NOTICES

OF THE

PROCEEDINGS

AT THE

MEETINGS OF THE MEMBERS

OF THE

Royal Institution of Great Britain,

WITH

ABSTRACTS OF THE DISCOURSES

DELIVERED AT

THE EVENING MEETINGS.

VOL. III.

1858—1862.

ROYAL · INSTITUTION · OF · GREAT · BRITAIN ·

ILLUSTRANS COMMODA VITAE

LONDON:
PRINTED BY WILLIAM CLOWES AND SONS,
14, CHARING CROSS.
1862.

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HER MOST GRACIOUS MAJESTY

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Assistant Secretary and Keeper of the Library—Mr. B. Vincent.
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Assistant in the Laboratory—Mr. Charles Anderson.

CONTENTS.

1858.

1859.

1860.

1861.

1862.

Royal Institution of Great Britain.

1858.

GENERAL MONTHLY MEETING,

Monday, November 1.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Cromwell Fleetwood Varley, Esq.

was duly *elected* a Member of the Royal Institution.

Mervyn Hamilton, Esq.

was *admitted* a Member of the Royal Institution.

The Special Thanks of the Members were returned to H. W. PICKERSGILL, Esq. R.A. for his Present of a Portrait (painted by himself) of the Rev. JOHN BARLOW, F.R.S. the Honorary Secretary of the Royal Institution.

The following PRESENTS were announced, and the thanks of the Members returned for the same:—

FROM

Board of Trade—Meteorological Papers, Nos. 2 and 3. 4to. 1858.
Commissioners in Lunacy—Twelfth Report. 8vo. 1858.
East India Company, the Hon.—Catalogue of Birds in the Museum. By T. Horsfield and F. Moore. Vol. 2. 8vo. 1856-8.

Actuaries, Institute of—Assurance Magazine, No. 32. 8vo. 1858.
Agricultural Society of England, the Royal—Journal, No. 41. 8vo. 1858.
Amsterdam, Koninklijke Akademie van Wetenschappen—Verhandelingen, Deel 4, 5, 6. 4to. 1857-8.
Jaarboek. 8vo. 1857-8.
Catalogus van de Boekerij. Ersten Deels Ersten Stuk. 8vo. 1857.
Verslagen, Natuurkunde, Deel 7, 8vo.
Letterkunde, Deel 3. 8vo. 1857-8.
Antiquaries, Society of—Archæologia, Vol. XXXVII. Part 2. 4to. 1858.
Proceedings, No. 47. 8vo. 1857.
Arts, Society of—Journal for July to Oct. 1858. 8vo.
Asiatic Society of Bengal—Journal, Nos. 264, 266, 267. 8vo. 1857.
Astronomical Society, Royal—Monthly Notices, 1858. 8vo.
Basel, Naturforschende Gesellschaft—Verhandlungen, Theil II. Heft 1. 8vo. 1858.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for July to Oct. 1858. 8vo.

Black, Judge, The Hon. (*Canada*)—Eight Maps of Canada. 4to. 1857.
Reports, Maps, and Estimates of the River St. Lawrence and Lake Champlain Canal. fol. 1856.
H. Y. Hind, Essay on the Insects and Diseases injurious to the Wheat Crops. 8vo. Toronto, 1857.
Boosey, Messrs. (*the Publishers*)—The Musical World for July to Oct. 1858. 4to.
Brett, J. W. Esq. M.R.I. (*the Author*)—Origin and Progress of the Oceanic Electric Telegraph, &c. 8vo. 1858.
British Architects, Royal Institute of—Proceedings in Session 1857-8. 4to.
Calcutta Council of Education—Report of Public Instruction in Bengal, 1856–7. 8vo. 1857.
Calver, Edw. K. Esq. (*the Author*)—On the Construction and Principle of a Wave Screen (for Harbours of Refuge). 8vo. 1858.
Canada, Parliament of—Catalogue of the Library, Vol. II. 8vo. 1858.
Chemical Society—Quarterly Journal, No. 42. 8vo. 1858.
Clay, Mr. Percival (*the Publisher*)—J. Mitchell, on the Extraction of the Precious Metals. 8vo. 1858.
Cornwall Polytechnic Society, Royal—The 25th Annual Report, for 1857. 8vo. 1858.
Department of State, Washington—Map of the Basin of La Plata.
Dublin Society, Royal—Journal, Nos. 9, 10. 8vo. 1858.
Dublin Geological Society—Journal, Vol. VIII. Part 1. 8vo. 1858.
Editors—The Medical Circular for July to Oct. 1858. 8vo.
The Practical Mechanic's Journal for July to Oct. 1858. 4to.
The Journal of Gas-Lighting for July to Oct. 1858. 4to.
The Mechanics' Magazine for July to Oct. 1858. 8vo.
The Athenæum for July to Oct. 1858. 4to.
The Engineer for July to Oct. 1858. fol.
The Artizan for July to Oct. 1858. 4to.
The Atlantis, No. 2. 8vo. 1858.
Faraday, Professor, D.C.L. F.R.S.—Königliche Preussische Akademie, Berichte, Juni–Aug. 1858. 8vo. Abhandlungen, 1857. 4to.
Atti dell'Accademia Pontificia de' Lincei Nuovi, Anno x. Sessioni 6, 7; Anno xi. Sessioni 1-5. 4to. Roma, 1857–58.
Oversigt over det Kongelige Danske Videnskabernes Selskabs Forhandlinger, 1857–58. 8vo. Kjöbenhavn.
Exposition Universelle de 1851. Travaux de la Commission Française. Vol. I. Introduction, par le Baron Dupin: Force Productive des Nations. 8vo. Paris, 1858.
Franklin Institute of Pennsylvania—Journal, Vol. XXXV. No. 6, Vol. XXXVI. Nos. 1–3. 8vo. 1858.
Geographical Society, Royal—Proceedings, Vol. II. Nos. 4, 5. 8vo. 1858.
Geological Society—Quarterly Journal, No. 55. 8vo. 1858.
Geologische Anstalt, Wien—Jahrbuch, 1857. Nos. 2-4. 4to.
Hamilton, Sir Charles, Bart. C.B. M.R.I.—Vocabulary of the Language of the Aborigines of Newfoundland. (MS.) 1820.
Hofmann, Dr. A. W. F.R.S. (*the Author*)—Report on Vegetable Parchment. 8vo. 1858.
James, Lieut.-Col. H. C.E. F.R.S. (*on behalf of the Secretary at War*)—Account of the Observations and Calculations of the principal Triangulation of the Ordnance Trigonometrical Survey of Great Britain and Ireland. 2 vols. 4to. 1858.
Jeffreys, Julius, Esq. F.R.S. M.R.I. (*the Author*)—The British Army in India, &c. 8vo. 1858.
Lendrick, W. E. Esq. (*the Author*)—The Ministry and the Parliament. A Review of the Session of 1858. 8vo. 1858.
Lewin, Malcolm, Esq. M.R.I. (*the Author*)—The Way to regain India. 8vo. 1858.
Linnean Society—Proceedings, No. 9. 8vo. 1858.
Liverpool Literary and Philosophical Society—Proceedings, No. 12. 8vo. 1858.
Manning, J. A. Esq. (*the Author*)—Observations on the Sewage of Glasgow and London. 8vo. 1858.

Newton, Messrs.—London Journal (New Series), July to Oct. 1858. 8vo.
Novello, Mr. (*the Publisher*)—The Musical Times, for July to Oct. 1858. 4to.
Petermann, A. Esq. (*the Editor*)—Mittheilungen auf dem Gesammtgebiete der Geographie. 1858. Heft 5–8. 4to. Gotha, 1858.
Photographic Society—Journal, Nos. 68–71. 8vo. 1858.
Radcliffe Trustees, Oxford—Radcliffe Astronomical Observations in 1856. Vol. XVII. 8vo. 1858.
Reeves, Charles Evans, M.D. (*the Author*)—Diseases of the Spinal Cord, &c. 12mo. 1858.
Royal Society of Edinburgh—G. Lawson's Botanical Papers. 8vo. 1858.
Royal Society of London—Proceedings, No. 32. 8vo. 1858.
Philosophical Transactions, 1858. Vol. 148. Part 1. 4to. 1858.
Sächsische Gesellschaft, Königliche—Abhandlungen. Band IV. (pp. 303–454.) Band VI. (pp. 253–302). 4to. 1858.
Berichte, Phil. Hist. Classe, 1856. Heft 3, 4. 1857, und 1858. Heft 1. Math. Phys. Classe, 1857. Heft 3, 4. 1858. Heft 1. 8vo. 1857–8.
Scoffern, J. M.B.—Capt. T. Thackeray's Manual of Rifle Firing. 12mo. 1588.
Sorby, H. C. Esq. (*the Author*)—Geological Tracts. 8vo. 1851–56.
Statistical Society—Journal, Vol. XXI. Part 3. 8vo. 1858.
St. Pétersbourg, Académie Impériale de—Compte Rendu Annuel, 1856. 8vo. 1857.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Mai–Aug. 1858. 4to.
Trutch, Mr.—Preston's Map of Oregon and Washington Territories. 1856.
Vincent, B. Assist. Sec. R.I.—T. Mapleson, History and Art of Cupping, 2nd edit. 12mo. 1821.
Whitehouse, E. Wildman, Esq. M.R.I. (*the Author*)—The Atlantic Telegraph. 8vo. 1858.
Reply to the Statement of the Directors of the Atlantic Telegraph Company. 8vo. 1858.
Wrey, J. W. Esq. M.R.I.—Reports on Amendment of the Law of Bankruptcy. 8vo. 1858.
Zoological Society—Proceedings, Nos. 350–362. 8vo. 1858.

GENERAL MONTHLY MEETING,

Monday, December 6, 1858.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Right Hon. James A. Stuart Wortley, M.P.
William George Armstrong, Esq.
George F. Chambers, Esq.
Rev. Edwin Prodgers, jun. and
Horace James Smith, Esq.

were duly *elected* Members of the Royal Institution.

Professor T. M. Goodeve, and
C. F. Varley, Esq.

were *admitted* Members of the Royal Institution.

The Secretary announced that the following Arrangements had been made for the Lectures before Easter, 1859:—

Six Lectures on METALLINE PROPERTIES (adapted to a Juvenile Auditory), by MICHAEL FARADAY, Esq. D.C.L. F.R.S. in the Christmas Vacation, 1858-9.

Twelve Lectures on FOSSIL MAMMALS, by RICHARD OWEN, Esq. D.C.L. F.R.S. Fullerian Professor of Physiology, R.I.

Twelve Lectures on THE FORCE OF GRAVITY, by JOHN TYNDALL, Esq. F.R.S. Professor of Natural Philosophy, R.I.

Nine Lectures on ORGANIC CHEMISTRY, by Dr. W. A. MILLER, Professor of Chemistry at King's College, London.

Mr. J. P. LACAITA will commence a Course of Ten Lectures on a Literary subject on Saturday, April 2.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same; viz.

FROM

Actuaries' Institute of—Assurance Magazine, No. 33. 8vo. 1858.
Arts, Society of—Journal for November, 1858. 8vo.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for November 1858. 8vo.
Boosey, Messrs. (the Publishers)—The Musical World for November 1858. 4to.
British Architects, Royal Institute of—Proceedings for November 1858. 4to.
Cambridge Philosophical Society—Transactions, Vol. X. Part 1. 4to. 1858.
Chemical Society—Quarterly Journal, No. 43. 8vo. 1858.
Editors—The Medical Circular for November 1858. 8vo.
The Practical Mechanic's Journal for November 1858. 4to.
The Journal of Gas-Lighting for November 1858. 4to.
The Mechanics' Magazine for November 1858. 8vo.
The Athenæum for November 1858. 4to.
The Engineer for November 1858. fol.
The Artizan for November 1858. 4to.
The British Workman. 1855-7. fol.
Franklin Institute of Pennsylvania—Journal, Vol. XXXVI. Nos. 4, 5. 8vo. 1858.
Geographical Society, Royal—Proceedings, Vol. II. No. 6. 8vo. 1858.
Geological Society—Quarterly Journal, No. 56. 8vo. 1858.
Proceedings for November 1858. 8vo.
Hamilton, Sir Charles, Bart. C.B. M.R.I.—Musci Americani: Specimens of the Mosses collected in British North America during Capt. Franklin's Arctic Expedition. By T. Drummond. 2 vols. 4to. 1828.
Hofmann, Dr. A. W. F.R.S. (the Author)—Report on Vegetable Parchment. 8vo. 1858.
Horner, L. Esq. F.R.S. (the Author)—Geological Researches near Cairo, &c. Part II. (Phil. Trans.) 4to. 1858.
Lee, Robert, M.D. F.R.S. M.R.I. (the Author)—Engravings of the Ganglia and Nerves of the Uterus and Heart. 4to. 1858.
Linnean Society—Proceedings, No. 10. 8vo. 1858.
Machie, S. J. Esq. F.G.S. (the Editor)—The Geologist, Vol. I. 1858.
Murchison, Sir R. I. F.R.S. M.R.I. (the Director)—Memoirs of the Geological Survey of Great Britain:—
Mining Records for 1857. 8vo. 1858.
British Organic Remains: Decade IX. 4to. 1858.
Catalogue of the Rock Specimens and of the Contents of the Mining Record Office in the Museum of Geology. 12mo. 1858.
Newton, Messrs.—London Journal (New Series), for November 1858. 8vo.

Nicholson, Sir Charles, D.C.L. F.R.S.—Catalogue of Egyptian and other Antiquities. 8vo. 1858.
Novello, Mr. (*the Publisher*)—The Musical Times for November 1858. 4to.
Owen, Professor, D.C.L. F.R.S. (*the Author*)—Address at the Meeting of the British Association at Leeds. 8vo. 1858.
Petermann, A. Esq. (*the Editor*)—Mittheilungen auf dem Gesammtgebiete der Geographie. 1858. Heft 9. 4to. Gotha, 1858.
Photographic Society—Journal, Nos 72, 73. 8vo. 1858.
Sorby, H. C. Esq. (*the Author*)—Geological Tracts. 8vo. 1851–6.
United Service Institution—Journal. Nos. 3–6. 8vo. 1857–8.
Watkins, C. R. Esq. (*the Author*)—Principles of Botany. 12mo. 1858.

1859.

WEEKLY EVENING MEETING,

Friday, January 28.

Sir Henry Holland, Bart. M.D. F.R.S. in the Chair.

W. R. Grove, Esq. Q.C. F.R.S. *V.P.R.I.*

On the Electrical Discharge, and its Stratified Appearance in Rarefied Media.

Few subjects of physical investigation possess greater interest than the electrical discharge; its brilliant effects and mysterious characteristics offer powerful stimuli to curiosity and enquiry. The speaker proposed first shortly to state the extent of knowledge we possess respecting it; then to pass to certain peculiar phenomena first discovered by him in 1852, and subsequently experimented on by others, and most elaborately by Mr. Gassiot; and then to offer an opinion as to their cause or rationale.

The best mode of examining and attempting to explain the electrical discharge is to compare it with its nearest analogue flame, to which one form of the discharge, viz. the Voltaic arc, has much seeming resemblance. The flame of a common candle results, as is well known, from the chemical combination of carbon and hydrogen with the oxygen of the air; and the combustion is most brilliant where the heated gases and particles are in proximity to the oxygen. It forms a hollow cone, as the oxygen of the air, being consumed or combined into water and carbonic acid at the exterior portion, cannot reach the interior: the course of the currents of heated air, and the particular form of this hollow cone of flame, are beautifully shown by the refraction it produces on a more brilliant light, such as that of the electric lamp; the flame issues from a single nucleus, the wick; and the amount of heat produced is definite for a definite amount of chemical combination.

In the Voltaic arc there are two points or *foci;* the polar terminals there undergo a change, but not a consumption equivalent or nearly so to the heat and light produced; but if the consumption of the zinc or the quantity of it combined with oxygen in the cells of the battery be compared with the amount of heat generated in the arc, plus that in the cells of the battery and conducting wires, the same amount of total heat will be found to be developed as if the same quantity of zinc were simply burned in oxygen.

By subdividing more and more the plates of the voltaic battery and proportionately increasing their number, we gradually increase the length and diminish the volume of the arc, until at length we arrive, as in the voltaic columns of De Luc and Zamboni, at the electric spark.

The spark from a Ruhmkorff coil was projected on a screen by the electric lamp, and the impression contrasted with that of the flame of a candle; in the former two cones are seen to issue from the terminals instead of the single one of the latter, one being more powerful, and overcoming or beating back the other; and this effect is reversed as the direction of the current is reversed.

In all cases hitherto observed there is a dispersion or projection of a portion of the terminals; this takes place in all forms of electric disruptive discharge, whatever be the materials of which the terminals are composed. In the voltaic arc there is a transmission of matter, principally from the positive, which is the more intensely heated, to the negative terminal; in the spark from the Ruhmkorff coil the dispersion is principally, and in some cases appears to be entirely, from the negative terminal, while this is now the more intensely heated.

In addition to this, there is generally, but not always, a change produced in the medium across which the discharge passes; compound liquids, vapours, and gases are decomposed, and even elementary gases are allotropically changed. There is also a polar condition of the electrical discharge, which produces the converse chemical effects at each pole—effects described by Mr. Grove in a paper in the Philosophical Transactions for 1852, and subsequently shown at an evening meeting of this Institution.

Gases offer a powerful resistance to the passage of the discharge, but this resistance is diminished as the gases are rarefied; and a discharge which would not pass across a space of half an inch in air of the ordinary density will pass through several feet in highly attenuated air.

In experimenting on the passage of the discharge through the vapour of phosphorus in 1852, Mr. Grove observed for the first time that the discharge was traversed by a number of dark bands or striæ. At first he was disposed to attribute this phenomenon to some peculiarity of the medium; but on trying good *vacua* of other vapours and gases, he found the striæ were in all cases visible, and seemed to depend on the degree of rarefaction of the gas. Many subsequent experiments have been made by himself and others on the subject, and more particularly by Mr. Gassiot; and the extent of knowledge we have acquired upon this still mysterious phenomenon was now discussed and illustrated.

In the vapour of phosphorus the striæ generally exhibit themselves like narrow ruled lines, about 0·05 inch diameter, transverse to the line of discharge; but with certain precautions they become wider and assume a conical form, somewhat resembling the whalebone snakes made as a toy for children. Mr. Gassiot has used most carefully prepared Torricellian *vacua*, and has also, in conjunction with Dr. Frankland, obtained excellent *vacua*, by filling tubes containing sticks of caustic potass with carbonic acid, exhausting them by the air-pump, and allowing the residual gas to be absorbed by the potass.

The following is a summary of the effects produced by the electric discharge through these *vacua*.

If the vacuum be equal to that generally obtained by an ordinary air-pump, no stratifications are perceptible; a diffused lambent light fills the tube: in a tube in which the rarefaction is carried a step further, narrow striæ are perceptible, like those first described in the phosphorus vapour experiment. A step further in rarefaction increases the breadth of the bands; next we get the conical or cup-shaped form; and then, the rarefaction being still higher, we get a series of luminous cylinders of an inch or so in depth, with narrow divisions between them. Lastly, with the best vacua which have been obtained, there is neither discharge, light, or conduction.* The fact of non-conduction by a very good Torricellian vacuum was first noticed by Walsh, subsequently carefully experimented on by Morgan (Philosophical Transactions, 1785), and subsequently by Davy (1822); the latter did not obtain an entire non-conduction, but a considerable diminution both of light and conducting power.

From these repeated experiments it may fairly be considered as proved, that *in vacuo*, or in media rarefied beyond a certain point, electricity will not be conducted, or more correctly speaking, transmitted; an extremely important result in its bearing on the theories of electricity.

The gradual widening of the strata, as the rarefaction proceeds, is in favour of the phenomena of stratification being due to mechanical impulses of the attenuated medium, and appears to support the following rationale of the phenomenon given by Mr. Grove; who does not advance it as conclusive, but only as an approximation to a theory to be sifted by further experiments. When the battery contact is broken, there is generated the well-known induced current in the secondary wire in the same direction as the original battery current, to

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* The production of vacua by carbonic acid, and the increasing breadth of the stratifications with increased rarefaction, was communicated by Mr. Gassiot in a paper, read to the Royal Society, Jan. 13, 1859. I incline to think that oxy-hydrogen gas, with potash, might give a better vacuum than carbonic acid, as the last residual portions of the gas would be slowly combined by the discharge, and the water so formed absorbed by the potash.—W. R. G.

which secondary current the brilliant effects of the Ruhmkorff coil are due: but in addition to this current in the secondary wire, there is also a secondary current in the primary wire, flowing in the same direction; the induction spark, at the moment following the disruption of contact, completing the circuit of the primary, and thus allowing the secondary current to pass. This secondary current in the primary wire produces in its turn another secondary, or what may be termed a tertiary, current in the secondary wire, in an opposite direction to the secondary current. There are thus, almost synchronously, two currents in opposite directions in the secondary wire; these, by causing a conflict or irregular action on the rarefied medium, would give rise to waves or pulsations, and might well account for the stratified appearance. The experimental evidence in favour of this view is as follows: when a single break of battery contact is made by drawing a stout copper wire over another wire, the striæ do not invariably appear in the rarefied medium through which the current of the secondary wire passes. This would be accounted for on the above theory by supposing that in some cases of disruption the induced spark passes across immediately on disruption, and thus completes the circuit for the secondary current in the primary wire; while in other cases, either from want of sufficient intensity, or from the mode or velocity with which contact is broken, or from the oxidation of the points where contact is broken, there is no induced spark by which the current can pass: in the former case there would be a tertiary current in the secondary wire, and therefore striæ; in the latter there would be none.

But the following experiment is more strongly in favour of the theory. It is obvious that the secondary must be more powerful than the tertiary current. Now supposing an obstacle or resistance placed in the secondary circuit, which the secondary current can overcome but the tertiary cannot, we ought by the theory to get no striæ. If an interruption be made in the secondary current in addition to that formed by the rarefied medium, and this interruption be made of the full extent which the spark will pass, there are, as a general rule, no striæ in the rarefied medium, while the same vacuum tube shows the striæ well if there be no such break or interruption. The experiment was shown by a large vacuum cylinder (16 inches by 4) of Mr. Gassiot, and his micrometer electrometer; this tube showed numerous broad and perfectly distinct bands when the points of the micrometer were in contact; but when they were separated to the fullest extent that would allow sparks to pass, not the slightest symptom of bands or striæ were perceptible, the whole cylinder was filled with an uniform lambent flame. With a spark from the prime conductor of the electrical machine, the striæ do not appear in tubes which show them well with the Ruhmkorff coil; occasionally, and in rare instances, striæ may be seen with sparks from the electrical machine, but, not as far as Mr. Grove has observed, when the spark is unquestionably single. All this is in favour of the theory given above; but without regarding that as conclusive or as a proved *rationale*, it is clearly demonstrated by the above experiments, that the identical vacuum tubes which show the striæ with certain modes of producing the discharge, do not show them with other modes, and that therefore the striæ are not a necessary condition of the discharge itself in highly attenuated media, but depend upon the mode of its production.

The study of the electrical discharge *in vacuo* is of the utmost importance in reference to the theories of electricity, and probably will assist much towards the proper conception of other modes of force, or, as they are termed, *imponderables*, heat, light, &c.

The experiments of Walsh and Morgan, corroborated as they now are by that of Mr. Gassiot, show, that although the transmission of electricity across gaseous media is aided by rarefaction of the medium up to a certain degree, yet that a degree of attenuation may be reached at which the transmission ceases, at all events for a given distance between the terminals and given intensity of electrical charge. Whether having arrived at this point a reduction of the space to be traversed, or an increase of intensity in electricity, or both, would again enable the electricity to pass, is not quite clear, though there is reason to believe that it would, and the increased intensity of electricity would probably be again stopped by a further improvement in the vacuum, and so on. But the experiments go far to prove that ordinary matter is requisite for the transmission of electricity, and that if space could exist void of matter, then there would be no electricity: thus supporting the views advocated by Mr. Grove and some others, that electricity is an affection or mode of motion of ordinary matter.

The non-transmission of electricity by very highly attenuated gas may also afford much assistance to the theory of the aurora borealis, a phenomenon, the appearance of which, the regions where it is seen, its effect on the magnet, and other considerations, have led to the universal belief that it is electrical.

The experimental result that a certain degree of attenuation of air forms a good conductor, or easy path for the electrical force, while either a greater or a less degree of density offers more resistance, and this increasing towards either extremity of density or rarefaction, show, that if there be currents of electricity circulating to or from the polar regions of the earth, the return of which, as is generally believed, gives rise to the beautiful phenomena of the aurora borealis or australis, the height where this transit of electricity takes place would be just that at which the density of the air is such as to render it the best conductor. By careful measurement of the degree of attenuation requisite to enable the electrical discharge to pass with the greatest facility in our laboratory experiments, we may approximatively estimate the degree of rarefaction of the atmosphere at the height where the aurora borealis exists. By these means we get a mode of estimating the height of the aurora by ascertaining, from the decrement of density in the atmosphere in proportion to its distance from the earth, at what elevation the best conducting state, or that similar to our best conducting vacuum tubes would be found, or conversely, by ascertaining the height of the aurora by parallactic measurements, we may ascertain the ratio of decrement in the density of the atmosphere. Thus by our cabinet experiments, light may be thrown on the grand phenomena of the universe, and the great questions of the divisibility of matter, whether there is a limit to its expansibility, whether there is a fourth state of attenuation beyond the recognised states of solid, liquid, and gaseous, as Newton seemed to suspect, (30th query to the Optics,) and whether the imponderables are specific affections of matter in a peculiar state, or of highly attenuated gaseous matter, may be elucidated. The manageable character of the electrical discharge, and the various phenomena it exhibits when matter is subjected to its influence in all those varied states which we are enabled, by experiment, to reduce it, can hardly fail to afford new and valuable information on these abstruse and most interesting enquiries.

[W. R. G.]

WEEKLY EVENING MEETING,

Friday, February 4, 1859.

Sir R. I. Murchison, D.C.L. F.R.S. Vice-President, in the Chair.

Professor Owen, F.R.S. &c.

FULLERIAN PROFESSOR OF PHYSIOLOGY, ROYAL INSTITUTION.

On the Gorilla.

The lecturer referred to the discourse "On the Anthropoid Apes and their relations to Man," delivered by him in 1855, in which so much as was at that time known respecting the extraordinary and then recently-discovered species of ape, the subject of the present lecture, had been detailed, with inferences drawn from the osteology and dentition of the gorilla, in regard to the hypothetical origin of man by transmutation and development of the ape. The additional facts, subsequently ascertained respecting the gorilla, although they prove its nearer approach to man than any other tailless ape, have not in any degree affected or invalidated the conclusions at which the lecturer had arrived in his previous discourse.

Since the date of that discourse, skeletons and the entire carcase preserved in spirits of the gorilla had successively reached the Museums of Paris, Vienna, and London ; and had formed the subjects of several memoirs, the results of the recorded observations differing only in regard to the interpretation of the facts.

Dr. Wyman, the accomplished anatomical professor at Boston, U.S., agreed with Professor Owen, in referring the gorilla to the same genus as the chimpanzee *(Troglodytes)*, but he regards the latter as more nearly allied to the human kind.

Professors Duvernoy and Isidore Geoffroy St. Hilaire consider the differences in the osteology, dentition, and outward character of the gorilla to be of generic importance; and they enter the species in the zoological catalogues as *Gorilla gina*, the trivial name being that by which the animal is called by the natives of Gaboon; the French naturalists also concur with the American in placing the gorilla below the chimpanzee in the zoological scale; and some have more lately been disposed to place both below the siamangs, gibbons or long-armed apes *(Hylobates)*.

Deferring the discussion of these questions, the lecturer, referring to a spirited and accurate painting, life-size, of the adult male gorilla, by Wolf, proceeded to describe the external characters of the animal, as they were exhibited by the specimen preserved in spirits which had shortly before been received at the British Museum, and had since been admirably prepared and mounted by Mr. Bartlett, the well-known taxidermist. The lecturer first called attention to the shortness, almost absence, of neck, due to the backward position of the junction of the head to the trunk, to the great length of the cervical spines, causing the "nape" to project beyond the "occiput," to the great size and elevation, of the scapulæ, and to the oblique rising of the clavicles from their sternal attachments to above the level of the angles of the jaw. The brain-case, low and narrow, and the lofty ridges of the skull, make the cranial profile pass in almost a straight line from the occiput to the superorbital ridge, the prominence of which gives the most forbidding feature to the physiognomy of the gorilla; the thick integument overlapping that ridge forming a scowling pent-house over the eyes. The nose is more prominent than in the chimpanzee or orang-utan, not only at its lower expanded part, but at its upper half, where a slight prominence corresponds with that which the author had previously pointed out in the nasal bones. The mouth is very wide, the lips large, of uniform thickness, the upper one with a straight, as if incised margin, not showing the coloured lining membrane when the mouth is shut. The chin very short and receding, the muzzle very prominent. The eyelids with eye-lashes, the eyes wider apart than in the orang or chimpanzee; no eyebrows; but the hairy scalp continued to the superorbital ridge. The ears smaller in proportion than in man, much smaller than in the chimpanzee; but the structure of the auricle more like that of man: it was minutely described and compared. On a direct front view of the face, the ears are on the same parallel with the eyes. The teeth had been described in the lecturer's former discourse.* The huge canines in the male give a most formidable aspect to the beast: they were not fully developed in the younger and entire specimen, now mounted. The profile of the trunk

* "On the Anthropoid Apes:" Proceedings, R.I. Vol. II. (1855) p. 26; and in the Transactions of the Zoological Society, 1848.

describes a slight convexity from the nape to the sacrum,—there being no inbending at the loins, which seem wanting, the thirteenth pair of ribs being close to the "labrum ilii." The chest is of great capacity; the shoulders very wide across; the pectoral regions are slightly marked, and show a pair of nipples placed as in the chimpanzee and human species. The abdomen is somewhat prominent, both before and at the sides. The pelvis relatively broader than in other apes.

The chief deviations from the human structure were seen in the limbs, which are of great power, the upper ones prodigiously strong. The arm from below the short deltoid prominence preserves its thickness to the condyles; a uniform circumference prevails in the forearm; the leg increases in thickness from below the knee to the ankle. There is no calf. These characters of the limbs are due to the general absence of those partial muscular enlargements which impart the graceful varying curves to the outlines of the limbs in man. Yet they depended, the lecturer remarked, rather on excess, than defect, of development of the carneous as compared with the tendinous parts of the limb-muscles, which thus continue of almost the same size from their origin to their insertion, with a proportionate gain of strength to the beast. The difference in the length of the upper limbs between the gorilla and man is but little in comparison with the trunk; it appears greater through the arrest of development of the lower limbs. Very significant of the closer anthropoid affinities of the gorilla was the superior length of the arm (humerus) to the fore-arm, as compared with the proportions of those parts in the chimpanzee. The hair of the arm inclines downward, that of the fore-arm upward, as in the chimpanzee. The thumb extends a little beyond the base of the proximal phalanx of the fore-finger; it does not reach to the end of the metacarpal bone in the chimpanzee or any other ape: the thumb of the siamang is still shorter in proportion to the length of the fingers of the same hand: the philosophical zoologist will see great significance in this fact. In man the thumb extends to, or beyond, the middle of tho first phalanx of the fore-finger.

The fore-arm in the gorilla passes into the hand with very slight evidence, by constriction, of the wrist; the circumference of which, without the hair, was fourteen inches, that of a strong man averaging eight inches. The hand is remarkable for its breadth and thickness, and for the great length of the palm, occasioned both by the length of the metacarpus and the greater extent of undivided integument between the digits than in man; these only begin to be free opposite the middle of the proximal or first phalanges in the gorilla. The digits are thus short, and appear as if swollen and gouty; and are conical in shape after the first joint, by tapering to nails, which, being not larger or longer than those of man, are relatively to the fingers much smaller. The circumference of the middle digit at the first joint in the gorilla is $5\frac{1}{2}$ inches; in man, at the same part, it averages $2\frac{3}{4}$ inches. The skin covering the middle phalanx is thick and callous on the backs of the fingers, and there is little outward appearance of the second joint. The habit of the animal to apply those parts to the ground, in occasional progression, is manifested by these callosities. The back of the hand is hairy as far as the divisions of the fingers; the palm is naked and callous. The thumb, besides its shortness, according to the standard of the human hand, is scarcely half so thick as the fore-finger. The nail of the thumb did not extend to the end of that digit; in the fingers the nail projected a little beyond the end, but with a slightly convex worn margin, resembling the human nails in shape, but relatively less.

In the hind limbs, chiefly noticeable was that first appearance in the quadrumanous series of a muscular development of the gluteus, causing a small buttock to project over each tuber ischii. This structure, with the peculiar expanse (in *quadrumana)* of the iliac bones, leads to an inference that the gorilla must naturally and with more ease resort occasionally to station and progression on the lower limbs than any other ape.

The same cause as in the arm, viz., a continuance of a large proportion of fleshy fibres to the lower end of the muscles, co-extensive with the thigh, gives a great circumference to that segment of the limb above the knee-joint, and a more uniform size to it than in man. The relative shortness of the thigh, its bone being only eight-ninths the length of the humerus (in man the humerus averages five-sixths the length of the femur), adds to the appearance of its superior relative thickness. Absolutely the thigh is not of greater circumference at its middle than is the same part in man.

The chief difference in the leg, after its relative shortness, is the absence of a "calf," due to the non-existence of the partial accumulation of carneous fibres in the gastrocnemii muscles, causing that prominence in the type-races of mankind. In the gorilla the tendo-achillis not only continues to receive the "penniform" fibres to the heel, but the fleshy parts of the muscles of the foot receive accessions of fibres at the lower third of the leg, to which the greater thickness of that part is due, the proportions in this respect being the reverse of those in man. The leg expands at once into the foot, which has a peculiar and characteristic form, owing to the modifications favouring bipedal motion being superinduced upon an essentially prehensile, quadrumanous type. The heel makes a more decided backward projection than in the chimpanzee; the heel-bone is relatively thicker, deeper, more expanded vertically at its hind end, besides being fully as long as in the chimpanzee. This bone, so characteristic of anthropoid affinities, is shaped and proportioned more like the human calcaneum than in any other ape. The malleoli do not make such well-marked projections as in man; they are marked more by the thickness of the fleshy and tendinous parts of the muscles that pass near them, on their way to be inserted into parts of the foot. Although the foot be articulated to the leg with a slight inversion of the sole, it is more nearly plantigrade than in the chimpanzee or any other ape. The hallux (great toe, thumb of the foot), though not relatively longer than in the chimpanzee, is stronger; the bones are thicker in proportion to their length, especially the last phalanx, which in shape and breadth much resembles that in the human foot. The hallux in its natural position diverges from the other toes at an angle of 60 deg. from the axis of the foot; its base is large, swelling into a kind of ball below, upon which the thick callous epiderm of the sole is continued. The transverse indents and wrinkles show the frequency and freedom of the flexile movements of the two joints of the hallux; the nail is small, flat, and short. The sole of the foot gradually expands from the heel forward to the divergence of the hallux, and seems to be here cleft, and almost equally, between the base of the hallux and the common base of the other four digits. These are small and slender in proportion, and their bases are enveloped in a common tegumentary sheath as far as the base of the second phalanx. A longitudinal indent at the middle of the sole, bifurcating—one channel defining the ball of the hallux, the other running towards the interspace between the second and third digit—indicates the action of opposing the whole thumb (which seems rather like an inner lobe or division of the sole), to the outer division terminated by the four short toes. What is termed the "instep" in man is very high in the gorilla, owing to the thickness of the carneo-tendinous parts of the muscles as they pass from the leg to the foot over this region. The mid-toe (third) is a little longer than the second and fourth; the fifth, as in man, is proportionally shorter than the fourth, and is divided from it by a somewhat deeper cleft. The whole sole is wider than in man—relatively to its length much wider—and in that respect, as well as by the off-set of the hallux, and the definition of its basal ball, more like a hand, but a hand of huge dimensions and of protentous power of grasp.

The hairy integument is continued along the dorsum of the foot to the clefts of the toes, and upon the first phalanx of the hallux: the whole sole is bare.

In regard to the outward coloration of the gorilla, only from the examination of the living animal could the precise shades of colour of the naked parts of the skin be truly described. Much of the epiderm had peeled off the subject of the present discourse; but fortunately in large patches, and the texture of these had acquired a certain firmness, apparently by the action of the alcohol upon the albuminous basis. The parts of the epiderm remaining upon the face indicated the skin there to be chiefly of a deep leaden hue; it is everywhere finely wrinkled, and was somewhat less dark at the prominent parts of the supraciliary roll and the prominent margins of the nasal "alæ:" the soles and palms were also of a lighter colour.

Although the general colour of the hair appears, at first sight, and when moist, to be almost black, it is not so, but is rather of a dusky grey: it is decidedly of a less deep tint than in the chimpanzee (*Trogl. niger*): this is due to an admixture of a few reddish, and of more greyish, hairs with the dusky coloured ones which chiefly constitute the "pelage:" and the above admixture varies at different parts of the body. The reddish hairs are so numerous on the scalp, especially along the upper middle region, as to make their tint rather predominate there; they blend in a less degree with the long hairs upon the sides of the face. The greyish hairs are found mixed with the dusky upon the dorsal, deltoidal and anterior femoral, regions; but on the limbs, not in such proportion as to affect the impression of the general dark colour, at first view. Near the margin of the vent are a few short whitish hairs, as in the chimpanzee. The epiderm of the back showed the effects of habitual resting, with that part against the trunk or branch of a tree, occasioning the hair to be more or less rubbed off: the epithelium was here very thick and tough.

It is most probable, from the degree of admixture of different coloured hairs above described, that a living gorilla seen in bright sunlight, would in some positions reflect from its surface a colour much more different from that of the chimpanzee than appears by a comparison of the skin of a dead specimen sent home in spirits. It can hardly be doubted also, that age will make an appreciable difference in the general coloration of the *Troglodytes gorilla.*

The adult male gorilla measures five feet six inches from the sole to the top of the head, the breadth across the shoulders is nearly three feet, the length of the upper limb is three feet four inches, that of the lower limb is two feet four inches; the length of the head and trunk is three feet six inches, whilst the same dimension in man does not average three feet.

In the foregoing remarks the lecturer had given the results of direct observations made on the first and only entire specimen of the gorilla which had reached England. At the period when they were made, no other description of its external characters had reached him; and if the majority of them be found to agree with previously recorded observations by naturalists enjoying earlier opportunities of studying similarly preserved specimens, the rarity and importance of the species might excuse, if it did not justify, a second description from direct scrutiny of a new specimen by an old observer of the anthropoid quadrumana. A much more important labour, however, remained. The accurate record of facts in natural history was one and a good aim; the deduction of their true consequences was a better. Professor Owen proceeded, therefore, to reconsider the conclusions from which his experienced French and American fellow-labourers in natural history differed from him, and in which it seemed he stood alone.

The first—it may be called the supreme—question in regard to the gorilla was, its place in the scale of nature, and its true and precise affinities.

Is it or not the nearest of kin to human kind? Does it form, like the chimpanzee and orang, a distinct genus in the anthropoid or knuckle-walking group of apes? Are these apes, or are the long-armed gibbons, more nearly related to the genus *Homo?* Of the broad-breast-boned quadrumana, are the knuckle-walkers or the brachiators, *i.e.* the long-armed gibbons, most nearly and essentially related to the human subject? Professor Owen proceeded to grapple with the first as the most important question.

At the first aspect, whether of the entire animal or of the skeleton, he freely admitted that the gorilla strikes the observer as being a much more bestial and brutish animal than the chimpanzee. All the features that relate to the wielding of the strong jaws and large canines are exaggerated; the evidence of brain is less; its proper cavity is more masked by the outgrowth of the strong occipital and other cranial ridges. But then the impression so made that the gorilla is less like man, is the same which is derived from comparing a young with an adult chimpanzee, or some small tailless monkey with a full-grown male orang or chimpanzee. Taking the characters that cause that impression at a first inspection of the gorilla, most of the small South American monkeys are more anthropoid; they have a proportionally larger and more human-shaped cranium, much less prominent jaws, with more equable teeth.

Referring to the diagrams of the skeletons of the adult males of the gorilla, chimpanzee, orang, and gibbon, Professor Owen remarked that the globular cranium of the last, and its superior size compared with the jaws and teeth, seemed to show the gibbons to be more nearly akin to man than any of the larger tailless apes. And this conclusion had been formed by a distinguished French palæontologist, M. Lartet, and accepted by a high geological authority at home.* The experienced Professor of Human Anatomy at Amsterdam had been also cited as supporting this view; but the lecturer had failed to find any statement of the grounds upon which it was sustained. In the art. *Quadrumana* of Todd's Cyclopædia, cited by Lartet,† Professor Vrolik briefly treats of the osteology of the *Quadrumana* according to their natural families. In "a first genus, *Simia* proper, or ape," he includes the chimpanzee or orang, noticing some of the chief points by which these apes approach the nearest to man. He next goes to the second genus, the gibbon *(Hylobates)*, notices their ischial callosities, and the nearer approach of their molars, in their rounded form, to the teeth of *carnivora* than the molars of the genus *Simia*. Then, comparing the siamang with other species of *Hylobates*, Vrolik says, "its skeleton approaches most to that of man," which may be true in comparison with other gibbons, but certainly is not so as respects the higher *Simiæ*. No details are given to illustrate the proposition even in its more limited application; but the minor length of the arms in the siamang, as compared with *Hylobates lar*, was probably the obvious character in Vrolik's mind.

The appearance of superior cerebral development in the siamang and other long-armed apes is due to their small size and the concomitant feeble development of their jaws and teeth. The same appearance makes the small platyrrhine monkeys of South America equally anthropoid in their facial physiognomy, and much more human-like than are the great orangs and chimpanzees. It is an appearance which depends upon the precocious growth of the brain, as dependent on the law of its development. In all *quadrumana* the brain has reached its full size before the second set of teeth is acquired, almost before

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* Sir C. Lyell, Supplement to the 5th Edition of a Manual of Elementary Geology, 1859, p. 15.

† Comptes Rendus de l'Académie des Sciences, Juillet 28, 1856.

the first set is shed. If a young gorilla, chimpanzee, or orang, be compared with a young siamang, of corresponding age, the absolutely larger size and better shape of brain, the deeper and more numerous convolutions of the cerebrum, and the more completely covered cerebellum, unequivocally demonstrate the higher organization of the shorter-armed apes; "in the structure of the brain," writes Vrolik,* in accordance with all other comparative anatomists, "they" (chimpanzee and orang-utan) "approach the nearest to man." The degree to which the chimpanzee and orang so resembled the human type seemed much closer to Cuvier, who knew those great apes only in their immaturity, with their small milk teeth and precociously developed brain. Accordingly, the anthropoid characters of the *Simia satyrus* and *Simia troglodytes*, as deduced from the facial angle and dentition, are proportionally exaggerated in the "Règne Animal."† As growth proceeds, the milk-teeth are shed, the jaws expand, the great canines succeed their diminutive representatives, the biting muscles gain a proportional increase of carneous fibres, their bony fulcra respond to the call for increased surface of attachment, the sagittal and occipital crests begin to rise; but the brain grows no more; its cranial box retains the size it showed in immaturity; it finally becomes masked by the superinduced osseous developments in those apes which attain the largest stature and wield the most formidably armed jaws. Yet under this disguise of physical force, the brain is still the better and the larger than is that of the little long-armed ape, which retains throughout life so much more of the characters of immaturity, especially in the structure of the skull.

The siamang and other gibbons have smaller lower but longer upper canines, relatively, than in the orangs and chimpanzees; the permanent ones more quickly attain their full size, and are sooner in their place in the jaws; consequently the last molar teeth—what we call the "wisdom-teeth"—come last into place as they do in the human species. But, if this be interpreted as of importance in determining the relative affinity of the longer-armed and shorter-armed apes to man, it is a character in which, as in their seeming superior cerebral development, the *Hylobates* agree with some much lower *quadrumana* with still smaller canines. The comparative anatomist, pursuing this most interesting comparison with clear knowledge of the true conditions and significance of a globular cranium and small jaws within the quadrumanous order, turns his attention to the true distinctive characters of the human organization.

In respect to the brain, he would look not so much for its relative size to the body, as for its relative size in the species compared one with another in the same natural group. He would enquire what quadrumanous animal shows absolutely the biggest brain? what species shows the deepest and most numerous and winding convolutions? in

* Art. *Quadrumana*, Cyclopædia of Anatomy, vol. iv., p. 195.
† Ed. 1829, pp. 87, 89.

which is the cerebrum largest, as compared with the cerebellum? If he found all these characters highest in the gorilla, he would not be diverted from the just inference because the great size and surpassing physical power attained in that species masked the true data from obvious view.

The comparative anatomist would look to the cæcum and the ischial integument: if he found in one subject of his comparisons (*Troglodytes*) a long "appendix vermiformis cæci," as in man, but no "callosities," and in another subject (*Hylobates*) the ischial callosities but only a short rudiment of the cæcal appendix, he would know which of the two tailless apes were to be placed next "the monkeys with ischial callosities and no vermiform appendix," and which formed the closer link toward man. He would find that the anthropoid intestinal and dermal characters were associated with the absolutely larger and better developed brain in the gorilla, chimpanzee, and orang; whilst the lower quadrumanous characters exhibited by the cæcum and nates were exhibited by the smaller-brained and longer-armed tailless gibbons.

Pursuing the comparison through the complexities of the bony framework, the comparative anatomist would first glance at the more obvious characters; and such, indeed, as would be given by the entire animal. The characteristics of the limbs in man are their near equality of length, but the lower limbs are the longest. The arms in man reach to below the middle of the thigh; in the gorilla they nearly attain the knee; in the chimpanzee they reach below the knee; in the orang they reach the ankle; in the siamang they reach the sole; in most gibbons the whole palm can be applied to the ground without the trunk being bent forward beyond its naturally inclined position on the legs. These gradational differences coincide with other characters determining the relative proximity of the apes compared with man. In no quadrumana does the humerus exceed the ulna so much in length as in man; only in the very highest and most anthropoid, viz. the gorilla and chimpanzee, does it exceed the ulna at all in length; in all the rest, as in the lower quadrupeds, the fore-arm is longer than the arm.

The humerus, in the gorilla, though less long, compared with the ulna, than in man, is longer than in the chimpanzee; in the orang it is shorter than the ulna; in the siamang and other gibbons it is much shorter, the peculiar length of arm in those "long-armed apes" is chiefly due to the excessive length of the antibrachial bones.

The difference in the length of the upper limbs, as compared with the trunk, is but little between man and the gorilla. The elbow-joint in the gorilla, as the arm hangs down, is opposite the "labrum ilii," the wrist opposite the "tuber ischii;" it is rather lower down in the chimpanzee; is opposite the knee-joint in the orang; and opposite the ankle-joint in the siamang.

Man's perfect hand is one of his peculiar physical characters; that perfection is mainly due to the extreme differentiation of the first from the other four digits, and its concomitant power of opposing them as a perfect thumb. An opposable thumb is present in the hand of most *quadrumana*, but is usually a small appendage compared with that of man. It is relatively largest in the gorilla. In this ape the thumb reaches to a little beyond the base of the first phalanx of the fore-finger; it does not reach to the end of the metacarpal bone of the fore-finger in the chimpanzee, orang, or gibbon; it is relatively smallest in the last tailless ape. In man the thumb extends to or beyond the middle of the first phalanx of the fore-finger. The philosophical zoologist will see great significance in the results of this comparison. Only in the gorilla and chimpanzee are the carpal bones eight in number, as in man; in the orangs and gibbons they are nine in number, as in the tailed monkeys.

The scapulæ are broader in the gorilla than in the chimpanzee, orang, or long-armed apes; they come nearer to the proportions of that bone in man. But a more decisive resemblance to the human structure is presented by the iliac bones. In no other than the gorilla do they bend forward, so as to produce a pelvic concavity; nor are they so broad in proportion to their length in any ape as in the gorilla. In both the chimpanzee and orang the iliac bones are flat, or present a concavity rather at the back than at the forepart. In the siamang they are not only flat, but are narrower and longer, resembling the iliac bones of tailed monkeys and ordinary quadrupeds.

The lower limbs, though characteristically short in the gorilla, are longer in proportion to the upper limbs, and also to the entire trunk, than in the chimpanzee; they are much longer in both proportions and more robust than in the orangs or gibbons. But the guiding points of comparisons here are the heel and the hallux (great toe or thumb of the foot).

The heel in the gorilla makes a more decided backward projection than in the chimpanzee; the heelbone is relatively thicker, deeper, more expanded vertically at its hind end, beside being fully as long as in the chimpanzee: it is in the gorilla shaped and proportioned more like the human calcaneum than in any other ape. Among all the tailless apes the calcaneum in the siamang and other gibbons least resembles in its shape or proportional size that of man.

Although the foot be articulated to the leg with a slight inversion of the sole it is more nearly plantigrade in the gorilla than in the chimpanzee. The orang departs far, and the gibbons farther, from the human type in the inverted position of the foot.

The great toe which forms the fulcrum in standing or walking is perhaps the most characteristic peculiarity in the human structure; it is that modification which differentiates the foot from the hand, and gives the character to his order (*Bimana*). In the degree of its approach to this development of the hallux the quadrumanous animal makes a true step in affinity to man.

The orang-utan and the siamang, tried by this test, descend far and abruptly below the chimpanzee and gorilla in the scale. In the orang the hallux does not reach to the end of the metacarpal of the second toe; in the chimpanzee and gorilla it reaches to the end of the first phalanx of the second toe; but in the gorilla the hallux is thicker and stronger than in the chimpanzee. In both, however, it is a true thumb, by position, diverging from the other toes, in the gorilla, at an angle of 60 degrees from the axis of the foot.

Man has 12 pairs of ribs, the gorilla and chimpanzee have 13 pairs, the orangs have 12 pairs, the gibbons have 13 pairs. Were the naturalist to trust to this single character, as some have trusted to the cranio-facial one, and in equal ignorance of the real condition and value of both, he might think that the orangs (*Pithecus*) were nearer akin to man than the chimpanzee (*Troglodytes*) are. But man has sometimes a thirteenth pair of ribs; and what we term "ribs" are but vertebral elements or appendages common to nearly all the true vertebræ in man, and only so called, when they become long and free. The genera *Homo*, *Troglodytes*, and *Pithecus*, have precisely the same number of vertebræ: if *Troglodytes*, by the development and mobility of the pleurapophyses of the 20th vertebra from the occiput seem to have an additional thoracic vertebra, it has one vertebra less in the lumbar region. So, if there be, as has been observed in the same genus, a difference in the number of sacral vertebræ, it is merely due to a last lumbar having coalesced with what we reckon the first sacral vertebra in man.

The thirteen pairs of ribs, therefore, in the gorilla and chimpanzee are of no weight, as against the really important characters significative of affinity with the human type. But, supposing the fact of any real value, how do the advocates of the superior resemblance of the gibbon's skeleton to that of man dispose of the thirteenth pair of ribs?

In applying the characters of the skull to the determination of the important question at issue those must first be ascertained by which the genus *Homo* trenchantly differs from the genus *Simia*, of Linnæus. To determine these osteal distinctions, the lecturer stated that he had compared the skulls of many individuals of different varieties of the human race together with those of the male, female, and young of species of *Troglodytes*, *Pithecus*, and *Hylobates;* Professor Owen referred to his Catalogue of the Osteological Series in the Museum of the Royal College of Surgeons, 4to., 1853, for the detailed results of these comparisons. On the present occasion he would restrict himself to a few of these results.

The first and most obvious differential character is the globular form of the brain-case, and its superior relative size to the face, especially the jaws, in man. But this, for the reasons he had already assigned, is not an instructive or decisive character, when comparing quadrumanous species, in reference to the question at issue. It is exaggerated in the human child, owing to the acquisition of its full, or nearly full size, by the brain, before the jaws have expanded to lodge the second set of teeth. It is an anthropoid character in which the quadrumana resemble man, in proportion to the diminution of their general bulk. If a gorilla, with milk-teeth, have a somewhat larger brain and brain-case than a chimpanzee at the same immature age, the acquisition of greater bulk by the gorilla, and of a more formidable physical development of the skull, in reference to the great canines in the male, will give to the chimpanzee the appearance of a more anthropoid character, which really does not belong to it; which could be as little depended upon in a question of precise affinity as the like more anthropoid characters of the female, as compared with the male, gorilla or chimpanzee.

Much more important and significant were the following characters of the human skull. The position and plane of the occipital foramen; the proportional size of the condyloid and petrous processes; the mastoid processes, which relate to balancing the head upon the trunk in the erect altitude; the small premaxillaries and concomitant small size of the incisor teeth, as compared with the molar teeth. This character relates to the superiority of the psychical over the physical powers in man. It governs the feature in which man recedes from the brute, as does also the prominence of the nasal bones in most, and in all the typical, races of man. The somewhat angular form of the bony orbits, tending to a square, with the corners rounded off, is a good human character of the skull; which is difficult to comprehend as an adaptive one, and therefore the better in the present inquiry. The same may be said of the production of the floor of the tympanic or auditory tube into the plate called "vaginal."

Believing the foregoing to be sufficient to test the respective degrees of affinity to man within the limited group of quadrumana to which it was proposed, in the present lecture, to apply them, the speaker would not weary his audience or weaken his argument by citing minor characters. The question at issue is, as between the anthropoid apes and man. Cuvier deemed the orang (*Pithecus*) to be nearer akin to man than the chimpanzee (*Troglodytes*) is. That belief has long ceased to be entertained. Professor Owen proceeded, therefore, to compare the gorilla, chimpanzee, and gibbon, in reference to their human affinities.

