stratified the influence of another of equal force, the stratification became waved; by adding a greater electrical force, the materials collected at the upper end were seen to be displaced and carried irregularly through the other parts of the mass in thin veniform portions. The author considers these experiments strongly confirmatory of his hypothesis.

Several tubes filled with the substances named, and answering in the arrangement of them to the description given by Mr. Leithart, were exhibited to the geological section, and the author was prepared to repeat his experiments for the satisfaction of the members. He remarks that discs of tin and silver answer best for closing the glass tubes.

On Faults, and Anticlinal and Synclinal Axes. By John Leithart.

It is the opinion of the author, that these remarkable interruptions to the symmetrical arrangement of the earth's strata are not to be explained as the consequence of real changes in level of the surface of the earth, but as the result of electro-dynamic agency in the interior, operating through the mass of the rocks. The truth of this opinion he attempts to demonstrate by comparing the real phenomena of faults and axes of displacement of the strata, with the effects of electrical action predicted upon the following suppositions:—

1.—That electrical currents circulate in the earth;

2.—That faults, veins, &c. are the chief channels by which the electrical equilibrium between the surface and interior of the earth is maintained;

3.—That the stratification of the rocks forming the earth's crust is the result of the electro-polar action of these currents;

4.—That each stratum possesses its own peculiar electric condition and currents.

On the Production of a Horizontal Vein of Carbonate of Zinc by means of Voltaic Agency. *By* Robert Were Fox.

In this experiment a quantity of finely pulverised slate was mixed up in an earthenware vessel with a strong solution of common salt, and allowed to subside and form a bed, resting on a plate of zinc, which had been previously placed at the bottom of the vessel. A plate of copper, connected by a wire of the same metal with the zinc, was then placed horizontally on the bed, which was about $1\frac{1}{2}$ inch in thickness; the whole being covered by salt water. On taking out the contents of the vessel, several months afterwards, a well-defined vein of carbonate of zinc, about $\frac{1}{30}$th of an inch thick, was found in the bed, in nearly a horizontal position. This vein occurred rather nearer to the copper than the zinc plate, and extended over several inches of surface. It was sufficiently hard to admit of its being taken out of the bed in plates, and many parts of it would scratch glass, in consequence of minute portions of quartz having been inclosed therein. The carbonic acid was doubtless derived from the atmosphere, and the flat or horizontal position of the vein may be ascribed to the perpendicular direction of the voltaic action; because, in other experiments, in which similarly moistened clay was placed between *vertical* plates of copper and zinc, similar veins were formed in a *perpendicular* direction. The veins were of different kinds when different metallic solutions were employed, and the effect was generally most satisfactory when a constant battery of several pairs was used.

In many instances, when copper was present in the solution, the carbonates of zinc and copper were found in the mass of clay, occurring together in the *same vein*, not mixed, but in parallel plates, side by side, the *copper* being on the side of the vein *nearest the zinc plate*, and the *zinc* on the *side nearest the copper plate*. This definite arrangement is too constant to be referred to any other cause than voltaic agency, and its resemblance to some of the phenomena of mineral veins is very striking. The most marked of these results have been obtained by T. Jordan, of Falmouth, by the long-continued action of a constant battery of several pairs of cylinders on clay moistened by a solution of sulphate of copper.

On the Structure of the Fossil Teeth of the Sauroid Fishes. *By Sir* D. Brewster, *K.H.*

The fossil teeth to which this notice refers were found imbedded in coal from Inverkeithing, in the county of Fife. They were deeply fluted at the base, but had no hollow cone within, like those figured by Dr. Buckland, in his Bridgewater Treatise, and discovered by Dr. Hibbert in the limestone of Burdie-house.

In all the teeth which Sir D. Brewster has examined, the interior was filled up with a yellowish brown mineralized substance, having in the centre of the section or the axis of the tooth a white substance of the same character. The surface of fracture was partially covered with a number of small and exceedingly thin scales, almost perfectly transparent. They adhered to the brown matter with such tenacity that it was difficult to detach them for the purpose of examination by the microscope. Within the fluted base of one of the teeth, the white and brown substances are united together very irregularly, and in some places are combined with a third substance of a coaly nature, which burns without flame or smell, upon a heated iron. The enamel is in many places finely preserved. It has a yellowish transparency, and exhibits a sort of ramified structure both by reflected and transmitted light, the reflected tints having in some places a sort of nacreous lustre.

During the examination of the brown substance by which the cavity of the tooth is filled, the author noticed something like a veined structure; and upon a narrower inspection succeeded in tracing a regular structure in every part of it, exactly similar to that of a nodule of agate. The brown substance, which consists of bituminous and calcareous matter, seems to have been deposited and indurated in successive layers concentric with one mould of enamel, by which they were inclosed. The annexed sketch, on a magnified scale, will convey some idea of this structure, which Sir D. Brewster found more or less distinctly developed in every tooth. Upon subsequently examining the fossil teeth of Burdie-house, deposited by Dr. Hibbert in the Museum of the Royal Society of Edinburgh, the author perceived distinct traces of the same structure in one or two which presented fractures capable of displaying it.

On the Geology and Thermal Springs of North America. By Dr. DAUBENY, *Professor of Chemistry and Botany, Oxford.*

In this communication the author gave a rapid sketch of the mineral structure and direction of the mountain chains in North America, with a view of explaining the position which the thermal springs in the same country occupy, with reference to the adjacent rocks.

He then proceeded to describe the thermal springs themselves which he had visited in the course of his visit to the western hemisphere.

1st. In the mountain region of Virginia, west of the Blue Ridge, occur two groups of thermal waters: the first, called the Warm Spring, possessing a temperature of 96° Fahr.; the second, the Hot Spring, having one of 102°. Both emitted copious bubbles of air, which by analysis were found to consist as follows:—

		Carbonic Acid.	Nitrogen.	Oxygen.
From the Warm Spring	Ladies' Bath ...	11	98	2
	Gentlemen's Bath	8	96	4
From the Hot Spring		6	94	6

Both these groups lie at a distance one from the other of about 3 miles, in a valley running nearly N. and S., which occurs exactly at the part at which Professor Rogers, of Virginia, has placed the anticlinal axis of this part of the Alleghany chain. The same series of rocks is in fact repeated immediately to the east and west of the Springs, and assumes a nearly vertical position in both cases.

2nd. In the state of New York, at Lebanon, west of Albany, is a thermal spring, possessing the temperature of 73°, and emitting bubbles of gas which consisted of nitrogen 89·4, oxygen 10·6, without a trace of carbonic acid. It occurs near the junction of talcose slate with highly inclined beds of limestone, belonging to the Transition or Silurian system, and there are traces of a fault near it.

The carbonated springs of Ballston and Saratoga are not in general regarded as thermal, but the temperature of one of those of Ballston was found to be 50·5, of the other 49·5; whilst at Saratoga, the New Congress Spring and Hamilton Spring both had a temperature of 49½, and Congress Spring one of 51°. Now the mean temperature of Schenectady, the nearest point to these springs at which a meteorological register has been kept, is stated to be only 46·20. The gas given out by both these groups of springs was of the same quality, consisting chiefly of carbonic acid, but containing also a small residuary portion of air, in which nitrogen existed in larger quantity than in the atmosphere.

3rd. In the state of Arkansas, near the river Wachita, between the 34th and 35th parallels of latitude, and 16 degrees of longitude west of Washington, occurs a group of thermal springs, varying in temperature from 148° to 118° of Fahr., and emitting bubbles of gas which were found to consist of carbonic acid 4, nitrogen 92·4, oxygen 7·6. They gush out from the junction of clay slate with quartz rock, both belonging to the primary chain of the Ozark mountains.

The professor concluded by pointing out the correspondence between the phenomena of these springs, both as regards the composition of the gases emitted, and their position amongst rocks that had been subjected to violent action in their immediate neighbourhood, with those which he had deduced in his report on mineral waters, published in the Transactions of the British Association for 1836, from a survey of the mineral waters existing in various parts of Europe.

Considerations on Geological Evidence and Inferences. By R. C. AUSTEN, *F.G.S.*

The object of this communication was to examine the soundness and applicability of certain geological inferences, regarding the ancient land and sea, which have been freely adopted, sometimes in a general sense, upon local and limited data, insufficiently compared (the author thinks) with the laws of existing nature. The subjects discussed were the geographical areas over which particular mineral characters extend, and the degree in which the conformity of such characters is to be esteemed evidence of contemporaneous deposition; the succession of organic life in the ancient land and sea, and the contemporaneity of identical species in unconnected deposits and distant quarters of the globe; and ancient climate. The investigation does not admit of condensation, but the following are among the author's conclusions:—

1.—The identification of strata by zoological characters can never be done, *except over very limited areas;* a few degrees of latitude must always have brought about a perfect change.

2.—Along the same line longitudinally analogy does not allow us to expect a much wider range of the same animal or vegetable forms.

3.—Organic remains offer no proof whatever that the distant deposits are contemporaneous, but rather are proofs to the contrary; viz. that *contemporaneous* deposits, in situations removed from each other, can never have *had zoological characters in common.*

4.—Mineralogical character is only evidence as to a certain condition of water, under which the deposit was formed. Nor is inclined stratification a necessary consequence of disturbance, as some of the beds of recent stratified sandstone in Devon and Cornwall have been deposited at high angles.

On Lunar Volcanos. By T. W. WEBB.

The author, after showing the inadequacy of some of the grounds upon which the activity of lunar volcanos is often maintained, states the result of particular observations which appear to him to support the conclusion.

"It is obvious," he observes, "that either the formation of new craters, or the enlargement of those previously existing, would afford convincing proof of the continuance of explosive or eruptive action: and having examined several portions of the moon with an excellent achromatic telescope of five-feet by Tully, with the express view of detecting any appearances of the kind, I think I am enabled to assert that both the one and the other of these changes have taken place since the observations of Schröter at the close of the last century."

The general result is, that craters apparently of recent origin, and not to be found in Schröter's plates, are now equally conspicuous with those which he has delineated, and in situations where it is hardly conceivable that he could have overlooked them; and that in other places those which he has represented now exhibit a difference in magnitude which cannot well be explained by any supposition of accidental haste or inaccuracy. The charts of Lohrmann unfortunately do not contain those portions of the lunar surface in which Mr. Webb conceives these alterations to have taken place; but he had the pleasure of finding several of his observations confirmed by the beautiful Mappa Selenographica of Messrs. Beer and Mädler; and he expresses a hope that a more extended and accurate investigation may, in the course of a few years, not only bring to light the progress of many interesting changes, but may even enable us to form some inferences as to the nature and mode of action of that power which has produced such extensive and multiplied revolutions upon the lunar surface.

On the Construction of Geological Models. By Thomas Sopwith, *F.G.S.*

Next to the actual inspection of any object, a model is the most perfect means of conveying a clear idea of the general appearance and construction of the object which it represents; and in some cases a model exhibits and explains details which either cannot be grasped at once by the eye in the real object, or which are hidden from actual inspection, as in the case of the geological structure of the earth, or the interior of mines.

Some difficulty, however, has been found in conveying to ordinary workmen such a knowledge of geological or mining details, as shall enable them to execute a work of so much intricacy as these subjects usually present. The following method was pursued in the construction of a model of Dean Forest, and it is equally applicable to any geological model of a district.

Being in possession of an accurate plan and sections of the district, derived from surveys which he had made for the Commissioners of Woods and Forests in 1834, Mr. Sopwith divided the tract of country, comprehending about thirty-six square miles, by two series of parallel lines intersecting each other at the distance of a mile from each other. A vertical section was then prepared, corresponding with each of these lines. These several sections were drawn upon thin pieces of wood, which were united together by being what workmen term *half lapped*, forming a skeleton model of vertical sections. After having been united, the sections were taken separately, and cut into portions corresponding with the contour of the several layers of strata to be represented, the corresponding points of intersection having been previously marked with figures. These respective portions are again united, to form the exterior boundary or vertical edge of a square mile of rocks. The interior of each of these squares is filled with wood, and carved so as to coincide with its boundary edges. Any intermediate portion of the square may be ascertained by inserting a slip of wood cut to any known section; and in this manner the dislocations of strata, or any other phenomena, may be at once delineated, so as to enable the workmen to execute it in the model. By this means a connexion is at once established between the scientific drawings of the geologist and the operations of a common workman.

The contour of the surface is obtained partly by the upper edge of the section or slips of wood already described, and partly by the use of a gauge or graduated pencil sliding in a frame, and acting in the same manner as the gauge used by sculptors in transferring dimensions from a cast to a block of marble.

This method of constructing geological models was illustrated by several examples.

On the Structure of the Teeth. By Professor OWEN. (See Medical Science.)

On the Antiquity of Organic Remains. By the Rev. G. YOUNG, *D.D.*

In this communication Mr. Young opposed the inferences generally admitted among geologists, as to the high antiquity of the stratified rocks, and the successive eras of existence of the organic remains of plants and animals imbedded in them. He endeavoured to show that the production of the phenomena observed was possible in less time, and with fewer changes in the condition of the globe, than modern writers commonly admit.

On Peat Bogs. By G. H. ADAMS, *M.D.*

From a microscopic observation of the substance of fresh and old peat, the author described the gradual growth of the vegetable mass, and its conversion into condensed peat. To render peat bogs fit for agricultural purposes, the author proposes to take off the upper part, and to burn it in large smouldering heaps, using the ashes as manure for the subjacent peat surface. He also notices the practice of sprinkling diluted sulphuric acid over and through heaps of the surface-cut peat, thus 'souring' the peat and rendering it of considerable value as manure.

GEOGRAPHY.

Recent Intelligence on the Frozen Soil of Siberia. By Professor VON BAER, *of St. Petersburgh. Communicated by* W. R. HAMILTON, *Esq. President of the Royal Geographical Society of London.*

It may be remembered that M. Baer has on a former occasion described a well, nearly 400 feet deep, at Yakútsk in Siberia, in which the temperature of the soil at the bottom was found to be about the freezing point;—and the object of the present communication is to explain the measures taken by the Imperial Academy of Sciences at St. Petersburgh fully to investigate this point, to ascertain precisely, not only the law which regulates the temperature of the ground to the depth which is affected by the periodical change of summer and winter, but also the influence of the external air in penetrating into the sides of the well or shaft at Yakútsk; and, finally, to ascertain the depth which the summer heats generally reach.

The experiments recommended for this purpose are, to introduce pairs of self-registering thermometers into the side of the well at the several depths of 1, 3, 5, 10, 20, 50, 100, 150, &c. to 350 feet; the thermometers to remain a whole year, and to be examined daily. M. Baer also points out the importance for physical geography, to ascertain the thickness of perpetually frozen ground in countries whose mean temperature is considerably below the freezing point; for if, as at Yakútsk, the ground never thaws at a depth of from 300 to 400 feet, all the small streams where superficial waters only are kept in a fluid state in the summer, must be in the winter entirely *waterless*; and *vice versâ*, we may conclude, that all rivers which do not come from the south, and whose course is entirely within those countries which preserve perpetual ground-ice, and yet do not cease to flow in the winter, must receive their waters from greater depths than those which remain in a frozen state. This circumstance is not devoid of interest in the theory of the formation of springs. Professor Baer also states that he is collecting materials to ascertain the *southern* limit of perpetual ground-ice; and concludes with an appeal to Great Britain, whose extensive possessions in North America afford so ample a field for experiment, to furnish a similar series of observations in the western hemisphere.

Sketch of the recent Russian Expeditions to Novaïa Zemlïa.—By Professor BAER.

The object of this sketch was briefly to enumerate the different expeditions sent out by the Russian government, in order to illustrate a map of Novaïa Zemlïa, in which the outline of the islands is marked as represented in our most modern maps, and its actual outline as it is now known to exist; whence it appears that more than half the eastern portion of the land must be obliterated from our maps.

Many curious details also were given with respect to the vegetation and climate of these regions, whose mean temperature appears to be that of the freezing point.

A brief Account of a Mandingo, native of Nyáni-Marú, on the River Gambia, in Western Africa. By Captain WASHINGTON, *R. N.*

Although the special object of our inquiries, as geographers, is the surface of the earth we inhabit, yet, observes the author of this notice, it may be permitted to pause for a moment in our more ordinary researches for the purpose of contemplating a native of one of the little-known regions of western Africa, and to mark the vicissitudes in the life of a Mandingo, who in his native village had been in company with Mungo Park, one of the first and best of our African travellers, and successively to notice him as a slave, a soldier in the British army, a freeman, and, finally as about to return to the house of his fathers, and to impart to his countrymen some few of the blessings of civilization which he may have acquired during an absence of more than a quarter of a century from the land of his birth.

It were needless here to enter into any detail of the life of Mohamed-u Siseï; suffice it to observe, that the chief points of geographical interest are, that we have been enabled to obtain from him some itineraries in the country of Senegambia, noting places not to be found in our maps, but more especially a vocabulary of more than 2000 words and phrases in the Mandingo tongue; and when we consider how extensively diffused is this language, perhaps the most so of any of the thirty-six families of language into which authors have classified the 115 languages (not dialects) of Africa, and that hitherto a vocabulary of about 400 words is all that we possessed of it, it may perhaps be admitted that this native of the Gambia has not offered an unprofitable subject of geographical inquiry.

On the recent Expeditions to the Antarctic Seas. By Captain WASHINGTON, *R. N.*

This paper was illustrated by a South Circumpolar Chart on a large scale, showing the tracks of all former navigators to these seas, from Dirk Gherritz, in 1599, to M. d'Urville, in 1838, including those of Tasman, in 1642; Cook, in 1773; Bellingshausen, in 1820; Weddell, in 1822; Briscoe, in 1831: and exhibiting a vast basin, nearly equal in extent to the Atlantic Ocean, unexplored by any ship, British or foreign. The writer pointed out that the ice in these regions was far from stationary; that Bellingshausen had sailed through a large space within the parallel of 60°, where Briscoe found ice that he could not penetrate; that where D'Urville had lately found barriers of field-ice, Weddell, in 1822, had advanced without difficulty to the latitude of $74\frac{1}{4}$°, or within 16 degrees of the pole; and that it was evident from the accounts of all former navigators, that there was no physical obstacle to reaching a high south latitude, or, at any rate, of ascertaining those spots which theory pointed out as the positions where, with any degree of probability, the southern magnetic poles will be found. The paper also mentioned the expedition to the South Seas, which has just left this country, fitted out by several merchants, but chiefly under the direction of that spirited individual, Mr. Enderby, whose orders were to proceed in search of southern land, and to endeavour to attain as high a south latitude as practicable; and concluded with an earnest appeal to the British Association, that the glorious work of discovery begun by our distinguished countryman, Cook, might not be left incomplete. Europe, the author observed, looks to this country to solve the problem of Terrestrial Magnetism in the southern hemisphere,—and unanimously points to that individual who has already planted the "red cross of England" on one of the northern magnetic poles, as the officer best fitted to be the leader of an expedition sent out for such a service.

A Summary Account of the various Government Surveys in Europe, illustrated by specimens of the Maps of England, France, Austria, Saxony, Tuscany, &c. &c. By Captain WASHINGTON, *R. N.*

On the recent Government-map of Mexico. By Lieutenant-Colonel VELASQUEZ DE LEON.

A brief notice was given of a map of the state of Mexico which has been constructed within the last few years. The sites of the coal mines near Chilpanzingo, about 100 miles south of Mexico, the iron mines of Amilpas, and the tin mines of Acambay, near the north-western frontier, were specified.

Sketch of the Progress and Present State of the Trigonometrical Survey in India. By Major JERVIS, *Surveyor-General.*

The author of this paper first gave a rapid but comprehensive sketch of the physical geography of India, noting its coast line, its elevated table lands, and the mountainous region of the Himálaya; he then drew attention to the origin, in 1759, and to the progress, of the measurement of the great meridional arc of 1320 geographical miles in length, extending from Cape Comorin on the south, to the foot of the Himálaya, and effected by Colonels Lambton and Everest; and concluded by an appeal to the British Association, that through their recommendation the future progress of that survey should be conducted in accordance with the present state of science in this country, and in a manner worthy of the munificent liberality of the East India Company, by whose orders this great national work has been undertaken and carried into execution.

On the Construction of a Map of the Western portion of Central Africa, showing the probability of the River Tchadda being the outlet of the Lake Tchad. By Captain W. ALLEN, *R. N.*

In this paper, the author gave a summary of the reasons, derived from Arabic as well as modern authorities, and from his own personal experience on the river Quorra, as to the possibility if not probability of the course of the river Tchadda having been mistaken; and that instead of flowing from west to east, as represented by Denham, that it flows from east to west and joins the rivers Shary and Quorra, thereby affording water communication to the interior of central Africa.

On the recently-determined Position of the City of Cuzco in Peru. By J. B. PENTLAND, *Esq. A. M. Consul i n Bolivia. Communicated by Captain* BEAUFORT, *Royal Navy, Hydrographer to the Admiralty.*

We learn from this brief notice that the position of the ancient temple of the Sun at Cuzco is in $13° 30' 55''$ south latitude, $72° 4' 10''$ west longitude, differing full 45 miles from its position in our present maps; and that it stands at an elevation of 11,380 feet above the level of the sea. Mr. Pentland has also determined the positions of all principal places between La Paz and Cuzco, and of the western shores of the great inter-alpine lake of Titicaca.

On the recent Ascent of the River Euphrates. By Lieutenant LYNCH. *Communicated by Lieutenant-Colonel* CHESNEY, *Royal Artillery.*

This letter, dated Hit, June 1, 1838, described the facility with which the steamer had ascended the river from Basrah to that place. Between Hillah and Hit, it speaks of a broad, deep, and beautiful stream, in some of the bends nearly a mile wide; the country extremely fertile; the crops of corn abundant, and just reaped; the population of Arabs along the banks extensive, and apparently happy, welcoming the approach of the steamer with shouting and dancing, and supplying their want of fuel with great readiness and cordiality. The productions of the country, as wool, naphtha, bitumen, ghi (or butter), tallow, corn in abundance, and horses of the finest breed, are mentioned as easy to be obtained, and in large quantities; and the letter concludes with an expression of the writer's conviction that a profitable trade might easily be established; and, after the experience he has had of the river, that there are no physical obstacles to its free navigation with properly constructed vessels.

An explanation was then given of maps which were exhibited, and particularly of that showing the line of levels carried between the Mediterranean at Iskenderún, and the river Euphrates at Birehjik, whence it appears that the city of Antioch is situated 300 feet above the sea; the town of Birehjik, 628 feet; and the highest point between the sea and the river rises 1720 feet above the Mediterranean.

ZOOLOGY.

On the Wild Cattle of Chillingham Park. By J. HINDMARSH, *of Alnwick.*

The author stated that he had been obligingly assisted in his present attempt to give an account of the wild cattle of Chillingham, by the following communication from their noble proprietor:

"*Grosvenor Square, 8th June,* 1838.

"Sir,—Some time since I promised to put down upon paper whatever I knew as to the origin, or thought most deserving of notice, in respect to the habits and peculiarities of the wild cattle at Chillingham. I now proceed to redeem my promise, begging pardon for the delay. In the first place, I must premise that our information as to their origin is very scanty; all that we know and believe in respect to it rests in great measure on conjecture, supported, however, by certain facts and reasonings, which lead us to believe in their ancient origin, not so much from any direct evidence, as from the improbability of any hypothesis ascribing to them a more *recent* date. I remember an old gardener of the name of Moscrop, who died about thirty years ago, at the age of perhaps eighty, who used to tell of what his father had told him as happening to him, when a boy, relative to these wild cattle, which were then spoken of as wild cattle, and with the same sort of curiosity as exists with regard to them at the present day. In my father's and grandfather's time we know that the same obscurity as to their origin prevailed; and if we suppose (as was no doubt the case) that there were old persons in their time capable of carrying back their recollections to the conversation still antecedent to them, this enables us at once to look back to a very considerable period, during which no greater knowledge existed as to their origin than at the present time. It is fair, however, to say, that I know of no document in which they are mentioned at any past period. Any reasoning, however, that might be built on their not being so noticed, would equally apply to the want of evidence of that which would be more easily remembered or recollected—the fact of their recent introduction. The probability is, that they were the ancient breed of the island, inclosed long since within the boundary of the park. Sir Walter Scott rather particularly supposes that they are the descendants of those which inhabited the Great Caledonian Forest, extending from the Tweed to Glasgow, at the two extremities of which, namely, Chillingham and Hamilton, they are found.

"I must observe, however, that those of Hamilton, if ever they were of the same breed, have much degenerated.

"The park of Chillingham is a very ancient one. By a copy of the endowment of the vicarage, extracted from the records of Durham, and referring to a period certainly as early as the reign of King John, about which time, viz. 1220, the church of Chillingham was built, the vicar of Chillingham was, by an agreement with Robert de Muschamp, to be allowed as much timber as he wanted for repairs, of the best oak out of the Great Wood of Chillingham, the remains of which were extant in the time of my grandfather. The more ancient part of the castle also appears to have been built in the next reign, that of Henry III., since which it has been held, without interruption, by the family of Grey. At what period, or by what process, the park became inclosed, it is impossible to say; but it was closely bounded by the domains of the Percies on the one side, and by the Hibburnes on the other, the latter of whom had been seated there since the time of King John; and as the chief branch of the Greys always made Chillingham their principal residence, until it passed into the hands of Lord Ossulston, by his marriage with the daughter and heiress of Ford Lord Grey, it is reasonable to suppose that, in order to secure their cattle, wild and tame, they had recourse to an inclosure probably at an early period. It is said there are some other places in which a similar breed is found: Lyme Park, in Cheshire; Hamilton, as I before mentioned; and Chartley Park (Lord Ferrers). The first I have not seen, but they are described as of a different colour, and different in every respect. Those at Hamilton, or rather Chatelherault, I have seen, and they in no degree resemble those at Chillingham. They have no beauty, no marks of high breeding, no wild habits, being kept, when I saw them, in a sort of paddock; and I could hear no history or tradition about them, which entitled them to be called wild cattle. Those at Chartley Park, on the contrary, closely resemble ours in every particular; in their colour, except some small difference in the colour of their ears—their size—general appearance; and, as well as I could collect, in their habits. This was a very ancient park, belonging formerly to Devereux, Earl of Essex, who built the bridge on the Trent, to communicate with his chace at Channock and Beaudesert, then belonging to him; and the belief is, that these cattle had been there from time immemorial. With respect to their habits, it is probable that you will learn more from Cole, who has now been park-keeper at Chillingham for many years, than from any information I can give. I can mention, however, some particulars. They have, in the first place, pre-eminently, all the characteristics of wild animals, with some peculiarities that are sometimes very curious and amusing. They hide their young, feed in the night, basking or sleeping during the day; they are fierce when pressed, but, generally speaking, very timorous, moving off on the appearance of any one, even at a great distance. Yet this varies very much in different seasons of the year, according to the manner in which they are approached. In summer, I have been for several weeks at a time without getting a sight of them, they, on the slightest appearance of any one, retiring into a wood, which serves them as a sanctuary. On the other hand, in winter, when coming down for food into the inner park, and being in contact with the people, they will let you almost come among them, particularly if on horseback. But then they have also a thousand peculiarities. They will be feeding sometimes quietly, when, if any one appear suddenly near them, particularly coming down the wind, they will be struck with a sudden panic, and gallop off, running one after another, and never stopping till they get into their sanctuary. It is observable of them as of red deer, that they have a peculiar faculty of taking advantage of the irregularities of the ground, so that on being disturbed, they may traverse the whole park, and yet you hardly get a sight of them. Their usual mode of retreat is to get up slowly, set off in a walk, then a trot, and seldom begin to gallop till they have put the ground between you and them in the manner that I have described. In form, they are beautifully shaped, short legs, straight back, horns of very fine texture, thin skin, so that some of the bulls appear of a cream colour; and they have a peculiar cry, more like that of a wild beast than that of ordinary cattle. With all the marks of high breeding, they have also some of its defects. They are bad breeders, and are much subject to the *rush*, a complaint common to animals bred in and in, which is unquestionably the case with these as long as we have any account of them. When they come down into the lower part of the park, which they do at stated hours, they move like a regiment of cavalry in single files, the bulls leading the van, as in retreat it is the bulls that bring up the rear. Lord Ossulston was witness to a curious way in which they took possession, as it were, of some new pasture recently laid open to them. It was in the evening about sunset. They began by lining the front of a small wood, which seemed quite alive with them, when all of a sudden they made a dash forward altogether in a line, and charging close by him across the plain, they then spread out, and after a little time began feeding. Of their tenacity of life the following is an instance. An old bull being to be killed, one of the keepers had proceeded to separate him from the rest of the herd, which were feeding in the outer park. This the bull resenting, and having been frustrated in several attempts to join them by the keeper's interposing, (the latter doing it incautiously,) the bull made a rush at him and got him down; he then tossed him three several times, and afterwards knelt down upon him, and broke several of his ribs. There being no other person present but a boy, the only assistance that could be given him was, by letting loose a deer-hound belonging to Lord Ossulston, which immediately attacked the bull, and by biting his heels drew him off the man and eventually saved his life. The bull, however, never left the keeper, but kept continually watching and returning to him, giving him a toss from time to time. In this state of things, and while the dog with singular sagacity and courage was holding the bull at bay, a messenger came up to the castle, when all the gentlemen came out with their rifles, and commenced a fire upon the bull, principally by a steady good marksman from behind a fence at the distance of twenty-five yards; but it was not till six or seven balls had actually entered the head of the animal, (one of them passing in at the eye,) that he at last fell. During the whole time he never flinched nor changed his ground, merely shaking his head as he received the several shots. Many more stories might be told of hair-breadth escapes, accidents of sundry kinds, and an endless variety of peculiar habits observable in these animals, as more or less in all animals existing in a wild state: but, I think I have recapitulated nearly all that my memory suggests to me, as most deserving of notice; and will only add, that if you continue in the intention of preparing a paper to be read before the approaching Scientific Association at Newcastle, on this subject, you are welcome to append this letter to it, as containing all the information I am able to give.—I have the pleasure, &c.,

"TANKERVILLE.

"*To J. Hindmarsh, Esq.*"

In addition to this letter, Mr. Hindmarsh communicated some information collected from Mr. Cole, the keeper, and from his own observation. There are about eighty in the herd, comprising twenty-five bulls, forty cows, and fifteen steers, of various ages. The eyes, eyelashes, and tips of the horns alone are black, the muzzle is brown, the inside of the ears red or brown, and all the rest of the animal white. Even the bulls have no manes, but a little coarse hair on their neck. They fight for supremacy, until a few of the most powerful subdue the others, and the mastery is no longer disputed. When two bulls are separated by accident, they fight when they meet, although friendly before, and do so till they become friends again. The cows commence breeding at three years old; the calves suckle nine months; they conceal their calves for a week or ten days after they are born, suckling them two or three times a day. The late Mr. Bailey, of Chillingham, found a calf, two or three days old, very poor and weak. On stroking it, it retired a few paces, and then bolted at him with all its force; he stepped out of its way, and it fell down, when the whole flock came to its rescue, and forced him to retreat. They do not often die from disease, but they are seldom allowed to live more than eight or nine years, at which period "they begin to go back." When slaughtered, they weigh from 38 to 42 stones. One was caught and kept, and became as tame as the domestic ox, and thrived as well as any short-horned steer could do, and, in its prime, was computed to weigh 65 stone. They are shy in summer, but tame in winter, and will eat hay from a fold, although they will not taste turnips. When one of the herd becomes weak or feeble, the rest set upon it and gore it to death. At the end of the last century similar cattle existed at Burton Constable, Yorkshire, and at Dunnlary, in Dumfries-shire, but these are now extinct.

The author quoted a passage from Boetius, which, allowing for a little colouring, described these animals very well, except in the non-existence of a mane. The cattle at Dunnlary had black ears, but in all other points resembled those of Chillingham; and this may be accounted for by a statement of Bewick, that about forty years ago some of the animals had black ears at Chillingham, and were shot by the keeper. On the whole, the author was inclined to think these animals the survivors of the Caledonian cattle, which undoubtedly extended through the northern provinces of England; and that, under the protection of the owners of Chillingham, they had escaped the general destruction consequent on the advancement of civilization, in the country.

On a rare Animal from South America. By Lieut.-Colonel SYKES, *F.R.S.*

The animal in question was described by Azara as *Canis jubatus*; but the description given by Azara himself led the author to suppose it ought not to be placed in that genus. It differed from the dog tribe in its nocturnal and solitary habits: it had a long mane, its tail was thicker and more bushy, the head flatter, the eyes smaller, the nose sharper, and the whole animal more bulky than the dog tribe. If it differed from the dog, it differed more from the fox and wolf; and he proposed to refer it to the genus hyæna, or, if this could not be admitted, he would make it a distinct genus, which would then be the representative of the hyæna tribe in America, which we must suppose possessed some analogue of that tribe in the old world. Colonel Sykes also exhibited the skin of a European Felis, which Temminck names *Felis pardina*, and states is known as the lynx of Portugal: it is not, however, known by this name amongst London furriers.

On certain Species of Sorex. By Rev. L. JENYNS, *F.L.S.*

The Rev. L. Jenyns exhibited a series of specimens of the square-tailed shrew (*Sorex tetragonurus*, Herm.) and pointed out the distinguishing characters between it and the common shrew (*S. rusticus*, Jen.). He also exhibited a specimen of the chestnut shrew (*S. castaneus*, Jen.) which he had formerly considered as a mere variety of the *S. tetragonurus*, but of which he had now seen three individuals, and which he was satisfied deserved to rank as a distinct species. It is principally characterized by the bright chestnut colour of the upper parts, though there are other differences in the tail and in the form of the cranium. It was observed generally that the characters of the cranium were found of great assistance in determining the several species of this genus.—Mr. Jenyns also exhibited two undescribed species of the genus *Cimex* as restricted by entomologists of the present day. One of these, which has been alluded to by Latreille, though never characterized, was found inhabiting in great numbers the nests of the common house martin. The other was taken from a Pipistrelle Bat. It was proposed to call these two species *C. hirundinis* and *C. pipistrelli*. At the same time the peculiar characters were pointed out, by which each was distinguished from the other, as well as from the *C. lectularius* of authors, or common *Bed-bug*.

On Marsupiata. By Professor OWEN, *F.R.S., &c.*

Mr. Owen briefly stated the results he had come to in the course of his investigation of these animals, under the three following heads: first, the zoology of Marsupiata; secondly, their relation to other Mammalia; and, thirdly, the peculiarities of their reproductive economy. 1. With regard to their zoological characters, they present as many forms, and as varied habits, as all the Carnivora put together. In their kind of food they are very various. Some are entirely carnivorous, as those of New Holland. Some are insectivorous, like the Orycteropus and Myrmecophaga, among the other Mammalia. A species of these is described by Captain King, as having a divided hoof like the Ruminantia. Some of them are arboreous, as the Didelphes and Perameles. Many of the Marsupiata are strictly herbivorous, as the kangaroo-rat, &c. Mr. Owen thought, however, with all the varieties of character and habit presented by these animals that they had been too largely subdivided by zoologists. 2. In regard to their relation to other animals, he was of opinion, that they ought to be considered as one group; for although they differed greatly in some respects, still they agreed in so many remarkable points, that they could not be consistently separated. Of these points the most remarkable were the development of the hind legs; the existence of the marsupial bag; the circulatory apparatus being less perfect than in the rest of Mammalia, the blood being returned to the heart by two veins, as in the hearts of reptiles and birds; and in the hemispheres of the brain, which are not united by a corpus callosum. In this last respect, they are like the oviparous division of vertebrate animals, a fact first pointed out by Mr. Owen; having the same relation to Mammalia, that the Batrachians have to the Ophidian, Saurian, and Chelonian divisions of reptiles. 3. The reproductive economy of these animals was slightly touched upon. It had been supposed, that the young were produced by budding from the marsupial pouch; but this was now proved to be erroneous, and the first stages of their uterine growth were known to be like that of other Mammalia.

Mr. Owen then entered into some geological account of these animals. Dr. Buckland had found the jaw of an animal in the Stonesfield strata, which, from a peculiar mark only seen in the jaw of Marsupiata, could be well identified, and proved to be analogous to the present genus Opossum, or Didelphis. Major Mitchell has in his collection a large number of bones belonging to extinct genera of Marsupiata. From the jaw of one of these animals, there is reason to conclude, that its possessor must have been double the size of any species of kangaroo existing at the present time.

On Pouched Rats. By J. RICHARDSON, *M.D., F.R.S., &c.*

Dr. Richardson exhibited four very distinct species of American pouched rats, or gophers, belonging to the genus *Geomys.*

Remarks on the Greenland and Iceland Falcons. By JOHN HANCOCK.

On the question of the specific identity or difference of these birds, Mr. Hancock, in opposition to some English writers, has arrived at the conclusion that they are truly distinct. This opinion he has formed from an examination of many individuals of the Iceland and Greenland birds, his attention having been first awakened to the subject by a sight of two individuals brought from Iceland in 1833 by Mr. G. C. Atkinson. Besides various other specimens, minutely described by Mr. Hancock, he was fortunate enough to be furnished, by the exertions of Mr. W. Procter, who visited Iceland last year, with an opportunity of inspecting a 'brood' of five Iceland falcons, viz. the parents and three young ones, which Mr. Procter shot on the same crag.