Most naturalists entering upon this question would first look to the premaxillary bones, or, owing to the early confluence of those bones with the maxillaries in the gorilla and chimpanzee, to the part of the upper jaw containing the incisive teeth, on the development of which depends the prognathic or brutish character of a skull. Now the extent of the premaxillaries below the nostril is not only relatively but absolutely less in the gorilla, and consequently the profile of the skull is less convex at this part, or less "prognathic," than in the chimpanzee. Notwithstanding the degree in which the skull of the gorilla surpasses in size that of the chimpanzee, especially when the two are compared on a front view, the breadth of the premaxillaries and of the four incisive teeth is the same in both. In the relative degree, therefore, in which these bones are smaller than in the chimpanzee, the gorilla, in this most important character, comes nearer to man. In the gibbons the incisors are relatively smaller than in the gorilla, but the premaxillaries bear the same proportional size, in the adult male siamang.

Next, as regards the nasal bones. In the chimpanzee, as in the orangs and gibbons, they are as flat to the face as in any of the lower *Simiæ*. In the gorilla, the median coalesced margins of the upper half of the nasal bones are produced forwards; in a slight degree it is true, but affording a most significant evidence of nearer resemblance to man. In the same degree they impress that anthropic feature upon the face of the living gorilla. In some pig-faced baboons there are ridges and prominences in the naso-facial part of the skull; but they do not really affect the question as between the gorilla and chimpanzee. All naturalists know that the semnopithèques of Borneo have long noses; but the proboscidiform appendage which gives so ludicrous a mask to those monkeys is scarcely the homologue of the human nose, and is unaccompanied by any such modification of the nose-bones as gives the true anthropoid character to the human skull, and to which only the gorilla, in the ape tribe, makes any approximation.

No orang, chimpanzee, or gibbon shows any rudiment of mastoid processes; but they are present in the gorilla, smaller indeed than in man, but unmistakeable; they are, as in man, cellular, and with a thin outer plate of bone. This fact led the lecturer to express, when in respect to the gorilla, only the skull had reached him, the following inference, viz.: "from the nearer approach which the gorilla makes to man in comparison with the chimpanzee, or orang, in regard to the mastoid processes, that it assumed more nearly and more habitually the upright attitude than those inferior anthropoid apes do." This inference has been fully borne out by the rest of the skeleton of the gorilla, subsequently acquired.

In the chimpanzee, as in the orangs, gibbons, and inferior *simiæ*, the lower surface of the long tympanic or auditory process is more or less flat and smooth, developing in the chimpanzee only a slight tubercle, anterior to the stylohyal pit. In the gorilla the auditory process is more or less convex below, and developes a ridge, answering to the vaginal process, on the outer side of the carotid canal. The processes posterior and internal to the glenoid articular surface, are better developed, especially the internal one, in the gorilla than in the chimpanzee; the ridge which extends from the ectopterygoid along the inner border of the foramen ovale, terminates in the gorilla by an angle or process answering to that called "styliform" or "spinous" in man, but of which there is no trace in the chimpanzee, orang, or gibbon.

The orbits have a full oval form in the orang; they are almost circular in the chimpanzee and siamang; more nearly circular, and with a more prominent rim in the smaller gibbons; in the gorilla alone do they present the form which used to be deemed peculiar to man. There is not much physiological significance in some of the latter characters; but, on that very account, the lecturer deemed them more instructive and guiding in the actual comparison. The occipital foramen is nearer the back part of the cranium, and its plane is more sloping, less horizontal, in the siamang, than in the chimpanzee and gorilla. Considering the less relative prominence of the fore part of the jaws in the siamang, as compared with the chimpanzee, the occipital character of that gibbon and of other species of *Hylobates* indicates well their inferior position in the quadrumanous scale.

In the greater relative size of the molars, compared with the incisors, the gorilla makes an important closer step towards man than does the chimpanzee. The molar teeth are relatively so small in the siamang, that notwithstanding the small size of the incisors, the proportion of those teeth to the molars is only the same as in the gorilla: in other gibbons (*Hylobates lar*), the four lower incisors occupy an extent equal to that of the first four molars, in the chimpanzee equal to that of the first three molars, in the siamang equal to that of the first two molars and rather more than half of the third, in man equal to the first two molars and half of the third: in this comparison the term molar is applied to the bicuspids.

The proportion of the ascending ramus to the length of the lower jaw tests the relative affinity of the tailless apes to man.

In a profile of the lower jaw, compare the line drawn vertically from the top of the coronoid process to the horizontal length along the alveoli. In man and the gorilla it is about 7-10ths, in the chimpanzee 6-10ths, in the siamang it is only 4-10ths. The siamang further differs in the shape and production of the angle of the jaw, and in the shape of the coronoid process, approaching the lower simiæ in both these characters. In the size of the post-glenoid process, in the shape of the glenoid cavity which is almost flat, in the proportional size of the petrous bone, and in the position of the foramen caroticum, the siamang departs further from the human type and approaches nearer that of the tailed simiæ than the gorilla does, and in a marked degree.

Every legitimate deduction from a comparison of cranial characters makes the tailless *quadrumana* recede from the human type in the following order,—gorilla, chimpanzee, orangs, gibbons; and the last-named in a greater and more decided degree.

Those comparisons have of late been invested with additional interest from the discoveries of remains of quadrumanous species in different members of the tertiary formations.

The first quadrumanous fossil, the discovery of which by Lieuts. Baker and Durand is recorded in the "Journal of the Asiatic Society of Bengal," for November, 1836, has proved to belong, like subsequently discovered quadrumanous fossils in the Sewalik (probably miocene) tertiaries, to the Indian genus *Semnopithecus*. The quadrumanous fossils discovered in 1839, in the eocene deposits of Suffolk, belong to a genus (*Eopithecus*) having its nearest affinities with *Macacus*. The monkey's molar tooth from the pliocene beds of Essex is most closely allied to the *Macacus sinicus*. The remains of the large monkey, 4 feet in height, discovered in 1839 by Dr. Lund in a limestone cavern in Brazil was shown by its molar dentition, $\left(p\,\frac{3-3}{3-3},\ m\,\frac{3-3}{3-3}\right)$ to belong to the platyrrhine family now peculiar to South America. The lower jaw and teeth of the small quadrumane discovered by M. Lartet in a miocene bed of the south of France, and described by him and De Blainville, is so closely allied to the gibbons as to scarcely justify the generic separation which has been made for it under the name *Pliopithecus*.

Finally, a portion of a lower jaw with teeth and the shaft of a humerus of a quadrumanous animal (*Dryopithecus*), equalling the size of those bones in man, have been discovered by M. Fontan, of Saint-Gaudens, in a marly bed of upper miocene age, forming the base of the plateau on which that town is built. The molar teeth present the type of grinding surface of those of the gibbons (*Hylobates*), and as in that genus the second true molar is larger than the first, not of equal size, as in the human subject and chimpanzee. The premolars have a greater antero-posterior extent, relatively, than in the chimpanzee; and in this respect agree more with those in the siamang. The first premolar has the outer cusp raised to double the height of that of the second; its inner lobe appears from M. Lartet's figure to be less developed than in the gorilla, certainly less than in the chimpanzee. The posterior talon of the second premolar is more developed, and consequently the fore and aft extent of the tooth is greater than in the chimpanzee; thereby the second premolar of *Dryopithecus* more resembles that in *Hylobates*, and departs further from the human type.

The canine, judging from the figures published by M. Lartet*, seems to be less developed than in the male chimpanzee, gorilla, or orang. In which character the fossil, if it belonged to a male, makes a nearer approach to the human type; but it is one which many of the inferior monkeys also exhibit, and is by no means to be trusted as significant of true affinity, supposing even the sex of the fossil to be known as being male.

The shaft of the humerus, found with the jaw, is peculiarly rounded, as it is in the gibbons and sloths, and offers none of these angularities and ridges which make the same bone in the chimpanzee and orang come so much nearer in shape to the humerus of the human subject. The fore part of the jaw, as in the siamang, is more nearly vertical than in the gorilla or chimpanzee, but whether the back part of the jaw may not have departed in a greater degree from the human type than the fore part approaches it, as is the case in the siamang, the state of the fossil does not allow of determining. One significant character is, however, present,—the shape of the fore-part of the coronoid process. It is slightly convex forwards, which causes the angle it forms with the alveolar border to be less open. The same character is present in the gibbons. The fore part of the lower half of the coronoid process in man is concave, as it is likewise in the gorilla and chimpanzee. I am acquainted with this interesting fossil, referred to a genus called *Dryopithecus*, only by the figures published in the 43rd volume of the *Comptes Rendus de l'Académie des Sciences*. From these it appears that the canine, two premolars, and first and second true molars are in place. The socket of the third molar is empty, but widely open above; from which I conclude that the third molar had also cut the gum, the crown being completed, but not the fangs. If the last molar had existed as a mere germ, it would have been preserved in the substance of the jaw.

In a young siamang, with the points of the permanent canines just protruding from the socket, the crown of the last molar is complete, and on a level with the base of that of the penultimate molar, whence

* Comptes Rendus de le'Académie des Sciences, Paris, vol. 43.

I infer that the last molar would have cut the gum as soon as, if not before, the crown of the canine had been completely extricated. This dental character, the conformation and relative size of the grinding teeth, especially the fore-and-aft extent of the premolars, all indicate the close affinity of the *Dryopithecus* with the *Pliopithecus* and existing gibbons; and this, the sole legitimate deduction from the maxillary and dental fossils, is corroborated by the fossil humerus, fig. 9, in the above-cited plate.

There is no law of correlation by which, from the portion of jaw with teeth of the *Dryopithecus*, can be deduced the shape of the nasal bones and orbits, the position and plane of the occipital foramen, the presence of mastoid and vaginal processes, or other cranial characters determinative of affinity to man; much less any ground for inferring the proportions of the upper to the lower limbs, of the humerus to the ulna, of the pollex to the manus, or the shape and development of the iliac bones. All those characters which do determine the closer resemblance and affinity of the genus *Troglodytes* to man, and of the genus *Hylobates* to the tailed monkeys, are at present unknown in respect of the *Dryopithecus*. A glance at fig. 5 (*Gorilla*), and fig. 7 (*Dryopithecus*), of the plate of M. Lartet's memoir, would suffice to teach their difference of bulk, the gorilla being fully one-third larger. The statement that the parts of the skeleton of the *Dryopithecus* as yet known, viz., the two branches of the lower jaw and the humerus, "are sufficient to show that in anatomical structure, as well as stature, it came nearer to man than any quadrumanous species, living or fossil, before known to zoologists*," is without the support of any adequate fact, and in contravention of most of those to be deduced from M. Lartet's figures of the fossils. Those parts of the *Dryopithecus* merely show—and the humerus in a striking manner—its nearer approach to the gibbons. The most probable conjecture being that it bore to them, in regard to size, the like relations which Dr. Lund's *Protopithecus* bore to the existing *Mycetes*. Whether, therefore, strata of such high antiquity as the miocene may reveal to us "forms in any degree intermediate between the chimpanzee and man" awaits an answer from discoveries yet to be made; and the anticipation that the fossil world "may hereafter supply new osteological links between man and the highest known quadrumana" may be kept in abeyance until that world has furnished us with the proofs that a species did formerly exist which came as near to man as does the orang, the chimpanzee, or the gorilla.

Of the nature and habits of the last-named species, which really offers the nearest approach to man of any known ape, recent or fossil, the lecturer had received many statements from individuals resident at or visitors to the Gaboon, from which he selected the following as most probable, or least questionable.

Gorilla-land is a richly wooded extent of the western part of Africa, traversed by the rivers Danger and Gaboon, and extending from the

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* Sir Chas. Lyell, Supplement to the Fifth Edition, of Manual of Elementary Geology, 8vo., 1859, p. 14.

equator to the 10th or 15th degree of south latitude. The part where the gorilla has been most frequently met with presents a succession of hill and dale, the heights crowned with lofty trees, the valleys covered by coarse grass, with partial scrub or scattered shrubs. Fruit trees of various kinds abound both on the hills and in the valleys; some that are crude and uncared for by the negroes are sought out and greedily eaten by the gorillas, and as different kinds come to maturity at different seasons, they afford the great denizen of the woods a successive and unfailing supply of these indigenous fruit trees. Professor Owen specified the following:—

The palm-nut (*Elais guiniensis*) of which the gorillas greatly affect the fruit and upper part of the stipe, called the "cabbage." The negroes of the Gaboon have a tradition that their forefathers first learnt to eat the "cabbage," from seeing the gorilla eat it, concluding that what was good for him must be good for man.

The "ginger-bread tree" (*Parinarium excelsum*), which bears a plum-like fruit.

The papau tree (*Carica papaya*).

The banana (*Musa sapientium*), and another species (*Musa paradisaica*).

The *Amomum Afzelii* and *Am. grandiflorum.*

A tree, with a shelled fruit, like a walnut, which the gorilla breaks open with the blow of a stone.

A tree, also botanically unknown, with a fruit like a cherry.

Such fruits and other rich and nutritious productions of the vegetable kingdom, constitute the staple food of the gorilla, as they do of the chimpanzee. The molar teeth, which alone truly indicate the diet of an animal, accord with the statements as to the frugivorous character of the gorilla: but they also sufficiently answer to an omnivorous habit to suggest that the eggs and callow brood of nests discovered in the trees frequented by the gorilla might not be unacceptable.

The gorilla makes a sleeping place like a hammock, connecting the branches of a sheltered and thickly leaved part of a tree by means of the long tough slender stems of parasitic plants, and lining it with the broad dried fronds of palms, or with long grass. This hammock-like abode may be seen at different heights, from 10 feet to 40 feet from the ground, but there is never more than one such nest in a tree.

They avoid the abodes of man, but are most commonly seen in the months of September, October, and November, after the negroes have gathered their outlying rice crops, and have returned from the "bush" to the village. So observed, they are described to be usually in pairs; or, if more, the addition consists of a few young ones, of different ages, and apparently of one family. The gorilla is not gregarious. The parents may be seen sitting on a branch, resting the back against the tree-trunk—the hair being generally rubbed off the back of the old gorilla from that habit—perhaps munching their fruits, whilst the young gorillas are at play, leaping and swinging from branch to branch, with hoots or harsh cries of boisterous mirth.

If the old male be seen alone, or when in quest of food, he is usually armed with a stout stick, which the negroes aver to be the weapon with which he attacks his chief enemy the elephant. Not that the elephant directly or intentionally injures the gorilla, but, deriving its subsistence from the same substances, the ape regards the great proboscidian as a hostile intruder. When therefore he discerns the elephant pulling down and wrenching off the branches of a favourite tree, the gorilla, stealing along the bough, strikes the sensitive proboscis of the elephant with a violent blow of his club, and drives off the startled giant trumpeting shrilly with rage and pain.

In passing from one detached tree to another the gorilla is said to walk semi-erect, with the aid of his club, but with a waddling awkward gait; when without a stick, he has been seen to walk as a biped, with his hands clasped across the back of his head, instinctively so counter-poising its forward projection. If the gorilla be surprised and approached while on the ground, he drops his stick, betakes himself to all-fours, applying the back part of the bent knuckles of his fore-hands to the ground, and makes his way rapidly, with an oblique swinging kind of gallop, to the nearest tree. There he awaits his pursuer, especially if his family be near, and requiring his defence. No negro willingly approaches the tree in which the male gorilla keeps guard. Even with a gun the negro does not make the attack, but reserves his fire in self-defence. The enmity of the gorilla to the whole negro race, male and female, is uniformly testified to. The young men of the Gaboon tribe make armed excursions into the forests, in quest of ivory. The enemy they most dread on these occasions is the gorilla. If they have come unawares too near him with his family, he does not, like the lion, sulkily retreat, but comes rapidly to the attack, swinging down to the lower branches, and clutching at the nearest foe. The hideous aspect of the animal, with his green eyes flashing with rage, is heightened by the skin over the prominent roof of the orbits being drawn rapidly backward and forward, the hair erected, and causing a horrible and fiendish scowl. If fired at and not mortally hit, the gorilla closes at once upon his assailant and inflicts most dangerous, if not deadly, wounds with his sharp and powerful tusks. The commander of a Bristol trader told the lecturer he had seen a negro at the Gaboon frightfully mutilated by the bite of the gorilla, from which he had recovered. Another negro exhibited to the same voyager a gun-barrel bent and partly flattened by the bite of a wounded gorilla, in its death-struggle. Negroes when stealing through the gloomy shades of the tropical forest become sometimes aware of the proximity of one of these frightfully formidable apes by the sudden disappearance of one of their companions, who is hoisted up into the tree, uttering, perhaps, a short choking cry. In a few minutes he falls to the ground a strangled corpse. The gorilla, watching his opportunity, has let down his huge hind-hand, seized the passing negro by the neck, with vice-like grip, has drawn him up to higher branches, and dropped him when his struggles had ceased.

The strength of the gorilla is such as to make him a match for a lion, whose tusks his own almost rival. Over the leopard, invading the lower branches of the gorilla's dwelling tree, he will gain an easier victory; and the huge canines, with which only the male gorilla is furnished, doubtless have been assigned to him for defending his mate and offspring.

The skeleton of the old male gorilla obtained for the British Museum in 1857, shows an extensive fracture, badly united, of the left arm-bone, which has been shortened, and gives evidence of long suffering from abscess and partial exfoliation of bone. The upper canines have been wrenched out or shed, some time before death, for their sockets have become absorbed.

The redeeming quality in this fragmentary history of the gorilla is the male's care of his family, and the female's devotion to her young.

It is reported that a French natural-history collector, accompanying a party of the Gaboon negroes into the gorilla woods, surprised a female with two young ones on a large boabdad (*Adansonia*), which stood some distance from the nearest clump. She descended the tree, with her youngest clinging to her neck, and made off rapidly on all fours to the forest, and escaped. The deserted young one on seeing the approach of the men began to utter piercing cries: the mother, having disposed of her infant in safety, returned to rescue the older offspring, but before she could descend with it her retreat was cut off. Seeing one of the negroes level his musket at her, she, clasping her young with one arm, waved the other, as if deprecating the shot; the ball passed through her heart, and she fell with her young one clinging to her. It was a male, and survived the voyage to Havre, where it died on arriving. Professor Owen had examined the skeleton of this young gorilla in the museum of natural history at Caen, and was indebted to Professor Deslongchamps, Dean of the Faculty of Sciences in that town, for drawings of this rare specimen.

There might be more difficulty in obtaining a young gorilla for exhibition than a young chimpanzee. But as no full-grown chimpanzee has ever been captured, we cannot expect the larger and much more powerful adult gorilla to be ever taken alive.

A bold negro, the leader of an elephant-hunting expedition, being offered a hundred dollars if he would bring back a live gorilla, replied, "If you gave me the weight of yonder hill in gold coins, I could not do it!"

All the terms of the aborigines in reference to the gorilla imply their opinion of his close kinship to themselves. But they have a low opinion of his intelligence. They say that during the rainy season he builds a house without a roof. The natives on their hunting excursions light fires for their comfort and protection by night; when they have gone away, they affirm that the gorilla will come down and warm himself at the smouldering embers, but has not wit enough to throw on more wood, out of the surrounding abundance, to keep the fire burning,—"the stupid old man!"

Every account of the habits of a wild animal obtained at second hand from the reports of aborigines has its proportion of "apocrypha." The lecturer had restricted himself to the statements that had most probability and were in accordance with the ascertained structures and powers of the animal, and would only add the averment and belief of the Gaboon negroes that when a gorilla dies, his fellows cover the corpse with a heap of leaves and loose earth collected and scraped up for the purpose.

A most singular phenomenon in natural history, if one reflects on the relations of things, is this gorilla! Limited as it is in its numbers and geographical range, one discerns that the very peculiar conditions of its existence—abundance of wild fruit—needs must be restricted in space; but, concurring in a certain part of Africa, there lives the creature to enjoy them.

The like conditions exist in Borneo and Sumatra, and there also a correlative human-like ape, of similar stature, tooth-armour, and force, exists at their expense. Neither ourangs nor gorillas, however, minister to man's use directly or indirectly. Were they to become extinct, no sign of the change or break in the links of life would remain. What may be their real significance?

Reverting finally to the ancient notices which might relate to the great anthropoid ape of Africa, Prof. Owen referred to his first Memoir, of February, 1848, in which was quoted (*Trans. Zool. Soc.*, vol. iii. p. 418), Dr. Falconer's *Translation of the Voyage of Hanno*, (London, 1797,) with his Dissertation vindicating the authenticity of the "Periplus." Professor Owen had lately been favoured by the venerable Bishop Maltby, the first amongst our Greek scholars, with the following translation of the passage supposed to allude to the species in question:—"On the third day, having sailed from thence, passing the streams of fire, we came to a bay called the Horn of the South. In the recess there was an island like the first, having a lake, and in this there was another island full of wild men. But much the greater part of them were women, with hairy bodies, whom the interpreters called 'gorillas.' But, pursuing them, we were not able to take the men; they all escaped, being able to climb the precipices, and defended themselves with pieces of rock. But three women (females), who bit and scratched those who led them, were not willing to follow. However, having killed them, we flayed them, and conveyed the skins to Carthage. For we did not sail any further, as provisions began to fail." This encounter indicates, therefore, the southernmost point on the west coast of Africa reached by the Carthaginian navigator.

To the inquiry by Bishop Maltby, how far the newly-discovered great ape of Africa bore upon the question of the authenticity of the Periplus, Prof. Owen had replied:—"The size and form of the great ape, now called 'gorilla,' would suggest to Hanno and his crew no other idea of its nature than that of a kind of human being; but the climbing faculty, the hairy body, and the skinning of the dead specimens, strongly suggest that they were large anthropoid apes. The fact that such apes, having the closest observed resemblance to the negro, being of human stature and with hairy bodies, do still exist on the west coast of Africa, renders it highly probable that such were the creatures which Hanno saw, captured, and called 'Gorullai."

The brief observation made by Battell in west tropical Africa, 1590, recorded in Purchas's "Pilgrimages, or Relations of the World," 1748, of the nature and habits of the large human-like ape which he calls "pongo," more decidedly refers to the gorilla. Other notices, as by Nieremberg and Bosman, applied by Buffon to Battell's pongo, were deemed valueless by Cuvier, who altogether rejected the conclusions of his great predecessor as to the existence of any such ape. "This name of pongo or boggo, given in Africa to the chimpanzee or to the mandril, has been applied," writes Cuvier, "by Buffon to a pretended great species of ourang-utan, which was nothing more than the imaginary product of his combinations." After the publication of Cuvier's "Règne Animal," the supposed species was, by the high authority of its author, banished from natural history; it has only been authentically reintroduced since the intelligent attention of Dr. Savage was directed to the skull which he first saw at the Gaboon in 1847, and took Professor Owen's opinion upon.

[The above lecture was illustrated by life-size drawings of the gorilla and chimpanzee, executed with characteristic accuracy and vigour by Mr. Joseph Wolf; also by diagrams of the skeletons of the anthropoid apes and man, by Messrs. Scharf and Erxleben.]

[R. O.]

GENERAL MONTHLY MEETING,

Monday, February 7, 1859.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

The Earl of Ashburnham,
John Derby Allcroft, Esq.
Capt. Augustus T. Hamilton, and
Mrs. M. A. Newman Smith.

were duly *elected* Members of the Royal Institution.

George F. Chambers, Esq.

was *admitted* a Member of the Royal Institution.

The following Presents were announced, and the thanks of the Members returned for the same:—

From

India Office: Committee of Finance, &c.—Catalogue of Lepidopterous Insects in the Museum of the East India Company. By T. Horsfield and F. Moore. Vol. I. 8vo. 1857.

Actuaries' Institute of—Assurance Magazine, No. 34. 8vo. 1859.
Arts, Society of—Journal for Dec. 1858, and Jan. 1859. 8vo.
Asiatic Society of Bengal—Journal, No. 268. 8vo. 1858.
Astronomical Society, Royal—Monthly Notices, Nov. and Dec. 1858. 8vo.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for Dec. 1858 & Jan. 1859. 8vo.
Bombay Medical Board—Deaths in Bombay, 1857. 8vo. 1858.
Boosey, Messrs. (the Publishers)—The Musical World for Dec. 1858 and Jan. 1859. 4to.
British Architects, Institute of—Proceedings for Dec. 1858 and Jan. 1859. 4to.
Druitt, Dr. R. (the Author)—Reports on the Health of St. George's, Hanover Square. 8vo. 1857-8.
Editors—The Medical Circular for Dec. 1858 and Jan. 1859. 8vo.
The Practical Mechanic's Journal for Dec. 1858 and Jan. 1859. 4to.
The Journal of Gas-Lighting for Dec. 1858 and Jan. 1859. 4to.
The Mechanics' Magazine for Dec. 1858 and Jan. 1859. 4to.
The Athenæum for Dec. 1858 and Jan. 1859. 4to.
The Engineer for Dec. 1858 and Jan. 1859. fol.
The Artizan for Dec. 1858 and Jan. 1859. 4to.
The Atlantis, No. 3.1859. 8vo.
Faraday, Professor, D.C.L. F.R.S. (the Author)—Experimental Researches in Chemistry and Physics. 8vo. 1859.
Répertoire de Chimie, par C. Barreswil et A. Wurtz. Nos. 1-4. 8vo. Paris, 1858-9.
Königliche Preussische Akademie, Berichte, Sept.-Okt. 1858. 8vo. Abhandlungen, 1857. 4to.
Franklin Institute of Pennsylvania—Journal, Vol. XXXVI. No. 6. 8vo. 1858.
Geological Society—Proceedings for Dec. 1858 and Jan. 1859. 8vo.
Linnean Society—Proceedings, No. 11. 8vo. 1859.
Mackie, S. J. Esq. F.G.S. (the Editor)—The Geologist, January 1859.
Mallet, Robert and J. W. Esqs. (the Authors)—The Earthquake Catalogue of the British Association. 8vo. 1858.
On Three Foot Shells. (From Journal of United Service Institution) 8vo. 1858.
Murchison, Sir R. I. V.P.R.S. M.R.I. (the Author)—Siluria. 3rd Edition. 8vo. 1859.
Newton, Messrs.—London Journal (New Series) for Dec. 1858 & Jan. 1859. 8vo.
Novello, Mr. (the Publisher)—The Musical Times for Dec. 1858 & Jan. 1859. 4to.
Nutt, Mr. D.—W. Engelmann's Bibliotheca Scriptorum Classicorum. 8vo. 1858.
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Phillips, R. Esq.—Cataloghi del Museo Campana. 4to. 1858.
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WEEKLY EVENING MEETING,

Friday, February 11, 1859.

William Robert Grove, Esq. M.A. Q.C. F.R.S Vice-President, in the Chair.

Edmund Beckett Denison, Esq. M.A. Q.C. M.R.I.

*On some of the Grounds of Dissatisfaction with Modern Gothic Architecture.**

It is an odd coincidence that the House of Commons has already been engaged this evening in listening to some lectures on the same subject as we are going to consider here. For Lord Palmerston, Sir Benjamin Hall, and a few other gentlemen, have been expressing their dissatisfaction with Gothic architecture, and assuring the House that it is essentially gloomy, awkward, barbarous, expensive, and altogether incapable of meeting the wants of this enlightened age, and the clerks of the Foreign Office. One would almost fancy that we were put back a couple of centuries, and listening again to the kind of description which used to be given of the Gothic style by the leaders of the public taste in the days of the Restoration, when all Gothic art was supposed to be dead and buried far too deep for any chance of revival. This is the character which the once celebrated John Evelyn, who was one of the Commissioners for rebuilding St. Paul's cathedral, gave in his treatise *On Architecture and Architects* to the style of all the great cathedrals of the world except his own and its prototype at Rome. He calls it 'a certain fantastical and licentious manner of 'building, introduced by the Goths and Vandals and other barbarous 'nations, who demolished the glorious Roman empire with its stately

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* I do not see how to comply with the numerous applications I have had for a report of this lecture, except by writing a fuller version of it than the usual abstract; for an abstract of a lecture on a subject of this kind would be almost unreadable, and therefore of no use. As it was not written beforehand, and I do not profess to remember accurately what I said, I can only use the notes from which it was given as an outline to be filled up, as if I were writing the lecture for delivery. So long as it is substantially the same, it cannot signify to anybody who heard—or did not hear it, whether the words are identical or not. In some parts they certainly are not, because I was obliged to hurry over some branches of the subject for fear of exceeding the usual time unreasonably. Of course the Royal Institution is not responsible for my opinions; and it must be remembered that Architecture is in these days an eminently controversial subject.—E. B. D.

'and pompous monuments, and filled not Europe only, but Asia and 'Africa also, with mountains of stone, vast and gigantic buildings 'indeed, but not worthy of the name of architecture, congestions of 'dark, heavy, melancholy, and monkish piles, [Lord Palmerston's view 'of Gothic almost verbatim] without any just proportion, use, or 'beauty, compared with the truly antient; so as, when we meet with 'the greatest industry and expensive carving, full of fret and lament-'able imagery, sparing neither of pains nor cost, a judicious spectator 'is rather distracted or quite confounded than touched with that admi-'ration which results from the true and just sympathy, regular propor-'tions, union, and disposition, and from the great and noble manner in 'which the august and glorious fabrics of the antients were executed;' with more nonsense of the same kind, to which other similar nonsense from other writers of the two last centuries might easily be added.

Besides adopting this enlightened view of the nature of Gothic building, Lord Palmerston has been kind enough to furnish us with a few tolerably conclusive measures of his own architectural taste and knowledge of the business he has been talking about. First of all it appears, that "Pall Mall, with all its magnificent clubs, each one handsomer than the other" (whatever that piece of oratory means) is his ideal of "a street of palaces;" such as he would like to see extended from the end of Whitehall to the precincts of the Abbey. Then, as for the Abbey itself, he has no hesitation in pronouncing that (he does not say whether he means Sir Christopher Wren's monstrifications of it, or the original work—perhaps he would be rather puzzled to distinguish them) quite inferior to St. Paul's. And by way of a light and cheerful model for a set of Government Offices, with no dark passages, and windows adapted for letting in the utmost quantity of light, and a refined and lively style of decoration, Somerset House is the thing; "he will venture to say that is much handsomer than the Houses of Parliament." As there is no resemblance between the style of the Houses of Parliament and any of the prize Gothic plans for the Foreign Office, that comparison, at any rate, is not much to the purpose. Indeed, it is only fair to Sir Charles Barry to remember, that the style prescribed for his building was the latest and worst of all the Gothic styles; and moreover, that it was designed nearly a quarter of a century ago, almost in the infancy of the Gothic revival, when there was scarcely anything designed which its authors would not be ashamed of now. The comparison between the two metropolitan cathedrals, even if Lord Palmerston's opinion of them was generally agreed with, is of a still less practical character, inasmuch as the architects are neither of them likely to be candidates for employment on the New Offices. If they were, I am inclined to think that very few people, except Lord Palmerston, Mr. Coningham, and Mr. Tite, would hesitate about calling up the unknown builders of the Abbey, or any of our old cathedrals, in preference to the architect of St. Paul's, Temple Bar, and St. James's, in Piccadilly, or even to Sir William Chambers, of Somerset House.

The two architectural potentates of the late Government seem to have agreed to compose a little difference which they left unsettled a year ago about this Public Office business, over the dead body of Mr. Scott and Gothic architecture. After some vain struggles by his late colleagues to deny that they did leave the matter "in a dead lock," Sir B. Hall resisting that pleasant job of his superiors for setting aside all the Westminster Hall prize-men together in favour of a gentleman who was assumed to be architect to the Government by divine right or special appointment, Lord Palmerston boldly took the bull by the horns, pushed his defenders aside, and declared that it was all perfectly true, that he did want to employ Mr. Pennethorne, and that he did so "because he thought his plan the best adapted to the requirements of the case; and that he still retained that opinion." Well, so much at any rate for the pretence of objecting to the choice of an architect or a design because it did not get the first prize—or rather the second; for it is another peculiarity of this case, that it is not the first prizeman after all, but one of the two seconds, whom these warm supporters of the Westminster Hall judges now select to fight their anti-Gothic battle with; knowing very well that the first will not do, in spite of Lord Palmerston's facetiousness about the unsportsmanlike notion of preferring a horse which is all but first in every race to that which wins in one. Without pretending to know more about racing than he does of architecture, I humbly venture to doubt, on mere principles of common sense, whether the experts in that line will agree with him. The first prizeman ran for £800, and got it; if you want to see how, you can read the evidence about the proceedings of the judges: it does not follow that we are to presume him capable of all other victories, when there were other races to be run, for which he did not start. And the case of the second classic prizeman is still worse, because he did run in the other race, and in the opinion of all the judges was "nowhere."

As for Lord Palmerston's preference for Mr. Pennethorne's plan, over all the prize ones, as a matter of taste, why, that is hardly a thing to argue about; it is one of those measures of his taste which I said he had been kind enough to give us. That design was exhibited in the committee room last year; and those who saw it and sat there for the very purpose of examining it do not appear to have been very much of Lord Palmerston's opinion about either its beauty or fitness. But setting aside the question of taste, there is a small matter of fact connected with it, of which he might as well have been informed before he assured the House of Commons that it was quite a mistake to suppose there was the least inconsistency between himself and his right honourable friend, the late Commissioner of Public Works. That fact is, that the said Commissioner had specially and very effectively devoted himself in the Committee to the blowing to pieces of this identical plan of Lord Palmerston's architect, by showing that it was particularly *ill* "adapted to the requirements of the case," and did not and could not comply with the prescribed conditions; with which it is admitted that the Gothic plan chosen by the present Government did comply entirely.

There is another simply practical matter, affecting not only this, but all other questions of public buildings in the Gothic style, of which, therefore, I say a few words. Mr. Tite, of the Royal Exchange, and some other classical architecture gentlemen, still go on asserting, as if it were an undoubted fact, that Gothic architecture is necessarily dark, expensive, inconvenient, and difficult to ventilate; that sash windows are better than Gothic windows, and so forth People who hear all this, and know that Mr. Tite was an active member of the Committee which sat and reported last year in favour of selecting the architect from among the prizemen, but distinctly repudiated the doctrine that either the first or the second should be taken unless the Government thought fit, of course have no idea that Mr. Tite heard several architects, who were themselves not Gothic men, admit before the Committee, that the Gothic style is *not* inferior to the other in any one of those respects, and also heard Mr. Scott explain that his windows *are* sash windows, and that they are not smaller, but larger, than those of the very buildings in the Italian style which had been referred to as models, and than the windows in the rival plans. It may be natural for Lord Palmerston and Mr. Coningham to talk in that way about Gothic darkness; but Mr. Tite is an architect, and therefore cannot possibly be ignorant that there are numbers of Gothic buildings in which the windows are even wider than the wall between them, and that the same is the case in no other style that ever existed.

However, I cannot afford more time to this preliminary topic, which having casually presented itself, you could hardly expect me to pass it without notice. I had intended otherwise to decline altogether discussing the relative merits of Gothic and Classical, or Renaissance architecture; first, because the subject is thoroughly exhausted in argument, until some new facts accrue to found new arguments upon; and secondly, because I have not much belief in anybody's opinion on a question of taste being altered by argument. The fact is, that opinions on artistic questions are hardly ever founded on the reasons which are given for them, but the opinions are formed first, and the reasons are invented afterwards to justify or enforce them. Accordingly, that which passes for logic in matters of art, however well executed it may be in point of style, is generally constructed out of definitions and axioms carefully framed to bear out some foregone conclusion—of analogies from nature, which the next writer cites for just the opposite purpose—of nicknames, either of praise or blame, which assume what is pretended to be proved, and all washed down by copious draughts of assertion that all this is as true and certain as that "iron has an affinity for oxygen." In order to avoid exposing myself to the same censure, I do not mean to "prove" anything that I am going to say, or to give any reasons for it, except that I have arrived at certain conclusions or opinions from experience and observation, which you can verify for yourselves.

I do not mean even to claim the merit of novelty for them. Most of them have been in substance published before, both by myself and by others. But what I have to say about them is, that within the last few months two buildings have been completed, in which these conclusions, or theories, or whatever you please to call them, have been enforced in practice for the first time, I believe, since the fall of Gothic architecture 400 years ago. Those buildings, as it happens, are both in the same town, and the same architect, Mr. Scott, was employed for both, and over both of them it has fallen to my lot, from circumstances of no interest to you, to exercise an unusual amount of superintendence, and indeed over the smaller and later of the two, something more than superintendence. The larger one, the rebuilt parish church of Doncaster, is no doubt known, by reputation at least, to all of you. But I dare say you are not aware that another new church in that town was opened at the same time, viz., the one called St. James's, built for the people employed on the works of the Great Northern Railway Company, at Doncaster, and their families, amounting to 3000 at least; which church you will see on the right hand, just before you enter the station from the south. The best view of the great church, of St. George, is also from the railway, a little north of the station. I believe it is no exaggeration to say, that that church is universally considered the finest Gothic building of modern times; and as far as its pretensions go, the other is pronounced by most people who have seen it, to be fully equal, and in some respects superior, even to its more magnificent neighbour. Nobody is more sensible than I am of the faults in each of them, notwithstanding their general success; but there is no building of any age without some faults quite as striking; and whether that be so or not, the conclusions which I have arrived at respecting the common causes of dissatisfaction with modern Gothic building are founded no less upon the defects than upon the beauties which are apparent in these two buildings.

The first cause which I shall notice is one which few persons can have failed to hear alleged as an excuse for the inferiority of modern to antient architecture in every style; and that cause is stinginess. But with the exception of a certain class of buildings which I will advert to presently, I believe it would be nearer the truth to say that so far from stinginess accounting for the general failure of modern architecture, there never was a time in the world when money was spent on building in such profusion. You cannot walk far through the good streets of London, Liverpool, Edinburgh, Glasgow, and probably other towns, without seeing buildings which have cost money enough to build York Minster; in some cases far more. Look at the few hundred yards of Lord Palmerston's street of palaces, and think what you would have had from a builder of the 13th century for the cost of them. Let any one reckon up the cost of such of the great country or London mansions as he can think of, built in our time, and then say whether the architects of the 19th century can lay any of the faults of modern architecture, in the popular style at least, to the account of the stinginess of their employers. In the Gothic style there is certainly more ground for the complaint; but even there it must be confined to that class of buildings which I alluded to just now, and which may briefly be described by a term borrowed from an enemy of Gothic, as *eleemosynary* buildings, such as churches built by subscription, schools, parsonages, almshouses, workhouses, and other things of that kind.

You observe that I say "churches built by subscription;" because, with a few noble exceptions, such as will readily occur to all of us, there is a striking and surprising difference between the conduct of rich men in building churches which they regard as their own, and in building or helping to build those which they do not so regard. I do not know that I can explain that difference better than by a single example, out of many similar ones, which could soon be put together. Not long ago, when a subscription was got up for rebuilding a large and handsome church, which was intended to cost, and has cost, about £30 a sitting, a certain rich man, who lives near it, said openly that he should give as little as he decently could, because he thought it was a great mistake to spend so much money on it. Well, of course, he had a right to please himself; but he was also pleased to furnish a rather odd commentary on his own, and I am afraid, the popular doctrine about expensive church-building; for within a year afterwards he began to build a small church in a secluded hamlet, not even within sight from his house, which has another church close to it, and he spent upon it very nearly £60 a sitting, and more money on the whole than has been spent on St. James's Church, at Doncaster, which holds about six times the congregation, and has cost only £8 a sitting, including all the incidental expenses; or speaking roundly, about £1 a foot of internal area; which I am convinced is the least that any church of ordinary size can be built for, if it is to have any real architectural character, except under some unusually favourable circumstances.

But these specimens of real extravagance, in what may be called private church-building, are almost always on a small scale; and therefore the architects are right in saying that they very seldom indeed have a chance of doing any great ecclesiastical work. Moreover, the prejudice against Gothic among the class of persons who spend their own, or other people's money upon great civil buildings not "eleemosynary," has hitherto confined Gothic works of this class to a very small number; and those are almost entirely private houses, in which there is less opportunity for display than in public buildings with large halls and other grand features belonging to them.* Consequently, it is true that much of the difficulty of producing good Gothic architecture,

* I am aware that a great authority in the religious world has gone a little beyond Lord Palmerston and Mr. Tite, and has ascertained that "Gothic architecture was invented by the Devil," and accordingly proscribed it for his Tabernacle competition. He does not reveal which the angelic style is; but it can hardly be that of St. Paul's Cathedral: for it is remarkable that while the nave of Westminster Abbey, and several other Gothic churches, such as Boston and Coventry, hold from 2000 to 3000 people on the floor, who can all hear any preacher with a good voice, it was necessary to deface St. Paul's with that frightful hoarding to enable the new congregation to hear at all, and that very imperfectly.

in the only line in which it has been allowed to expatiate, is due to what is commonly called stinginess, but should rather be called the perverted and selfish extravagance of modern times; and extravagance which almost invariably fails in its object as it deserves. The £5000 or £6000 for which a man might have been celebrated as the builder of such a tower as this of Doncaster, as the restorer of the splendid choir of Selby, unrivalled among the parish churches of England, or the rebuilder of the half-ruined but once beautiful steeple of Redcliffe, the only one of its style remaining, or which would have redeemed Doncaster Church from its only great defect, the want of length in the nave and transepts, is little better than thrown away in erecting some gaudy little structure in a country village or a park, which all the neighbourhood goes to look at once, and pronounces very pretty, and nobody ever wants to see again; although the rude and simple Early English chapel at Skelton, and the still ruder Norman arches of the humble church of Adel, and many others in all the old Gothic styles, are still visited, and painted, and engraved, and pretended to be copied, while all this modern finery produces no more lasting effect on those who see it, than the painting and gilding and upholstery in the drawing-room of the neighbouring hall.

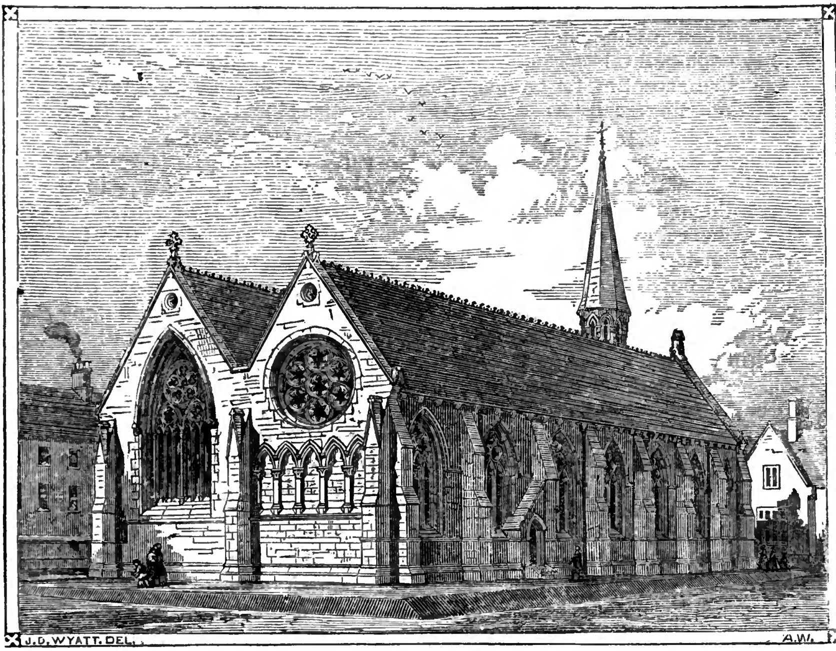
But here I am afraid the concession to the architects, that the failure of modern architecture is due either to the stinginess, or to the misplaced extravagance of their employers, must stop. For, putting aside the numerous buildings of the eleemosynary class which are evidently starved, can it be said that the rest are satisfactory? Are they even very distinctly the best? Nay, are the most expensive parts of the same building certain to be superior to the rest? I must say, that according to my observation and experience, none of these things are so. Of course, such a proposition is not to be considered proved either way by two or three examples; but the north side of the great Doncaster Church is unquestionably better than the south, although the south side is far more elaborately designed, and more costly than the other. Again, St. James's Church is a good deal plainer still, and its walls and windows are only a few feet higher than those of the aisles of St. George's; and yet I believe there are few persons who do not think the plainer ones the best: not because they are plainer, but because, in spite of their plainness, they have more of certain qualities which the old Gothic buildings generally possessed, and which new ones hardly ever have. No doubt in the best Gothic times it was the case that the more elaborate a building was, the handsomer it generally was; and so it ought to be in all times. But so far is it from being so now, that I should almost be inclined to say that if you wish to make quite certain of a failure in the most essential elements of good building, you had better give out at once that you do not care how much you spend. No doubt that looks like a paradox, and it is not a complimentary one to the present race of architects; nevertheless, I adopt it, not from theory, but from observation, and I believe that if you will use your own observation you will come to much the same conclusion.

If it is necessary to account for such a phenomenon, I should attribute it mainly to the existence of one of the most mischievous delusions ever propagated on this subject—that "*ornamentation is the principal part of architecture.*" Of course its author, or rather its most distinguished advocate (for it had been secretly or openly believed in long before his time) demonstrates its truth with the usual imposing array of axioms and definitions in the most logical and precise form; and I am far from intending to break in upon that splendid fabric of reasoning. I will only say of it, that I prefer, as a matter of convenience and simplicity, if not for any other reason, the natural to the non-natural sense of words generally understood; and therefore I decline accepting for 'a definition of architecture,' that it is the art 'of designing sculpture for a particular place,' and placing it there on the 'best principles of building.' Everybody knows that the thing which mankind have agreed to call Architecture, is nothing of the kind, and that definitions of that sort, and all that is built upon them, are a mere rhetorical artifice. And what is of still more consequence, everybody who chooses to use his eyes can see that the great majority of the old buildings, whose grandeur and beauty nobody but Lord Palmerston and his allies now disputes, owe very little of their architectural effect to sculpture, or to anything which would be generally understood by the word ornamentation; and in this country at any rate, not one to colouring, which is the only alternative to sculpture allowed by the authority in question. Mind, I am not saying anything so absurd as that architecture is not improved by suitable decoration; but I say that, as a bare matter of fact, a great deal of what is universally admired as architecture of a very high order, much finer than the most highly decorated modern building, is almost or altogether independent of both carving and colouring.

The contrary theory has led architects and their employers too often to sacrifice everything to ornamentation. That critical and exact eye for beautiful forms and proportions, definable by no rules, and capable of infinite variety, as nature itself shews, which appears to have been possessed by the builders of the 13th century, and by those of this country more than any, but now by hardly the best architects, seems in a fair way for being put out and lost altogether by this vulgar and foolish passion for ornamentation; the same, I suppose, which has come to treat the female form as nothing better than a frame, or rather as the prop for a frame, to hang out heaps of clothes upon. Look at any of the most extravagant specimens of modern churchbuilding, and consider them apart from their alabaster, and carving, and mosaics, and frescoes; if their constructional elements and proportions do not strike you as thoroughly base, feeble, and contemptible in comparison with those of five or six centuries ago, all I can say is, that I advise you to study the old examples till they do.

The day before we laid the first stone of St. James's Church at Doncaster I had to address a meeting of the Yorkshire and Lincolnshire architectural Societies in and upon the then unfinished church of

St. George*, and I then announced that St. James's Church had been designed expressly to expose and refute this ornamentation theory, as well as some other modern notions which I shall speak of presently. At that time the result of the experiment was only conjectural, and I daresay many of the audience thought it a very foolish one; though even then it had been observed that some of the plainest parts of the church we were in were superior to some of the most highly decorated.



ST. JAMES'S CHURCH, DONCASTER. NORTH-EAST VIEW.

Now, however, the result is before you, so far as this large painting can exhibit it;† and if you will take the trouble to stop at Doncaster

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* See *Two Lectures delivered at Doncaster on the 23rd of September,* 1857, *by G. G. Scott and E. B. Denison.*—Brooke and Co., Doncaster; Bell and Daldy. Price 6*d*.

† Two pictures of these churches, on a very large scale, were drawn for the lecture by the Rev. James Bell, who was curate of Doncaster when the parish church was burnt down, and who painted at the time a remarkable picture of the church on fire, since lithographed in colour in the Rev J. E. Jackson's *History of St. George's Church at Doncaster,* with various other illustrations of the old and new churches. Messrs. Brooke and Co. have lent me the accompanying woodcut of St. James's Church. Unfortunately there is not one in existence of St. George's, with the tower as it is now built: in other respects the picture of it in my *Lectures on Churchbuilding* is correct.

for the interval between two trains, and look for yourselves, you will be in a better condition to judge how far Mr. Ruskin and the modern taste are right in pronouncing ornamentation to be the principal part of architecture, than by reading a dozen books or listening to anything more that I could say about it. Of that we had a curious proof when the church was opened last October. A friend of mine, who is, or was, a firm believer in Mr. Ruskin and the essentiality of ornamentation to architectural effect, and an advocate of the height which is characteristic of foreign Gothic, against the old English characteristic of length, and had for these reasons pronounced the success of this church impossible from the drawings of it, nevertheless entirely gave in on seeing it, and admitted that it was in some respects superior to the much grander church not far off; and at that time, it may be worth while to add, that even the very small quantity of ornamentation that there is, only £57 worth of carving, and four marble shafts in the pulpit, was not done. So much for Mr. Ruskin's eloquent demonstration of the ornamentation theory, and his solemn and emphatic deduction from it: "Therefore, observe, no man can be an architect who is not either a sculptor or a painter." The moment it is tested by a building which has some real architectural character of its own to depend on, and openly defies that theory, instead of attempting as usual a sneaking compromise with it, the theory "bursts" like the Trolls in the Norse legends when the sunlight falls upon them.

I do not know whether it is owing to the latent or avowed belief in this theory that decoration should be aimed at first and principally, before instead of after all the other conditions of grand building are satisfied, or to pure and simple ignorance and incapacity for observation, that there are hardly any modern buildings which are not deficient in two primary elements of all grand building in any style whatever: I mean massiveness, and depth of shadow, which is the visible evidence of massiveness, besides having a beauty of its own. So little indeed do the Gothic builders of the 19th century (and I mean not merely architects, but many other persons as well) seem to know or believe that it has anything to do with Gothic effect, that we constantly hear and see that architecture exalted for its *lightness* over the more clumsy and unscientific heaviness of the large stone styles of the Egyptians, Greeks, and Romans. In this there is no doubt a kind of truth, though the classical architecture gentlemen in Parliament seem to know about as much of one side of the truth as the other, and actually imagine that Gothic is the dark and heavy style, and the classical style light in both meanings of the word. But though the area of the supports in St. Paul's or St. Peter's is nearly twice as great in proportion to their size as in most of the Gothic minsters, or in other words, the walls and pillars are very much heavier, yet it does not follow that if the walls and buttresses and pillars of York or Westminster were thinner still, the buildings would be grander, or anything like so grand. In this, as in many other things, we seem incapable of getting hold of an idea without riding it to death. Thus Rickman remarked that the general characteristic of

Gothic was verticality, and of Grecian building, horizontality; which is true enough in a general way, but is forthwith converted into nonsense and monstrosity by people jumping to the conclusion that the *more* vertical and "aspiring" (as they like to call it) they can have their buildings the more Gothic they must be. It is always necessary to receive those neat and succinct definitions and comprehensive statements about anything with extreme caution. Even when they are exact enough for a general summary, or to describe some wide distinction, as in the case just mentioned, they are utterly unsafe to adopt as rules for criticism, and much less for action. Therefore I shall not attempt to lay down any rules for the proper thickness of walls, or window-tracery and mullions. You will find some thicknesses of certain old and new buildings, including the Doncaster churches, in the Lecture before referred to, and others in the second edition of my *Lectures on Churchbuilding.** I will only mention this fact, which is not given in either of them, that the sectional area of all the mullions in those two churches, except a few in St. George's, which everybody can now see to be too thin, is more than double of some which I measured myself in a large church in London, which is by no means an unfair specimen of a rather ambitious building of very recent erection.

The same fault is generally noticeable in the woodwork as in the stone of modern buildings; frequently, and with some architects invariably, it is even worse. The clerk of the works told me that an architect looking at the roof of St. James's said it contained just three times as much timber as he should have put in; no doubt meaning it as a severe censure on our extravagance or ignorance of mechanics: not that modern builders have much to boast of on the score of mechanics; for a year seldom goes by without two or three new buildings either falling down dead, or having their roofs taken off to anticipate them. I think these gentlemen may take for granted that the old builders, who knew how to build stone roofs and wooden roofs which are stronger now after five centuries than many that were built last year, knew very well that they were using more materials than were requisite for merely satisfying the mechanical conditions. But they knew also that engineering is not architecture, and that science is not art, though it is involved in it, at least in the art of Gothic building, the architecture of pointed arches, and vaults, and buttresses, of open roofs, and tall spires; and why should we not add of domes,—not the low and clumsy Roman or Byzantine dome, incapable of standing on its own base, but the truly scientific and beautiful Indian or Saracenic pointed dome, chiefly built I suppose in horizontal courses, and therefore capable of standing alone, and presenting an outline like a tall Gothic arch standing on its piers, and with a more than Gothic independence of abutment beyond them? †

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* Also published by Bell and Daldy, Fleet Street, and Brooke and Co., Doncaster.

† The Gothic effect of some of these high-domed buildings, including even the square parts below, in some lately published photographic views of Cairo, is very

Mr. Ruskin is manifestly right in describing Redundance, or visible superfluity of materials or dimensions, as an essential element of Gothic effect, and probably of all effect in architecture. Redundance in ornamentation we are only too capable of appreciating, as I have said already; and on redundance in height the architects seem disposed to run equally wild; but redundance in mass, and in length, and in depth of external shadows, all of them eminently characteristic of the best English Gothic, they choose steadily to ignore. And as long as they do, and as long also as people will require architects to erect public buildings, ecclesiastical or civil, of such capacity and for such a price that they cannot afford to put in materials enough for architectural effect, even if they wished, so long is there no hope for any real revival of architecture in any style whatever; unless indeed the iron and glass style is to be dignified with the name of architecture; as I should think it would be by the gentlemen in the House of Commons who keep crying out for more light, when they have learnt, as they perhaps may some day, that the style which they are advocating now is one of the darkest in the world, and was intentionally made so for good reasons in its own country, while the people of the south wondered at the largeness of our Gothic windows here.

The professional architects are naturally very angry at the severe things which are sometimes written of them by the amateurs (on whose behalf I shall have something more to say before I have done); but none of us have said anything so severe as what I am now going to quote from a recent book written by one of themselves. Speaking of these two cognate faults of modern architecture, thinness and want of shadow, Mr. Garbett says:—

> "The want of thickness in the walls, and recess in the openings, renders the whole of the architectural ornament applied to many of our public buildings worse than thrown away, since it makes them more ridiculous as architectural façades than they would otherwise have been as brick walls. The draughtsmen of competition drawings are well aware of this source of effect [depth of windows], and committees should be on their guard against it; for many of the disappointments experienced when these pretty designs have been executed may be traced back to the *direct falsehood* of representing their walls twice or thrice as thick as they were intended to be. By a most unfortunate seeming fatality, the great national work of the age, which must have such a powerful influence on its taste, has been doomed to afford an instance of this disappointment. In the original prspective views of its famous river front, the windows were recessed at least three feet from the plane of the wall; but as executed they do not seem to be one foot therefrom: that is to say (taking the front as 800 ft. by 70 in round numbers) the glass has been so advanced as to rob the exterior of 112,000 cubic feet of apparent solidity. This single circumstance would be sufficient by itself, if not counteracted [which it is not], to make all the difference between a sublime building and a mean one; but among many complaints at imaginary grievances, no critic raises the cry of 'Give us back our 112,000 cubic feet.'"—*Rudimentary Treatise on Design*, p. 103.

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striking. I have no doubt that something truly grand, Gothic, and yet novel, might be done in that line by an architect of genius. But the domes must be large: I believe a small dome, standing by itself at least, is an inevitably poor thing.

I have reason to believe that the great Doncaster church is the first modern building in which the windows have been set as far back as half the thickness of the wall, or from 18 inches in the thinnest walls to 2½ feet in the thickest. Those in St. James's Church and in the tower of St. George's are deeper still; for the upper windows of the tower were altered to 3 feet deep, and all the windows in St. James's are set nearer to the inside than the outside of the walls, so that none are less than 20 inches deep; and the east window, though it is of course smaller than St. George's, has the same depth, 2 ft. 6 in. outside. I promised not to prove anything this evening, and therefore I abstain from giving the reasons why external depth of windows must be of far more architectural value than internal, especially as I have done so at some length elsewhere,* and exposed the common excuses of the architects for the contrary practice. I shall only say here, as a matter of fact, that everybody, so far as I can learn, admires St. George's Church for its superiority to other modern buildings in this respect, and the St. James's windows for their superiority to many in St. George's, both in depth of setting and in massiveness, though they are much plainer in the mouldings and in the general design. Mr. Scott was afraid that people would be disappointed at finding the depth of these windows inside no greater than it usually is in walls of the common thickness, after seeing that it is just a foot more outside, and that it would be considered a kind of cheat. For anything I know, the architect of the 19th century, who always puts his windows at least twice as near the outside as the inside of the wall, may think it his duty to be disappointed; but nobody else is: on the contrary, I have heard more admiration generally expressed after people have seen the inside of the church than before, even when their attention has been directed to this peculiarity of the windows—for modern ones; which is only one amongst the many proofs we have had there, that as soon as the right thing is done, it is generally acknowledged to be right, however little the necessity for it may have been appreciated beforehand.

But you must not imagine that it is only in the windows that modern Gothic architecture suffers from want of visible depth. It is equally true of doorways, buttresses, corbels, arch-mouldings, cornices, eaves of roofs; everywhere, in short, where it is possible to give the building a sort of meagre, shaven, eyebrowless look, precisely the opposite of that which is eminently characteristic of all the buildings of the three great Gothic centuries, and hardly perished even in the fourth. The principal expense we were put to in altering work already done in both churches at Doncaster arose from the necessity for correcting (as far as could be done) the stupid and perverse shaving down and cropping of projections and reducing of depths by the clerks who made the working drawings of things which had actually been right in the original pictures: just what you see Mr. Garbett complains of at Westminster

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* See pp. 79, &c., of the 2nd edition of *Lectures on Churchbuilding*, and p. 33 of the other lectures at Doncaster.

and elsewhere as the common fallacy of drawings. And although the want of depth in the buttresses at St. George's was painfully apparent at an early stage—but too late to be cured, except at great expense, those of St. James's were only just saved in time, after the foundation had been laid; and there the consequence of want of depth would have been worse, because it was on depth, and massiveness, and general boldness of construction, that the architectural effect of that church, as of all plain ones, had to depend entirely. Mr. Bell has been kind enough to paint for the purpose of this lecture what we may call a bad St. James's; which, you see, is as like the good one as possible in general dimensions, size of windows, the arcading under the round window, and so forth, except that the details are made as thin and shallow as usual in modern Gothic buildings.* As far as pictures can exhibit the difference of effect, and they ought to be stereographic pictures to do it completely, you can now form some idea of it; you will form a better if you will compare the building itself with almost any other modern church which you can carry in your eye for the purpose, or if you compare any other modern building with almost any old one of the same size. I say *almost* any old one, because I am well aware that there are a few exceptions, such as the north aisle windows of Bolton Abbey, which are in the flowing decorated style, and are nearly as bad as modern ones, and with which the architects will probably knock you down, unless you are prepared to ask them why they insist upon copying the few exceptionally bad models they can find, and pay no attenton to the infinitely greater number of good ones of just the opposite character. And so I leave this topic, adding only the warning (which may not be thrown away upon some people), that there is no relation between external depth of shadow and internal light: on the contrary, since Gothic windows are nearly always splayed as wide as the depth of their setting, and Italian ones never are, deep-set Gothic windows will let more light into a room than Italian ones of the same size.

The next common modern mistake which I have to notice, is another instance of a truth turned into falsehood by exaggerations. You have all heard or seen how St. Peter's at Rome fails, from the magnitude of all its parts and details, in producing the proper impression of its size, and you will see in Mr. Fergusson's and Mr. Garbett's books some strong observations on and illustrations of the same want of subdivision of even the Gothic cathedrals on the Continent, into parts of moderate size with a distinct outline to each—an art which was far better understood by the English architects, who knew how to give to our cathedrals an appearance of almost unlimited extent, though they are generally smaller than the foreign ones. Well, all this is perfectly true and right, and it is undoubtedly an architectural blunder to have great size and yet not to get the full effect out of it. But the modern architect interprets this truth to mean, that if you only make

* This cannot be represented in a small drawing; so I do not attempt it.

plenty of distinct parts in a building or a window, of any size, and put plenty of stages in a buttress, or mouldings in an arch, the eye will be cheated into believing that the building is large, the window a grand one, the buttress high and deep, and the wall thick: precisely the reverse of all which is the fact. The eye is not such a fool: though its owner may not be wise enough to know how he has discovered the cheat, yet he always does discover it—and more; for by a strange but just retribution, buildings designed on this fraudulent principle generally look even smaller than they are; and for this reason: the eye soon perceives that everything within its power of measuring is small and paltry, and therefore the mind instinctively concludes that the whole is, whether it is in fact or not. Of the two mistakes, the old foreign one is immeasurably the best; inasmuch as it is far better that you should find a building to be greater than you thought at first, instead of gradually discovering that the more you see of it the more paltry and mean and good for nothing it is: not that buildings are the only things of which this is true.

Some persons think, that in the desire to avoid this common fault in St. James's Church at Doncaster, we have run a little into the opposite error of St. Peter's at Rome; and it certainly is true that its size is not always appreciated at first. But then it is generally seen after the much larger church near it, and is talked of as the small one; and therefore people are not prepared for an unbroken internal length such as you find in hardly any of the largest London churches. In fact, I doubt if there is any other modern Gothic church with even one continuous roof of 116 feet, much less two; though there are plenty of old ones longer. Moreover, you must not suppose that I set up this church as the best that could be designed; but the best that we were able to do for the specific sum of money which was allowed for it; and it was particularly intended to prove that a church of that size, truly Gothic in construction, and most convenient in arrangement, with nothing mean, or unreal, or pretentious* about it, could be built for a very moderate sum of money, on principles exactly opposite to those which are commonly adopted. In that we have certainly succeeded, for it seems to be generally considered the handsomest building for its cost and size that has been erected in modern times. St. George's Church also, considering its size, is remarkable for the simplicity of its design, and the due magnitude of all its parts; except that in a few of the windows the work is somewhat too minute; and their comparative feebleness entirely proves what I said just now, viz., that you do not gain but lose in apparent magnitude by subdivision, as soon as the parts become too small in themselves; and therefore all such work as that is worse than thrown away.

I have already alluded to the modern passion for the foreign characteristic of height, and the disposition to sacrifice to it the more

* Mr. Garbett justly says, that what it is the fashion to designate "an unpretending structure," generally means one which is only pretence all over.

English and certainly more useful one of length. I should say of height, as of ornamentation, of course it is a valuable element of architectural effect, but it ought to come last, not first; and I will add, as my own opinion, that it ought very seldom indeed to be the leading characteristic of a large building; for when it is, it is extremely likely to destroy that character which everybody understands by the term Repose, and for which our own long cathedrals seem distinguished above all others in the world. Some of them are undoubtedly too low; but too low for their breadth, not their length; and with our usual discrimination and good taste, breadth is the dimension into which we almost invariably expand now when size is wanted; and thus the length is doubly overpowered. I am sorry to have to say that it is plain to my mind, that this consequence of raising the roof of this St. George's Church 20 feet (in itself a vast improvement) besides the widening the nave (which was necessary) was overlooked in fixing the length; and that the ground plans of some other churches, which were produced to prove that the present proportions would turn out right, proved nothing of the kind; because they are none of them so high. If you look at any good pictures of the old and new churches together, with the information that the clearstory walls are of just the same height in both, and therefore by no means of that exaggerated proportion which is affected in Gordon Square and Margaret Street, you would never imagine that the new church is 16 feet longer inside than the old one, and still more outside. The old church looked a long one, and yet did not look too low; the new one, everybody observes, is too short for its other dimensions; and there is scarcely a modern Gothic church anywhere, which is not, especially if height has been aimed at.

A still worse fault of the same family is the passion for building high spires on bases about half as wide as the old ones. This vile practice seems to me to concentrate all the bad taste, and pretension, and ignorance of proportion, which will be recorded some day as the characteristic of this age. The architects profess to be reviving the architecture of the 13th or 14th centuries—"taking that as their starting point" for further development; and yet they seem to have discovered that the builders of Salisbury, and Norwich, and Grantham, and Coventry, and Louth, and Newark, and Lincoln, and Ely, and old Doncaster, and all the towers of the west, knew nothing of their business, and of the proper width for a tower in proportion to its height. To be sure, we need hardly speak of towers, because now-a-days nobody is satisfied without a spire, and the spire must be higher than some other in the neighbourhood; and as for its width, it may be anything the architect likes: probably they think (if they think about it at all) the narrower it is the taller it will look, and certainly the cheaper it will be. And so has grown up the plan of putting the steeple over an aisle, instead of the nave, or making a narrow transept on purpose for it, as in the rebuilt parish church at Leeds, or putting it anywhere else where it can escape the necessity for being as wide as the nave, as the old towers almost invariably are where they aim at any considerable height, even if they stand at the corner, like that of Redcliffe, or St. John's at Chester, or on the transept, as at Fountain's Abbey. In short, though tower-building was always the glory of architecture in all ages and styles when towers grew at all, this century has hardly produced a single tower or spire whose proportions would not be laughed to scorn by any of the old steeple-builders, except the towers of the Houses of Parliament and of Doncaster church; the latter of which is 34 feet square where it rises from the roofs, and 170 feet high; and the Victoria tower is the largest in the world, and as it happens, exactly double the height of Doncaster; the great arches however are only about a yard higher than our central ones.

There is only one more physical cause of the general failure of modern Gothic architecture which I mean to speak of. And on this one I am glad to say that I can agree with Mr. Ruskin as thoroughly as I differ from him about some others, especially his ornamentation theories. The thing I refer to now however is no theory, but a simple matter of observation so plain and obvious that there is very little to say about it, except to desire people to pay no attention to theories of perfect building, but to use their eyes; which, however, is generally the last thing we learn to do. Now everybody who does condescend to learn in that way, will see, the first time he really looks at any old Gothic building in good preservation, and at any new one, that the old one is, as a builder would say, shockingly inferior in the execution of the work. He will see no kind of precision about the joints anywhere; no edge, or *arris* (as the builders call it), is either quite a straight line if it professes to be generally straight, or quite a regular curve if it is curved. Ornaments of repetition, such as the Norman zigzags or others of that kind, and the early English tooth moulding (which is really four narrow leaves set in a pyramid), and the decorated ball-flower, though alike in a general way, are never exactly alike; no two of them would fit the same mould, any more than any two of the millions of similar leaves of the same tree will. If you take the trouble to measure the dimensions of corresponding parts, you will find that the old builders were not very particular about them either, so long as they were tolerably near what was intended. In short, you will see what will strike you as a general carelessness about execution; and usually the inequalities are greater in large work, heavy mouldings, and bold carvings, than in small; or, as Mr. Ruskin well expresses it, "the execution is always subordinate to the design." Whereas, the pride of modern work is precision and *finish*, as the masons and clerks of the works will tell you themselves, and probably call your attention to the excellence of their performance in that way, if you give them a chance.

Now one of these things must be wrong. You may read Mr. Ruskin's reasons why the precision is wrong and the carelessness right, either in his *Seven Lamps*, or in the little tract *On the nature of Gothic*, which he has taken out of the *Stones of Venice*, and published for sixpence—in my opinion the most valuable part of that overgrown and costly book. I will content myself with asking you, without reference to any reasoning about the matter, which you think looks best on the whole, the old work with what you think its carelessness, or the new with all its precision, and pointing, and "cleaning down?" To Lord Palmerston and the classical architecture gentlemen, no doubt, this very carelessness (if they ever observed it) is one of the proofs of barbarism of old Gothic, and perhaps they think that the men who rub up stones and point the joints so nicely, and work such sharp edges and exact curves, are a much higher and more intelligent order of workmen than the old masons, who very likely could not read, but could work better mouldings and ornaments out of their own heads than our architects can design now, and never stopped to see whether they were exactly alike, or were of any particular size; as no man ever would who is capable of judging by his brains, instead of his compasses, of the effect he is producing. Hence too comes that curious fact which Mr. Ruskin ascertained, that even now the workmen who are set to carve ornaments of their own design (the best symptom of real Gothic revival that we have seen), not only do them much better and with more life, character, and variety, but also much faster than those who merely copy drawings or models. I was struck myself with the rapidity with which lumps of stone on our buttresses and pinnacles at Doncaster were converted into birds and beasts, and how the men who carved in this way never added a stroke too much; while the smoothers, and scrapers, and copiers of classical models of ornaments never seem to think they have done enough.

I have described in the book before referred to how I accidentally learnt that "finishing" is really spoiling, by finding a moulding lying about which seemed to me exactly right, and like an old one, and being told by the man whom I asked how it came to be so, that "it was not finished." I have not time to dwell further on this point now, beyond saying that with extreme difficulty and in spite of resistance which required a very strong hand to put it down, I succeeded in getting most of the work at Doncaster left "unfinished;" but in that respect also there is just enough done in the usual way to enable anybody to judge of the difference, for the joints of the nave pillars were pointed up as usual, and present their white lines like a band of paint at every joint, and the inside stone work was scraped; whereas the great pillars were not allowed to be touched after they were built, nor any of the outside work or window tracery. At St. James's nothing of the kind was allowed. I may observe however, that where the stone is mottled, or has a natural variety of colour, it may baffle even the modern builder, and look well in spite of him, as is fortunately the case with the stone which was used for most of the inside of St. George's Church, and has a singularly good effect both in the walls and some of the arches. All this is equally true of woodwork, and even of the glazing. Everywhere precision, and extreme regularity, and smoothness, are fatal to Gothic effect, and are essentially dull and feeble; as indeed precision for its own sake, where there is no object to be gained by it, generally is in other things besides building. Of this however, the vast majority of architects seem to be still profoundly ignorant or incredulous, though every cast in their museum, and every Gothic building in the world testifies against them.

They may possibly, in the vain hope of finding some *ex post facto* excuse for the practice which they adopt for no real reason, but from an ignorant acquiescence in the Italian spirit which yet infests a great deal of pretended Gothic, ask whether I mean to prohibit the polishing of marble; and if I do, how its proper effect is to be brought out. Of course I do not: and for this simple reason. Marble does owe its effect to polish, and therefore polishing is proper for it.* In this respect it is precisely opposite to stone-work; for the variegation of surface and the colour of marble do not come out till it is polished; whereas the smoothing or regular working of stone work, and the pointing up of joints, or any formal working of them, destroys the variety of surface, (except in the one case which I have mentioned, and which is so far like marble,) and brings it all to a dull and dead monotony which would spoil the best Gothic building in the world. The artists who make the picturesque views of intended buildings for the architects are perfectly aware of this, and take as much care to avoid all appearance of uniformity of surface and precision of details, as the architects themselves, or the builders who work to their taste and approval, take afterwards to obtain it: another instance of the fallacy of architectural drawings. In the days when everybody's views of grand architecture were like Lord Palmerston's, even the drawers of the old cathedrals thought it necessary to present them in as much of a classical dress as they could without altering their form; and if you look at any of those pictures you will see how completely they are ungothicised, simply by this artifice of giving a classical face to the walls, and marking out the stones into regular patterns. Many of the prints in modern architectural books have the same fault, though in a less degree; and if you can find the same thing drawn in the smooth and prim style in one of these books, and truly drawn in Britton's *Cathedrals* or *Antiquities* (for he knew better than to improve upon the Gothic he was copying), you will almost doubt whether it is the same, so different does it look with a Gothic surface, and an Italian one. And do not let the architects delude you with the nonsense that this roughness is the effect of age. That you may dispose of in a moment by the first old building you can find (and there are plenty) in which the tool-marks remain visible. Moreover age without absolute decay cannot alter the forms and sizes of ornaments or mouldings, or make things irregular and various which were once precise and uniform. And still further, you will find that the flat and monotonous modern work never acquires the variety of the old even where the stone is already perished, and is therefore older in effect than old stone work which has not perished.

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* Nevertheless, I have used marble unpolished, and should do so again, for a monumental slab set against a wall; the effect is certainly better than of the usual polished ones, though the case is different where the marble is expressly used for ornament and colour.

To be sure, the classical notion of symmetry, in its strict sense of making everything on one side match something on the other, either as a reality or a sham, is pretty well exploded out of Gothic, except in the minds of mere builders; and there is almost more necessity for putting in a caution against the opposite extravagance of fanciful and ostentatious departure from symmetry and uniformity. All the arguments on both sides from the analogy of nature must end in this:—that nature is never at all symmetrical in stationary objects; and even in those in which the symmetry of nature is constantly appealed to, viz. animals, it is very seldom quite symmetrical, and if you take in colour, never.* But for all that, it is of no use disputing the notorious and universal fact that the eye is pleased with a considerable amount of symmetry in building, as well as with a certain amount of variety. Some styles require or admit more variety than others; and the difference between an architect of genius and one of none is that the latter does not know, and the former does, probably better than he or any one else can explain, how far to carry the symmetry, and what degree of variety will be most pleasing and beautiful. But it requires no genius to find out so much as this, that even where the general arrangement is symmetrical, the subordinate parts may well be varied, as in the opposite or adjacent windows of aisles; and on the other hand, that the great leading features of a building ought not to be duplicated. Therefore, the double western towers of our cathedrals are always rather narrow, and never very high for the size of the building; but the great single one at Ely is; and several of the double ones have some variety of detail besides. The duplication of the huge steeples at Cologne is only another of those "blunders worthy of a German," which, as Mr. Fergusson observes of some others, reduce that vast cathedral from "a work of art in the highest sense of the word" to nothing more than "the noble conception of a mason:" even a single one at the west, like Hamburgh, would have been better; and if one of the size of the intended two had risen out of the middle, instead of the trumpery lantern shown there in the pictures, it would have been infinitely finer, and would have made the transepts right also, which are as much too long without a central tower, as those of Doncaster are too short with one.†

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* It seems to be supposed that the ogee curve, or bend of contrary flexure, must be right and beautiful, because it is so common in nature. So far as I can see, a real ogee curve, uninterrupted by angles or nodosities, is very uncommon in nature, in stationary objects. I do not say that proves it to be unfit for architecture, for circular arcs are equally unnatural. But the circle is a form of strength, wherever it is used in architectural construction; while the ogee is essentially a curve of weakness, except on a very small scale, or composed of very few stones. Even turrets with ogee tops, which are really strong because of the domical principle involved in them, are almost universally condemned to the nickname of pepper-boxes; and architecture certainly began to fall soon after the ogee curve began to appear on any large scale.

† From St. James's park, near Marlborough House, the Victoria tower in a dim light looks like a great central tower of the Abbey; and the improvement to the effect of them both is striking.

I am not sure how far these and perhaps other physical defects of modern Gothic architecture are due to the want of anything like architectural criticism: I mean such criticism of individual buildings, completed or in progress, as books and pictures undergo. No doubt a great deal of that criticism is as great rubbish as the majority of the books and pictures themselves; but not all. And I cannot help thinking it is a misfortune both to the architects and to the public that they can learn absolutely nothing from criticism on architecture, because there is not only no good criticism, but none at all. I know there are plenty of magniloquent descriptions in the newspapers of Mr. So-and-So's beautiful church, consecrated last Wednesday, and of the Halls and Exchanges which the Queen is good-natured enough to open, where everybody lauds everybody, and the architect goes to bed convinced that in his hands at least "architecture is progressive," and that "the architecture of the future" is now really on the point of being "inaugurated," as it is always going to be next year by somebody. But of course everybody understands these exhibitions as mere local glorifications, which the London newspapers are kind enough to publish when they have nothing better to do. I think it was creditable to the people of Doncaster that they took no pains to get up anything of the kind, even on such an unusual event as the simultaneous opening of the two new churches last October. But on the other hand the absence of any notice of such churches in the daily newspapers, which give us now the best criticisms of books and pictures, proves what I was saying, that there is no real criticism of architecture. The two architects' papers prudently abstain from much architectural criticism; and probably if they did not, those criticisms would generally be traced to some prejudice or partiality which would be well understood in the profession, if not out of it.

The only publication that I know of, which really claims for itself the rank of a critic on the public works of the year, and this year does so expressly in contradistinction to the two professional journals, is the *Companion to the British Almanac.* As you test encyclopædias by looking at what they say of the subject you know most about, I have taken the trouble to read what the Companion has to say about these two churches I have so often mentioned. If you take the same trouble, you will see that, after giving the principal dimensions of the great church, they tell us that the new tower in the geometrical style is certainly superior to the old perpendicular one, fine as that was; but that they cannot approve of its having been "privately arranged" from the beginning so to change the style, though the arrangement was kept secret for three or four years (which is all a pure invention of their own, or of the critic whom they hire for the occasion, as they might have learnt from any one who knew the facts, or from my lecture on the church, which was published both in the *Doncaster Gazette,* and in the pamphlet already referred to). Then comes a quantity of nonsense, complaining that, as in many other churches and cathedrals, in which there is not width enough of flat wall, we have not defaced the tower with a clock face, but have given the people quarter-chimes instead, which are far more useful and did not exist before. Of course all this is sent up by some local malcontent, like the paragraph which has been going round the newspapers about the impossibility of hearing in the church; where any man with a good voice can be heard perfectly, not merely from the middle where the pulpit is, but from one end of the church to the other, 169 feet; and some eminent singers have pronounced it a singularly good place for sound, as is also proved by the surprising effect of a small temporary organ, now in the corner of the church farthest from the congregation. So much for the power of criticism of the Society for the Diffusion of Useful Knowledge, exhibited upon that building. Of the other new church, St. James's, all that they can find to say is, that such a church has been built, "*but it is a comparatively plain structure.*" I think we need spend no more time in considering the value of this, the only annual commentary on the progress of architecture in England.

I am aware that it is becoming a fashion among the second-rate architects to say that there is only too much criticism, and too much interference by "meddling wiseacres," who will never let the cultivated taste and true genius of the professional architects have a fair scope for action. No doubt it is a very pleasant theory that the business of amateur architects is to find money and praise for the professional ones; but it is a little too one-sided to have much chance of being adopted by those whose consent is essential to its success; and so I shall not stop to argue about that. But if it be true, as these non-interference gentlemen would have you believe, that their failures are due to us, and their successes to themselves, nothing can be easier than to prove it and have done with it. Why don't they say, "Here is such a building: these were my drawings for it, but Mr. Denison, or Mr. Somebody else, would insist upon altering them in this way, and now you see how it is spoilt?" If they did that, and showed that any considerable number of buildings really are spoilt by interference of that kind, they would prove their case, and we should have very little to say for ourselves. But unless they can, they had far better have held their peace than ventured upon this singularly perilous defence. For if they will drive us to try that issue, which of their works will they select to try it by? Indeed the proportion of cases in which their plans are altered or interfered with is so small, that even if the interference always made them worse, still the general character of modern architecture would not be sensibly affected by it. But small as that proportion is, will any architect venture to say that those buildings are worse than the many in which they have entirely their own way, sometimes with as little restriction on the cost as the design? If interference is fatal to success, it is tolerably notorious that the two churches whose pictures are behind me ought to be the greatest of modern failures. But I do not want to press this point, because it is impossible to say all that might easily be said upon it without involving the better class of architects (who, as far as I have seen, do not join in this foolish outcry against the amateurs) in the folly of the bad ones, who never did anything fit to be seen in their lives, and naturally want to find somebody else to throw the blame upon, and expect probably that no one will take the trouble to expose the trick.

But as I am speaking of architectural literature, I will mention a fact, from which some inferences will, no doubt, present themselves to you without my lengthening this lecture to extract them : I mean the singularly small proportion of books professing to teach any principles of architecture, which are written by the present architects. There is that Rudimentary Treatise of Mr. Garbett's, to which I have several times referred, and which, I agree with Mr. Fergusson, contains "a great deal of common sense criticism;" though probably neither he nor I should be inclined to endorse the whole of it; and Mr. Penrose's celebrated investigation of the principles of construction of the Greek temples, of course, belongs to the class of scientific works on architecture. But besides these, I do not know any book by a living architect, which does. Mr. Scott's two books are written more for the purpose of advocating particular opinions or views of architecture, (with which for the most part I agree) than of teaching it; and Mr. Street's, on Italian architecture, belongs rather to the class of descriptive books than of scientific. (By the bye I am glad to find that both there and elsewhere he has disclaimed the idea of exalting Italian Gothic above Northern; as it seems Mr. Ruskin also has of late, and says that he never had that intention, which was very naturally imputed to him by probably every reader of the *Stones of Venice.*) It is hardly worth while to enumerate as architectural works, the various small decoctions of Rickman, and older writers on the Gothic Styles and the Classical Orders: and on the whole it appears that the professional architects have a very small array of literature to set against such books as the *Seven Lamps* or the *Stones of Venice*, Mr. Petit's several books on Architecture, Mr. Freeman's History of Architecture, Mr. Parker's Glossary, and other works edited by him, Professor Willis's several treatises on Mediæval buildings, and Dr. Whewell's on French and German Churches. And if none of these had been written, that truly wonderful "Handbook" of Mr. Fergusson's, would be alone sufficient to turn the scale in favour of the amateurs against all the professional literature of the present generation.

It is no less remarkable, that of all these books, professional and unprofessional, there is literally not one that is expressly devoted to that style of architecture which some of our Parliamentary professors of the fine arts assure us is the only style fit to be cultivated by a civilised nation. For the Greek style is now almost as universally abandoned or altered into something very different, as it was universally believed in and talked about fifty years ago. Perhaps the time is not so far off as some people imagine, when it will only be thought a few degrees less absurd for the Northern nations to throw away their own architecture, and import an older one from latitude 43°, than it is already seen to be to set up the style of 36° as the only grand style of building for the whole world, from Athens to Edinburgh and Washington.

After six years' experience in building at Doncaster, besides a little elsewhere, I am convinced, without meaning thereby any reflection upon architects, that there can be no good building anywhere unless there is somebody constantly, or at any rate very frequently, on the spot, who has what we may call an architectural eye, and the power to stop at once anything which he sees is going to turn out wrong. I know that it is one of the common fallacies inculcated by architects, that you cannot tell until things are done whether they will look well or ill. Why not, I should like to know? Do they expect us to believe that a man sitting in London and drawing an elevation of a pinnacle, or a section of an arch, knows better how it will look in its place, than other men who see it begun upon the spot, and see that it looks wrong *already?* I say "already," for there is nothing I am more certain of than that a thing which looks ugly already, is not going to look beautiful when something more is added to it. Old ruins, which are simply incomplete buildings, are not ugly. Of course a man must know how to distinguish between ugliness and incompleteness of proportions; but supposing that he does, and is generally a person whose judgment of a finished building is likely to be right, then I would rather have his opinion of the promise of a building, or any part of it, long before it is finished, than of the best architect who has never seen it, nor even any correct view of it. For remember, that such a thing as a correct view of a building, as it will look, is never made, or so rarely, that the exceptions are not worth regarding. You have seen what Mr. Garbett says of the artistic or perspective view, which is generally made of an intended building, from what is expected to be the best point of view. But this is not the building plan: it is the captivating picture, which no doubt every honest architect believes will turn out a true one also, at least as far as it extends; but whether it will or not, depends on whether the sections and elevations made afterwards by himself or his clerks, are really such as would be accurately translated into that perspective; and generally they are not. Moreover, the greatest part of the details are never designed or drawn at all as they will appear; I mean in such perspective as they would present, if drawn from actual view in their places. I know it would involve a good deal of trouble to do this; but so it does to do most things well. In old times, when the people on the spot designed and superintended the details, the absence of accurate drawings, or of any drawings, did not signify. But the case is very different now that nobody with any more knowledge or taste than is required for "setting out work" is allowed (if it can be prevented) to intervene between the architect in London, or his clerks who make his working drawings according to the stereotyped rules of the office, and the masons who turn them into stone. You would be astonished if I were to tell you the number of instances in which working drawings of details were sent down to Doncaster, which it was discovered, sometimes just in time, and sometimes too late, would produce, or had produced a totally different effect from that of the general drawings of the whole, even in points to which particular attention had been directed. For these reasons I am convinced that both architecture and architects would be great gainers, if there were, not fewer, but many more people who had ability and power to interfere with buildings while they are in progress, and with the designs beforehand, at least to the extent of requiring alterations to be made where they see that the thing will fail and be a blemish if it is not altered. At the same time I am aware that architects have sometimes difficulty enough at present in working down to the bad taste, capriciousness, ostentation, stinginess, and vulgarity of their employers; and that although competitions have some advantages, they are very likely to increase the difficulty of architects in designing what they know will look well; as they will be sure to speculate upon the probable prejudices, bad taste, and ignorance of the majority of the judges, and their almost certain inexperience in judging of architectural drawings, and will bait their designs accordingly, or they might as well save the trouble of making them.

I believe the progress of Gothic architecture, rapid as it has been, would have been more rapid, and at any rate more complete as it went on, but for the impatience which is one of the characteristics of these times. I do not mean merely that kind of impatience which is evinced by those who told the Gothic architects only eighteen years ago, that they must 'be content to lay the foundations of an edifice 'which *future generations* shall see completed, and to toil for the 'recovery of hidden principles and lost harmonies which the master 'spirit of a succeeding age may awaken into life;' and then, within less than half the time of a single generation, turn round upon the men who have been so toiling, and declare that they can wait no longer; and that they are now convinced that some unknown and scarcely imagined compound of the Classical and Gothic styles is the thing to toil after and harmonize. This particular phase of impatience may not be common; but when we think of the variety of architectural fashions which have now flourished within less time than was occupied by the shortest-lived of all the old Gothic styles, we must come to the conclusion that our architects are either much cleverer than the old ones, or else much more impatient. At one time Early English was the style in vogue, for all cheap churches especially, the public or the architects supposing that as there was no tracery to make for the windows it must be the easiest and cheapest of all the pointed styles. By degrees they found out that it is probably the hardest to do well, and very far from the cheapest if it is to be done with anything like the massiveness, and depth, and number of shafts, and mouldings of the real Early English. So, instead of learning to do it properly, or paying for having it done well, there was next a run upon the latest style; and for a time everything was Perpendicular, varied occasionally with the most distant extreme of Norman, which was supposed to be a simple, easy, and neat style for small village churches; and being often executed in white brick, and the stone-work always done with the precision of the modern Perpendicular, the result was certainly such as to justify any degree of impatience in flying away from that mode of building as rapidly as possible. By degrees the contest seemed to settle itself between the Flowing or late Decorated, and the Geometrical or early Decorated styles, and the early one gradually prevailed; and I am certainly far from complaining of that victory. But now that it is gained, have we made the proper use of it? The other four styles were one by one dismissed, or abandoned, not so much because people were dissatisfied with the genuine old specimens of them, but because the new ones were manifestly defective; and is not the same thing now taking place with the Geometrical? Never mind whether you agree with me that it was the best of the old ones, but assume whichever you like to have been the best: still the same question may be asked, was that best style ever fairly tried by modern architects who had really mastered its principles and spirit, before it was thrown aside to try something else? If it was, let them tell us where we are to find the specimens. I confess I do not know them.

A few years ago came in the marble and colour mania, imported principally by Mr. Ruskin from Venice; a magnificent discovery for the architects, but not so certainly for architecture just at present; for the apparently inevitable effect of a rage for colour seems to be that it makes people more careless about form than ever; and no one I think will say that the knowledge of architectural form and proportion is in such a highly advanced state that we can afford to let it take its chance for a few years while we are speculating in some other element of beauty. I am far from objecting to the introduction of marble shafts, or granite pillars, wherever people like to pay for them: but if they fancy that a building otherwise bad, feeble, and in short, ungothic, will be made into a good one, capable of giving lasting satisfaction or pleasure, by putting in a certain quantity of marble instead of stone, they will be mistaken, and will find that the choir of Selby, and the nave of St. Alban's, and fifty other whitewashed churches, are still perversely admired, while all their beautiful colours are despised. *A fortiori*, the same is true of painting; for marble being composed by nature is sure not to be bad in itself, whereas painting has an unlimited capacity for badness in itself, and unfitness for its place besides. The Lady Chapel at Chester is just disclosing a pleasing prospect for our cathedrals, if the painting mania lasts a few years, and is cultivated with the same moderation as we usually display in following whatever happens to be the fashion. For, to begin with, they are painting and gilding some of the arches in the style of that splendid daub in the Crystal Palace on the copy of a door at Rochester, which at any rate is directly contrary to Mr. Ruskin's apparently correct remark that in nature colour never follows the lines of form, but rather runs across them; to say nothing of its hiding the stone entirely, and making it no better than plaster. But they have far surpassed this; for the wall itself is painted stone colour, and marked out into an uncommonly good imitation of our old friend the churchwarden style of the Georgian era; only to redeem it from that character, a red Aries, ♈, is painted on every joint, of course of the most uniform and exact pattern; from which the beholder is to understand that these very stone-like divisions are not intended for sham stones at all, but are only "suggestive of construction," and he cannot fail to admire it besides as a refined and highly inventive specimen of architectural polychromatics. I know perfectly well that Mr. Scott's last book indicates that there is some old authority for something of this kind. There always is for the worst things that are done. And considering the discrimination with which the one ridiculous or feeble example is generally selected for imitation out of a hundred good ones, the eclectic theory (which is one of the latest fashions of the season) seems likely to introduce us to some very striking novelties indeed.

I am not sure that I know what that imposing, but only plagiarised phrase, "the architecture of the future" means; and even those who are fondest of using such pieces of cant magniloquence have not always a very distinct apprehension of their own meaning; but I am tolerably certain that if it means a style of architecture which is to last as long as the shortest of the old styles, or to have any chance of being treated with respect, or looked at with satisfaction when the future has become the past, as all the genuine styles of old still are, it must be something very different from what is contemplated by our present seers. Vitruvius, the great authority on the classical styles, is quoted by Evelyn as saying that among the twelve things which an architect ought to know is astrology. I wonder from what signs it is divined that a new architecture, to be celebrated in future ages as the Victorian style (for so it has been already christened by persons who are too impatient even to postpone that ceremony till the usual time), is so near the ascendant now. I cannot help thinking that we are more likely to glide into a new style (if there is ever to be one again deserving the name) by learning first to stand a little more firmly on the old ground, from which most people now agree that we must take our start, before we are in such a hurry to take it. If the new style is to be a composite one, it is not very likely to be developed except by men who show by their works that they are thoroughly master of the others from which it is to be compounded. We are a little too ready to assume that a knowledge of dates and facts and measures, and all the history of architecture, (which is probably greater now than ever,) is a proof of a practical mastery of principles: but knowledge is not necessarily power, in spite of competitive examinations.

The last cause of dissatisfaction with modern Gothic building which I have to notice is of just the opposite kind to that of impatient running after novelties, whether "antiquated novelties," or novelties of invention, which I have been complaining of just now. It is too often forgotten that architecture is not archæology, nor ecclesiology either; and many foolish things are set down to the credit or discredit of Gothic architecture, which it has nothing in the world to do with, except that somebody has chosen to embody his own antiquarian or ecclesiastical fancies in a Gothic building. I fully appreciate the obligations of Gothic architecture to the archæologists, who saved many a fine building from being burnt into mortar like the greater part of that most beautiful of ruins, St. Mary's Abbey at York, at a time when almost everybody thought, as Lord Palmerston and his adherents still do, that Gothic architecture was a relic of barbarism. And the modern advance of architecture in the only style which has advanced is undoubtedly due in a great degree to the ecclesiologists. But for all that we cannot afford to let either of them be our masters. Architecture is the art of erecting new buildings for use and beauty; even Mr. Ruskin, with all his love of decoration, puts use and fitness for its purpose as the first condition to be satisfied by a building, though of course not the only one. Archæology (so far as it relates to our business) deals with the preserving of old buildings; a very good, but a very different thing; and ecclesiology is obviously more different still; and at any rate it has nothing to do with civil architecture, about which the fight happens to be just now; for our opponents say that we have carried the ecclesiastical and eleemosynary position, even the Protestant Dissenters having now joined us in it; the Romanists alone seem to be discovering that the architecture of Rome after all suits their purposes the best,* and it is remarkable that their modern Gothic buildings are seldom among the best specimens; there are many better dissenting chapels.

I think therefore that everybody who feels interested in the progress of architecture ought to resist all attempts to overwhelm and choke it by either ecclesiastical or antiquarian pedantry. There is nothing like a real example: so there is a certain city where the people of common sense are just now protesting against a scheme for building a set of new stalls within the line of almost the thickest pillars in quite the narrowest cathedral choir in England; using therefore less than a third of the entire width of the church for sitting room; and all for the sake of indulging a vision of four and twenty canons all of a row, who it is hoped will some day occupy them; and when they do, it is supposed they will not look so imposing if a couple of pillars are allowed to intersect the array as if it is perfectly continuous. And yet the people who do these things talk about the adaptability of Gothic to all forms and all uses. How do they expect anybody to believe it, who is inclined to disbelieve it, when they give him such advantages? And how are others to fight their battle for them, when they dare not stir a step for themselves to do what would be rational, convenient, and beautiful, (if well done,) because they "know no precedent for it?"

That is a case of what I call the pedantry of ecclesiology. Here is another of archæology, perhaps in the most ridiculous form in which it ever presented itself. The well known tower of St. Mary's, Taunton, has ceased to exist. *Etiam periere ruinæ:* for not only was the fabric

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* A paragraph to this effect has been going round the newspapers on the authority of Dr. Newman; and it has long appeared to me that nothing could be more natural, as all the authorities agree that Gothic architecture never took any real root in Italy. But these results are a singular end of all the nonsense we used to hear about Gothic being "essentially Popish." We shall see whether pronouncing it essentially barbarous answers any better.

too ruinous to stand, but the individual stones were too much decayed to be rebuilt. Otherwise it would have been a proper case for mere restoration, within the dominion of archæology, and out of that of architecture in its proper sense, which implies the designing of something new. Now, although this tower was a fine one for its age, it was of the very worst Gothic age; and although, from the effects of time, and its size, and general elegance of outline, it was on the whole beautiful, still its faults were so notorious and glaring that they have always been admitted even by the writers who have most celebrated it. It is enough to say that the buttresses were feeble in projection, the arrangement of the four and twenty windows the most monotonous of all the towers in England—in fact it could not possibly have been more so; the upper part looked top-heavy, and the decoration of it was of the most unimaginative character. Yet two of our first architects are now employed to build up a copy of this tower exactly as it was; or as near as they can make it. For as in all copies, of course the faults will be more apparent and the beauties less. Even if that were not certain, still the defects of the old tower are precisely those which will be more glaring in a new copy of it; because the monotonousness of this late perpendicular style is always strikingly aggravated by restoration in new stone; as is painfully apparent at St. Mary Redcliffe, where one can hardly believe that the old work ever was like the new, which is, or at least professes to be, exactly copied from it. The new tower will therefore inevitably not look like the old one, and will inevitably look worse. And for this of course the architects will be blamed, probably by the foolish people who have insisted on or acquiesced in the rebuilding as much as anybody. And they will deserve it. If nobody at Taunton has the sense to know, they have, that the idea of regarding a new copy of an old building as an antiquity is an absurdity below the reach of ridicule. They cannot but know that the copy, however good, will be no copy, because it cannot resemble what the old tower has appeared to every person who ever set eyes upon it for the last two hundred years, and probably much longer, and that people who are silly enough to expect it must be deceived. They will deserve the blame of the inevitable failure for another reason too; because they are descending from the position of architects to that of mere clerks of the works, having only to see that the builder copies a certain model, not designed by them, but by another architect in the time of Henry VII., and a model worse than either of them could design if left to himself.

The precedent of Doncaster enables me to say this with confidence respecting one of those architects; and I have no doubt about it with regard to the other, Mr. Ferrey. That case was a far more trying one than Taunton, for the old Doncaster tower was much earlier in date and absolutely faultless for its style; indeed it was the well known beauty of the tower which raised the wonderful subscription of nearly £40,000 for the rebuilding of the church, the rest of which was poor enough, as everybody could perceive when the tower was no more. But for all that, we ultimately determined to change the style in the new tower, as I had expected from the beginning that we should when the superiority of the geometrical or early decorated style had shown itself in the body of the church; and you have seen that certainly no favourable critic admits that the new tower is superior to the old one. It is however very singular, and it may be another warning to the Taunton people, that there is one point in which it has miscarried, and that is just the one in which we were assured that the old arrangement had been followed most exactly; but it turned out, when the failure was perceived, too late, that the clerk who had professed to measure the ruins had just left out one critical measure; which was accordingly filled up in the new tower by a guess; and that guess was wrong, and has made the crown of pinnacles too wide, as the tops of the later towers generally were, but as the earlier and better one of old Doncaster church was not. If then a tower of the best age of the perpendicular style could be improved where the design was avowedly altered (retaining however the same general outline and character), and has only come short of the old one where no alteration was intended, it will be a strange thing indeed, if a tower of the worst perpendicular age could not be still more improved when it has become necessary to rebuild it. And further I say that the wilful refusal to endeavour to improve it because improvement is alteration, is as monstrous an instance as could be conceived of architecture being over-ridden by a sham archæological pedantry, and no ridicule can be too great for either those who require it, or those who condescend to do it.

Such are the causes which, as I think, have retarded and are still retarding the efficient revival of what was for four centuries and more the architecture of all kingdoms north of the Alps, which had any; the only architecture which ever was in them, indigenous, progressive, universal; serving for all purposes, and adapting itself equally well to all; fettered by fewer restrictions than any architecture that the world has seen; capable perhaps yet of expansion beyond what was then thought of when the wants of mankind were fewer, but of expansion on its own principles, and not (as I believe) on the principles of that other architecture, which served to bridge over the gap from the archless styles of the "pre-scientific ages," but was itself absorbed in the infinitely greater capacity of the pointed arch styles, and has never been able to advance since: reappearing indeed when the decay of Gothic principles, and the absence of all power of artistic invention, left an opening for it, but always going on upon the same dead level of heavy uniformity and pompous dulness, sometimes rather better and sometimes rather worse, as the leading architect of one age happens to be better or worse than of another; harmonising with no face of nature where nature has a face to show, and tolerating no individuality where it has not, but sinking the thing which each man calls his house in a monotonous array of windows and pilasters, which may begin and end anywhere; incapable of any effect without the aid of great size, and even then not knowing how to rise with it except by mere repetition of features of no beauty by themselves. Is the best window in St.

Paul's, or in "the street of palaces," to be compared with any common three-light window built 600 years ago, or even with a good modern one of the same style revived? For it is now shown that good modern ones in this style can be built; and they can be built of any size you want, and in any number, and almost any position.

I think it is shown too, by these two provincial churches which I have referred to so often for that reason, that "the hidden principles and lost harmonies," which Mr. Petit has lately given up looking for in despair, are not too deeply sunk to be recovered by those who have the patience to seek them where they were lost, instead of rambling after them into unknown regions.

Some of you, I dare say, came here believing that the old Gothic ways have been thoroughly explored already, that the great Gothic revival is progressing charmingly, and that the time is fully come when it should be carried forward into those other unknown regions where the architecture of the future is waiting to be disclosed. If so, I hope your faith has had a shake. And on the other hand, those who have been in the habit of condemning Gothic architecture—on other grounds, I mean, than from mere shallow ignorance and flippancy—may possibly be led to reflect whether a great deal of what you are justly dissatisfied with has any right to be called Gothic architecture at all, and is not rather a flimsy modern figment, a kind of painted skeleton of Gothic without either flesh or spirit. I cannot forget how a great man, whose name you will recognise immediately, said to me, while he was living among the wretched modern Gothic of the new buildings at Cambridge, on my complaining of the new University Library being Italian, and Italian of almost unrivalled badness—"Gothic architecture is obsolete;" but soon after, when he was translated into the shadow of the great cathedral with which his name will ever be associated, he became one of the warmest admirers and the most zealous and successful restorer of the real Gothic architecture which he then saw before him. Some of that restoration is so well done, that it is not easy to distinguish the new work from the old, and so it is in some other places. That it is not so in all, and that buildings are continually erected (even where there is not the excuse of stinginess) in which Gothic principles are pretended to be followed, but are really set at defiance, is manifestly not the fault of Gothic architecture, but of architects.

[E. B. D.]

WEEKLY EVENING MEETING,

Friday, February 18, 1859.

Charles Wheatstone, Esq. F.R.S. Vice-President, in the Chair.

Somerville Scott Alison, M.D.

On certain Auditory Phenomena.

[Through the indisposition of Dr. Alison, the subject of the discourse, of which the following is an abstract, was kindly laid before the Members by Dr. Tyndall.]

Certain auditory phenomena, bearing upon the correlation of the ears, and possessing some interest, have been recently made out by the employment of a double stethoscope, which has been called the differential stethophone, contrived by Dr. Scott Alison.* This instrument, (*see* p. 64) like the first double stethoscope, viz., that of Dr. Leared, is applied to both ears, and has the self-adjustment of that of Dr. Camman of New York. It consists of two tubes for the two ears respectively, each independent of the other except for purposes of adjustment, manual management, and convenience of application. Each tube consists again of two parts, a tube part and a cup or sound-collecting aperture. The cup, made of mahogany or other freely vibrating wood, is about one inch in diameter at its mouth, and about one-eighth of an inch in diameter at its proximal extremity. The tube near the cup is made of flexible wire, and is covered with silk; the part nearer the ear is made of metal, and at the aural extremity is furnished with an ear-knob of ivory for insertion into the cavity of the external ear. The bore of the knob and of the metal part of the tube is about one-eighth of an inch in diameter.

An instrument possessing the same acoustic value may be made at much less expense than that above described. A piece of india rubber tube, about 18 inches long, having a bore of one-fifth of an inch in diameter, may have fitted upon one extremity an ear-knob, and

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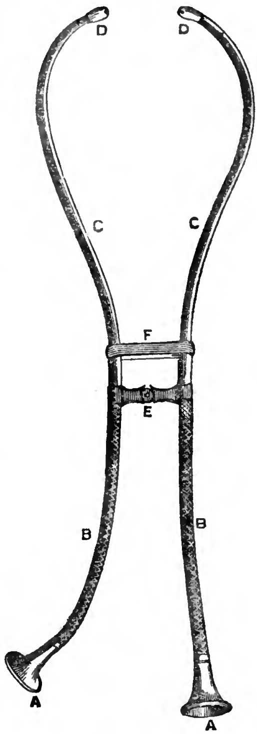
* It is to be observed, that for the differential stethophone to have its properties made available, it is necessary that both ears of the observer should be alike in acuteness.

upon the other a sound-collecting cup. Two of these, held together by means of a ring of ivory or steel, will make an excellent differential stethophone or phonoscope.



The differential stethophone was designed with the view of collecting different sounds from two parts of the body at the same time, and conveying them separately to each ear. It formed in reality a stethoscope for each ear, and it differed from all binaural or double stethoscopes hitherto employed, which collected sound through one aperture or cup only. Though the old binaural stethoscopes could not be made differential instruments, the differential stethophone might be made a binaural stethoscope, simply by placing the two sound-collecting cups at the same part of the chest. When this is done, the same auditory result is obtained as when the simple binaural stethoscope is employed, viz. a full sound and a distinct auditory sensation, fuller than when one ear only is employed. The advantages sought by means of the differential stethophone were, 1st, to give facility in comparing the intensity of the fine breath sound of the lung at two different parts of the chest at the same time; and 2nd, to ascertain with exactitude the relative commencement and termination of two sounds generated at different parts of the thorax; which hitherto was impracticable: for, as is obvious enough, it is not possible to have one ear at the same moment at two different parts or to have the two ears in the same plane, which however is now virtually effected by the differential stethophone. The second object was fully obtained; but the first was not secured when the two ears were simultaneously employed, though by using the two ears in succession this great advantage of comparing the intensity of one part with the intensity of another was fully gained. It was found that the weak or defective respiratory sound of one part produced no sensation in the ear to which it was conveyed when the stronger sound of another part was communicated to the other ear.

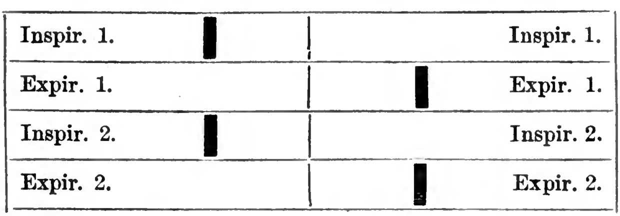
This failure of the differential instrument, though disappointing at the moment, has led to the ascertaining of an important acoustic principle and to the practical application of it in medicine, viz. that a major impression made on one ear will prevent all consciousness or perception of a minor impression made at the same time on the other ear, by the same sound; and that an impression on one ear which produces a distinct sensation may be made to produce no sensation whatever, by conveying at the same time a major impression of the same sound to the other ear: the sensation obtained through the latter ear totally destroying or obliterating all sensation of the same sound in the other ear. By the same sound is meant the sound proceeding from the same body, as a watch, a bell, or from two bodies of the same kind, as two tuning-forks of the same size and note.

Major impressions on one earprevent sensations of minor impressions on the other ear only in the case of the same sound, and not in the case of sounds of a different character, unless indeed the major sound happens to be very intense and deafening. Therefore the loud sound of one watch and the weak sound of another watch may be distinctly heard in the two ears, one in one ear only and the other in the other ear only, provided that one ear be favoured with a major impression of one watch, and the other ear be favoured with a major impression of the sound of the other watch. In this case, the stronger impression of either watch nullifies in a sensorial sense the weaker impression; and as the strong impression of one watch is made in one ear, and the strong impression of the other watch is made upon the other ear, one watch is heard in one ear and the other watch is heard in the other.

The principle of restriction of hearing of the same sound to that ear on which a major impression is made is illustrated in respiration. The ear connected with a part where the respiration is weak fails, as has been already stated, to convey any sensation, while the ear connected with a part where the respiration is strong produces sensation.

The diagram represents the sounds occurring alternately in two sides of the chest in a *consumptive* patient. The dark spots represent the *sounds*. The right side has the Inspiration strong, the Expiration faint. The left side has the Inspiration weak, the Expiration coarse.

HEALTHY. Right Side of Chest. UNHEALTHY. Left Side of Chest.



The same principle may be made to perform an important part in the diagnosis of diseases of the heart, accompanied by murmur. If the two cups of the stethophone be applied at two points of the area of a murmur differing in intensity, the sound is heard by that ear only connected with the point where the murmur is more intense. Now as the source of a murmur is determined by the point of its greatest intensity, it must be obvious that a ready mode of diagnosis is offered.