On comparing the male, female, and young of these gray Iceland birds with the corresponding white falcons of Greenland, the differences became manifest, and Mr. Hancock endeavoured to show, by an investigation of other allied species, that the supposition of continual change of plumage, after maturity, by which it has been attempted to account for these differences, is not tenable. Mr. Hancock does not admit a white variety of the Iceland falcon, and thinks it doubtful whether this bird inhabits Greenland; while the white bird of Greenland is rare in Iceland, except during winter and on the northern parts of the island.

The author concluded his communication by a minute comparative description of the two birds. The following are characters of the mature plumage:—

Falco Islandicus.—Ground of the upper plumage, a dark lead or mouse colour, barred and spotted with cream colour; ground of the under parts, buff, marked with streaks, heart-shaped spots, and bars of dark mouse colour; wings reaching to within about $1\frac{1}{3}$ inch of the end of the tail. *Dimensions.*—Adult male: length, 1 foot $9\frac{1}{14}$ inches; extent of wings, 3 feet $10\frac{1}{12}$ inches. Female: length, 1 foot 11 inches; extent of wings, 4 feet 2 inches; like the male, but darker. (The young have the bars on the middle two tail feathers discontinuous.)

Falco Grœnlandicus, Linn.—Ground of the plumage, pure white; upper parts elegantly marked with arrow-shaped spots of a dark gray; under parts and head streaked with the same; wings reaching to within 2 inches of the end of the tail, second primary longest. *Dimensions.*—Adult male: 1 foot 9 inches. Female: length, 1 foot 11 inches; extent of wings, 3 feet 10 inches; like the male, but with more dark in the plumage. In some individuals the bill has two processes in the upper mandible. (The young have the bars on the middle two tail feathers continuous.)

On the Ardea Alba. By ARTHUR STRICKLAND.

Mr. Strickland stated, that this bird had been unjustly excluded from the catalogue of occasional visitors to this country by late authors, as he could prove on unquestionable authority, that it had been killed of late years in more cases than one. The first instance was twelve or thirteen years ago: a bird of this species was seen for some weeks about Hornsea Moor in the East Riding of Yorkshire; it was some time after presented to the author, in whose collection it is at present, in perfect preservation. Another, in full summer plumage, was killed by a labourer in the fields of James Hall, Esq., of Scorborough, near Beverley, about three years ago, and is now in the possession of that gentleman. Another specimen of this bird is in the collection of Mr. Foljambe, of Osberton, with a label on the case, stating it to have been killed near that place. A careful examination of these specimens will, Mr. Strickland has no doubt, prove that this bird is properly separated from the large egret of North America, which has been frequently placed in our collections for the British species.

On a species of Scyllium, taken on the Yorkshire Coast. By Arthur Strickland.

Mr. Strickland described a large fish of this genus, which had been caught in Bridlington Bay on the 11th August, 1838.

On the Toes of the African Ostrich, and the number of Phalanges in the Toes of other Birds. By T. Allis.

The author's attention was directed to this subject by Dr. Riley, of Bristol, who had stated at one of the meetings of the British Association, that he had found the rudiments of a third toe in the ostrich. Neither in the specimens which he has placed in the Museum at York, nor in one that he obtained lately, for the express purpose of looking for this rudimentary toe, has he been able to discover any thing like this third member of the foot. He further stated, that Cuvier had given the number of the phalanges of the toes wrong in the following birds. In the cassowary, which has three toes, the real numbers of the phalanges are three, four, and five. In the ostrich, four and five. The Caprimulgus has the outer and middle toe, having four phalanges each. The swift has only three phalanges, except in the hallux. The humming-bird has the full number of phalanges in all its toes.

On Tetrao Rakelhahn. By Edward Charlton, *M.D.*

Dr. Charlton, in this communication, described the appearance of two individuals of the species or variety of *Tetrao* above named, and assigned the reasons which induce him to believe that it is a hybrid between the *Tetrao urogallus* and *Tetrao tetrix*. In favour of this opinion he quotes Bechstein and Nillson, though on the other side Temminck describes the bird as a distinct species. Mr. Charlton stated that he found the Norwegian peasantry perfectly aware of the existence of this hybrid, giving to it the name of 'Rockelhanar.'

Notice of the Annual Appearance on the Durham Coast of some of the Lestris tribe. By EDWARD BACKHOUSE.

The author stated, from observations made during a series of years, while occasionally residing in the neighbourhood of the Tees' mouth, that the *Lestris Richardsonii* is the earliest of the genus in its appearance on these shores when on its southern migration.

The young birds seem usually to arrive in the beginning of September, and in the middle of the same month the adults, accompanied by the young of the *Lestris Pomarinus*, make their appearance, generally continuing for about three weeks, when they are succeeded in the middle of October by the mature *Pomarine Skuas*; these, as far as Mr. Backhouse has been able to discover, continue for the like space of three weeks and then disappear.

He last year met with *Lestris Pomarinus* in its mature state in considerable abundance off Hartlepool and the Tees' mouth.

Early in the autumn of 1836, while at the same place, he obtained one of the Lestris tribe which materially differed from any he had before met with.

This specimen is in the immature plumage, very much resembling in its markings the young of *L. Pomarinus*. In size and proportions it nearly approaches *L. parasiticus*; and having recently compared it with a nearly mature specimen of *L. parasiticus*, also shot on the coast of Durham, now in the collection of Mr. John Hancock of Newcastle, he is induced to conclude it to be the young of that bird.

Its admeasurements are as under, viz.:—

Length from bill to tail		17 inches.
Expanded wings		32
Elongated tail feathers, rounded at the end, project		$\frac{7}{8}$ths of an inch.
Bill, from forehead to tip,	nearly	1 inch.
Length of *tarsi*		$1\frac{3}{4}$ inch.

He also stated that *L. cataractes* was met with, though rarely, on this coast.

The paper was accompanied with drawings and stuffed specimens of the various species.

On a New Species of Smelt from the Isle of Bute. By W. YARRELL, *F.L.S.*

In the month of November, 1837, Mr. Yarrell received from W. Ewing, Esq., of St. Vincent Street, Glasgow, a specimen of a smelt, which was at the first glance so obviously different from our well-known and esteemed favourite, as at once to claim for it the title of a distinct species; and the specimen was the more interesting from the circumstance that this fish is not only new to our own country, but is also entirely new to ichthyology, no second species of the genus *Osmerus* having hitherto been made known.

The gentleman just named passed part of the summer of 1837 near

Rothsay in the isle of Bute, and the fish in question was brought to him by a fisherman, who stated that he caught it on a hand-line in the bay of Rothsay about 200 yards from the shore, in twelve-fathom water; that it was, though well known, but rarely seen; that specimens varied from 6½ to 8 inches in length; that they were full of roe in June, and when first caught the cucumber-like smell was very apparent.

Mr. Yarrell thus describes the characters of this new species of *Osmerus*, for comparison with the common smelt:—In the new fish the jaws are of equal length, without teeth upon either, but there are four long teeth upon the tongue; the eye very large; the upper surface of the head convex; the form of the operculum circular; the dorsal fin commencing half-way between the point of the nose and the anterior edge of the adipose fin; the anterior edge of the adipose fin is at the end of the second third of the space between the dorsal fin and the end of the fleshy portion of the tail, while the ventral fins, which are in the middle of the whole length of the head and body in both species, are, by the proximity of the first dorsal fin to the head in the new smelt, brought in a vertical line underneath the posterior edge of the first dorsal fin; the anal fin, like the adipose fin above it, commences much nearer the tail than in the common species; the ends of the caudal rays not tipped with black. The numbers of the various fin rays are as follows:—

	D.	P.	V.	A.	C.
Osmerus vulgaris	11	11	8	15	19
New species	11	14	12	12	19

The form of the body is elongated and slender; the lateral line straight; above it the colour of the body is of a pale yellowish green; below it is a broad longitudinal stripe of bright silvery white, passing, by a shade of yellowish olive, to an iridescent silvery white on the belly.

To identify this species with the locality from which it was derived, Mr. Yarrell proposes to distinguish it by the name of the Smelt of the Hebrides—*Osmerus Hebridicus*.

On some new and rare British Fishes. By Richard Parnell, *M.D., F.R.S.E.*

The author exhibited a large collection of British fishes, and read notes upon their specific characters and synonymy, which he proposed to embody in a work devoted to the Natural History of Fishes*. The species most copiously illustrated by Dr. Parnell's observations, were *Motella cimbria* of Linnæus, *Pagellus acarine*, *Raia chagrinea*, hitherto seen by very few naturalists, and *Raia intermedia*, which he thinks has not been previously described.

Besides various other interesting fishes, Dr. Parnell exhibited to the Section a dish of white bait (*Clupea alba*) which had been caught the preceding day in the Frith of Forth, and were recognized by Mr. Yar-

* The Natural History of the Fishes of the Forth. This interesting work is published.

rell and other Ichthyologists present as identical with the fish of the Thames.

Mr. Forbes stated, that he had lately taken off the Isle of Man two specimens of the lancelot.

On the Sternoptixineæ, a Family of Osseous Fishes. By P. D. Handyside, *M.D., F.R.S.E., of Edinburgh.*

After giving a sketch of the history of this family, and especially of the genus Sternoptix, Dr. Handyside entered into a minute description of a new species, which he proposed to call *S. cœlebes,* distinguishing it from *S. Hermani* and *S. Olfersii,* to which it most nearly approached. He considered these three fishes to form a distinct group or sub-family of Salmonidæ.

On a Fish with Four Eyes. By W. H. Clarke *and* John Mortimer.

In this communication the authors state, that at Fort Amsterdam (Surinam) shoals of small fishes appear periodically, having four distinct organs of vision. They observed that the water of the river, or rather estuary, on which the fort is built, was blackened for miles along its margin by innumerable multitudes of these fishes, which were followed by scarlet flamingos. The description given of these fishes agrees in several points with the character of the Anableps*, the only fish known to naturalists which, by the double pupils of its eyes, may deserve the title of *tessarophthalmoid,* proposed by the authors for the little fishes they observed. It is, however, stated in the paper, that the eyes of these fishes are really four, separated in two pairs by a transverse horny protuberance, and separately moveable. A drawing in pencil accompanied the communication.

On a new British Shell. By J. E. Gray, *F.R.S., &c.*

The shell in question had been discovered by Miss Isabella Mark in the stomach of a haddock taken on the coast of Northumberland, and it was believed had not hitherto been described. Mr. Gray proposed to make of it a new genus, which he would call *Neara,* and which would be peculiar for the slender produced form of the under edge, and the large size of the lateral teeth. He stated, that he knew two species belonging to the same genus, one from China figured by Chemnitz, and called *Anatina rostrata,* by Lamark; and the other from the Adriatic, described and figured by Olivier, under the name of *Tellina cuspidata,* and that he was not certain, without comparison, that the British species was distinct from the latter. Mr. Gray also exhibited a very splendid specimen of *Balanus scoticus,* attached to a

* Cobitis anableps, *Linn.*; Anableps tetrophthalmus, *Bloch.*

species of Fusus, which had been obtained from the museum of Mr. Fryer.

On the Formation of Angular Lines on the Shells of certain Mollusca. By J. E. Gray, *F.R.S., &c.*

The annular marks, and those in the direction of the growth of the shell, and in the substance of the shell itself, are easily explained by the increased or diminished degree of activity of the secreting surface of the mantle. But the coloured angular lines are not so easily explained. Mr. Gray supposed that the colouring of the shell was the consequence of glandular secretion; that, as the shell increased in size, there was a tendency to divergence in the glands. He stated, that it frequently happened in the progress of growth, that these glands were obliterated, and the immediate consequence of this obliteration was the production of a new gland: this gland was double, and, as it had a tendency to diverge, it formed two angular lines which proceeded to a certain distance, when it met with a gland formed in a similar way to itself, and, on meeting, it became obliterated: after this obliteration, a new double gland was formed, which proceeded in the same mode as the first, and thus produced the angular coloured lines apparent on so many shells.

Notice of the Wombat. By J. E. Gray, *F.R.S., &c.*

Mr. Gray stated, that in the Museum of the Natural History Society, was the wombat which was sent by Bass to Bewick, and from which he took his original description: from a misprint, this specimen was said to have more teeth than it really has; and, on this account, Illiger having seen a specimen of the wombat, supposed this must be another genus, and named this one in his work, *Amblotis wombattus*. The condition, too, of this specimen assisted in the mistake, for, having been originally kept in spirit, it had lost its true colour.

On the Boring of Pholades. By J. E. Gray, *F.R.S., &c.*

A difference of opinion prevails as to whether the action of these animals in excavating the rocks in which they are found is chemical or mechanical. At one time Mr. Gray was inclined to think it was the former; however, he had lately an opportunity of remarking the action of these animals in the chalk at Brighton, and he now believed it to be mechanical. He then exhibited several specimens of chalk which had been bored by the pholas, and pointed out some circular grooves which were made in their interior by spines on the outside of the shell, as well as a central impression produced by an elongation of the shell to a point at its inferior surface. He stated that the animal did not occupy the whole of the cavity it made, but the upper part only. Why he had formerly supposed the action of these animals on the rocks to be chemical was, that the *patella* was known to bore; and this would be impossible by the action of its flat shell. There was a little annelide, called Diplotis, which made elongated cavities in rocks. Now this animal had no shell, and its action must of course be chemical.

On the Distribution of Terrestrial Pulmonifera in Europe. By Edward Forbes, *F.L.S.*

For the purpose of stimulating zoologists to the investigation of the distribution of the terrestrial and fluviatile Mollusca in Great Britain, and the collection of data for that object, Mr. Forbes presented a sketch of the laws which apparently regulate the distribution of terrestrial Pulmonifera in Europe. "At present," he observes, "the materials are few for such an investigation, and most of the published catalogues are almost unavailable from the authors not having guarded against certain sources of fallacy." These Mr. Forbes points out in order that they may be avoided in future.

Instead of the political and artificial divisions for which European local catalogues are published, the author proposes to consider the distribution of terrestrial Mollusca according to natural districts, and presents the following classification as suited to Europe:

1st district. The greater part of Scandinavia, Iceland, the north of Russia, Scotland, probably Ireland, and the greater part of England.

2nd district. Germany (except Austria), the south of Sweden and Denmark, the south of England, northern and central France, the chief part of Switzerland.

3rd district. The peninsula of Spain and Portugal, the south of France, the west of Italy, the western Mediterranean Isles, Africa, Barbary States, the Canaries.

4th district. Dalmatia, Wallachia, Turkey, Greece, the eastern Mediterranean Isles, Asia Minor, Syria.

5th district. Southern and eastern Russia, as far as Caucasus, extending into Asia in Georgia.

6th district. Austria, Styria, Croatia, Carniola, appear, by their peculiar conchological products, to be in this point of view a separate district.

"Each of these great divisions has a conchological character of its own; in some certain genera prevail, in others certain species; these divisions may be regarded as climates, and the Flora of each will be found to correspond in its distribution with the Fauna."

The author pointed out certain defects in the ordinary form of local catalogues, and suggested the introduction of notices of the soil, rock, frequency of occurrence, influences on distribution, variations of form, &c., and proposed a series of queries relating to Mollusca in furtherance of his general object.

Remarks on the Modern Classification of Insects.
By Rev. F. W. Hope, *F.R.S.*

The following is an outline of the communication:—1st. That modern entomologists, in their arrangements, have attended almost entirely to external organization. 2ndly. Internal organization has only been partially attended to: the alimentary canal, on which much stress is placed, cannot be considered as a criterion of an animal or a vegetable feeder, and is ill-adapted for the classification of insects. 3rdly. No uniform principle of arrangement has been entirely carried out: all have been interfered with by the introduction of other principles of secondary and minor importance. 4thly. It is only from increased attention to the *Nervous System* that we can expect a more natural system than what exists at present.—The author illustrated his positions by extensive tables of genera.

On the Noxious Insects which have this year (1838) *seriously injured the Apple Trees and Hops. By the Rev.* F. W. Hope, *F.R.S.*

Mr. Hope described the *Aphides* as unusually destructive to various species of plants. An insect named *Tipula Tritici* has appeared in great abundance in some parts of the counties of Hereford, Worcester, Gloucester, and Salop. From an examination of various samples of wheat submitted to Mr. Hope, it appears that the damage done by the *Tipula* is less than in previous years.

Mr. Wailes and Mr. Charles Adamson exhibited the two sexes of the rare insect *Psalidognathus Friendii*, found in the interior of a decayed palm tree in South America.

On a new Species of Goliathus and some Lucani, from the Coast of Africa. By J. A. Turner.

The goliathus belongs, it was stated, to the genus constituted by the Rev. F. W. Hope, under the title of Dicronorhina. Some other lamellicorn beetles were exhibited, especially two splendid Lucani, all from Southern Africa.

On the Gemmiferous Bodies and Vermiform Filaments of Actiniæ. By T. P. Teale.

The author stated, that as great differences of opinion existed amongst zoologists as to the nature of the gemmiform bodies and vermiform appendages of Actiniæ, he had undertaken their investigation. Some general remarks on the structure of Actiniæ were premised, the author pointing out, by means of a large diagram, the various directions of the muscular septa, some lining the cavity and supporting the stomach of the animal, whilst others more delicate terminated in a mesentery, supporting the *gemmiferous bodies*, or what has been erroneously called the ovary. The division of the stomach into two lateral parts, giving to the whole animal a bilateral symmetry, was pointed out.

The *Gemmiferous bodies* are about 200 in number, and appear as elongated masses attached along the inner border of most of the leaflets. Each is composed of several horizontal folds or plaits, which, when carefully unfolded, may, by the assistance of a lens, be seen to consist of two delicate layers of membrane, enveloping one closely compacted layer of gemmules. After enveloping the gemmules, the membranous layers become placed in opposition, and form the mesentery, by which the gemmiferous body is attached to the leaflet. The gemmules are round, except in an advanced stage of development, when their outline becomes interrupted by the pressure of neighbouring gemmules. A well-marked central depression may also be seen indicating the situation of the oral aperture, but without tentacula; when of large size, they form considerable depressions in the gemmiform bodies, protruding before them their delicate investing membrane. In this state, they are readily detached by the point of a needle. Their size is nearly uniform, except a few small ones, scattered very generally amongst the whole. There is no gradation in size amongst them, as if they successively arrived at maturity, as imagined by Dr. Spix. Some of the gemmules are, however, less developed than others; and at the same season of the year, it is not uncommon to find individuals with the gemmules in very different stages of development, and this is not limited to any particular season. The colour of the gemmules varies considerably. *The Vermiform Filaments.*—They are attached by a delicate mesentery to the internal border of each gemmiferous body; they are formed of numerous convolutions extending from the superior to the inferior part of the gemmiferous body. They are of a milk-white colour, about as thick as a horse-hair, extremely soft, yielding readily to pressure with a needle. Superiorly, the filaments are very minute, so that their origin cannot be detected. Inferiorly, they are of larger size and less convoluted, passing in a simple wavy line to the stomach, where they terminate. During life, these filaments exhibit a distinct vermicular motion, even after removal from the animal. On removing some from the animal, and placing them in sea-water, they exhibited considerable locomotive power, which lasted for some time, when their outline became obscured, and in twenty-four hours nothing remained but a whitish flocculent substance. This structure is best seen in its living state. In fresh water it decomposes in half an hour, but in proof spirit less rapidly. The author has succeeded in preserving it best by spreading the filament and its mesentery upon glass, upon which they may be dried. The function of these filaments is involved in obscurity. By many, they have been regarded as oviducts, but this the author thinks is very improbable, both from the minuteness of their terminations, the size of the gemmules, and the fact of ova never having been detected in them. In fact, the reproduction of Actiniæ must be looked upon as a strictly internal gemmiparous process, in which the gemmules, when sufficiently matured, burst their envelope, and become lodged in the interseptal spaces, where they are exposed to the access and continued supplies of sea-water, the grand stimulus to their future development. In the absence of any direct evidence as to the nature of the vermiform filaments, the author suspects that they are elongated follicular glands, analogous to the salivary, pancreatic, and hepatic follicles of animals a little higher in the scale of organization, supplying secretions subservient to the digestive process.

A drawing was exhibited, and a description given, of a new species of Ascaris, discovered by Dr. Bellingham, which he called *A. alata*. The distinctive character of this species was, that its posterior extremity was larger than its anterior.

On certain Monstrosities of the Genus Encrinus. By G. B. Sowerby, *F.L.S.*

Mr. Sowerby's immediate object was to point out certain monstrosities to which the *Encrinus moniliformis* is subject, which chiefly affect the arms. The plates of the pelvis (Miller) are also affected in number and somewhat in form.

The radiaria are usually divided by five, i.e. the normal number of plates of the pelvis is five; though there are not wanting instances of genera whose pelvis consists of only three plates: we shall, however, find that even these return to the normal number (Pentatrematites), for they have five scapulars, ten intercostals, &c. In *Encrinus* the normal number of pelvic plates is five, the costals five, and the scapulars five; these are then usually divided, so that there are usually ten arms setting off from the five scapulars. One of the monstrosities in question has only nine arms, though it has five plates to the pelvis, five costals, and five scapulars; in this instance one of the scapulars only has produced one arm, the other four having produced the usual number. Another of the monstrosities has eleven arms, though this also has only the normal number of pelvic plates, costals, and scapulars; one of these last sends off three arms. Another has eleven arms, arising from an increased number of pelvic, costal, and scapular plates, one of the scapulars sending off only one arm, the remaining five sending off two each. Another specimen has twelve arms, arising from six pelvic plates, six costals, and six scapulars; in this instance, though each scapular sends off two arms, one pair of pelvic plates, costals, and scapulars is uniformly smaller than the other three pairs. Another individual, the last instance mentioned of monstrosities in the number of arms, has thirteen, which arise from six pelvic plates, six costals, and six scapulars, one of these latter sending off three arms.

The author has observed two circumstances which induce him to believe it probable that Miller might be correct in his surmise that the animals were *soft* when living:—1, when two portions of vertebral columns have been pressed together, each has taken a corresponding impression from the other; 2, the great variety in the form and prominence of the tubercles on the joints of the arms. In some instances these joints are nearly free from tubercles; different parts of the same individual vary in this respect: some have very prominent and accumulated tubercles; in others these tubercles are extremely irregular. This cannot be taken as positive proof of their having been soft, but may nevertheless be regarded as confirming Miller's opinion.

Notice of Microscopical Discoveries. By Professor Ehrenberg.

In this extemporaneous address the learned Professor stated, that he had the honour of exhibiting before the Section as much as he had been able to effect of his great work on microscopic forms of life,—a work which, he observed, he should never complete, as the subject was inexhaustible, but that he should continue to extend it, as far as opportunity would allow. After explaining many of the subjects represented in the engravings, he submitted to the inspection of the members present a bottle of the material, collected in considerable quantity in the vicinity of Lake Lettnaggsjön, in Sweden, to which the inhabitants of the district give the name of *Bergmehl*, or mountain meal. This earth, which resembles fine flour, has long been celebrated for its nutritious qualities, and was found, on examination, to be entirely composed of the shells of microscopic animalcules. The Professor also explained some circumstances to be observed in studying the interior structure of microscopic animalcula.

Mr. Trevelyan exhibited a young living specimen from Rome, of the *Coluber natrix* of Italian authors, but evidently differing from the English species so called; also a specimen in spirits of *Polyodon folium* of North America, a small collection of Neapolitan insects, and specimens, gathered by him in the island of Elba in 1837, of an *Urtica*, probably an undescribed species.

BOTANY.

On the Production of Vanilla in Europe. By Professor Morren, *of Liege.*

The Professor commenced by stating, that some difficulty at present existed in determining the species from which the vanilla of commerce was produced, but the *Vanilla planifolia* would produce it. This plant does not naturally produce odoriferous fruit; but Mons. Morren had succeeded in obtaining, for two years running, fruits as large and odoriferous as those of commerce. The author remarked, that the cultivation of this plant might be now attempted in our intertropical colonies, with the application of the principles of modern botanical science, and that this substance might be obtained at a much lower price than at present. The author thought the cultivation of the vanilla plant could not take place in the British isles. In order to obtain good fruit, the plant should be allowed to grow five or six years; the fruit is not in proportion to the flowers; and the older, the larger, and the more branches the plant possessed, the better is its fruit. Exposure to the sun is not necessary for the maturation of the fruit; shade, heat, and humidity being the three conditions necessary for the flowers. The stigma of the plant is supplied with a peculiar appendage, which covers over the stigmatic surface in the form of a veil, and this requires to be lifted up before the artificial impregnation of the plant can take place. The author went into several particulars necessary to be attended to for the successful cultivation of the plant.

On the Botany of the Channel Islands. By CHARLES C. BABINGTON, *M.A., F.L.S., &c.*

In this communication the author mentioned the discovery of the following eight plants in these islands in addition to those noticed at the Liverpool Meeting, namely,

Ranunculus ophioglossifolius, *Ononis reclinata*,
Orchis laxiflora, *Potamogeton plantagineus*,
Linaria pelisseriana, *Carex punctata*,
Myriophyllum alterniflorum, and *Polygala oxyptera*.

He said that twenty species existed in the islands which had not as yet been noticed in Britain, and announced his intention of publishing an outline of their Flora in a few months.

On the Genera Pinus and Abies. By Capt. J. C. COOK, *R.N.*

The author commenced by stating, that not less than seventy species of Pinus and Abies had been lately introduced into this country. The distribution of these throughout the world he divided into five groups: —1. Those of Old America, which included the United States, the Mississippi and Canada, with Labrador. 2. Those growing between the Pacific and Atlantic Oceans, in the district known by the name of the Rocky Mountains, and which might appropriately be called the "Douglas Group." 3. The uplands of Mexico. 4. The Himalaya Mountains. 5. Europe. The first group contains about twenty species, none of which can be said to produce more than second-rate timber. They are fine trees in their native forests, but degenerate in Europe. 2. The "Douglas Group." Of these there are about fifteen species, possessing all the qualifications for good timber, at the same time that they are evergreens, and grow quickly; and from the present condition of the young plants in England, the most sanguine anticipations of their successful culture in this country may be entertained. At present, however, little positive information had been obtained with regard to this group. 3. The species from Mexico are at present few in number; and too little information about them is possessed to warrant any conjectures as to their worth. 4. Those in the Himalaya range are also few, and for the most part little known. Some of them may probably become naturalized in this country. The *Abies Webbiana* is a gigantic tree, but has not perfectly stood the last winter. *Abies Morinda* stood the winter very well. Both species are propagated by cuttings. 5. The European series is the most valuable. In this group the quality of the species is, as nearly as possible, in a direct ratio to the ability of the tree to resist cold; all the best species being found in an extreme northern latitude, or in an equivalent situation on mountains in the south ; no valuable species at all being found on the shores of the Mediterranean or the Baltic. The highest place in the European series is assigned to the *Pinus cembra* and *P. uncinata*, both of which grow in their respective Alpine and Pyrenean forests, above the *P. sylvestris*, or Scotch fir, and both excel it in the quality of the timber. The *Pinus sylvestris* is next, and its range is from the Arctic circle to the Sierra da Guadarama, in Spain. The next tree in the series may be considered *P. Laricio*, which grows in the mountains of Corsica, at rather a higher elevation, and in lat. 43°, and does not descend to the level of the Mediterranean. With this, both in latitude and elevation, is associated *P. Hispanica*, although in most respects it differs from other pines. Its range is from 39° to 43° N. lat., at the foot of the highest Pyrenees. These two species form about a middle zone in the European pinal vegetation, and their timber is found to occupy about a middle rank in quality, being superior to those below, and inferior to those above it in its range. The next species is *Pinus pinaster*, whose northern habitat is the Sierra da Guadarama, and ranges immediately under *Pinus sylvestris*; it is not so good a tree as might be supposed from its range, as it grows in sultry valleys and situations unfavourable for the production of timber. The *Pinus pinea* (Stone pine) has a timber nearly allied to the *P. pinaster*, its most northern habitat being in Old Castile, where it occurs in great quantity; and although it reaches a medium altitude, it is, like the last, found growing on sultry flats, as those of Andalusia, &c. The last of this series is the *Pinus Halepensis*, of which three varieties are known, which clothe the shores of the Mediterranean, on both sides, throughout its whole extent.

The species of Abies do not admit of the same extended observation, the series being less in number and extent. The European species are certainly inferior to those of Pinus. The *A. excelsa* is the hardiest, and resists a damp soil probably better than *Pinus sylvestris*. The *A. pectinata* is found much further south than the last, which extends no further than Savoy, whilst this is found in the Pyrenees and Navarre, and a variety has been observed in Cephalonia; and no doubt great use could be made of it in our own culture. The larch, although in some respects an anomaly in the genus, follows the same rules. Its southern site is the highest part of the Apennines in Piedmont, and its northward range is very great, but is never found at a low elevation. The *Pinus austriaca* probably belongs to this group, but the author knows little of it at present; as also *Pinus taurica*, which grows in the Crimea. The cultivation of the hardier and more valuable species of these genera was strongly recommended from the results of the experiments of the Duke of Athol, who had found that timber of sufficiently good quality for the ordinary consumption of the navy might be grown at 1-140th the expense of oak, taking into consideration the rental of the land, and the ground occupied, besides the vast value given to the land by the fertilizing properties of the larch. The author estimated that 100,000 acres of waste, taken from the Grampian hills, for the growth of larch, would, in two generations, not only supply all the ordinary wants of the country, but enable us to export the timber. In the west and south of England the *Pinus Laricio* and *P. Hispanica* would probably succeed best; the cedar of Lebanon might also be tried in these districts. He also recommended the larch to be cultivated by the proprietors of cold clay land in the north of England, as a means of improving the land by the deposition of its spiculæ, the trees being kept open for the admission of sheep for fifteen or twenty years, when the trees being gradually thinned, open woodland would be formed, the soil of which would be good. No other species of tree should be mixed, as the larch is recommended merely as a fructifier or ameliorator of the soil.

On Lycopodium Lepidophyllum. By G. B. Sowerby, *F.L.S.*

Upon instituting a comparison between specimens in his possession and the description and figures of Hooker, in his *Icones Plantarum*, t. 162, 163, Mr. Sowerby found them to agree so nearly that he has no doubt these specimens belong to *Lycopodium lepidophyllum*: but there are some points of difference.

The first point of difference is in the disposition of the stalks, as Hooker calls them, or rather of the stalk as it appears in Mr. Sowerby's plant; for Hooker says of his *Lycop. lepidophyllum*, "caulibus plurimis, cæspitosis, stellatim dispositis;" whereas in Mr. Sowerby's plant the stalk is *spiral* and very much branched.

The next point of difference, and this must indeed be regarded as a very trifling one, is in the form of the stipules, which Hooker describes as "folio subsimilibus;" whereas in the specimens and in Hooker's figure they appear to be more pointed than the leaves.

The third and last point of difference is in the form of the fructifying spikes, which Hooker describes in his plant as "acute triquetris;" while in Mr. Sowerby's plant they are *four-sided* and *acute-edged*.

Hooker says, "This plant in South America has long enjoyed such a celebrity, from its remarkable hygrometric property, that specimens form an article of commerce between Mexico and Peru. Like the *Anastatica Hierochuntica*, or famous Rose of Jericho, in a dried state, the stems and branches are incurved, so that the whole plant forms an elastic ball; on being moistened, the stems and branches spread out horizontally, and this experiment may be repeatedly performed."

One of Mr. Sowerby's specimens was presented to him by Mr. Cuming, who gave an equal weight in gold for the specimen which he furnished to Sir W. Jackson Hooker.

On Vegetable Monstrosities. By the Rev. W. Hincks.

The author made some introductory remarks on the importance of the study of monstrosities, and concluded by a distribution of them into five classes:—1. Cases of coherence and adherence of parts not usually united, or of separation of those which are ordinarily connected. 2. Anomalies depending on the comparative development of parts of one circle. 3. Anomalous transformations of organs. 4. Monstrous exuberances of growth, by which the number of parts is altered, independently of transformation, the number of circles of parts is increased, or the axis irregularly extended. 5. Anomalous abortions or suppressions of parts usually present in the species. Among the monstrosities produced belonging to the first class, were a specimen of *Convallaria multiflora*, in which the two lowermost leaves cohered by their edges into a sort of bag, which considerably obstructed the growth of the stem; a specimen of *Tulipa Gesneriana*, in which the leaf on the stem, folding round it, had cohered by its edges, so as completely to inclose the flower-bud, which, as it enlarged, carried up the upper part of the leaf, like the calyptra of a moss, or the calyx of Escholzia, and some adherent flowers, of which a specimen of *Salpiglossis straminea* was remarkable for the complete union of two flowers, so as to have but one calyx and corolla each, with a double number of parts. In the third class, a specimen was exhibited of *Campanula rapunculus*, with the bell-shaped corolla transformed into five additional stamens; and one of *Lilium longiflorum*, with the stamens partially transformed into pistils, a stigma being produced at the extremity of each, whilst an imperfect anther was borne lower on the filament. Various other examples were produced in the several classes, which cannot be particularly noticed.

An Account of an Inosculation observed in two Trees. By Mr. Wallace.

MEDICAL SCIENCE.

Observations on Plague and Quarantine, made during a residence in the East. By Dr. Bowring.

The opinions and practices of the people of eastern regions much exposed to the ravages of the plague were narrated by Dr. Bowring, who drew from his observations the conclusion that plague was not contagious, and that quarantine laws were of no avail in checking its progress.

The following information was communicated to Dr. Bowring, by a physician of long experience, in answer to a series of direct queries. The plague is indigenous in Egypt, never entirely absent, never imported; it *frequently* occurs spontaneously, and cordons afford no security against its diffusion. While contact very frequently does not produce it, and it is not occasioned by linen which has been exposed to the infection, the most cautious often suffer from it. Free ventilation is effective in checking the disease, and when a number of persons exposed to its influence remove from the infected spot, the mortality amongst them becomes much diminished.

On the Origin and subsequent Development of the Human Teeth. By Mr. Goodsir.

The author has observed dentition commence by the formation of what he denominates the primitive dental groove, on the floor of which the rudiments of the pulps of the milk teeth appear as globular or conical papillæ; septa afterwards pass from the outer to the inner side of the groove, between the papillæ, and thus each of the latter becomes situated in an open-mouthed follicle, which is the primitive condition of the future sac. After the formation of the milk follicles, the lips of the groove still remain prominent; and when in this condition Mr. Goodsir denominates it the secondary groove. The rudiments of the ten anterior permanent teeth appear as little depressions in the secondary groove, internal to the mouths of the milk follicles. The papillæ of the milk teeth now begin to be moulded into the form of pulps, a change which is synchronous with the closure of the mouths of the follicles by two or more laminæ, which agree in number, shape, and position with the cutting edges and tubercles of the future teeth. The lips and walls of the secondary groove now adhere, except in the situations of the ten depressions for the permanent teeth, and for a small extent posteriorly on each side, where a portion of the primitive dental groove remains in its original condition. In this portion the papillæ and follicle of the first large molar tooth appear, and, after it closes over, the lips of the secondary groove above it adhere, but not the walls; so that there is in this situation a cavity which produces the sacs of the two posterior permanent molars. The first large grinder may, therefore, be considered in some measure a milk tooth. The author observes, that dentition begins, and is always in advance, in the upper jaw, except in the case of the incisive teeth, which, although they appear first, are later in coming to perfection. This he explains by the tardy development of the lateral elements of the intermaxillary system. The author divides dentition into three stages. The first is one with which the author states anatomists have hitherto been unacquainted,—viz. the follicular. The second and third they are familiar with—the saccular and the eruptive. From his researches, he concludes that the human teeth originate from mucous membrane, that the permanent teeth have no connexion with the deciduous set, and that the sac and pulps must be referred to the class of organs denominated bulbs. He anticipates the discovery of the follicular stage in the dentition of all animals, and if so, that it will explain the varying and complicated forms of the pulp and sacs.

Experiments and Observations on the Cause of the Sounds of Respiration. By Dr. Spittal.