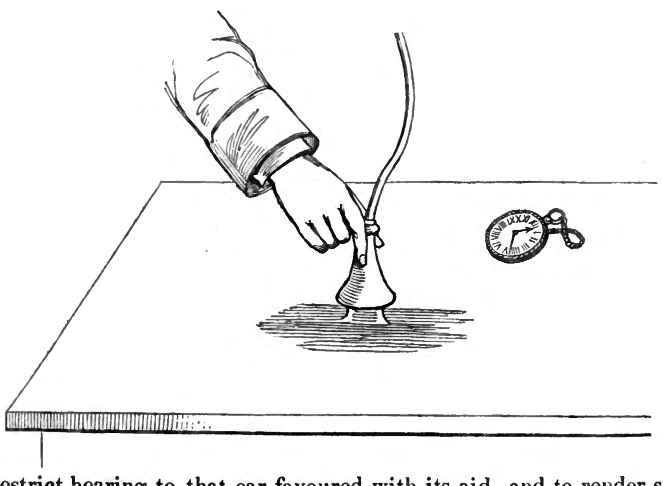
If there be a material difference at the two spots examined, sound is heard only by that ear connected with the more intense point. The test is absolute; for in one ear there is no hearing, and in the other there is hearing. There is consequently no room for error in judgment as to degree, as in the case of consecutive observations with one ear only, with the ordinary stethoscope.

The human ear being capable of hearing many sounds at the same moment, (if one be not extremely intense,) and as the principle so to speak of restriction of hearing to the favoured ear holds in the case of all, it follows that if one can be favoured with a major impression of fifty sounds, they shall all be heard through that ear, and through that ear only. And if some of these fifty sounds be carried in major intensity to one ear and some in major intensity to the other ear, some of these sounds will be exclusively heard through one ear, and some exclusively through the other ear; a division of sounds thus virtually taking place. Indeed, if we possessed fifty ears, and if of fifty sounds one could be carried in a major intensity to each of these fifty ears, each of the fifty sounds would be heard exclusively through one ear, *i.e.* each ear would hear one sound exclusively.

This apparent division of sounds may be effected by placing a watch upon a musical box. If both cups of the stethophone be placed so as to receive sound from these bodies equally well, both sounds will be perceived through both ears, and if one cup be placed nearer these two bodies than the other cup, both sounds will be heard in the ear connected with this cup; but if one cup be held a little nearer the musical box than the other cup, and if this other cup be held a little nearer the watch, *i.e.* if one cup be a little favoured in respect of one sounding body, and the other cup be a little favoured in respect of that of the other sounding body, the musical box will be heard through that ear only which is favoured in respect of it, and the watch will be heard exclusively through that ear that is favoured in respect of that body. When two murmurs occur at the heart, one at the base and another at the apex, possessing as they usually do different characters, (say one being "blowing" and the other being "rasping,") and they blend together, they may be thus sensorially separated by placing one cup at the base, and the other at the apex, points of major intensity of the two murmurs respectively.

Of the amount of difference which shall suffice to render one ear as it were sensationless and to effect a monopoly of sensation in the other, it is difficult to speak with great precision. The difference must be considerable, but need not be very great. In the case of a watch, if one cup be placed fully on the watch, and the other one-third or one-half off it, sensation is limited to the ear connected with the first; if the cup wholly on the watch be now moved off the watch, but kept within half an inch of it, sensation is limited to the other ear; and if the cup half or one-third off the watch be now moved wholly off it, to the distance of an inch or more, sensation is again transferred to the other ear. Transitions of sensation may be thus indefinitely carried on from one ear to the other. This corresponds with ordinary audition: for if we draw a circle around the head in the plane of the horizon, which may be called an acoustic circle, we shall find that on carrying a sounding body round it, binaural sensation is procured only so long as the body is within about 10° on either side of a line drawn forwards and backwards at right angles with the transverse line of the head. In the case then of such a circle we have distinct binaural sensation limited to a region of about 20° in front and 20° behind. The intermediate arcs are the regions of uno-aural sensation; it thus appears that in ordinary audition a moderate superiority in respect of intensity of sound gives a monopoly of hearing to one ear.

The intensity of sound in the ear which has served to give it a monopoly of hearing to the disadvantage of the other ear, has in the foregoing observations been chiefly obtained by greater proximity of one cup of the stethophone to the sounding body over the other; but this may be procured by other means. A superior mode of conduction in respect of one ear over the other, or of one cup of the stethophone over the other, when the ears and the cups are similarly placed in respect of the sounding body, will give a like intensity. A solid rod in the case of one ear, while the atmosphere is the only medium of communication in the case of the other, in respect of a sound of a solid body, will suffice to give the monopolizing intensity. In the same way it has been recently ascertained by Dr. Scott Alison, that water placed at the distal extremity of a hearing tube will give an advantage of intensification or of more perfect conduction, sufficient



to restrict hearing to that ear favoured with its aid, and to render sensationless the ear connected with the sounding body by means of a hearing tube having *no* water at its extremity to intensify or economize sound by superior conduction. A ring of water between the edge of the hearing tube and the solid sounding body will suffice for this result. The restriction of hearing to one ear is perfect, although the conditions of the ears and of the two limbs of the differential stethophone are precisely alike, with the exception of the ring of water. As we form our notions of sounding bodies being on our right hand or on our left, by perceiving through which ear auditory sensations are acquired, and as water under the circumstances gives an augmentation of sound sufficient to restrict to one ear, an aural illusion may be produced by having two hearing tubes of equal length and of the same conditions brought in contact with two windows respectively, and placing under the cup of one of them a thin bag of water, which may be called a hydrophone. A church bell or a barrel organ will be heard only through the ear connected with the window having the water bag upon it, although these sounding bodies may be nearer the other window. The mind is led to believe that the sounding bodies are nearer the window which is the more distant from them.

In the case of some few sounds, the influence of water in modifying their tone is so great, that virtually a sound different in kind is heard. Thus, a watch, in Dr. Alison's possession, gives simply a short ticking sound to that ear connected with one cup of the differential stethophone held in the air; and imparts a full soft tick, or rather tack, and a musical bell sound to the other ear connected with the other cup of the instrument placed upon the thin bag of water lying on the watch. The sound of the watch is so modified by the different media, that as it were two different sounds are obtained, a sharp tick and a full soft tack, and though the watch-sound is conveyed in greater intensity to one ear than to the other, it is heard in both ears. No loss of sensation takes place, as in the case of the same sounding body, sounding through the same medium. The reason of this exception to the law of auditory obliteration, so to speak, is found in the fact that the sound is no longer one, but has become virtually two, by being conveyed through two different media, and it has been already stated that a major impression sensorially nullifies a minor in the case of its own sound only and of no other. The fine bell sound has been simply made audible by passing through water. It is probable that the double hearing of some persons which has been commented upon by physicians has been due to a difference in the two ears involving a disagreement in the media through which sounds have had to pass. The double hearing referred to has consisted of hearing the sounds of the same sounding body, very different in character in the two ears; the sounds in one ear being soft and in the other sharp, and so on. Perhaps, likewise, certain sounds inaudible in one ear have been rendered audible in the other, as in the case of the fine bell sound above referred to, by passing through better media. It is to be observed, that in order to have a sensation in this manner in both ears the sound must not preponderate greatly in one.

In the case of sounds conveyed to the ear through the bones of the head, a restricting intensity may be procured by closing the aperture of the external ear, as has been observed and commented upon by Mr.

Wheatstone. A tuning-fork placed upon the middle of the forehead is heard in that ear only that is closed. The closure prevents the escape of sound, and promotes resonance to an extent sufficient to give the restricting intensity. This restriction of hearing to the closed ear in the case of sounds communicated through the bones of the head may be imitated by an analogous closing of the apertures of the differential stethophone. Sounds communicated in equal intensity to both limbs of the stethophone at some point between the closed apertures and the ear-knobs are heard louder than when the apertures are open; and if one aperture only be closed, the sounds are heard in that ear only that is connected with the closed aperture. The wetted cotton of Mr. Yearsley—really a cotton and water membrane—and the gutta-percha membrana tympani of Mr. Toynbee, thus applied, greatly increase sounds conveyed to the stethophone at points between the closed apertures and the ear knobs. By closing one aperture with wetted cotton, and the other with gutta-percha, the comparative value of these appliances so far as intensification of sound communicated in the manner under consideration, may be tested. Both of these appliances have been found extremely beneficial in cases of deafness proceeding from perforate membrana tympani. It would appear that it is by some closure of the passages of the ear in persons partially deaf of one ear, that these persons hear sounds communicated through the bones of the head, in that ear only that is deaf; a fact ascertained in an extensive enquiry instituted by the author, and that had been previously observed in a few cases.

When one cup of the differential stethophone is held decidedly nearer a sounding body than the other cup, the sound is perceived, as it were, *in* the ear connected with the nearer cup; but if the further cup be brought somewhat towards the sounding body, so as to obtain more sound, and to be more upon a par with the other cup, the sensation changes its seat and is felt less *in* the ear and more towards the centre of the head, or the spot mid-way between the two ears. This centripetal character of sensation is more marked as the two cups attain to a parity; and when this is fully accomplished sensation is located at a central spot. If the cup, which was at first further from the sounding body, be now gradually brought nearer than the other cup, a further transition of sensation is produced; it leaves the central spot and moves towards the other ear, and becomes exclusively located there, as it was exclusively located at the first part of the experiment in the other ear. By alternately and rapidly bringing the cups nearer the sounding body, this movement of sensation may be rendered very striking.

Lastly, the differential stethophone affords an unfailing test of the existence of differences of intensity of sounds communicated by different bodies, solid, liquid, or gaseous. If consecutive trials be required on two bodies, this is done by using the two cups in succession, the necessary movements being effected without changing the position of the head, and with only a very little motion of the fingers. The contrast is readily made, and the difference, if any, with facility discovered. If both limbs of the instrument be simultaneously employed, and if the difference in the amount of the same sound conveyed to the instrument be material, an absolute test is procured at once; for, as has been already explained, no sound will be heard in one ear, and a full sound will be heard in the other. For example, a musical box placed upon the banks of the Serpentine, is heard in that ear which is supplied with that limb of the stethophone (an elongated one) whose cup is immersed in the river, and not at all in that connected with that limb whose cup is held upon the ground. In the same way this test of restriction of hearing to one ear, or of uno-aural hearing, is available for deciding upon the comparative acoustic value of different arrangements. If we desire for instance to know whether surrounding a glass with water and another with air, both filled with water and previously being equal in communicating sound to the stethophone, gives a difference of sound, the fact is immediately made known. The ear connected with the limb of the stethoscope immersed in the glass surrounded with water hears nothing, while the ear connected with the glass surrounded with air has a distinct sensation.

[S. S. A.]

WEEKLY EVENING MEETING,

Friday, February 25, 1859.

H. R. H. The Prince Consort, K.G. D.C.L. F.R.S.
Vice-Patron, in the Chair.

Professor Faraday, D.C.L. F.R.S.

On Schönbein's Ozone and Antozone.

Ozone has already been before the members of the Royal Institution on two occasions: on the 13th June 1851, when Schönbein's early views of it were given, and on the 10th June 1853, when the results of MM. Frémy and E. Becquerel, obtained by passing the electric spark through dry oxygen, were described; and also the opinion of Schönbein respecting the entrance of ozone as such (and not as simple oxygen) into combination. Since then, Schönbein has been led to the belief that oxygen can exist in a third state, as far removed by its properties from ordinary oxygen in the one direction as ozone is in the other; and therefore, in a certain sense antagonistic to ozone. This substance he names *antozone*, and believes that it also enters into combination, retaining, for the time, its special properties. Hence there is not merely ozone and antozone, but also ozonide and antozonide compounds. Thus, permanganic acid, chromic acid, peroxides of manganese, lead, cobalt, nickel, bismuth, silver, &c., form a list of bodies containing more or less of ozone in combination; and the characters of ozone, and of these bodies because of the ozone in them, is that they are electro-negative to the antozonides, *i.e.* as copper to zinc; they evolve chlorine from chlorides; they cannot generate peroxide of hydrogen; and they render blue the precipitated tincture of guaiacum. On the other hand, oxywater and the peroxides of potassium, sodium, barium, strontium, and calcium, form a list of substances containing antozone. These bodies are electro-positive to the former; they cannot evolve chlorine from hydro-chloric acid, or the chlorides; they evolve the peroxide of hydrogen when treated either by oxy-acids or even the hydro-chloric acid, and they not only do not render blue the white precipitated guaiacum, but they restore that which has been rendered blue by ozone to the white or colourless condition. Now when two ozonides or two antozonides are put together, with the addition of water or an indifferent acid, they mingle but do not act on each other; but if one body from each list be associated in like manner, they mutually act, oxygen is evolved from both, and ordinary oxygen is set free; or rather, as Schönbein believes, ozone separates from one body, and antozone from the other; and these uniting produce the intermediate or neutral oxygen. Thénard, who discovered the peroxide of hydrogen, showed that the peroxide of silver, when brought into contact with it, not only caused the separation of part of the oxygen of the fluid, but also itself lost oxygen, that element leaving both bodies and appearing in the gaseous state. This experiment, with others of a like nature, and many new ones, were referred to and made in illustration of Schönbein's views. As to the independent existence of oxygen in these two new and antithetical states, ozone has been so obtained, *i.e.* out of combination, and independent of any other body; but antozone has not as yet afforded this proof of its possible separate condition. Oxywater is the compound in which it seems nearest to a free condition. As Schönbein's view includes the idea that oxygen in these two states can retain their peculiar properties when out of combination, and have them conferred otherwise than by combination, and as ozone does fulfil these conditions and does exist in the independent state, so it is important that antozone should be pursued by experiment until it gives a like result.

In relation to this subject the view of Mr. Brodie should be referred to, respecting the condition of certain elements at the *moment* of chemical change, on which he published a paper in the Phil. Trans. for 1850, p. 759, and another in the Chemical Society's Journal in 1855. He assumed oxygen as capable of existing in two states; the particles being polarized to each other by the action of associated particles, and for the moment in the relation of oxygen and hydrogen to each other; he also made many numerical experiments for the purpose of obtaining the equivalent action of the oxygens assumed to be in these opposed polar states.

[M. F.]

WEEKLY EVENING MEETING,

Friday, March 4, 1859.

Sir Roderick I. Murchison, D.C.L. F.R.S. Vice-President, in the Chair.

John Tyndall, Esq. F.R.S.
PROFESSOR OF NATURAL PHILOSOPHY, ROYAL INSTITUTION.

On the Veined Structure of Glaciers.

If a transparent colourless solid be reduced to powder, the powder is white: familiar illustrations of this fact were brought forward at the commencement of the discourse: specimens of transparent rock crystal and of the substance pounded, of solid glass and of glass in powder, of rock salt and common culinary salt were exhibited. A glass jar, partially filled with a solution of carbonate of soda, with a little gum added to give it tenacity, presents, on the addition of a little tartaric acid, the appearance of a tall white column of foam. In all these cases, the whiteness and the opacity were due to the intimate and irregular admixture of a solid or a liquid with air; in like manner the whiteness of snow was due to the mixture of air and transparent particles of ice.

This snow falls upon mountain eminences, and above the snow line each year leaves a residue; the substance thus collects in layers, forming masses of great depth. The lower portions of such masses are squeezed by the pressure of the mass above them, and a gradual approach to ice is the consequence. The air is gradually expelled and the transparency of the substance augments in proportion

Nevertheless, the ice in the upper glacier region always contains a large amount of the air originally entrapped in the snow; this air is now distributed through the solid in the form of bubbles, which give the ice a milky appearance. Thus at the upper part of a glacier the ice is white and more or less opaque; while, at its lower extremity, as almost every tourist knows, it is blue and transparent. The transition from one state to the other is, in most cases, not a gradual change which takes place equally throughout the entire mass; the white ice, on the contrary, of the middle glacier region is usually striped by veins of a more transparent character than the rest of the mass; the transparency being due to the fact that the air which gives to the ice its whiteness has by some means or other been wholly or partially ejected from the veins. These veins partake of the blue colour of transparent ice, and, contrasted with the white ice in which they are imbedded, often give to the substance a most beautiful laminated appearance; vast portions of many glaciers consist of this laminated ice.

The object of the discourse was to offer an explanation of this veined structure of glacier ice.

The theory of the veins which perhaps first presents itself to the mind, and which is still entertained by many intelligent Alpine explorers, is that the veining of the middle glaciers is simply a continuation of the bedding of the névé; that not only do the annual snow-falls produce beds of great thickness, but every successive fall tends to produce a layer of less thickness, which layers, or rather the surfaces separating them, ultimately appear as the blue veins. This theory, it was admitted, demanded the most serious consideration: on the exposed sections of the névé the lines of stratification were very manifest, and exhibited in many cases appearances closely resembling that of the veined structure. Indeed, it was with a view to examine this subject more closely, that the speaker withheld his observations on the structure of the Mer de Glace made in 1857, and betook himself once more to the mountains during the summer of 1858; his desire being to settle once for all the rival claims of the only two theories which then deserved serious attention, namely those of pressure and of stratification.

In pursuance of this idea, he first visited the lower glacier of Grindelwald, one of the most accessible, and at the same time most instructive in the entire range of the Alps. Ascending the branch of this glacier which descends from the Schreckhorn, the Strahleck, and the Finsteraarhorn, he came to the base of an ice-fall which forbade further advance. Quitting the glacier here, he ascended the side of the flanking mountain, so as to reach a point from which the fall, and the glacier below it, were distinctly visible; and from this position he observed the gradual development and perfecting of the structure at the base of the fall. On the fall itself no trace of the transverse structure was manifest; but where the glacier changed its inclination at the bottom, being bent upwards so as to throw its surface into a state of intense longitudinal compression; where, moreover, it had to bear the thrust of the descending mass behind, the blue veins first made their appearance. The base of the fall was a true *structure mill*, where the transverse veins were manufactured, being afterwards sent forward, giving a character to portions of the glacier which had no share in their formation.

The speaker afterwards examined the fall from the opposite side of the valley, and corroborated his observations. It is difficult, in words, to convey the force of the evidence which this glacier presents to the mind of the observer who *sees* it; it seems in fact like a grand laboratory experiment made by Nature herself with especial reference to the point in question. The squeezing of the mass, its yielding to the force brought to bear upon it, its wrinkling and scaling off, and the appearance of the veins at the exact point where the pressure begins to manifest itself, left no doubt upon the speaker's mind that pressure and structure stood to each other in the relation of cause and effect, and that the stratification could have nothing to do with the phenomenon as here exhibited.

He afterwards crossed the Strahleck, descended the glaciers of the Aar, crossed the Grimsel, and examined the glacier of the Rhone. This glacier has also its grand ice-fall. In company with Professor Ramsay he climbed in 1858 the precipices which flank the fall at the Grimsel side. In company with Mr. Huxley, he had in 1856 ascended the heights on the Furca side of the fall. What he has stated regarding the Grindelwald ice-fall is true of that of the Rhone; the base of the cascade is *the manufactory of the structure;* and, as all the ice has to pass through the mill, the entire mass of the glacier from the base of the fall downwards is beautifully laminated.

He afterwards descended the valley of the Rhone to Viesch, ascended the Eggishorn, and remained for eight days in the vicinity of the great Aletsch glacier—the noblest ice-stream of the Alps. A highly intelligent explorer had adduced certain phenomena of the Aletsch glacier as an evidence against the pressure theory of the veined structure; and the speaker did not think himself justified in quitting the place until he had perfectly satisfied himself that the glacier not only presented no phenomena at variance with the pressure theory, but exhibited some which seemed fatal to the theory of stratification.

He subsequently proceeded to Zermatt, and spent ten days on the Riffelberg, exploring the entire system of glaciers between Monte Rosa and the Mont Cervin. These glaciers exhibit, perhaps, in a more striking manner than any others in the Alps, the yielding of glacier ice when subjected to intense pressure. The great western glacier of Monte Rosa, the Schwartze glacier, the Trifti glacier, the glaciers of St. Theodule, are first spread out as wide and extensive névés over the breasts of the mountains. They move down, and are finally forced into the valley containing the trunk, or Görner glacier. Here they are squeezed to narrow stripes, which gradually dwindle in width until they form driblets, not more than a few yards across. From the Görner-grat, or from the summit of the Riffelhorn, these parallel strips of glacier, each separated from its neighbour by a medeal moraine, present a most striking and instructive appearance.

The structure of these glaciers was carefully examined, and in all cases as the observer travelled from regions where the pressure was feeble to regions where it was intense, the ice changed from a state almost, if not entirely, structureless, to a state in which the veining was exhibited in great perfection. Each glacier where it met the opposing mass in the trunk valley, and was pressed against the latter by the thrust behind it, exhibited a beautifully developed structure.

In a former discourse he had adduced proofs that the Glacier du

Géant was in a state of longitudinal compression; he had also shown that the seams of white ice which intersect this glacier are due to the filling up of the channels of glacier streams by snow, and the subsequent compression of the substance. Here then we have a vast ice-press which furnishes us with a test of the pressure theory. Both in 1857 and 1858, the speaker found many of these seams of white ice inte-sected by blue veins of the finest and most distinct character, their general direction being at right angles to the direction of pressure.

The speaker next considered the objections which might be made to his conclusions by an advocate of the theory of stratification; referring to the notions of M. Agassiz as to the turning up of the strata so as to expose their edges at the surface, and also to the acute remarks and arguments of Mr. John Ball. He admitted that these remarks might well tend to cast a doubt upon the pressure theory, by suggesting a possible, though extremely improbable, solution of the question, in harmony with the theory of stratification.

Hence his strong desire to discover some crucial phenomenon which should set this question for ever at rest, and leave no room for doubt, even on the minds of those who never saw a glacier. On Wednesday, the 18th of August, he was fortunate enough to make this discovery upon the Furgge glacier.

This ice-field spreads out as an almost level plain at the base of Mont Cervin. The strata pile themselves one above the other without disturbance, and hence with great regularity. The ice at length reaches a brow which forms the termination of a lower valley, shutting up the latter as a *cul de sac;* and down this brow it is precipitated, forming in its descent four great terraces. Huge blocks of ice stand, like rocking stones, upon the topmost ledge, and numbers, which have fallen, have been caught by the other ledges and occupy very threatening positions: the base of the fall is cumbered with crushed ice, and large boulders of the substance have been cast a considerable way down the glacier.

On the faces of the terraces horizontal lines of stratification are shown in the most perfect manner. Here and there the exertion of a powerful lateral squeeze is manifest, causing the beds to crumple, and producing numerous faults. Examining the fall from a distance through an opera glass, the speaker thought he could discover lines of *veining* running through the strata, at a high angle, exactly as the planes of cleavage often run at a high angle to the bedding of slate rocks. The surface of the ice was, however, weathered; and he was unwilling to accept an observation upon such a cardinal point with a shade of doubt attached to it. Leaving his glass with his guide, who was to give warning should the blocks overhead give way, he advanced to the wall of ice, and at several places, cut away with his axe the weathered superficial portions. Underneath he found the true veined structure, *running nearly at right angles to the planes of stratification.*

He afterwards climbed the glacier to the right, and, as he ascended, still better illustrations of the *coexistence* of the structure and the strata than those which he had observed upon the terraces exhibited themselves. The ice was greatly dislocated, and on the faces of the crevasses the beds were shown *with the veins crossing them.* The idea that the veins could be due to the turning up of the strata is plainly irreconcileable with these observations.

The speaker subsequently visited the Mer de Glace, and its tributaries, and found the same key applicable to their phenomena. The transverse structure of the Glacier du Géant is formed at the base of the series; that of the Talèfre branch of the Mer de Glace at the base of the Talèfre ice-fall, where the change of inclination and the thrust from behind develop the requisite longitudinal compression. He has already had occasion to remark upon the peculiar dipping of the structure, and the scaling-off of the protuberances, which are effects of the same cause. These phenomena are exhibited at the base of all the ice cascades visited by the speaker.

He divided, finally, the principal kinds of structure into three; as follows:—

1st, *Marginal structure,* developed by pressure due to the swifter motion of the centre of the glacier.

2nd, *Longitudinal structure,* due to mutual pressure of two tributary glaciers; the structure here is parallel to the medial moraine which divides the tributaries.

3rd, *Transverse structure,* produced by pressure due to change of inclination and to the longitudinal thrust endured by the glacier at the base of an ice-fall.

The speaker then entered upon the physical analysis of the manner in which the pressure produced the veins. When a liquid is heated, the attraction of the molecules operates against the action of heat, which tends to tear them asunder; at a certain point the heat triumphs, the cohesion is overcome, and the liquid boils. But supposing we assist the attraction of the molecules by the application of an external pressure, the difficulty of tearing them asunder will be increased, more heat will be required for this purpose, and we say that the boiling point of the liquid has been elevated by the pressure.

Spheres of sulphur were exhibited, which had been cast in a mould; these, on cooling, contracted so as to leave a large space hollow within each sphere; the same occurred, though in a less degree, with lead and most other substances. Conceive the sphere replaced in the mould, and the latter heated: to liquefy, it is necessary that the sulphur, or the lead, should *swell.* Here, as in the former case, the swelling of the substance is opposed by the attraction of its molecules; but with a certain amount of heat this attraction is overcome; we reach the *fusing point* of the solid. But suppose we assist the molecules by external pressure, a greater amount of heat will then be necessary to tear them asunder: and we say the fusing point has been *elevated* by the pressure. Reference was made to the researches of Mr. Hopkins and Mr. Fairbairn, in which this reasoning was experimentally verified.

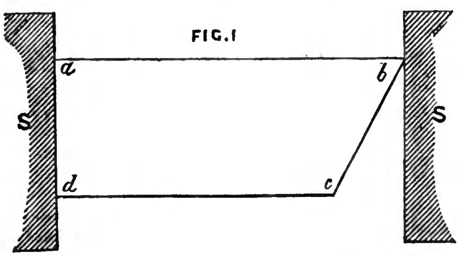
But the speaker also exhibited a sphere of bismuth, and showed that this metal *expanded* during its solidification; hence, in order to liquefy a sphere of bismuth, the substance must *contract:* the molecules must arrange themselves more closely together. Heat produces this contraction; and it is manifest that external pressure in this case *assists* the heat, instead of opposing it, and that to fuse the metal, under pressure, a less amount of heat will be necessary: hence the fusing point of bismuth is *lowered* by the pressure. Now *ice* is a substance which behaves in this respect exactly like bismuth; it *contracts* on being liquefied, and if we subject it to pressure it will liquefy *under* 32° Fahr. —in its case also the melting point is lowered by pressure. Reference was made to the theory of Mr. James Thomson, and the experiments of Professor Wm. Thomson, which bore upon this question.

Supposing then a prism of solid ice at 32° to be subjected to pressure. This pressure reduces its melting point, say for the sake of simplicity to 31°; what must be the consequence? The ice possesses a temperature higher than its new melting point, and the excess of heat which it possesses is applied to the liquefaction of a portion of it. This effect was experimentally exhibited before the audience. A prism of ice was placed between the surfaces of a small hydraulic press, and the prism was illuminated by the bright beam of an electric lamp. The beam had been previously sifted of *its heat* by sending it through a solution of alum, so that *the light* passed afterwards through the ice without melting it. By means of a convex lens placed in front of it, a magnified image of the prism of ice was cast upon a screen, and when the pressure was gradually applied, lines were observed drawing themselves across the pressed mass *in a direction at right angles to the pressure:* these lines the speaker had proved by strict examination to be the edges of flat spaces in which the ice had been liquefied by the pressure.

The speaker's theory of the blue veins then was, that the glacier, when subjected to intense pressure, also liquefied *in flats* perpendicular to the direction of pressure: a means was thus provided for the expulsion of the air entangled in the ice, or in other words for the production of veins containing less of air than the general mass of the glacier. A portion of the water would be absorbed by the adjacent bubbled ice, and refrozen when released from the pressure; and the veined structure would follow.

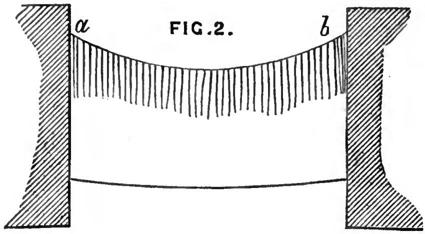
The following experiment was referred to. A prismatic mass of

FIG. I



ice was taken, the shape of which, in section, was that of *a b c d*, fig. 1.

The side *a b* was purposely left longest in order to throw the pressure upon that side. The mass was squeezed between the slabs of wood



S and *S'*, and the result was that shown in fig. 2; the side *a b*, which had borne the pressure became liquefied in flats, as shown in the figure.

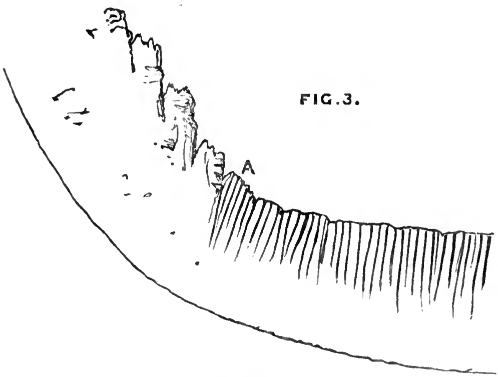


Fig. 3 is a section of the portion of the Grindelwald glacier already referred to: at the point A the glacier changes its inclination, and pressure comes into play; at this point the veins begin to be found, and in a very short distance reach perfection. A comparison of both diagrams at once suggests their relationship.

[J. T.]

GENERAL MONTHLY MEETING,

Monday, March 7, 1859.

WILLIAM POLE, ESQ. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Thomas B. Baskett, Esq.
James Bateman, Esq. M.A. F.R.S. L.S. &c.
William De la Rue, Esq. F.R.G.S.
Howard Warburton Elphinstone, Esq. M.A.
Philip Charles Hardwick, Esq.
Henry Warwick Hunt, Esq. B.A.
Frederick Hardwick Johnson, Esq.
Francis Le Breton, Esq.
Joseph Montefiore, Esq.
James Nasmyth, Esq.
John Pearson, Esq.
Hall Rokeby Price, Esq.
Arthur John Edward Russell, Esq. M.P.
Benjamin Leigh Smith, Esq.

were duly *elected* Members of the Royal Institution.

The Earl of Ashburnham, and
John Derby Allcroft, Esq.

were *admitted* Members of the Royal Institution.

The following PRESENTS were announced, and the thanks of the Members returned for the same:—

FROM
Agricultural Society of England, Royal—Journal, Jan. No. 42. 8vo. 1858.
Arts, Society of—Journal for Jan. 1859. 8vo.
Astronomical Society, Royal—Monthly Notices, Jan. 1859. 8vo.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for Feb. 1859. 8vo.
Boosey, Messrs. (the Publishers)—The Musical World for Feb. 1859. 4to.
British Architects, Institute of—Proceedings for Feb. 1859. 4to.
Editors—The Medical Circular for Feb. 1859. 8vo.
The Practical Mechanic's Journal for Feb. 1859. 4to.
The Journal of Gas-lighting for Feb. 1859. 4to.
The Mechanics' Magazine for Feb. 1859. 8vo.
The Athenæum for Feb. 1859. 4to.
The Engineer for Feb. 1859. fol.
The Artizan for Feb. 1859. 4to.

Faraday, Professor, D.C.L. F.R.S.—Répertoire de Chimie, par C. Barreswil und A. Wurtz. No. 5. 8vo. Paris, 1859.
Königliche Preussischen Akademie, Berichte, Nov. and Dec. 1858. 8vo.
Franklin Institute of Pennsylvania—Journal, Vol. XXXVII. No. 1. 8vo. 1858.
Geological Society—Proceedings for Dec. 1858 and Jan. 1859. 8vo.
Quarterly Journal, No. 57. 8vo. 1859.
Glassford, C. F. Esq. (the Author)—London Sewage: Shall it be Wasted or Economized. 8vo. 1858.
Howlett, S. B, Esq. (the Author)—Table for Mountain Barometers.
Leeds Philosophical Society—J. P. Crawford, Esq. F.R.S.—On China and its Trade. 8vo. 1858.
Sir J. F. Herschel—On Sensorial Vision. 8vo. 1858.
Chr. Tremblay—Comets, their Constitution and Phases. 8vo. 1859.
Leighton, John, Esq. F.S.A. M.R.I. (the Author)—The Library: Books and Binding. A Lecture. 8vo. 1859.
Linnean Society—Proceedings, Supplement No. 1, 8vo. 1899.
Transactions. Vol. XXII. Part 3. 4to. 1859.
Mackie, S. J. Esq. F.G.S. (the Editor)—The Geologist, Feb. 1859.
Newton, Messrs.—London Journal (New Series) for Feb. 1859. 8vo.
Novello, Mr. (the Publisher)—The Musical Times for Feb. 1859. 4to.
Petermann, A. Esq. (the Editor)—Mittheilungen auf dem Gesammtgebiete der Geographie. 1859. Heft 1. 4to. Gotha, 1859.
Photographic Society—Journal, Nos. 79, 80. 8vo. 1859.
Royal Society—Proceedings, No. 33. 8vo. 1858.
South Yorkshire Viewers' Association—First Annual Report. 8vo. 1858.
Statistical Society—Journal, Vol. XXII. Part 1. 8vo. 1859.
Taylor, Alfred S. M.D. F.R.S. M.R.I. (the Author)—On Poisons. 2nd Edition. 16to. 1859.
Taylor, Rev. W. F.R.S. M.R.I.—Anecdotenjäger. 8vo. 1858.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Nov. and Dec. 1858. 4to.
Wilson, Thomas, Esq. M.R.I. (the Author)—An Enquiry into the Origin and Intimate Nature of Malaria. 8vo. 1858.

WEEKLY EVENING MEETING,

Friday, March 11, 1859.

Charles Wheatstone, Esq. F.R.S. Vice-President, in the Chair.

William Odling, M.B.

SECRETARY TO THE CHEMICAL SOCIETY.

On Magnesium, Calcium, Lithium, and their Congeners.

The majority of the metals known at the beginning of the present century were observed to occur naturally in the earthy or oxidised state. The alkalies and earths proper, from their many analogies to the metal-yielding earths, were long suspected to be the oxides of certain unknown metals, whose tendencies to maintain the oxidised condition were stronger than those of any metals which had up to that time been isolated. This conception was first verified by Sir Humphrey Davy in 1807, and has since been abundantly realized. From their characteristic property of neutralizing acids to form salts, the earths and alkalies received the name of bases, and the metals eventually extracted from them became known as basic or basylous metals. Some of these metals, particularly those obtained from magnesia, lime, and lithia, have only of late been procured in quantities sufficient to allow of a demonstration of their properties.

The highly basylous and the commercial metals are alike obtained by three principal processes, namely electrolysis, precipitation by means of another metal, and reduction by charcoal at a red or white heat.

α. Electrolysis.—Very many metallic compounds, when submitted to the action of a galvanic current, are decomposed, with a deposition of metal upon the negative pole of the battery. Although a cheap electrolytic process has been devised for the extraction of copper from its ores, yet, altogether, electrolysis is too expensive to admit of employment for the mere separation of the commercial metals. It is, however, largely applied in the fine and useful arts for the production of different metals in certain required forms, as in the well-known operations of electrotyping, electroplating, &c. The liquid state, which is an essential condition for electrolysis, is usually obtained by dissolving the metallic compound in water; but for procuring the highly basylous metals, this means does not succeed, inasmuch as these metals cannot exist in contact with water. The basylous metals seem indeed to be deposited upon the negative pole; but, simultaneously with their production, they are acted upon by the water of the solution, and thereby converted into the state of caustic alkali. The liquid condition is consequently attained by means of fusion. Certain salts of the basylous metals, usually the chlorides, from their ready fusibility, are melted in suitable crucibles, and then submitted to electrolysis; this is the only mode by which the metals calcium and lithium are obtainable.

β. Precipitation of one metal by another.—This process is largely employed on a manufacturing scale. At the mines of Freyburg, metallic silver is obtained by agitating chloride of silver with scrap iron. The iron enters into combination with the chlorine, and turns out the silver. At the Royal Mint, silver is obtained by immersing plates of copper into solution of sulphate of silver. At the Cornish mines, considerable quantities of copper are annually obtained by immersing pieces of iron in solutions of copper. These processes are performed in the wet way. As an example of the dry way, we may adduce the metal antimony, which is made commercially by fusing sulphide of antimony with scrap iron. The iron turns out the antimony and unites with the sulphur. Similarly the metals aluminium and magnesium are prepared by fusing their respective chlorides with metallic sodium; the sodium unites with the chlorine and turns out the aluminium or magnesium. To obtain metals by this process of substitution, it is ordinarily necessary that the metal used to expel another must be more basylous than the metal expelled; hence it is that sodium is required for the production of magnesium. With the exception of potassium, which is much more expensive, sodium is the most basylous of the metals; it even serves to displace the quasi-metallic grouping of hydrogen and nitrogen, known as ammonium. Amalgam of sodium, introduced into a solution of chloride of ammonium, forms chloride of sodium and amalgam of ammonium. But these most highly basylous metals, potassium and sodium, afford remarkable exceptions to the law that basylous metals replace less basylous metals. Thus, although when sodium is heated with hydrate of iron, the sodium expels the iron, as might be anticipated, yet when hydrate of sodium and iron borings are heated together, a reverse action takes place, and the iron turns out the sodium, as in Gay-Lussac's process for the production of that metal. This reciprocity of results is only an extreme instance of a tolerably general law. In a similar manner, though mercury displaces silver from argentic nitrate, yet silver displaces mercury from mercurous nitrate. Though copper displaces silver from argentic sulphate, yet silver displaces copper from cupric sulphate. Though cadmium displaces copper from cupric chloride, yet copper displaces cadmium from cadmic chloride, &c. &c. Some of these results appear to depend on Brodie's law of homogeneous affinity: thus, when cadmium is deposited upon copper, we have the reaction,

$$\overset{+}{\text{Cu}}\,\overset{-}{\text{Cu}} + \overset{+}{\text{Cd}}\,\overset{-}{\text{Cl}} = \overset{+}{\text{Cd}}\,\overset{-}{\text{Cu}} + \overset{+}{\text{Cu}}\,\overset{-}{\text{Cl}}$$

γ. *Reduction by charcoal.*—This is the most usual means adopted for the production of metals on a manufacturing scale. Brunner's process for obtaining potassium and sodium is an exact counterpart of the commercial process for obtaining zinc; in each case the metal is evolved in the gaseous state, or distilled, from a heated mixture of its carbonate with charcoal. The same reciprocity exists between sodium and charcoal as between sodium and iron. Thus carbon decomposes oxide and carbonate of sodium to form carbonic oxide or anhydride (acid). But sodium heated in carbonic oxide or anhydride liberates carbon, and forms oxide or carbonate of sodium. Indeed chemical re-actions are not absolute but conditional; under reversed condition we obtain reversed results.

Magnesium.—Chloride of magnesium is the source from which the metal is usually obtained. Becquerel succeeded in procuring octahedral crystals of magnesium by the electrolysis of a solution of chloride of magnesium. But the metal is preferably obtained by electrolysing the fused salt. Matthiessen employs a common tobacco-pipe for the purpose: the bowl is filled with fused chloride of magnesium, or rather with a mixture of chloride of magnesium and chloride of potassium, which is more easily prepared than the pure salt. The negative pole, to which the magnesium attaches itself, consists of an iron wire passing through the pipe-stem. The positive pole consists of a pointed piece of gas-carbon dipping into the fused mixture of salts. Magnesium is however obtained most abundantly by heating its chloride with metallic sodium, as before referred to. The sodium turns out the magnesium, which collects in globules that may be melted together under a flux of low density. Magnesium is a solid metal of a silver-white colour. Its appearance contrasts favourably with that of aluminium, which has a decided bluish tinge. The freshly cut surface of metal is highly lustrous; it does not tarnish in dry air, and acquires only a film of oxide in moist air. Magnesium is but very slowly acted upon by pure cold water. It decomposes boiling water somewhat rapidly with evolution of hydrogen. Its specific gravity is 1·75. It is about one and a half times lighter than aluminium, and is indeed the lightest of all metals that are permanent in the air. One cubic inch of platinum balances 12¼ cubic inches of magnesium, and only 8¼ cubic inches of aluminium. At ordinary temperatures magnesium is somewhat brittle, and may be readily cut or filed. It is neither very malleable nor very ductile; but, at an increased temperature, may be hammered into plates, and drawn or rather pressed into wire by Matthiessen's process. The metal is placed in a small hollow steel cylinder, having a hole drilled in its anterior face. Through this hole the metal is forced in the form of wire, by means of a press, acting through the intervention of an iron piston, fitting into the hollow of the cylinder. Magnesium is readily volatile; so much so indeed, as to allow of its being purified by distillation in an atmosphere of hydrogen. It is a highly combustible metal, and burns brilliantly in air or oxygen, with a pure white flame. A magnesium wire, ignited at one end, will continue to burn through its entire length. Magnesium corresponds closely in its properties with zinc, and through zinc approximates to cadmium. The atomic weight of zinc is the mean of the atomic weights of magnesium and cadmium; and the atomic volume of zinc is the mean of the atomic volumes of magnesium and cadmium.

Calcium.—This metal does not result from the action of sodium upon chloride of calcium, but is obtainable only by the electrolysis of that salt, rendered a conductor by fusion. It is a solid metal, of a somewhat yellowish colour. It is highly lustrous, but tarnishes quickly in the air, and gradually becomes converted throughout into oxide of calcium, or lime. It decomposes cold water rapidly with evolution of hydrogen. Its specific gravity is 1·58. It is moderately hard, malleable, and ductile. It has not been volatilized. When heated to redness in the air, it burns with a scintillating flash; but, in consequence of its want of volatility, does not inflame. Calcium bears to its congeners, strontium and barium, relations similar to those which magnesium bears to its congeners zinc and cadmium; save that the members of the calcic family are associated rather by an equality, and those of the magnesian family by a gradation of properties. The atomic volume and atomic weight of strontium are respectively the means of the atomic volumes and atomic weights of calcium and barium. Despite many points of resemblance, the differences between calcium and magnesium are well marked. Thus magnesium and zinc are volatile and inflammable metals, permanent in the air, scarcely acted upon by cold water, and obtainable by treating their respective chlorides with sodium. Calcium is neither volatile nor inflammable, is quickly oxidised in the air, is rapidly acted upon by water, and is not obtainable by treating its chloride with sodium. The hydrate of calcium is soluble in water, the hydrates of magnesium and of zinc insoluble. Hydrated chloride of calcium, when heated, evolves water, but the hydrated chlorides of magnesium and zinc evolve chlorhydric acid, &c.

Lithium is a very sparingly distributed element. It occurs native, in proportions varying from 3 to 12 per cent., in certain complex silicates, fluorides, and phosphates; and is obtained in the metallic form by the electrolysis of fused chloride of lithium. The specific gravity of lithium is 0·59. With the exception of bodies in the gaseous state, it is the lightest substance in nature. It floats upon every known liquid. One cubic inch of platinum balances 36½ cubic inches of lithium. Lithium is a white-coloured lustrous metal, rapidly oxidised by exposure to the air. It is softer than lead, and may be cut with a knife, or squeezed between the fingers. It is readily obtained in the form of wire, by Matthiessen's process. It melts at 180°C, and at a higher temperature volatilizes. When heated to redness in the air, lithium takes fire, and burns with a brilliant, highly luminous, white flame, that is in curious contrast with the crimson colour which its compounds impart to the flame of ordinary combustibles. The presence of lithium in any substance is usually ascertained by means of this crimson-coloured flame, which, however, is altogether irrecognizable in the presence of even a small quantity of sodium salts, owing to the intense yellow-coloured flame which they produce. Cartmell has recently pointed out a ready mode of detecting the lithium coloration, even in the presence of a large excess of sodium salts, namely by viewing the flame through a layer of the blue solution of sulphate of indigo, which completely cuts off the yellow rays due to the sodium, but allows the uninterrupted transmission of the crimson rays due to the lithium. Gradational relations, similar to those of magnesium, zinc, and cadmium, exist between lithium, sodium, and potassium. In the solubility of its carbonate, in its degree of oxidisability, and in many other properties, sodium is strictly intermediate between its two congeners. De la Rue has observed, that metallic sodium from its inferior degree of oxidisability, may be preserved unacted upon in an aqueous solution of caustic soda. The atomic weight and atomic volume of sodium are respectively the means of the atomic weights and atomic volumes of lithium and potassium.

Lithium undoubtedly belongs to the same family as sodium and potassium. Like these two metals it is soft, readily fusible and volatile, highly oxidisable, and of lower specific gravity than water. Moreover, its hydrate and carbonate are sensibly soluble in water. But having regard to the totality of its characters in the free and combined states, and particularly to the properties of its hydrate, carbonate, and phosphate, it appears that the analogies of lithium to calcium and magnesium respectively, are scarcely less marked than are its relations to the true alkaline metals. It seems, indeed, as if the metals lithium, calcium, and magnesium stood upon the same level, and that while the gradation of lithium, sodium, and potassium, diverged in one direction, the gradation of magnesium, zinc, and cadmium diverged in another.

Lithium.	Calcium.	Magnesium.
Sodium.	Strontium.	Zincum.
Potassium.	Barium.	Cadmium.

Or, we might say, that, as regards their properties, potassium, barium, and cadmium, are highly specialized forms, while lithium, calcium, and magnesium are degraded or general forms, which, from the comparative absence of special characters, approximate to one another. The sums and means of the atomic volumes of the three groups of elements are shown below.

	Sums.	Means.
Mg. Zn. Cd. . . .	14·2	4·7
Ca. Sr. Ba. . . .	24·9	8·3
Li. Na. K. . . .	37·4	12·4
	3) 76·5	3) 25·4
	25·5	8·4

The atomic volume of the calcic family is observed to be exactly intermediate between those of the lithian and magnesian families. Similar relations exist between the atomic weights of the three groups.

When, irrespective of these curious numerical relations between the different elements and groupings of elements, we find the groups characterized by distinctive but correlated properties, and the members of the groups associated by community of characters, and separated by gradational differences only, we perceive that the possession, by each element, of its own special properties, is not an accidental endowment, but is a necessary result of the development of one general comprehensive plan.

[W. O.]

WEEKLY EVENING MEETING,

Friday, March 18, 1859.

CHARLES WHEATSTONE, ESQ. F.R.S. Vice-President, in the Chair.

REV. WALTER MITCHELL, M.A.

On a New Method of rendering visible to the Eye some of the more abstruse problems of Crystallography, hitherto considered only as Mathematical Abstractions.

THE unpopularity of crystallography may be attributed to the difficulties so many people, especially those who have not had a good mathematical training, meet with in attempting to master the conception of forms involving some of the principles of solid geometry. To a certain extent this may be removed by a well-arranged system of solid models: thus the first three propositions of the 15th book of Euclid, "the inscription of a regular tetrahedron in a cube, of a regular octahedron in the tetrahedron, and of the octahedron in the cube," may be demonstrated to the eye by a dissected cube, illustrating the natural cleavage of fluor spar. Indeed the cleavage of a cube of fluor spar is a natural demonstration of the three principal propositions of the last book of Euclid's Elements of Geometry.

There are many propositions of crystallography which require some mechanical means beyond that of the use of solid models to make them appeal to the eye for clearer perception. The most perfectly symmetrical solid forms of the crystallographer belong to the cubical or tessular system. There are seven different kinds or orders of forms belonging to this system, perfectly symmetrical; four of which admit of an infinite variety of species. These forms are associated in nature as well as in their mathematical relations to each other. They are found in crystals of the same substance, either in their simple forms or else associated in combination with each other, in the different faces of a compound crystal; thus the cube, the octahedron, and the rhombic dodecahedron, are found as simple crystals of the diamond, or faces parallel to all three or two of them, may be discovered on a more complex natural crystal.

The three forms we have just enumerated, the cube, the regular octahedron, and the rhombic dodecahedron, may be considered as the permanent or limiting forms of the cubical system; they admit of no varieties; their angles, whether those of the inclination, of adjacent faces, or of the planes constituting their faces, are invariable; they are also limiting forms. Between the octahedron and the rhombic dodecahedron we may conceive an infinite number of varieties of the three-faced octahedron, passing from the form of the octahedron to that of the rhombic dodecahedron ; similarly, the octahedron and the cube are limiting forms of an infinite series of twenty-four-faced trapezohedrons, and the cube and rhombic dodecahedron of a series of four-faced cubes. The forty-eight-faced scalenohedron or the six-faced octahedron is a form varying within the limits of all the others.

To represent to the eye the passage of all the varieties of these forms between their respective limits is the object of the mechanical contrivance which is the subject of this paper. A skeleton or armillary sphere is constructed of iron wire, so as to mark out the principal zones of the sphere of projection of the forms of the cubical system ; three circles are united at right angles to each other, so as to represent eight equilateral spherical triangles, each of whose sides are arcs of 90°. The six points where the arcs cross each other are the poles of the six faces of the cube ; the lines joining each pair of opposite poles represent the cubical axes, each axis being perpendicular to two faces of the cube which can be inscribed in the sphere. Each arc is now bisected. These twelve points of bisection are the poles of the rhombic dodecahedron ; the lines joining the opposite pairs of these poles are the rhombic axes, each of these axes being perpendicular to two faces of the rhombic dodecahedron inscribed in the spheres, or inscribed in the cube inscribed within the sphere. Let each of the eight equilateral spherical triangles be divided into six equal and similar spherical triangles by arcs, joining the angle of each triangle with the centre of its opposite side ; the armillary portion of the sphere is now completed. The point within each of the eight equilateral spherical triangles, formed by the intersection of the three arcs by which it is divided, is the octahedral pole. There are of course eight of these ; the lines joining the opposite pairs of these poles are the octahedral axes, each one being perpendicular to two opposite faces of the regular octahedron inscribed in the sphere, or in the cube inscribed within the sphere. If we now join each pole of the octahedron with the three poles of the octahedron in the three adjacent equilateral spherical triangles by straight wires, and do this symmetrically for the eight poles, we shall then have the edges of the cube inscribed within our armillary sphere. The octahedral axes joining the opposite solid angles of this cube and the rhombic axes passing through the centres of each opposite edge.

Within this skeleton cube we now inscribe a regular octahedron, using elastic strings for its edges, by uniting the point where each cubical axis passes through the face of the cube, with the similar points on the two adjacent faces. Each face of the octahedron is therefore represented by an equilateral triangle of elastic cord. We now suppose each side of the eight equilateral triangles to be bisected. Every angle of the eight equilateral triangles is joined to the bisection of its opposite edge, by another series of elastic cords. We have now an octahedron inscribed, in the cube inscribed within our armillary sphere. Every face of the octahedron having marked upon it, the traces formed by an imaginary plane passing through the zones of the sphere and its centre. It will now be seen that the cubical axes join the opposite solid angles of the octahedron; the rhombic axes, the bisections of its opposite edges; while the octahedral axes pass through the intersections of the elastic cords, which join each solid angle of the octahedron with the centres of the edges opposite to it.

The points where the elastic cords meet, and the octahedral axes pass through the faces of the octahedron are now fastened to cords. These cords are made to run round pulleys and are united together, so that by pulling them simultaneously, the points uniting, every one of the three elastic cords which are described on the the face of the inscribed octahedron can be made to travel uniformly and symmetrically along each of the octahedral axes from the face of the octahedron to the solid angle of the circumscribing cube. Another series of cords are united to each of the four elastic cords, which meet at the point bisecting each of the edges of the inscribed octahedron. These, by a similar contrivance, are made to draw these points along the rhombic axes. The instrument is now completed. By simply pulling the eight cords united together, which cause the elastic cords to ascend the octahedral axes, the inscribed octahedron passes through every form of the three faced octahedron till it reaches the limiting form of the rhombic dodecahedron. Each three-faced octahedron being inscribed within the cube, inscribed within the sphere.

In a similar manner, by pulling the cords, running along the rhombic axes in combination with those running along the octahedral axes, all the other forms are shown as passing within their prescribed limits. As soon as the cords are loosened, the elastic bands immediately resume the form of the inscribed octahedron. In addition to these forms, the instrument also can be made to demonstrate the passage of all the hemihedral forms of the cubical system with inclined faces within their limits. In this manner it was demonstrated that this instrument can make visible to the eye all the changes and varieties of an interesting series of forms and their mutual relations, which could otherwise only be conceived by a considerable power of mathematical abstraction. This armillary sphere, by some other small additions, can be made use of for tracing out some of the most beautiful portions of the zone-theory of the poles of crystals.

[W. M.]

WEEKLY EVENING MEETING,

Friday, March 25, 1859.

Sir Henry Holland, Bart. M.D. F.R.S. in the Chair.

Robert Angus Smith, Esq. Ph.D. F.R.S.

On the Estimation of the Organic Matter of the Air.

After describing the opinions concerning organic matter in the air, and the various attempts made to estimate the amount, the speaker described a method of obtaining the relative quantity by means of mineral chameleon, permanganate of potash or soda. This mineral had been proposed by Forchammer, as a mode of estimating the organic matter in water, but it was capable of estimating quantities much more minute. At first the air was passed through the solution of chameleon, but this was not found to cause complete action. It was necessary that the air should remain for some time in contact with the solution to be decomposed. It was then ascertained that the relative amount of organic and other oxidisable matter in air could be found by a simple metrical experiment in a few miuutes.

The speaker then said :—In working out this idea, it has been found that a vessel of the capacity of 80 to 100 cubic inches is the most convenient. This is equal to rather less than a quart and a half and rather more than a litre and a half.

The solution of chameleon used must be extremely weak, so that small quantities cannot readily be distinguished by gaslight. 600 grains of it are required to decompose 5 grains of a standard solution of oxalic acid. The standard solution of oxalic acid is so made that 1000 grains neutralize one grain of carbonate of soda. A thousand grains contain therefore 1·184 grains of crystallised oxalic acid.

To prepare the solution a manganate was formed by heating nitrate and carbonate of soda and manganese, assisted by a little chlorate of potash. There was the most minute trace of nitrate remaining in the solution. Perhaps chlorate of potash would have been better, but I had no idea at the time of the difficulty afterwards found in obtaining the same quality. A solution of this manganate was made in pure water, and carbonic acid passed through until a reddish purple shade was obtained. It was then tested by oxalic acid, adding three or four drops of pure sulphuric acid. The purest water obtainable was added to dilute it to the proper amount. This often failed ; and I have sometimes for a whole week failed to obtain the proper solution. Although I call it permanganate, it is not entirely so ; it is a mixture of manganate and permanganate. A permanganate of the strength described has a dingy appearance and uncertain colour. I do not doubt that a pure permanganate of a suitable strength may be obtained pleasant to work with. There is some difficulty in obtaining pure water for preparing the solution. If allowed to stand for some time with a manganate it becomes purified.

The solution of chameleon is apt to change, although slowly, even when it is hermetically sealed in a glass tube. The solution described had become nearly colourless when sealed up hermetically for about three months. It is found readily to change when it is exposed to air by frequent removal of the stopper of the bottle containing it. Its strength must be tested occasionally; and if it differs from the standard, a calculation must be made for its reduction. The strength of the permanganate solution is extremely small. A few grains of the ordinary solutions of manganese used will make some thousand grains of the solution here employed. The reason of this lies in the extremely small amounts of organic matter found in even the worst air.

The vessel used is simply a bottle, with a perforated stopper, through which pass two tubes. To one of these a stopcock is attached, to the other a clasp or stopcock. The standard size proposed is 100 cubic inches; and to this all the experiments have been reduced; the vessels actually used contain between 80 and 100 cubic inches of air. The stopcock is of glass, or of hard caoutchouc, which is still better. When the bottle is to be filled with the air to be tested, the stopper is removed, and the pipe of an exhausting pump is inserted, reaching to the bottom of the bottle. The pump is made like a cylindrical bellows of about 8 inches long when stretched out, and about 4 in diameter, and is compressible into the thickness of about two inches. The sides are made of thin Mackintosh cloth. By the use of the pump the air of the vessel is removed, and the external air of course enters. A few strokes of the pump are sufficient, *i.e.* from 6 to 10. After ten strokes I perceive no change, and am inclined to think that it is an unnecessary number. The test liquid is poured into a graduated tube or burette, containing somewhat more than will be required. A portion is then poured into the tube which passes through the stopper, and the stopcock is opened to allow it to pass. Small quantities are used; when it has entered the bottle, the liquid is made to spread over the sides, and time given it to be exposed to the action of the air; it is found that in five or six minutes a decided epoch is attained from which to date the comparative action.

In order to see the colour the liquid must be allowed to trickle down the sides of the vessel, and collect itself at one point of the circumference at either end of the cylindrical part of the bottle. This part must be raised up to the level of the eye so that the longest axis may be presented to the sight, and thereby the deepest shade of colour. It requires some time to accustom oneself to the sight of such a small amount of colour; but when it is once well observed, it will be found to be a method which will admit of the greatest precision. The first few drops which are poured in will probably be decolorised at once; a few drops more must then be added; if they become decolorised a few more must be used; and so on until there is a perceptible amount of colour remaining. When this occurs, the experiment is concluded. The amount of the reagent used is then read off from the graduated measure. If the liquid be of the proper strength, and the bottle the required size, the number of grains gives the comparative quantity at once. Sometimes the amount of organic matter is so small that there is no appreciable action, on even the smallest amount of solution by one vessel of air. In this case it is necessary to fill the bottle several times. The mode of doing this is apparently extremely rude, but the results are such as not to demand a finer method at present. A finer method, of course would need little ingenuity to contrive. At present I merely remove the stopper and fill again with air as before. During the period of filling the vessel the surface of the liquid is reduced to its smallest amount, and the change it undergoes is either inappreciable, or so constant as not to affect the results.

In analysing the air in this manner, it is found that a decided result is attained in about five minutes. Sometimes the result is decided in one: that is, there is a termination to the rapid action. This peculiarity is probably to be explained by the following experiments. If we pour decomposing matter on the permanganate solution, it is rapidly destroyed. If the matter be not in a state of decomposition the action is much slower.

These different results promise a mode of dividing the organic matter of the air into classes according to its quality. These facts are mentioned merely as germs of a future inquiry. In large towns where coals, containing much sulphur, are burnt, the sulphurous acid takes the oxygen of the chameleon, and an apparently large amount of organic matter results. This sulphurous acid is of itself an impurity, perhaps as hurtful as some kinds of organic matter.

We measure by this means the amount of oxygen needful for the oxidisable matter of the atmosphere, and all such matter is impurity, in some places entirely organic, in others, such as towns, mixed with inorganic gases.

Some of the principal results obtained by this method were as follows:

RELATIVE QUANTITIES OF ORGANIC AND OTHER OXIDISABLE MATTER IN THE AIR OF*

Manchester (average of 131 experiments)	52 9
,, All Saints, E. wind (37 experiments) . .	52·4
,, ,, W. wind, less smoky (33 expts.) .	49·1

* A few of these results were published in the "Athenæum" during last summer. The present numbers are somewhat higher, being reduced, for the sake of uniformity, to correspond to a vessel of 100 cubic inches.

Manchester, All Saints, E. wind, above 70° Fahr. (16 expts.)	58·4
„ „ below „ (21 experiments)	48·0
„ In a house kept rather close	60·7
In a pigstye uncovered	109·7
Thames at City, no odour perceived after the warmest weather of 1858	58·4
Thames at Lambeth	43·2
„ Waterloo Bridge	43·2
London in warm weather (six experiments)	29·2
„ after a thunderstorm	12·3
In the fields S. of Manchester	13·7
„ N. of Highgate, wind from London . .	12·3
Fields during warm weather in N. Italy	6·6
Moist fields near Milan	18·1
Open sea, calm (German Ocean, 60 miles from Yarmouth)	3·3
Hospice of St. Bernard, in a fog	2·8
N. Lancashire	about same
Forest at Chamouni	2·8
Lake Lucerne	1·4

The first experiments undertaken were in Manchester, and the average amount obtained was in the city about 50, gradually diminishing in moving towards the country until it was found in the fields at 13; on passing a sewer stream about a mile from the outskirts, the amount rose to 83. The atmosphere on the Thames was not measured whilst at its worst, but immediately afterwards; when however it had ceased to affect the senses of most persons at least, the amount was very high, viz., 58. I was anxious to know how far the Thames affected the atmosphere of London, and tried some experiments: the result was that the influence appeared to cease almost immediately; the fact of a block of houses standing in the way was enough to prevent the influence; when at the worst this may not have been the case; to arrive at the other side of the block, the vapour would generally require to rise high, so that it would become mixed with a great deal of air. The amount obtained in a few trials in the streets of London was 22 to 34; going on to Highgate, the numbers sank from 33 to 24; on descending the north side of Highgate-hill a distinct change was perceived, the numbers being 18; the wind meantime was blowing from the city: the few experiments made in the fields in summer gave 10 to 12. The numbers 6 to 18 were obtained in Switzerland and Lombardy. The moist fields around Milan gave 18; when the water passes off the rice fields, producing the unhealthy season, I do not doubt that the amount will be much higher. It was not convenient for me to stay, nor to go farther to places distinctly infected with malaria. I was desirous of trying it in some of the hovels of the Vallois and the Val d'Aosta, but the weather being fine, and the people living much out of doors, the enquiry was not encouraging. The few experiments made did not give very striking results, whereas the lower parts of our own towns gave results most decided: I imagine the cause of this to be that a drier air does not allow the offensive matter to rise so readily. This fact has many ramifications, but it will explain several difficulties in our sanitary science. It is with the assistance of moisture that the organic matter is conveyed into the air.

Moisture itself, as may be supposed, does not produce any action on the test; one of the lowest numbers obtained was on the German Ocean, about 60 miles from land; the day was calm and clear. In the straits of Dover, when the wind was blowing briskly from the German Sea, the amount obtained was very high, but as there was a slight spray the experiments were disregarded. About 8000 feet high on the Alps, a dense fog showed also one of the smallest amounts obtained; the ground was entirely bare rock, and could not give out organic matter. The amount was 2·8.

The influence of height was very decided. In the higher grounds of Lancashire, near Preston, the numbers being from 2 to 4. A wind blowing down from the Mer de Glace gave rather more than at a lower point, although coming down the hill; a dry pine forest in the neighbourhood, although very fragrant, did not appear to raise the number. The influence of the sea and of height seem equally decided.

A few hasty experiments made in the hothouses at Kew led me to believe that there was less increase there than might have been expected, the amounts obtained being less than in London, but more than outside the houses, where it was cooler. At the same time weeks or months should be given, when only hours were allowed for the experiments.

The influence of heat appears to be to increase the amount, when there is moisture present.

The influence of dryness seems to be towards diminishing the amount.

The influence of great cold has not been tried yet.

The influence of rain in hot weather, to some extent of course a cooling influence but chiefly a means of washing the air, seems most decided. After a thunderstorm and shower at Camden-square, the number, which was previously 31, fell to 12.

The influence of our towns, especially our smoky towns, is most decided also; it is easy to tell by this test, when in the outskirts of a town, whether the wind is blowing from the town or the country.

A distinct difference was always found between the front and back of Manchester houses: a similar difference obtained when a room had been inhabited for some time, and the difference was of course very marked when the smell of a sewer came into the house. I had a good opportunity of observing this in my laboratory last year.

It must be remembered that the numbers given for some places were obtained on one day of the year only, and we must be careful not to draw too many conclusions: we have yet to learn what kind of organic matter is wholesome and what is unwholesome. I believe that this is the next great point to be attended to; at present we are only becoming able to ascertain the gross amount. I feel this caution to be needful, lest the numbers should be used to prove too much.

At an early opportunity all the experiments made will be published; but we may already see the range of the action of this test to be so great as to make it promise to be of some practical as well as scientific value. Dr. Southwood Smith has observed that the facts on which sanitary economy are built are exceedingly difficult of comprehension by a large number of people, because the cause of the evil cannot be brought directly under the observation; if however any plan were invented of showing that these dreaded emanations existed even when the senses could not perceive them, belief would be easily gained, and the requisite carefulness would then take place. If the method explained be found to be no more valuable than this, it will at least not be reckoned among useless discoveries.

We may hope, however, that it will be found to prove not only that much of that which we have already known is true, but that many other now hidden things are true also; we may find that every wind will have attached to it its mark of unwholesomeness with respect to this test, and that every season also will have its co-efficient. It may also be found that changes of season or of condition of the air will be ascertained with much more certainty, delicacy, and rapidity than now. We may even hope to find some premonitory symptoms of disease in the atmosphere before it affects the human body; the exciting cause itself existing long before it has been able to take effect, so that useful precautions may be made in time, and an efficient defence prepared. At the same time no proof whatever has yet been given that a plague or any infectious disease can be estimated by it, although reason has been given for such an expectation, whilst the air over different fields differs enough to promise some knowledge of miasm.

But what is abundantly established and made clear to the eye is that the air of our large cities is sufficiently impure to account for much of their unhealthiness, and the air of our hills and seas and lakes sufficiently pure to account for its salubrity. It is to be hoped that greater consequences will follow in proper time; although this itself was needed to set at rest some questions which have cooled the enthusiasm of many in the cause of sanitary reform.

[R. A. S.]

WEEKLY EVENING MEETING,

Friday, April 1, 1859.

Sir Roderick I. Murchison, D.C.L. F.R.S. Vice-President, in the Chair.

Nevil Story Maskelyne, Esq. M.A.

*On the Insight hitherto obtained into the Nature of the Crystal Molecule by the instrumentality of Light.**

The horizon of man's view extends in two directions. The one is turned towards the infinitely vast, and carries his eye into the regions of space, spanning distances that leave his world a speck in creation. The other direction along which he strains his gaze, is into the infinitesimal; and the microcosm of the crystal is the region it must traverse before it can reach the ultimate units of material consistence, which form the centres of chemical force. That region, however, is to be explored by the reason rather than by the eye, for the minutest thing cognisable by the microscope is left far behind in the first step taken downwards into the crystal world.

In a crystal there exists a complete *regularity of arrangement* and of the distribution of its powers of resisting, of transmitting, or of converting and modifying any forces that may solicit it. Of this, to a great extent, the external symmetry of its facettes is an expression. But the physical properties of the crystal afford the most perfect evidence of it.

The different varieties of symmetry, exhibited by outward crystalline shape, are found to indicate, with much precision, a correspondingly

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* In this notice a larger space is given to preliminary and other details than could be devoted to them in an hour's discourse. This was felt to be necessary, partly because the speaker entered more on these points than he had intended in the scheme of his discourse; and having done so it is his wish to give a clear statement of them: partly too because the subsequent views for which he is much indebted to the conversations and private letters of his friend Professor Grailich, are more intelligible to a general audience by the help of such preliminary explanations. He believes they may render a beautiful subject—which must be difficult, and cannot be popular in the childish sense of the word—sufficiently intelligible, by patient study, to be interesting to persons such as those who did him the honour of forming his audience on the 1st of April.

ordered arrangement in the physical properties of the various crystals that illustrate them.

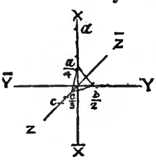
The definition of the terms *crystallographic axes, parameters, indices,* and *morphological axes* being necessary for elucidating what was to follow, were given. Thus *crystallographic axes* are geometrical directions, determined by the intersections of any three planes of the crystal. They are taken as axes of co-ordinates, and called severally x y and z, and such are chosen as express in the most simple and smallest numbers the relations of all the planes of the crystal to one another. These relations are indicated by the *Law of Crystallography*, viz., by this, that a plane parallel to any facette will cut these axes at certain definite relative distances, measured from the centre at which the axes cross, and controlled by the principle to be next enunciated. Certain of such planes can always be found in every crystal species for which the ratios of these distances are simpler than in any others. The distances for such particular planes are called the *parameters* of that species, and their values are generally indicated by the letters a b and c, which represent the distances measured severally along the axes x y and z. The ratios of these parameters cannot be expressed (except, of course, where they are equal) by any rational numbers. In this respect they bear some analogy to the chemical equivalents; thus in topaz the parameters are

$$a ; b : c :: 1.: 0.5284 : 0.47698 \text{ approximately.}$$

Now every facette on any topaz crystal must cut the axes at distances, the ratios of which are represented by very simple fractions of these numbers, such as

$$\tfrac{1}{2} \; \tfrac{1}{3} \; \tfrac{1}{4} \text{ \&c.}$$

Hence any facette represented by the form (423) will be parallel to a plane cutting the axes x y z in points whose distances along these axes are found by the proportions $\frac{1}{4}$ a along axis x : $\frac{1}{2}$ b along axis y : $\frac{1}{3}$ c along the axis z. Just as by the law of definite proportions in chemistry, if the equivalent of iron be taken as 28.042 and of oxygen 8.



We have 28.042 ... iron + 8 oxygen, forming one oxide of iron,
2 × 28.042 ... iron + 3 × 8 oxygen forming another do.

4, 2, 3, are the *indices* of that facette, and by the symbol (423) is indicated a group of eight facettes which the law of symmetry of the system requires, and which will, therefore, be found on every complete topaz crystal that carries one of these facettes of that form. The indices are generally indicated by the letters h k l, which, therefore, express rational numbers, while the parameters a b c always (where unequal) express irrational numbers.

The term *morphological axis* was defined as an axis round which the facettes are symmetrically arranged, but which is not necessarily a crystallographic axis (*e.g.*, in the rhombohedron).

The terms *elasticity*, and *axes of elasticity*, were next explained—the former term as implying a power of counter-resistance to any force tending to displace the particles of the crystal (*e.g.*, the compressing force of a blow, or any vibration, such as sound, &c.), the axes of elasticity being those directions in the crystal along which alone the displacement and the counter resistance opposed to it by the crystal, coincide and operate in the same line. A force acting in any other direction is met by counter-forces distributed along the directions of these axes of elasticity.

These definitions being explained, the speaker entered on a short illustration of the laws of crystallographic symmetry in crystals, exhibiting, by means of diagrams, their general morphological relations, so far as was necessary for the subsequent discussion, and pointed out the analogy in complex crystal forms, with the symmetry of certain floral types of form.

The representation of the magnitude and directions of the axes of elasticity in each crystalline system was shown to be possible by means of one of three solid figures.

These, in the case of the octahedral system—from the mutual convertibility of every axis of elasticity in it—were shown to be represented by a sphere. No more was said of this system.

The pyramidal and rhombohedral systems were morphologically associated, by the fact of each having one morphological axis, round which a perfect symmetry reigned—though crystallographic laws ranged the facettes in multiples of 4 round the axis of the first, and of 3 round that of the second. The elasticity in these crystals was represented by a spheroid—prolate (called negative), or oblate (called positive), according as the elasticity was greater in an axial or an equatorial direction.

The prismatic system was represented, as regarded its elasticity, by an ellipsoid, a figure whose axes are all unequal, though rectangular.

The oblique systems, also approximately represented by ellipsoids, were discussed later.

These preliminary statements having been gone through, the speaker entered on the question of the internal molecular arrangement of the crystal, and first briefly reviewed what has been established regarding the physical relations of crystals.

He introduced the name of Professor Grailich, of Vienna, in connection with this subject as one who, with his pupils, had worked over the whole of this large field of research, and had both added much to the facts themselves, and contributed greatly towards the extension of the theoretical views that must be called in to explain them. Some of the more recent of these results of Professor Grailich and his *collaborateur*, Dr. Viktor von Lang, formed the chief subject matter of what followed.

The experiments made hitherto to determine the action of a crystal upon different physical powers, were then reviewed and shown to lead to the general conclusion that there is a very close relation between its morphological (and therefore geometrical) symmetry and its physical properties.

The mechanical elasticity possessed by the crystal in its different directions, may be examined through its cohesion, as indicated by its cleavage, its hardness, its acoustic properties, &c.

The Cleavage, where it exists in a crystal, invariably occurs in the direction of actual or of (in obedience to the great crystallographic law), possible crystal-planes, and is furthermore found to follow planes represented by very simple indices. Besides this relation between the direction in which the crystal splits and its crystallographic form, there is also exhibited a remarkable relation between the degrees of facility in which the substance yields to the cleaving force and the symmetry of the crystal. Such crystals as have equal parameters exhibiting equal facility of cleavage in three directions; such as have two only exhibit equally easy cleavage in two directions.

The Hardness of a crystal also varies on its different facettes, and in different directions on the same facette; and it would seem to be a general law that the greatest hardness is exhibited in a direction and on a plane at right angles to that of cleavage, and that the hardness graduates in its degree in proportion as the plane experimented on tends to coincide with that plane. [Frankenheim, Franz, Grailich and Pekarck.]

The Acoustic characters of a crystal are of an analogous kind. So far as they have been investigated they seem to follow the law of its symmetry. Thus rhombohedral crystals exhibit a triple acoustic symmetry round the morphological axis; and the axes of acoustic elasticity, appear to coincide with the crystallographic axes, *i.e.*, with the intersections of the planes of the fundamental rhombohedron. In some cases the the acoustic characters of the crystal (indicated by the tones produced, as also by the lines into which finely sprinkled particles arrange themselves on a plate of the substance when set vibrating by a violin bow), seem to provide a more subtle means of investigating its elastic structure than the coarser methods of cleavage and hardness [Savart's Experiments—this is especially exemplified in the case of quartz], and crystal acoustics would, in this respect, seem to offer a fertile field for investigation.

In the Magnetic relations of the several parts of a crystal much has been done [Faraday, Plucker, Tyndall and Knoblauch, Grailich and Von Lang]. The results of Professors Tyndall and Knoblauch have been completely confirmed and extended by Grailich and Von Lang, who have added to their results a large series of magne-crystalline determinations, and expressed them by a nomenclature at once comprehensive and concise. Tyndall and Knoblauch have shown that the *intensity* of the dia-magnetic (*i.e.*, equatorially tending), or of the paramagnetic (*i.e.*, axially tending) action, depends on the density of the substance along particular directions. The Vienna observers prove that isomorphous bodies, of similar (dia- or para-) magnetic kinds, have a similar magnetic set; and that those of opposite kinds (one of para- the other of dia-), magnetic nature, comport themselves in ways precisely inverse to one another, and that thus the magnetic *orientation** depends upon the different relative densities of the crystal along different crystallographic directions in it.

There is evidently here a qualitative action (an axial or an equatorial set) which is to be distinguished from the amount of energy, of this setting or directive force, in various directions of the crystal. The former depends on the chemical nature of the substance; the latter solely on the crystalline arrangement.

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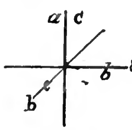
* By orientation is implied the directions in space, relatively to some given directions, of any axes (whether crystallographic, magnetic, acoustic, optic, thermic, or any other). Thus if *a b c* indicate the order of magnitude of the parameters of a trimetric crystal, say one of aragonite, and these are fixed in position, *a*, for instance, being vertical, *b* horizontal, running from right to left, and *c* horizontal, running from front to back, the magnetic orientation of such a crystal is found to be such that the line *c* has the strongest tendency to stand equatorially. Aragonite is a diamagnetic substance, and this, together with its orientation, is designated by Professor Grailich by the symbol δ (c a b) which serves to indicate that the lines of greatest, mean, and least magnetic action are in the order *c a b*, the greatest crystallographic axis being that of least magnetic action (and therefore in this diamagnetic substance also that of greatest molecular density), while the mean and least crystallographic axes are respectively the least and the mean of the magne-crystalline axes. The magnetic orientation of the mineral staurolite, which is a para-magnetic substance, is expressed by π (a c b), so that here the same axis is the direction at once of the greatest crystallographic parameter and of the greatest magnetic action, *i.e.*, the axis of greatest density,



The passage of ELECTRICITY through crystals has been studied [Wiedemann, Senarmont, Knoblauch], and given general results indicative of analogy with those obtained in respect to the transmission of light and heat; while the remarkable development by heating or cooling a crystal, of electric tensions on parts of it morphologically polar to each other [analogue and antilogue poles in terminally-polar, and also central-polar pyro-electricity—Riess and Rose, Karsten Pasteur, &c.] indicates that here, as in magnetically polar substances, and as in the alterations in volume effected by change of temperature, the powers that, so to say, reside in the crystal, and of which its crystalline form is the outward expression, are potent to modify the character and the amount of the tensions induced by the natural forces, which we call electricity, magnetism, and heat, and that those powers have thereby a direction or localisation imposed on them in accordance with the crystalline structure.

HEAT, indeed, must be viewed in more than one *modo operandi.* As *radiant heat;* in the influence of the crystal upon its forward propagation; in its polarisation, absorption, or transmission by (*i.e.* the diathermanence of) the crystal, we have to associate it intimately with light, to view it in short, as light endowed with longer wave length.

As *heat of temperature* (intensity of thermic excitement) we must follow up its action on crystals as an agent causing increase of volume, and therewith inducing a series of concomitant results of the highest interest. In this respect it affords one of the most instructive means at our disposal for the examination of crystal structure. Upon this Professor Grailich and Dr. Viktor Von Lang have brought mathematical analysis to bear; and by showing that the increase of temperature, while changing the relative values of the parameters of a crystal, never alters the irrational character of those parameters, have given to Mitscherlich's beautiful and well-known results a new significance, as Von Lang has also done with Rudberg's investigation on the change in the action of aragonite on light induced by a change of temperature. They have shown that all which is symbolised by the indices (the general symbol ($h\ k\ l$), in a crystal—its symmetry, and therefore its system—remains unaltered: the lengths of the parameters may vary, the inclination of a leaning axis in the oblique systems, may change by change of temperature, but the principle which Grailich establishes as the "Law of Conservation of Zones," remains presiding over the general crystal form, so that it is impossible for a crystal, by the mere agency of a changing temperature, to drift from one system into another.*

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while the mean and least parameters are the directions respectively of the least and mean magne-crystalline set.

* *Thermic axes* are those directions in a crystal along which it is altered, by change of temperature, *only in linear dimensions.* They are are fixed crystallographic directions. Every other crystallographic line that can be drawn through the crystal, changes not in length only, but also in *direction* relatively to these axes.

But it is to Light that we are to look as the most subtle instrument for aiding our reason in scrutinizing the inner nature, or most intimate structure of the molecular system, which we call a crystal. It seems a true prerogative of light to do this. The frequent transparency, and the varied colours in different directions of so many crystals; and the changes which the light is subject to within the crystal, its polarisation, absorption, fluorescence, and the other modifications it undergoes, all point to this most subtle agency as a discriminative power the best adapted for our purpose.

In any of the systems under consideration, the light, on entering a crystal, is, except along certain directions, divided and polarised. The two polarised rays into which it becomes thus divided pursue new and different paths in the crystal, each ray differing from the other in the velocity of its propagation, the crystal retarding the progress of the ray vibrating in one plane, more than it does the ray vibrating in a plane polar to this. The spheroids, or ellipsoids of elasticity before alluded to present admirable geometrical expressions for the degree and relative amounts of the retardations effected, and for the directions of the planes of vibration thus induced by the crystal on the waves of light. Without entering into an explanation of the polarisation of light, or its precise relations to the directions of optical elasticity in the crystal, it was deemed enough to remark that parallel to one direction the sections of the spheroid are circles, and that the (locus of, or) line formed by the consecutive centres of these circles is the morphological as well as the optical axis of all crystals belonging to those systems, whose elasticity can be represented in magnitude and direction by a spheroid (the pyramidal—rhombohedral.) In the prismatic, (and approximately in the clinohedric systems,) whose elasticity is represented by an ellipsoid, there are two circular sections that may be made through the centre of the ellipsoid of elasticity.

In either case lines perpendicular to these circles are the "*optic axes*" of these systems, that is to say, are directions along which a ray goes with only one velocity, and is, therefore, not broken up and polarized; as can be demonstrated by a very simple geometrical construction. The spheroidally elastic systems have, therefore, one optic axis—one only direction along which light passes unchanged (*uniaxial systems*). The systems whose optical elasticity is represented by ellipsoids have two such directions (*biaxial systems*). Moreover, in the latter the plane of these can be readily shown to lie necessarily in the same plane as contains the greatest and least axis of the ellipsoid, *i.e.* the greatest and least axis of elasticity.

The *first mean line* of the optic axes in biaxial crystals, is the line bisecting the *acute* angle formed by the optic axes. In prismatic crystals it is, according as the mean elastic axis is proportionately small or large, either the greatest axis of optical elasticity (optically negative crystal), or the least (an optically positive crystal). The *second mean line* is the axis bisecting the *obtuse* angle, formed by the optic axes. The *law of the prismatic system* regarding the position of the optic axes for different colours, appears to be that the first mean line of the optic axes is the same for every colour, in any given crystal species, but that the angle of the optic axes for each colour may be different. Furthermore, in this system, the axes of optical elasticity, and the *mean lines*, as well as the planes containing the optic axes, coincide in *direction* with the morphological axes.

A beam of polarized light was employed to throw on a screen the stauroscopic phenomena produced in sections of crystals, cut perpendicularly to their optic axes, or to the first mean lines of these; and, by the use of absorbing coloured glasses, the different optic axes were shown to diverge more for some colours than for others in the prismatic system.

Thus Rochelle salt exhibits the centres of the rings or brushes as far more divergent for the red rays than for blue, while in aragonite the converse is the case, though not in so eminent a degree. Mellate of ammonia (and Brookite also) exhibit (as shown by Grailich) the wonderful fact of a divergence of the red rays with the optic axes in the plane $x\ z$, while the optic axes for the blue rays lie in the plane $y\ z$, and those for green, converge at the centre, into a uniaxial system.

The more complicated phenomena exhibited by sections of crystals belonging to the clinohedric (*oblique and anorthic*) systems were next made the subject of illustration, partly by diagrams and in part too by experiment. In the monoclinohedric (the *singly leaning prismatic system*), there is one *plane* of morphological symmetry, and it contains the two crystallographic axes that are inclined to each other; the third is *called* "the axis of symmetry," and is the only true crystallographic axis in this system, that is fixed by morphological conditions. The laws of the distribution *and of the dispersion* of the optic axes in this system, as given by Ångstrom [Beer's Höhere Optik, and Grailich's Translation of Professor Miller's Crystallography], are—1st, That this morphological axis is also one *axis of polarisation** for all colours, without dispersion (*i.e.*, without any divergence of the directions of vibration of the rays for different colours); but that the other two *axes of polarisation* are *rectangular* axes, and are dispersed for different colours differently in the plane of symmetry; and 2ndly [Ångstrom], there are three different cases peculiar to this system possible: gypsum illustrates one of these, that namely, in which the morphological axis is the *second* mean line, and the axes of the blue and red rays stand to each other in the positions of B R B R.

Borax illustrated another of these cases, where the planes of the optic axes for the blue and for the red were so dispersed in the plane of symmetry as to be crossed thus: $\begin{matrix} R & B \\ B & R \end{matrix}$ the morphological axis being the *first mean line.*

Finally, Adularia illustrates the position of the optic axes, in which

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* The use of the word *axis of polarisation* instead of axis of elasticity, in this case is afterwards explained.

the planes containing them, for each colour, cross the plane of symmetry, and the morphological axis is the *second* mean line, the first mean *lines* being dispersed for each colour along the plane of symmetry.

This position is that of $\begin{array}{cc} R & R. \\ B & B. \end{array}$

Thus far the conformity between crystalline symmetry and the distribution of optical elasticity, would *seem* sufficiently near to bring the latter into the same category with the elasticity exhibited by the crystals under the solicitation of less subtle forces. But a nearer view of the phenomena goes far to dispel this expectation.

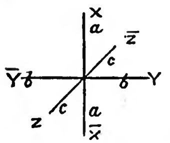
For all the results that had been previously reviewed, an explanation more or less complete may be found in a simple hypothesis; namely this, that the centres of gravity of the crystal-molecules are always arranged in planes which represent either actual or possible facettes: while the relative distances of molecule from molecule are the same in the same direction, but different for different directions. To disturb a molecule so as to move it out of its plane, would be to destroy the integrity of the molecular system. Magnetic orientation would only indicate the lines of greater or less distance between the molecules; increase of thermic intensity (temperature), too, only causes a greater interval between the molecules without altering the crystallographic relations of the planes they lie in—albeit that this increase of interval is different in different directions. These, and, in all probability also, the other as yet less elaborated results of physical experiment, are thus explicable as dependent on the disposition of the mass centres of the molecules, and are so capable of being rendered subordinate to the fundamental law of crystallography.

But the hypothesis above sketched in outline fails to explain a series of facts which Light reveals to us, and which show that the optical properties of a crystal cannot be directly dependent on the arrangement of the molecules. Among these are the following:—

Firstly. The action of increased temperature on a crystal, while it alters the volume of the crystal, does so by changing the values of the parameters, changing thereby too the refractive powers of the substance. As such refractive powers vary generally inversely as the amount of elasticity and directly as the density of the light-transmitting medium varies, we might expect the variations in the parameters and refractive indices and also in the axes of elasticity to follow *pari passu* with the changes in the molecular density in different directions as the crystal becomes expanded: nothing of the kind occurs. The axes of optical elasticity vary indeed (in aragonite for instance), but their orientation is quite dissimilar from that of the altered crystallographic axes; *i. e.*, the directions of the greatest mean and least parameters of the heated crystal, which measure its physical density, entirely differ from the corresponding directions of the greatest mean and least of the new axes of optical elasticity.

2ndly. The speaker had already alluded to the remarkable fact of the crossing of the planes of the optic axes for different colours in mellate of ammonia, brookite [Grailich], and other crystals, such as at a high temperature, glauberite [Brewster, Descloiseaux], and gypsum [Mitscherlich]. It is strange to think that thus in one and the same substance there should co-exist a principal direction of vibration of a different order (*e.g.* the a axis for the red, with the b axis for the violet), for different colours in one and the same direction.

3rdly. Again, the prolific labours of Grailich, and his pupils, have established a most interesting and important law regarding isomorphous substances, viz. this: that the optical similarity between any series of isomorphous bodies frequently diminishes in proportion as the isomorphous chemical substances, *not* common to the different bodies, preponderate over those which are common to them.



The *crystallographic* orientation of the form common to the isomorphous group of the following substances, is expressed by the parameters in the order of their magnitude, *a b c.*

In sulphate of potash the *optical elasticity* is expressed by the symbol a c $\underset{+}{\text{b}}$; in sulphate of ammonia by b a $\underset{+}{\text{c}}$; in chromate of potash, by $\underset{-}{\text{a}}$ c b; the order of the letters indicating in each case the orientation of the different magnitudes, as expressed by the letter (a being greater than b and b than c), and as compared with the directions in space of the original crystallographic parameters *a b c.*

Thus the chemical *substance* entirely overrules the crystalline arrangement in impressing on the crystallized body an optical elasticity; so that we are driven to seek the *fundamental cause of optical character, not in the arrangement of molecules, but in the inner nature,* constitution, structure *of these.*

How, then, are these results, discordant as they are with crystallographic facts, to be reconciled with that general harmony between morphological symmetry and crystallo-optical phenomena, which we have already in part accepted?

To this question, the answer is not readily found. It is, however, the more probable view, that the chemical units of matter (of the nature of which we are profoundly ignorant, and are without even analogies to guide us; but which the atomic theorists, assume to be compounded of atoms in some form of geometrical arrangement, and clustered together in chemical groups), form the basis of the crystalline system: that these are ordinated (with the ether of the mathematical theory) in higher groups which form the ultimate crystal-units-of-mass, or crystal molecules—but that the form and internal arrangement of the molecule bears a close analogy, and a general identity of symmetry, with the form of the crystal of which it is the constituent mass-unit.

The chemical units of mass we can at any rate treat as distinct mechanical units with centres of gravity, and centres of volume (probably distinct), and endowed with powers that cannot be the same in different directions. These then would thus upbuild the molecular units of mass of the crystal, and would be arranged in the interior of those molecules in a manner closely *analogous* to the arrangement of the molecules in the crystal itself; which, as was before stated, may be looked on (at any rate as an illustration to the mind of crystallo-physical facts) as so co-ordinated in direction as that every plane in which a series of the centres of gravity of the molecules lies, must be (an actual or a possible) crystal plane. The mysterious ether assumed and adopted by the undulatory theory plays its part within the molecules and enshrouds its constituent chemical units of mass.

On the one hand, then, axes, such as those of acoustic elasticity, of magnetic induction, or thermic axes, belong to *the crystal as a whole*, depending for their magnitude and direction on the statical condition and the relative arrangement of the molecules *inter se*. The axes of optical elasticity on the other hand will be the axes of elasticity of the *molecule* itself, that is, of the ether within it. But the elasticity of the ether, as exhibited by the velocity with which a ray is propagated in it along its different directions is controlled by the action of the reposing ponderable units (or atoms at rest) on the ether particles in motion. As long as the directions in which the ether is constrained to vibrate, by reason of this perturbing action – its axes of elasticity—are continuous throughout the crystal (as will be the case if the neighbouring molecules are arranged *inter se* in the same symmetry as the ether particles in any one of the molecules, so that the neighbouring molecules do not distract those directions by a perturbing action oblique to theirs,) so long will the influence of the material particles be confined to the exercise of a constraint on the *velocity* of each wave, whatever its length, *i.e.*, colour; but it will not cause any dispersion of these. This condition of coincidence in direction of the crystalline and molecular axes occurs only in the rectangular systems; and in the biaxial ones, therefore, only in the Prismatic system. But the coincidence is confined to *direction*, neither the magnitude of the axes, nor their orientation (as had been shown) presenting any identity.

In the oblique systems there is no reason to assume the obliquity in the molecule as of the same inclination as that of the inclined axes of the crystal. Thus these two sets of oblique axes cannot coincide in direction. Hence the perturbing influence of the ponderable matter operates, by reason of this want of coincidence, in directions other than those of the axes of the individual molecule, and extends its results to the *directions of vibration*, that is to say, effects the *dispersion* of these directions for different colours. In fact, for any light-wave now to traverse the crystal without being broken up, it must take directions neither coincident with the axes of elasticity of the molecule, nor with the crystalline axes. These directions will have different orientation for different colours), and, not being coincident with the thermic axes, will vary with the temperature. They will be rectangular axes—axes of a polarisation-ellipsoid, different for each colour—and will indicate the directions of the principal planes of polarisation, which must be normal to each other, and will also prescribe the limits of the wave surface. They are the resultant *axes of polarisation* before alluded to; they have no statical significance in the crystal.

But this subject enters here into the domain of mathematical analysis. Indeed, though experiment has effected much, and mathematical interpretation more, for the establishment of the laws that control the difficult phenomena exhibited by oblique crystals, the subject is still one in which much has to be done, especially in determining the true axes of elasticity in these crystals.

The last subject touched on, and that briefly, was the impossibility of explaining the growth of a crystal by any architectural view, like that of Haüy, *i.e.* by any view that supposes the crystal developed by the addition, one by one, as it were, of molecules, endowed only with forces acting at minute distances and depositing themselves by virtue of these alone in their position. The facts connected with the growth of a crystal in its mother-liquor point to quite a different conclusion.

The simultaneous production of the corresponding facettes, however minute they may be, and however complicated the crystal form, upon the opposite ends or parts of it, and in the precise positions where symmetry requires them, needs for its explanation something akin to an instinct in the molecules, if we are to suppose them so to deposit themselves, as that their deposition is independent of influences extending at once over the whole crystal, and to the mother liquid investing it.

Hemihedrism points to the same result.*

So too how are the infinite numbers of tesselated crystalline fragments that interpenetrate without any symmetrical orientation so many crystals [Leydolt's Etching Experiments on Quartz, Apophyllite, &c.] without interfering with their general form, to be explained?

Finally, the forms of crystals, say of fluor spar, or of calc spar, from the same mine, are similar, while those met with in a neighbouring locality, where the conditions of deposit were different, are different from these. The typical forms of barytes as found in Cumberland, or in Auvergne, Schemnitz, &c. are different for each locality, and so are often the hornblendes that occur in different rocks. Over large extents of country a mineral (*e.g.* the augite of Southern Tyrol) will present constantly the same combinations of crystalline form. Salt is deposited in cubes from its simple aqueous solution,—in octahedra when that solution contains uric acid; and alum presents the form of the cube when alumina is present in excess.

In all these cases, therefore,—indeed, in every case—the growth of a crystal is an inexplicable thing, so long as we endeavour to trace its cause to powers residing in and confined to the molecules. A crystal, like a plant, is developed in a medium, and as the plant owes the special peculiarities of its individual form, notwithstanding the seemingly perfect freedom of its growth, to special circumstances in the

* Grailich connects hemihedrism (*i.e.* the development of only the alternate planes required by the symmetry of the system of a crystal) with a difference in position of the centres of gravity and centres of volume in the molecules.—(*Private Letter.*)

soil, the air, the weather during that growth; and its general similarity to other plants of its kind, to the organic laws that controul the conditions of its species; so must the crystal be considered as the result of many co-operating influences, including those of the foreign constituents of the mother liquid, those of temperature and other physical conditions, and involving the principle that the molecules, whether those deposited, or those about to become so, affect and are affected by —and that to considerable distances—the whole of the formed and forming crystal matter.

It would be as useless to expect to explain the growth of a crystal without some such view as this, as to endeavour to account for the growth or outward form of a particular plant by the development of a single leaf.

In closing the remarks made in this discourse upon the theoretical bearings of crystallo-physical and especially optical investigation, on our views of the structure and constitution of crystals, the speaker could only allude to the important *practical* services they have already rendered to mineralogy, especially in the able hands of M. Descloiseaux in Paris, who has been enabled to determine several mineralogical species by their means.

[N. S.-M.]

GENERAL MONTHLY MEETING,

Monday, April 4, 1859.

William Pole, Esq., M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Sir Thomas Fowell Buxton, Bart.
John Stuart Glennie, Esq.
Herbert William Hart, Esq.
James Hopgood, Esq.
John Henry Le Marchant, Esq.
Arthur Giles Puller, Esq.
Charles Ratcliffe, Esq. F.S.A. F.L.S.
William Salmon, Esq.

were duly *elected* Members of the Royal Institution.

Thomas B. Baskett, Esq. and
Hall Rokeby Price, Esq.

were *admitted* Members of the Royal Institution.

The following Letter from SIR HENRY HOLLAND, Bart. V.P.R.I. to the Secretary was read:—

"BROOK STREET, *April 4th*, 1859.

"MY DEAR SIR,—As I may be unable to attend the Board of Managers to-day, will you let me ask you to be the medium of a proposal, which I trust will be accepted, as an expression of my earnest desire to contribute, in some degree, to maintain the Royal Institution in the high position it holds among the scientific institutions in England?

"Two or three years ago notice was taken of the expediency of adding to and replacing in parts the apparatus belonging to the Institution. In reference to this important object, I now propose to pay over to the Treasurer for the present year £40, and to continue the same every succeeding year of my life; this sum being appropriated to the purchase or renewal of apparatus, under such arrangement as it may seem good to the present Board of Managers to direct. The only suggestions I venture to offer are, that the Professors of the Royal Institution should have an *official* part in the appropriation, and that the term of *apparatus* should be taken in its largest sense: as including whatever relates to the uses of the Laboratory and to the requirements of the Lectures, which have so greatly tended to sustain the character of the Royal Institution.

Believe me, dear Sir,
Yours very faithfully,
H. HOLLAND.

"P.S.—Let me add a line further to say, that if there were found to be any practical convenience in the immediate application of a larger sum to the purposes in question, I shall gladly bring into the present year the payment of the next also."

THE REV. JOHN BARLOW, M.A,
Secretary R.I. &c.

Whereupon, it was RESOLVED—

That the special thanks of the Members of the Royal Institution be offered to SIR HENRY HOLLAND for a gift so valuable in itself, and so calculated to advance both the scientific character and the popular usefulness of the Institution.

The Special Thanks of the Members were returned to THE DUKE OF NORTHUMBERLAND, for his present of Mr. McLauchlan's Works on "Roman Remains in the North of England;" to M. H. SAINTE-CLAIRE DEVILLE, Hon. M.R.I. for his present of his work "De l'Aluminium;" and to MISS ELIZABETH TWINING, for her presents of her magnificent works, "Illustrations of the Natural Order of Plants," (160 coloured plates, with letter-press descriptions; handsomely bound in two volumes folio); and her "Lectures on Plants."

The Secretary announced that the following arrangements had been made for the Lectures after Easter:—

Seven Lectures on the GENERAL FACTS AND LEADING PRINCIPLES OF GEOLOGICAL SCIENCE, by PROFESSOR JOHN MORRIS.

Seven Lectures on the SEVEN PERIODS OF ART, by AUSTEN HENRY LAYARD, ESQ

Seven Lectures (*in continuation*) on MODERN ITALIAN LITERATURE, by J. P. LACAITA, Esq.

The following PRESENTS were announced, and the thanks of the Members returned for the same:—

FROM

Arts, Society of—Journal for March 1859. 8vo.
Asiatic Society of Bengal—Journal, No. 259. 8vo. 1858.
Astronomical Society, Royal—Monthly Notices, March 1859. 8vo.
Bayerische Akademie der Wissenschaften—Abhandlungen. Band VIII. Abth. 2. 4to. 1858.
Annalen der Königlichen Sternwarte bei München. Band X. 8vo. 1858.
Meteorologische Beobachtungen, München, 1825-37. 8vo. 1857.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for March 1859. 8vo.
Boosey, Messrs. (the Publishers)—The Musical World for March 1859. 4to.
British Architects, Institute of—Proceedings for March 1859. 4to.
Chambers, Geo. F. Esq. M.R.I.—Mitchell's Newspaper Press Directory. 8vo. 1859.
Chemical Society—Quarterly Journal, No. 44. 8vo. 1859.
Deville, H. Sainte-Claire, Hon. M R.I. (the Author)—De l'Aluminium: ses Propriétés, sa Fabrication et ses Applications. 8vo. Paris, 1859.
Editors—The Medical Circular for March 1859. 8vo.
The Practical Mechanic's Journal for March 1859. 4to.
The Journal of Gas-Lighting for March 1859. 4to.
The Mechanics' Magazine for March 1859. 8vo.
The Athenæum for March 1859. 4to.
The Engineer for March 1859. fol.
The Artizan for March 1859. 4to.
Faraday, Professor, D.C.L. F.R.S.—Répertoire de Chimie, par C. Barreswil und A. Wurtz. No. 6. 8vo. Paris 1859.
Akademie der Wissenschaften, Wien—Mathematisch-Naturwissen. Classe: Denkschriften. Band XIV. 4to. 1858. Sitzungs-Berichte. Mai 1857 zu Juni 1858. 8vo.
Almanach, 1858. 12mo.
Festreden bei der feierlichen Uebernahme des Universitätsgebaudes durch die Akademie, Oct. 29, 1857. 4to. 1857.
Jahrbuch der K. K. Central-Anstalt für Meteorologie und Erdmagnetesmus. Band V. 1853. 4to. 1858.
Physikalske Meddelelser ved Adam Arndtsen, &c. 4to. Christiania, 1858.
Memorie della Reale Accademia delle Scienze di Torino. Tomo XVII. 4to. 1858
Report on the Teneriffe Astronomical Experiment of 1856, by Professor C. Piazzi Smyth. 4to. 1858.
Franklin Institute of Pennsylvania—Journal, Vol. XXXVII. No. 2. 8vo. 1859.
Genève, Société de Physique—Mémoires, Tome XIV. Partie 2. 4to. 1858.
Geological Society—Proceedings for March 1859. 8vo.
Geographical Society, Royal—Proceedings, Vol. III. Nos. 1, 2. 8vo. 1859.
Halswell, Edmund, Esq.—Reports of the Visitors of the Hanwell and Colney Lunatic Asylums, for 1859. 8vo.
Newton, Messrs.—London Journal (New Series), for March 1859. 8vo.
Novello, Mr. (the Publisher)—The Musical Times, for March 1859. 4to.
Northumberland, The Duke of, K.G. F.R.S. President R.I.—Henry Mac Lauchlan, Esq. Memoir written during a Survey of the Roman Wall through Northumberland and Cumberland, 1852-54. With Charts. 8vo. and fol. 1858.
Memoir written during a Survey of the Watling Street from the Tees to the Scotch Border, in 1850-1. With Charts. 8vo. and fol. 1852.
Petermann, A. Esq. (the Editor)—Mittheilungen auf dem Gesammtgebiete der Geographie. 1859. Heft 2. 4to, Gotha, 1859.

Photographic Society—Journal, No. 81. 8vo. 1859.
Rachmaninow, M. (*the Author*)—Note sur la Théorie de la Roue Hydraulique en dessous à Aubes Planes. 4to. 1858.
Scharf, George, Esq. (*the Secretary*)—Catalogue of the National Portrait Gallery, Jan. 1, 1859. 8vo.
Twining, Miss Elizabeth (*the Author*)—Illustrations of the Natural Orders of Plants, arranged in Groups; 160 Coloured Plates, with Descriptions. 2 vols. fol. 1849-55.
Short Lectures on Plants. 12mo. 1858.
Yates, James, Esq. F.R.S. M.R.I.—Rev. J. S. Porter: On the Metrical System of Weights and Measures. 8vo. 1859.

Tuesday, April 12, 1859.

Conclusion of the Twelfth Lecture of a Course "On Fossil Mammals,"

BY

Richard Owen, F.R.S.

FULLERIAN PROFESSOR OF PHYSIOLOGY, ROYAL INSTITUTION, ETC.

"*Summary of the Succession in Time and Geographical Distribution of Recent and Fossil Mammalia.*"*

Having thus recounted the chief steps which have been made in the restoration of the extinct quadrupeds of Australia, I conclude the physiological deductions from this class of organic phenomena, and the selection of topics, which seemed to me to be best adapted for the present Fullerian course. In the discourse of to-day, as in the preceding one on South American extinct mammals, you could not fail to be struck with the forcible and cumulative evidence which they supplied in proof of the law that with extinct as with existing mammalia, particular forms were assigned to particular provinces, and that the same forms were restricted to the same provinces at a former geological period as they are at the present day. That period, however, was the more recent tertiary one.

In carrying back the retrospective comparison of existing and extinct mammals to those of the eocene and oolitic strata, in relation to their local distribution, we obtain indications of extensive changes in the relative position of sea and land during those epochs, through the degree of incongruity between the generic forms of the mammalia which then existed in Europe, and any that actually exist on the great natural continent of which Europe now forms part. It would seem, indeed, that the further we penetrate into time for the recovery of extinct mammalia, the further we must go into space to find their

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* Printed by Request.

existing analogues. To match the eocene palæotheres and lophiodons we must bring tapirs from Sumatra or South America; and we must travel to the antipodes for myrmecobians, the nearest living analogue to the amphitheres and spalacotheres of our oolitic strata.

On the problem of the extinction of species I have little to say; and of the more mysterious subject of their coming into being, nothing profitable or to the purpose, at present. As a cause of extinction in times anterior to man, it is most reasonable to assign the chief weight to those gradual changes in the conditions affecting a due supply of sustenance to animals in a state of nature which must have accompanied the slow alternations of land and sea brought about in the æons of geological time. Yet this reasoning is applicable only to land animals; for it is scarcely conceivable that such operations can have affected sea-fishes.

There are characters in land-animals rendering them more obnoxious to extirpating influences, which may explain why so many of the larger species of particular groups have become extinct, whilst smaller species of equal antiquity have survived. In proportion to its bulk is the difficulty of the contest which the animal has to maintain against the surrounding agencies that are ever tending to dissolve the vital bond, and subjugate the living matter to the ordinary chemical and physical forces. Any changes, therefore, in such external agencies as a species may have been originally adapted to exist in, will militate against that existence in a degree proportionate to the size which may characterise the species. If a dry season be gradually prolonged, the large mammal will suffer from the drought sooner than the small one; if such alteration of climate affect the quantity of vegetable food, the bulky herbivore will first feel the effects of stinted nourishment; if new enemies be introduced, the large and conspicuous animal will fall a prey while the smaller kinds conceal themselves and escape. Small quadrupeds, moreover, are more prolific than large ones. Those of the bulk of the mastodons, megatheria, glyptodons, and diprotodons, are uniparous. The actual presence, therefore, of small species of animals in countries where larger species of the same natural families formerly existed, is not the consequence of degeneration—of any gradual diminution of the size—of such species, but is the result of circumstances which may be illustrated by the fable of the "Oak and the Reed;" the smaller and feebler animals have bent and accommodated themselves to changes to which the larger species have succumbed.

That species should become extinct appears, from the abundant evidence of the fact of extinction, to be a law of their existence; whether, however, it be inherent in their own nature, or be relative and dependent on inevitable changes in the conditions and theatre of their existence, is the main subject for consideration. But, admitting extinction as a natural law which has operated from the beginning of life on this planet, it might be expected that some evidence of it should occur in our own time, or within the historical period. Reference has been made to several instances of the extirpation of species, certainly, probably, or possibly, due to the direct agency of man; but this cause avails not in the question of the extinction of species at periods prior to any evidence of human existence; it does not help us in the explanation of the majority of extinctions, as of the races of aquatic invertebrata and vertebrata which have successively passed away.

Within the last century academicians of St. Petersburg and good naturalists have described and given figures of the bony and the perishable parts, including the alimentary canal, of a large and peculiar fucivorous Sirenian—an amphibious animal like the Manatee, which Cuvier classified with his herbivorous *Cetacea*, and called *Stelleria* after its discoverer. This animal inhabited the Siberian shores and the mouths of the great rivers there disemboguing. It is now believed to be extinct, and this extinction seems not to have been due to any special quest and persecution by man. We may discern, in this fact, the operation of changes in physical geography which have, at length, so affected the conditions of existence of the *Stelleria* as to have caused its extinction. Such changes had operated, at an earlier period, to the extinction of the Siberian elephant and rhinoceros of the same regions and latitudes. A future generation of zoologists may have to record the final disappearance of the Arctic buffalo (*Ovibos moschatus*). Fossil remains of *Ovibos* and *Stelleria* show that they were contemporaries of *Elephas primigenius* and *Rhinoceros tichorrhinus*.

The Great Auk (*Alca impennis*, L.) seems to be rapidly verging to extinction. It has not been specially hunted down, like the dodo and dinornis, but by degrees has become more scarce. Some of the geological changes affecting circumstances favourable to the well-being of the *Alca impennis*, have been matters of observation. A friend,* who last year visited Iceland, informs me that the last great auks, known with anything like certainty to have been there seen, were two which were taken in 1844 during a visit made to the high rock called "Eldey," or "Meelsoekten," lying off Cape Reykianes, the S.W. point of Iceland. This is one of three principal rocky islets formerly existing in that direction, of which the one, specially named from this rare bird, 'Geirfugla Sker,' sank to the level of the surface of the sea during a volcanic disturbance in or about the year 1830. Such disappearance of the fit and favourable breeding-places of the *Alca impennis* must form an important element in its decline towards extinction. The numbers of the bones of *Alca impennis* on the shores of Iceland, Greenland, and Denmark, attest the abundance of the bird in former times. A consideration of such instances of modern partial or total extinctions may best throw light, and suggest the truest notions, of the causes of ancient extinctions.