The object of this communication was to show that the theory of Laennec, in regard to the cause of the respiratory sounds,—viz. that all those known by the terms vesicular, bronchial, tracheal, as well as cavernous and amphoric respiratory murmurs, are caused by the friction of the air against the parietes of the air cells, bronchial tubes, trachea, and of cavities of different dimensions,—has never been proved; that the few experiments which have been advanced in support of it, are far from establishing the conclusions which have been deduced from them; and that it is highly probable that, according to the theory of M. Beau*, these sounds either owe their existence to, or are in part produced or modified by, the transmission or reverberation of a sound which takes place in the superior respiratory passages, and which has been termed by M. Beau the "guttural" respiratory sound. In support of the first theory, it was observed, that the best and almost the only experiment was that of Magendie, in which air was blown into the lungs by means of a pair of bellows, and sounds, resembling the respiratory murmur, were perceived; from which M. Magendie drew the conclusion, that because air passed to and from the lungs during this experiment, as well as during respiration, therefore the respiratory sounds are produced by the friction of the air against the parietes of the bronchial tubes and air cells of the lungs. It was stated that the similarity between the sound produced by a pair of bellows and the guttural sound was admitted by Laennec; and that it was also observed that a similar sound could be produced by blowing air through almost any tube; differing in tone and degree according to the diameter or shape of the opening in the tube, the force with which the air is made to issue from it, and the nature of the materials of which it is composed. The experiments of M. Beau, in support of his particular theory, it was noticed, were open to objections, and did not seem to bear out very clearly the conclusions at which he arrives; which may perhaps account for the neglect his view of the subject has met with. For the purpose of obviating these difficulties, and showing in a more distinct manner the probable truth of this theory, to a certain extent at least, several experiments were devised by Dr. Spittal. In these experiments *no stream* of air was allowed to pass through those parts which were the subject of observation; they were only allowed to become, and remain, distended with air; while, at the same time, the

* Archives Générales, Paris, 1834.

sound produced by the issuing of the air from an air-condensing apparatus, or from the mouth,—which very nearly resembled that of the bellows, and the guttural respiratory sound,—was observed to have passed freely, in one experiment, throughout an artery of eighteen inches in length, and to be perceived very nearly, if not quite, as loud in this as in another artery connected with it, and through which a current of air passed. In another experiment, in which the lungs of a lamb were used, sounds analogous to the tracheal, bronchial, and vesicular respiratory murmurs were distinctly perceived, although no current of air passed along the air tubes or cells; and in the case of a bladder attached to one of the great bifurcations of the trachea, a sound louder than that in the bronchial tubes was perceived, when the former was contracted to about an inch and a half or two inches in diameter; feebler when larger, and assuming, as its size was increased, a gentle, shrill, ringing, amphoric character. Dr. Spittal's experiments were not advanced to prove that the guttural sound, or that which takes place in the superior respiratory passages, is the only source of the respiratory murmurs; but to show that in all probability it exerts a considerable influence, if not in producing, at least in modifying, the different respiratory sounds, known as the vesicular, bronchial, tracheal, cavernous, and amphoric respiratory murmurs, all of which have hitherto been explained according to the views of Laennec.

On the Medicinal and Poisonous Properties of some of the Iodides. By Dr. A. T. Thomson.

The principal preparation whose action was detailed was the iodide of arsenic. Different modes of preparation were pointed out, the characters of the substance described, and specimens exhibited. The action of this medicine in very minute doses, namely, from one-eighth to one-third of a grain, was stated to have proved peculiarly serviceable in lepra vulgaris, and chronic impetigo. A case of numerous tumours resembling carcinoma, dispersed under the skin, especially that over the mammæ and in the axillæ, was found to yield to its continued action, and it was found equally successful in a more decided case of incipient carcinoma. Its action as a poison when given in an overdose was minutely detailed in a series of experiments on dogs; the effects being very similar to those of arsenious acid. Coloured drawings of the morbid effects of this substance on the alimentary canal were exhibited. When injected into a vein, its effect was to destroy life, by destroying the irritability of the heart.

On the Placental Souffle. By Dr. Adams.

The author detailed some remarkable stethoscopic phenomena occasionally heard in connexion with placental souffle.

Experimental Investigation into the Functions of the Eighth Pair of Nerves. By Dr. J. Reid.

This communication was a continuation of the paper which the author laid before the last meeting of the Association, and was chiefly confined to the functions of the gastric and pulmonary branches of the nervus vagus.

From a great number of experiments upon the effects of division of the nervus vagus, it was observed that no dyspnœa was induced, if a sufficient quantity of air reached the lungs, and that the only constant and invariable effect of division of both vagi nerves was a great diminution in the frequency of the respiratory movements. Though the nervus vagus is, however, the principal exciter of the respiratory movements, it is not the sole nerve which transmits to the medulla oblongata the impressions which excite the movements of respiration; for in several experiments performed to ascertain this point, the respiratory movements continued (though much diminished in frequency) after the removal of the hemispheres of the cerebrum and the lobes of the cerebellum, and the division of the vagi and recurrent nerves. Dr. Reid believes that all the morbid changes observed in the lungs after death are to be explained by the effects of the diminished frequency of respiration.

Several experiments were related to prove that digestion is not necessarily arrested after division of the vagi. These appeared satisfactory to Drs. Alison, Knox, and others who witnessed them.

Experiments were related to prove that narcotic poisons produce their deleterious effects as rapidly when injected into the stomach after the division of the vagi as when these nerves are left entire.

Experiments were brought forward to show that division of the vagi nerves previous to the introduction of a poisonous dose of arsenic into the system, does not arrest the usual mucous and watery secretions from the inner surface of the stomach and intestines.

The results of a great number of observations were stated to prove that the contraction of the pupil, and the half-closed state of the eyelids, which accompany section of the vagi in those animals in which the sympathetic is intimately conjoined with the vagus, are not the result of the inflammation of the conjunctiva, but are independent of this circumstance.

On the Beneficial Effects of Mercurial Action rapidly induced, more especially in certain forms of Neuralgic Disease. By T. M. Greenhow, *one of the Surgeons to the Newcastle Infirmary.*

The purpose of this paper is to show the greater safety and efficiency, in many forms of disease, of introducing into the system such quantities of calomel combined with opium, in repeated doses, as will secure the early production of the specific action of this medicine, as evinced by tenderness of the gums.

The author maintains that in various diseases, and more especially in severe neuralgic complaints, characterised by acute paroxysms of suffering of a periodic character, the efficacy of this method of employing mercury is found strikingly beneficial.

Two cases of this description of disease were adduced by the author in proof of the correctness of his views.

On the Functions of the Rete Mucosum and Pigmentum Nigrum, in the Dark Races of Mankind. By R. M. Glover.

The paper of Sir Everard Home, published in the Philosophical Transactions for 1821, is the first attempt to investigate the subject experimentally. This author attributed the power of resisting the solar heat, manifested by the dark races, to a property possessed by dark surfaces of destroying the scorching and blistering effects of the solar rays on the skin. Thus, according to him, were a black and a white skin exposed to the same degree of solar heat, the former should rise to the higher temperature, yet inflame the least.

Mr. Glover endeavoured to prove, that the experiments of Sir Everard Home are incorrect; and that a black surface does not only rise to a higher temperature than a white one under the sun's rays, but also scorches and blisters the skin in a greater degree. He further attempts to show experimentally that the scorching and blistering effects of white and black surfaces are precisely in the ratio of their powers of absorbing heat, a conclusion which is entirely opposed to the opinion of Sir Everard Home, who supposed that the rays of luminous caloric can blister the skin in a degree greater than what is accounted for by the quantity of heat contained in them.

But although the skin of the black may absorb more heat than the skin of the white man, and although we are unable to explain the superior tolerance of heat by the possessor of the former in the mode adopted by Sir Everard Home, yet it is established that the organization of the inhabitant of the tropic, and especially of the negro, is peculiarly fitted to enable him to perspire freely on the application to him of the stimulus of heat; while in the adaptation of his system to respond to this stimulus, and in the cooling effects of perspiration, which are shown by many experiments, must be sought the mode in which he is protected from the heat.

The dark-coloured skin, the author is also of opinion, must radiate at night very freely; this agrees with the well-known fact that negroes are exceedingly chilly in the nights of the tropics.

Remarks on the Skull of Eugene Aram. By Dr. Inglis.

In this communication the author endeavoured first to substantiate the fact that the skull produced was really taken from the body of Eugene Aram; secondly, that the development of the mental faculties, as indicated by the skull, corresponded remarkably with the character of Aram as recorded in history.

On the Chemical Analysis of the Liquor Amnii. By Dr. G. O. REES, *F.G.S., &c.*

The author related his experiments on four specimens of the fluid procured from different individuals, all drawn off at $7\frac{1}{2}$ months of utero-gestation. The specific gravities varied from 1·0070 to 1·0086, the proportion of solid contents being much the same in each specimen: the solid ingredients varied, however, in relative proportion. Urea was found in all the specimens, the other ingredients being albumen, fatty matter, lactates, alkaline chloride, traces of sulphate, carbonate, and phosphate of lime, with oxide of iron.

On Mr. Farr's Law of Recovery and Mortality in Cholera. By ROBERT D. THOMSON, *M.D.*

The principal facts elicited are the following:

1. The probability of *recovery* can be determined at every stage of disease by a simple tabular construction.

2. The mean future duration can be determined at any given point.

3. The *rate of mortality*, deduced day by day, shows that the fatality increases up to a given point, then becomes stationary, and afterwards decreases, according to a determined law. The rate of mortality attains its maximum at different periods in different diseases: in *cholera* it is at its maximum in twenty-two hours (eighteen to twenty-four hours); in *small-pox* in ten to fifteen days; in *phthisis pulmonalis* in six to nine months. When the disease has attained its acme and begins to decline, the *rate* of mortality on any day being given, the rate of mortality on any *future day* can be calculated, and *vice versâ*. In cholera the rate of mortality declines nearly 12 per cent. daily, from the 4th to the 30th day: in its course the diagram describes a regular curve, which will represent in space what takes place in time. The rate of decrease varies in small-pox, but the variation is regulated by a certain law.

4. The *mortality* no doubt *increases* according to a determined rate; but in cholera it attains its maximum so rapidly, that the law of *increase* cannot yet be determined. The rate must be determined at four or five equal periods in succession before the law of its changes can be ascertained: this has not yet been done in any disease.

5. The rate of *recovery*, like the rate of mortality, follows a prescribed rule: it *increases* according to a determined law, which has to a certain extent been determined in cholera and small-pox.

For the purpose of this investigation, the cases of each disease should be recorded in the following form:

CHOLERA EPIDEMICA.

	Age.	Date of Attack.	Date of Termination.	Duration.
		1837.		
Edward Evans	30	June 21.	Died, June 21.	5 hours.
Sarah Mills ...	25	June 22.	Recovered, June 26.	4 days.

Care should be taken to fix distinctly the time of invasion and the time of recovery, whether it imply the time when the patient can first digest food, or resume his ordinary occupation. The time of death is easily determined. The cases of small-pox and cholera comprehend all the diseases to which they give rise, or which follow in the same uninterrupted series of morbid phenomena. The nature of the consecutive diseases should be recorded. The author is anxious to impress strongly upon every practitioner the utility of this simple registry of diseases, following them from the beginning to the end of their course. The results already obtained prove that the harvest will be rich: they show the advantage too of extended observations, carried on by numerous observers, on a uniform plan. No one person could have observed 900 cases of cholera, and the law could not have been deduced from a small number of observations.

Several practical inferences are suggested by this investigation.

1. It demonstrates the important fact, that pathological phenomena are as regular in their course as physical phenomena observed in inorganic matter; and that the instruments of physical investigation are applicable to medicine: for, after due allowance has been made for errors of observation and the limited number of cases, it will be found that the facts can be as exactly expressed by formulæ, as any facts in the province of natural philosophy.

2. A new field will be opened to the mathematician, and many interesting problems will arise for solution when accurate observations have been collected. If the abstract sciences are every day descending to practical applications, the empirical arts are also rapidly rising into the region of knowledge.

On Sleep, and an Apparatus for promoting Artificial Respiration. By JOHN DALZIEL, *M.D.*

In an essay on sleep drawn up in 1833, which constitutes the first part of this communication, the author directed particular attention to the dependence of this state on the feeling of fatigue in the muscles of respiration. The effects which follow this feeling are, diminished action of the organs and function of respiration; diminished action of the organs and function of circulation; diminished supply of arterial blood in a given time to the brain; and, finally, sleep as an immediate consequence of the latter condition. In the subsequent part of the paper, Dr. Dalziel describes an apparatus for ascertaining the practicability of promoting artificial respiration, as a remedial means in certain states of the system.

Some time ago it occurred to the author, that certain diseases, accompanied with depression, might be mitigated, and other depressed states of the system effectually remedied, by immersing the limbs and trunk of the body in air which should be alternately rarefied and recondensed, at the same time allowing the patient to inhale the air of the external atmosphere; the rarefaction and recondensation to correspond with the motions of inspiration and expiration respectively. By this means, it was expected the function of respiration might be directly supported and the general system invigorated. Inspiration being assumed to be the more laborious part of the process of respiration, and expiration to require little or no effort, it is the former part of the process that in depressing affections requires assistance.

The degree of assistance which might in these cases be afforded, could, Dr. Dalziel supposed, be tested by experiments on persons in health, with apparatus of very simple construction. That which he made use of consisted of an air-tight box, large enough to contain the person to be experimented on (the head and neck excepted,) in a sitting posture, and a pair of circular bellows inside, which were used as a forcing air-pump. The bellows were worked from without by a piston rod, and the air which at every stroke they discharged was prevented by a valve from returning. In the side of the box were two small convex windows; one for the admission of light, the other for allowing an attendant to inspect the surface of the body during the experiment.

When, by this apparatus, the pressure of the air was, to a certain extent, removed from the parietes of the chest, the feelings of exertion and repose attending respectively on inspiration and expiration were completely interchanged. The comparatively heavy air of the external atmosphere, which the person breathed, rushed along the air-passages and distended the chest without effort. There was a prevailing disposition to inspire. When the respiratory muscles were relaxed, the chest remained permanently distended, and a sensation of fulness of this cavity was distinctly experienced. Expiration on the other hand became difficult and laborious. The feeling in the chest attendant on the effort was analogous to that which is experienced in ordinary circumstances during inspiration, while supporting with the hands a heavy weight upon the breast, and attempting to elevate it. The voice in the mean time became so weak as to be almost inaudible.

In order to produce the results above-stated, the air contained in the box was rarefied by abstraction of about one-nineteenth of its volume.

Dr. Dalziel suggested the application of this apparatus in all diseases and affections of the system in which the functions of respiration and circulation require to be roused or supported.

A model of more refined apparatus to perform the same effects, and several testimonials respecting the period of the invention, (1832,) were presented to the Section.

On an Improved Acoustic Instrument. By Dr. YELLOLY, *F.R.S.*

This communication was illustrated by a model of the instrument which Dr. Yelloly proposed for the purpose of assisting in cases of partial deafness. Allusion was made to the very defective nature of our present instruments, both as to utility and conveniency, and the importance of appointing some experimental investigation on the subject *.

On the Action of various Substances on the Animal Economy, when injected into the Veins. By J. BLAKE.

The author described a number of experiments with various substances, and their effect on the vascular system, as measured by an instrument which he termed a Hæmadynameter, formed by a glass tube, bent at an angle. One limb of this tube being attached to a scale, allows of measuring the height to which a column of mercury is raised by the action of the current of blood in the artery, into which the extremity of the other branch is introduced. The substances introduced, in solution, into the veins, were divided into three classes according to their effects. In the *first* were those which produced death, by directly acting on the contractility of the heart, amongst which were nitrate of potassa, arseniate of potassa, sub-carbonate of soda, biniodide of arsenic, oxalic acid, and solution of galls; all these acted locally on the heart, and agreed in effecting a change in the colour of the blood, turning it black, probably by forming definite combinations with its constituents. A remarkable difference was observable in the effects produced by the same substances when absorbed from the stomach. In the *second* class were those substances which acted directly on the nervous system; such were strychnia, hydrocyanic acid, and conia. And in the *third* were those producing death by affecting the capillary circulation; such were tobacco, euphorbium, and digitalis. The last two classes of substances did not produce any change on the composition of the blood. Several other substances were experimented with, not falling under the above classes, such as morphia and cantharides, the effects of which were the same, and nitric acid: when the latter was injected into the vein, the column of mercury in the instrument fell from seven inches to one; and after death, the right side of the heart was distended with solid blood.

On an Improved Stethoscope. By A. B. GRANVILLE, *M.D.*

By this invention the patient may be examined without the necessity of his rising, and the necessity of having the practitioner's head immediately parallel to the part examined is avoided. These advantages are effected by the addition of a half ball-and-socket joint attached to

* A recommendation to this effect was adopted by the General Committee.

the ear-piece, which of course becomes moveable to a greater or less angle with the cylinder, as circumstances may require.

Mr. Baird detailed a case of successful excision of the elbow joint. The patient was presented to the Section, and considerable motion was shown to exist in the joint; so much so, as to enable him to pursue his ordinary occupation in a glass manufactory.

On Fractures. By T. M. Greenhow.

In illustration of this communication a model of a new sling fracture bed was introduced, applicable to every fracture in the lower extremity, but peculiarly adapted to the treatment of compound fractures of the femur. The following advantages were attainable by this apparatus:—1st, Ease of position; 2nd, Easy and gradual extension by means of a screw at the termination, beyond the heel of the patient, whose action was connected with the ancle joint and instep; 3rd, Facility of examining and dressing the limb in cases of compound fracture, without disturbing the fractured ends; 4th, The freedom of slight motion enjoyed in such a way as to be of no injury to the process of reparation. Mr. Greenhow detailed some interesting cases treated with this apparatus, demonstrating its peculiar advantages.

Case of Anthracosis in a Lead Miner. By Dr. Crawford.

The attention of the profession was first drawn to the subject of anthracosis, or black infiltration of the lungs, in 1831, by the late Dr. James C. Gregory, and subsequently other cases have been published in the Edinburgh Medical and Surgical Journal.

Dr. William Thomson, of Edinburgh, has also published a paper, in the London Medico-Chirurgical Transactions for 1836, on black expectoration and black matter on the lungs.

The subjects of all the cases made public have been either coal-miners or moulders in iron work, whose occupations have seemed to give countenance to the opinion that the disease originated either from the inhalation of coal-dust, gun-powder smoke, lamp smoke, choke damp, or the impure air of mines, consisting of a mixture of carbonic acid gas and the atmospheric air. In some of these cases there was, during life, dark-coloured expectoration with evidence of serious organic disease of the lungs, *e. g.* phthisis; in others, there was bronchites, without expectoration of this dark matter; in a third class there were bronchites and emphyseura of the lungs, likewise without the dark-coloured matter; and in the fourth and last class there was found, after death, the black matter, no symptom nor sign of chest affection of any kind having been present during life.

The author presented a short report of a case which disproved the opinion that this deposit is found only in coal miners and moulders in iron work. The individual was William Ritson, æt. 77, many years inmate of the Tynemouth Union Workhouse. He had been employed from an early period of life as a lead miner, continuing at the employment till within ten or twelve years of his death. During the middle period of his life he had been at sea for five years.

On the Amount of Air required for Respiration. *By Dr.* D. B. Reid.

From a very extensive series of experiments made upon the respiration of upwards of a hundred individuals, who placed themselves successively in an apparatus for this purpose; from trials made upon greater numbers varying from 3 to 234, in apartments specially constructed for the purpose; and from observations made under his direction in the House of Commons every day that Parliament had met during the last two sessions; Dr. Reid contended that the amount of air usually allowed for respiration, in public buildings and private dwelling-houses, was far below the standard required for sustaining either the bodily or intellectual faculties in health and vigour. He remarked that great errors in estimates upon this point had arisen from a variety of causes; more especially—

1. From the extreme difficulty of calculating and regulating precisely the supply of air in apartments constructed in the usual manner.
2. From the supply of air having been determined hitherto not by precise experiments upon the person, but from calculations, which, from the state of science, are at present necessarily imperfect.
3. From the amount of air required to sustain the functions of the skin, and to facilitate by gaseous diffusion the removal of the matter of insensible perspiration having been in a great measure overlooked.
4. From neglecting the influence which even an excessively minute quantity of some gaseous and volatile substances diffused through the atmosphere may exert in gradually undermining the system.

Dr. Reid's communication contained a variety of details in reference to the constitutional peculiarities of different individuals in respect to air, and he contended that they differed as much in this respect as in reference to food and drink, exercise, temperature, clothing, &c.

In adverting to the influence of heat, light, and electricity, he brought forward a number of instances showing that the effect of light upon the human constitution is as important in its action as in the power it is known to possess upon the vegetable kingdom, and referred more particularly to a case pointed out by Sir James Wylie, in one of the largest barracks, where there were THREE cases of disease among the soldiers whose apartments looked to a dark and dull court, for ONE among those who were necessarily exposed to a bright light, the temperature, food, clothing, and discipline, being precisely the same.

In concluding, Dr. Reid contended, 1. That the supply of air should amount at least to eight or ten cubic feet per minute in an atmosphere at ordinary temperatures.

2. That the amount of supply should increase greatly with the temperature. In the House of Commons he had never given less than thirty cubic feet for each individual when very crowded, and on one occasion he had supplied sixty cubic feet for each member for three weeks successively.

3. That the same attention should be paid to the moisture in the air as to the temperature, and that the hygrometer is as indispensable in providing a proper atmosphere as the thermometer and anemometer: 5000 feet of moist surface were used at the House of Commons.

4. That the air may be filtered from suspended impurities, and in many local situations others may be separated with extreme facility.

5. That from the pernicious effects of minute quantities of impurities acting for a long period, it is desirable in providing artificial light to exclude hermetically from every apartment all the products of combustion.

On the Modus Operandi of Nitrate of Silver as a Caustic and Therapeutic Agent. By Robert D. Thomson, *M.D.*

When a solution of animal matter, (for example, a solution of what is usually termed albumen, obtained from the eggs of birds,) is added to a solution of nitrate of silver, white coagula are immediately precipitated resembling chloride of silver, but differing on closer inspection, as under the microscope, considerably from the latter salt. When the precipitation is induced in considerable quantity, the upper portion of the deposit soon begins to turn darker coloured, and gradually assumes a brownish appearance. The lower portion of the precipitate, however, still retains its original aspect with the addition of bands of dark matter, which traverse it in different directions. In addition to this we observe, that it becomes matted together and has a stringy consistence approaching to that observed in the incipient formation of the mucous membrane. Having added an excess of animal matter to the nitrate of silver solution, the author threw the precipitate on a filter and washed it repeatedly with distilled water. On testing the liquid with a solution of the animal matter no precipitation ensued, demonstrating that nitrate of silver in its original form no longer existed in the solution, but had been entirely removed, or at least its properties obscured, in consequence of the addition of the animal matter. To determine, however, whether any silver existed in the fluid, the latter was evaporated on the sand bath. The solution gradually became dark-coloured, and the evaporating basin in which the experiment was made was coated at the upper surface of the fluid with a brown deposit. On the total evaporation of the solution, a brown glazed-looking matter covered the bottom of the vessel, which, on exposure in the cold, gradually absorbed moisture from the atmosphere, and was readily scraped off from the vessel in the form of a moist powder. These experiments show that there are two compounds of albumen and nitrate of silver, one of which is insoluble and the other soluble in water—the one being an acid and the other a basic compound. The author proceeded to state the result of his endeavours to ascertain the operation of nitrate of silver upon the animal economy. After numerous experiments upon the secretions of the mucous membranes of the œsophagus, stomach, &c., he found that precisely similar compounds are formed by these fluids,—a circumstance which admits of ready explanation, if we consider these fluids as merely solutions of some modification of albumen. The same compounds are formed by bringing the nitrate of silver in contact with the cutis, which is generally considered to consist of gelatin, although it possesses most of the properties of albumen. Until, however, we know what albumen is and are acquainted more accurately with the nature of the causes which give rise to its modifications, it will be in vain to attempt to assign any definite composition to these compounds; we can only study their physical and chemical properties; but this we can do most efficiently so as to render the facts of great importance in a therapeutic point of view. From the facts ascertained by the author in respect to the compounds of animal matter and nitrate of silver, he has drawn the following conclusions:—

1. That nitrate of silver acts as a caustic by combining with the animal matter of the textures to which it is applied, probably in definite proportions. The compounds are partly soluble and partly insoluble in water, which renders them readily separable from the surface on which they are produced. Cauterization is, therefore, the removal of a portion of organized matter by chemical means.

2. When nitrate of silver is taken into the stomach, chemical compounds of a similar nature are formed with solid matter dissolved in the secretions, and with the food contained in that organ. Hence, no nitrate of silver can ever reach the blood as nitrate of silver.

3. The author pointed out the importance of studying the action of these compounds upon the constitution, and the fallacy of supposing that nitrate of silver can act *per se* upon the animal œconomy.

Observations upon Uterine Hæmorrhage and Practical Hints on the best mode of arresting it. By R. TORBOCK.

After noticing the circumstances attending uterine hæmorrhage, the difficulty of sometimes restraining it, and the remedial means devised and recommended by the best writers, Mr. Torbock described an instrument which he had invented and employed with success in cases of this nature. The instrument for this purpose is simply an India-rubber bag, so prepared that it may be greatly distended. When introduced into the vagina (or even into the uterus if necessary), it will, by its perfect adaptation, preclude the possibility of blood escaping, and form an effectual plug. Cases were detailed in support of this statement.

On the occurrence of Crystals in the Human Intestines. By O. B. Bellingham, *M.D.*

The author prefaced a description of the circumstances attending a case of this nature by a short review of the previous observations of Ehrenberg, who found microscopical crystals in the meconium; Schönbein, who found small crystals in the intestinal discharges of typhus patients, and imagined they might be considered as a diagnostic of typhus; and Müller, who detected them in persons that died of various disorders.

Dr. Bellingham's case was that of an individual (male), æt. about 40, who died of pleuropneumonia and gastritis, in St. Vincent's Hospital, Dublin, after being admitted only a few days. On examining the intestinal canal, the contents of the colon were found to be very fluid, and of a lighter colour than usual. Suspended in the contents were numerous small hard parts, which proved to be crystals, somewhat less than the third of a line in length. Their colour was white (superficially yellowish), their form a slender four-sided prism, terminated by four-sided pyramids. On analysis, Dr. Apjohn found them to be composed of phosphate of ammonia and magnesia, or the triple phosphate. They were found only in the colon.

The author remarks, that the ammoniaco-magnesian phosphate found in the urinary bladder is usually formed in short three-sided prisms, terminated by three or six-sided pyramids.

The composition of the crystals found by Schönbein was very different from these, consisting chiefly of phosphate of lime, some sulphate of lime, and a salt of soda, and presented the appearance of rhombs or rhombic prisms. In one case, however, he found four-sided prisms, and Dr. Bellingham remarks, that the triple phosphate has been frequently noticed in the intestinal secretions of quadrupeds, as by Fourcroy, Vauquelin, and Marcet.

On Abscess of the Lungs. By Thomas Barnes, *M.D.*

Regarding the frequency of this disease, a difference appears between the statements of ancient and modern medical authorities, the former affirming it to be of common occurrence, the latter uniformly asserting its rarity. The formation of an abscess in the lung being now considered so very rare, Dr. Barnes thought a brief account of two cases which lately occurred to himself worthy the attention of the Meeting.

The first case was of a gentleman 40 years of age; his disease originated in influenza (Feb. 1837). He was of a sound and healthy constitution, without any predisposition to phthisis or pulmonary disease, and in Aug. 1838 he had recovered; but the symptoms he had suffered left no doubt in the minds of Dr. Barnes, Dr. Headlam, and Mr. Edmonson of the true nature of the disease.

The second case is that of a stone-mason, aged 45, whose illness commenced on the 28th March, 1838, and was attributed by himself to working in warm rooms, and throwing off his clothes when heated. His disease proved fatal on the 18th May.

The symptoms of treatment of each case, and the *post mortem* examination of the latter by Dr. Elliot, were fully described.

On the Structure of Teeth, and the resemblance of Ivory to Bone, as illustrated by microscopical examination of the Teeth of Man, and of various existing and extinct Animals. By Professor Owen, *F.R.S.*

Mr. Owen commenced by showing, that he had availed himself of the advantage afforded by the British Association, viz. that in the communications brought before a sectional committee, a fuller and more detailed retrospect of the progressive steps which have led to any remarkable discovery, is not only permissible, but peculiarly congenial to its general views and objects; and he therefore entered into a full detail of the recent investigations, especially those of Purkinje, Müller, and Retzius, on the intimate structure of the teeth, and particularly dwelt on the discoveries of the latter author, as regarded the structure of the human tooth. After describing the mode of arrangement of the particles of the earthy salts, which characterizes true bone, Professor Owen proceeded to state, that until a very recent period the analogy of tooth to bone was supposed to extend no further than related to the chemical composition of the hardening material, while the arrangement of this earthy constituent, as well as its mode of deposition during the growth of the entire tooth, were considered to be wholly different from that of bone, and to agree with the mode of growth of hair, and other so-called extra-vascular parts, with which teeth in general closely correspond in their vital properties. He observed, that the supposed proofs of the laminated structure of teeth, derived from the appearances presented by the teeth of growing animals, fed alternately with madder and ordinary food, and by those which often occur during the progress of decomposition of certain teeth, which are then resolved into a series of concentric or superimposed laminæ, were equally applicable to true bone, and were quite unavailable in illustrating the point under consideration; and that the appearances presented by the superficies of vertical sections of teeth, viewed with the naked eye or a low magnifying power, were due, not to the intervals of separate and superimposed lamellæ, but to the different refractions of light, caused by the parallel undulations or alternations of structure of minute tubes proceeding in a contrary direction to the supposed lamellæ. This apparent lamellated structure, however, is not constant, nor equally plain in different teeth; on the contrary, the fractured surface, or the polished section of the human and many other teeth, presents a silky or iridescent lustre, which has attracted the attention of several anatomists. Professor Owen observed, that Malpighi, in whose works may be detected the germs of several important anatomical truths, which have subsequently been matured and established, conceived that the teeth were composed of minute fibres reticularly interwoven; and Leewenhoek, in

1683, had discovered that the apparent fibres of tooth were, in reality, minute tubes. The tubular structure of ivory was rediscovered by Purkinje and Fraenkel, in 1835, and the disposition of the tubes is accurately described and figured by them in the different kinds of human teeth. In these descriptions the tubes are spoken of according to their *prima facie* appearance as fibres, but their true nature is explained in different passages of the work*. Purkinje and Fraenkel also added to Dental Anatomy several new and interesting facts relating to the structure of the enamel, pointing out more especially the form and characteristic transverse striæ of the component crystals: and, lastly, they determined the true osseous nature of that distinct layer of substance which had been previously known to surround the fang in the teeth of man, and which they once observed to be continued upon the enamel of a human incisor. This observation, Mr. Owen proceeded to state, he had confirmed, and he exhibited several sections of the simple teeth of the Mammalia in which both the ivory and enamel were invested by a layer of osseous substance, identical in its structure with the cement which enters more abundantly into the composition of the compound teeth of the Herbivora.

The interesting experiments of Professor Müller, on the nature and contents of the *dental tubuli* were then noticed; and, lastly, a condensed analysis was given of the laborious and accurate microscopical observations of Professor Retzius, as related in the original Swedish memoir of that author on the structure of teeth. Besides confirming the fact, that the ivory or bony constituent of a human tooth consists of minute tubes lodged in a transparent medium, disposed in a radiated arrangement, with the lines proceeding in a direction perpendicular to the superficies of the tooth, Professor Retzius has more particularly observed and described the dichotomous branching of the primary tubes; the minuter ramuli sent off throughout the course of the main tubes into the clear interspaces; the calcigerous cells with which those fine branches communicate; the terminal ramifications of the tubuli, and their anastomoses with each other, and with calcigerous cells at the superficies of the ivory or bony part of the tooth. Professor Owen also discussed the opinion advanced by Professor Retzius, as to the function of this elaborate contexture of branched and anastomosing tubes and cells, in conveying, by capillary attraction, a slow current of nutritive or preservative fluid, through the entire substance of the tooth; which fluid might be derived either from the superficies of the pulp in the internal cavity of the tooth, or from the corpuscles or cells of the external layer of cortical substance or *cæmentum*,—with the tubes radiating from which corpuscles, the fine terminal tubes of the ivory anastomose. Professor Owen concluded the critical portion of his communication, by explaining the views entertained by Professor Retzius on the analogy subsisting between tooth and bone, which analogy he then proceeded to illustrate by his own observations on the structure of recent and fossil teeth.

* *De Penitiori Dentium Humanorum Structura Observationes.* Vratislaviæ, 1835.

With respect to the component structures of a tooth, Professor Owen commenced by observing, that in addition to those usually described and admitted, there were other substances entering into the composition of teeth, and presenting microscopic characters equally distinct both from ivory, enamel, and cement, and from true bone, and as easily recognisable.

One of these substances was characterized by being traversed throughout by numerous coarse canals, filled with a highly vascular medulla or pulp, sometimes anastomosing reticularly,—sometimes diverging, and frequently branching,—sometimes disposed nearly parallel with one another, and presenting more or fewer dichotomous divisions. The canals in many cases are surrounded by concentric lamellæ, and thus resemble very closely the Haversian canals of true bone; but the calcigerous tubes which everywhere radiate from them are relatively much larger. The highly-organized tooth-substance just described differs from true osseous substance, and from the cæmentum, in the absence of the Purkingian corpuscles or cells. This structure is exemplified in the teeth of many fishes and of some of the Edentate Mammalia.

Another component substance of tooth more closely resembles true bone and cement, inasmuch as the Purkingian cells are abundantly scattered through it; it differs, however, in the greater number and close parallel arrangement of the medullary canals. This structure is exhibited in the teeth of the *Megatherium*, *Mylodon*, and other extinct Edentata.

Mr. Owen then proceeded to describe the modifications of the above-mentioned dental substances in the teeth of different classes of the vertebrate animals, of which the following examples are selected.

1st. *Teeth of Fishes.*—With respect to this class, although the lowest of the vertebrate series, their teeth present in general the most highly organized condition, approximating most closely to the vascular character of true bone, and being in many species fixed by anchylosis or continuity of substance with the bones supporting them.

It was in the teeth of fishes that, in recent times, the tubular structure had been first recognised. Cuvier*, *e. g.* describes them as presenting three different structures, of which one kind (*les composées*) are formed of an infinity of tubes, all united and terminated by a common covering of enamel; of this kind he instances the tesselated teeth (*dents en forme de pavé*), as those of *Rays*.

Dr. Born also describes what he terms the "fibrous teeth of fishes," as being composed of hollow fibres†, and he compares these hollow fibres, or tubes, to those which enter into the composition of the teeth of the *Orycteropus*, &c.

The tubes here spoken of, as well as those mentioned by Cuvier, are sufficiently large to be distinguished by the naked eye; they do not, however, form the constituent texture of the teeth instanced, but only the coarser part of that texture. They contain a vascular medulla,

* Leçons d'Anat. Comp. 2d ed. tom. iii. p. 209.

† Heusinger's Zeitschrift, B. i. p. 184. "Die Faserzähne bestehen in ihrem Innern aus hohlen Fasern," &c.

and are the centres from which the true calcigerous tubes radiate, and they are, therefore, analogous to the simple pulp-canal of the human incisor, which, with its radiating microscopic calcigerous tubes, may be compared to a single medullary canal with its corresponding microscopic radiating tubes in the *Rays*, *Orycteropus*, &c.

Myliobatis.—A longitudinal section of a single dental plate, viewed by a low power of an inch focus, exhibits at its base a coarse network of large irregular canals, filled with a vascular medullary pulp. From this network smaller medullary canals proceed in a slightly-diverging course, subdividing dichotomously with interspaces equal to six or eight of their own diameters. In a transverse section of the tooth, seen under the same power, the area of the medullary canals is seen to present generally an elliptical form, from which radiating calcigerous tubes are faintly perceptible. Each canal and its series of tubes is surrounded by a line of generally an hexagonal form, and which constitutes the boundary between contiguous canals and tubes, the whole tooth being thus composed of an aggregate of simple elongated, commonly six-sided prismatic teeth, placed vertically to the grinding surface. A section through the roots of the tooth shows that these parts are occupied by a network of irregular canals, which anastomose by arched branches with the network of the contiguous root, and these with the network of coarser tubes which occupy the basis of the tooth for an extent exceeding the length of the root itself.

With a higher power, $\frac{1}{10}$th inch focus, the calcigerous tubes are seen to radiate in all directions from the medullary canals, and are sent off throughout the whole course of the canal. The tubes are short, wavy, richly arborescent, and form numerous anastomoses with each other. The transverse sections of the tooth show that the area of each medullary canal has been filled up or diminished by the deposition of a series of concentric lamellæ.

The ramification of the tubes in this tooth presents the same general character as those of *Acrodus*, but they are shorter, and each group in the transverse section is separated from the contiguous one by the regular boundary lines above-mentioned, which distinguish the teeth of the *Myliobatis* from those of the *Acrodus*, *Psammodus*, *Cestracion*, or any of the shark tribe. The tooth of the *Orycteropus* is that which has the nearest resemblance to the tooth of the *Myliobatis*.

Acrodus nobilis.—The crushing teeth of this extinct genus are composed of two substances, viz. a thin external almost colourless layer, which represents the enamel, and an amber-coloured coarser ivory composing the body of the tooth, and continuous with and passing into the coarse cellular bony basis and support of the tooth. Microscopic sections of this tooth afford the most beautiful appearances, and, perhaps, the most instructive illustration of the relation of ivory to bone. The body of the tooth consists of groups of beautifully branched and irregularly wavy medullary canals imbedded in a clear matrix. These canals are surrounded by concentric strata, and closely resemble the canals of Havers in true bone. The calcigerous tubes, which radiate from the medullary canals, have a graceful undulatory course and are much branched; but towards the periphery of the tooth, the ramified tubes are all directed, as in true ivory, at right angles to the superficies, and thus constitute a regular layer of calcigerous tubes, disposed so as to offer the greatest resistance to pressure. This layer is equal in thickness to about one-fifteenth part of the vertical diameter of the thickest part of the tooth.

The finest or terminal branches of this peripheral layer of tubes, I have traced in various places into what at first sight appears to be the enamel. Under a magnifying power of 400 diameters, however, this outermost layer is seen to be composed of extremely minute tubes, $\frac{1}{1000}$ th of a line in diameter; they are branched like the coarser tubes of the body of the tooth; irregularly wavy in their course; having a general tendency to an arrangement at right angles to the superficies, but inextricably interwoven, and connected anastomotically together, so as to require a strong light to penetrate even the thinnest section, and render their structure and arrangement visible. The continuation of these finer superficial tubes, with the coarser tubes of the body of the tooth, is best observed by changing the focus, which brings the transitional tubes at different depths in the section into view. In some parts of the section, a medullary or Haversian canal is displayed longitudinally; and the parallel lines of the surrounding concentric strata on each side are exhibited. The canal maintains a general uniform diameter, but slightly dilates where it divides or sends off a cross branch to communicate with the adjoining canals. These canals commence from the large cells of the bone of the base, and pass into the substance of the tooth towards its periphery; communicate by transverse canals, but all ultimately terminate in bundles of the wavy ramified calcigerous tubes of the body of the tooth. I conclude that the coarser canals were occupied by a vascular pulp in the living animal, and that the fine terminal tubes were the seat of the salts of lime. The silex occupying the longitudinal canals and coarser tubes, has received a dark stain, probably from the colouring matter of the vascular pulp,—but the finer tubes, from the want of this difference of colour, are in many parts obscurely visible, if at all. They are discernible in some situations crossing the concentric lamellæ at right angles to the central canal. The chief difference between the appearance presented by the Haversian canals of the tooth of Acrodus, and those in true bone, is in the absence of the cells or corpuscles. These are apparent only at the base of the tooth—irregular in size and form, very minute, and appearing like simple granules without radiating lines. The character of the main or coarser canals and calcigerous tubes of the ivory of the tooth of Acrodus, reposes on their undulating course, their rapid diminution and branching, and the moderately acute angles at which the branches are given off, except at the circumference of the tooth, where they run nearly parallel to each other. In other parts they closely resemble the branching of trees. The line of demarcation between the coarser and finer ivory is formed by a series of small cells of a similar granular appearance to those at the base, in which many of the finer branches of the coarse ivory terminate, and from which the minute tubes of the enamel-like ivory commence. The superficies of the tooth is slightly punctated, but the depressions do not correspond with the mouths of tubes, but with the interspaces of whole groups of the coarser tubes.

Psammodus.—A transverse section of the tooth of this genus presents the appearance, under a moderate magnifying power, as if it were composed of close-set coarse tubes, the areæ of which were thus exposed. Such a section, viewed with a power of 400 diameters, shows that these tubes are surrounded by concentric lamellæ, exactly as the Haversian canals; and that these lamellæ, and the clear interspace, which is generally equal to the thickness of the lamellæ, are permeated by minute irregularly disposed tubes, which anastomose in the clear interspace, and open into extremely minute cells, scattered in the same part. A longitudinal section of the same tooth shows the whole course of the canals; they run nearly perpendicularly to the convex superficies of the tooth, and, consequently, incline outwards at the sides of the section. They lie nearly parallel with each other, with interspaces equal to from 6 to 8 times their own diameter, and branch dichotomously once or twice in their course. Each canal is surrounded by concentric layers of a dark colour, encroaching upon one-third of the interspace, which thus presents two dark streaks and one intermediate right line: the whole of these interspaces is perforated by the irregular wavy, branched, anastomosing calcigerous tubes. The terminations of the canals near the periphery of the tooth are slightly dilated, and give off in every direction calcigerous tubes corresponding to those in the interspace of the canals. The structure of the tooth of *Psammodus* differs from that of *Acrodus* in the greater number and more parallel course of the canals, their fewer branches, and want of anastomoses, and in the absence of a distinct external enamel-like layer of very fine tubes.

Ptychodus latissimus.—The structure of this tooth has a close affinity to that of *Psammodus*: it is composed of Haversian canals and calcigerous tubes proceeding therefrom. The base of the tooth is composed of close-set and irregular canals, and is very opaque: the canals emerge from this part half-way to the grinding surface, to which they proceed perpendicularly. They differ from those of the *Psammodus* in being wider, more close-set, and more branched,—the branches being given off at more open angles, and the terminal ones being larger in proportion to the trunks. The papillose surface of the tooth is composed of the terminations of the inextricably interwoven fine calcigerous tubes given off from the terminations of the canals. The interspaces of the canals are also occupied by the same minute anastomosing reticulate tube-work. Numerous minute calcigerous cells are also present in the interspaces. There is a clear substance coating the grinding surface of the tooth, in which neither tubes nor any definite structure could be detected, though, from analogy, such doubtless exist. The darker substance, forming the concentric lamellæ around the canals, occupies the same proportion of their interspace as in the *Psammodus.*

Chimæra.—The tooth of this fish appears, when a section of it is viewed with the naked eye, to be composed of a close-set series of parallel coarse tubes, dividing dichotomously, and united together here and there by short transverse arches with the convexity towards the grinding surface. The diameter of the interspaces of these canals is generally equal to between two and three diameters of their areæ.

Viewed by a higher power, the tubes are seen to be immediately surrounded by a clear amber-coloured substance, analogous to that which forms the concentric layers around the canals, which I have compared to those described by Havers in true bone.

Under a power of 400, the large canals are seen to send off from every part of their course numerous minute tubes, generally at right angles, to the medullary canals; these tubes run irregularly, ramify, and anastomose in the interspaces of the medullary canals, and form a coarse matting or plexus of tubes, the number of which sometimes quite intercepts the light.

In the teeth of the genus *Lamna,* a number of medullary canals are continued from the short and small pulp-cavity at the base of the tooth, which ramify and anastomose, so as to form a beautiful reticulate arrangement of tubes, very similar to a network of capillary vessels, throughout the whole substance of the tooth: they ultimately terminate in a flattened sinus, which seems to extend over the whole tooth at a very short distance from its superficies. The whole of the superficial part of the tooth is occupied by minute calcigerous tubes, which proceed in a wavy course, generally at right angles to the external surface; they ramify, and their terminal branches anastomose, and many of them terminate in a stratum of calcigerous cells, situated between the body of the tooth and what appears to be the outer stratum of enamel. In this stratum, however, there are evident traces of a series of much finer tubes, continued from the preceding layer of cells, which proves that this is not true enamel, but a fine kind of ivory, like that in the tooth of the sloth and megatherium. The coarse reticulate canals in the body of the tooth are surrounded by concentric layers, traversed by the calcigerous tubes which are everywhere given off at right angles from the larger canals; these canals are occupied, in the recent fish, by a sanguineous medulla, closely resembling that which fills the medullary cells of the coarse bone, to which the base of the tooth is anchylosed, and with which cells the anastomosing reticulate canals of the tooth are directly continuous.

Carcharias Megalodon.—The calcigerous tubes at the superficies of this tooth are disposed in groups which, with an insufficient magnifying power, appear like single coarse tubes, but with a higher power, are seen to be composed of congeries of parallel tubes, apparently twisted together. The interspaces are nearly equal to the diameter of these curious fasciculi: they are occupied by more scattered tubes, and by short oblique or transverse anastomosing branches. At one part of a section of this tooth, the peripheral coarse sinus or canal, which always runs parallel with the superficies, gave off an infinite number of minute tubes, which formed a plexus, (or plexiform stratum,) and from the outer part of this plexus, the tubes above described passed, at right angles, to the surface. In the longitudinal section of this tooth, the twisted appearance, above described, of the peripheral calcigerous tubes, was seen to be due to the number of side branches given off at an acute angle to the main tube. At the apex the tubes radiate, and suddenly diverge to proceed transversely to the sides. In the body of the tooth the main canals are surrounded by concentric lamellæ, traversed by radiating and anastomosing calcigerous tubes, which form a fine network in the interspaces.

Dictyodus, a *sphyrenoid genus.*—The body of the conical maxillary teeth of this fossil species presents a beautiful assemblage of medullary canals, having a general parallel course from the basis to the apex, dividing and subdividing as they approach the latter, with interspaces generally equal to three or four of their own diameters, and anastomosing by short branches crossing the interspaces, and thus intercepting quadrangular, sub-elliptical, pentagonal, or hexagonal spaces, elongated in the axis of the tooth, but becoming shorter as they approach the apex, which presents the appearance of a coarse irregular lace-work. The interior of some of the larger canals is occupied with a granular matter.

I have been able to detect the fine calcigerous tubes only at the circumference of the tooth radiating from the peripheral side of the superficial canal into the clear enamel-like coating of the tooth. They immediately begin to ramify at acute angles.

The larger canals are continued directly from the coarse medullary cells at the bony base of the tooth: the longitudinal ones are mostly larger than the transverse or oblique short anastomosing canals. This tooth resembles in general structure that of the *Anarrhichas Lupus.*

The round pharyngeal teeth of the extinct genus *Sphærodus* are anchylosed to a bone of a cellular structure. The body of the tooth consists of coarse tubes, which arise insensibly from the basis, where they have a diameter of $\frac{1}{3500}$th of an inch, and proceed directly and perpendicularly to the surface of the tooth. The characteristics of these tubes are, first, that they are so closely arranged together, that only one-fourth of their own diameter intervenes between them at their origins. Secondly, they present the appearance of a closely-twisted bundle of smaller tubes, and begin immediately to give off short and somewhat coarse branches at very acute angles; these branches increase in number, and the trunks proportionally diminish, until they have traversed two-thirds of the vertical diameter of the tooth; they resolve themselves into fasciculi of extremely minute twigs, which interlace together, and in many places dilate into, or communicate with, numerous minute calcigerous cells, and form so dense a layer as to intercept the light, excepting towards the circumference of the tooth, and consequently at the two extremities of the section, where only the structure above described is visible. Several small twigs pass beyond this plexus into the clear enamel-like outer layer of the tooth, in some parts of which traces are perceptible of a plexus of still more minute tubes, or *striæ*, which gradually diminished until they escaped the highest magnifying power employed in this examination.

Lepidotus.—The pharyngeal teeth of some of the species of this genus, *e. g. Lepid. Fittoni*, correspond so closely in size and form with those of the preceding genus, (*Sphærodus*,) as not to be distinguishable from them but by a comparison of their microscopic structure. They are composed of fasciculate tubes continued directly from the cells of the osseous base, radiating, with a direction vertical to the surface of the tooth, and giving off branches, at an acute angle, from their very commencement: thus far the general character of the texture of the tooth is the same; but the fine branches into which the fasciculate tubes resolve themselves, diverge at a much more open angle from the main trunk, are spread out more widely, have a more wavy course, and present the appearance of corn beaten down with heavy rain. These five terminal branches are inextricably interwoven, and present the appearance of numerous anastomoses, but do not form so dense a structure as to intercept the light, as is the case in the teeth of *Sphærodus*.

Gyrodus.—In the pharyngeal teeth of this genus, the tendency to the structure of the dense ivory of the teeth of the higher *vertebrata*, which is obvious in the teeth of *Sphærodus* and *Lepidotus*, is carried on to a close correspondence. The base of the tooth is excavated by a large and simple pulp-cavity, presenting a quadrate figure in a vertical section of the tooth; this cavity is immediately continuous with the large cells and reticulate canals of the bony base. The body of the tooth consists of close-set minute calcigerous tubes, having a diameter of $\frac{1}{700}$th of a line at their origin, radiating in a direct line, but with a minute and regularly undulating course, and a gradually diminishing diameter to the superficies: the lateral tubes pass horizontally, those continued from the summit of the pulp-cavity vertically, to the grinding surface. They give off very regular, but extremely minute branches, which are lost in the clear and dense enamel-like superficial layer of the tooth.

Barbel. Pharyngeal tooth.—In this tooth the structure characteristic of the ivory of the simple mammalian tooth is beautifully displayed. The cavity of the pulp is single, elongated and narrow, and the tubes radiate to the surface of the tooth at right angles to that surface, and chiefly, therefore at right angles to the axis of the tooth. The tubes are minute and numerous, beautifully and regularly undulating, seldom dividing, and then dichotomously, each branch proceeding nearly in the direction of the trunk. A detached fossil pharyngeal tooth of this kind would be distinguishable from a mammalian carnivorous tooth of similar form by the circumstance, that in the tooth of the fish the pulp-cavity becomes directly continuous with the coarse cells and medullary canals of the bone with which it is anchylosed; the base of the tooth is not diminished to a fang, and the calcigerous tubes are larger and more irregular the closer they are to the base of the tooth.

The large conical carnivorous teeth of the extinct genera *Holoptychus* and *Megalichthys* present a similar grade of structure to that of the pharyngeal tooth above described. The whole body of the tooth is here composed of minute close-set calcigerous tubes, having a diameter of $\frac{1}{1500}$th of a line in diameter, with interspaces of nearly twice that diameter. The calcigerous tubes have a minutely undulated course, and pass in nearly a straight line from the internal to the external surface of the tooth: the pulp-cavity extends about half-way through the body of the tooth, and has a narrow elliptic transverse section; it becomes gradually smaller at the base of the tooth, and there branches out into several processes, which are continued into the cylindrical processes of the dental substance, which are imbedded, like so many piles, in the coarse osseous texture of the jaw. It is this peculiar mode of fixation of the tooth to the jaw-bone that would serve at once to distinguish the tooth of *Holoptychus* from that of any saurian or mammiferous species which it might resemble in external form.

In not any of the teeth of fishes above described was there an external covering of enamel, presenting the characteristic transversely-striated prismatic crystalline structure which distinguishes the enamel of the higher Vertebrata. In all cases, where structure could be detected in the dense exterior layer representing the enamel, it presented the organized tubular character, differing from the subjacent ivory only in the more minute size of the tubes.

Of the teeth of reptiles, Prof. Owen described those of several genera, recent and fossil. In the *Sharp-nosed Alligator* (*Crocodilus acutus*), the exposed part of the tooth is covered with true enamel, and that part which is lodged in the socket is coated with a layer of *cæmentum*. The tubuli are very fine, not exceeding at the widest part $\frac{1}{1000}$th of a line. With a low magnifying power they appear to radiate in straight lines from the *cavitas pulpi* to the superficies of the tooth, proceeding at right angles to that surface: under a higher power, they are seen to be slightly undulating, and to have interspaces equal to five times their own diameters. The main tubes begin to divide soon after their origin, and the branches diverge from each other; these send off numerous finer ramuli, which are generally turned towards the root: these terminate or dilate, in many places, into calcigerous cells, which form numerous layers, generally arranged parallel with the contour of the cavity of the pulp, and most numerous at the circumference of the ivory. It is to these layers of calcigerous cells, and to the parallel curvatures of the tubes, that the apparent laminated structure is seen to be due, when sections of these teeth are examined with a low magnifying power. A thin membrane lines the cavity of the pulp of even the oldest teeth.

The fossil teeth of the extinct Reptiles reveal an equally complicated structure. The fang of the fluted teeth of the *Ichthyosaurus* is covered with a thick layer of *cæmentum*, which fills the interstices of the grooves. The tubuli of the ivory-constituent are extremely minute; they resemble in their arrangement and ramification those of the crocodile, but the undulations are more numerous and more marked.

In the *Iguanodon*, the ivory is composed of close-set tubes, radiating in a wavy course from the *cavitas pulpi* to the superficies: each tube is also minutely undulating. They are coarser than those of the Ichthyosaurus; and the ivory further differs in the presence of large medullary canals, which are seen here and there radiating from the cavity of the pulp, and traversing the dense ivory.

In the class *Mammalia* the teeth of the animals belonging to the order called *Edentata* by Cuvier, present the nearest resemblance to the vascular and organized structures above described in the teeth of cartilaginous fishes. The close resemblance, in this respect, between the teeth of *Orycteropus* and *Myliobatis* has already been alluded to, but their outward form and mode of attachment are widely different. The teeth of *Orycteropus* present the form either of a simple cylinder, or of two joined laterally together. In these, as in the tesselated teeth of the *Rays*, Cuvier had recognised a tubular structure; but the tubes described by that great anatomist were merely the medullary or pulp-canals which run parallel with the axis of the tooth, at regular distances from each other. These visible medullary canals, which are widest at the base of the tooth, diminish at first rapidly, and afterwards very gradually in diameter, and some of them divide dichotomously in their course from the base to the grinding-surface of the tooth. Throughout their course they send off at right angles and from every part of their circumference the true calcigerous dental tubes. These tubes, at their origin are $\frac{1}{700}$th of a line in diameter, but quickly diminish, as they proceed in a wavy course to the interspace which divides them from the contiguous medullary canals and their systems of calcigerous tubes: the tubes give off numerous branches, which form, near the boundary space, a moss-like reticulation of extremely fine tubes. Nearly the whole extent of the medullary canal is occupied with a vascular pulp, and its parietes near the base is likewise surrounded with a thin vascular capsule; the whole tooth is in fact composed of a closely-packed congeries of slender prismatic elongated miniature simple teeth, each of which is provided with its pulp and capsule, its medullary cavity, and its radiated series of calcigerous tubes. The capsule of each component prismatic tooth becomes ossified at a little distance from the base of the tooth. A transverse section of the whole compound tooth above this part presents a series of hexagonal, pentagonal, or tetragonal groups of calcigerous tubes radiating from an elliptical space occupied by a vascular pulp, and separated from each other by a thin boundary line of bone or *cæmentum*, characterized by the presence of Purkingian corpuscles. The vascular pulp, likewise, becomes ossified near the grinding-surface of the tooth, and consequently a transverse section taken near this part presents the centres of the radiation of the calcigerous tubes filled up with bone or *cæmentum*.

Bradypus didactylus.—The substance in the tooth of this species which corresponds to the true ivory forms only a very thin layer, situated near the superficies of the tooth; the central yellowish substance of the tooth presents a number of coarse canals, about one-tenth of a line in diameter; these radiate in a beautiful manner from the upper part of the pulp-cavity, those in the middle proceeding parallel to the axis of the tooth, those at the circumference curving outwards. These canals are unequal, presenting partial dilatations, which, however, are sometimes, though rarely, discernible in the tubuli of human teeth; they give off numerous tortuous branches of different sizes, and these open into very distinct calcigerous cells scattered about the interspaces of the coarser canals. The fine crust of ivory above mentioned is formed by minute tubes directly continued from the finer ramifications of the large canals of the central substance, and terminated in plexus of still finer tubes, which at length escape the highest magnifying powers. The fang, or inserted part of the tooth, of the sloth is coated with a ayer of *crusta petrosa*, which is characterized by large canals and abundant *Purkingian corpuscles*. There is no enamel in the composition of these teeth or of those of any of the existing Edentata.

Megatherium.—Microscopic examinations of the structure of the tooth of this extinct mammifer have undeceived me with respect to its conformation; the thin dense layer between the crusta petrosa and the internal substance composing the body of the tooth is not enamel, but a layer of ivory composed, like the dense ivory of the teeth of other Mammalia, of minute tubes having a parallel course at right angles to the surface, and minutely undulating in that course, and corresponding with the thin cylinder of true ivory in the tooth of the sloth. The central part of the body of the tooth consists of a coarser ivory, much resembling the teeth of *Psammodus* or *Myliobatis*, among fishes. It is traversed by large medullary canals parallel to each other and to the finer ivory tubes, having angular interspaces equal to one and a half diameter of their own area, and generally anastomosing in pairs by a loop whose convexity is close to the origin of the fine ivory tubes, as if each pair so joined was composed of one reflected canal. Some, however, are continued across the fine ivory, and anastomose with the corresponding canals of the *cæmentum*; the interspaces of the coarse ivory tubes appear at first view granular, but they are principally occupied by reticular branches given off from the canals: some of these anastomosing branches are seen coming off from the concavity of the loops, and retrograding. Numerous minute cells are scattered about the terminal loops of the medullary canals of the coarser ivory. The origin of the fine ivory tubes is from the convexity of the peripheral loops of the above medullary canals. The ivory tubes are separated by interspaces equal to one and a half their own diameter; they divide and subdivide, growing smaller and more wavy towards the periphery or *cæmentum*; here their terminal branches assume a bent direction, and form anastomoses, dilate into small cells, and many are clearly seen to become continuous with the radiating fibres or tubes of the corpuscles of the contiguous *cæmentum*. The cement is traversed by large canals running, like the canals of the coarse ivory, parallel to each other and to the course of the fine ivory tubes, with interspaces of about five times their own diameter, occasionally, but rarely, dividing dichotomously,—in which case the branches usually anastomose and form loops with the convexities towards and close to the outer layer of fine calcigerous cells, in which the fine ivory tubes terminate. The cement differs from the coarse ivory in the fewer number of canals, and more especially by the presence of the bone corpuscles or radiated cells in the interspaces of the canals. The irregular tortuous fine tubes forming a network in the interspaces, and especially those proceeding from the convexities of the loops, are much more distinct than the corresponding tubes in the coarse ivory. The primary branches of the canals go off generally at right angles. In a few places I have distinctly seen the large canals of the *cæmentum* traversing the substance of the fine ivory to anastomose with those of the central coarse ivory.

We have thus, then, in the tooth of the *Megatherium*, an unequivocal example of a course of nourishment of the teeth distinct from and superadded to that which proceeds from the surface of the pulp and the cavity of the fang in which it is lodged, viz. by a direct communication between the vascular canals of the external organized *cæmentum* and the tubuli of the ivory. *Retzius* observes of the human tooth, that "the fine tubes of the *cæmentum* enter into immediate communications with the cells and tubes of the ivory, so that this part can obtain from without the requisite humours after the central pulp has almost ceased to exist." In the Megatherium, however, those anastomoses have not to perform a vicarious office, since the pulp maintains its full size and functional activity during the whole period of the animal's existence. It relates to the higher organized condition, and doubtless to the higher vitality of the entire grinder in that extinct species.

The views entertained by Cuvier of the affinity of *Megatherium* to *Bradypus*, derive full confirmation from the microscopic investigation of its teeth. It needs but to compare the preceding description with that published by Retzius of the structure of the teeth of the *Armadillo*, to perceive how much more closely the Megathere resembles the Sloth in the structure of its teeth. The Megatherium has ten teeth in the upper jaw, five on each side; differing slightly in form and size, but all presenting the same characteristic vascular structure as above described. The structure of the coarse central ivory may be compared with that of which the entire tooth of the Orycteropus is composed, with these differences,—first, that the parallel medullary canals and their systems of calcigerous tubes are not separated from their neighbours by a layer of *cæmentum*, and, secondly, that the medullary canals anastomose at their peripheral extremities.

Toxodon.—The teeth of this extinct animal have an external but incomplete investment of enamel, which is deficient for a small extent at the anterior and posterior surfaces of the tooth; but these parts, as well as the enamel, are covered with a thin exterior layer of *cæmentum*. The body of the tooth is composed throughout of compact ivory, consisting of minute wavy calcigerous tubes, $\frac{1}{500}$th of a line in diameter at their origin, which radiate in directions vertical to the superficies of the tooth, or of the inflected fold of enamel, from the central pulp-cavity.

In the discontinuity of the enamel surrounding the ivory of the tooth, the Toxodon differs from all known Pachyderms, and exhibits an approach to the *Rodentia* and *Edentata*.

In the *Leopard*, the tubuli of the canine teeth are chiefly remarkable for the number of their ramifications, and the beautiful curvatures of the same. In the *Mole*, the main tubes are remarkable for their width and shortness; they are as large at their commencement as in the human tooth, but soon divide at their extremities into a number of smaller branches, which again subdivide, the terminal twigs anastomosing and communicating with minute calcigerous cells immediately beneath the enamel.

The teeth of those orders of Mammalia in which they present the usual structure of compact ivory, enamel, and *cæmentum*, have been described in several genera with so much accuracy by Professor *Retzius*, that there are few modifications or examples worthy of particular attention.

In the simple teeth of the Marsupial animals, the external layer of *cæmentum* covering the enamelled crown is thicker in many of the species than is usually seen. The Phalangers, Koala, and Wombat, offer good examples of the superficial layer of cement on the exposed crown. It possesses the usual high degree of organization, and abounds in the Purkingian cells.

In the incisors of the Orang-Utan, the main calcigerous tubes of the ivory, which radiate from the central cavity of the pulp, are somewhat larger than those of man; they present the same primary curvatures, but less numerous and less strongly-marked secondary undulations*. In the crown of the tooth of the Orang, the dental tubes are chiefly branched at their extremities, while towards the apex of the fang the main tubes are surrounded by exceedingly fine and close-set branches, which subdivide in their course. The nearer the crown, the larger are these branches; they are curved, with the concavity towards the pulp.

In the summary of this series of observations which Professor Owen detailed, he observed, that in the human and similarly organized teeth, the analogy of ivory to bone, as to texture, was only seen in the existence and intercommunication of the minute calcigerous tubes and cells; but that there was no trace of medullary or Haversian canals, with their characteristic concentric laminæ, unless the entire tooth were regarded as analogous to a single enlarged Haversian canal, when the cavity of the simple pulp would represent the medullary cavity of the canal; while the tubes, with the appearance of laminæ occasioned by their undulations, might be deemed equivalent to the concentric lamellæ and the calcigerous tubes, which, in bone, traverse these lamellæ, and radiate from the Haversian canal. In the teeth of many of the lower animals, however, and especially that of the extinct *Acrodus*, amongst the cartilaginous fishes, the resemblance of the dental tissue to bone was extended to the existence of the characteristic Haversian canals in great numbers. The presence of these canals was explained by the progress of the development of these bone-like teeth, as observed by Professor Owen in recent cartilaginous fishes. The large pulp, at the commencement of the formation of the tooth, had exercised its ordinary function in the secretion of a close-set series of calcigerous

* The primary curvatures Professor Owen explained to be those which belong to the general course of the dental tube, and which are seen with a lower power; in man they resemble the curves of the Greek Zeta (ζ). The secondary curves are minute undulations in the whole course of the tube, requiring a high power for their perception, and affecting both the main trunks and their branches; these probably indicate and are due to the movements of the formative pulp during the deposition of the ivory.

tubes, having a general direction perpendicular to the surface of the tooth, and closely resembling true ivory. The pulp then, instead of continuing to form similar tubular ivory, by adding to the extremities of the previously formed tubes, became subdivided, or broken up into numerous processes, to which those forming the three fangs of a human grinder are analogous. But each process here becomes the centre of an active formation of similar branched tubes, radiating in all directions from that centre, and anastomosing by their peripheral branches with those from contiguous centres, or communicating with interposed calcigerous cells. The cavities containing the above subdivisions of the pulp, like the Haversian canals containing the processes of medulla in true bone, have had their area diminished in like manner by the successive formation of a series of concentric lamellæ, traversed, as in true bone, by radiating and minutely ramified calcigerous tubes, communicating with each other and with the minute cells in the interspaces. The resemblance between the pulp canals of the teeth of *Acrodus* and of the medullary canals of bones, is further exemplified in the existence of lateral communications in teeth; and in function as well as structure they may be regarded as being identical.

With reference to the application of the tubular structure of the teeth to the explanation of their pathology, Professor Owen observed, that it was a new and fertile field, which would doubtless be replete with interesting results, and might suggest some good practical improvements in dental surgery. Ordinary decay of the teeth commenced, in the majority of instances, immediately beneath the enamel, in the fine ramifications of the peripheral extremities of the tubes, and proceeded in the direction of the main tubes, and, consequently, by the most direct route to the cavity of the pulp. The decayed substance, in some instances, retains the characteristic tubular structure, which is also observable in the animal basis of healthy teeth after the artificial removal of the earthy salts. The soft condition of the decayed portion of a tooth is well known to all dentists; it depends upon the removal of the earthy salts from the containing tubes and cells, in which process the decay of teeth essentially consists. The main object of the dentist, in reference to ordinary caries of the teeth, seems, therefore, to be, to detect those appearances in the enamel which indicate the commencement of decay—to break away the enamel, whose natural adhesion to the subjacent softened ivory will be found to be more or less diminished—to remove the softened portion of the ivory and fill up the cavity with incorrodible substance. Experience proves, what could not be intelligibly explained before the true structure of the dental substance was known, viz. that the progress of the decay is sometimes thus permanently arrested. Such cases sometimes exhibit a thin dense layer of ivory in contact with the stopping, apparently resulting from an exudation of the calcareous salts from the extremities of the tubes divided in the operation.

In conclusion, Professor Owen passed in general review over the structures which he had described in detail. He particularly pointed out the important application of the microscopic examination of thin slices of fossil teeth to a determination of the natural family, or genus, to which such teeth had belonged, when other characters fail, or a complete tooth is unattainable. Finally, Mr. Owen remarked, that through the endless diversity which the microscopic texture of the teeth of different animals presented, the universal law of the tubular structure could be unequivocally traced; and that the general tendency of the modifications observable in descending from man to the lower classes of the vertebrate animals, was a nearer approximation of the substance of the tooth to the vascular and organized texture of bone.

MECHANICAL SCIENCE.

On the Use of Wire Ropes in Deep Mines. By Count Augustus Breunner.

There had been introduced into the silver mines of the Hartz Mountains, about seven years ago, ropes composed of twisted iron wire, as a substitute for the flat ropes previously in use. Since that time they have been adopted throughout the mines of Hungary and most of those in the Austrian dominions, to the almost total exclusion of flat and round ropes made of hemp. These iron ropes are of equal strength with a hempen rope of four times the weight. One has been in use upwards of two years without any perceptible wear, whereas a flat rope performing similar work would not have lasted much more than a single year. The diameter of the largest rope in ordinary use in the deepest mines of Austria is one inch and a half. This rope is composed of iron wires, each *two* lines in diameter; *five* of these are braided together into strands, and *three* of these strands are twisted tightly into a rope. Great care is requisite in making the rope that the ends of the wires be set deep in the interior of the rope, and that no two ends meet near the same part. The strength of these ropes is little less than that of a solid iron bar of the same diameter. The usual weight lifted is 1000 lbs. The rope on leaving the shaft must be received on a cylinder of not less than eight feet diameter, and be kept well coated with tar. There is a saving of about one-third of the power in one case mentioned, for *four* horses with a wire rope are doing the same work as *six* horses with a flat rope. It was suggested by Count Breunner, that the substitution of iron ropes for the flat ropes in our deep mines and coal-pits would be attended with the same, if not greater advantages than have attended their introduction into the mines of the Austrian dominions.

On the Timber Viaducts now in progress on the Newcastle and North Shields Railway. By B. Green.

The object of this paper was to give a description and explanation of the principle of constructing timber bridges on a more durable, stronger, and cheaper principle than the timber framing heretofore used in this country.

For this purpose the author exhibited models on a large scale of the peculiar 'lamination' of the timber in the arches, by which, along with other advantages, a decided advantage is gained in strength.

With a model upon this principle for a bridge across the Tyne 120 feet span, experiments were made in the presence of part of the managing directors of the railway, and some scientific persons, which proved highly satisfactory; for a weight of 250 stone was placed upon it, without the slightest deflexion of the arch being perceptible. This, multiplied by 144, according to the scale of the model, gives 34,900 stone, or upwards of 218 tons, as the weight the arch of 120 feet would bear without being affected. A great surplus strength was therefore manifest, to cover all contingencies, and make allowances for the increased span and extra dimensions.

The plans and elevations of two viaducts at Ouseburn and Willington Dean were laid before the section. The former, at the eastern suburb of Newcastle, is 920 feet in length and 108 feet in height; there are five arches, of 116 feet span each, and two stone arches, at each end, of 45 feet span each. These were introduced in order to prevent the mounds coming too close upon the very steep banks of the ravine. The latter bridge is 1150 feet long, and consists of seven arches, 120 feet span each. The height up to the roadway is 82 feet. Stone arches were not requisite here, as the banks are of a more gradual slope.

The piers and abutments are of stone. Each arm is composed of three ribs, formed to the proportionate curve shown on the model. Every rib is put together with 3-inch deck deals, in lengths of from 20 to 45 feet, and two of the deals in width. The first course is formed of two whole deals in width, and the next of one whole and two half deals; and so on alternately until the whole rib is formed. Each rib consists of 15 deals in height or thickness, and the ends are butted one against the other, breaking joint, so that no two of the horizontal or radiating joints shall come together. The whole are connected with oak trenails or pins, each of which passes through three of the deals in thickness. Between every deal a layer of brown paper, dipped in boiling tar, is laid, to secure the joints from being affected by wet, and so as to make the timbers bed tightly one upon the other. The ends of each rib are inserted into large cast iron shoes or sockets, which are first fixed to the springing stones of the masonry, and secured with long iron bolts, four to each plate, run in with lead. The three ribs are connected together with diagonal braces and iron bolts.

The spandrils, formed by the arches, being great, on account of the span, the framing is made in proportionate strength. A beam 14 inches square is fixed about the middle of the spandril, inclining upwards to the crown of the arch; from which struts are carried, both above and below it. Those above are perpendicular to the longitudinal beams of the roadway, and those below are radiating to the centre of the arch.

The longitudinal beams under the roadway are 14 inches square; and transverse joists, 3 feet 6 inches apart, and projecting about 2 feet on each side, are laid across to receive the 3-inch planking, which is covered with a composition to form a roadway.

The rails for the locomotive engine and train are raised above the planks about 8 inches, on longitudinal beams or sleepers of timber, about 12 inches by 6. A strong framed railing is then fixed along each side, the length of the bridge, and completes the structure.

The spandril framing is connected and bound, both to the roadway and to the ribs, by means of iron bolts, straps, and keys, in the different situations shown on the model. One of the radiating struts in each spandril is carried on from the rib to the longitudinal beams, and connected thereto, and to the masonry, by bolts passing through and run down the piers about 8 feet.

In this system of timber bridge building, the straight trussing in the main principle of support is dispensed with; for the spandril framing must not be looked upon as such; it is merely a combination of woodwork, to convey the weight coming upon the roadway on to the simple curved rib; and all timbers in a state of tension are avoided; for when a weight comes upon the roadway, the whole structure undergoes compression.

Mr. Green has also applied this laminating principle to a more durable material, viz. iron, and he described the modifications which this application rendered necessary, and the advantages it offered.

Outline of the Principles of the Oblique Arch.
By Peter Nicholson.

The oblique arch is an invention of comparatively recent date; but the general use of the locomotive engine rendering it urgent to preserve the most direct line for railways, has caused the general adoption of oblique bridges on all the lines of railway now in progress, and it has become a matter of importance that the theory of their construction should be fully understood.

The principles of the oblique arch which the author proposes for the guidance of engineers, require that five of the faces of each stone be prepared in such a manner that four of them shall recede from the fifth; and, when the stones are arranged in courses, the surfaces of the fifth face shall form one continued cylindric surface, which is the intrados, and the other four surfaces shall form the beds and ends of the stones on which they join each other. In every course two of the opposite surfaces of the first stone, two of the opposite surfaces of the second stone, and so on, shall form two continued surfaces throughout the whole length of each course; and the edge of each of these continued surfaces in the intrados shall be a spiral line. If a straight line be drawn through any point in one of the spiral lines, perpendicular to the axis of the cylinder, the straight line shall coincide with that continued surface which is a bed of that course, and the straight line thus drawn shall be perpendicular to a plane which is a tangent to the curved surface of the cylinder at that point in the spiral line; therefore the straight line thus drawn shall be perpendicular to another straight line which is a tangent to the spiral line at that point.

When the intrados is developed, the spiral lines which form the edges of the courses shall be parallel, and their distances shall be equal; and the spiral lines which are the edges of the ends of the stones shall be developed in straight lines perpendicular to those lines which are the developments of the spirals of the edges of the courses.

It is evident that each of these spiral lines will have a certain radius of curvature, and that this radius of curvature, at any point of the spiral line, will be equal to the radius of curvature at any other point in the same spiral; and that the radius of curvature at any two given points in two spiral lines which have parallel developments, are equal to one another.

Therefore, if two points be taken in a spiral line, and if a straight line be drawn from one of them parallel to the axis, and if, through the other, the cylinder be cut by a plane perpendicular to the axis, and if the surface of the cylinder be developed; the development will be a right angled triangle, of which the quotient arising, by dividing the product of the square of the hypothenuse and the radius of the cylinder by the square of the development of the circular arc intercepted between the spiral and the straight line, will be the radius of curvature of that spiral.

By these principles the geometrical construction of an oblique arch may be easily made for the use of the workmen, or calculations of all the parts may be expeditiously and accurately performed by the engineer; it is only necessary to have given the angle of obliquity of the acute-angled pier, the width of the arch within its abutments, the height of the intrados above the level of the springing, the perpendicular distance between the planes of the two faces, and the number of arch stones in each elevation, in order to construct the arch.

On an Alteration in the Construction of Wollaston's Goniometer, by which its Portability is increased. By W. H. Miller, *M.A. F.R.S., Fellow and Tutor of St. John's College, and Professor of Mineralogy in the University of Cambridge.*

In this instrument, which has a circle 4·4 inches in diameter, provided with two verniers reading to minutes, the branch which carries the crystal screws into the end of the inner axle, instead of being in one piece with it, as in the usual construction, and is taken out when the Goniometer is put into its case. The distances of the milled heads, by which the circle and inner axle are turned, from the collar through which the axle of the circle passes, are considerably reduced. The foot of the instrument is a plate of brass 4·4 inches long, and 1·6 inch wide, capable of being fastened, by means of two screws, to one half of the case, which is provided with three adjustable foot-screws. A mirror of dark glass, making an angle of about 40° with the foot of the Goniometer, an improvement which appears to have been invented independently by Mr. Sang and M. Degen, enables the observer to use an object seen by reflection for the lower signal. The whole packs into a case, the external dimensions of which are 5·5 inches long, 4·9 inches wide, and 1·7 inch thick.