As to the successions, or coming in, of new species, one might speculate on the gradual modifiability of the individual; on the tendency of certain varieties to survive local changes, and thus progressively diverge from an older type; on the production and

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* John Wolley, junr., Esq., F.Z.S.

fertility of monstrous offspring; on the possibility, *e.g.*, of a variety of auk being occasionally hatched with a somewhat longer winglet, and a dwarfed stature; on the probability of such a variety better adapting itself to the changing climate or other conditions than the old type—of such an origin of *Alca tordu, e.g.*;—but to what purpose? Past experience of the chance aims of human fancy, unchecked and unguided by observed facts, shows how widely they have ever glanced away from the gold centre of truth.

Upon the sum of the evidence, which, in the present course I have had the honour to submit to you, I have affirmed that the successive extinction of *Amphitheria, Spalacotheria, Triconodons*, and other mesozoic forms of mammals, has been followed by the introduction of much more numerous, varied, and higher-organised forms of the class, during the tertiary periods.

There are, however, geologists who maintain that this is an assumption, based upon a partial knowledge of the facts. Mere negative evidence, they allege, can never satisfactorily establish the proposition that the mammalian class is of late introduction, nor prevent the conjecture that it may have been as richly represented in secondary as in tertiary times, could we but get evidence of the terrestrial fauna of the oolitic continent. To this objection I have to reply: in the palæozoic strata, which, from their extent and depth, indicate, in the earth's existence as a seat of organic life, a period as prolonged as that which has followed their deposition, no trace of mammals has been observed. It may be conceded that, were mammals peculiar to dry land, such negative evidence would weigh little in producing conviction of their non-existence during the Silurian and Devonian æons, because the explored parts of such strata have been deposited from an ocean, and the chance of finding a terrestrial and air-breathing creature's remains in oceanic deposits is very remote. But, in the present state of the warm-blooded, air-breathing, viviparous class, no genera and species are represented by such numerous and widely dispersed individuals, as those of the order *Cetacea*, which, under the guise of fishes, dwell, and can only live, in the ocean.

In all cetacea the skeleton is well ossified, and the vertebræ are very numerous: the smallest cetaceans would be deemed large amongst land mammals; the largest surpass in bulk any creatures of which we have yet gained cognizance: the hugest ichthyosaur, iguanodon, megalosaur, mammoth, or megathere is a dwarf in comparison with the modern whale of a hundred feet in length.

During the period in which we have proof that *Cetacea* have existed, the evidence in the shape of bones and teeth, which latter enduring characteristics in most of the species are peculiar for their great number in the same individual, must have been abundantly deposited at the bottom of the sea; and as cachalots, grampuses, dolphins, and porpoises are seen gambolling in shoals in deep oceans, far from land, their remains will form the most characteristic evidences of vertebrate life in the strata now in course of formation at the bottom of such oceans. Accordingly, it consists with the known characteristics of the cetacean class to find the marine deposits which fell from seas tenanted, as now, with vertebrates of that high grade, containing the fossil evidences of the order in vast abundance.

The red crag of our eastern counties contains petrified fragments of the skeletons and teeth of various *Cetacea*, in such quantities as to constitute a great part of that source of phosphate of lime for which the red crag is worked for the manufacture of artificial manure. The scanty evidence of *Cetacea* in cretaceous beds seems to indicate a similar period for their beginning as for the soft-scaled cycloid and ctenoid fishes which have superseded the ganoid orders of mesozoic times.

We cannot doubt but that had the genera *Ichthyosaurus*, *Pliosaurus*, or *Plesiosaurus*, been represented by species in the same ocean that was tempested by the Balænodons and Dioplodons of the miocene age, the bones and teeth of those marine reptiles would have testified to their existence as abundantly as they do at a previous epoch in the earth's history. But no fossil relic of an enaliosaur has been found in tertiary strata, and no living enaliosaur has been detected in the present seas: and they are consequently held by competent naturalists to be extinct.

In like manner does such negative evidence weigh with me in proof of the non-existence of marine mammals in the liassic and oolitic times. In the marine deposits of those secondary or mesozoic epochs, the evidence of vertebrates governing the ocean, and preying on inferior marine vertebrates is as abundant as that of air-breathing vertebrates in the tertiary strata; but in the one the fossils are exclusively of the cold-blooded reptilian class, in the other of the warm-blooded mammalian class. The *Enaliosauria*, *Cetiosauria*, and *Crocodilia*, played the same part and fulfilled similar offices in the seas from which the lias and oolites were precipitated, as the *Delphinidæ* and *Balænidæ* did in the tertiary, and still do in the present seas. The unbiassed conclusion from both negative and positive evidence in this matter is, that the *Cetacea* succeeded and superseded the *Enaliosauria*. To the mind that will not accept such conclusion, the stratified oolitic rocks must cease to be monuments or trustworthy records of the condition of life on the earth at that period.

So far, however, as any general conclusion can be deduced from the large sum of evidence above referred to, and contrasted, it is against the doctrine of the Uniformitarian. Organic remains, traced from their earliest known graves, are succeeded, one series by another, to the present period, and never re-appear when once lost sight of in the ascending search. As well might we expect a living Ichthyosaur in the Pacific, as a fossil whale in the Lias: the rule governs as strongly in the retrospect as the prospect. And not only as respects the *Vertebrata*, but the sum of the animal species at each successive geological period has been distinct and peculiar to such period.

Not that the extinction of such forms or species was sudden or simultaneous: the evidences so interpreted have been but local: over the wider field of life at any given epoch, the change has been gradual; and, as it would seem, obedient to some general, but as yet, ill-comprehended law. In regard to animal life, and its assigned work on this planet, there has, however, plainly been an ascent and progress in the main.

Although the mammalia, in regard to the plenary development of the characteristic orders, belong to the Tertiary division of geological time, just as "*Echini* are most common in the superior strata; *Ammonites* in those beneath, and *Producti* with numerous *Encrini* in the lowest"* of the secondary strata, yet the beginnings of the class manifest themselves in the formations of the earlier preceding division of geological time.

No one, save a prepossessed Uniformitarian, would infer from the *Lucina* of the permian, and the *Opis* of the trias, that the Lamellibranchiate Mollusks existed in the same rich variety of development at these periods as during the tertiary and present times; and no prepossession can close the eyes to the fact that the Lamellibranchiate have superseded the Palliobranchiate bivalves.

On negative evidence *Orthisina, Theca, Producta*, or *Spirifer* are believed not to exist in the present seas: neither are the existing genera of siphonated bivalves and univalves deemed to have abounded in permian, triassic or oolitic times. To suspect that they may have then existed, but have hitherto escaped observation, because certain Lamellibranchs with an open mantle, and some holostomatous and asiphonate Gastropods, have left their remains in secondary strata, is not more reasonable, as it seems to me, than to conclnde that the proportion of mammalian life may have been as great in secondary as in tertiary strata, because a few small forms of the lowest orders have made their appearance in triassic and oolitic beds.

Turning from a retrospect into past time to the prospect of time to come,—and I have received more than one inquiry into the amount of prophetic insight imparted by Palæontology—I may crave indulgence for a few words, of more sound, perhaps, than significance. But the reflective mind cannot evade or resist the tendency to speculate on the future course and ultimate fate of vital phenomena in this planet. There seems to have been a time when life was not; there may, therefore, be a period when it will cease to be.

Our most soaring speculations still show a kinship to our nature: we see the element of finality in so much that we have cognizance of, that it must needs mingle with our thoughts, and bias our conclusions on many things.

The end of the world has been presented to man's mind under divers aspects:—as a general conflagration; as the same, preceded by a millennial exaltation of the world to a Paradisiacal state,—the abode of a higher and blessed race of intelligences.

If the guide-post of Palæontology may seem to point to a course ascending to the condition of the latter speculation, it points but a very short way, and in leaving it we find ourselves in a wilderness of con-

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* A generalisation of WILLIAM SMITH'S, justly regarded by EDWARD FORBES and other philosophical Naturalists as of higher importance than the identification of strata by *species*.

jecture, where to try to advance is to find ourselves "in wandering mazes lost."

With much more satisfaction do I return to the legitimate deductions from the phenomena we have had under review.

In the survey which I have taken in the present course of lectures of the genesis, succession, geographical distribution, affinities, and osteology of the mammalian class, if I have succeeded in demonstrating the perfect adaptation of each varying form to the exigencies, and habits, and well-being of the species, I have fulfilled one object which I had in view, viz., to set forth the beneficence and intelligence of the Creative Power.

If I have been able to demonstrate a uniform plan pervading the osteological structure of so many diversified animated beings, I must have enforced, were that necessary, as strong a conviction of the unity of the Creative Cause.

If, in all the striking changes of form and proportion which have passed under review, we could discern only the results of minor modifications of the same few osseous elements,—surely we must be the more strikingly impressed with the wisdom and power of that Cause which could produce so much variety, and at the same time such perfect adaptations and endowments, out of means so simple.

For, in what have those mechanical instruments,—the hands of the ape, the hoofs of the horse, the fins of the whale, the trowels of the mole, the wings of the bat,—so variously formed to obey the behests of volition in denizens of different elements—in what, I say, have they differed from the artificial instruments which we ourselves plan with foresight and calculation for analogous uses, save in their greater complexity, in their perfection, and in the unity and simplicity of the elements which are modified to constitute these several locomotive organs.

Everywhere in organic nature we see the means not only subservient to an end, but that end accomplished by the simplest means. Hence we are compelled to regard the Great Cause of all, not like certain philosophic ancients, as a uniform and quiescent mind, as an all pervading *anima mundi*, but as an active and anticipating intelligence.

By applying the laws of comparative anatomy to the relics of extinct races of animals contained in and characterizing the different strata of the earth's crust, and corresponding with as many epochs in the earth's history, we make an important step in advance of all preceding philosophies, and are able to demonstrate that the same pervading, active, and beneficent intelligence which manifests His power in our times, has also manifested His power in times long anterior to the records of our existence.

But we likewise, by these investigations, gain a still more important truth, viz., that the phenomena of the world do not succeed each other with the mechanical sameness attributed to them in the cycles of the epicurean philosophy; for we are able to demonstrate that the different epochs of the history of the earth were attended with corresponding changes of organic structure; and that, in all these instances of change, the organs, as far as we could comprehend their use, were exactly those best suited to the functions of the being. Hence we not only show intelligence evoking means adapted to the end; but, at successive times and periods, producing a change of mechanism adapted to a change in external conditions. Thus the highest generalizations in the science of organic bodies, like the Newtonian laws of universal matter, lead to the unequivocal conviction of a great First Cause, which is certainly not mechanical.

Unfettered by narrow restrictions,—unchecked by the timid and unworthy fears of mistrustful minds, clinging, in regard to mere physical questions, to beliefs, for which the Author of all truth has been pleased to substitute knowledge,—our science becomes connected with the loftiest of moral speculations; and I know of no topic more fitting to the sentiments with which I desire to conclude the present course.

If I believed,—to use the language of a gifted contemporary, that the imagination, the feelings, the active intellectual powers, bearing on the business of life, and the highest capacities of our nature, were blunted and impaired by the study of physiological and palæontological phenomena, I should then regard our science as little better than a moral sepulchre, in which, like the strong man, we were burying ourselves and those around us in ruins of our own creating.

But surely we must all believe too firmly in the immutable attributes of that Being, in whom all truth, of whatever kind, finds its proper resting-place, to think that the principles of physical and moral truth can ever be in lasting collision.*

[R. O.]

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* Sedgwick, Address to the Geological Society, 1831.

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, April 8, 1859.

Sir Henry Holland, Bart. M.D. F.R.S. in the Chair.

James Paget, Esq. F.R.S.

On the Chronometry of Life.

The design of the discourse was, to illustrate the law that the processes of organic life are regulated with a regard to time, as exact as that which is observed by them in respect of size and weight and quantity of material employed in them; and to show that such an observance of time is characteristic of life, depending essentially on properties inherent in the living bodies themselves, and not on conditions external to them.

Laws indicating the limitation of the organic formative processes, in respect of quantity, are evident in the facts that, in the ordinary conditions in which each living being is found, it and all its parts have appropriate size and weight and mutual proportion. These may, indeed, be modified by the variations of external conditions, or by events that are of the nature of accidents: but the range of possible variations is, in nearly all cases, comparatively narrow; and the boundaries are soon reached, in which changes of external conditions become incompatible with life.

An instance of a corresponding limitation of the organic processes in regard to time might be noted in the natural duration of each creature's life. It is, indeed, not possible to assign any exact number of hours, days, or years, as the constant limit of life in any species; but it is enough to prove a law of time, as limiting the total duration of the organic processes in each, when we see that, in man, and in other species, the length of life, when not diminished by disease or violence, is as fixed as the natural weight or stature is, and that the term of life is marked by changes whose source is inherent in the living body. Watching these changes in the senile degenerations of the human body, it is evident that life does not cease, naturally, because of any change in the external conditions of living; and that the body is not, with advancing years, gradually worn out, as if there were a gradual consumption of a store of material or of force; but that, as, at a set time, the development of the body ceases and growth goes on, and then growth ceases and the body is only maintained in its perfection, so, after a time of such maintenance, the method of the formative processes in the body changes, it slowly degenerates, and through degeneracy dies. And all these stages are, alike, natural, constant, timely; all, too, are together, characteristic of life; there is no such succession of events to be traced in any form of dead matter.

Observance of time may, again, be noted in the formative processes concerned in any of the organs whose changes mark the divisions of a life into its chief periods; *e.g.* in the teeth. Considering merely the conditions in which the teeth of the first set are placed before they project from the gums, there appears no reason why one should be cut before the other, or why they should not all grow with equal speed. Yet while they all grow alike in regard of structure and composition, they have very different rules in regard to the time-rate of their formation. And a yet more marked instance of time-regulation is in the contrast of the teeth of the first set with those of the second. In all essential characters, except those of strength and size, the two sets are much alike; yet there is the widest difference in the rates at which they are formed, and in their duration. The second teeth require as many years for their formation as the first require months; the first live but a few years, the second should live as long as the rest of the body, and sometimes do so. Now there appears nothing to which, as to an efficient cause, this difference can be referred. Its utility and final cause can be discerned; but, as to that which verily determines the rates of growth, and the durations of the teeth, it can only be referred to a First Cause; or it may be said, as of other things subordinate to a First cause, that it depends on some of those properties which each living being inherits from its parents, and through which it results that, in respect of time, as well as of method and quantity, the formative processes in the offspring are a repetition of those of the parent.

The observation of the development and changes of the teeth affords, moreover, an excellent instance of the punctuality with which time-work is regulated in the organic processes, and of the manner in which several different, and really independent, processes, being set to the same time-rate, are made to co-operate to the end of utility in the economy. This is evident in the coincidence of the development of the teeth of the second set, with the removal of those of the first; and in the coincident growth of the jaw, and all its muscles and other apparatus for mastication. In all of these (and the same might be said of any other system of organs in any species) the formation of every part is achieved with an admeasurement of time as precise, and as perfectly designed, as that of its shape, or size, or structure.

For examples of organic processes, adjusted to be complete in definite periods of time, the germination of seeds, and the hatching of eggs, could be cited. In plants, and in cold-blooded animals, the time varies according to temperature, yet not without evidence of a proper time-rate; but among birds, each species has its own time for incubation, as fixed as its other specific characters. In other words, the development of the structures of an egg into those of a young bird, appropriately fitted for life in the open air, is timed to a certain rate of progress; so much work is to be done in so many days, neither more nor less; and on each day its appropriate and special portion of the work. And it is evident that the time occupied in the process is determined by the inherent properties of the egg itself. For if the eggs of any number of species be exposed to the same heat and other conditions, in a hatching machine, then, as surely as the bird produced from each will be like its parents, so surely will it be hatched in the same time as its parents were; in other words, the observance of a specific time-rate in the process of development is as exact as that of any other specific character.

With this observance of time in the development of the young might be noticed that which is, commonly, coincident in the parent. Not to cite the example of all the mammalia, that of pigeons might be taken, in which, during the incubation of their eggs, the crops of the parents are remarkably developed, so that they may be fitted for the secretion of a fluid destined to make the food of their young offspring more suited for their sustenance. The correspondence of these time-rates, observed, at once, in the development of the young pigeons, and in that of the crops of the parents, demonstrates, in both, a provision for chronometry in their organic processes, as clearly as the faces of two clocks, constantly keeping time together, would prove that they both have some apparatus for chronometry within.

Further, the provisions made by parents for their future young afford evidence of the time-regulation of organic processes, in so far as those provisions seem to indicate a reckoning of the time necessary for their completion. For example, certain turtles lay their eggs in hollows made in the sand, leave them there to be hatched, and at the time of hatching return to them for the sake of their yonng. It might be asked, how can these creatures, and many others in similar cases, reckon the passage of time? Most probably, they do not reckon it at all; but just as the timely attained fitness of their organization for preparing and filling their nests impelled them to those acts, so some time-regulated organic processes, taking place in them after the laying of their eggs, bring about at length a new condition, of which a dim consciousness becomes an impulse to them to return to their nests. Such an explanation would involve little guess-work; for changed organization is, manifestly, often the source of impulse to instinctive actions, and the parental organization does commonly change at a rate commensurate with that of the development of the offspring. And a similar reference to chronometric processes in the body, might explain many, though probably not all, other instances in which animals seem to have a power of reckoning the passage of time.

The phenomena of disease, especially in fevers, agues, the consequences of injuries, and many cutaneous eruptions, would afford abundant instances of the observance of time in the organic processes. The vaccine disease might be generally watched as an illustration, being characterized by a vesicle at each place of insertion of the virus, which vesicle begins to appear on the third day, and on the following days passes through changes which are as exactly regulated in time as they are in visible characters. The changes in this vesicle are, moreover, indicative of a coincident succession of events in, or produced by, the virus inserted, which, in the blood of the vaccinated person, increases, and, incorporating itself in the vesicle, reaches its highest development and greatest inoculating power on the eighth day, and then degenerates.

The vaccine might, in most essential points, be regarded as a type of morbid poisons, *i.e.*, of such as are the products of disease. Whether inserted in the blood by inoculation, or bred therein, they commonly occupy definite periods of time in their development, and increase, and decline ; as with a life which is chronometric in all its phases and in its total length.

The instances of morbid poisons would supply examples of organic processes timed to various numbers of days ; and many that are completed in a day, or in given portions of a day, are traceable in the events of sleep and waking in animals (and, perhaps, also in plants), in the daily variations of the pulse, and of breathing, the returns of hunger and thirst, the regulated times of the digestive functions, &c. In man, indeed, consciousness and will are so concerned in some of these functions, that they may seem to lack that regularity which belongs to merely organic processes ; but, if studied generally, and in other species as well as man, they all tell of such processes accomplished with regular measurement of time, and not determined by the external events or conditions of the day or night. Thus, for sleep and waking, and the times of hunger and thirst, man's independence in regard to day and night, or light and darkness, and the habits of different species whose times of activity are, severally, in the early or later day, in twilight or at night, may prove that the earth's diurnal changes are not the causes of these diurnal peculiarities of animal life. The very cause of sleep, and of that which is yet more mysterious, waking, may be unknown ; but they are evidently connected and correlated with those alternating conditions of the structures, of which men, and probably all animals that sleep and wake, are conscious in the sensations of fatigue and of refreshment. The ordinary activities of one portion of the twenty-four hours, the activities, especially, of the muscles and nervous centres and the senses, produce an amount of structural, or chemical, change which is exactly repaired in rest during sleep. In other words, the organic processes for the repair of structures changed (as all structures are) by exercise, are adjusted to such a rate, that, in general, and on an average, in the time of sleep, they may completely restore the parts that are impaired in the activity of waking time. And so, of that replacement of substances in the several structures and in the blood, which is the purpose of feeding ; the processes of digestion and of the several stages of assimilation are so timed, as to accord exactly with the times of daily taking food.

The most minute observances of time in organic processes might be noted in organs that have rhythmic motions, as in hearts and breathing muscles, ciliæ, the vacuoles of certain zoospores, as Volvox and

Gonium. In the Croonian Lecture at the Royal Society, in 1857, the speaker had endeavoured to prove that these and other rhythmic movements in plants, as well as animals, are due to corresponding time-regulated nutrition. He had expressed his belief that "rhythmic motion is an issue of rhythmic nutrition, *i.e.*, of a method of nutrition, in which the acting parts are, at certain periods, raised, with time-regulated progress, to a state of instability of composition, from which they then decline, and in their decline may change their shape and move with a definite velocity, or (as nervous centres) may discharge nerve-force." And this would be still maintained; but whether it were true or not, the rhythmical nutrition of rhythmically acting muscles would be certain. If not a cause, it must be a consequence of such acting; for it is inconceivable that the heart (for example) or the diaphragm, or any other rhythmic muscle, should be free from waste or impairment in its action, or from the necessity of being renovated in its rest. Difference of mode of action could not determine a difference in the immediate effect of action. With long exercise, muscles become so changed that their changed state can be felt in the sensation of weariness, and proved by chemical analysis. But the change thus proved is only the accumulation of the changes wrought in many muscular actions, each of which has contributed a share to the whole amount, just as each revolution of a wheel contributes to the final wearing out. Similarly, every action of the heart, or of the breathing muscles, is attended with change or impairment of composition; but, the impairment is repaired in the next following period of rest or relaxation. In other words, the alternating actions in shortening, and rests in lengthening, of the muscular fibres are correlative and synchronous with their alternating impairments and repairs of composition. The chronometry of such organic processes seems perfect; nutrition is in them divided, as it were, into units; and for each unit, there might be reckoned a unit of time.

Two results of this constant maintenance of rhythmic muscles are remarkable; namely, the enormous power they are capable of exerting, and their freedom from fatigue when only naturally acting. The latter result is proved to depend on the constant maintenance of the muscles, in their timely intervals of rest, by the weariness which is produced in the same muscles when they act otherwise than rhythmically, as in the muscles of respiration when employed in any voluntary movements, or in coughing or other violent respiratory acts.

The instances adduced thus far might supply examples of organic processes adjusted to periods of time varying from the length of human life to less than a second. They were all examples of large classes of facts, from which might be filled up the instances of observance of other and very diverse periods of time; and in all of them, the time-rate is essentially determined, not by external conditions (though these may, in some measure, modify it) but by the inherent properties of the organic bodies themselves.

In another large group of instances, those, namely, in which vital processes are completed, or attain some climax, in a year or in a set portion or season of a year, an independence of external conditions appears less evident. The higher organisms, chiefly by reason of their having in themselves the power of generating heat, may manifest their own time-laws with comparatively little disturbance from without. But in the vegetable world, and in the lower animals, the organic processes are, for the most part, suspended during part of the year, for want, chiefly, of the heat which is a necessary condition of their activity, and the variations of which, for the rest of the year, very greatly affect their rate. Yet even in these, there appear sufficient indications that the times in which the processes of organic life are accomplished depend, essentially, on the specific properties of the several organisms themselves.

Thus, under the same external conditions, each species observes a proper rate of its own. All the plants, for example, of a given locality are subject to the same temperature, and other seasonal conditions; but their rates of living, like those of various eggs placed in the same heat, are different; each reaches the chief events of its life at a certain period of the year. Variations of the seasons may affect all of them; but their method of succession is not thereby changed; they observe the same proportions in the times severally required for their organic processes; and this unaltering proportion indicates a time-rate specific for each, though equally variable in all.

Moreover, among plants, there are numerous examples of varieties, which differ from the general characters of their species only, or chiefly, in regard to the times at which their vital processes are accomplished. Such are the variations that are known as "late," and "early," among flowers or fruits; of which some may be propagated by seeds. [Specimens were shown from two horse-chesnuts growing opposite to one another by the great gate of the Kew Gardens, of which one is, every year, three weeks earlier than the other, in all the processes of its life; and of varieties of Erythronium Dens Canis, from the same gardens, the plants of which, growing side by side in the same bed, always present a similar difference in their times of flowering, &c., though in all other respects alike.] It would be difficult to imagine a variety thus marked only by a peculiarity in rate of living, if temperature, or the influence of the seasons, alone determined the rate of life in the species. The simplest explanation seemed to be that, as there may be varieties in size and number of organs, and almost all the other properties of a species, which together make up its specific character, so there may be also varieties in regard to that time-rate of the processes of organic life which, even by this variability, is indicated as essentially dependent on the properties of the organism itself.

Again, there are some species in which there seems to exist a singular independence of external conditions. Instances of this are found in the Eriogaster lanestris, and the other moths mentioned by Kirby and Spence. If pupæ, formed in June or July, be "selected of the same size, and exposed to the same temperature, the greater number of them will disclose the perfect insect in the February following; some not till the February of the year ensuing, and the remainder not before the same month in the third year." (Vol. iii. p. 264.) The design of so singular an arrangement is, as they observe, to secure that insects, coming into active life in February or March, may not be utterly exterminated by the ungenial weather of a single season, or of two such seasons in succession: but the very cause of the differences among the pupæ, in their relations to the same external conditions, must be in their own properties.

A somewhat similar instance of apparent complete likeness among seeds in all respects except that of time, is in those of a Begonia, which, if taken from the same pod, and all planted together, and all kept in the same conditions, will germinate, some in a day, some at the end of a year, and some at various intermediate times.

To these indications of self-dependent time-rates in the lower organisms, might be added all the facts of another class, which show punctuality in the adjustment of several distinct processes. Scarcely an event of life could be watched which would not show it.

[The instance by which it was illustrated, was that of a Saxifraga, whose stamens, like those of Parnassia, arrive at their very maturity, not all together, but in pairs, and in pairs bend upon the pistil, each pair rising again before another pair bends down.]

And, lastly, the influence of temperature on the rate of the formative processes in the lower organisms is scarcely, or not always, greater than that of nutriment and other external conditions, is on their quantity. The occurrence of "good" and "bad" seasons indicates the latter influence, as that of "early" and "late" seasons does the former. Plants of the same species growing, some in an arid, others in a rich soil, differ exceedingly in size; the one are stunted, the others exuberant; here nutriment modifies the quantity of formation as, in other instances, varieties of heat will modify its rate. But this being so, it may be held that as a certain average size or quantity of growth is a characteristic of each species, and an issue of its very nature, so is a certain average time or rate of growth. Quantity and rate may alike be varied by external circumstances, but the standard or medium of both, as well as the limits of variation compatible with life, are determined by the natural and inherent properties of the species.

Whatever evidence these and the like facts might supply, that, in connexion with the seasons, the time-rates of the organic processes in the lower organisms are essentially dependent on the inherent properties of each organism, similar evidence might be adduced for the case of the higher, and especially the warm-blooded animals. In these the varieties of seasons have less influence in modifying the rate, as well as all the other measures, of life; and the less influence, the higher the species, or the degree of development of the individual. Moreover, there are in birds some instances in which organic processes have a tendency to observe certain times of the year even when the seasons are changed. Thus among those brought from Australia to this country, some of the parakeets breed here in December; the black swan sometimes breeds in November as well as in May; the New

Holland Cereopsis-goose has bred at the Zoological Gardens every February for five or six years.* Among migratory birds, also, it has been observed that when they are kept in confinement, and removed from all the circumstances that might be supposed to induce or necessitate their journeys, they yet become restless at the return of the season for their migration.

In these and the like facts there appear indications of a chronometry in the organic processes of warm-blooded animals, which corresponds with that of the seasons, but is essentially independent. And, if it be so, these might form a group of facts, in addition to those of the diurnal variations of the organic processes, in which vital changes are set to the same rules of time as changes of the surface of the earth, yet have their own proper laws; and concerning which it might be said, that the cycles of life, and of the earth, do, indeed, correspond, but only as concentric circles do, which are drawn round one centre, but are not connected, except in design and mutual fitness.

But, however this might be, all the instances of time-regulation cited in the discourse (all being examples of large groups of facts), would seem sufficient to prove, that the observance of time in organic processes is as exact and as universal as that of any other measure; that each species has a certain time-rate for the processes of its life, variable, but not determined, by external conditions; and that the several phenomena commonly studied as the periodicities of organic life, are only prominent instances of the law which it was the object of the discourse to illustrate.

[J. P.]

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* Mr. Sclater, to whom the speaker was indebted for this fact, supplied also dates which tend to prove that the Australian parakeets, in this country, breed less often in December than in the months from May to September, inclusive; but even a minority of instances of the observance of times, and a general tendency towards it, when the force of such external conditions as those of the seasons is strong against it, is good evidence that inherent properties are the mainsprings determining the rates of life.

WEEKLY EVENING MEETING,

Friday, April 15, 1859.

Sir Roderick I. Murchison, D.C.L. F.R.S. Vice-President, in the Chair

Sir Charles Lyell, M.A. D.C.L. F.R.S.

On the Consolidation of Lava on Steep Slopes, and on the Origin of the Conical Form of Volcanoes.

During two recent excursions made in the autumns of 1857 and 1858 to Mount Etna, Sir C. Lyell had an opportunity of examining sections of lava-currents of known date, which had descended steep slopes, and had consolidated thereon in tabular and stony masses, the inclination of which sometimes exceeded 30°. This fact has an important bearing on the theory of "craters of elevation," it having been affirmed by geologists of high authority, that when lavas congeal on a declivity exceeding 5° or 6°, they never form continuous beds of compact stone, but consist entirely of scoriaceous and fragmentary materials.

The origin of such mountains as Etna and Vesuvius had of old been referred to the cumulative effect of a long series of ordinary eruptions, it being seen that reiterated showers of ashes and streams of lava were often poured out from a permanent central vent. This opinion was advocated by Mr. Scrope in his work on volcanoes in 1825, and by Sir C. Lyell in his *Principles of Geology*, after his exploration of Vesuvius and Etna in 1828; both authors considering the injection from below of melted matter, in the shape of dykes, as part of the cone-making process.

But in place of this simple explanation of the phenomena, Von Buch substituted the following hypothesis: that a vast thickness of horizontal or nearly horizontal sheets of lava and scoriæ, having been first deposited, an expansive force operating from below, exerts a pressure both upwards and outwards, from a central axis towards all points of the compass, so as suddenly to uplift the whole stratified mass, making it assume a conical form; giving rise at the same time, in many cases, to a wide and deep circular opening at the top of the cone, an opening called a "crater of elevation."

In all great volcanoes of which sections can be obtained, there are some layers of compact stone, inclined at angles of 10°, 20°, and sometimes much higher angles, and these beds are often among the uppermost, or last formed of the whole series. Hence it was logically inferred, when once the law above laid down respecting the consolidation of melted matter had been accepted, that every mountain containing such inclined and compact layers, must owe its conical form almost exclusively to the development of mechanical force exerted at the close of the volcanic operations, or after all the alternating lavas and scoriæ were heaped up. The hypothesis of a sudden and violent movement was perhaps the more readily embraced, because it relieved its advocates from the necessity of making unlimited drafts on past time, thousands of centuries being required if lofty cones, like Mount Etna, are to be built up by successive eruptions of ordinary intensity. The magnitude also of certain craters or "calderas" (implying, probably, one or more great explosions, followed by aqueous erosion), and the occasional steepness of the dips of certain lavas, beyond that which is found on the flanks of ordinary cones, (many of which might have been assigned to local dislocation,) afforded additional arguments in favour of the new hypothesis. The lecturer then gave a rapid review of the controversy respecting "craters of elevation," stating the objections made to it by English and Continental writers, including the late M. Constant Prevost; and he went on to observe that the principal object of this discourse was to show that the law laid down by M. E. de Beaumont, and by the late M. Dufrénoy, as governing the cooling and solidification of lava currents, on steep slopes, has no foundation in fact. Signor Scacchi had already, in 1855, seen and described a compact stony lava which in that year had flowed down the flanks of Vesuvius from near the margin of the great crater to the base of the cone in the Atrio del Cavallo, having a thickness of from 1½ in the upper to 4½ in its lower part, and dipping at angles varying from 32° to 38°. The interior of this current was laid open to view by a rare accident, namely, the sinking down in the same year (1855) of a certain portion of the north flank of the cone, whereby one side of the new lava stream was engulfed, and a section of the remainder rendered visible. Although this current had cooled on an average declivity of 35°, it was as compact and as free from vesicles as many lavas which have congealed on level ground at the foot of Vesuvius.*

The first exemplification of a similarly inclined stony lava of known date on Mount Etna, described by the lecturer, and of which a pictorial representation was given, occurs in a ravine called the Cava Grande, near Milo, about 17 miles north of Catania, and 7 from the sea, above the level of which it is elevated about 2000 feet. A branch of the lava-current of 1689 descending from the Val del Bove, cascaded over the right bank of that ravine 220 feet high, and on cooling, formed a tabular mass more than 16 feet in thickness, inclined at an average angle of about 35°, and concealing the face of the precipice for a width of about 400 feet. The internal structure of this new lava has been exposed to view by the falling down and partial removal of its scoria-

* This section, seen by Signor Scacchi in 1855, was looked for by Sir C. Lyell, in company with Signor Scacchi in 1857, and found to be totally buried and concealed by the lavas poured out in the early part of that year.

ceous crust on the left side; a removal caused by the annual waste of the steep bank of the ravine produced by the action of rain, and the torrent which flows at the bottom. The ravine intersects alternating beds of tuff, scoria, and lava, slightly inclined to the east, or seawards, being a series of the older products of Etna. This new and steeply inclined lava consists of three parallel layers, an upper fragmentary and scoriaceous mass about 8 feet thick; a central stony layer, 5 feet thick; and a lower bed consisting of thin strata of fragmentary scoriæ, in all three feet thick, but the bottom part of which is not visible. The compact central portion is a dolerite or trachi-dolerite, containing crystals of felspar with some olivine, and is of the ordinary specific gravity of trap. It is divided by joints, 9 or 10 feet apart, so that among the fragments detached by denudation, and strewed over the sloping bank and bottom of the ravine, are angular masses of huge size, with a fracture like that of many ancient igneous rocks. The normal thickness of this bed of compact dolerite is 5 feet, where it dips at 32° and 35°, but near the top where it first enters the ravine, and where the inclination increases to 45° and 47°, the thickness is reduced to one-half or 2½ feet; yet when dipping at 47°, it is still not only stony and compact, but there is no breach whatever of continuity in the mass, and not more joints than in the less inclined portion. This branch of the lava of 1689, which has given a new facing to part of the right bank of the Cava Grande, exhibits but slight inequalities on its surface, appearing almost even when contrasted with the main current of the same date, from the surface of which many parallel and longitudinal ridges project prominently, sometimes 40 feet above their base, and with very steep sides sloping at angles of from 35° to 70°. The dip of the main current is between 10° and 16° east. From this and other examples, it is inferred that wherever the slopes are excessive (between 25° and 45°) the surfaces of the cooling lavas are less rugged than where the melted matter has congealed on more level ground.

Allusion was next made to some lavas which have cascaded over sea-cliffs 500 feet high, between Aci Reale and Santa Tecla. One of these at a place called the Scalazza of Aci Reale, exhibits a longitudinal section of a tabular mass of stony rock 20 feet thick, inclined at angles of 23° and 29°, which is connected uninterruptedly with the main body of the same lava resting on the gently sloping platform above, of which the sea-cliff is the abrupt termination. The above-mentioned highly inclined stony lava is covered as usual by a parallel layer of scoriæ (in this case 12 feet thick,) and its base consists of another bed of scoriæ of slight thickness.

Several other sections of modern lavas of Etna, which have not been disturbed in their position since the day of their formation, and which are inclined at angles exceeding 30° were then enumerated. For a detailed account of those, reference was given to a paper by the lecturer, recently published in the *Philosophical Transactions* (Part 2 for 1858, p. 703). Among them is a current, inclined at 35°, occurring in the Cava Secca, a deep valley near Zafarana; and another reposing on the face of the great precipice at the head of the Val del Bove, under the sunk space called "The Cisterna." This remarkable current has a mean inclination of 35°, and the central stony layer is seven feet thick. Above and below are parallel overlying and underlying masses of scoriæ five and seven feet thick respectively. The flanks of the stream have been undermined and denuded by that constant waste which makes the innumerable dikes to stand out in relief on all the precipices surrounding the Val del Bove. Perhaps, also, in this instance, the lateral excavation of the lava may have been assisted by a rush of water like that of 1755, commonly called Recupero's flood, which descended the same precipice, the "Balzo di Trifoglietto." Suggestions were then offered on the probable cause of that singular inundation, which swept in a few hours from near the summit of Etna through the Val del Bove to the sea. The Canon Recupero traced its course, a few months after the event, by following the line of sand and boulders which it had left in its track; and calculated that the volume of water was so great, that, had all the snows of the top of Etna been melted instantaneously, they could not have furnished enough water for such a deluge. He, therefore, concluded that the water was vomited forth from the summit-crater itself. Sir C. Lyell conjectures that there may have been masses of ice in the cone during the eruption which is recorded to have accompanied the flood of 1755, and the ice may have been suddenly melted by hot vapours and injected lava. In support of this hypothesis, he mentioned his having ascertained the continued existence, in 1858, of the same glacier which was alluded to by him, in the first edition of his *Principles of Geology*, as occurring at the base of the cone, and which had been quarried before 1828. This mass of ice the Catanians again quarried, four years ago, to a depth of four feet, without reaching the bottom. It is covered by ten feet of volcanic sand, and this again by lava. The tale of the mountaineers, who assured Recupero that the water of the flood of 1755 was hot, may have been correct, if the origin here assigned to it be true.

Some account was next given of the lavas of 1852-53, which were still hot, and emitting columns of vapour at the time of Sir C. Lyell's last visit. They were more voluminous, perhaps, than any ever poured forth from Etna in historical times, except those of 1669, which overflowed a great part of the city of Catania. The narrative of the people of Zafarana, of the manner in which the frontal wall of lava, 30 feet high, and inclined at an angle of 37°, had crept slowly over green pastures and vineyards, and overwhelmed habitations in the suburbs of that town, reminded Sir Charles of similar tales which he had listened to seven weeks before in the Alpine valley of Zermatt, where the great glacier had, in the preceding spring, been pushing onwards with irresistible force, an equally steep mound of stony fragments, forming the frontal moraine by which green meadows, gardens, and chalets had been overwhelmed. A description was then given of the changes brought about by the lavas of 1852-53 in the scenery of the Val del Bove, and in that of the lower Valley of Calanna, in the interval since 1828, when the lecturer first visited Etna. These changes are very striking; the fresh currents having run from the head of the Val del Bove both in a north-east and in a south-east direction for a distance of six miles, with a breadth in each case of two miles, and having been piled up one over the other in some places (as at the Portella of Calanna) to a depth of more than 100 feet. The longitudinal and nearly parallel ridges on the surface of this new lava field are from 20 to 70 feet high; and there is now a black and monotonous wilderness in many places, where, in 1828, there were verdant forests.

One branch of this lava of 1852 cascaded over a precipitous declivity 500 feet high, at the head of the Valley of Calanna, and consolidated at angles of 35°, 45°, and even 49°. The scoriaceous crust having been partially washed off, the surface of a continuous crystalline and stony mass is exposed to view, only moderately vesicular, and having the steep inclinations above alluded to. This same current rests on an older one, that of 1819, which passed down the same steep cliff, and which has at some points a dip of more than 40°.

The structure of the nucleus of Etna, or of the oldest visible part of the volcano, as shown in sections in the Val del Bove, was next treated of, and the doctrine of a double axis of eruption deduced from the varying dip of the beds. The oldest of these beds, composed of trachyte and trachytic tuff at the base of the lofty precipices at the head of the Val del Bove, are inclined at angles of 20° to 30° to the north-west, or towards the present great central axis of eruption. Other similar beds, two miles to the south-east, in the hill of Zoccolaro, dip in an opposite direction; while in the north and south escarpments of the Val del Bove, the dips are north-east and south-east respectively. On the whole, there is a quâquâversal dip away from some point situated in the centre of the area called the Piano di Trifoglietto. Here a permanent axis of eruption seems to have existed for ages in the earlier history of Etna, for which the name of the axis of Trifoglietto is proposed; while the modern centre of eruption, that now in activity, may be called the axis of Mongibello. The two axes, which are three miles distant the one from the other, were illustrated by an ideal section through the whole of Etna, passing from west to east through the Val del Bove, or from Bronte to Zafarana.* Touching the relative age of the two cones, it is suggested that the upper portion only of that of Mongibello may be newer than the cone of Trifoglietto. The latter, when it became dormant, was entirely overwhelmed and buried under the upper and more modern lavas of the greater cone. This doctrine of two centres, originally hinted at by the late Mario Gemmellaro, had been worked out (unknown to Sir C. Lyell at the time of his visit in 1857) by Baron Sartorius von Waltershausen, and has been since supported in the fifth and sixth parts of his great work, called "The Atlas of Etna," both by arguments founded on

* *See* Phil. Trans., Part 2, for 1858, p. 740.

the quâquâversal dip of the beds as above explained, and by the convergence of a certain class of greenstone dikes towards the axis of Trifoglietto. Von Waltershausen has also shown that the lavas and volcanic formations in the middle of the precipices at the head of the Val del Bove, from the Serra Giannicola to the Rocca del Corvo inclusive, are horizontal and unconformable to the highly inclined beds in the lower half of the same precipice; or where some of the superior beds are inclined, they dip in such directions as would imply that they slope away from the higher parts of Mongibello. All these facts were fully confirmed by Sir C. Lyell in his explorations of these lofty precipices in 1857 and 1858.

The double axis of Etna was then compared to the twofold axis of the volcanic island of Madeira, as observed by Mr. G. Hartung, and the lecturer, in 1853-54; and it is observed that the admission of this theory is entirely adverse to the hypothesis of craters of elevation, for it implies that the force of upheaval plays no more than a subordinate part in the cone-making process. Although one cone of eruption may envelope and bury an adjoining cone of eruption; it is obviously impossible that one cone of upheaval should mantle round and overwhelm another cone of upheaval.

It is, however, conceded that in some parts of the central nucleus of Etna, there are lavas which dip at higher angles than can with any probability be ascribed to the original steepness of the sloping flanks of an active cone. Some of these instances are regarded as exceptional, and due to local disturbance; others may be connected with the abundance of fissures, often of great width, which have been filled with lava in the central nucleus of the mountain, forming dikes which are much less frequent and sometimes entirely wanting at points remote from the centre. The injection of so much liquid matter into countless rents may imply the gradual tumefaction and distension of the volcanic mass, and may have been attended by the tilting of the beds, causing them to slope away at steeper angles than before, from the axis of eruption. But instead of ascribing to this mechanical force, as many have done, nearly all, or about four-fifths of the whole dip, Sir C. Lyell considers that about one-fifth may, with more probability, be assigned as the effect of such movements.

The alleged parallelism and uniformity of thickness in the volcanic beds of the Val del Bove, when traced over wide areas, was next considered; and the lecturer remarked that neither in the northern nor southern escarpments of the great valley, could he verify the existence of such parallelism. Drawings exemplifying a marked deviation from it were exhibited; these views being taken from the northern and southern cliffs of the Val del Bove.

The discovery that lava is capable of forming continuous and tabular masses of crystalline rock on steep slopes, often exceeding 30°, enables us henceforth to dispense with that paroxysmal and terminal upheaval, which the advocates of "craters of elevation" legitimately inferred from their premises, for it was as necessary for them, so long as the volcanic beds were assumed to have been originally horizontal, to ascribe the whole elevation to a force acting from below, as it would have been if the uppermost layers of each volcanic mountain could be assumed to be of marine origin. In opposition to such a doctrine, Sir C. Lyell maintains that mechanical force has nowhere played such a dominant part in the cone-making process as to warrant our applying any other term save that of "cones of eruption" to volcanic mountains in general.

In conclusion, the lecturer gave a brief sketch of the series of geological events which he supposed to have occurred on the site of Etna since the time of the earliest eruptions, events which may have required thousands of centuries for their development. The first eruptions are believed to have been submarine, occurring probably in a bay of the sea, which was gradually converted into land by the outpouring of lava and scoriæ, as well as by a slow and simultaneous upheaval of the whole territory. The basalts, and other igneous products of the Cyclopean Islands were formed contemporaneously in the same sea, the molluscous fauna of which approached very near to that now inhabiting the Mediterranean; so much so, that about nineteen-twentieths of the fossil species of the sub-Etnean tertiary strata still live in the adjoining seas. Hence, as that part of Etna which is of subaerial origin is newer than such fossils, the age of the mountain is proved to be, geologically speaking, extremely modern. During the period when the volcano was slowly built up, a movement of upheaval was gradually converting tracts of the neighbouring bed of the sea into land, and causing the oldest volcanic and associated sedimentary strata to rise, until they reached eventually, a height 1200 feet (and perhaps more) above the sea-level. At the same time the old coast-line, together with the alluvial deposits of rivers, was upraised, and inland cliffs and terraces formed at successive heights. The remains of elephants, and other quadrupeds, some of extinct species, are found in these old and upraised alluviums. Fossil leaves of terrestrial plants also, such as the laurel, myrtle, and pistachio, of species indigenous to Sicily, have been detected in the oldest subaerial tuffs. At first the cone of Trifoglietto, and probably the lower part of the cone of Mongibello, was built up; still later the cone last-mentioned, becoming the sole centre of activity, overwhelmed the eastern cone, and finally underwent in itself various transformations, including the truncation of its summit, and the formation of the Val del Bove on its eastern flank. Lastly, the phase of lateral eruptions began, which still continues in full vigour.

[C. L.]

ANNUAL MEETING,

Monday, May 2, 1859.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors was read, and adopted.

The statement of Sums Received shows a steady and gradual increase in the yearly income. The amount of Annual Contributions of Members and Subscribers in 1858 amounted to £2109. 9*s.* 0*d.*, being more than had been received in any previous year: the Receipts from Subscriptions to Lectures were £739. 14*s.* 6*d.* The total Annual Income amounted to £5060. 8*s.* 8*d.*

On December 31, 1858, the Funded Property was £25,831. 1*s.* 8*d.*; and the Balance £927, with Six Exchequer Bills of £100 each. There were no Liabilities.

A List of Books Presented accompanies the Report, amounting in number to 132 volumes; making, with those purchased by the Managers and Patrons, a total of 712 volumes (including Periodicals) added to the Library in the year.

Thanks were voted to the President, Treasurer, and Secretary, and to the Committees of Managers and Visitors, for their services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:—

President—The Duke of Northumberland, K.G. F.R.S.
Treasurer—William Pole, Esq. M.A. F.R.S.
Secretary—Rev. John Barlow, M.A. F.R.S.

Managers.

John J. Bigsby, M.D. F.G.S.
Sir Benjamin Collins Brodie, Bart. D.C.L. President R.S.
Edmund Beckett Denison, Esq. Q.C.
Col. George Everest, F.R.S.
Sir Charles Hamilton, Bart. C.B.
Sir H. Holland, Bt. M.D. F.R.S. F.G.S.
Henry Bence Jones, M.D. F.R.S.
John Percy, M.D. F.R.S.
Frederick Pollock, Esq. M.A.
Lewis Powell, M.D.
Robert P. Roupell, Esq. M.A. Q.C.
Rev. William Taylor, F.R.S.
John Webster, M.D. F.R.S.
The Lord Wensleydale.
Col. Philip James Yorke, F.R.S.

VISITORS.

John Charles Burgoyne, Esq.
Rev. Charles John Fynes Clinton, M.A.
C. Wentworth Dilke, jun. Esq.
John George Dodson, Esq. M.P.
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Thomas Lee, Esq.
John Lubbock, Esq. F.R.S.
Charles Lyall, Esq.
Edmund Macrory, Esq. M.A.
Sir Edwin Pearson, M.A. F.R.S.
Henry Pemberton, Esq.
John Bell Sedgwick, Esq.

WEEKLY EVENING MEETING,

Friday, May 6, 1859.

THE LORD WENSLEYDALE, Vice-President, in the Chair.

ROBERT DRUITT, ESQ.

MEMBER OF THE ROYAL COLLEGE OF PHYSICIANS, LONDON.

On Houses in Relation to Health.

THE speaker having alluded to the sickness, bereavement, and ruinous expense which sometimes ensue from the wrong choice of houses by private individuals, and to the disorders liable to be diffused amongst all classes, from the unhealthy dwellings of the poor, proceeded to consider the subject of houses and their influence on health, under three heads. Under the first, he treated of deficiencies of air, light, warmth, and dryness, and of the maladies of degeneration to which they give rise, of which consumption and scrofula are types; under the second, he spoke of the common typhoid fever of this country, and of choleraic disorders, and of their origin in defective house drainage; and under the third, he discussed the conditions which give intensity and power of propagation to certain diseases, such as scarlatina and diphtherite.

Amongst the details noticed under the first head, he observed that the ground on which a house is built should have the qualities of porosity and firmness; porosity is required in order that all water charged with organic debris, which happens to penetrate it, may pass onwards and undergo that rapid oxydation which is so happily effected by the London gravel. Wherever the soil is deficient in this quality, or where beds of gravel or sand come in contact with beds of clay, a thorough subsoil drainage is as essential for the health of man, as it is for the growth of sweet herbage. Spots can be pointed out in which the subsoil is swampy, and where fever has prevailed in consequence. Moreover, the land on which houses are being built around London, is sometimes raised artificially by what is called made-earth: that is to say, on a low, wet spot, quite undrained, are heaped all sorts of rubbish, road scrapings, mud, and refuse, mixed with organic debris; and over this the houses are built. Besides, the excavation of sweet wholesome gravel, and the filling in the vacuity with rubbish, has long been prevalent at the west of London, and was much to be condemned; and an instance was quoted of a house, whose rental was £400 per annum, built over a laystall of the last century, that is a pit where every kind of impurity was deposited, and now filled with black mould mixed with sheep bones. This earth, when dried and analyzed, yielded 10 per cent. of organic matter, and was as unwholesome to build on as an old churchyard.

Want of firmness might be remedied by a solid floor of concrete, after the Roman manner, which would enable houses to be built with less chance of settlements and cracks, which are causes of unhealthiness, not merely by allowing cold and damp to enter, but by permitting currents of fetid air to come from unknown and distant sources. Thin walls, too, render a house not only extremely cold, but unwarmable in winter.

Passing over the grouping and position of houses, and the width of streets, which ought to be regulated by the height of the sun in winter, we come to the internal arrangements, and air supply. The last may be provided by some special ventilating apparatus; or may be left to take care of itself; but even if left to itself, it will be prudent to see that the basement doors, and other chief apertures, are not near any source of contamination; moreover, great caution should be exercised in roofing in yards and areas with glass, as is often done, because the glass may cover over some sources of effluvia, and bring them into the house. This has been the cause of fever.

The supply of air to the apartments should be large; diffused and not in perceptible draughts; and warmed; and should be so contrived as not to attract attention: otherwise persons who are afraid of pure air, especially servants and the poor, will speedily close up the apertures. The plan suggested by the Commissioners on Warming and Ventilation was exhibited, and described as fulfilling most of these conditions—inasmuch, as the air is warmed by the heat otherwise wasted by the chimney, and is introduced in ascending currents; and care is taken that there is a channel of escape separate from the chimney, and that the upper part of the apartments should be the coolest. Any plan for bringing currents of cold air to the level of the feet, or of the bed, is to be condemned. Nothing can be a better evidence of the carelessness of our present system of air-supply to houses, than the fact, that much of it comes down the chimneys when there is no fire, bringing with it abundance of soot, and many, possibly unwholesome, products of imperfect combustion of organic matter.

A free supply of pure air is the *sine qua non* to persons who lead in-door lives; inasmuch as the direct influence of close bedroom air in producing scrofula and consumption has been proved incontestibly by Carmichael of Dublin; and later, by Dr. Guy. But till people have a sufficiency of food and clothing, it is a mockery to speak of pure air. Man's instinct chooses foulness before hunger; when the contact of pure air with the lungs, and of pure water with the skin, creates an appetite for food which he has no means of satisfying.

Further researches on the composition of house air are to be desired. It is evident that the amount of carbonic acid represents that of the most volatile, and probably that of the most innoxious product of the presence of living beings, and consequently the *minimum* of atmospheric impurity. The speaker had made several determinations which showed how rapidly the quantity fluctuated according to the amount of ventilation and the state of the wind. For example, in a small room in May's Buildings, Brick-street, inhabited by two adults and three children—

	Volumes of Carbonic Acid in 10,000 vols. of air.
April 20th. Noon; family dining	13·74
„ 21st. „ „ „	9·19
„ 24th. Room empty, door open	3·34
„ 20th. All night	15·97
„ 21st. „	15·10
„ 22nd. „	12·27
In the Infant School-room, Curzon Schools, Mayfair, room crowded, with a window open at each end . . .	9·46
In a bedroom, unoccupied, but with the doors and windows closed all day, the quantity fluctuated between 5 and 7 volumes per 10,000	

The list of maladies arising from cold, damp, airless and sunless dwellings, is a long one, and is identical with the maladies caused by hunger, depraved nutrition, solitude, and grief; moreover, the effects of dark, monotonous, and decayed dwellings upon the mind must not be overlooked, inasmuch as they produce a direct temptation to indulge in spirituous liquors.

Under the second head, the speaker alluded to the great number of modes in which a house may receive poisonous vapours from sewers. If the house drainage has not been revised during the last ten years, there may probably be every variety of imperfect and dilapidated drainage; wells that once supplied a family with water, may have been converted into dead wells and receptacles of filth; and even when a house possesses a perfect drainage system of its own, it may be invaded by its neighbours: or the remains of old drainage systems may have been left by the carelessness or ignorance of workmen. Every pipe, too, of whatever sort, may be a means of bringing foul air from sewers; every such apparatus should, therefore, be examined from time to time, and be regulated with philosophical precision. The possibilities were described of the entrance of air from the public sewers into houses; either from stagnation and decomposition, or through the wind which at certain times penetrates the great sewers from the river; then, wherever a sewer ends abruptly, the gases are sure to be driven out of it, through the apertures in the streets made for ventilating purposes.

The effects of bad house drainage, and of sewer poison, are found in the prevalence of typhoid fever and choleraic maladies.

The great fevers at Clapham, Croydon, Westminster, and Windsor, were all of this sort, and traceable to this cause; and instances were given of illness, year after year, visiting a family, and robbing it of one or more lives; and when too late, the discovery made that an ancient, unknown, and decayed sewer ran under the premises. Many such sewers exist underground, not noticed in any map, and unknown to the present generation of officials; serving only as reservoirs of foul gases, which find vent through most unexpected channels.

It is in vain for the physician to discuss remedies, whilst the patient is still breathing the vapours which caused the disease.

Lastly, the property which diseases have of lurking in certain quarters, and then breaking out with virulence, and acquiring a self-propagating force, was ascribed to impurity in general and to defective drainage in particular. Scarlet fever, especially, was asserted by the speaker to be caused *ab initio*, as well as to receive power of extension from this source; but the limits of the hour did not permit him to develop the evidence on this point. Thirteen contagious maladies, at the least, can be produced at will; and the speaker believed that, in time, epidemic diseases would be found subject to human control; and that the surest mode of protecting the dwellings of the rich was to cleanse and ventilate the dwellings of the poor.

Diagrams were exhibited, showing the mortality in several parts of the parish of St. George, Hanover-square, by which it appeared that, out of 20,000 inhabitants of first and second class streets there died *at home*, in the three years 1856, 1857, and 1858, 10·78 per thousand per annnm; whilst, in third and fourth class streets, there died *at home*, in those three years, 20· per thousand per annum, exclusive of deaths in hospitals and workhouses. Taking the *mews* separately, there died *at home* 15·36 per thousand per annum.

The mortality and population of several streets during these years was also exhibited in a diagram, which contrasted the low mortality of purely aristocratic, and first-class business streets, with that of the nest of low streets between Grosvenor-square and Oxford-street, where, owing to the crowded and unventilatable state of the houses, there is a mortality of 30 per thousand; a mortality enhanced, of course, by the deaths of the children who are born but cannot be reared in such habitations.

[R. D.]

GENERAL MONTHLY MEETING,

Monday, May 9, 1859.

THE LORD WENSLEYDALE, Vice-President, in the Chair.

The Secretary announced that His Grace the President had nominated the following Vice-Presidents for the ensuing year :—

Sir B. C. Brodie, Bart. D.C.L. Pres. R.S.
Sir Henry Holland, Bart. M.D. F.R.S.
The Lord Wensleydale.
Col. Philip J. Yorke, F.R.S.
William Pole, Esq. M.A. F.R.S. *the Treasurer.*
The Rev. John Barlow, M.A. F.R.S. *the Secretary.*

Charles Brett, Esq.
David Chapman, Esq.
Miss Geraldine Clendinning,
Peter Dickson, Esq.
Flemyng George Gyll, Esq.
Richard Coxwell Rogers, Esq.
Charles William Siemens, Esq. and
The Earl Stanhope, D.C.L. F.R.S. Pres. Soc. Antiq.

were duly *elected* Members of the Royal Institution.

Sir Thomas Fowell Buxton, Bart. and
Alfred Henriques, Esq.

were *admitted* Members of the Royal Institution.

The following Professors were re-elected :—

WILLIAM THOMAS BRANDE, Esq. D.C.L. F.R.S. as Honorary Professor of Chemistry.

JOHN TYNDALL, Esq. F.R.S. as Professor of Natural Philosophy.

The following PRESENTS were announced, and the thanks of the Members returned for the same :—

FROM

The Lords Commissioners of the Admiralty—Capt. T. Spratt: Investigation of the Effect of the Prevailing Wave-influence on the Nile's Deposits. fol. 1859.
Commissioners in Lunacy—Appendix to Twelfth Report. 8vo. 1859.

Actuaries, Institute of—Assurance Magazine, No. 35. 8vo. 1859.
Airy, G. B. Esq. F.R.S. Astronomer-Royal (the Author)—Note on Oltmann's Calculation of the Eclipse of Thales. (Mem. Astron. Sec. 1858.) 4to.
Greenwich Observations. 1857. 4to. 1859.
American Academy of Arts and Sciences—Proceedings, 1856–8. 8vo.
Anonymous—Political Perils in 1859. 8vo. 1859.
Arts, Society of—Journal for April 1859. 8vo.
Asiatic Society of Bengal—Journal, No. 265. 8vo. 1859.
Astronomical Society, Royal—Monthly Notices, April 1859. 8vo.
Bache, Professor A. D. (the Superintendent)—United States' Coast Survey: Report for 1856. 4to.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for April 1859. 8vo.
Boosey, Messrs. (the Publishers)—The Musical World for April 1859. 4to.
British Architects, Institute of—Proceedings for April 1859. 4to.
Burgoyne, J. C. Esq. M.R.I. (the Author)—Chronological Account of India. 16to. 1859.
Chemical Society—Quarterly Journal, No. 45. 8vo. 1859.
Dilke, C. Wentworth, jun. Esq. M.R.I.—N. Rondot, Notice du Vert de Chine et de la Teinture en Vert chez les Chinois: suivie de Mémoires par J. Persoz et A. F. Michel. 8vo. Lyon, 1858.
N. Rondot, Rapport au Musée d'Art et d'Industrie à Lyon. 4to. 1859.
Editors—The Medical Circular for April 1859. 8vo.
The Practical Mechanic's Journal for April 1859. 4to.
The Journal of Gas-lighting for April 1859. 4to.
The Mechanics' Magazine for April 1859. 8vo.
The Athenæum for April 1859. 4to.
The Engineer for April 1859. fol.
The Artizan for April 1859. 4to.
The Horological Journal, No. 9. 8vo. 1859.
St. James's Medley. No. 17. 8vo. 1859.
Faraday, Professor, D.C.L. F.R.S.—M. A. Masson: Application de l'Electricité à la Médecine. 8vo. 1857.
Istituto Veneto di Scienze, &c. Memorie. Vol. VII. Parts 1 e 2. 4to. 1857–8.
Répertoire de Chimie, par C. Barreswil und A. Wurtz. Nos. 6, 7. 8vo. 1859.
Königliche Preussischen Akademie, Berichte, Jan. 1859. 8vo.
Franklin Institute of Pennsylvania—Journal, Vol. XXXVII. Nos. 3, 4. 8vo. 1859.
Geologische Anstalt, Wien—Jahrbuch: Jahrgang IX. Nos. 1, 2. 8vo. 1858.
Geological Society—Proceedings for April 1859. 8vo.
Quarterly Journal, No. 58. 8vo. 1859.
Geological Survey of India—Memoirs. Vol. I. Part 2. 8vo. 1858.
Graham, Lieut.-Col. J.—Chart of Chicago Harbour and Bar. 1857.
Granville, A. B. M.D. F.R.S. M.R.I. (the Author)—The Sumbul: a New Asiatic Remedy. 2nd Edition. 12mo. 1859.
Linnean Society—Proceedings, Vol. III. No. 12. 8vo. 1859.
Botanical Supplement, No. 2. 8vo. 1859.
Mackie, S. J. Esq. F.G.S. (the Editor)—The Geologist, April 1859.
Maury, M. F. Esq. (the Author)—Gales in the Atlantic—Charts.
Medical and Chirurgical Society, Royal—Proceedings, Vol. III. No. 1. 8vo. 1859.
Newton, Messrs.—London Journal (New Series) for April 1859. 8vo.
Novello, Mr. (the Publisher)—The Musical Times for April 1859. 4to.
Petermann, A. Esq. (the Editor)—Mittheilungen auf dem Gesammtgebiete der Geographie. 1859. Heft 3, 4. 4to. Gotha, 1859.
Photographic Society—Journal, Nos. 82, 83, 84. 8vo. 1859.
Radcliffe Trustees, Oxford—Radcliffe Astronomical and Meteorological Observations in 1857. 8vo. 1859.
Royal Society—Proceedings, No. 34. 8vo. 1858.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Jan. und Feb. 1859. 4to.
Yates, James, Esq. F.R.S. M.R.I. (the Author)—Mining Operations of the Romans in Britain. 8vo. 1859.
Memorial on Decimal Weights. 8vo. 1859.

WEEKLY EVENING MEETING,

Friday, May 13, 1859.

Sir Henry Holland, Bart. M.D. F.R.S. Vice-President, in the Chair.

William Hopkins, Esq. M.A. LL.D. F.R.S.

On the Earth's Internal Temperature, and the Thickness of its Solid Crust.

If we descend beneath the surface of the earth, and observe the temperature at different depths, it is found that within a depth ranging from 50 to 80 feet, the temperature changes periodically, being affected to that depth by the heat which the earth receives from the sun at different seasons of the year. The annual variation, however, becomes less as the depth increases, till at the depth above mentioned it becomes insensible. At greater depths, the temperature is invariable at each point, but increases with the depth, at the rate, on an average, of 1° (F.), for a depth of between 60 and 70 feet. The best observations which have been made on this subject are those in deep mining shafts and deep artesian wells; the greater the depth the more completely do anomalous influences counterbalance each other. The greatest depths at which such observations have been made in Western Europe, are at Monkwearmouth and Dukinfield in this country; the Puits de Grenelle, at Paris; Mondorff, in the Duchy of Luxembourg; New Seltzwerk, in Westphalia; and at Geneva. At the first two places the observations were made in vertical shafts of coal mines; the depth of the one at Monkwearmouth being upwards of 1800 feet, and that at Dukinfield upwards of 2000 feet; and in both cases the observations were made while the workmen were sinking the shafts, and with every precaution against the influence of any extraneous causes which might affect the observations. The former gave an increase of 1° (F.) for every 60 feet of depth, the latter for about every 72 or 73 feet. The sinking of the Puits de Grenelle was superintended by Arago. The mean increase of temperature was 1° for every 60 feet. At Mondorff the bore was 2400, being that of an artesian well; the increase was 1° for 57 feet. At New Seltzwerk the artesian well, penetrating to the depth of 2100 feet, giving an increase of 1° (F.) for 55 feet. The average of these is very nearly 1° for 60 feet. Numerous other observations are confirmatory of those results, though observations at smaller depths present many anomalies indicating the operation of local causes.

If a sphere of very large dimensions, like the earth, were heated in any degree and in any manner, and were left to cool in surrounding space, it is shown, by accurate investigation, that after a sufficient and very great length of time, the law according to which the temperature would increase in descending beneath the earth's surface, within depths small compared with the earth's radius, would be—that the increase of temperature would be proportional to the increase of depth. This coincides with the observed law, if we neglect the anomalous irregular variations which are found to exist more or less in each locality. Now according to this law, the temperature at the depth of 60 or 70 miles would probably be sufficient to reduce to a state of fusion nearly all the materials which constitute the earth's external solid envelope; and hence it has been concluded, that the earth probably consists of a central molten mass, as a fluid nucleus, and an external solid shell, of not more than 60 or 70 miles in thickness: and some geologists, desirous of rendering the conclusion the foundation of certain theories, have considered the thickness even less than that now mentioned.

This conclusion, however, rests on reasoning in which an important element is wanting. It involves the hypothesis that the *conductive power* of the rocks which constitute the lower portions of the earth's crust, is the same as that of the rocks which form its upper portion. This conductive power of any substance measures the facility with which heat is transmitted through it; and it is easily proved, by accurate investigation, that when the same quantity of heat passes through superimposed strata of different conductive powers, the increase of depth corresponding to a given increase of temperature (as 1°), is in any stratum proportional to the conductive power. Consequently, if the conductive power of the lower portions of the earth's solid crust be greater than that of the thin upper portion of it through which man has been able to penetrate, the depth to which we must proceed to arrive at a certain temperature (as that of fusion for the lower rocks) will be proportionally greater. The precise nature of the rocks situated at a great depth can only be judged of by analogy with those which are accessible to us; but those geologists who adopt the conclusion of the extreme thinness of the earth's crust, will doubtless admit that its inferior part must be of igneous origin, and must therefore be allowed to bear a certain resemblance to igneous rocks on the surface of the earth. Mr. Hopkins had recently made a great number of experiments on the conductive powers of various rocks. That of the softer sedimentary rocks, which are great absorbents of water, is very much increased by the quantity of moisture they contain; but taking chalk, one of the best absorbents, its conductive power, even when saturated, is not half so great as that of some of the igneous rocks on which Mr. Hopkins had experimented. Calcareous, argillaceous, and siliceous substances reduced to fine powder, stand, with reference to their conductive powers, in the order in which they are now mentioned, the conductivity of the first being the least; and when in a compact state, all that contributes to give a hard and crystalline character to the substance, and continuity to the mass through which the heat is conducted, increases the conductive power. These considerations lead to the conclusion that the conductivity of the inferior portions of the earth's solid crust must be much greater, and may be very much greater, than that of the less consolidated and mere superficial sedimentary beds. Moreover, the temperature of fusion of certain substances, as Mr. Hopkins had shown by experiment, is much increased by great pressure; and by analogy it may be concluded, that such would, at least in some considerable degree, be the case with the mineral matter of the earth's crust. The chalk is that formation in which the most numerous and some of the best observations on terrestrial temperatures have been made; and it would seem impossible to conclude from actual experiment and the considerations above stated, that its conductive power can exceed one-third of that of the inferior rocks, and may not improbably be a considerably smaller fraction of it. Now the increase of depth in the chalk corresponding to an increase of 1° (F.) is well ascertained to be very nearly 60 feet, and therefore the rate of increase in the inferior rocks must probably be at least three times as great as in the chalk, and may be very considerably greater still. Hence, supposing that the thickness of the solid crust would be about 60 miles, if the conductive power of its lower portion were equal to that of chalk, its actual thickness must probably be at least about 200 miles, and may be considerably greater, even if we admit no other source of terrestrial heat than the central heat here contemplated.

There is also another way of investigating the thickness of the earth's crust, assuming the whole terrestrial mass to consist of a fluid nucleus, inclosed in a solid envelope. If the earth were accurately spherical, instead of being spheroidal, its axis of rotation would always remain exactly parallel to itself, on the same principle as that on which the gyroscope preserves, in whatever position it may be held, the parallelism of the axis about which it rotates. But the attraction of the sun and moon on the protuberant equatorial portions of the earth's mass causes a progressive change in the position of the earth's axis, by virtue of which the North Pole, or that point in the heavens to which the northern extremity of the earth's axis is directed, instead of being stationary, describes a circle on the surface of the heavenly sphere about a fixed point in it called the pole of the ecliptic with a radius of nearly 23½°, equal to the inclination of the equator to the ecliptic, or the *obliquity*. The whole of this revolution is completed in about 25,000 years; but, as follows from what has just been stated, without any change, beyond small periodical ones, in the obliquity. A corresponding change of position must manifestly take place also in the position of the equinoxes, which have thus a motion along the ecliptic in a direction opposite to that in which the signs of the zodiac are reckoned, completing a revolution in the period above mentioned of 25,000 years. It is called *the precession of the equinoxes.*

This precessional motion has been completely accounted for under the hypothesis of the earth's entire solidity, and that of a certain law according to which the earth's density increases in approaching its centre; but some years ago Mr. Hopkins investigated the problem with the view of ascertaining how far the observed amount of precession might be consistent with the existence of a fluid nucleus. The result was, that such could only be the case provided the thickness of the solid shell were much greater than that which, as above stated, has been supposed by many geologists. The numerical result was, that the least admissible thickness of the crust must be about one-fifth of the earth's radius; but without assigning any great importance to an exact numerical result, Mr. Hopkins had a full confidence in the investigation, as showing that the thickness of the crust could not be so small as 200 or 300 miles, and consequently that no geological theory can be admitted which rests on the hypothesis of the crust being nearly as thin as it has been frequently assumed to be.

The influence of the interior fluidity on the precessional motion above described, is due to the difference between the motions which the attractions of the sun and moon tend to produce on a solid mass in one case, and a fluid mass on the other. It has been recently stated, as an objection to this investigation, that the interior fluid mass of the earth *may* move in the same manner as if it were solid. The only reply which could be given to such an objection was, Mr. Hopkins conceived, that it was mechanically impossible that these motions should be the same, though the resulting precessional motion for the solid crust, under certain conditions, to be determined only by the complete mathematical solution of the problem, might be the same as if the whole mass were solid. The effect of the attractions of the sun and moon also depends on the ellipticity of the inner surface of the solid shell; and it has been said that since that ellipticity depends on the law of the earth's density, which can only be imperfectly known, no result can be depended on which involves that ellipticity. This was not a correct statement of the problem. It was assumed in the solution referred to, that the ellipticity of the inner surface would depend partly on the law of density, and partly on the forms of the isothermal surfaces. Mr. Hopkins had supposed it possible, at the time he was engaged in this investigation, that a surface of *equal solidity* might approximate to a surface of *equal pressure;* he has now experimental reasons for believing that it must approximate much more nearly to an internal surface of *equal temperature.* Now for depths greater, probably much greater, than those which have often been supposed to correspond to the thickness of the earth's solid crust, there is no doubt that the internal isothermal surfaces have a greater ellipticity than the external surface itself; a conclusion which is independent of the law of density. Hence, a like conclusion will hold with reference to the internal surface of the shell, if it approximate sufficiently to the surface, of equal temperature; and this is the conclusion most unfavourable to the thin shell supposed by some geologists. Restricting the interpretation, then, of Mr. Hopkins's results to the question, whether the earth's solid shell be as thin as some geologists have supposed, or at least several hundred miles in thickness? (and this is the only question of geological importance)—Mr. Hopkins denied the validity of either of the objections above stated.

Thus, both the modes of investigation which had been described, lead to like conclusions respecting the least thickness which can be assigned to the solid envelope of our globe. It must be much greater than geologists have frequently imagined it to be.

[W. H.]

WEEKLY EVENING MEETING,

Friday, May 20, 1859.

Henry Bence Jones, M.D. F.R.S. Vice-President, in the Chair.

John Hall Gladstone, Ph.D. F.R.S. M.R.I.

On the Colours of Shooting Stars and Meteors.

All are familiar with the smaller kinds of shooting stars, and most have observed those of a larger size which shoot across the sky like a rocket, and burst perhaps in a shower of sparks; many persons also have been witnesses of the grander displays called fire-balls, or bolides, and some few have seen those bright clouds that have occasionally appeared and rained down stones upon the earth. It is not certain that all these are connected phenomena, or that there is a solid nucleus to every shooting star; yet it is impossible to draw any exact line of distinction, and there is every gradation between the most striking and the most simple of these appearances. The investigations of scientific men have made us acquainted with many facts relating to these bodies: thus, their direction is never perpendicular to the earth, but more frequently almost horizontal, and though they fly from every quarter of the heavens, the majority come from that part towards which the earth is at the time moving; their velocity averages about 20 miles per second; their height above the earth is, of course, very various, yet the more brilliant fire-balls seem to begin their luminous course at somewhere about 40 miles above us; their size is probably small in all instances, although, from irradiation, they frequently appear to present a considerable diameter; they occur often in showers; and these showers have been observed to have an annual periodicity. At the present time these star-showers occur generally about August 10 and November 13, but at the end of the 11th century the most remarkable period was April 4; and those wonderful people the Chinese, who have kept records of showers of meteors since March 23, B.C. 687 (when Manasseh was ruling over Judah, and European history scarcely existed) tell of other periods, pre-eminent among which is July 22.

The meteorolites which fall from the sky are of two sorts; the stony, consisting of silicate of magnesia, with more or less admixture of lime, potash, or soda combined with silicic acid; and the metallic, consisting of iron, which always contains a small quantity of nickel, with phosphorus and sulphur, and often contains in addition cobalt, and zinc, tin, lead, manganese, or chromium, with carbon or chlorine. Other elements have also been mentioned as found in certain meteoric stones. Three specimens were exhibited ; a broken piece of silicate interspersed with metal, which fell at Triguerre, in France, and a huge mass of supposed meteoric iron, the property of Prof. Tennant ; and a fragment of a piece of iron found in Mexico, now in the Royal Institution, and which, from its chemical composition, is believed to be meteoric.

The cosmical theory is the only one capable of explaining the known facts of these meteorites, though that is not without its difficulties. It supposes that in the interplanetary spaces, at least near the earth's orbit, there are a vast number of minute solid bodies revolving round the sun, either singly or in streams, and that our globe in its passage comes into collision with some of these, or periodically cuts the orbit of these streams of planetary dust. The small pieces of solid matter are supposed to become incandescent or ignited by their rapid friction against the air.

As to the colour of meteors, we have much information given in the lists of the Chinese, in those of the Rev. Baden Powell, published in the Reports of the British Association, and in those of M. Coulvier Gravier. M. Poey of Havannah has taken the trouble of arranging all these observations according to the colour, and the month of appearance: and the totals of his tables form the basis of the accompanying table, in which however a little liberty has been taken with the classification, all the recorded colours being referred to the six principal divisions of the prismatic spectrum, and these combined with white, and white itself. The Chinese colour observations are rather under, and the English rather above a thousand, but for the sake of comparison they have been reduced to that proportion.

	Chinese.	English.	French.
Red	5·1	12·2	4
White-Red	0·5	4·9	6
Orange	56·8	10·5	4
Yellow	0·6	14·2	7
White-Yellow	0·5	1·8	1
Green	0·0	0·6	0
White-Green	0·0	0·6	1
Blue	0·8	30·8	0
White-Blue	32·7	5·4	41
Purple	1·0	0·5	0
White	2·0	18·5	3
Total	100·0	100·0	67

The very apparent dissimilarities in these three lists are capable of more or less explanation. The Chinese mention orange (or rather yellowish red and reddish yellow) meteors in great numbers, but these seem to be balanced in a great measure by the numerous observations of red, pale red, and yellow, as well as orange in the English and French lists. It should be remembered that there may be every gradation from red through orange to yellow, and it may be fairly open to doubt whether the inhabitants of the Celestial empire gazing at the stars one or two thousand years ago distinguished colours just as their French translator would do. Again, the Chinese and French give white-blue in great numbers; but this is evidently the same as the English blue. The English lists also make numerous mention of white meteors, because in Prof. Powell's Catalogue the fact of a meteor being white or colourless is usually noted, which is not generally the case with either the French or the Chinese observations. The number of French meteors classified as white-blue, is swollen by many described by M. Coulvier Gravier as "white becoming bluish in the horizon."

The points of similarity in the three lists are, the small number of green meteors—what there are occurring generally among those fireballs that change colour; the small number of purple; the absence of brown; and the fact that the large majority of meteors exhibit some distinctive colour. They may be generally divided into two groups, the one blue, the other orange, inclining more or less either to red or yellow.

If these appearances are really produced by the passage of pieces of stone or iron through the earth's atmosphere during its annual course round the central orb, it is very possible that the stream of little bodies that intersects our orbit at one time of the year may differ in composition from those that cut our path at another period. It occurred to the speaker that this might be evidenced by a difference of colour during their combustion, and that the monthly tables of M. Poey afforded the means of determining whether such was really the case. On examining the Chinese record it was found that the prevailing colour of a great shower of falling stars is very rarely given; the colour observations are almost confined to large single meteors; and little can be observed beyond the fact that the blue meteors are more numerous in comparison with the orange during the months of August, September, October, and November, than during the rest of the year. M. Poey has also made the remarkable observation, that the Chinese meteors "show a remarkable constancy of tints during a long period of years, when an equally constant but different scale of colour prevails, and this for several successive periods;" a fact that may possibly be due to the changes in the periodical showers already adverted to. If, however, we turn to the monthly tables of the English observations, we are at once struck with the marked difference in the relative proportions of the different colours. Thus, confining our attention to the months of August and November, when the great showers occur, we observe a difference that cannot be attributed to mere accident. In the following table red and white-red have been added together, yellow and white-yellow, blue and white-blue.

	August.	November.
Red	49	24
Orange . . .	8	23
Yellow . . .	44	16
Blue	164	30
	265	93

On glancing at these numbers, we cannot fail to remark, the great deficiency of orange meteors in August, and their comparative abundance in November, while conversely blue meteors occur in great numbers during August and are comparatively rare at the later period. The yellow appear in about average quantity in each month as compared with the whole year, but the red exceed the average somewhat in November. Hence it may be deduced, that at the autumnal period the meteorites generally burn with a red or orange glow, while those which cross our orbit about the 10th of August display in combustion a blue colour, and this is in perfect accordance with what is observed on a closer inspection of Prof. Powell's lists. The speaker stated that last August he had had the good fortune to witness these streaming blue meteors passing from east to west, and leaving a phosphorescent train in their wake.

Another fact of interest connected with this subject, is the change that is frequently remarked in the colour of a meteor during its passage. Thus the French observations make repeated mention of falling stars that changed from white when overhead, to bluish or reddish in the horizon; from white they have been also seen to change to orange-yellow and blue-green, to reddish and bluish with a reddish train; and from yellowish white to orange-yellow and greenish-white, the meteor being broken into several fragments, two of which passed from white to the colour of red-hot iron. The changes from orange-yellow to green, and from yellow and red to greenish-yellow have also been recorded by M. Coulvier Gravier. The Chinese tell of the converse change from red to white, and British observations record the passage from blue to red; from blue to green, and finally red; from green to crimson; and from green to orange and red. The trains left by meteors during their flight, are usually of the same colour as the meteors themselves, but not always so; thus red meteors have sometimes left a blue luminosity, and conversely red sparks have frequently fallen from meteors of another colour. The Chinese record such instances in such terms as "at the moment when the globe of fire fell, a flame appeared, and a score of little red stars jumped out of it."

On turning over the British Association Catalogues we observe many indications of these changes of colour; thus, in an account of a meteor seen at Poona on Sept. 7, 1847, we read:—"Before the first bursting the meteor was of exceeding brightness, of an intense blue colour, and at the instant of explosion it changed into red." The following is nearer home. A fire-ball is thus described by an observer at Lambeth:—"As it brightened it displayed the most lovely colours, which could be distinctly traced to the radial colours produced by the sun; at one period green, violet (deep), pale red, &c., and their effects through the thin stratum of clouds which were in its path were most gorgeous." In a most graphic description, given by a lady, of a meteor that appeared over Hampstead, we find the remarkable statement:—"It shot forth several fiery coruscations, and while we were gazing at it, broke into an *intensely* radiant cloud. . . . It cast a most brilliant light on the houses there, brighter than moonlight, and unlike any light I ever saw. It appeared of a blue tint on the bricks, but there was no *blue* light in the cloud itself."

In discussing these reputed facts it is necessary to take into account certain illusions to which observers are subject. Thus, at the outset, there is the diversity of names given to the same colour by different persons. No two individuals, however perfect their perception of colour, would perhaps agree in their mode of naming the colours of all the stationary objects around them, how very likely then would they be to disagree in naming the colour of a light which appeared suddenly and unexpectedly in the sky, and as suddenly disappeared! Many instances of this discrepancy might be cited from the accounts of observed meteors; but the most curious instance with which the speaker was acquainted had occurred in the descriptions of the beautiful meteor that travelled over England on the 12th of last September, being visible in the evening before even the daylight had disappeared. Of the many eye-witnesses who described the phenomenon in the *Times* newspaper, five mentioned its apparent colour; of whom F.A.B. states it to have been "green at first;" N.R. "green, surrounded by white;" W. Rowlett, "white;" and W. "vivid, whitish blue;" while B.H. asserts that it was "primrose." Here, however, the discrepancy is so great as to lead us to the conviction that the meteor of Sept. 12, 1858, was really one of those that change colour during their passage through our atmosphere, and thus present different appearances to observers in different places.

It is quite possible that a meteor may emit rays which in the aggregate would produce one colour, and yet may affect the observer with a sensation of a different colour. This may arise either from absorption, intensity, or contrast.

In illustration of the effect of absorption Dr. Gladstone exhibited the prismatic spectrum by means of the electric lamp, and showed how certain glasses produced a similar absorption of the rays to that which takes place in the common phenomena of the red sun, or orange moon. The effect of dispersion was rendered visible by the non-transmission of the extreme blue and violet rays through water into which a little milk had been poured. This "sky-blue" mixture, produced by a substance itself colourless, represented the light clouds and vapours which must frequently affect the apparent colour of meteors, and suggested a simple explanation of the fact that of the variable meteors observed in the misty skies of England, so many terminate in red. Smoke has much the same effect on the spectrum as milk and water.

In illustration of the effect of intensity in causing lights actually coloured to produce very nearly the sensation of white, the electric light, from charcoal points, was exhibited under red and blue glasses, when it appeared dazzling and almost white; though no white light was really transmitted, and such objects as common paper, when illuminated by it, reflected the coloured radiance. This will explain the phenomenon so frequently observed by M. Coulvier Gravier, of a white meteor becoming bluish or reddish as it approached the horizon; and also the paradox of an "*intensely* radiant cloud" white in itself, but throwing a *blue* light on the walls of houses. In these cases it seems highly probable that the incandescent meteors were really emitting coloured light, but that this colour did not become apparent till the light was reduced either by distance, or by reflection from other objects. In a similar manner, the fact of a yellowish meteor becoming more or less green as it passes away from the spectator, so frequently noticed in the French observations, may be explained by the well-known changes in the chromatic impressions produced by yellow rays according to their intensity.

In reference to the effect of contrast it was remarked, that every lady is aware of the alteration that may be produced in the apparent tint of any article of dress by the juxtaposition of some other bright colour; and indeed it may be laid down as a general law that the apparent colour of every object is affected to a greater or less degree by the colours of all other objects seen at the same time. This remark holds good equally of self-luminous bodies, as, for instance, the flame of a gas-lamp, which assumes a rather bluish tint when the intensely yellow soda flame is brought beside it. This also must be a source of error in the observations of meteors.

After making due allowance for these points of difficulty and probable fallacy, we may approach the question—How far are these chromatic phenomena in accordance with the cosmical theory? Were pieces of iron to be shot through our atmosphere at the rate of twenty miles per second, there is good ground for believing that the friction would make them red-hot, if not incandescent. An iron wire, heated in the galvanic circuit, was observed by the speaker to emit at first principally orange and green rays, but as the heat increases the true red rays are emitted in an increasing degree, till "bright redness" is attained; and when combustion begins blue rays are also given forth, the general impression being then that of a reddish whiteness. The combustion and scintillation both of ordinary and of meteoric iron were shown in several ways. The metallic masses which fall through the air are never composed solely of iron, and it is difficult to say which metal or which other constituent would be the first ignited. Nickel in combustion displays a larger amount of green rays; sulphur, as is well known, burns with a blue, and phosphorus with a white flame. Two pieces of iron pyrites, attached to the wires of a powerful galvanic battery, when brought momentarily into contact, were ignited with a very luminous flame, which exhibited the characters of both burning iron and sulphur; and on one occasion, when the experiment was tried, the lambent blue flame of the latter element was visible for some time after the circuit was completely broken, and the ferruginous scintillations had ceased. The other metals occasionally found in meteorites, such as cobalt, zinc, or lead, will of course burn with their distinct flames; and the silicates, though incapable of combining with oxygen, may give out an intense light when strongly heated. This was demonstrated by placing the fragment of the meteoric stone that fell at Triguerre in the oxy-hydrogen blowpipe, when it began to fuse, and became brilliantly incandescent. In all these observations on the colours displayed by them there is nothing antagonistic to the idea that these luminous meteors are produced, as some have certainly been, by the combustion of such solid masses of metal and stone as occasionally strike the earth; but we are not yet in a condition to deduce its composition from the colour of any particular meteor.

[J. H. G.]

WEEKLY EVENING MEETING,

Friday, May 27, 1859.

The Lord Wensleydale, Vice-President, in the Chair.

William Pengelly, Esq. F.G.S.

On the Ossiferous Caverns and Fissures of Devonshire.

The limestone districts of Devonshire abound in caverns. These are of three kinds, chambers, tunnels, and shafts; their walls being either lined, angular, corroded, or eroded.

The following caverns were briefly noticed by Mr. Pengelly:—the Pixies' Hole at Chudleigh, the Chudleigh "Cavern," the Ugbrooke fissures, the Ogwell Cavern, the caverns at Buckfastleigh, Oreston, Yealmpton, and Ansty's Cove, near Torquay; the celebrated Kent's cavern, also near Torquay; the "Ash Hole," near Brixham; and finally, the recently discovered cavern on Windmill-hill, Brixham.

In November 1837, some waste lands on Windmill-hill, Brixham, were sold, when a small piece was bought by a dyer named Philp, who immediately commenced quarrying, with the intention of building upon it. In January 1858, the workmen came upon a hole, at first only the size of a man's hand, but which soon became large enough to permit Mr. Philp to enter. He proceeded as far as fifty feet, and brought out bones, of which he forthwith made an exhibition, and thereby attracted the attention of local geologists. The cavern was speedily visited by Sir R. I. Murchison, Drs. Falconer and Percy, Professor Ramsay, Mr. Prestwich, and other eminent geologists. The Royal

Society granted £100 as a contribution towards the expense of a scientific exploration of the cavern; additional sums were quickly subscribed; and a committee was formed to arrange and direct the course of proceeding.

Mr. Pengelly described the structure and formation of the cavern, and the mode of exploration adopted; and stated that there had been discovered in it a very considerable number of bones of animals, extinct and recent (the rhinoceros, ox-tribe, horse, cave-bear, hyæna, &c.), and also several well marked specimens of the objects commonly known as "flint knives," and which are generally considered to be of human manufacture. Similar articles had also been found in Kent's Cavern, in a corresponding situation, namely, in the "bone-earth," with the bones of extinct and recent animals, beneath the floor of stalagmite. Many fossils from the Oreston fissures were placed on the lecture-table; and on the wall were suspended diagrams of the ground plan of the Brixham cavern, &c.

Mr. Pengelly briefly explained his views on the probable origin of caverns in general, and of the Brixham cave in particular: which he referred to,—1st, The production of a line of fractures; 2nd, The chemical action of acidulated water, through such fractures; 3rd, The mechanical action of running water charged with rock debris, &c.

With respect to the chronology of the cavern and its contents, the speaker referred to the remains of the great herbivora, as evidences of the place having had a tropical or sub-tropical climate at the time of these deposits, and considered that whatever was the antiquity of the bone-earth in the cavern, the human period is as ancient. He thought that many facts concur to suggest a re-investigation of the antiquity of the human race; and he also considered it highly desirable to organise a system for the general exploration of caverns.

In the course of the lecture, Mr. Pengelly alluded to the various papers which had been published on the Devonshire caverns, viz.: Mr. Whidbey's Description of the Fissures at Oreston, near Plymouth, in the ***Philosophical Transactions*** for 1817. A paper on the Yealmpton Caverns, by Lieut. Colonel Mudge, read before the Geological Society of London, March 23, 1836; Mr. Austen's paper on the Bone Caverns of Devonshire, read before the Geological Society, March 25, 1840; and the Rev. Mr. McEnery's "Cavern Researches," being principally a memoir of Kent's Cavern, which was long supposed to be lost, but recently discovered and published by Mr. Vivian, of Torquay.

The following List (revised by Professor Owen) of Fossil Mammalia found in the Devonshire Caverns, was suspended on the Wall.

Extinct Species.	Recent Species.
Ursus priscus.	*Rhinolophus Ferrum-equinium*—Great Horse-shoe Bat.
Ursus spelæus—Great Cave Bear.	*Sorex vulgaris*—Shrew.
Hyæna spelæa—Cave Hyæna.	*Meles taxus*—Badger.
Felis spelæa—Great Cave Lion.	

EXTINCT SPECIES.	RECENT SPECIES.
Machairodus latidens.	*Putorius vulgaris*—Polecat.
Lagomys spelæa—Cave Pika.	*Putorius ermineus*—Stoat.
Elephas primigenius—Mammoth.	*Canis lupus*—Wolf.
Rhinoceros tichorinus—Tichorine Two-horned Rhinoceros.	*Vulpes vulgaris*—Fox.
Equus fossilis—Fossil Horse.	*Felis catus*—Wild Cat.
Equus plicidens.	*Arvicola amphibia*—Water Vole.
Asinus fossilis—Fossil Ass or Zebra.	*Arvicola agrestis*—Field Vole.
Hippopotamus Major—Large fossil Hippopotamus.	*Arvicola pratensis*—Bank Vole.
Megaceros Hibernicus—Gigantic Irish Deer.	*Lepus variabilis*—Norway Hare.
Strongyloceros spelæus—Gigantic round-antlered Deer.	*Lepus cuniculus*—Rabbit.
Cervus Bucklandi—Buckland's Deer.	*Cervus elephas*—Red Deer.
Bison minor.	*Cervus tarandus*—Rein Deer.
Bos longifrons—Long-fronted Ox.	*Cervus capreolus*—Roe Deer.

WEEKLY EVENING MEETING,

Friday, June 3, 1859.

SIR HENRY HOLLAND, BART. M.D. F.R.S. Vice-President, in the Chair.

THOMAS H. HUXLEY, ESQ. F.R.S.
PROFESSOR OF NATURAL HISTORY, GOVERNMENT SCHOOL OF MINES.

On the Persistent Types of Animal Life.

THE successive modifications which the views of physical geologists have undergone since the infancy of their science, with regard to the amount and the nature of the changes which the crust of the globe has suffered, have all tended in one direction, viz. towards the establishment of the belief, that throughout that vast series of ages which was occupied by the deposition of the stratified rocks, and which may be called "geological time," (to distinguish it from the "historical time" which followed, and the "pre-geological time," which preceded it) the intensity and the character of the physical forces which have been in operation, have varied within but narrow limits; so that, even in Silurian or Cambrian times, the aspect of physical nature must have been much what it is now.

This uniformitarian view of telluric conditions, so far as geological time is concerned, is, however, perfectly consistent with the notion of a totally different state of things in antecedent epochs, and the strongest advocate of such "physical uniformity" during the time of which we have a record might, with perfect consistency, hold the so-called "nebular hypothesis," or any other view involving the conception of a long series of states very different from that which we now know, and whose succession occupied pre-geological time.

The doctrine of physical uniformity and that of physical progression are therefore perfectly consistent, if we regard geological time as having the same relation to pre-geological time as historical time has to it.

The accepted doctrines of palæontology are by no means in harmony with these tendencies of physical geology. It is generally believed that there is a vast contrast between the ancient and the modern organic worlds—it is incessantly assumed that we are acquainted with the beginning of life, and with the primal manifestation of each of its typical forms: nor does the fact that the discoveries of every year oblige the holders of these views to change their ground, appear sensibly to affect the tenacity of their adhesion.

Without at all denying the considerable positive differences which really exist between the ancient and the modern forms of life, and leaving the negative ones to be met by the other lines of argument, an impartial examination of the facts revealed by palæontology seems to show that these differences and contrasts have been greatly exaggerated.

Thus, of some two hundred known orders of plants, not one is exclusively fossil. Among animals, there is not a single totally extinct class; and of the orders, at the outside not more than seven per cent. are unrepresented in the existing creation.

Again, certain well marked forms of living beings have existed through enormous epochs, surviving not only the changes of physical conditions, but persisting comparatively unaltered, while other forms of life have appeared and disappeared. Such forms may be termed "persistent types" of life; and examples of them are abundant enough in both the animal and the vegetable worlds.

Among plants, for instance, ferns, club mosses, and *Coniferæ*, some of them apparently generically identical with those now living, are met with as far back as the carboniferous epoch; the cone of the oolitic *Araucaria* is hardly distinguishable from that of existing species; a species of *Pinus* has been discovered in the Purbecks, and a walnut (*Juglans*) in the cretaceous rocks.* All these are types of vegetable structure, abounding at the present day; and surely it is a most remarkable fact to find them persisting with so little change through such vast epochs.

Every subkingdom of animals yields instances of the same kind. The *Globigerina* of the Atlantic soundings is identical with the cretaceous species of the same genus; and the casts of lower Silurian *Foraminifera*, recently described by Ehrenberg, assure us of the very close resemblance between the oldest and the newest forms of many of the *Protozoa*.

Among the *Cœlenterata*, the tabulate corals of the Silurian epoch are wonderfully like the millepores of our own seas, as every one may convince himself who compares *Heliolites* with *Heliopora*.

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* I state these facts on the authority of my friend Dr. Hooker.—T. H. H.

Turning to the *Mollusca*, the genera *Crania*, *Discina*, *Lingula*, have persisted from the Silurian epoch to the present day, with so little change, that very competent malacologists are sometimes puzzled to distinguished the ancient from the modern species. *Nautili* have a like range, and the shell of the liassic *Loligo* is similar to that of the "squid" of our own seas. Among the *Annulosa*, the carboniferous insects are in several cases referable to existing genera, as are the *Arachnida*, the highest group of which, the scorpions, is represented in the coal by a genus differing from its living congeners only in the disposition of its eyes.

The vertebrate subkingdom furnishes many examples of the same kind. The *Ganoidei* and *Elasmobranchii* are known to have persisted from at least the middle of the Palæozoic epoch to our own times, without exhibiting a greater amount of deviation from the typical characters of these orders, than may be found within their limits at the present day.

Among the *Reptilia*, the highest group, that of the *Crocodilia*, was represented at the beginning of the Mesozoic epoch, if not earlier, by species identical in the essential character of their organization with those now living, and presenting differences only in such points as the form of the articular faces of their vertebræ, in the extent to which the nasal passages are separated from the mouth by bone, and in the proportions of the limbs. Even such imperfect knowledge as we possess of the ancient mammalian fauna leads to the belief that certain of its types, such as that of the *Marsupialia*, have persisted with no greater change through as vast a lapse of time.

It is difficult to comprehend the meaning of such facts as these, if we suppose that each species of animal and plant, or each great type of organization, was formed and placed upon the surface of the globe at long intervals by a distinct act of creative power; and it is well to recollect that such an assumption is as unsupported by tradition or revelation as it is opposed to the general analogy of Nature.

If, on the other hand, we view "Persistent Types," in relation to that hypothesis which supposes the species of living beings living at any time to be the result of the gradual modification of pre-existing species—a hypothesis which though unproven, and sadly damaged by some of its supporters, is yet the only one to which physiology lends any countenance—their existence would seem to show, that the amount of modification which living beings have undergone during geological time is but very small in relation to the whole series of changes which they have suffered. In fact, palæontology and physical geology are in perfect harmony, and coincide in indicating that all we know of the conditions in our world during geological time, is but the last term of a vast and, so far as our present knowledge reaches, unrecorded progression.

[T. H. H.]

GENERAL MONTHLY MEETING,

Monday, June 6, 1859.

COLONEL PHILIP JAMES YORKE, F.R.S. Vice-President, in the Chair.

John Bathurst Graver Browne, Esq.
Samuel Parsons, M.D. and
William Simpson, Esq. C.E.
were duly *elected* Members of the Royal Institution.

Charles Brett, Esq.
was *admitted* a Member of the Royal Institution.

The following Letter from JACOB BELL, Esq. M.R.I., was read:—

"MY DEAR DR. FARADAY, "*Langham Place, June 2nd,* 1859.

"I have the works of JOHN GOULD, F.R.S. from the commencement up to the present time. Understanding that the Library of the Royal Institution is not provided with these works, I am desirous of offering them as a donation. The volumes up to a certain period are bound: and I regret my inability to include the entire works in the same state: but I find on inquiry that three of the works are still in progress, and therefore not yet in a state for binding. As the state of my health gives me no prospect of living to see the works completed,* and I could not impose on my executors an indefinite trouble and responsibility, I have only the alternative of requesting you to accept, on behalf of the Institution, all that it is in my power to give.

"I remain, my dear Dr. Faraday, yours very sincerely,

"JACOB BELL."

Works of JOHN GOULD, F.R.S.—

Century of Birds from the Himalaya Mountains. fol. 1832.
Monograph of the Ramphastidæ or Toucans. fol. 1st Ed. 1834. 2nd Ed. 1854.
Monograph of the Trogonidæ or Trogons. fol. 1st Ed. 1838. 2nd Ed. Part 1. 1858.
Birds of Europe. 5 vols. fol. 1837.
Mammals of Australia. Parts 1–10. fol. 1845–58.
Birds of Australia. 7 vols. fol. 1848. Supplement, Parts 1, 2. fol. 1851–5.
Monograph of the Trochilidæ, or Humming Birds. Parts 1–16. fol. 1849–58.
Birds of Asia. Parts 1–11. fol. 1850–9.
Monograph of the Odontophorinæ, or Partridges of America. fol. 1850.
Icones Avium. Part 2. fol. 1838.

RESOLVED,—That the warmest thanks of the Members be offered to Mr. BELL for his munificent present; and they beg to express their regret at the state of health of one who has so long been so earnest and active a Member of the Institution.

* Mr. Bell's decease took place on June 12 following.

The following Presents were announced, and the thanks of the Members returned for the same:—

From

Lords Commissioners of the Admiralty—Professor Piazzi Smyth's Report on the Teneriffe Astronomical Experiments of 1856. 4to. 1858.
American Philosophical Society—Proceedings, Nos. 57, 58. 8vo. 1857.
Anonymous—The Mutinies and the People; or Statements of Native Fidelity, exhibited during the Outbreak of 1857-8. By a Hindu. 8vo. Calcutta, 1859.
Arts, Society of—Journal for May 1859. 8vo.
Astronomical Society, Royal—Monthly Notices, May 1859. 8vo.
Barlow, Rev. John, M.A. F.R.S. V.P. & Sec. R.I.—Rev. T. Reynolds, Iter Britanniarum: or, that Part of the Itinerary of Antoninus which relates to Britain, with a New Comment. 4to. 1799.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for May 1859. 8vo.
Boosey, Messrs. (the Publishers)—The Musical World for May 1859. 4to.
British Architects, Institute of—Proceedings for May 1859. 4to.
Editors—The Medical Circular for May 1859. 8vo.
The Practical Mechanic's Journal for May 1859. 4to.
The Journal of Gas-Lighting for May 1859. 4to.
The Mechanics' Magazine for May 1859. 8vo.
The Athenæum for May 1859. 4to.
The Engineer for May 1859. fol.
The Artizan for May 1859. 4to.
The Horological Journal, No. 10. 8vo. 1859.
Faraday, Professor, D.C.L. F.R.S.—Répertoire de Chimie, par C. Barreswil und A. Wurtz. No. 8. 8vo. Paris 1859.
Franklin Institute of Pennsylvania—Journal, Vol. XXXVII. No. 5. 8vo. 1859.
Geological Society—Proceedings for May 1859. 8vo.
Holland, Sir Henry, Bart. M.D. F.R.S. V.P.R.I.—M. F. Maury's Sailing Directions. 8th Ed. 2 vols. 4to. Washington, 1858-9.
Lincoln's-Inn, Hon. Society of—Catalogue of the Printed Books in the Library. By W. H. Spilsbury, Librarian. 8vo. 1859.
Mosley, Sir Oswald, Bart. M.R.I. (the Author)—A Short Account of the Ancient British Church. 16to. 1858.
Newton, Messrs.—London Journal (New Series), for May 1859. 8vo.
Novello, Mr. (the Publisher)—The Musical Times, for May 1859. 4to.
Photographic Society—Journal, No. 85. 8vo. 1859.

WEEKLY EVENING MEETING,

Friday, June 10, 1859.

The Prince Consort, Vice-Patron, in the Chair.

John Tyndall, Esq. F.R.S.

PROFESSOR OF NATURAL PHILOSOPHY, ROYAL INSTITUTION.

On the Transmission of Heat of different qualities through Gases of different kinds.

Some analogies between sound and light were first pointed out: a spectrum from the electric light was thrown upon a screen—the spectrum was to the eye what an orchestra was to the ear—the different colours were analogous to notes of different pitch. But beyond the visible spectrum in both directions there were rays which excited no impression of light. Those at the red end excited heat, and the reason why they failed to excite light probably was that they never reached the retina at all. This followed from the experiments of Brücke and Knoblauch. These obscure rays had been discovered by Sir Wm. Herschel, and the speaker demonstrated their existence by placing a thermo-electric pile near to the red end of the spectrum, but still outside of it. The needle of a large galvanometer connected with the pile was deflected and came to rest in a position about 45 degrees from zero. A glass cell, containing the transparent vitreous humour of the eye of an ox, was now placed in the path of the rays: the *light* of the spectrum was not perceptibly diminished, but the needle of the galvanometer fell to zero, thus proving that the obscure rays of the spectrum, to which the galvanometric deflection was due, were wholly absorbed by the humours of the eye.

Reference was made to the excellent researches of Melloni. In a simple and ingenious manner he had proved the law of inverse squares to be true of radiant heat passing through air, and the eminent Italian inferred from his experiments that for a distance of 18 or 20 feet, the action of air upon radiant heat was totally inappreciable. This is the only experimental result now known regarding the transmission of radiant heat from terrestrial sources through air; with regard to its transmission through other gases it was believed that we were without any information.

It was, however, very desirable to examine the action of such media—desirable on purely scientific grounds, and also on account of certain speculations which had been based upon the supposed deportment of the atmosphere as regards radiant heat. These speculations were originated by Fourier; but it was to M. Pouillet's celebrated Memoir, and the recent excellent paper of Mr. Hopkins, to which we were indebted for their chief development. It was supposed that the rays from the sun and fixed stars could reach the earth through the atmosphere more easily than the rays emanating from the earth could get back into space. This view required experimental verification, and the more so, as the only experiment we possessed was the negative one of Melloni, to which reference has been already made.

The energetic action of the solid and liquid compounds into which the element hydrogen enters, suggested the thought that hydrogen gas might act more powerfully than air, and the following means were devised to test this idea. A tube was constructed, having its ends stopped air-tight by polished plates of rock-salt held between suitable washers, which salt is known to be transparent to heat of all kinds; the tube could be attached to an air-pump and exhausted, and any required gas or vapour could be admitted into it. A thermo-electric pile being placed at one end of the tube, and a source of heat at the other, the needle of an extremely sensitive galvanometer connected with the pile was deflected. After it had come to rest, the air was pumped from the tube, and the needle was carefully observed to see whether the removal of the air had any influence on the transmission of the heat. No such influence showed itself—the needle remained perfectly steady. A similar result was obtained when hydrogen gas was used instead of air.

Thus foiled, the speaker put his questions to Nature in the following way: a source of heat, having a temperature of about 300° C., was placed at one end of the tube, and a thermo-electric pile at the other—a large deflection was the consequence. Round the astatic needle, however, a second wire was coiled, thus forming a so-called differential galvanometer; a second pile was connected with this second wire, so that the current from it circulated round the needle in a direction opposed to that of the current from the first pile. The second pile was caused to approach the source of heat until both currents exactly neutralised each other, and the needle stood at zero. Here then we had two powerful forces in equilibrium, and the question now was whether the removal of the air from the tube would disturb this balance. A few strokes of the air-pump decided the question, and on the entire removal of the air the current from the pile at the end of the tube predominated over its antagonist from 40° to 50°. On readmitting the air the needle again fell to zero; thus proving beyond a doubt that the air within the tube intercepted a portion of the radiant heat.

The same method was applied with other gases, and with most remarkable results. Gases differ probably as much among themselves with regard to their action upon radiant heat as liquids and solids do. Some gases bear the same relation to others that alum does to rock-salt. The speaker compared the action of perfectly transparent coal-gas with perfectly transparent atmospheric air. To render the effect visible to the audience, a large plano-convex lens was fixed between two upright stands at a certain height above a delicate galvanometer. The dial of the instrument was illuminated by a sheaf of rays from an electric lamp, the sheaf being sent through a solution of alum to sift it of its heat, and thus avoid the formation of air-currents within the glass shade of the instrument. Above the lens was placed a looking-glass, so inclined that the magnified image of the dial was thrown upon a screen, where the movements of the needle could be distinctly observed by the whole audience. Air was first examined, the currents from the two piles being equilibrated in the manner described, the tube was exhausted, and a small but perfectly sensible deflection was the result. It was next arranged that the current from the pile at the end of the tube predominated greatly over its antagonist. Dry coal-gas was now admitted into the tube, and its action upon the radiant heat was so energetic, the quantity of heat which it cut off was so great, that the needle of the galvanometer was seen to move from about 80° on one side of zero to 80° on the other. On exhausting the tube the radiant heat passed copiously through it, and the needle returned to its first position.

Similar differences have also been established in the case of vapours. As representatives of this diverse action, the vapour of ether and of bisulphide of carbon may be taken. For equal volumes, the quantity of heat intercepted by the former is enormously greater than that intercepted by the latter.

To test the influence of *quality*, the following experiment was devised. A powerful lime light was placed at one end of the tube, and the rays from it, concentrated by a convex lens, were sent through the tube, having previously been caused to pass through a thin layer of pure water. The heat of the luminous beam excited a thermo-electric current in the pile at the end of the exhausted tube; and this current being neutralised by the current from the second pile, coal-gas was admitted. This powerful gas, however, had no sensible effect upon the heat selected from the lime light; while the same quantity of heat, from an obscure source*, was strongly affected.

The bearing of this experiment upon the action of planetary atmospheres is obvious. The solar heat possesses, in a far higher degree than that of the lime light, the power of crossing an atmosphere; but, and when the heat is absorbed by the planet, it is so changed in quality that the rays emanating from the planet cannot get with the same freedom back into space. Thus the atmosphere admits of the entrance of the solar heat, but checks its exit; and the result is a tendency to accumulate heat at the surface of the planet.