A short Account of a Method by which Engravings on Wood may be rendered more useful for the Illustration and Description of Machinery. By C. BABBAGE, *F.R.S.*

The principle of this method consists in making one woodcut, which represents a plan or projection of any piece of machinery. Several stereotype plates are then taken from this block, from each of which various parts of the mechanism are cut out, leaving only such parts as may be clearly understood together.

The author illustrated this plan by impressions from several complicated woodcuts, intended for the description of his calculating engine. From one of these originals five stereotype plates had been taken and properly prepared. By removing certain parts from two of these plates, two different parts of the machine were shown; and by taking two other pairs of stereotype plates, each of these separate parts was again shown as dissected—one plate containing nothing but the framing supporting that part, and the other nothing but its moving parts. The author suggested the employment of this method for colouring geological maps.

On the Odontograph. By Professor WILLIS, *F.R.S.*

Professor Willis described his instrument called the Odontograph, designed for enabling workmen to find at once the centres from which the two portions of the tooth are to be struck, so that the teeth may work truly together. The position of these centres is pointed out by theory, and this instrument may be considered as the practical means of carrying out the theory*. He also described the construction and use of some scales of measurement invented by Mr. Holtzapfel.

Description of an Improved Leveling Stave for Subterranean as well as Surface Leveling. By THOMAS SOPWITH, *F.G.S.*

Of late years, the method of reading the figures of the stave itself, instead of using a sliding vane, has been adopted by the most experienced engineers and surveyors.

The staves now exhibited are of this construction. The figures are engraved on copperplate on an enlarged scale, so as to contract in drying to the proper length, which is determined by a very accurate gauge.

* Reports of British Association, vol. vi. p. 135.

Mr. Sopwith described the improvements made by him in the construction of these staves, and also the use of a stave for subterranean leveling, the face of which is protected by a glass shield. It is hinged so as to admit of being used in any seam of coal from 3 to 5 feet in height; and the same principle may be applied to any greater or less extent. Mr. Sopwith stated, that from the experience of himself and assistants in conducting extensive leveling operations, fully one half time is gained, as well as great additional accuracy.

Description of Instruments to facilitate the Drawing of Objects in Isometrical Projection. By THOMAS SOPWITH, *F.G.S.*

Mr. Sopwith exhibited several instruments, diagrams, &c., adapted to facilitate the process of isometrical projection. The first of these is a set of triangular rulers, the angles of which are coincident with the angles in the isometrical projection of a cube; and hence they can be applied with great ease and rapidity to the delineation of geometrical forms in isometrical drawings.

Isometrical squares and circles engraved on drawing-paper are adapted to facilitate this mode of projection. The circles are graduated; and hence any angles, whether on a horizontal or vertical plane, can be correctly delineated and subsequently measured. Mr. Sopwith illustrated the advantages of this method of drawing by several examples of its application to astronomy, to architecture, and to constructive designs generally. One remarkable property of isometrical drawings was pointed out, viz. that drawings made on a flat surface may have other flat surfaces pasted on by an edge, and which will appear in true projection when turned over upon the edge, and horizontal and vertical movements may be thus shown in one drawing; thus forming what may be appropriately termed a picture model.

The Isograph is an entirely new instrument, invented by Mr. Sopwith, for transferring plans from orthographical to isometrical projection. This is effected by means of a simple mechanical movement. The isograph consists of a number of parallel rulers, made of brass or ivory, the fiducial edges of which are an inch distant. These rulers are fixed at each end by pivots to two brass bars, the centre of each pivot exactly coinciding with the line of each fiducial edge. A brass gauge is used to fix this series of rulers so as to form a true geometrical square; and when in this position, the principal lines or points are marked off upon the respective edges of the rulers. The instrument is then moved into a lozenge shape, and a gauge, equal in length to one side of the square, is fitted so as to form the shorter diagonal. Hence the opposite angles of the instrument are respectively 60° and 120°, which is the required condition of isometrical projection.

The Isometrical Protractor, made of brass, is used for the delineation of bearings in isometrical projection. A representation of a mining district and a plan of Newcastle, drawn isometrically, were exhibited.

On an Improved Method of constructing large Tables or Writing-Cabinets, adapted to save much time, and to secure a systematic arrangement of a great number and variety of Papers. By THOMAS SOPWITH, *F.G.S.*

In the arrangements of official business, in literary and professional pursuits, and in conducting an extensive correspondence, great inconvenience and loss of time result from the want of method consequent on papers being kept in various drawers, closets, boxes, &c., each requiring a separate key. The principle on which the improved writing-tables are constructed is intended to obviate this inconvenience.

Drawings were exhibited, showing various modes of constructing large writing-tables and cabinets in such a manner that, by means of a single key, the whole of the drawers, closets, &c., are at once opened, and the whole of the partitions, drawers, &c., can be reached by any person writing at the table, without stirring from the seat in front. The whole, in like manner, is closed by a spring lock; and the simplicity and strength of the arrangements are such, that the movements are not liable to be deranged. One of the drawings represented a writing-cabinet in Mr. Sopwith's office, in the upper part of which there are one hundred divisions for papers to be assorted, besides drawers for writing and drawing materials, and small shelves for colours and mathematical instruments. An upright door, hinged at the bottom, falls, when unlocked, upon a flat table, and forms a writing-desk; and any papers left upon it at the time of its being shut up, are of course ready to be resumed the moment it is opened again. This door is rebated on its three edges, so as to overlap the adjoining doors; and the shutting of this door also forces in an iron bar, which fastens the drawers in the lower part of the table by a very strong and simple mechanical movement.

There is also an apparatus for hanging keys upon, so that when any key is removed, a slip of wood, with the name of the key upon it, falls down so as to prevent the door from being closed; and the person using the desk is therefore reminded of having forgotten to replace the key. The facility of reference and arrangement admits of many short periods of time being devoted to study or business which, but for such an arrangement, would be totally lost.

Suggestions on the practicability and importance of preserving National Mining Records. By THOMAS SOPWITH, *F.G.S.*

The commercial prosperity of Great Britain mainly depends on its mineral productions. Whatever tends to promote economy in the working of mines, and to afford increased facilities for the discovery of mineral treasures, eminently deserves the attention of every enlightened statesman who regards the future as well as the present welfare of the country.

The great value of, and increasing necessity which exists for, a regular system of preserving mining records, has been repeatedly urged by the most eminent and experienced geologists and miners. No such system has yet been pursued in this country; and the importance of the subject renders it deserving of the attention of the British Association during its meeting in the midst of the mining districts of the north of England.

Mr. Sopwith's paper proceeds to point out the inconveniences and serious loss of capital, and even of human life, resulting from the preservation of mining records being neglected, and to explain several practical details connected with the subject.

On Improvements in Ship Building. By Mr. LANG.

Mr. Lang described and exhibited some models illustrative of the safety keel, which had been introduced with great success; and mentioned instances in which vessels fitted with these keels had struck and come off without sustaining material injury. He then entered into some details respecting the proper construction of merchantmen, and exhibited some models of the bottoms of merchantmen*. He also exhibited a method of securing a round-headed rudder, and a model of a tube-scuttle to admit light between decks, and which had been used with great success.

On the Construction of a Railway with Cast-Iron Sleepers, as a Substitute for Stone Blocks, and with continuous Timber Bearing. By T. MOTLEY.

The cast-iron sleepers, which are wedge-shaped and hollow, having all their sides inclined inwards towards the under side, are to be laid transversely, and the timber is to pass longitudinally through the centre, and to be secured by wedges of iron and wood. The sleepers are to be six inches apart, and the timber of such a thickness as to prevent any perceptible deflexion betwixt the rails. The road is to be ballasted up to the top of the sleeper, and the timber to stand out sufficiently, and to have any approved rail laid upon it.

On a Suspension Bridge over the Avon, Tiverton. By T. MOTLEY.

The peculiar feature of this bridge is, that each chain is attached to the roadway, and the suspending bars are carried up through each chain above it. The length of the bridge is 230 feet, the breadth 14 feet, and the cost, including the towers and land abutments, under 2400*l.* This bridge is superior to the common suspension bridge, in that it is more firm, and experiences much less friction, owing to the absence of vibration.

* See Reports of the British Association, vol. vi. p. 135.

On an Improved Method of constructing Railways. *By* J. Price.

This method consists in fixing rails on a continuous stone base, a groove having been made in the stone to receive a flange or projection of the lower side of the rail. The stones and rails are to break joint with each other, and the chair by which the rails are to be secured is to be made fast to the rail by a bolt, not riveted, but slipped in. The chair is to be sunk until the top is level with the top of the stone, and fastened to it by two small wooden pins. Any sinking of the road is to be obviated by driving wedges of wood underneath the stone until it is raised to the required height. The chairs are to be fixed at about four feet apart, and to weigh, if of malleable iron, 14 pounds; but if of cast iron, 20 pounds: the rail to weigh 50 pounds per yard.

Machine for raising Water by an Hydraulic Belt. *By Mr.* Hall.

In this machine, an endless double woollen band, passing over a roller at the surface of the earth, or at the level to which the water is to be raised, and under a roller at the lower level, or in the water, is driven with a velocity of not less than 1000 feet per minute. The water contained betwixt the two surfaces of the band is carried up on one side and discharged at the top roller by the pressure of the band on the roller, and by centrifugal force. This method has been in practice for some time in raising water from a well 140 feet deep in Portman Market, and produces an effect equal to 75 per cent. of the power expended, which is 15 per cent. above that of ordinary pumps. This method would be exceedingly convenient in deep shafts, as the only limit is the length of the band, and many different lifts may be provided.

On Cliff's Dry Gas Meter. *By Mr.* Samuda.

This instrument consists of a pulse glass, that is, two thin glass globes united by a tube. These globes are partially filled with alcohol, and hermetically sealed when all the air is expelled from their interior. In this state, the application of a very slight degree of heat to one of the globes will cause the alcohol to rise into the other. The pulse glass is fixed on an axis, having a balance-weight projected from it, and the axis works in bearings on the sides of a chamber through which the gas to be measured is made to pass the gasometer in two currents, one of which is heated and the other cold. The hot gas is made to enter opposite to, and to blow upon the top globe of, the pulse-glass, while the cold gas blows upon the other. The difference of temperature thus established between the globes causes the alcohol to rise into the upper one, and the glass turns over on its axis, thus varying its position, and bringing the full globe opposite to the hot stream of gas. This stream, with the assistance of the cold gas, which condenses the vapour in the top globe, repeats the operation, and the speed at which the globes oscillate will be precisely in proportion to the quantity of gas which has been blown upon them, provided a uniform difference of temperature is always maintained between the two streams of gas. The difference of temperature is established and rendered uniform by a small flame of gas, which heats a chamber through which the lower current of gas has to pass, and the arrangements for securing an equality in the difference of temperature are very ingenious. The instrument is first tested by making a given quantity of gas pass through it, and observing the number of oscillations of the pulse-glass. This once established, the instrument registers the quantity passed with extreme accuracy.

Sir John Robison mentioned a circumstance which he considered of peculiar importance to the lower orders. Mr. Strutt, of Derby, to whom the country owed so much, had some years ago expressed to him an opinion, that coal-gas would be found by the lower orders the cheapest fuel for cooking. This he had applied; and the whole apparatus, which might be considered as the converse of the Davy safety-lamp, consisted in fixing a piece of wire-gauze at the extremity of a gas-pipe of about six inches in diameter. He referred to the account in Loudon's 'Encyclopædia of Cottage Architecture,' for some valuable remarks and directions on this subject. The wire-gauze was liable to be destroyed under a long-continued intense heat: this, however, was obviated by sprinkling a small quantity of sand upon it. Bulk for bulk, gas was more expensive than coal, but the former was more economical and convenient for occasional use and the smaller operations in cooking.

Sir John Robison explained a model of the bucket of a pump in use in Sweden, the peculiar feature of which was, that the pressure of the sides of the bucket outwards against the pipe is exactly proportional to the load to be raised. This bucket is peculiarly applicable for raising foul water.

On a New Day and Night Telegraph. By JOSEPH GARNETT.

The paper on this subject was accompanied by a model, to exhibit the construction and method of working of the telegraph, which it is proposed should consist of two ladders, about 41 feet long, framed together at about 24 inches asunder at the bottom, and 20 at the top, so as to constitute the frame for the machinery. There are two arms, one at the top, the other about midway up the frame-work, counterpoised by weights, and worked by machinery, consisting of 8 bevel mitre wheels. At the bottom of the frame-work is a dial plate, with a pointer, and the workman, in setting the pointer, brings the arm of the telegraph into the required corresponding position.

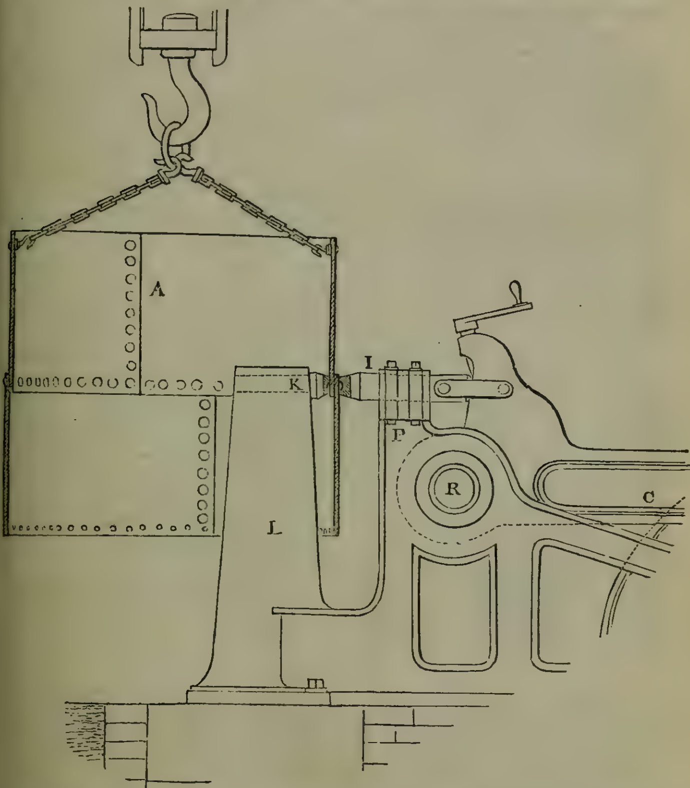
On an Improved Method of working the Valves of a Locomotive Engine. By Mr. HAWTHORN.

Professor Willis described the method recently introduced by Mr. Hawthorn, for working the valves of a locomotive without the usual eccentrics. The motion is derived at once from the connecting rod, by means of a pin placed at the centre of the connecting rod, and giving to a frame a reciprocating motion in a vertical direction at every revolution of the crank. To this frame are attached arms, by which motion is communicated to the slides. It is necessary that the slide should be open for the admission of the steam into the cylinder, a little *before* the piston has completed the stroke; this, which is technically termed the *lead* of the slide, must be provided for with great care, so as to correspond with the various speeds of the piston; this arrangement cannot be made where eccentrics are used without considerable difficulty; but this is provided for in Mr. Hawthorn's method by simply changing the angle at which the frame is set, an operation which can be performed by adjusting a screw.

On the Application of Machinery to the Manufacture of Steam-Engine Boilers, and other Vessels of Wrought Iron or Copper, subject to Pressure. By WM. FAIRBAIRN.

Having described the usual process of hand-riveting, and the imperfections to which it is subject, the author adverts to the riveting machine, which obviates these defects, and produces sound and perfect work. As the time occupied in the process of hand-riveting, allowing the rivet to cool, and thus destroying its ductility, is the chief cause of all the defects, it is evident that an instrument having a force to compress the rivet within an indefinitely short period must obviate or entirely remedy these evils. The commencement of the process is the same in both methods. In a circular boiler, such as is represented at A, in the annexed figure, the plates having been first bent to the circular form, with all the requisite holes punched in them, two rings of plates are put together by temporary bolts, and suspended over the machine, by three chains, from blocks over the centre of the boiler. These blocks are arranged to move backwards and forwards for circular boilers, to suit any diameter, and are also made to move crossways for plain flat work. Having now placed the boiler so as to inclose the circular portion of the machine marked L, (which performs the part of the "holder-on,") and having brought the rivet-holes in a line with the dies marked I and K, a rivet being previously inserted, and the bent lever C, turning on the centre R, being lifted up by the power used to work the machine, —the die I is advanced through the fixed head P, and the rivet is now compressed with great force against the die K. The faces of the dies have each a circular cavity, when employed in the performance of the usual work, but may be formed so as to give any required shape to the head and point of the rivets.

From this description it will appear evident that no time is lost; that no hammering of the rivet takes place after it is cooled, to render it brittle: but the action is completed so rapidly, as to leave it in a perfectly sound and ductile state. This is a point of the utmost importance, as the joint is so firmly united by the subsequent cooling and contraction of the rivets as to render the usual precaution of "caulk-



ing" almost unnecessary. Caulking is an operation universally adopted to prevent leakage, by *setting up* the edge of the plates upon the seam or joint with a hammer and a square-ended tool of cast steel.

By the use of the machine much time and labour is saved, by the substitution of instantaneous compression instead of a long series of impacts. It is applicable to all kinds of circular tubes and boilers, and also to every description of flat and square work. It fixes and completes eight rivets of three-fourths of an inch in diameter in a minute, with the attendance of two men and two boys to the plates and rivets; whereas the average work that can be done by two riveters and one holder-on and a boy, is 40 three-quarter rivets per hour; the quantity done in the two cases being 40 to 480, or in the proportion of 1 to 12.

On a Steam-engine Boiler. By J. PRICE.

The author exhibited a model of a steam-engine boiler of 18 horse power which is at work daily at the Durham Glass-works, Gateshead, and described its peculiar construction and advantages in regard to safety and cheapness.

He states, that by the construction of the flues the whole of the heat is rendered available. Owing to the draught, the dust that accumulates in the flues is only ten pounds per week.

There are two cocks in the laggings of the furnace, and three along the bottom of the boiler to let off water and sediment when required; in consequence of which there have only accumulated 29½lbs. of thin mud in six weeks, which contains 2 oz. 6½ drachms of fine powder. He mentions, as one principal advantage of this boiler, the impossibility that either flues or boiler can collapse.

A steam safety-valve is applied in the form of a ball in a cup which rises from its seat and allows the steam to escape so soon as it comes to within one pound of the safety pressure; it is covered with a cap, which is secured by nuts within the boiler, and cannot be removed or weighted without cooling the boiler: consequently it is beyond the reach of the working engineer, who has his own valve to regulate as he likes.

A New Rotatory Steam-Engine. By S. ROWLEY.

It was stated by Mr. Evans, that the novelty in this construction consisted in the excentric being on the inside.

Remarks on the Construction of Steam-Boilers. By W. GREENER.

Mr. Greener stated his opinion, that the accidents which happen to steam-boilers are principally due to defect in the material of which they are constructed. He detailed several experiments made on slips of iron cut from plates of different quality. He found that slips cut latitudinally from a plate sustained less pressure by 30 per cent. than slips of the same dimensions cut longitudinally; in some cases the difference was much greater. He also had immersed plates in a mixture of sulphuric acid and water, and found that the injury done in twenty-four hours varied from 6¼ to 15 per cent. of the original strength. Many boilers will stand so long as the form remains perfect; but should any part, as the crown of the arch, in cylindrical boilers, collapse, an accident becomes probable.

On a Substitute for the Forcing-Pump in supplying Steam-Boilers, &c. By Mr. Maule.

This was a hollow cock having an orifice, which being uppermost, the plug became filled with the liquid, and then, being turned half round by the motion of the piston, the liquid could run into the vessel below.

Notices on the Resistance of Water. By John Scott Russell, *F.R.S.E.*

The author, in conjunction with Sir John Robison, being still engaged in researches bearing on this subject, it is deemed unnecessary to anticipate by partial notices the full Report which is expected from these gentlemen.

On Methods of Filtering Water. By J. T. Hawkins.

In this paper the author detailed the various essentials for a durable and simple filter for obtaining pure water. The charcoal must be perfectly well burnt, and kept from exposure to the atmosphere. A test of good charcoal is, that when pulverized, it sinks rapidly in water. The charcoal must be supported on an indestructible material, as a plate of burnt clay perforated with holes. The filter may consist of a common garden-pot, or similar vessel, with holes at the bottom: the lower part may be filled with round pebbles, then some smaller pebbles, then some coarse sand, and finally a stratum of pounded charcoal, of about three or four inches in thickness. It is a great mistake to put any material, as sand, above the charcoal, with the view of arresting the grosser particles of impurity, as the sand will quickly stop up, and be impervious to water. A filter thus prepared will render water perfectly clear and sweet for many years.

On a Method of making Bricks of any required Colour. By Mr. Dobson.

On Coal-Mine Ventilation. By Mr. Fourness.

Models were exhibited and partially explained of a suspension bridge of wire, erected over the river Avon, near Bath, by Mr. Dredge.—— A method of Pumping Water from Leaky Vessels at Sea, by Mr. Dalziell. The machine is worked with a piston, the motion of the vessel being given by the steam when the vessel is sailing, to paddle-wheels on the sides.——An instrument for measuring Timber, by J. Smith.——A peculiar Combination for the Wheel-Work of a Crane, by W. Horner.

On the Water-works of Newcastle-on-Tyne. By JOSEPH GLYNN, *F.R.S., M. Inst. C.E., &c. &c.*

In the month of April 1833, Mr. Glynn was requested, by a body of gentlemen in Newcastle, to report to them the best means of supplying the dense and increasing population of the town with water, the want of which was daily becoming more urgent.

A water company already in existence, incorporated by an act or charter of ancient date, claimed a right to all the springs which had been or might be discovered for the supply of the town. But as all the higher portions of land have been perforated by coal works and quarries, and the town stands on a hill sloping rapidly to the Tyne, it was obvious that from natural springs or from wells an adequate supply could not be obtained. In a long course of years the shares of the ancient company had fallen into the hands of a few persons not disposed to invest their money in new and extensive works; their pipes were chiefly of wood, with a small portion of leaden pipes, none exceeding six inches in diameter, and all incapable of bearing much pressure. It was therefore necessary to have recourse at once to the river Tyne. Recent experiments on the largest scale had shown that the water of rivers flowing past populous towns might be applied with advantage to the purposes of domestic uses, after being rendered free from impurity by artificial filtration. For the lower or filtering station Mr. Glynn fixed on a field at the river-side, near Elswick, $51\frac{1}{4}$ feet above low-water mark, this elevation lessening the great height to which it was requisite to force the filtered water. The upper reservoir, near the Quarry House on the west turnpike road, being at its lowest part 237 feet above low-water mark, or about 186 feet above the level of the works at Elswick. The bottom of this reservoir is level with the arches of the crown on the tower of St. Nicholas church above the statues on the corner pinnacles.

As the town then contained a population, by the latest estimate, amounting to 54,000, and about 3500 houses of 10*l.* and upwards of annual value, the number of which was fast increasing, it was recommended that the new works should be capable of supplying at least 400,000 gallons per day of filtered water, and that the upper reservoir should contain not less than four days' water at that rate of consumption. As the flood-tide brings up a portion of salt water, it is necessary that the supply should be raised when the tide is down, and to pump it in a short time. A steam-engine, having a cylinder 54 inches in diameter, with a stroke of 8 feet, was therefore erected at the works at Elswick, with three boilers, any two of which are capable of working the engine.

The engine works two pumps of different sizes; one of them raises the daily supply from the river in three hours, and the other forces it in eight hours to the upper reservoir, through a main pipe of 14 inches diameter and 1700 yards in length.

The water when raised from the river is received into "subsidiary tanks" of brickwork set in Roman cement, of which there are two, each of them containing a day's supply; by this means it will take 24 hours to settle and deposit the earthly particles suspended in it before it be suffered to run upon the filtering apparatus, which is a large brick tank, containing a series of beds or strata of sand and gravel, resting upon brick tunnels set in Roman cement, the uppermost bed being of fine sand, the lowest of pebbles. The area of the filter bed is 10,000 square feet; the water filters by descent, and is received into the tunnels, whence it is forced by the steam-engine through the main pipes a distance of 1700 yards, to a height of 200 feet to the high reservoir, and is thence distributed in cast-iron pipes of various diameter over the whole of the town. These works have been completely successful. The ancient company have disposed of their charter and their wooden pipes to the directors of the Subscription Water Company, who, during the meeting at Newcastle, opened their works for the inspection of the members of the British Association.

STATISTICS.

On Educational Statistics of Newcastle. By Mr. CARGILL.

From this paper it appears, that of a population of 64,000, and of 16,000 children between the ages of 5 and 15, there are 7761 or $48\frac{1}{2}$ per cent. of the whole, apparently receiving no education, either real or nominal, in schools. In a district of the town, every place of abode in which was visited, and the number of children from 3 to 15 years of age found to be contained 4352, it appears $44\frac{1}{2}$ per cent. were found able to read and write: of 736 persons who registered births and deaths in all the parishes, 513 could not write, or 41 per cent: of 1264 persons committed to prison, chiefly for grave offences, during 18 months, only 30 could read and write well. A large proportion of the schools for the poor were found totally inadequate for the purposes of real education, and the teachers often as ignorant as the scholars. That the inhabitants of a large district (26,000), were in general living in a state of misery and apparent destitution, though the wages earned by a great part were as high as 18*s*., 21*s*., 25*s*., 30*s*., and even as high as 40*s*. per week,—showing the inadequacy of high wages alone to produce the happiness arising from order, cleanliness, and mental culture, when improvidence, drunkenness, &c. &c. retain a hold of the working classes.

On the Church- and Chapel-room in All Saints' Parish, Newcastle. By D. H. Wilson, *in a Letter to* A. Nichol, *Esq.*

NAME.	DENOMINATION.	SEATS.	AUTHORITY.
All Saints'	Established Church	1400	Church of England Circular in behalf of Byker Chapel of Ease.
St. Ann's	Ditto	500	Ditto.
Silver Street	Primitive Methodist	900	Private information, under rather than over the truth.
Friends' Meeting	Quakers	500	Richardson's Companion through Newcastle.
Roman Catholic	Roman Catholic	1500	Ditto.
Carlisle Street	Presbyterian	700	Ditto.
Bethel	Ditto	500	Ditto.
Walknoll	Ditto	500	Ditto.
Forster Street	Glassites	120	Private information.
New Road	Wesleyan Methodist	1200	Richardson's Companion through Newcastle.
Ebenezer	New Connexion	150	Ditto, but reduced in size since his report.
Gibson Street	Wesleyan Association	1200	Ditto.
Stepney Bank	Wesleyan Methodist	200	Private information.
Ballast Hill	Primitive Methodist	150	Ditto.
St. Peter's Quay	New-Connexion Methodist	500	Richardson's Companion.
St. Lawrence	Wesleyan Methodist	150	Private information.
Sandgate	Independent	200	Ditto.
Sailor's Bethel, Quay	Various Dissenters	130	Ditto.
Trinity Chapel	Church of England	60	Ditto.
		10,560	

A Return of Prisoners coming under the cognizance of the Police in Newcastle, from the 2nd of October, 1837, *to the 2nd of August,* 1838. *By* John Stephens.

The total number was 2169; of whom 1274 were convicted, 20 acquitted, and 875 discharged; but of the number of offenders 267 were strangers in the town. There were only 17 cases of manslaughter, highway-robbery with violence, burglary, and shop-burglary, and 12 of these offences were by strangers. Of the rest of the offences, 325 were for assault, and 617 being drunk and disturbing the public peace, and 283 lying insensibly drunk in the streets: so that nearly one-half of the offences were for drunkenness. The committals, to the population, were 1 in 275 inhabitants, or 3·4 per cent. Seventy-eight of the offenders returned to an honest means of livelihood.

The offences under the Bye-Laws, Town Improvement Act, Beer Acts, &c., independently of the above, were 382; of which number 98 were discharged. In this return the disorderly beer-houses appeared to be 30. The total offences therefore are 2551, or 1 in 25·1 inhabitants, or 4 per cent; but this apparently large proportion of committals is equalled in London, where in 1836 every 1 in 24 of the inhabitants, or 4·09 per cent were committed.

Statistical Notices of the Asylum for the Blind lately established at Newcastle-upon-Tyne. By Rev. J. M'Alister.

Some tables were presented to the meeting, showing the relative proportion of the blind and seeing in a particular district of the town, from which it appeared that the proportion of blind was greater than in any of the late continental returns.

The author of the paper stated, that the great number of wandering blind which frequented some parts of the suburbs, had induced a few benevolent individuals, during the last year, to direct their attention to some means for improving the condition of this neglected portion of the population. An asylum had been opened for a small number of blind at first, to test the various modes of instruction. A competent master and matron having been appointed, the inmates were soon engaged in learning various branches of manufacture suitable to their capacities. After the trial of different alphabets, they were now taught to read in books from the press of Mr. Alston of Glasgow, the alphabet employed being the raised Roman character. This system was on many accounts considered preferable to an arbitrary system of typography.

In the course of his attention to the internal economy of institutions, the author has found, that although it is very practicable to teach those born blind to *read*, yet it is a truly difficult task to teach them to *think* accurately. In every passage where a visible image is introduced to them, the meaning is more or less vitiated; and the integrity of the intellect, by indulging a habit of receiving what it cannot understand, is sadly endangered, unless a careful and peculiar mental culture accompany all literary instruction.

On the State of Agriculture and the Condition of the Ariculturat Labourers of the Northern Division of Northumberland. By Mr. Hindmarsh.

In this paper the author gave an interesting account of the mode of employing labourers, in the north of Northumberland, by the system of bondagers, who are employed by the hinds, and the hinds are themselves in the employment of the farmers.

(See Statistical Journal, Nov. 1838.)

Dr. Taylor read a paper, communicated by Saxe Bannester, Esq., on the population of New Zealand. It described the state and condition of the natives, the white residents, the white visitors, and the mixed race. This paper also related the laws of New Zealand relating to land, and very minutely detailed the moral, social, and political condition of the white settlers, the progress of commerce, and more especially, the exports and imports of the Bay of Islands.

Statistical Report of the Parish of Bellingham in Northumberland. By W. H. CHARLTON.

This report comprised a detailed reply to the statistical queries put forth by the Society in London.

(See Statistical Journal for November 1838.)

The following are extracts:—

			Increase per cent.
The Population in	1811	was 1232	
......	1821	.. 1396	$13\frac{1}{4}$
......	1831	.. 1460	$4\frac{1}{2}$

State of Education—Parish of Bellingham.

Number who could read, 1319.			Number who could write, 551.		
Families.	Members.	Readers in each Family.	Families.	Members.	Writers.
77	461	All.	2	14	All.
26	99	1	86	505	1
54	288	2	62	390	2
32	201	3	29	196	3
35	238	4	23	203	4
25	184	5	6	40	5
17	152	6	4	41	6
14	126	7	3	28	7
7	63	8	1	9	8
4	45	9	1	14	9
6	72	10	2	35	10
1	21	11	86	528	None.
11	50	None.			
309	2000	...	309	2000	...

On the Statistics of Ramsbottom. By P. M'DOWALL.

The following are extracts:—*Houses*: No. of cottages, 309; with good furniture, 294; with bad, 15; with one bedroom, 137; with two, 172. Several of these families numbered from 2 to 13 persons. *Families*: males, 968; females, 1032;—total 2000: married, 285; and having no children, 5. No. of widows, 24. *Lodgers*: males, 50; females, 33;—total 83. Ages: above 50, 81; 60, 28; 70, 11; 80, 3. Total number who receive wages, 1134. Total number who could read, 1319. Total number who could write, 531.

Statistical Tables of the Engines, Ventilation, Screens, Sales, &c.; and Pitmen; and the Strata of Nine principal Collieries in the County of Durham: the first eight being situated on the East or "Dip" Side of the Great Durham Coal-field. By W. L. Wharton. The following is an abstract:—

SCREENS.

	Distance between Bars.	Small Coal by Screening.	Small Coal consumed by Engines.	Small Coal left unsaleable.	Small Coal left in Mine.
Manor Wallsend Colliery	$\frac{3}{8}$ in.	One-fourth of quantity drawn	3445 tons per annum	None	None.
Monkwearmouth Colliery	$\frac{1}{2}$ in.	One-third do.	85 tons per week	None	——
Newbottle Colliery, Littletown Colliery	$\frac{1}{4}$ and $\frac{5}{8}$ in.	One-sixth and one-third do.	52 do	None except dust	None.
Hetton Colliery	$\frac{3}{8}$ in.	28 per cent. do	5 per cent.	$1\frac{3}{4}$ per cent. (4)	5 per cent., in some cases none.
South Hetton Colliery	$\frac{1}{2}$ in.	One-fourth to one-third do.	....	None except dust	None.
Haswell Colliery	$\frac{3}{8}$ to $\frac{1}{2}$ in.	29 per cent. do	10 per cent. of small; remainder sold or used by workmen.	Do	None.
Thornley (Hartlepool) Colliery	$\frac{3}{8}$ in.	nearly $\frac{1}{4}$ (adding refuse)	....	None except dust, amounting to nearly $\frac{1}{3}$ of *small* c.	——
Black Boy (Tees W.E.) Colliery	7-16 in.	25 per cent.	60 tons per week	....	$\frac{1}{200}$

SALES, &c.

	Average shipping prices in 1836-7.		Tons sold in London Coal Market in 1837.	Prices, at Coal Market, May and Sept. 15, 1837.	
	s. d.	s. d.		s. d.	s. d.
Manor Wallsend Colliery	9 9	...	48,005	21 0	0 0
Monkwearmouth Colliery	9 3	11 6...	16,768	——	
Newbottle Colliery, Littletown Colliery	11 6	... ...	166,791	22 6	24 0
Hetton Colliery	11 6	... ...	204,668	22 9	24 0
South Hetton Colliery	11 6	... ...	121,628	22 9	23 9
Haswell Colliery	11 6	... ...	84,471	22 6	23 9
Thornley (Hartlepool) Colliery	10 4	11 6...	57,774	22 6	24 0
Black Boy (Tees W.E.) Colliery	10 9	... ...	65,563	22 0	23 0

A Series of Statistical Illustrations of the principal Universities of Great Britain and Ireland. By the Rev. H. L. Jones, *M.A.*

These illustrations consisted principally of tables, in which Mr. Jones calculated the amount of income derived from the ancient endowments of the colleges, and from various other sources of revenue, the degrees conferred in the Universities, and the number of members qualified by their degrees to take a part in the government of the Universities.

At the present time, it appears that the total number of members in each of the two great English Universities, of Oxford and Cambridge, exceeds 5000, and that the number of resident members in each of these Universities is about 1600.

Mr. Jones states that the total annual expenditure of 1600 members amounts to about 480,000*l.* per annum in Oxford, and about 400,000*l.* per annum in Cambridge.

A Description of the "London Fire Engine Establishment," and of the Number, Extent, and Causes of the Fires in the Metropolis and its Vicinity, during the Five Years from 1833 *to* 1837. *By* W. R. Rawson.

It appears that the average annual number of fires during this period was 495, the number of alarms from chimneys on fire 108, and the number of false alarms 68.

Of the total number of fires, nearly one third occasion serious damages, and in 6 out of 100 instances the buildings were wholly consumed. In 13 instances, 2 buildings were destroyed by a single fire; in 4 instances, 3 buildings; in 6 instances, 4; in 2 instances, 5; and in 1 instance, 8.

The number of fires is on the increase, but probably not out of proportion to the great increase of buildings.

The number of fires accompanied by loss of human life was 41; the number of lives lost was 57.

It appears that although the greatest number of fires have occurred in December, yet the number from May to October slightly exceeds that from October to May.

Private houses and the dwelling parts of other houses furnished nearly one half of the whole number of fires. Next in frequency were sale-shops or offices, victuallers, carpenters, bakers, oil- and colourmen, stables, cabinet-makers, tinmen, booksellers, warehouses, hatmakers, &c. The number of fires in the houses of lucifer match-makers was considerable.

The causes of fires have been discovered in four-fifths of the cases. Of these causes accidents from candles form by far the largest class, or 3 in 10 cases; and of these 43 per cent. arose from the setting on fire of bed-curtains; 22 per cent. from the setting on fire of window-curtains, and 35 per cent. from other accidents. The next most frequent cause is the defective construction or imperfect cleansing of flues, chimneys, and stoves, amounting to 22 per cent. of the whole number. The number of accidents from gas was $7\frac{1}{2}$ per cent., from fire-heat applied to various processes 7 per cent., and from linen hung before the fire 8 per cent. These are the principal causes. The number of wilful fires was small, only 1 in 64 fires.

As regards insurance, two-fifths of the houses were wholly uninsured; one-third were insured for both building and contents; 11 per cent. were insured for the building only, and 17 per cent. for the contents only.

During the five years under review only 5 large fires have occurred at which property to the extent of more than 20,000*l.* has been destroyed. These were the Houses of Parliament, in October, 1834; a fire in Silver-street, Golden-square, in March 1835; that at the Western Exchange, in March 1836; that at Fenning's Wharf, in August of the same year; the fire at Davies' Wharf, in December 1837. The only large fire this year was at the Royal Exchange, on the 10th of January.

Abstract of the Report of the Railway Commissioners in Ireland. By W. R. Rawson.

The Commissioners of Railways for Ireland have founded their recommendations upon the distribution and employment of the population, the commerce, traffic, and number of passengers in the various districts, the facilities which the geological features of the country present, and the comparative power of the several districts to avail themselves of railway communication.

It appears that the population is most dense in the northern counties; next in order are the midland counties, east of the Shannon; and then the southern counties of Tipperary, Limerick, and parts of Cork and Waterford. The population in the north is in the best condition, then that in the south; the midland districts nearly resemble the southern, but the western is far inferior to either.

The order of the principal towns as regards the amount of their traffic, is as follows: Dublin, Cork, Belfast, Limerick, Waterford, Galway, &c. Their order as regards the value of imports and exports is the following: Belfast, Dublin, Cork, Waterford, Londonderry, Newry, Limerick, &c. The returns of the amount of passengers, traffic, of postage collected, and of banks, confirm the preponderance of Dublin, Cork, Belfast, Limerick, and Waterford. The efforts therefore of the commissioners have been directed to lay down lines of railway between these towns. The geological features of the country offer peculiar facilities for effecting this object, the principal part of the proposed lines being carried through a country of carboniferous limestone, the most level and easy formation for such works.

The commissioners propose two main lines, one to the south from Dublin to Cork, throwing off branches to Kilkenny and Limerick, the expense of which they estimate at 2,329,000*l.*, and the annual profit at $3\frac{1}{2}$ per cent. upon that sum. Connected with this line is a branch from Limerick to Waterford, the cost of which is estimated at 400,000*l.*, and the annual dividend at $3\frac{3}{4}$ per cent.

The proposed lines in the north are, from Dublin to Cavan, and thence two branches, one to Armagh, to join the line already commenced to Belfast, and the other in a north westerly direction to Enniskillen. The cost of these two lines is estimated at 2,015,000*l.* If this line, which will be more expensive than the south, cost 11,000*l.* a mile, the annual return will be $4\frac{3}{4}$ per cent.

It is impossible upon the present occasion to enter more fully into the extensive and important subjects embraced in this report.

Abstract of Statistics respecting the Working Classes in Hyde, Cheshire. By W. Felkin.

The township of Hyde contained 830 inhabitants in 1800—upwards of 11,000 in 1837. The overseers' report of expenditure for the latter year was, to 9 resident paupers, 56*l.* 4*s.* 10*d.*; 3 deceased paupers, 15*l.* 2*s.* 6*d.*; 3 insane, 20*l.* 6*s.* 8*d.*; 18 paupers living out of the township, 115*l.* 9*s.* 10*d.*; being 207*l.* 3*s.* 10*d.* to 18 men and 15 women (of the latter, 4 widows); total, 33 paupers; added to which, expenses for law, and removals, and medicine, 17*l.* 16*s.* 5*d.*; workhouse, 6*l.* 13*s.* 6*d.*; overseer and sundries, 84*l.* 19*s.* 0*d.*:—making 316*l.* 12*s.* 9*d.* total expenses for paupers.

Population engaged entirely in factory and retail trade, and getting the coals they consume; highway rate 750*l.* for same year. In 1800, the poor rate was 12*s.*, in 1837, 6*d.* per head, per annum. On examining one of the factories (employing 1600 hands), and the dwellings of many of the hands, there was found one public house, and no spirit, beer, or pawn shop; well furnished cottages generally; a good rate of wages, 25*s.* men, 12*s.* women, 4*s.* children; average 12*s.* 6*d.* of the whole. 73 men's earnings were 78*l.* 15*s.* 0*d.* a year. The earnings of 120 families averaged 6*s.* $10\frac{1}{2}$*d.* including every individual composing them. Their income was about 120*l.* a year; 3 felonies had been committed in 35 years by those employed in this factory. None had ever been pauperised, nor had pauper relatives living in the town, when the census took place a few weeks ago. Ten of the workmen possess 46 cottages, rated at 7*l.* or 8*l.* each, bought by their savings; and about 300 more belong to probably 40 others. These houses are good and substantial dwellings. They are fond of music, and cultivate it; no dwelling was without a Bible or New Testament, and generally other books and pictures. Children well taught and in good schools; many were seated at meals both substantial and good. But few gardens or flowers. Intemperance gradually decreasing; and though many well-paid families live always upon credit, yet prudence, benevolence, and good morals generally prevail, not only in these but amongst 4 or 5000 of the surrounding workpeople.

Other large manufacturing establishments which have been visited, show the excellent influence and effects of wise and economic principles and management upon the happiness of the workpeople and the community.

A Short Account of the Darton Colleries' Club. By T. WILSON, *F.S.S.*

The Society thus designated must be considered purely as an experiment—as an attempt to ascertain, in certain circumstances, on what terms a miner might insure *to himself and his family* a certain "relief during any illness that might arise from any *accident* happening to them while at work."

The Club was established in February 1833 at Silkstone Colliery, near Barnsley, and has since been adopted at the Darton Colleries. The subscriptions are deducted from the wages of the men, the allowances have been paid and the accounts kept by the owner of the mine, and the meetings have been held at the Colliery; so that all expenses have been avoided, and the funds may be considered as wholly applicable to the purposes of relief.

A member whose wages are less than 7*s.* a week, pays 6*d.* on entrance, and one halfpenny per week, and receives when ill 3*s.* 6*d.* a week so long as the illness continues; all whose wages exceed 7*s.*, pay 1*s.* on entrance, and one penny per week, and receive when ill 7*s.* a week.

YEARS.	No. OF SUBSCRIBERS.			Accidents chargeable.	RECEIPTS.			PAYMENTS.		
	Minimum.	Maximum.	Average.		£	s.	d.	£	s.	d.
1833	93	125	107	5	24	7	11	7	14	0
1834	115	232	158	33	41	9	2½	24	14	0
1835	197	228	212	52	47	3	3	40	7	4
1836	200	243	224	45	52	8	1	51	9	6
1837	217	272	245	53	56	12	8	72	16	0
1838 August.	262	289	277	22	36	12	7	11	2	0
					258	13	8½	208		10

The surplus fund is now £50 10*s.* 10½*d.*

No account of the cause and nature of the accidents was kept previous to July 1836, since which time 96 accidents have occurred which have been chargeable to the Club. Of these, 90 have been reported with their causes, of which the following is a summary:—

25 from the roof falling,
20 from the coal falling,
19 from corves hurting them,
6 from falls,
7 from wounds from tools,
8 from various things falling on them,
5 from fire damp.

90

A Letter was read to the Statistical Section, from the Rev. Dr. POTTER, accompanying a donation of the last Annual Reports of the Regents of the University of the State of New York.

A Statistical View of the recent Progress and present Amount of Mining Industry in France, drawn from the Official Reports of the "Direction Générale des Ponts et Chaussées et des Mines." By G. R. PORTER.

The data from which the reports of the French Mining Engineers are drawn are collected under the authority of a law passed by the Legislative Chambers in 1833; and Mr. Porter draws attention to the fact, that the productiveness of mining industry in France has increased in a greatly accelerated ratio since that time as compared with previous periods, which circumstance he considers is, in part at least, attributable to the suggestions made to proprietors of mines and works by the engineers, a highly educated and intelligent body of men, to whom the task of inspection is confided; and occasion is thence taken to point out the desirableness of adopting some system for the collection of similar data in this country. The value of the coal, iron, lead, antimony, copper, manganese, alum, and sulphate of iron produced in France, has been increased from 4,230,000*l.* in 1832 to 6,170,000*l.* in 1836, or 45 per cent, while the increase in a like period preceding the visits of inspectors amounted only to 12,000*l.*, or very little more than a quarter per cent (0·28).

Coal is produced in thirty of the departments of France. There are in these 258 mines in operation, giving employment to 21,913 workmen.

The quantity raised in 1814 was 665,000 English tons. Double that quantity was raised in 1825. In 1832, before the plan of inspection was adopted, the produce was 1,600,000, and in 1836 was raised to 2,500,000 tons.

There are iron-works in sixty out of the eighty-six departments of France. The number of works in 1836 was 894, and of workmen 15,738: the product, 303,739 English tons of pig-iron, and 201,691 tons of bar-iron, valued at 3,580,000*l.* Taking into account the further processes connected with this branch of industry, the total value created was in 1836, 4,975,000*l.*, and the workmen were 43,775. In 1824 the quantity of pig iron made was 194,636. In 1832 (the year preceding inspection), 221,660 tons, and in 1836, 303,739 tons.

Some further particulars as to the modes of manufacture were given, and slight notices of other branches of mining industry. It appears that, taken in its full extent, this class of employment gives support to 273,364 workmen, the value of whose labour is 15,100,000*l.*; this includes the produce of stone-quarries, salt-works, glass-works, pottery, and various chemical products having a mineral origin.

On the Statistics of Vitality in Cadiz. By Colonel SYKES.

From this elaborate memoir the following are extracts:

Population.—Cadiz has 4 parishes within the walls and 1 outside, and for police and municipal objects it is divided into 12 barriers or districts. These barriers comprise a population of 58,525 souls, agreeably to the census of December last, the males being 27,301, and the females 31,224.

Census of Cadiz, as the same stood on the 1*st of December,* 1837.

Quarters or Parishes.	Districts.	Males.	Females.	Total.
No. 1. Santa Cruz	1. De las Escuelas . . .	2,306	2,780	5,086
	2. Del Populo	2,208	2,235	4,443
	3. De la Merced. . . .	4,203	4,327	8,530
2. Rosario .	4. De San Carlos . . .	392	510	902
	5. De San Francisco . .	1,321	1,375	2,696
	6. Del Correo	1,749	1,993	3,742
3. St. Antonio	7. De la Constitucion . .	2,235	2,590	4,825
	8. Del Hercules . . .	2,072	2,842	4,914
	9. De las Cortes	1,410	1,927	3,337
4. St. Lorenzo	10. De la Palma	2,514	2,519	5,033
	11. Del Hospicio	3,386	3,861	7,247
	12. De la Libertad . . .	2,833	3,709	6,542
	Parish outside of the Walls.	672	556	1,228
		27,301	31,224	58,525

From the tables of Burials are deduced the following results, manifesting even in a more marked manner than in other countries, the excess of mortality amongst males, whether adults or children, over females.

	Total Deaths.	Annual Average Deaths.	Per Cent. of Deaths.
In 38 years the total deaths were	110,345	2640	4·51, or 1 in every 22·13 inhabitants.
In 34 years, striking out the 4 years of yellow fever	85,786	2523	4·31, or 1 in every 23·2 inhabitants.
In the 4 years of yellow fever	24,559	2903	4·96, or 1 in every 20 inhabitants nearly.
Average of last 15 years, no yellow fever	35,007	2334	3·98, or 1 in every 25·07 inhabitants.
For the year 1837–8	2,258	2258	3·86, or 1 in every 25·91 inhabitants.
Children for 38 years......................	42,554	1119	38·56, or 1 in every 2·59 deaths.
Proportion of children to the whole deaths, 1837-8.........................	...	...	47·08, or 1 in every 2·12 deaths.
For the first 25 years, the deaths of men to the whole deaths............	81,007	...	35·77, or 1 in every 2·79 deaths.
Ditto of Women to the whole deaths.	81,007	...	26·46, or 1 in every 3·77 deaths.
Ditto of boys to the whole deaths......	81,007	...	22·02, or 1 in every 4·54 deaths.
Ditto of girls to the whole deaths ...	81,007	...	15·75, or 1 in every 6·35 deaths.
For 1837-8, the male deaths to the male population of 27,301 were...	...	...	4·25, or 1 in every 23·49 males living.
For 1837-8, the female deaths to the female population of 31,224 were	...	...	3·51, or 1 in every 28·48 females living.
For 1837-8, the deaths of men to the whole deaths	...	...	24·62, or 1 in every 4·06 deaths.
For 1837-8, the deaths of women to the whole deaths......................	...	...	28·34, or 1 in every 3·53 deaths.
For 1837-8, the deaths of boys to the whole deaths	..	...	26·83, or 1 in every 3·72 deaths.
For 1837-8, the deaths of girls to the whole deaths......................	...	...	20·25, or 1 in every 4·94 deaths.

The next subject is the Parochial Returns of Births, Deaths, and Marriages.

Average of the Annual Births, Marriages, and Deaths, in the different Parishes of Cadiz, from the year 1827 *to* 1836 *inclusive.*

	BIRTHS.		Marriages.	DEATHS.			
	Boys.	Girls.		Men.	Women.	Boys.	Girls.
Parish of Santa Cruz	250·8	238·9	127·3	113·8	132·0	83·4	76·6
Parish of the Rosary.........	106·1	93·4	45·1	48·0	54·6	18·5	18·6
Parish of St. Antonio	102·4	103·8	55·9	90·4	83·7	16·9	19·4
Parish of St. Lorenzo	258·9	239·7	97·5	105·8	156·4	98·6	88·4
Parish of Castrense	29·3	28·3	9·1	12·9	8·3	4·6	2·9
Parish of St. Joseph without the walls	21·0	18·5	7·7	10·2	9·5	7·9	7·2
Total annual average...	768·5	722·6	342·6	381·1	444·5	229·9	213·1
	1491·1			825·6		443	
				1268·6			
Absolutely buried in the Cemetery..........................				1332·2		858	
				2190·2			

EDUCATION.

Return of the Establishments for Education in Cadiz, distinguishing Day Scholars from Boarders and Males from Females, January, 1838.

ESTABLISHMENTS.				DAY SCHOLARS.		BOARDERS.	
Colleges.	Schools.	Academies.	Small Schools.	Males.	Females.	Males.	Females.
2	...	...	...	189	...	81	...
...	29	...	...	1,989	...	12	...
...	...	43	...	16	1,030	...	4
...	...	...	20	37	167	...	1
			Total ...	2,231	1,197	93	5
					Total ...	2,324	1,202
				Total Males and Females.........		3,526	

The total number of children attending schools being 6·02 per cent. of the population, or one in every 16·51 inhabitants; and supposing the children between 5 and 15 years of age to be 25 per cent. of the population, or 14,631, the per-centage receiving instruction is only 1 in 4·13, or 24·1 per cent.—a proportion much below the lowest averages yet ascertained in England, as Liverpool $47\frac{1}{4}$ per cent., and Newcastle $51\frac{1}{2}$ per cent. But the 455 poor children receiving instruction in the Hospicio are to be added, and this will slightly improve the averages; making the number of children educated 27·2 per cent., or 1 in every 3·70.

On an Outline for Subjects for Statistical Inquiries. By Mr. HARE, *President of the Leeds Statistical Society.*

The author observed how much the importance and value of statistical societies would be augmented by a strict attention, so far as is practicable, to uniformity in the designs they have in view, by a general agreement in reference to the principles on which they are based, the terms and numerals employed in their investigations, and the documents necessary to their elucidation.

With a view to the attainment of these desirable objects, Mr. Hare has sketched an outline of the subjects of inquiry, comprising a series of tables, intended to be filled up by different societies.

From such tables, of which there are upwards of 120, each town where a society is established may have the number and description of papers which its peculiar locality may require.

Criminal Returns of the Empire. By JEFFRIES KINGSLEY.

The author presented, in this communication, a summary of criminal returns according to the system of fiscal and criminal statistics as propounded by himself in the *Standard County Book* for the treasurers of Ireland. The subjects were arranged under the following heads:—Statistical references. Population census, 1831. Number of souls to the square mile, English. Comparative standard: Carlow, least populous county, rated as one crime. Counties in the order of population. Offences against the person. Offences against property with violence. Offences against property without violence. Offences (malicious) against property. Offences and forgery against the currency. Offences not included in the foregoing. Total of all offences. Deaths. Free pardon. Executed. Petty Sessions' courts. General Sessions' courts. Assize courts.

The return to these several heads was collected from the Prison Reports, Ireland, for 1836; but, seeking to establish a principle, the author did not hold himself responsible for the accuracy of the particulars which were stated in the return.

INDEX I.

TO

REPORTS ON THE STATE OF SCIENCE.

INDEX II.

TO

MISCELLANEOUS COMMUNICATIONS TO THE SECTIONS.

THE END.

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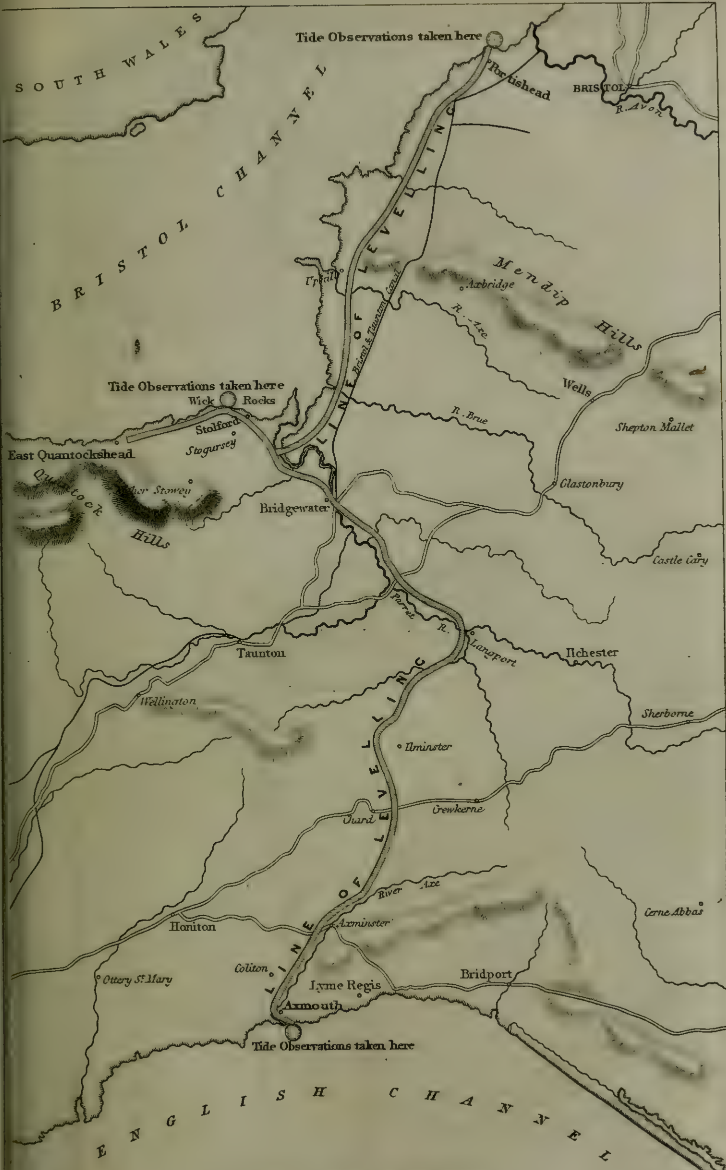
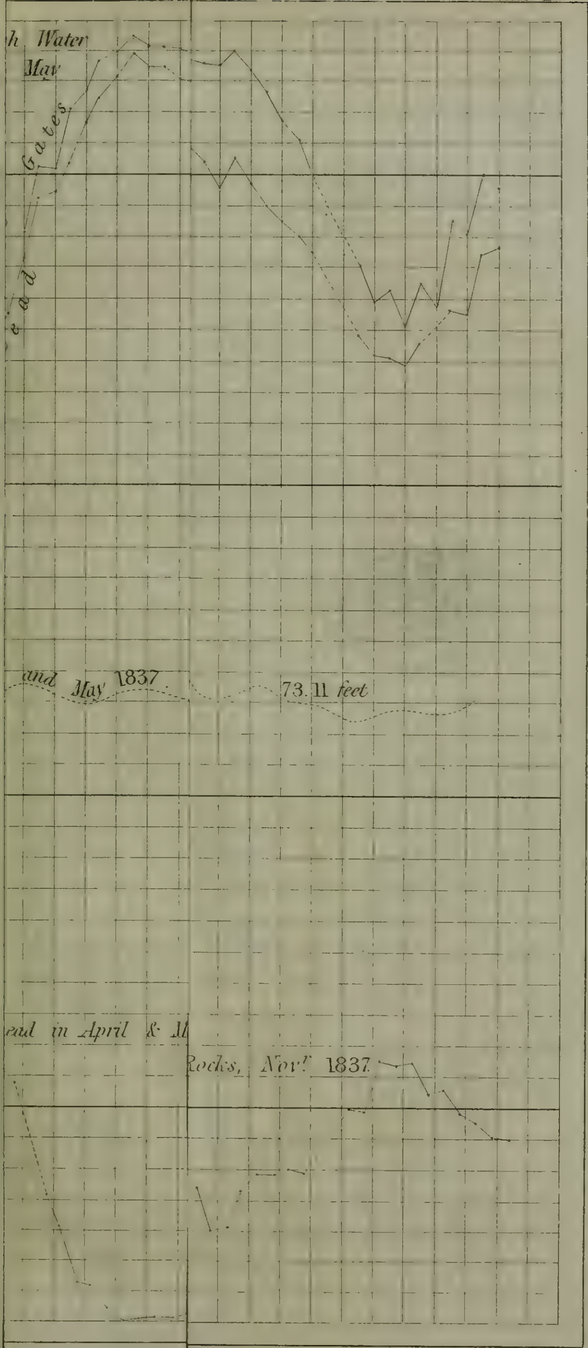


Relative Levels of the Marks.

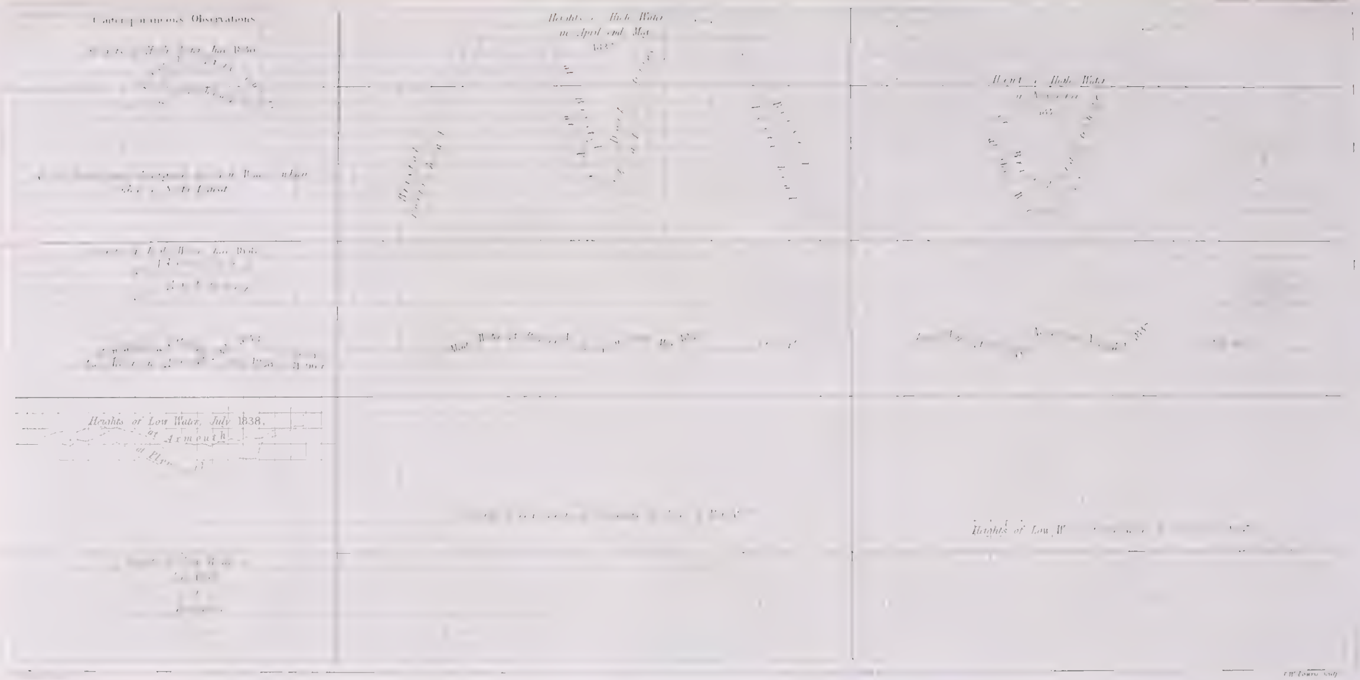
	Feet
Iron Bar at Portishead Fort	102.5795
Copper Bar at Stolford, Stogursey	125.1114
D.º at Perry Farm, East Quantockshead	244.4365
D.º in Axmouth Tower (Devon)	89.5318
D.º in Axmouth Pier (D.º)	83.6513

J.W. Lowry, sc.

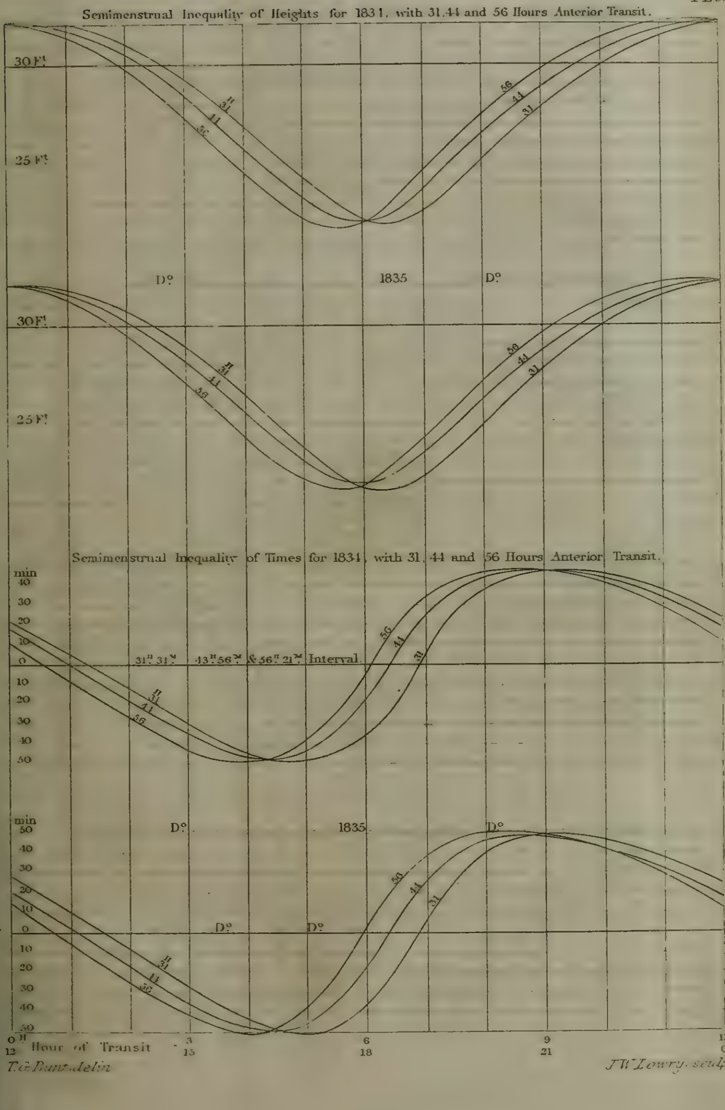
Plate 2.



J.W. Lowry Sculp.


Contemporaneous Observations
Heights of Low Water, July 1838.
Axmouth

PL. 3.



T. G. Bunt delin.

J. W. Lowry sculp.

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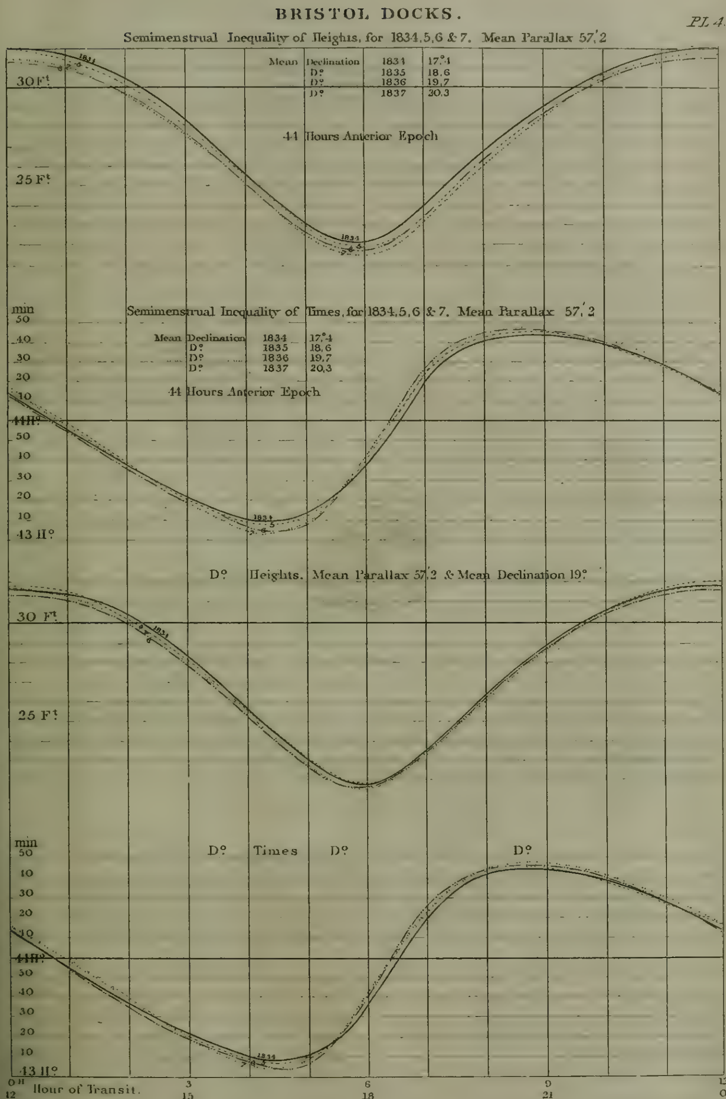

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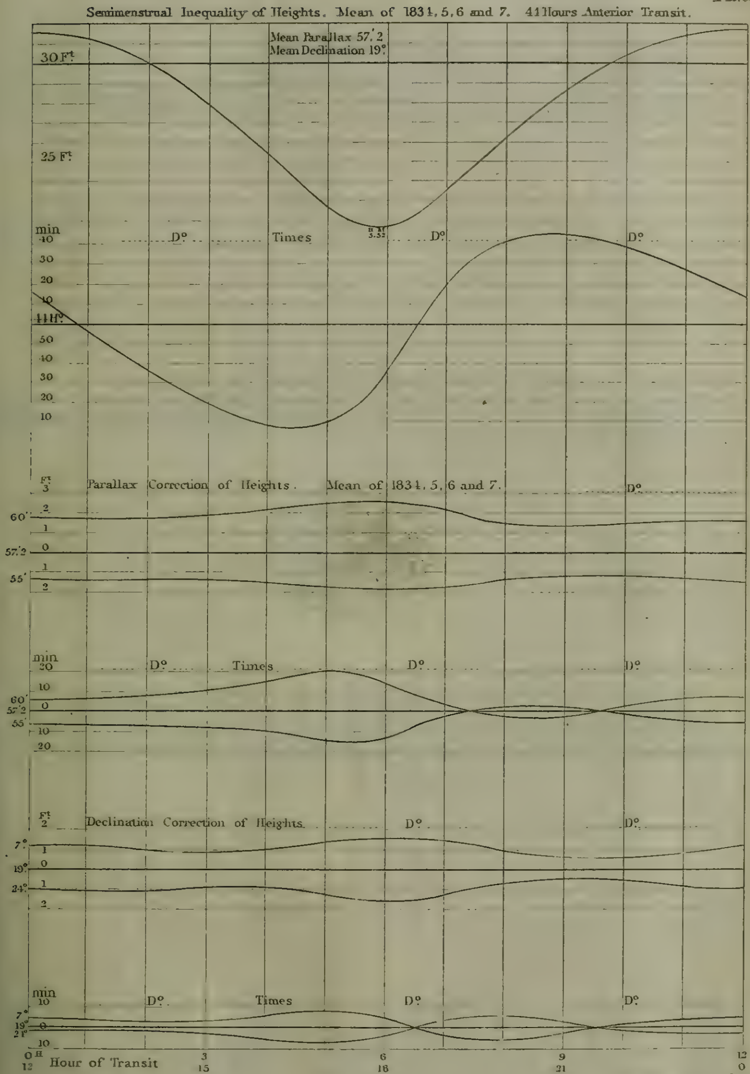
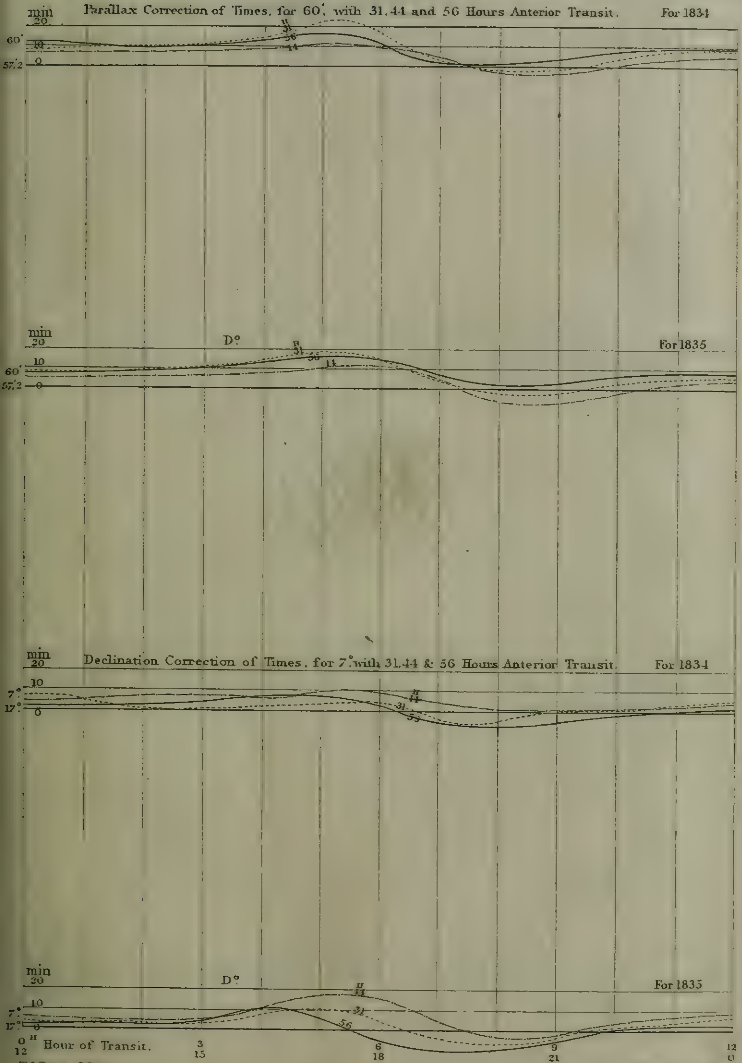
PL. 6.



T. G. Bunt, delin. J. W. Lowry, sculp.



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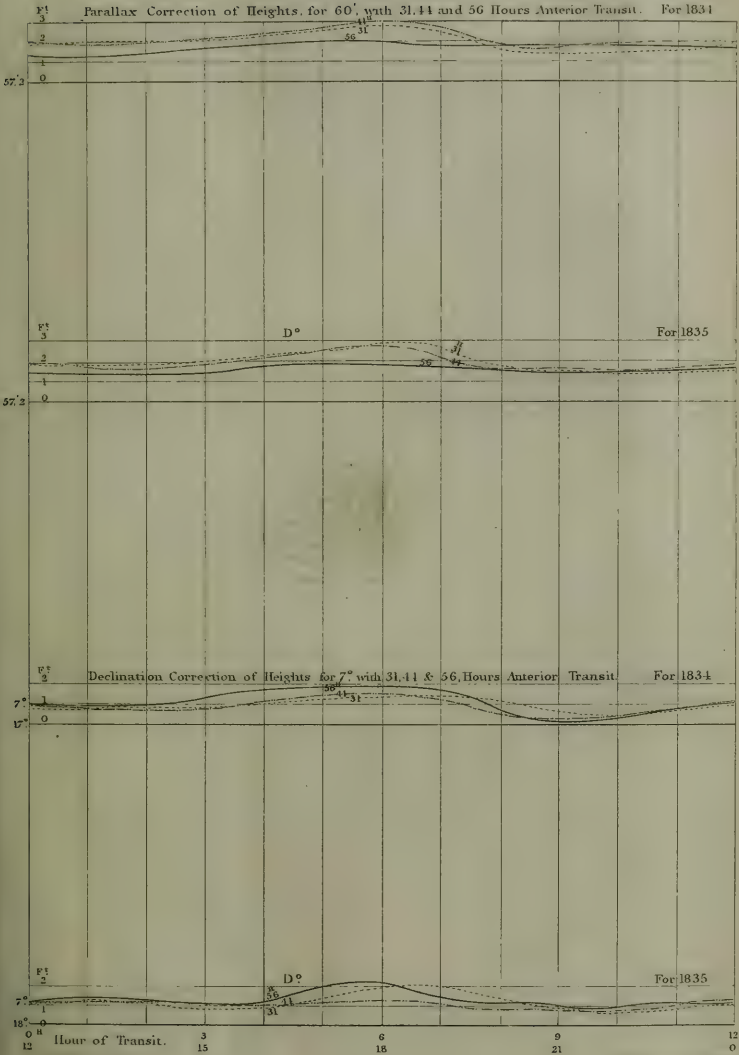
J. W. Lowry sculp.

Pl. 7.



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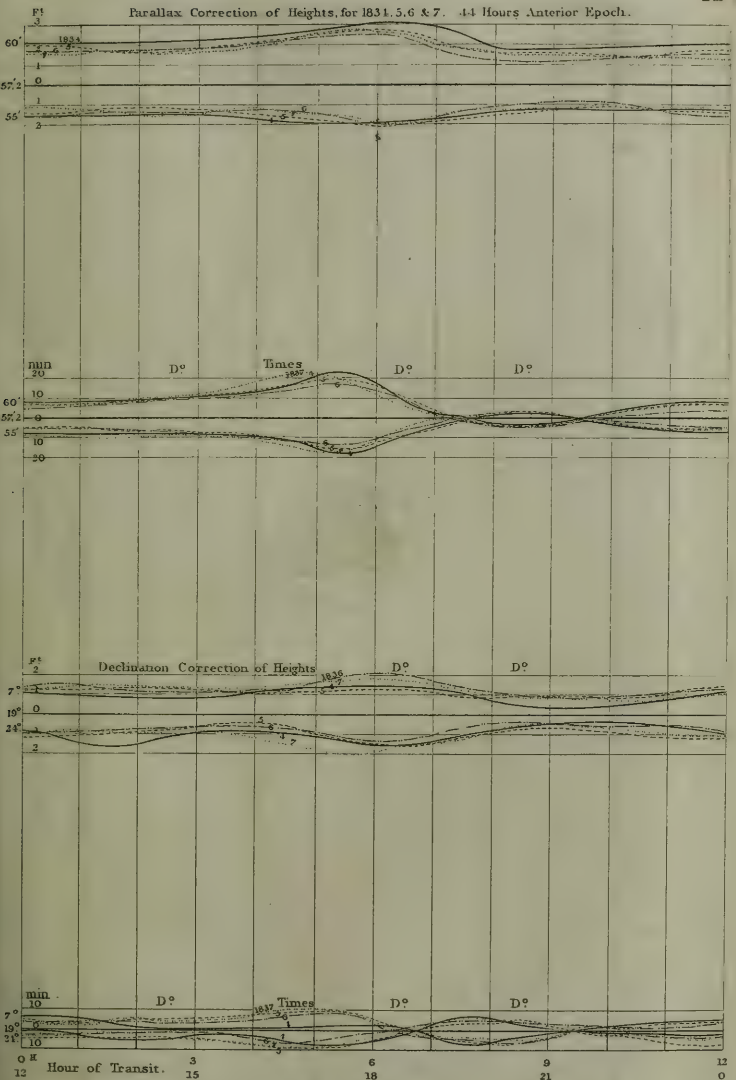
Pl. 8.

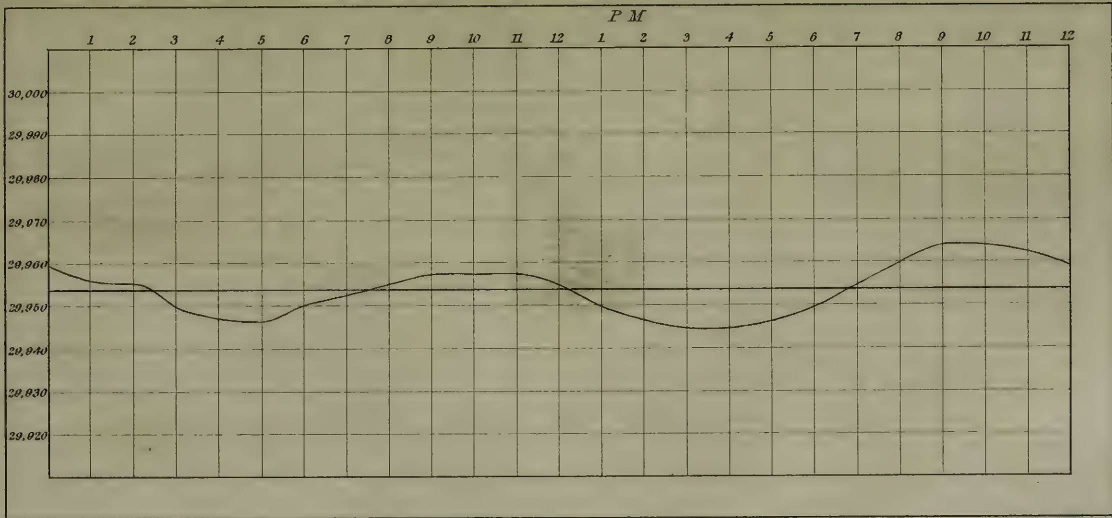


T. G. Burt, delin.

J. W. Lowry, sculp.

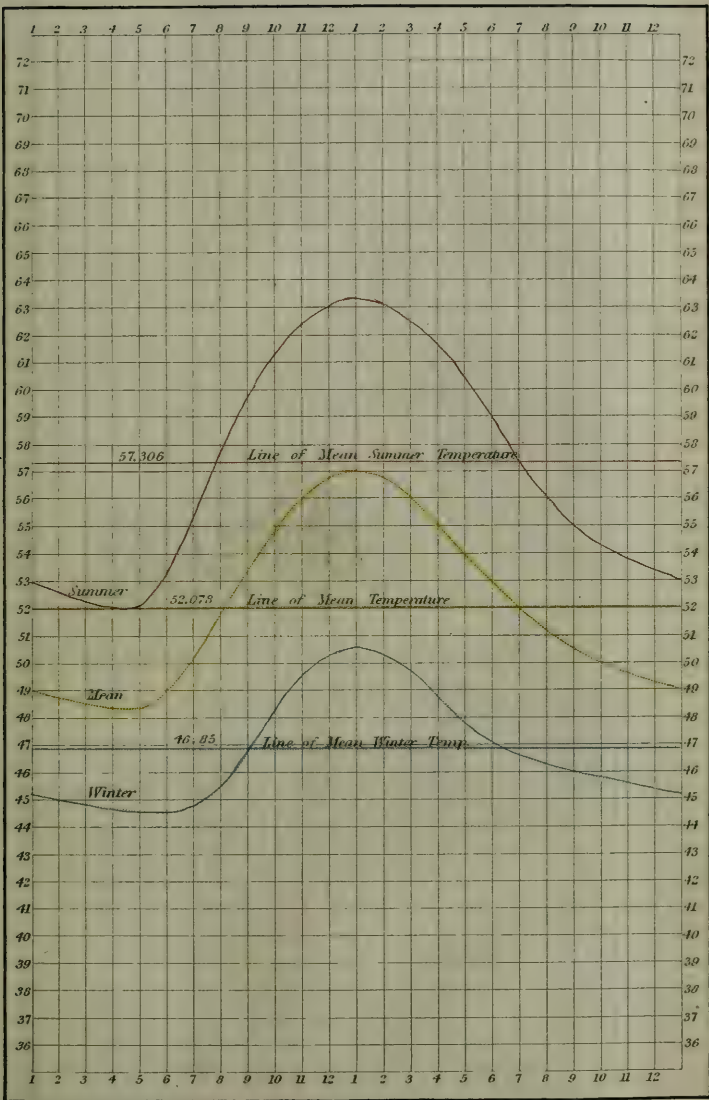
Shewing the mean hourly preſsure for the year 1837.

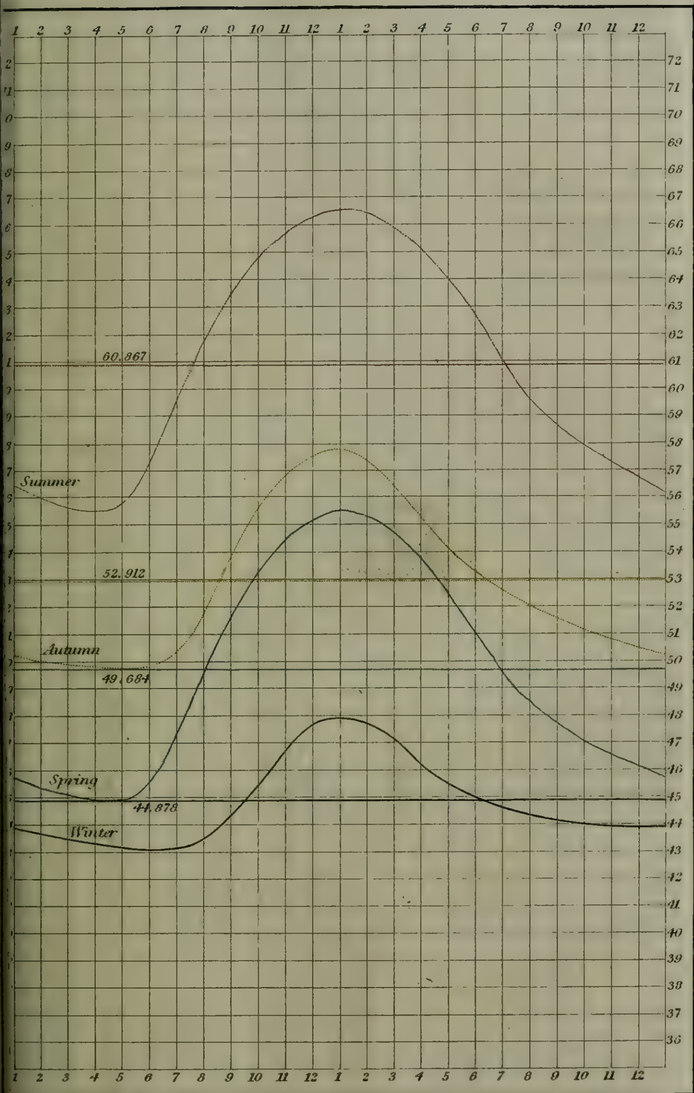
Plate 9.



J. W. Lowry ſc.

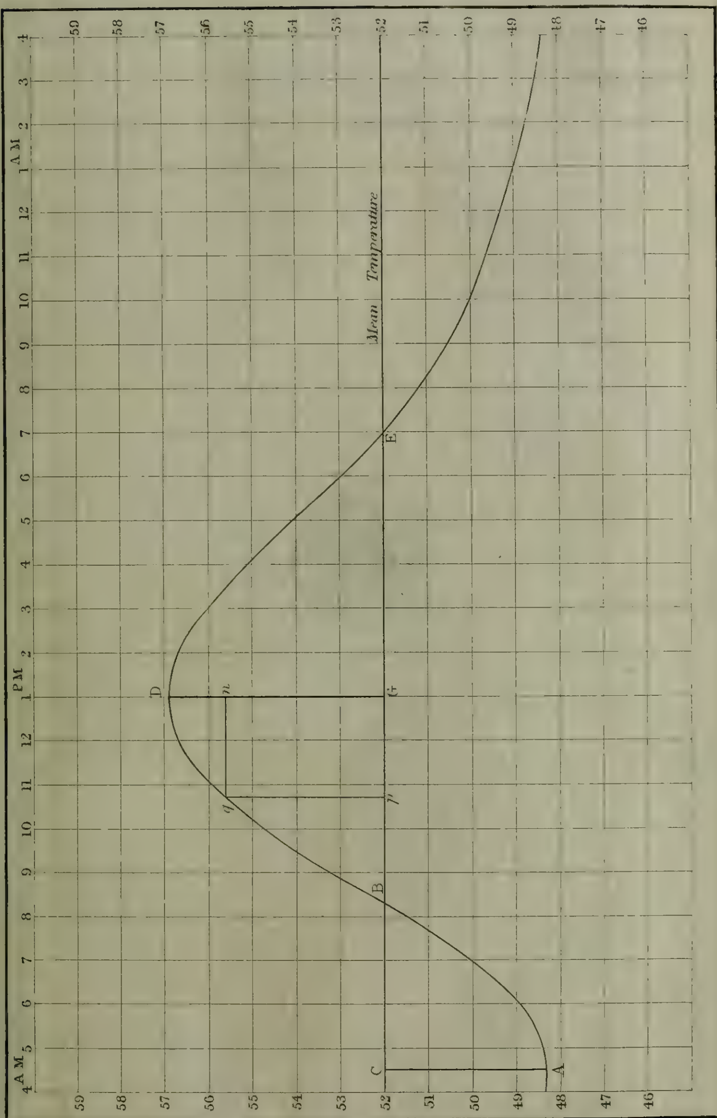
Plate 10.


57.306
Line of Mean Summer Temperature
Summer
52.073
Line of Mean Temperature
Mean
46.85
Line of Mean Winter Temp.
Winter



J. W. Lowry, Sculp.

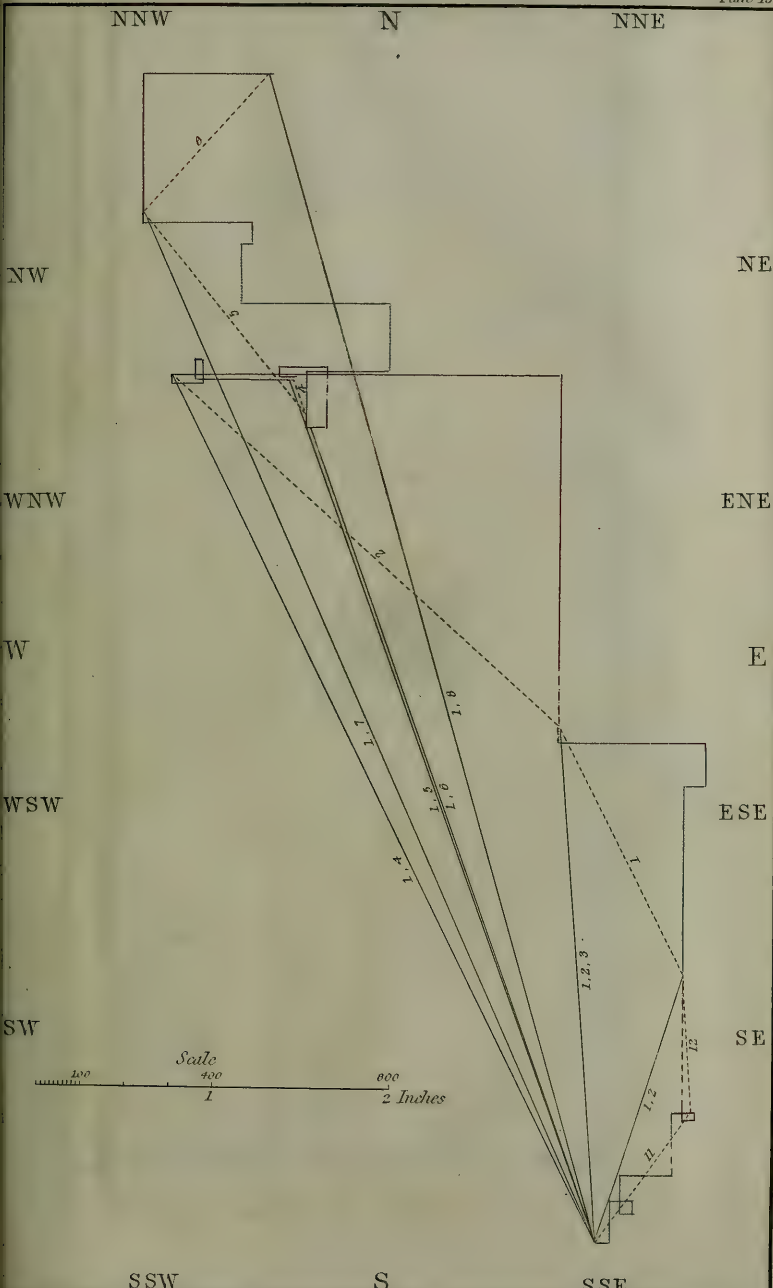
Plate 12.



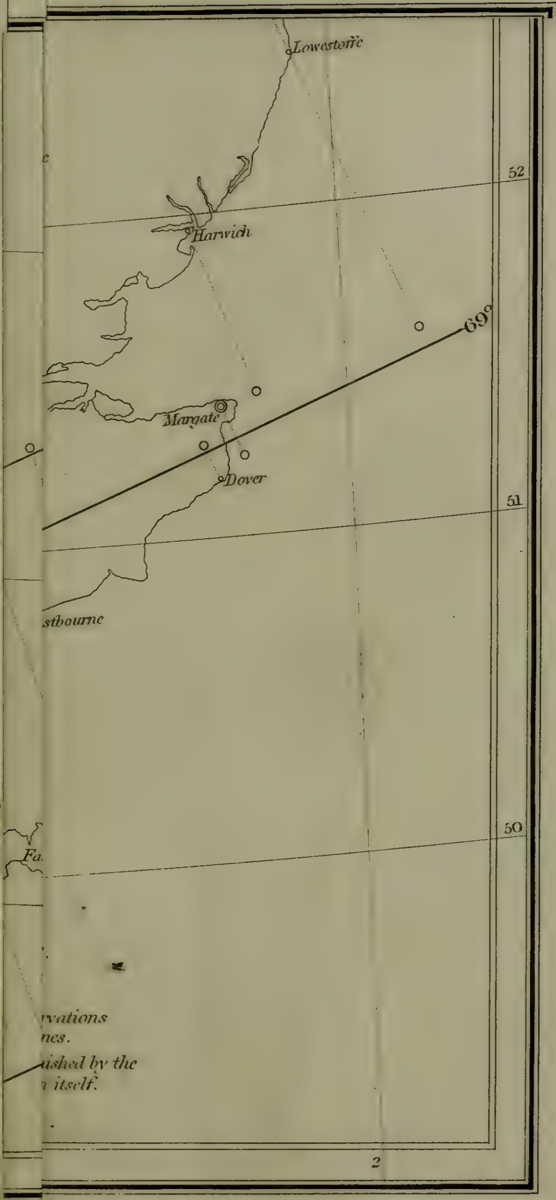
J. W. Lowry Sculp.

General Summary.

Plate 13.



J. W. Lowry fc.



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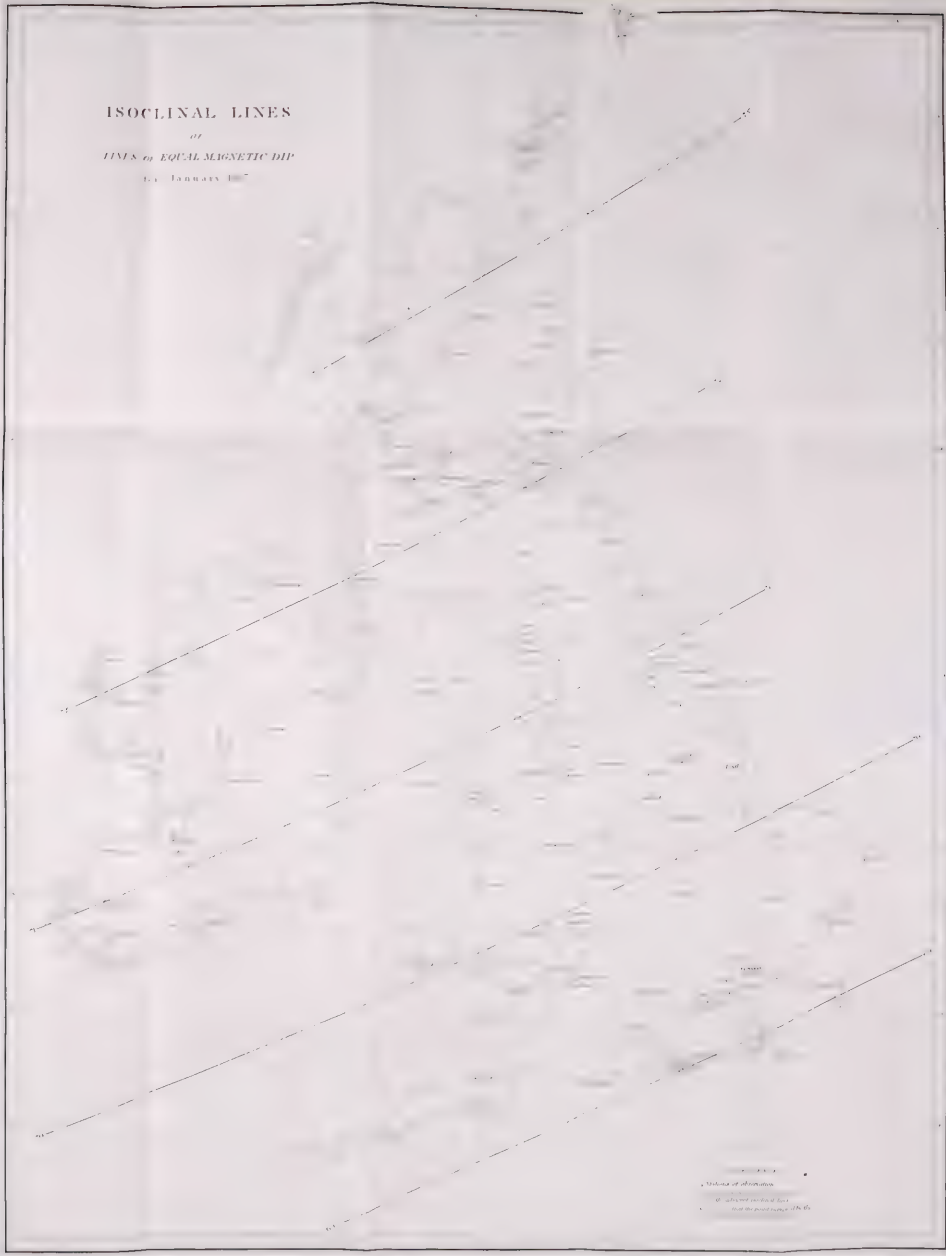
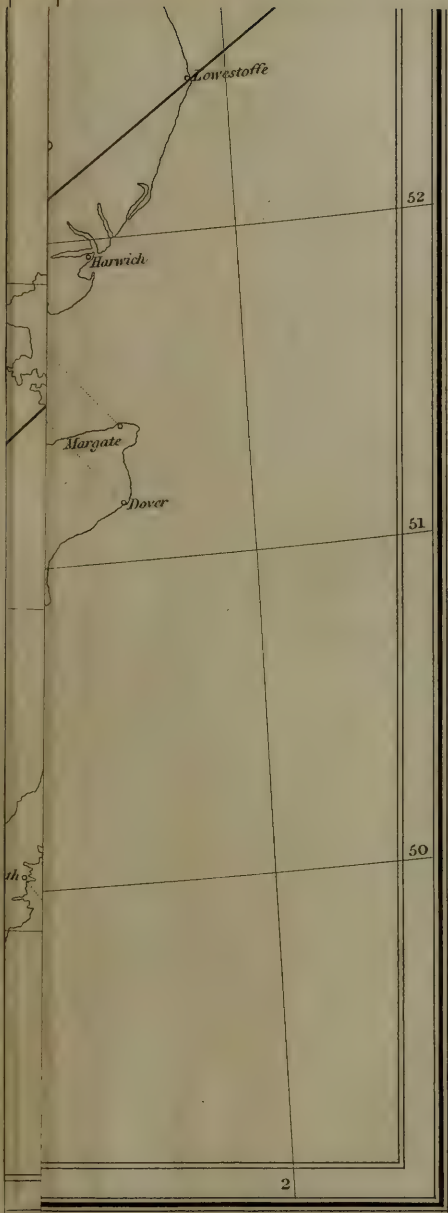

ISOCLINAL LINES
or
LINES of EQUAL MAGNETIC DIP
Stations of observation



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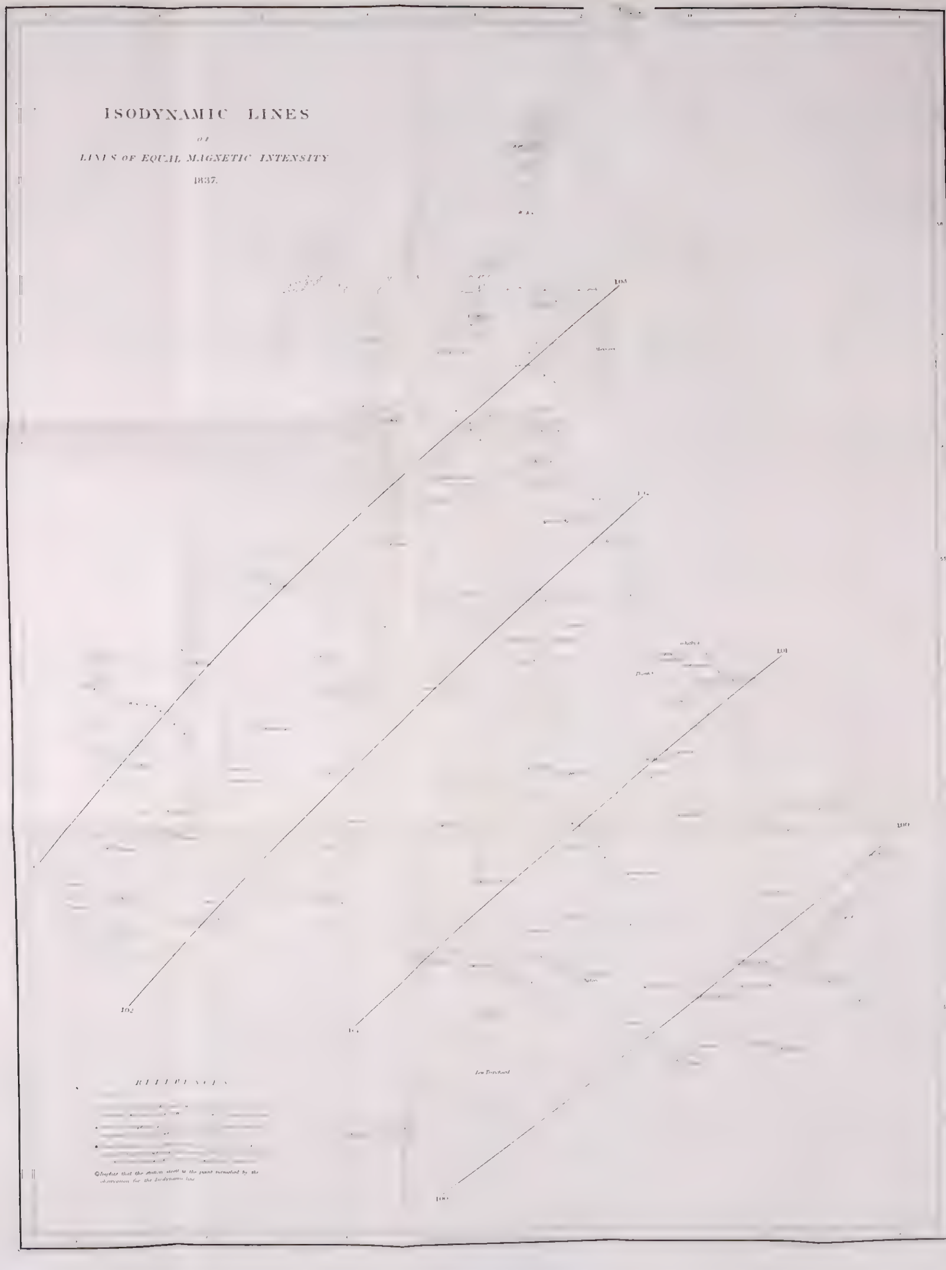
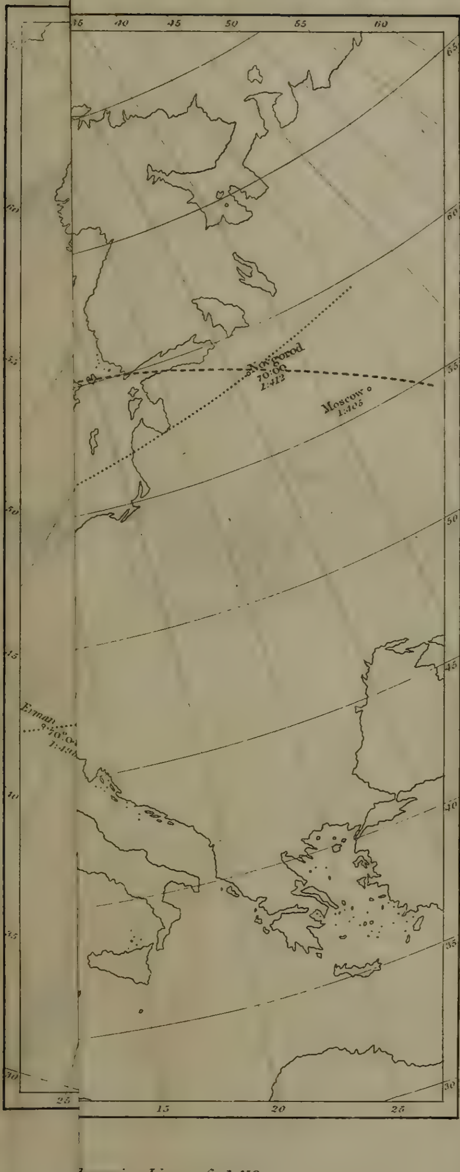
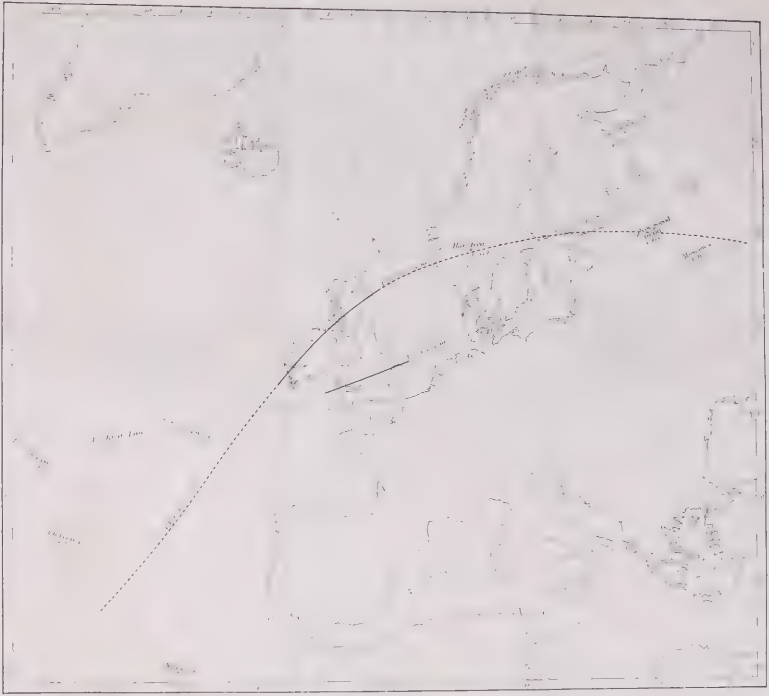

ISODYNAMIC LINES
OR
LINES OF EQUAL MAGNETIC INTENSITY
1837.
103
102
101
100
Implies that the station stood at the point intersected by the observation for the Isodynamic line



{ynamic Line of 1.413.
ꞃport.
iz. Lerwick; Valencia; & Bushey.



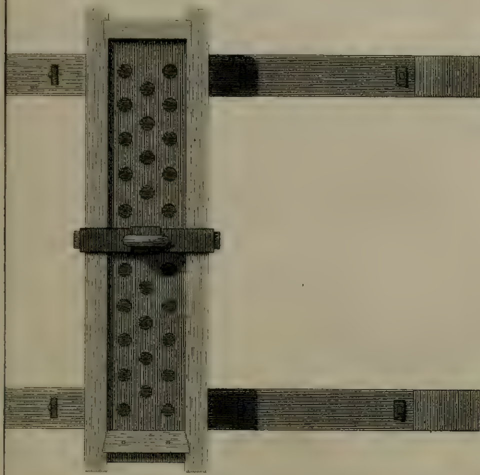
R E F E R E N C E S

Isoclinal Line of 70°.00 in 1837. ——— - - - - - - - - - - - - - - - - - - - Isodynamic Line of 1·413

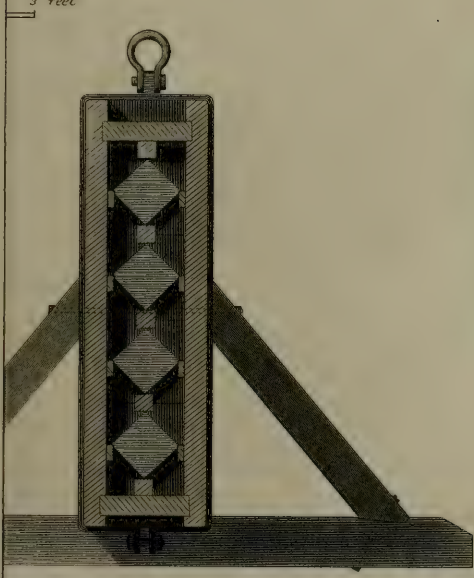
The unbroken portions of these lines are determined by the observations in this report.

The arrows represent the direction of the horizontal needle at three extreme points of the British Islands viz Lerwick, Valencia & Bushey

Eighth Report of the Brit. Assoc. for the advancement of Science, 1838.



Plan.



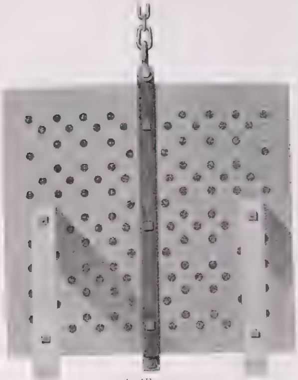
Transverse Section.

J.W. Lowry Sculp.

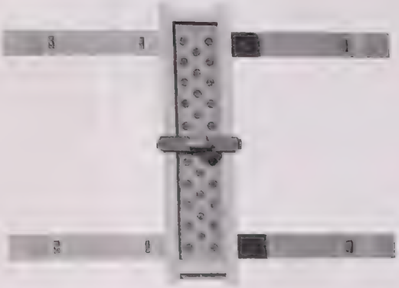
Boxes of specimens of Iron exposed to the action of Sea & River Water.

PLATE 17

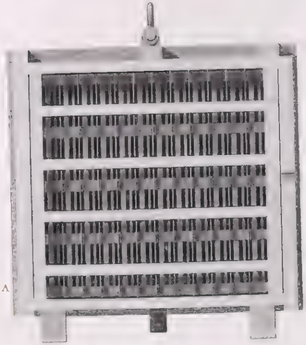
Eighth Report of the Brit. Assoc. for the advancement of Science 1838



Side Elevation

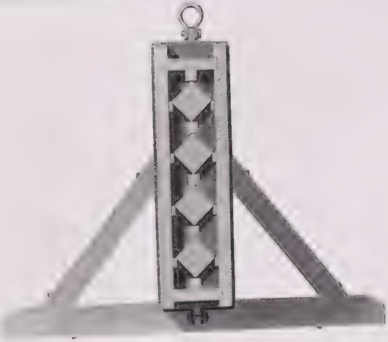


Plan



Longitudinal Section.

Series. commences at A



Transverse Section

CATALOGUE

OF THE

PHILOSOPHICAL INSTRUMENTS

MODELS OF INVENTIONS,

PRODUCTS OF NATIONAL INDUSTRY,

&c. &c.

CONTAINED IN

THE FIRST EXHIBITION

OF THE

British Association for the Advancement of Science.

NEWCASTLE-UPON-TYNE,

AUGUST, 1838.

NEWCASTLE-UPON-TYNE: PRINTED BY JOHN HERNAMAN, 19, GREY-STREET.

At a Meeting of the General Committee of the British Association, held at Liverpool, on the 16th of September, 1837.

THE EARL OF BURLINGTON IN THE CHAIR;

It was moved by Sir D. Brewster, seconded by R. I. Murchison, and carried unanimously,—"That a Committee be appointed to superintend the Exhibition of Mechanical Inventions at Newcastle, viz., Sir D. Brewster, Mr. Babbage, Professor Wheatstone, Professor Willis, Professor Powell,—Professor Johnston to be the Secretary, with power to add to their number."

In accordance with this resolution the Secretary, after consulting with Sir D. Brewster, Mr. Babbage, and Professor Wheatstone, drew up the following circular, and transmitted it to the various manufacturing districts in Great Britain and Ireland:

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

NEWCASTLE MEETING, 1838.

EXHIBITION OF MODELS, PHILOSOPHICAL INSTRUMENTS, AND PRODUCTS OF NATIONAL INDUSTRY.

It is a subject of regret to many that the facilities afforded by the Meetings of the British Association for the Exhibitions of Models, &c. have not hitherto been generally known or properly appreciated. The general Committee of the Association appointed for this purpose, in conjunction with the Local Committee in Newcastle, beg leave, therefore, to call your attention to the opportunity afforded by the meeting together of the most eminent men of science, from all parts of the kingdom, for making known the merits of inventions, the excellence of instruments, and the value of the products of industry in general, and to invite you to avail yourself of this opportunity.

Philosophical instruments—models of inventions, of improvements in machinery, of new applications of the mechanical powers, of workings in mines, &c.—products of national industry, new, rare, remarkable in any respect, or exhibiting the progress of any department of the arts, or of the application of scientific principles to their improvement—illustrations of the rise of new arts from new discoveries in science—remarkable natural and artificial productions, especially such as are likely to be of use in the arts—interesting geological sections, &c., are among the objects it is desirable to exhibit. An accurate description, pointing out especially what is considered new or remarkable in it, should accompany each specimen. Convenient rooms are provided for their reception, and, if the owners are not themselves present at the meeting, the articles will be returned as may be directed.

Packages must be addressed (carriage paid) to the local Secretaries, or to the assistant general Secretary of the British Association, at the Rooms of the Literary and Philosophical Society, Newcastle, and should arrive on or before Friday the 10th August, of the present year.

JAMES F. W. JOHNSTON, Secretary.

Newcastle, July 1, 1838.

PLAN OF THE EXHIBITION.

THE EXHIBITION WILL CONSIST OF TWO PARTS.

I.—*Specimens connected with the Arts and the Developement of National Industry.*

A. LOCAL.—Articles manufactured in the district, showing the nature of the products of local industry—the present state of the manufactures—Specimens illustrating the improvement or progress of the several branches.

B. GENERAL.—Products of industry from all parts of the kingdom—Specimens illustrating the different steps from the raw material to the finished article.

Raw materials of a less common kind, which are or may be applied to useful purposes in the arts—which are used or abundantly produced, or may be so, either at home or abroad, and are susceptible of beneficial application to industrial purposes.

II.—*Mechanical and Philosophical.*

A.—Models of Machines, or parts of Machines—old, new, or improved ; or illustrating the gradual progress of invention.

Models of workings in mines.

B.—Philosophical instruments—new, nicely adjusted, or for the purpose of comparison.

C.—Remarkable Minerals—interesting Geological Sections—Fossils—rare or curious Specimens in any of the branches of Natural History.

N.B. The Exhibition will be open to all Members of the Association and their families, and to *all other persons* who may have transmitted any article considered worthy of a place in the collection.

Mr. Grainger, to whose liberality the Association is in other respects so much indebted, having placed an elegant and spacious room at the disposal of the local Committee for the purposes of the Exhibition; the objects of interest contained in the following catalogue were arranged in it, and access given to the members of the Association on Tuesday the 21st of August.

In future, it would be very desirable to insist on the models, &c. being sent in at least one week before the day of meeting of the Association, that the Exhibition may be opened before the arrival of the strangers. The aid of wood cuts or other illustrations should also be employed in order that the list of articles contained in the Exhibition may be made as much of a *catalogue raisonnée* as possible. The delays and inconveniences attending the reception and arrangement of the specimens in the present Exhibition have prevented the catalogue from appearing so soon, or in so complete a form, as on future occasions may reasonably be expected.

JAMES F. W. JOHNSTON, Secretary.

Newcastle, 24th August, 1838.

CATALOGUE

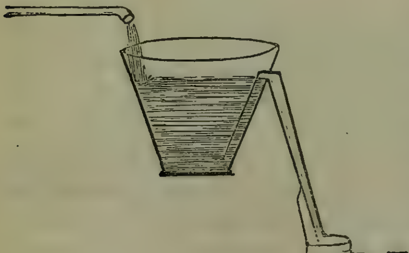
OF

PHILOSOPHICAL INSTRUMENTS, &c.

MECHANICAL INVENTIONS.

1. *The Pendulous Printing Press.*—By Thomas Edmonson, Milton Station.—This instrument has been invented for the purpose of dating the tickets given to passengers on the Newcastle and Carlisle Railway with facility and despatch. Upwards of ten thousand tickets can be printed by it with one supply of ink. This is accomplished by means of a ribbon saturated with a peculiar inking composition, attached to two small rollers, and shifted by the pressure of the finger against the instrument. The impression, which is dry and permanent, is obtained by simply putting the ticket into a space left for it in the centre of the press.

2. *Model of a Chair and Rail, with Guard,* to prevent Carriages running off Railways, with drawings of a steam boiler, &c. By Ralph Rewcastle.



3. *Intermitting Spring.*—By W. L. Wharton, Esq., of Dryburn.—A model to prove that the flowing water, *in all intermitting springs,* first forms *a valve* for the exclusion of the atmosphere, and then *a pump* for forcing out the air from the syphon. An intermitting fountain is formed by closing the tube T of the cup, (which when filled with water forms the valve at the foot of the syphon.) Upon opening T, the water flows without intermission.

4. *Stand and Staff for a Level.*—By W. L. Wharton, Esq., Dryburn. A stand and staff for a level, intended to expedite the common operations of levelling. The axis of the telescope, and the Zero of an ascending and also a descending scale upon the staff, are fixed *at one height* (5 feet) above the ground, and the difference of level of the respective sites of the staff and stand is at once ascertained from one or the other scale, upon directing the telescope (when adjusted) to the staff.

5. *Safety Coal Gin.*—J. G. Wright, Wakefield.

6. *Specimens illustrating the Process of manufacturing Needles by Patent Machinery.*—Invented by Mr. Samuel Cocker, Porter Works, Sheffield.

No. 1.—Soft steel wire in lengths for 2 needles.
2.—Do. pointed on conical files, making 10,000 revolutions per minute.
3.—Do. grooved with an indentation for the eye.
4.—Do. do. and eyed.
5.—Do. filed, headed, and eyed, by punching.
6.—Do. filed, headed, drilled, and countersunk.
7.—Needles made according to the old mode.
8.—Finished needles (sharps) made by patent machinery.
9.—Do. (betweens) do.

The value of labour from the wire No. 1 to No. 7 (inclusive) would be 1s. 1d. per thousand. The expence by patent machinery from No. 1 to No. 5 or 6, inclusive, 1d. per thousand.

100 patent machines, which would occupy four rooms, each about 25 yards by 10, will, by the power of a six horse steam engine, be sufficient to produce 14,000,000 needles per week.

The fash, made in grooving, is filed off by circular cutters, in the last operation of the machine, leaving the needle in the state of No. 5 or 6.

7. *A Specimen of Flooring Deals with Hoop Iron Sliffers.*—Wm. Holmes, Newcastle.—The advantages of which are—firm-

ness in the floor, more security against depredation, the iron presenting an additional obstruction to the floor being sawn through, and the preservation of the strength of the deals in a greater degree than by the ordinary wood sliffers.

8. *Sling Fracture Bed.*—By T. M. Greenhow, Esq. Newcastle. The sling fracture bed consists of two parts. 1. The first part is of the nature of a splint to which the injured limb is fixed; and the several portions of which admit of extension by means of screws to accommodate them to the case under treatment. 2. The second part is a support by means of which the former part of the instrument is slung in an easy position. The mode of application will be easily understood on inspection of the instrument. The advantages of this plan of treatment are—ease and security to the patient, the power of moving to a certain degree without hazard of displacement, and facility of dressing wounds in cases of compound fractures.

9. *Splint for Fractures of the Lower Extremities.*—By J. Baird, Newcastle.

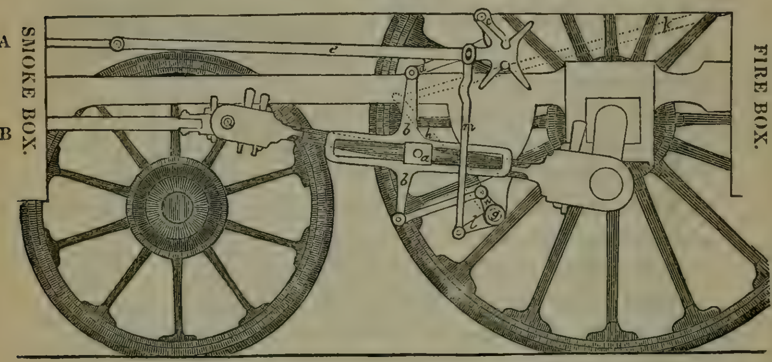
10. *A Cultivator.*—By Anthony Hall, Prudhoe.—Model one-fourth the working size. With this instrument a man can dig one-fifth of an acre per day.

11. *Improved Colliery Bow and Hook*, for the prevention of accidents, in coal mines, occasioned by corves (in which the coals are brought up from the pit) falling from the hooks affixed at the end of the rope or chain. The improvement of the Bow consists in the under surface of it being lozenge shaped, so that not having a flat surface, as is usually the case, the bow cannot rest on the point of the hook, and must either slip into the bend of the hook at once, or fall off altogether, in either of which cases no accident can happen.

12. *Union Joint for Fire Engine Pipes.*—By John Gardner, Gun-maker, Newcastle-upon-Tyne.—When the joint is put together the bolt is pressed with the thumb, and the self-acting spring prevents the bolt from shifting by any action whatever, and when the joint is to be taken asunder the bolt head is pressed with the finger and thumb and drawn at the same time, and then

giving the joint a quarter turn it will come asunder. The putting together and taking asunder will not take more than a very few seconds. It is perfectly tight to air and steam.

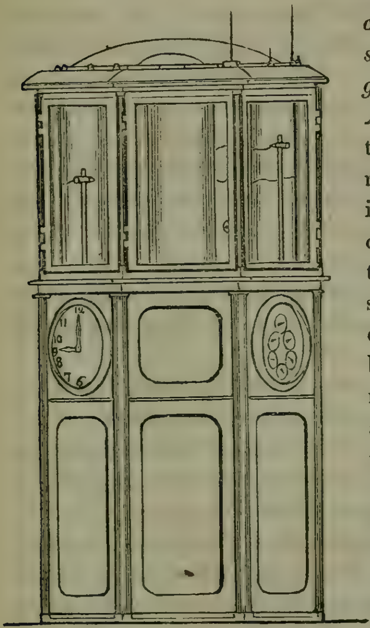
13. *Model of a new method of Working the Valves of a Locomotive Engine, by* R. and W. Hawthorn, Newcastle-upon-Tyne.



A.—Centre of Slide Rod. B.—Centre of Cylinder.

The slider *a* is attached by a pin to the centre of the connecting rod, working in the frame *b b*, which gives to the frame a reciprocating motion vertically from the connecting rod at every revolution of the cranked axle. The upper arm of the frame *b b* is conected with the arm *d*, fixed on the same weighbar as the double arm *c*, by which the motion is communicated to the slide valve by the change rod *e*. *g* is the reversing weighbar, having three arms, *h l n* fixed upon it. The arm *h* is moved by the rod *k*, communicating with the reversing handle or lever worked by the engineman. *l* is connected to the change rod *e*, by the bar *m*, which by moving the reversing handle or lever can be changed from one of those pins to the other at the ends of the double arm *c*, and the engine made to go in either direction required.

The arm *n* has a box with a set screw for adjusting its length, by which any angle in a few minutes can be given to the frame *b b*, which is a very important part in this arrangement; as the readiness and simplicity with which the lead can be given to the slide valves, to correspond with the various speeds and loads the engine may be put to, is a desideratum which could not be obtained by eccentrics, without much labour and time in loosening and resetting the eccentric wheels.



14. *Dr. Lardner's Self-Recording Steam Journal, constructed for the Steam Navigation Committee of the British Association.*—The purpose of this apparatus is to register by mechanism those several varying effects connected with the operation of the engines and the performance of steam vessels, on which their efficiency depends. It will be attempted by means of the present mechanism to make and preserve a constant record from five minutes to five minutes, of

1.—The pressure of steam between the slides and the steam valve.

2.—The pressure of steam in the boiler.

3.—The state of the vacuum in the condensor.

4.—The part of the stroke at which the steam is cut off when it works expansively.

5.—The quantity of water in the boilers.

6.—The saltness of the water in the boilers.

7.—The velocity of the paddle wheels.

8.—The draft of the vessel.

9.—The trim of the vessel.

10.—The rate of the vessel.

11.—The course of the vessel.

12.—The apparent force of the wind.
13.—The apparent direction of the wind.

The above will be registered by self-acting mechanism, except the eleventh, (the course of the vessel,) the indicator of which will require occasional manipulation.

15. *Dr. Lardner's Self-Registering Steam Guage, or Experiments with Locomotive Engines, constructed for the Committee of the British Association on Railway Constants.*—The purpose of this apparatus is to register by self-acting mechanism the pressure of steam in the boiler at each point of the road over which the engine passes.

16. *Improved Writing Table*, by T. Sopwith, F.G.S.—This table is so contrived that the whole of the drawers, closets, and partitions are fastened by means of a single lock, placed on the flap which forms the writing desk.

17. *A beautiful Miniature Working Model of a High Pressure Steam Engine and Planeing Machine.*—By James Frazer, Brass-finisher, Newcastle.

18. *Working Model of a Steam Engine with vibrating Pillar*, made by John Brown and Son, Grey-street, Newcastle.

19. *Model of a Patent Air Engine.*—By Crossley & Parkinson.

20. *Model of Hay and Corn Rake*—Hutchinson and Swales, Agricultural Instrument Makers, Newcastle.

21. *Model of a new Telegraph.*—By Dr. Clanny, Sunderland. By this Telagraph, either spelling as in short hand or a vocabulary as in the common Telegraph may be employed; and by attaching to it a powerful lamp, it may be rendered as serviceable by night as by day. The introduction of spelling will permit communications to be interchanged in regard to *all* subjects, which in respect to meteorology and other scientific subjects appears very important.

22. *Improved Two-armed Telegraph.*—By Joseph Garnett, Newcastle.—This model exhibits the construction of the telegraph, and the mode of working the arms by a more easy and rapid method than the telegraph in common use.

23. *Drawings of the Signals made by means of J. Garnett's*

Telegraph.—No. 1, shows the position of the two arms for day signals. No. 2, shows the position of the lamps for night signals. No. 3, shows the arrangement for effecting the night signals.

24. *Model of a Self Acting Fire Alarm.*—Constructed on the principle of the expansion of metals by heat, acting on compound levers and disengaging a spring, which sets off a bell.

25. *Model of a Self-acting Ventilator.*—Founded on the same principles as the above Fire Alarm, but in this a door or aperture is opened or shut by the application of heat.

26. *Model of an Improved Mode of Ship-building, and of a new Safety Keel.*—By Oliver Laing, Esq. Woolwich.

27. *Model of a Machine for pumping Vessels and extinguishing Fires on Shipboard.*—By J. Dalziel, M.D.—The moving power of this machine is derived from the action of the water on paddle wheels, when the vessel is under way.

28. *Model of Railway formed on continuous Blocks of Stone.*

29. *Model of Steam Boiler Copper.*

30. *Ditto,* *Ditto.*

31. *Ditto,* *Ditto.* *Wood.*

By Joseph Price, Esq. Gateshead.

32. *Model of a Patent Windlass.*—By George Straker.

33. *Model of a Force Pump.*—By John Lightfoot.

34. *Improved Articulated Stethescope.*—By Dr. Granville.

35. *Model of an Apparatus for promoting Artificial Respiration.*—By J. Dalziel, M.D.

36. *Gas Meter.*—By Crossley, with transparent cut back and front, shewing its action.

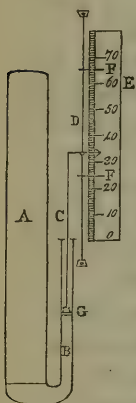
37. *Working Model of another Gas Meter.*—By Crossley, with three transparent chambers attached to a gas holder.

38. *Telescope exhibiting a new method of Mounting.*—By C. Garbutt, Gateshead.

39. *Mode of applying the Common Level to the purposes of the Transit Instrument.*—By the Rev. N. S. Heineken, Sidmouth. The object of this model is to show the application of the common Y level, slightly altered, to the purposes of the transit instrument, so that any clockmaker may construct for himself an

instrument by which he may ascertain the time for rating his clocks with far greater accuracy than by the ordinary means of the sun-dial and meridian line—the instrument may be constructed at a very trifling cost and made to serve the purposes of a level also.

40. *Eye Piece with Cavallo's Pearl Micrometer.*—By the Rev. N. S. Heineken. This is intended to obviate the objections to the application of this simple micrometer to the reflecting telescope.



41. *New Self-registering Thermometer.*—By John Brown and Son, Opticians, &c. Grey-street, Newcastle.—The construction of this instrument will be understood by the annexed diagram; A is a glass tube filled with pure spirit of wine; B is a continuation of the same but much smaller, which is to be about half full of quicksilver to support the spirit in the long tube; upon the quicksilver at G is a float supporting the wire C, which wire has a knee or bend in it with a small eye, which runs upon the fixed wire D, carrying an index or pointer; E is the scale which must be made experimentally. The action of the instrument is obvious. If any change takes place in the bulk of the spirit the quicksilver is also affected, and with the silver the ivory float G carrying the index or pointer, which shews at once the degree of temperature upon the scale; this is the simple action of the thermometer. To make it register, the two light indexes or pointers F move upon the wire D, their own friction keeping them where-ever they are placed. To set it the pointer F below the thermometer's index must be pushed close up to it, and the pointer F above, pushed down it; and it is evident that if any change of temperature takes place the thermometer's index will move the registering index either above or below, and leave it there, thereby shewing the extreme rise and fall of the thermometer in any given time. The action of the air upon the quicksilver is also provided against.

42. *An improved Marine and Mountain Barometer.*—By Sir David Milne.

The construction of this instrument allows it to be used as a Marine Barometer. In the middle of the tube, or half way down, the bore is reduced to the size of an ordinary thermometer bore, in consequence of which the mercury is prevented from flowing rapidly and violently into the upper part of the tube, when the instrument swings about. The open end of the tube through which the atmosphere acts on the mercury, is so constructed, that the mercury does not run out or escape when the instrument is upset.

43. *Patent Safety Spring for Carriages.*—By Barton.—This spring unites greater strength and safety with a much less weight of material than is contained in the ordinary carriage spring.

44. *Portable Mercurial Pendulum.*—By E. J. Dent, London.

45. *Pneumatic Apparatus.*—By Dr. Clanny.—For extracting and analysing the air contained in blood, and other fluids, at low temperatures by which the possibility of chemical changes by the action of heat is prevented.—*See Lancet, 23rd Aug.* 1834.

46. *Apparatus for receiving blood from the hand in tepid water.*—By Dr. Clanny.

47. *Apparatus for receiving blood in vacuo.*—By Dr. Clanny.

48. *Instruments for crushing Stone in the Bladder.*—John Brown and Son, Grey-street, Newcastle.

49. *A new Chuck for turning Wire into Screws, &c. or for holding Drills.*—By Charles T. Couthope, Bristol.

50. *Hall's Patent Hydraulic Belt for raising Water.*

A woollen belt is passed over a roller at the top of the shaft and under one at the bottom, and by giving the belt a velocity of about 1000 feet per minute, the water adheres to the belt, and is brought up and discharged into a trough by the centrifugal force in passing over the top roller. It possesses most important advantages over the pump, and is particularly applicable to coal pits, mines, &c. &c.

1st.—Producing more water with the same power.

2d.—The economy in cost and repairs.

3d.—The saving of time during the repairs, &c.

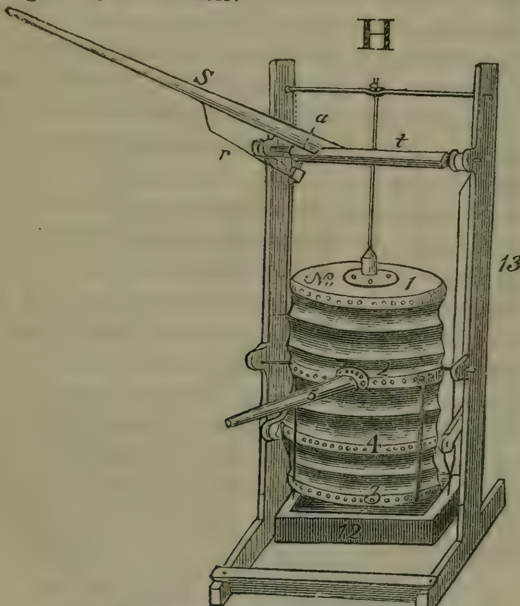
51. *Model of the present House of Commons*, illustrating the ventilating arrangements introduced by Dr. D. B. Reid.

The air that supplies the House being properly prepared and purified enters either by the ceiling or by the floor, according to the arrangement of the valves. The sketch No. 1, illustrates the progress of the air from the ceiling to the floor, and No. 2, shows the progress of the air when the current is reversed, proceeding from the floor to the ceiling.

52. *Else's Patent Cylinder for Malt Drying, and Portable Kiln for Drying Corn and Seed*, by which machine uniformity of drying, and the production of any shade of colour in the malt can with certainty be effected; and manual labour on the kiln is wholly dispensed with. The malt can also be screened while drying, and the whole of the malt-dust preserved uninjured.

53. *Drawings of a Planeing Machine—a Self-acting Drilling Machine—and a Slide Lathe.*—By Joseph Whitworth and Co. Engineers, Manchester.

H



54. *Model of Lindley's Patent Bellows, working in an upright frame, by means of a rack staff, axle, rod, &c.*

It consists of four boards, the second and fourth are fixed and kept a certain distance from each other (according to the length of the stroke required) by means of pieces of iron, the upper board rises and falls, the third plays between the second and fourth (or bottom board). Between the boards are frames of wood to support the leather. Three of the boards are so furnished with valves and other apparatus that the machine, when covered with leather and fitted up, works like a double acting steam cylinder, the space between the two upper boards forming the common chamber, whence the air is conveyed through the pipe to the fire. Upon the third board *going down* the air will rush in and fill the space between the second and third boards; upon the same board [illegible] *up* the air will rush in and fill the space between the third and fourth boards; when the rack staff is *pulled down* the air is forced from between the second and third boards into the chamber between the first and second boards; when the *staff returns* the air is driven from between the third and fourth boards into the *same* chamber; thus, when the staff rises or falls the air is forced into the common chamber, by which means a middle pipe is supported, and a constant and steady, as well as powerful blast is obtained, capable of fusing a bar of iron when one end of it is made red hot.

55. *An Odontograph.*—By Professor Willis, of Cambridge. This instrument is intended for setting out the forms of teeth, so that any two wheels of a set may work truly together.—*Vide Trans. of Civil Engineers. Vol. II. p. 2.*

56. *Specimens of Scales of equal parts.*—By Charles H. Holzapffel.—These scales are applicable to various purposes of engineering, architecture, and general science.

57. *Bate's Anaglyptograph for representing relieved surfaces.*—By the use of this machine lights and shadows are truly represented.

58. *Model of a Life Preserver.*—By J. Clark, of York.—To be put on as a short jacket, buttoned and buckled in front; made of jean prepared with caoutchouc and perfectly air tight; on this is another covering of jean, and the whole is inclosed in caoutchouc.

PRODUCTS OF INDUSTRY, ARTS, AND MANUFACTURES.

59. *Specimens of Printing in Oil Colours.*—By G. Baxter, London.

60. *Six Inch Speculum for Reflecting Telescope.*—Cut and ground by Joseph Redshaw, tailor, Gateshead.

61. *A Plate of Fine Silver.*—Benjamin Johnson, Esq.

62. *Specimens of Colours produced by thin films of Oxide on the surface of Metallic Lead.*—Benjamin Johnson, Esq.

63. *Plate of Fine Silver.*—Locke, Blackett and Co.

64. *Fire Bricks.*—By T. Carr, Scotswood.—Showing the present state of the manufacture. Also a specimen of raw clay as brought out of the mine, same as the bricks are manufactured from.

65. *Printed and Dyed Leather.*—From Jon. Priestman, Low Friar-street, Newcastle.

66. *Specimens of Paper Staining.*—From Daniels and Co., Newgate-street, Newcastle-upon-Tyne.

67. *Model of a Coal Waggon in Silver.*—Messrs. Reid & Son.

68. *Specimen of Silk Waste (Strusa.)*—James Holdforth, Esq., Leeds.

About thirty years ago this article was considered of no value except for tillage for which it was used by the Italians. Various experiments and attempts were at intervals made to apply machinery to its manufacture, which of late have been so far perfected that it can now be wrought into a beautifully fine thread as the accompanying sample will shew, and has become an article of considerable importance and value.

69. *Specimen of Silk Yarn.*—James Holdforth, Esq., Leeds.

In one pound weight of this there are 150,000 yards, which is equal to 85¼ miles in length.

70. *Specimens of Flaxes and Yarns*, from Messrs. Hives and Atkinson, Leeds.

A. the coarsest line yarns we produce. B. a medium quality and size. C. The finest ever produced, and we believe to be finer than has been ever produced elsewhere, *from flax.* The three sorts of flaxes are what we are generally using, worth from A. £40 per ton, B. about £60, and C. £60 to £70. Our finest flaxes similar to those the fine hank is produced from, are worth £200 per ton.

71. *Specimens of Poplins or Tabinets figured and brocaded.* By Messrs. Atkinson & Co., Dublin.

1st. A splendid Pink and Silver Tissue Poplin same as worn by Majesty at her first Drawing-Room.

2nd. White, Gold, and Green, worked on an improved principle, which obtained the highest class premium from the Royal Dublin Society, in June, 1838.

3rd. Several patterns of Gold and Silver Tissues for Gentlemen's waistcoats.

4th. Furniture Tabouret, which obtained a silver medal from the Royal Dublin Society.

72. *Map of the World,* by James Wild.—This map is designed to show the languages and dialects into which the British and Foreign Bible Society have translated the Scriptures.

73. *Geographical Clock, exhibiting Diurnal motion of the Earth, &c.*—By Geo. R. Taylor, Sunderland.

74. *A Painting on Glass.*—By Joseph Price, Esq. Gateshead.

75. *Specimens of Engraving on Glass.*—By Joseph Price, Esq. Gateshead.

76. *Models of a Rifle Bullet for Small and Great Guns.*—By Oliver Byrne.—With casts of the barrels.

77. *Prints and Metal Plates, illustrating Woone's Patent Metallic Relief Engravings.*—This invention affords an easy and expeditious method of obtaining Engravings or Etchings in Relief, capable of being printed at the type press in the manner of wood engravings. The process employed for this purpose is to form a mould from which casts can be taken in metal by drawing with a steel point or etcher through a thin composition of Plaster of Paris and White Lead, laid on an even plate of metal.

78. *Specimens of Patent Encaustic Tiles.*—By Davis and Co. Blackett-street, Newcastle.—The improvement exhibited by these beautiful tiles consists in their being ground perfectly flat before the dssign and glaze are applied. They can be made of any size, and are applicable to a great variety of useful purposes.

79. *Model of a Railroad for facilitating the draught of heavy weights up inclined planes in common carts or waggons, and drawn by one horse.*—By Sir Charles Monteith, Bart.

80. *The Coronation Medal.*—By Pistrucci.

81. *Medal to commemorate her Majesty's visit to the City of London.*—By W. Wyon.

82. *Specimen of the New Coinage.*—By W. Wyon.

83. *Stockings knit by Patent Machinery.*—By Whitworth and Co., Manchester.

84. *Plate Glass Prism and Polygon, richly cut.*—By Robert Walter Swinburn, Esq.

85. *Specimens of Raper's Patent Waterproof Cloth.*—Deposited by Mr. William Maugham.

86. *Patent Wire Rope for Standing Rigging.*—By Mr. Andrew Smith, London.

87. *Specimens of a Web of Copper Wire.*—That numbered 90, contains 8,100 appertures to the square inch ; and number 100, 10,000 ditto, ditto.—From Wm. Mountain and Sons.

88. *The Brandling Knife.*—By John Brown and Son, Grey-street, Newcastle.

89. *Flax, Line, and Tow.*—Marshall and Co. Leeds.—August 8, 1838.

Price at Leeds per Ton.

Common Flemish Flax.	Antwerp $\frac{2}{\text{IV}}$	...£63	Producing Line for 20 Leas and 5lbs. Yarn—Tow for 18 Leas and 30 Yarn.
	Courtrai $\frac{2}{\text{IV}}$	...£67	
Fine Flemish Flax.	Antwerp $\frac{1}{\text{II}}$	...£140	Producing Line for 120 Leas and 240 Yarn—Tow for 60 Leas and 140 Yarn.
	Courtrai $\frac{1}{\text{II}}$	...£160	

ARCHITECTURE, BRIDGES, &c.

90. *Timber Bridge*—200 feet span, which is the largest span in timber that has been attempted in this country.—Each curved under bearer forms an abutment for each pair of struts which support the upper bearers, both the upper and under bearers are so connected together by the struts and suspending pieces, that the framing forms a solid rib, upon which the joists for supporting the roadway are sustained, and the whole weight of the bridge itself and the weights passing along it, ultimately rest upon the abutments. This bridge is intended for turnpike-road traffic, but the principle may be applied to any other kind of traffic, such as railway traffic, with heavy locomotive engines passing at rapid rates. A bridge of two arches, each similar to the above, is now about to be erected over the Tweed at Norham.—J. Blackmore, Esq. engineer, Newcastle.

91. *Model of Piers for a Suspension Bridge.* By J. Green, Architect and Engineer.—Proposed to be erected across the mouth of the Tyne, in 1827, at the height of 110 feet above high water level, and 1,000 feet span.

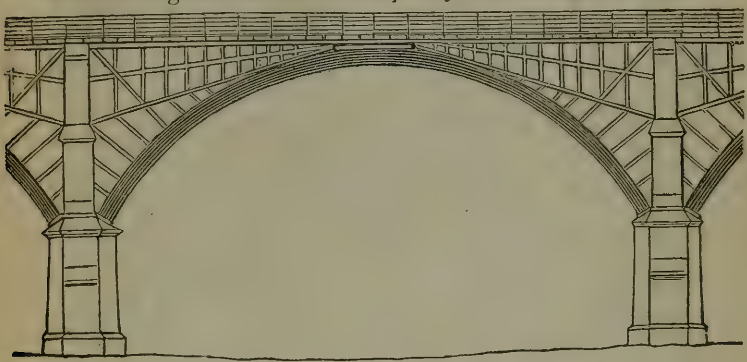
92. *Model of a Suspension Bridge,* in which the suspending rods are inclined instead of being vertical as in the usual construction.—By James Dredge, Esq. Bath.

93. *Model of a Monument*—erected in the Westgate Cemetry, Newcastle, by Benjamin Green, Architect.

94. *Model of an Arch*—120 feet span, by John and Benjamin Green, architects, being part of a bridge of five arches, designed in 1834, which obtained the premium offered by the Newcastle and Carlisle Railway Company at that time, for crossing the River Tyne above Scotswood. This arch is on the same principle as the immense viaducts now in progress on the Newcastle and North Shields Railway.

95. *Model of a Timber Arch, on a new construction.* By Messrs. Green, Architects.—In this arch the ribs are put together with Dantzic deals, in lengths of from 21 feet to 45 feet, 11 inches broad, and $3\frac{1}{2}$ inches thick, fixed together with oak tree-nails. The experiments tried have sufficiently proved the strength

of such arches, and the durability they possess over other wooden bridges arises from the simplicity of their construction.



The model is on a scale of half an inch to the foot.

A viaduct of 5 arches on this principle is in progress of construction over Ouseburn, and another at Willington Dean, of 7 arches, the span of these arches being from 116 to 118 feet, and the height of the bridges from 82 to 108 feet. The plans and sections are to be seen in the model room.

96. *Model of the Grey Column.*—By John & Benjamin Green, architects, now erecting at the north end of Grey-street, Newcastle.—The total height to the top of the figure will be 133 feet, the diameter of the shaft is 9 feet 11 inches. The order is of the Roman Doric, and there is a staircase consisting of 164 steps to the top of the abacus of the capital, from which there is a fine panoramic view of the town and the surrounding country.

97. *Model of a Gothic Cross*—designed by Benjamin Green.

98. *Model of a Church*—designed by J. Green, Architect.

99. *Model of St. Nicholas' Steeple, in Newcastle.*—Made by Charles M'Kenzie, Brazier, Newcastle.—Deposited by T. Sopwith, Esq.

100. *Model of the New Corn Exchange and other Buildings attached thereto, now in progress opposite St. Nicholas' Church, Newcastle.*—John and Benjamin Green, architects.

101. *Isometrical Drawing of a proposed New Street in Newcastle.*—By T. Sopwith, F.G.S.

102. *Drawing of one of Mr. Owen's Communities for the Superior or Educated Classes.*—Deposited by Mr. Owen.

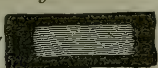
CHEMICAL PRODUCTS.

103. *Specimens of Prussian Blue and of large Crystals of Prussiate of Potash.*—Thomas Bramwell, Esq., Newcastle.—The quantity of these substances manufactured by the sole maker on the Tyne is about 110 tons a year. The average price is one shilling and a penny per pound—total value, £24,640. In 1828 the average price was 5s. per pound.

104. *Specimens illustrating the action of the preserving substance* employed in Kyan's patent process.—Deposited by Richard Fell.—These specimens of fir pit props and calico bags, containing pieces of No. 2 canvas, cordage and twine, prepared according to the patent process, together with unprepared articles of the same description, which had been respectively exposed for a lengthened period of time to the same destructive agents.

105. *Specimens of Leather, and Model of a Machine, illustrating a new mode of Tanning.*—By William Herapath, Esq. Bristol.—For an account of this important improvement in the art of tanning see the Mechanics' Magazine for April, 1828.

Action of Water on Herapath's Patent Leather.

No. 1  On thick sole after five hours.

2  Do......after nine hours.

3  On thin sole after nine hours.

106. *Large Crystals of Alum and Soda.*—From John Lee and Co.—The quantity of Soda annually manufactured on the Tyne amounts at present to about 20,000 tons of crystalized at £11, and 10,000 of dry at £15 per ton—total value £370,000. The price of the two varieties in 1828 was £20 and £25 respectively. Of Alum about 2,000 tons are manufactured by the sole direct action of Sulphuric Acid on clay. The present price is £10 per ton.

107. *Of Soda and Sulphate of Copper.*—By Messrs. Cookson.—About 150 tons of Sulphate of Copper are manufactured annually on the Tyne. The price is £35 per ton—total value £5,250.

108. *Specimens of Flint Glass Manufacture.*

109. *Ditto Imperial Sheet Glass (Flint.)*

110. *Ditto Plate Glass.*

111. *Ditto Crown Glass.*

112. *Ditto Double Crown.*—By Joseph Price, Esq. Gateshead.

113. *Crystals of Bicarbonate of Soda.*—Messrs. Cookson.

114. *Large groups of Crystals of Nitrate of Soda.*—Messrs. Cookson.

115. *Specimens of artificial Pyrites.*—George Lowe, Esq. London.

116. *Large group of Crystals of Sulphate of Iron.*—William Caley, Esq.—About 2,000 tons of this salt are annually manufactured on the Tyne.

117. *Eleven Specimens of Bohemian Glass.*—Deposited by the Rev. John Collinson, Gateshead.

118. *Specimens of Glass and Porcelain.*—Deposited by Joseph Townsend, Newcastle.

GEOLOGICAL, &c.

119. *Profile of the Coast and Longitudinal Section of the Coal Strata near Whitehaven.*—By Williamson Peile, Esq. F.G.S.—Exhibiting (on a natural scale) the succession of mountain limestone, coal strata, lower red sandstone, magnesian limestone, and new red sandstone rocks.

Transverse sections of Whitehaven Colliery on the small natural scale of four inches per mile, and—

Four sections of Whitehaven Colliery, exhibiting the numerous slip dykes, natural scale 50 yards per mile.—Williamson Peile, Esq., of Whitehaven.

120. *Model of the Town of Whitehaven, and the Coal Mines beneath.*—Deposited by W. Peile, Esq.

121. *Model of Dean Forest, in the county of Gloucester.*—Made for the Honourable Commissioners of Woods and Forests, by T. Sopwith, F.G.S. This model is constructed so as to show the relative elevation of the principal seams of coal.

122. *Sections of the Strata in Dean Forest.*—By T. Sopwith, Esq.

123. *Plans and Sections to illustrate the Strata and Mining operations in Alston Moor.*—By T. Sopwith, F.G.S.—The sections exhibit the succession of strata and the whole of the workings in the lead mines.

124. *Model, illustrating the method of ventilating Coal Mines.* By John Buddle, Esq.

125. *Miners' Air-measuring Machine.*—By Thomas Elliott, Pensher Colliery.—For the purpose of measuring the velocity of air in mines at any instant of time, and also for registering its mean velocity for any given time. No machine previously invented for measuring air in mines has been applied to the useful and important purpose of registering its mean rate. The coal trade meeting of this town presented the inventor with ten guineas, as a mark of their approbation of this invention.

126. *Safety Lamp, with Extinguisher.*—Thomas Bonner, Monkwearmouth.

127. *An Instrument for distinguishing Precious Stones and Minerals.*—By Sir David Brewster, K.H., F.R.S., &c. &c.

128. *Levelling Staves for taking Surface and Subterranean Levels.*—By Thomas Sopwith, F.G.S.—The arrangement of the springs in these staves admits of their being fixed at any required height, with great ease.

129. *Model of the Workings and mode of Ventilation in Monk-Wearmouth Colliery.*—By George Elliot, Viewer.—This model shews the coal in its true position with the faults which traverse it on a horizontal scale of 99 feet to an inch, and 12 feet vertical to an inch.

130. *Model of France*, in Papier Maché, presenting the inequalities of the mountains.—Deposited by T. Sopwith, Esq.

131. *Model of an Apparatus for Ventilating Coal Mines.*—By W. Fourness, Leeds.—This apparatus acts on the principle of exhaustion.

132. 1,600 *Specimens of Dried Plants.*—Mr. Wells, Durham.

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