In the admirable paper of M. Pouillet already referred to, this action is regarded as the cause of the lower atmospheric strata being warmer than the higher ones; and Mr. Hopkins has shown the possible influence of such atmospheres upon the life of a planet situated at a great distance from the sun. We have hitherto confined our attention to solar heat; but were the sun abolished, and did stellar heat alone remain, it is possible that an atmosphere which permits advance, and cuts off retreat, might eventually cause such an accumulation of small savings as to render a planet withdrawn entirely from the influence of the sun a warm dwelling-place. But whatever be the fate of the speculation, the experimental fact abides—that gases absorb radiant heat of different qualities in different degrees; and the action of the atmosphere is merely a particular case of the inquiry in which the speaker was at present engaged.†

[J. T.]

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* The *quantity* of heat is measured by the amount of the galvanometric deflection which it produces; its power of passing through media may be taken as a test of *quality*.

† While correcting the proof of this abstract, I learned that Dr. Franz had arrived at the conclusion that an absorption of 3·54 per cent. of the heat passing through a column of air 90 centimeters long takes place; for coloured gases he finds the absorption greater; but all colourless gases he assumes show no marked divergence from the atmosphere.— *Poggendorff's Annalen*, xciv. p. 337.

WEEKLY EVENING MEETING,

Friday, June 17, 1859.

The Lord Wensleydale, Vice-President, in the Chair.

Professor Faraday, D.C.L. F.R.S.

On Phosphorescence, Fluorescence, &c.

The agent understood by the word "light," presents phenomena so varied in kind, and is excited to sensible action by such different causes, acting apparently by methods differing greatly in their physical nature, that it excites the hopes of the philosopher much in relation to the connexion which exists between all the physical forces, and the expectation that that connexion may be greatly developed by its means. This consideration, with the great advance in the experimental part of the subject which has recently been made by E. Becquerel, were the determining causes of the production of this subject before the members of the Royal Institution on the present occasion.

The well known effect of light in radiating from a centre, and rendering bodies visible which are not so of themselves, as long as the emission of rays was continual—the general nature of the undulatory view, and the fact that the mathematical theory of these assumed undulations was the same with that of the undulation of sound, and of any undulations occurring in elastic bodies, were referred to as a starting position. Limited to this effect of light it was observed that the illuminated body was luminous only whilst receiving the rays or undulations.

But superadded occasionally to this effect is one known as *phosphorescence*, which is especially evident when the sun is employed as the source of light. Thus, if a calcined oyster-shell, a piece of white paper, or even the hand, be exposed to the sun's rays and then instantly placed before the eyes in a perfectly dark room, they are seen to be visible *after* the light has ceased to fall on them. There is a further philosophical difference, which may be thus stated ; if a piece of white oyster-shell be placed in the spectrum rays issuing from a prism, the parts will, as to illumination, appear red, or green, or blue, as they come under the red, green, or blue rays: whereas if the phosphorescent effect be observed, *i.e.* that effect remaining after the illuminating rays are gone, the light will either be white, or of a tint not depending upon the colour of the ray producing it, but upon the nature of the substance itself, and the same for all the rays.

The ray which comes to the eye in an ordinary case of visibility, may be considered as that which, emanating from the luminous body, has impinged upon the substance seen, and has been deflected into a new course, namely towards the eye; it may be considered as the same ray, both before and after it has met with the visible body. But the light of phosphorescence cannot be so considered, inasmuch as *time* is introduced; for the body is visible for a time sensibly after it has been illuminated, which time in some cases rises up to minutes, and perhaps hours. This condition connects these phosphorescent bodies with those which phosphoresce by heat, as apatite and fluor-spar; for when these are made to glow intensely by a heat far below redness, it is evident that they have acquired a state which has enabled them for a time to become original sources of light, just as the other phosphorescent bodies have by exposure to light acquired a like state. And then again there is this further fact, that as the fluor spar which has been heated, does not phosphoresce a second time when reheated, still it may be restored to its first state by passing the repeated discharge of the electric spark over it, as Pearsall has shown.

Then follows on (in the addition of effect to effect) the phenomena of *fluorescence*, and the fine contributions to our knowledge of this part of light by Stokes. If a fluorescent body, as uranium glass, or a solution of sulphate of quinine, or decoction of horse-chestnut bark are exposed to diffuse day-light, they are illuminated, not merely abundantly but peculiarly, for they appear to have a glow of their own; and this glow does not extend to all parts of the bodies, but is limited to the parts where the rays first enter the substances. Some feeble flames, as that of hydrogen, can produce this glow to a considerable degree. If a deep blue glass be held between the body and the rays of the sun, or of the electric lamp, it seems even to increase the effect; not that it does so in reality, but that it stops very many of the luminous ray, yet lets the rays producing this effect pass through. By using the solar or electric spectrum, we learn that the most effectual rays are in most cases not the luminous ones, but are in the dark part of the spectrum; and so the fluorescence appears to be a luminous condition of the substance, produced by dark rays which are stopped or consumed in the act of rendering the fluorescent body luminous: so they produce this effect only at the first or entry surface, the passing ray, though the light goes onward, being unable to produce the effect again; and this effect exists only whilst the competent ray is falling on to the body, for it disappears the instant the fluorescent substance is taken out of the light, or the light shut off from it.

When E. Becquerel attacked this subject he enlarged it in every direction.* First of all, he prepared most powerful phosphori; these being chiefly sulphurets of the alkaline earths, strontia, baryta, lime. By treatment and selection he obtained them so that they would emit a special colour: thus, seven different tubes might contain preparations which exposed to the sun, or diffused day-light, or the electric light, should yield the seven rays of the spectrum. The light emitted

* Annales de Chimie et de Physique, 1859, tome lv. p. 1.

generally possessed a lower degree of refrangibility than the ray causing the phosphorescence; but in some instances he was able to raise the refrangible character of the ray emitted to that of the exciting ray. By taking a given preparation, and raising it to different temperatures, he caused it to give out different coloured rays by the single action of one common ray; this variation in power returning to a common degree as the temperatures of the phosphori became the same in all. He showed that *time* was occupied in the elevation of the phosphorescent state by the ray; and also that time was concerned in various degrees during the emission of the phosphorescent ray: that this time, which in many cases was long, might be affected, being shortened by the action of heat, and then the brilliancy of the phosphorescence for the shortened time was increased. He showed the special relation of the different phosphori to the different rays of the spectrum, pointing out where the maximum effect occurred; also that there were the equivalents of dark bands, *i.e.* bands in the spectrum, where little or no phosphorescence was produced.

These phosphori were many of them highly fluorescent. Thus, if one of them was exposed to the strong voltaic light, and then placed in the dark, it was seen to be brilliantly luminous, gradually sinking in brightness, and ultimately fading away altogether: but if it were held in the rays beyond the violet end of the spectrum (the more luminous rays being shut off) it was again seen to be beautifully luminous, but that state disappeared the instant it was removed from the ray. Now this is fluorescence, and the same body seemed to be both phosphorescent and fluorescent. Considering this matter, and all the circumstances regarding time, Becquerel was led to believe that these two luminous conditions differed essentially only in the *time* during which the state excited by the exposure to light continued; that a body being really phosphorescent, but whose state fell instantly, was fluorescent, giving out its light while the exciting ray continued to fall on it, and during that time only; and thata phosphorescent was only a more sluggish, body, which continued to shine after the exciting ray was withdrawn. To investigate this point he invented the *phosphoroscope;* an apparatus which may vary in its particular construction, but in which discs or other surfaces illuminated by the sun or an electric lamp, might, by revolution, be rapidly placed before the eye in a dark chamber, and so be regarded in the shortest possible space of time after their illumination. By such an apparatus Becquerel showed that all the fluorescent bodies were really phosphorescent; but that the emission of light endured only for a very short time.

An extensive series of experimental illustrations upon the foregoing points was made with fine specimens of phosphori, for which the speaker was indebted to M. Becquerel himself. The phosphoroscope employed consisted of a cylinder of wood, one inch in diameter and seven inches long, placed in the angle of a black box with the electric lamp inside, so that three-fourths of the cylinder were external, and in the dark chamber where the audience sat, and one-fourth was within the box, and in the full power of the voltaic light. By proper mechanical arrangements this cylinder could be revolved, and the part which was at one instant within, rapidly brought to the outside, and observed by the audience. As the cylinder could be made to revolve 300 times in a second, and as the twentieth part of a revolution was enough to bring a sufficient portion of the cylinder to the outside, it is evident that a phosphorescent effect which would last only the 1-3000th or even the 1-6000th of a second might be made apparent. All escape of light between the moving cylinder and the box was prevented by the use of properly attached black velvet.

The cylinder was first supplied with a surface of Becquerel's phosphori. The effect here was, that when by rotation the part illuminated was brought outside the box it was found phosphorescent. If the cylinder continued to rotate it appeared equally luminous all over, and when the rotation ceased, or the lamp was extinguished, the light gradually sank as the phosphorescence fell. Then a cylinder having a surface of quinine or æsculin was put into the apparatus. Whilst the cylinder was still it was dark outside; but when revolving with moderate velocity it became luminous outside, ceasing to be so the moment the revolution stopped. Here the fluorescence was evidently shown to occupy time; indeed, the full time of a revolution: and taking advantage of that, the self-shining of the body was separated from its illumination within, and the fluorescence made to assume the character of phosphorescence. Another cylinder was covered with crystals of nitrate of uranium, a hot saturated solution having been applied over it with a fine brush. The result was beautiful. A moderate degree of revolution brought no light out of the box; but with increased motion it began to appear at the edge. As the rapidity became greater, the light spread over the cylinder, but it could not be carried over the whole of its surface. It issued as a band of light where the moving cylinder left the edge of the box, diminishing in intensity as it went on, and looking like a bright flame, wrapping round half the cylinder. When the direction of revolution was reversed, this flame issued from the other side; and when the motion of the cylinder was stopped, all the phenomena of fluorescence or phosphorescence disappeared at once. The wonderfully rapid manner in which the nitrate of uranium received the action of the light within the box, and threw off its phosphorescence outside, was beautifully shown.

The electric light, even when the discharge is in rarefied media, or as a feeble brush, emits a great abundance of those rays, which produce the phenomena of fluorescence; but then if these rays have to pass through common glass they are cut off, being absorbed and destroyed even when they are not expended in producing fluorescence or phosphorescence. Arrangements can however be made in which the advantageous circumstances can be turned to good account with such bodies as Becquerel's phosphori or uranium glass. If these be enclosed within glass tubes, having platinum wires at the extremities, and which are also exhausted of air and hermetically sealed, then the discharges of a Ruhmkorff coil can be continually sent over the phosphori, and the effects both fluorescent and phosphorescent be beautifully shown. The first or immediate light of the body is often of one colour, whilst on the cessation of the discharge the second or deferred light is of another; and many variations of the effects can be produced.

In connexion with rarefied media it may be remarked, that some of the tubes by Geissler and others have been observed to have their rarefied atmospheres phosphorescent, glowing with light for a moment or two after the discharge through them was suspended. Since then Becquerel has observed that oxygen is rendered phosphorescent, *i.e.* that it presents a persistent effect of light, when electric discharges are passed through it. I have several times had occasion to observe that a flash of lightning, when seen as a linear discharge, left the luminous trace of its form on the clouds, enduring for a sensible time after the lightning was gone. I strictly verified this fact in June, 1857, recording it in the "Philosophical Magazine,"* and referred it to the phosphorescence of the cloud. I have no doubt that that is the true explanation. Other phenomena, having relation to fluorescence and phosphorescence, as the difference in the light of oxygen and hydrogen exploded in glass globes, or in the air, were referred to, with the expression of strong hopes that Becquerel's additions to that branch of science would greatly explain and extend them.

[M. F.]

GENERAL MONTHLY MEETING,

Monday, July 4, 1859.

Colonel Philip James Yorke, F.R.S. Vice-President, in the Chair.

Thomas Harlin, Esq. M.A.
James Merryweather, Esq. M.R.C.E. and
James Watney, Esq.

were duly *elected* Members of the Royal Institution.

The Special Thanks of the Members were returned to His Royal Highness the Prince Consort, for his present of the works of Kuhlmann and Von Fuchs on Water-Glass, which have been translated and printed for private circulation, by command of His Royal Highness; and to Sir John Rennie, for his present of his work on the Breakwater in Plymouth Sound.

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* Philosophical Magazine, June, 1857, p. 506.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same ; viz.

From

H.R.H. The Prince Consort—F. Kuhlmann on the Applications of Water-Glass (Soluble Alkaline Silicate) in the Arts. 8vo. 1859.

Dr. J. Von Fuchs, on the Manufacture, Properties, and Application of Water-Glass. 8vo. 1859.

Lords Commissioners of the Admiralty—Capt. T. Spratt on the True Position of Pelusium and Farama. fol. 1859.

Secretary of State for India—A Treatise on Problems of Maxima and Minima, solved by Algebra. By Ramchundra. 8vo. 1859.

Government of Canada—Geological Survey, Report for 1857. 8vo. 1858.

Anonymous—Reports on Newcastle and Welsh Coal. 8vo. 1859.

Arts, Society of—Journal for June 1859. 8vo.

Astronomical Society, Royal—Monthly Notices, June 1859. 8vo.

Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for June 1859. 8vo.

Boosey, Messrs. (the Publishers)—The Musical World for June 1859. 4to.

Boston Society of Natural History, U.S.—Journal, Vol. VI. No. 4. 8vo. 1857.

Proceedings, Vol. VI. Nos. 11–22. 8vo. 1857–8.

British Architects, Royal Institute of—Proceedings for June 1859. 4to.

Crookes, W. Esq. (the Editor)—The Photographic News, Vol. I. 1858–9. 8vo.

Dresser, Christopher, Esq. (the Author)—Unity in Variety, as deduced from the Vegetable Kingdom. 8vo. 1859.

Editors—The Medical Circular for June 1859. 8vo.

The Practical Mechanic's Journal for June 1859. 4to.

The Journal of Gas-Lighting for June 1859. 4to.

The Mechanics' Magazine for June 1859. 8vo.

The Athenæum for June 1859. 4to.

The Engineer for June 1859. fol.

The Artizan for June 1859. 4to.

Faraday, Professor, D.C.L. F.R.S.—Répertoire de Chimie, par C. Barreswil und A. Wurtz. No. 9. 8vo. Paris 1859.

Königliche Preussische Akademie, Berichte, März zu Mai, 1859. 8vo.

Geological Society—Proceedings for June 1859. 8vo.

Geologische Anstalt, Wien—Jahrbuch, 1858. Nos. 3 and 4. 4to. 1859.

Mendicity Society, The—Forty-first Report. 8vo. 1859.

Newton, Messrs.—London Journal (New Series), for June 1859. 8vo.

Novello, Mr. (the Publisher)—The Musical Times for June 1859. 4to.

Perigal, Frederick, Esq. (the Author)—Chart of the Navy of Great Britain, 1859.

Photographic Society—Journal, No. 86. 8vo. 1859.

Rennie, Sir John, F.R.S. M.R.I. (the Author)—Account of the Breakwater in Plymouth Sound. fol. 1848.

Royal Society of London—Philosophical Transactions, Vol. 148. Part 2. 4to. 1859.

Scoffern, John, Esq. M.B. (the Author)—The Manufacture of Sugar. 8vo. 1849.

Statistical Society—Journal, Vol. XXII. Part 2. 8vo. 1859.

Vereins zur Beförderung des Gewerbfleisses in Preussen—März und April 1859. 4to.

Royal Institution of Great Britain.

1859.

GENERAL MONTHLY MEETING,

Monday, November 7, 1859.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Thomas Harlin, Esq. M.A. and
James Watney, Esq.

were *admitted* Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same; viz.

FROM

Her Majesty's Government (through Sir R. I. Murchison)—Geological Survey of the United Kingdom: Annual Report. 8vo. 1859.
Memoirs: British Organic Remains. Monograph I. 4to. 1859.
Secretary for India—Maps of the Indian Survey, Nos. 24, 25, 26, 38, 39, 40, 41, 47, 54, 55, 57, 67, 68, 73, 74, 88, 89, 102, 103, 108, 111, 112.
Colonial Secretary's Office, New Zealand—Lecture on the Geology of Auckland, New Zealand. By Dr. F. Hochstetter. fol. 1859.
Actuaries, Institute of—Assurance Magazine, No. 36. 8vo. 1859.
Agricultural Society of England, Royal—Journal, No. 42. 8vo. 1859.
Arts, Society of—Journal for July–Oct. 1859. 8vo.
Asiatic Society of Bengal—Journal, Nos. 271, 272. 8vo. 1859.
Astronomical Society, Royal—Memoirs, Vol. XXVII. 4to. 1859.
Proceedings, Vol. XVIII. 8vo. 1859.
Basil Naturforschende Gesellschaft—Verhandlungen. Theil II. Heft 2, 3. 8vo. 1859.
Bavarian Academy of Sciences, Royal—Discourses at the Centenary of the Academy, &c. 4to. 1859.
Almanach. 8vo. 1859.
Belgique, Académie Royale de—Bulletin des Sciences. 1858. 8vo. Bruxelles, 1859.
Annuaire, 1859. 16mo.
Boosey, Messrs. (the Publishers)—The Musical World for July–Oct. 1859. 4to.
British Architects, Institute of—Proceedings for Nov. 1859. 4to.
British Association—Report of the Meeting at Leeds. 8vo. 1859.
Chemical Society—Quarterly Journal, Nos. 46, 47. 8vo. 1859.
Colyar, A. Esq., M.R.I.—R. R. Torrens: On the South Australian System of Conveyancing by Registration of Title. 8vo. Adelaide. 1859.
Commissioners in Lunacy—Thirteenth Annual Report. 8vo. 1859.

Editors—Artizan for July–Oct. 1859. 4to.
Athenæum for July–Oct. 1859. 4to.
Atlantis, No. 4. 8vo. 1859.
Engineer for July–Oct. 1859. fol.
Horological Journal, Nos. 12–15. 8vo. 1859.
Journal of Gas-Lighting for July–Oct. 1859. 4to.
Mechanics' Magazine for July–Oct. 1859. 8vo.
Medical Circular for July–Oct. 1859. 8vo.
Practical Mechanic's Journal for July–Oct. 1859. 4to.
Revue Photographique, Juillet–Oct. 1859. 8vo.
St. James's Medley, Nos. 19, 20. 8vo. 1859.
Faraday, Professor, D.C.L. F.R.S.—Répertoire de Chimie, par C. Barreswil et A. Wurtz. Nos. 9–12. 8vo. Paris, 1859.
Königliche Preussischen Akademie, Berichte, Juni zu Juli, 1859. 8vo.
Franklin Institute of Pennsylvania—Journal, Vol. XXXVII. No. 6; Vol. XXXVIII. Nos. 1, 2, 3.
Gautier et De la Rive, MM. (the Authors)—Rapports sur les Travaux de la Société de Physique de Genève. 1857–9. 4to.
Geographical Society, Royal—Journal, Vol. XXVIII. 8vo. 1859.
Proceedings, Nos. 4, 5. 8vo. 1859.
Geological Society—Quarterly Journal, Nos. 59, 60. 8vo. 1859.
Glasgow Philosophical Society—Proceedings. Vol. IV. Part I. 8vo. 1859.
Grinsted, John, Esq (the Author)—The Finance of the Railways of the United Kingdom. 8vo. 1859.
Gyll, Gordon W. J. Esq. M.R.I. (the Author)—A Tractate on Language. 8vo. 1859.
Hamel, Dr. J. (the Author)—Historical Account of the Electric Telegraph. 16mo. 1859.
Horticultural Society—Proceedings. Nos. 1, 2, 3. 8vo. 1859.
Institut Impérial de France—Mémoires présentés par divers Savans à l'Académie des Sciences. Vol. XV. 4to. 1858.
Kupffer, A. T. (le Directeur)—Annales de l'Observatoire Physique Centrale de Russie, 1856; et Compte Rendu, 1857. 4to. 1858.
Leeds Philosophical Society—Report, 1858–59. 8vo.
Linnean Society—Proceedings, Vol. III. No. 14. 8vo. 1859.
Liverpool Literary and Philosophical Society—Proceedings. No. XIII. 8vo. 1859.
Mackie, S. J. Esq. F.G.S. (the Editor)—The Geologist, July–Oct. 1859.
Macrory, E., Esq. M.R.I.—The Chronicles of Eri, being the History of the Gaal Sciat Iber; or the Irish People. Translated from Phœnician MSS. by O'Connor. 2 vols. 8vo. 1822.
Medical and Chirurgical Society, Royal—Proceedings, Vol. III. No. 2. 8vo. 1859.
Murchison, Sir R. I., F.R.S., &c. (the Author)—Address to the Royal Geographical Society. May, 1859. 8vo.
Newton, Messrs.—London Journal (New Series), for July–Oct. 1859. 8vo.
Novello, Mr. (the Publisher)—The Musical Times for July–Oct. 1859. 4to.
Petermann, A. Esq. (the Editor)—Mittheilungen auf dem Gesammtgebiete der Geographie. 1859. Heft 5–9. 4to. Gotha, 1859.
Photographic Society—Journal, Nos. 87–90. 8vo. 1859.
Royal Society—Proceedings, No. 36. 8vo. 1859.
Royal Society of Literature—Transactions, Vol. VI. Part 1. 8vo. 1859.
Royal Society of Tasmania—Meteorological Observations at Hobarton for 1856–7–8. 4to. 1859.
St. Petersbourg, Académie Impériale des Sceinces—Bulletin, Vol. XVII. 1859.
Saxon Society of Sciences, Leipsic—Abhandlungen. 4 Parts. 4to. 1859.
Berichte. 2 Parts. 8vo. 1859.
Scottish Society of Arts (Royal)—Transactions. Vol. V. Part I. 8vo. 1859.
Smithsonian Institution, Washington—Smithsonian Contributions. Vol. X. 4to. 1858.
Reports for 1857. 8vo. 1858.

Statistical Society—Journal, Vol. XXII. Part 3. 8vo. 1859.
Statistical Society of Spain—Censo y Nomenclator de la Poblacion de la España. 2 vols. fol. Madrid, 1859.
Sylvester, J. J. Esq. (the Author)—Outlines of Lectures on the Partition of Numbers. 8vo. 1859.
Taylor, Rev. William, F.R.S., M.R.I. (the Author)—On the Education of the Blind; and on the Establishment of a College. 16mo. 1859.
Sketch of the Life of J. W. Klein; and on the Relation of the Blind to the World around them; by J. W. Klein; translated by the Rev. William Taylor. 16mo. 1859.
C. Carton, Les Etablissements pour les Aveugles en Angleterre. 8vo. Bruges, 1838.
Tyndall, Professor, F.R.S. (the Author)—On the Physical Phenomena of Glaciers, &c. Part I. (from Phil. Trans.) 4to. 1859.
Way, Albert, Esq., M.A., F.S.A.—History of Northumberland. Part I. By J. Hodgson Hinde. 4to. 1858.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Mai und Juni. 1859. 4to.
Yorkshire (West Riding) Geological and Polytechnic Society—Proceedings, 1858. 8vo.
Zoological Society of London—Transactions. Vol. IV. Part 6. 4to. 1859. Proceedings, 1859. Part 2. 8vo. 1859.

Banting, William, Esq.—Specimens of an Acacia Tree struck by Lightning; and Two Photographs of the Same.
Gore, George, Esq.—Apparatus with Three Metallic Balls for exhibiting the Phenomena of Rotation by Ordinary Heat.

The Special Thanks of the Members were returned to SAMUEL LEIGH SOTHEBY, Esq. for the liberal present of his valuable work, entitled:

Principia Typographica: The Block-books, or Xylographic Delineations of Scripture History, issued in Holland, Flanders, and Germany during the Fifteenth Century. With a Supplement on the Block-books in the Bibliothèque Impériale at Paris. 3 vols. 4to. 1858–9.

GENERAL MONTHLY MEETING,

Monday, December 5, 1859.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Thomas Richards Andrews, Esq.
George March Harrison, Esq.
Andrew Halley Knight, Esq.
Hon. Augustus Henry Vernon,
Lachlan Mackintosh Rate, Esq. M.A.
Alfred Smee, Esq. F.R.S., and
William Swann, Esq.

were duly *elected* Members of the Royal Institution.

The Secretary announced that the following Arrangements had been made for the Lectures before Easter, 1860:—

Six Lectures on the VARIOUS FORCES OF MATTER (adapted to a Juvenile Auditory) by PROFESSOR FARADAY, D.C.L. F.R.S, &c. Fullerian Professor of Chemistry, R.I. To be delivered in the Christmas Vacation, 1859-60.

Twelve Lectures on FOSSIL BIRDS AND REPTILES, by PROFESSOR OWEN.

Twelve Lectures on LIGHT, INCLUDING ITS HIGHER PHENOMENA, by PROFESSOR TYNDALL.

Ten Lectures on the RELATIONS OF THE ANIMAL KINGDOM TO THE INDUSTRY OF MAN, by DR. EDWIN LANKESTER, M.D. F.R.S.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same; viz.

FROM
Actuaries, Institute of—Assurance Magazine, No. 37. 8vo. 1859.
Arts, Society of—Journal for Nov. 1859. 8vo.
Astronomical Society, Royal—Proceedings, No. 10. 8vo. 1859.
Bombay Geographical Society—Transactions, Vol. XIV. 8vo. 1859.
Boosey, Messrs. (the Publishers)—The Musical World for Nov. 1859. 4to.
Brettell, Thomas, Esq.—Thomas Smith, Recollections of the British Institution for Promoting the Fine Arts in the United Kingdom, 1805-59. 8vo. 1860.
British Architects, Institute of—Proceedings for Nov. 1859. 4to.
British Meteorological Society—First, Second, Third, Fourth, and Eighth Reports. 8vo. 1851-59.
Chambers, G. F. Esq. M.R.I.—R. Jameson's System of Mineralogy. 3 vols. 8vo. 1816.
Dublin Royal Society—Journal, No. 15. 8vo. 1859.
Editors—Artizan, for Nov. 1859. 4to.
Athenæum for Nov. 1859. 4to.
Engineer for Nov. 1859. fol.
Horological Journal, No. 16. 8vo. 1859.
Journal of Gas-Lighting for Nov. 1859. 4to.
Mechanics' Magazine for Nov. 1859. 8vo.
Medical Circular for Nov. 1859. 8vo.
Practical Mechanic's Journal for Nov. 1859. 4to.
Revue Photographique, Nov. 1859. 8vo.
Faraday, Professor, D.C.L. F.R.S.—Répertoire de Chimie, par C. Barreswil et A. Wurtz. Nos. 13, 14. 8vo. Paris 1859.
Th. Du Moncel, Exposé des Applications de l'Electricité. 4 vols. 8vo. Paris, 1856-9.
Franklin Institute of Pennsylvania—Journal, Vol. XXXVIII. No. 4.
Geographical Society, Royal—Proceedings, No. 6. 8vo. 1859.
Glaisher, James, Esq. F.R.S.—Various Meteorological Papers. 8vo. 1856-8.
Hamilton, Capt. A. M.R.I.—Lepsius, Denkmäler aus Ægypten. Letzte Lieferung. fol. 1859.
Manning, Frederick, Esq. M.R.I.—Photographs of the Birthplace of Sir Isaac Newton and of his Statue at Grantham, 1859.
Mackie, S. J. Esq. F.G.S. (the Editor)—The Geologist, Nov. 1859.
Newton, Messrs.—London Journal (New Series), for Nov. 1859. 8vo.
Novello, Mr. (the Publisher)—The Musical Times for Nov. 1859. 4to.

Petermann, A. Esq. (the Editor)—Mittheilungen auf dem Gesammtgebiete des Geographie. 1859. Heft 10. 4to. Gotha, 1859.
Photographic Society—Journal, No. 91. 8vo. 1859.
Royal Society of London—Transactions, Vol. CXLIX. Part 1. 4to. 1859.
Places of 5345 Stars, observed from 1828 to 1854, at the Armagh Observatory. By the Rev. T. R. Robinson. 8vo. 1859.
Trustees of the British Museum—List of Books of Reference in the Reading-room. 8vo. 1859.
Catalogue of Orthopterous Insects. Part I. with Plates. By J. O. Westwood. 4to. 1859.
Catalogue of the Coleopterous Insects in Madeira. By T. V. Wollaston. 8vo. 1857.
Catalogue of Hispidæ. By J. S. Baly. 8vo. 1858.
Catalogue of Batrachia Salientia. By Dr. A. Günther. Plates. 8vo. 1858.
Catalogue of Apodal Fish. By Dr. Kaup. Plates. 8vo. 1856.
Guide to the Systematic Distribution of Mollusca. By Dr. J. E. Gray. 8vo. 1857.
Catalogue of Mammalia and Birds of New Guinea. By J. E. and G. R. Gray. 8vo. 1859.
List of Lepidopterous Insects. By F. Walker. Parts 9 to 18. 12mo. 1856–9.
Catalogue of Hymenopterous Insects. By F. Smith. Parts 5, 6, 7. 12mo. 1857–9.
List of Homopterous Insects. By F. Walker. Supplement. 12mo. 1858.
Catalogue of Coleopterous Insects. Part 9. By C. Boheman. 12mo. 1859.
Catalogue of Neuropterous Insects. By Dr. Hagen. Part 1. 12mo. 1858.
Catalogue of British Fossorial Hymenoptera, Formicidæ, and Vespidæ. By F. Smith. 12mo. 1858.
Catalogue of Colubrine Snakes. By Dr. A. Günther. 12mo. 1858.
Catalogue of Birds. By G. R. Gray. Part 3; Sect. 2. 12mo. 1859.
Catalogue of Mazatlan Shells. By P. Carpenter. 12mo. 1857.
Catalogue of Auriculidæ, Proserpinidæ, and Truncatellidæ. By Dr. L. Pfeiffer. 12mo. 1857.
United Service Institution—Journal, No. 10. 8vo. 1859.
Vereins zur Beförderung des Gewerbfleisses in Preussen—July und Aug. 1859. 4to.

1860.

WEEKLY EVENING MEETING,

Friday, January 20, 1860.

SIR BENJAMIN COLLINS BRODIE, BART. President of the Royal Society, Vice-President R.I. in the Chair.

JOHN TYNDALL, ESQ. F.R.S.

PROFESSOR OF NATURAL PHILOSOPHY IN THE ROYAL INSTITUTION.

On the Influence of Magnetic Force on the Electric Discharge.

THE intention of the speaker was to bring before the meeting a series of experiments illustrative of the constitution of the electric discharge and of the action of magnetism upon it. The substance of the discourse was derived from the researches of various philosophers, its form being regulated to suit the requirements of the audience.

1. The influence of the transport of particles was first shown by an experiment suggested, it was believed, by Sir John Herschel, and performed by Professor Daniell. The carbon terminals of a battery of 40 cells of Grove were brought within one-eighth of an inch of each other, and the spark from a Leyden jar was sent across this space. This spark bridged with carbon particles the gap which had previously existed in the circuit, and the brilliant electric light due to the passage of the battery current was immediately displayed.

2. The magnified image of the coal points of an electric lamp was projected upon a white screen, and the distance to which they could be drawn apart without interrupting the current was noted. A button of pure silver was then introduced in place of the positive carbon, a luminous discharge four or five times the length of the former being thus obtained. The silver was first observed to glow, and afterwards to pass into a state of violent ebullition. A narrow dark space was observed to surround one of the poles, corresponding probably with the dark space observed in the discharge of Ruhmkorff's coil through rarefied media.*

3. The action of a magnet upon the splendid stream of green light obtained in the foregoing experiment was exhibited. A small horseshoe magnet of Logemann was caused to approach the light, which was bent hither and thither, according as the poles of the magnet changed their position: the discharge in some cases formed a magnificent green bow, which on the further approach of the magnet was torn asunder, and the passage of the current thereby interrupted. It was Davy who first showed the action of a magnet upon the voltaic arc. The transport of matter by the current was further illustrated by a series of deposits on glass obtained by Mr. Gassiot from the continued discharge of an induction coil.

4. A discharge from Ruhmkorff's coil was sent through an attenuated medium; and the glow, which surrounded the negative electrode was referred to. One of the most remarkable effects hitherto observed was that of a magnet upon this negative light. Plücker had shown that it arranges itself under the influence of the magnet exactly in the direction of the magnetic curves. Iron filings strewn in space, and withdrawn from the action of gravity, would arrange themselves around a magnet exactly in the manner of the negative light.

An electric lamp was placed upon its back; a horseshoe magnet was placed horizontally over its lens, and on the magnet a plate of glass: a mirror inclined at an angle of 45° received the beam from the lamp, and projected it upon the screen. Iron filings were scattered on the glass, and the magnetic curves thus illuminated were magnified,

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* Mr. Faraday noticed this dark stripe while the speaker was making his preparatory experiments.

and brought to clear definition upon the screen. The negative light above referred to arranges itself, according to Plücker, in a similar manner.

5. The rotation of an electric current round the pole of a magnet, discovered by Mr. Faraday in the Royal Institution, nearly forty years ago, was next shown; and the rotation of a luminous current from an induction coil in an exhausted receiver by the same magnet was also exhibited, and both shown to obey the same laws. This beautiful experiment was devised by De la Rive.

6. Into a circuit of 20 cells a large coil of copper wire was introduced, and when the current was interrupted, a bright spark, due to the passage of the extra current, was obtained. The brightness and loudness of the spark were augmented when a core of soft iron was placed within the coil. The disruption of the current took place between the poles of an electro-magnet; and when the latter was excited, an extraordinary augmentation of the loudness of the spark was noticed. This effect was first obtained by Page, and was for a time thought to denote a new property of the electric current.

But Rijke had shown in a paper, the interest of which is by no means lessened by the modesty with which it is written, that the effect observed by Page is due to the sudden extinction of the primary spark by the magnet; which suddenness concentrates the entire force of the extra current into a moment of time. Speaking figuratively, it was the concentration of what, under ordinary circumstances, is a mere push, into a sudden kick of projectile energy.

7. The contact-breaker of an induction coil was removed, and a current from five cells was sent through the primary wire. The terminals of the secondary wire being brought very close to each other, when the primary was broken by the hand, a minute spark passed between the terminals of the secondary. When the disruption of the primary was effected between the poles of an excited electro-magnet, the small spark was greatly augmented in brilliancy. The terminals were next drawn nearly an inch apart. When the primary was broken between the excited magnetic poles, the spark, from the secondary jumped across this interval, whereas it was incompetent to cross one-fourth of the space when the magnet was not excited. This result was also obtained by Rijke; who rightly showed, that in this case also the augmented energy of the secondary current was due to the augmented speed of extinction of the primary spark between the excited poles. This experiment illustrated in a most forcible manner the important influence which the mode of breaking contact may have upon the efficacy of an induction coil.

The splendid effects obtained from the discharge of Ruhmkorff's coil through exhausted tubes were next referred to. The presence of the coil had complicated the theoretic views of philosophers, with regard to the origin of those effects; the intermittent action of the contact-breaker, the primary and secondary currents, and their mutual reactions, producing tertiary and other currents of a higher order, had been more or less invoked by theorists, to account for the effects observed. Mr. Gassiot was the first to urge, with a water battery of 3500 cells, a voltaic spark across a space of air, *before* bringing the electrodes into contact: with the self-same battery he had obtained discharges through exhausted tubes, which exhibited all the phenomena hitherto observed with the induction coil. He thus swept away a host of unnecessary complications which had entered into the speculations of theorists upon this subject.

8. On the present occasion, through the kindness of Mr. Gassiot, the speaker was enabled to illustrate the subject by means of a battery of 400 of Grove's cells. The tension at the ends of the battery was first shown by an ordinary gold-leaf electroscope; one end of the battery being insulated, a wire from the other end was connected with the electroscope; the leaves diverged; on now connecting the other end of the battery with the earth, the tension of the end connected with the electrometer rose, according to a well-known law, and the divergence was greatly augmented.

9. A large receiver (selected from Mr. Gassiot's fine collection), in which a vacuum had been obtained by filling it with carbonic acid gas, exhausting it, and permitting the residue to be absorbed by caustic potash, was placed equatorially between the poles of the large electro-magnet. The jar was about six inches wide, and the distance between its electrodes was ten inches. The negative electrodes consisted of a copper dish, four inches in diameter, the positive one was a brass wire.

On the 16th of this month an accident occurred to this jar. Mr. Faraday, Mr. Gassiot, and the speaker had been observing the discharge of the nitric acid battery through it. Stratified discharges passed when the ends of the battery were connected with the electrodes of the receiver; and on one occasion the discharge exhibited an extraordinary effulgence; the positive wire emitted light of dazzling brightness, and finally gave evidence of fusion. On interrupting the circuit, the positive wire was found to be shortened about half an inch, its metal having been scattered by the discharge over the interior surface of the tube.

10. The receiver in this condition was placed before the audience in the position mentioned above. When the ends of the 400-cell battery were connected with the wires of the receiver, *no discharge passed;* but on touching momentarily with the finger any portion of the wire between the positive electrode of the receiver and the positive pole of the battery, a brilliant discharge instantly passed, and continued as long as the connexion with the battery was maintained. This experiment was several times repeated: the connexion with the ends of the battery was not sufficient to produce the discharge, but in all cases the touching of the positive wire caused the discharge to flash through the receiver.

Previous to the fusion of the wire above referred to, this discharge usually exhibited fine stratification: its general character now was that of a steady glow, through which, however, intermittent luminous gushes took place, each of which presented the stratified appearance.

11. On exciting the magnet between whose poles the receiver was placed, the steady glow curved up or down according to the polarity of the magnet, and resolved itself into a series of effulgent transverse bars of light. These appeared to travel from the positive wire along the surface of the jar. The deflected luminous current was finally extinguished by the action of the magnet.

12. When the circuit of the magnet was made and immediately interrupted, the appearance of the discharge was extremely singular. At first the strata rushed from the positive electrode along the upper surface of the jar, then stopped, and appeared to return upon their former track, and pass successively with a deliberate motion into the positive electrode. They were perfectly detached from each other; and their successive engulphments at the positive electrode were so slow as to be capable of being counted aloud with the greatest ease. This deliberate retreat of the strata towards the positive pole was due, no doubt, to the gradual subsidence of the power of the magnet. Artificial means might probably be devised to render the recession of the discharge still slower. The rise of power in the magnet was also beautifully indicated by the deportment of the current.

After the current had been once quenched, as long as the magnet remained excited, no discharge passed: but on breaking the magnet circuit, the luminous glow reappeared. Not only then is there an action of the magnet upon the particles transported by an electric current, but the above experiment indicates that there is an action of the magnet upon the electrodes themselves, which actually prevents the escape of their particles. The influence of the magnet upon the electrode would thus appear to be *prior* to the passage of the current.

13. The discharge of the battery was finally sent through a tube, whose platinum wires were terminated by two small balls of carbon: a glow was first produced.; but on heating a portion of the tube containing a stick of caustic potash, the positive ball sent out a luminous protrusion, which subsequently detached itself from the ball; the tube becoming instantly afterwards filled with the most brilliant strata. There can be no doubt that the superior effulgence of the bands obtained with this tube is due to the character of its electrodes: *the bands are the transported matter of these electrodes.* May not this be the case with other electrodes? There appears to be no uniform flow in nature; we cannot get either air or water through an orifice in a uniform stream; the friction against the orifice is overcome by starts, and the jet issues in pulsations. Let a lighted candle be quickly passed through the air; the flame will break itself into a beaded line in virtue of a similar intermittent action, and it may be made to sing, so regular are the pulses produced by its passage. Analogy might lead us to suppose that the electricity overcomes the resistance at the

surface of its electrode in a similar manner, escaping from it in tremors; the matter which it carries along with it being broken up into strata, as a liquid vein is broken into drops.*

[J. T.]

WEEKLY EVENING MEETING,

Friday, January 27, 1860.

Sir Henry Holland, Bart. M.D. F.R.S. Vice-President, in the Chair.

Professor Owen, D.C.L. F.R.S.

FULLERIAN PROFESSOR OF PHYSIOLOGY IN THE ROYAL INSTITUTION.

On the Cerebral System of Classification of the Mammalia.

The speaker commenced with a brief review of the principal systems which naturalists had proposed for the Classification of the Mammalia, dwelling more particularly on the following, viz.:—that which, on the ground of certain generalisations in the 'Historia Animalium,' had been attributed to Aristotle; on the scheme published by Ray, in his 'Synopsis Methodica Animalium Quadrupeduum, 1693'; on the classification adopted by Linnæus in the 12th edition of the 'Systema Naturæ, 1766'; and on that in the 2nd edition (1829) of Cuvier's 'Règne Animal.'

The Aristotelian generalisations on the structures and characters of the locomotive organs, as well as those on the dentition, of the 'Zootoka,' had continued to be used as the groundwork of the primary division of the mammalian class to the time of Cuvier. The chief merit of Ray's system was its exemplification of the principle of the subordination of characters, or of their different values as applicable to groups of different degrees of generalisation. The great step in advance made by Linnæus, was his accurate definition of the class, and his perception of the significant outward character which suggested the appropriate term which the class has since retained.

Cuvier, adopting the Linnæan primary divisions, '*Unguiculata,*' '*Ungulata,*' and '*Muticata,*' or '*Cetacea,*' subdivides them into more naturally defined orders, according to various characters afforded by the dental, osseous, generative, and locomotive systems, which his great anatomical knowledge had made known to him.

That heterogeneous order which Linnæus—prepossessed in favour of the easily recognisable outward character by which he distinguished the

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* Mr. Gassiot has shown that a *single discharge* of the Leyden jar produces the stratification. May not every such discharge correspond to a single draw of a violin bow across a string?

class—had characterised by the '*Mammæ pectorales binæ:* dentes primores incisores: superiores IV *parelleli,*' was shown, by the correlation of anatomical distinctions with the threefold modification of the limbs of the *Primates*, to be divisible into as many distinct orders. The hands on the upper limbs alone, and the lower limbs destined to sustain the trunk erect, characterised the order *Bimana*, the equivalent of the Linnæan genus *Homo*. The genus *Simia* of Linnæus, with hands on the four extremities, became the order *Quadrumana* of Cuvier. The genus *Vespertilio* with the 'manus palmatæ volitantes' formed the group *Cheiroptera*, answerable to the *Dermaptera* of Aristotle.

Ray had pointed out certain viviparous quadrupeds with a multifid foot as being 'anomalous species,' instancing as such 'the tamandua, the armadillo, the sloth, the mole, the shrew, the hedgehog, and the bat.' The first three species are associated with the scaly ant-eaters (*Manis*) of Asia and Africa, with the Australian spiny ant-eaters (*Echidna*), and with the more strange duck-moles (*Ornithorhynchus*) of the same part of the world, to form the order *Edentata* of Cuvier, which answers to that called *Bruta* by Linnæus, if the elephant and walrus be removed from it. The rest of *Ray's* 'anomalous species' exemplify the families *Cheiroptera* and *Insectivora* of the Cuvierian system, in which they are associated with the true *Carnivora* in an order called 'Carnassiers,' answering to the *Feræ* of Linnæus.

Cuvier had early noticed the relation of the Australian pouched mammals, as a small collateral series, to the unguiculate mammals of other parts of the globe; he discerned many relations of mutual affinity in their osseous structure, and he grouped them together under the name *Marsupialia*, to form a family of the 'Carnassiers' in the first edition of the 'Règne Animal': and he raised them to the rank of an order in the 2nd edition, where they terminate the carnassial or carnivorous series of the *Unguiculata*, possessing the three kinds of teeth.*

The hoofed animals (Ungulata, "animaux à sabots") are binarily divided into those that do, and those that do not, chew the cud; the former constituting the order *Pachydermata*, the latter that of *Ruminantia*.

The third primary group or subclass of Mammalia is indicated, but without receiving any name distinct from that of the single order *Cetacea* exemplifying it in the Cuvierian system—an order which would be equivalent to the *Mutica* of the Linnæan system, save that the *Manatee* which Linnæus placed in the same group as the elephant is associated with the whale in the *Règne Animal*.

Important as was the improvement which the Mammalian system of Cuvier presented on previous systems, the progress of anatomical and physiological knowledge, mainly stimulated by the writings and example of Cuvier himself, soon began to make felt the defects of his

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* *See* page 15, Table No. II., Cuvier.

system. These, indeed, were early suggested by the results of the application of the characters employed by Cuvier in the formation of the primary and secondary groups of the class; the sloth, for example, was placed above the horse, the mole above the lynx, and the bat above the dog: even the *Ornithorhynchus paradoxus*—shown by accurate anatomical scrutiny to be the most reptilian of the mammalian class—takes precedence of the colossal and sagacious elephant in the Cuvierian scheme.

The question of the truly natural and equivalent primary groups of the class had been kept in view by the speaker whenever opportunities occurred of adding to the knowledge of the anatomy of the mammalian class, and especially when engaged in dissecting any of the rarer forms which died at the gardens of the Zoological Society of London, and in which the structure of the brain could be unravelled.

The facts so acquired gradually impressed a conviction that the modifications of the cerebral organ were those which most truly indicated the primary groups of the mammalia.

Prior to the year 1836, the brain in *Mammalia* was supposed to differ from that in all other vertebrate animals by the presence of the large mass of transverse white fibres, called 'corpus callosum,' by the anthropotomist; which fibres, overarching the ventricles, and diverging as they penetrate the substance of either hemisphere of the cerebrum, bring every convolution of the one into communication with those of the other hemisphere, whence the other name of this part—the 'great commissure.' In that year Prof. Owen discovered that the brain of the kangaroo, the wombat, and some other marsupial quadrupeds, wanted the 'great commissure;' and that the cerebral hemispheres were connected together, as in birds, only by the 'fornix' and 'anterior commissure.' Soon afterward, he had the opportunity of determining that the same deficiency of structure prevailed in the *Ornithorhynchus* and *Echidna*. Since many other modifications of structure, more or less akin to those characterizing birds and reptiles, were found to be associated with the above oviparous type of brain, together with some remarkable peculiarities in the economy of reproduction, he then had suggested that the *Mammalia* might be divided into 'placental' and 'implacental.'

Impressed, however, with the fact that such binary division, like that which might be based upon the leading differences of dentition, was too unequal to be natural, and other modifications of the mammalian brain being found to be associated with concurrent conditions of other organs, the speaker had subsequently proposed a four-fold primary division of the Mammalia, based upon four leading types or conditions of the cerebral structure in that class.

The first or lowest modification is that in which the cerebral hemispheres of small relative size and simple exterior, are connected together only by the anterior commissure and fornix.

The second is that in which the great commissure or 'corpus callosum' is superadded, without any other advance in the development of the hemispheres; which are exteriorly smooth, or with few convolutions, and not extended backward over the cerebellum.

The third is that in which the cerebral hemispheres show, with the corpus callosum, an increased size, extending backward more or less upon the cerebellum, and, save in a few cases in which the size of the mammal is small, having the exterior surface convoluted.

The fourth modification exhibits, with all the preceding complications, a marked and sudden augmentation in the relative and absolute size of the cerebral hemispheres, which now extend above and beyond the cerebellum, and have a posterior lobe, with a posterior horn of the lateral ventricle, and a 'hippocampus minor,' and which have the gray exterior matter increased by numerous and deep convolutions.

The mammals exemplifying the lowest types of brain, were called LYENCEPHALA,* from the comparatively loose or disconnected state of the cerebral hemispheres: they are unguiculate: some have the 'optic lobes' simple, others partly subdivided; the lobes being then called 'bigeminal bodies.'

The *LYENCEPHALA* with simple optic lobes are 'edentulous' or without calcified teeth, and are devoid of external ears, scrotum, nipples, and marsupial pouch: they are true 'testiconda:' they have a coracoid bone extending from the scapula to the sternum, and also an epicoracoid and episternum as in Lizards: they are unguiculate and pentadactyle, with a supplementary tarsal bone supporting a perforated spur in the male. The order so characterized is called 'MONOTREMATA,' in reference to their single excretory and generative outlet. It includes two genera—*Echidna* and *Ornithorhynchus.* Of the first, the species are terrestrial, insectivorous, chiefly myrmecophagous, having the beak-like slender jaws and long cylindrical tongue of the true anteaters; but they are covered, like the hedgehog, with spines. Of the second genus, the species are aquatic, with a flattened beak, like that of a duck, which is used in the anserine manner to extract insects and worms from the mud: but they are clothed with a close fine fur like that of a mole, whence the name 'duck-mole' by which these anomalous quadrupeds are commonly known to the colonists. Both genera of Monotremes are strictly limited to Australia and Tasmania.

The *LYENCEPHALA* with divided optic lobes, forming the 'corpora bigemina' and 'quadrigemina' of anthropotomists, have teeth, and with rare exceptions, the three kinds, viz. incisors, canines, and molars. They are called the MARSUPIALIA, because they are distinguished by a peculiar pouch, which, in the female, contains the nipples and shelters the young for a certain period after their birth: both sexes have the marsupial bones in common with the Monotremes; a much varied denition, especially as regards the number of incisors, but usually including four true molars; and never more than three premolars: the angle of the lower jaw is more or less inverted.

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* λύω, to loose; ἐγκέφαλος, brain.

With the exception of one genus, *Didelphys*, which is American, all the known existing Marsupials belong to Australia, Tasmania, New Guinea, and its adjacent isles, where the order is represented by the genus *Cuscus* only. The grazing and browsing Kangaroos are rarely seen abroad in full daylight, save in dark rainy weather. Most of the Marsupialia are nocturnal. Zoological wanderers in Australia, viewing its plains and scanning its scrubs by broad daylight, are struck by the seeming absence of mammalian life; but during the brief twilight and dawn, or by the light of the moon, numerous forms are seen to emerge from their hiding-places and illustrate the variety of marsupial life with which many parts of the continent abroad. We may associate with their low position in the mammalian scale the prevalent habit amongst the Marsupialia of limiting the exercise of the faculties of active life to the period when they are shielded by the obscurity of night.

The second type of brain, is exemplified by the mammalia which constitute the order *Rodentia*, *Insectivora*, *Cheiroptera*, and *Bruta*: they are grouped together, in the cerebral scheme of classification, under the name LISSENCEPHALA,* having reference to the smooth unconvoluted exterior of the brain.

The RODENTIA are characterized by two large and long curved incisors in each jaw, separated by a wide interval from the molars; the teeth being so constructed, and the jaw so articulated, as to effect the reduction of the food to small particles by acts of rapid and continued gnawing, whence the name of the order. The orbits are not separated from the temporal fossæ. The male glands pass periodically from the abdomen into a temporary scrotum, and are associated with prostatic and vesicular glands. The placenta is commonly discoid, but is sometimes a circular mass (Cavy), or flattened and divided into three or more lobes (Lepus). The Beaver and Capybara are the giants of the order, which chiefly consists of small, numerous, prolific and diversified unguiculate genera, subsisting wholly or in part on vegetable food. Some Rodents, *e. g.* the Lemmings, perform remarkable migrations, the impulse to which, unchecked by dangers or any surmountable obstacles, seems to be mechanical. Many Rodents build very artificial nests, and a few manifest their constructive instinct in association. In all these inferior physical manifestations we are reminded of Birds. Many Rodents hibernate like Reptiles. They are distributed over all continents. About two-thirds of the known species of *Mammalia* belong to the Rodent order.

The transition from the Marsupials to the Rodents, seems to be made by the Wombats; but the Marsupials graduate more closely by the smaller Opossums to the INSECTIVORA. This term is given to the order of small smooth-brained Mammals, including the Hedgehogs, Moles, and Shrews, the molar teeth of which are bristled with cusps, and are associated with canines and incisors: they are unguiculate,

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* λισσός, smooth; ἐγκέφαλος, brain.

plantigrade, and pentadactyle, and they have complete clavicles. Like Rodents, they are periodical testiconda, and have large prostatic and vesicular glands: like most other *Lissencephala*, the Insectivora have a discoid or cup-shaped placenta. They do not exist in South America and Australia; their office in these continents is fulfilled by Marsupialia: but true Insectivora abound in all the other continents and their contiguous islands.

The order Cheiroptera, with the exception of the modification of their digits for supporting the large webs that serve as wings, repeat the chief characters of the Insectivora: a few, however, of the larger species are frugivorous and have corresponding modifications of the teeth and stomach. The mammæ are pectoral in position.

The most remarkable examples of periodically torpid Mammals are to be found in the terrestrial and volant Insectivora. The frugivorous Bats differ much in dentition from the true Cheiroptera, and would seem to conduct through the Colugos or Flying Lemurs, directly to the Quadrumanous order, from which, in Buffon's hypothesis of degeneration, they might be derivatives. The Cheiroptera are cosmopolitan.

The order Bruta, called *Edentata* by Cuvier, includes two genera (*Myrmecophaga* and *Manis*) which are devoid of teeth; the rest possess those organs, which, however, have no true enamel, no fangs or roots, are never displaced by a second series, and are very rarely implanted in the premaxillary bones. All the species have very long and strong claws. The ischium as well as the ilium unites with the sacrum; the orbit is not divided from the temporal fossa. The Three-toed Sloths (*Bradypus*) manifest their affinity to the oviparous Vertebrata by the supernumerary cervical vertebræ supporting false ribs and by the convolution of the windpipe in the thorax; and the unusual number—three and twenty pairs—of ribs, forming a very long dorsal, with a short lumbar, region of the spine, in the Two-toed Sloth (*Cholœpus*), recalls a lacertine structure. The same tendency to an inferior type is shown by the abdominal testes, the single cloacal outlet, the low cerebral development, the absence of medullary canals in the long bones in the Sloths, and by the great tenacity of life and long-enduring irritability of the muscular fibre, in both the Sloths and Ant-eaters.

The order Bruta is but scantily represented at the present period. One genus, *Manis* or Pangolin, is common to Asia and Africa; the *Orycteropus* is peculiar to South Africa; the rest of the order, consisting of the genera *Myrmecophaga*, or true Anteaters, *Dasypus* or Armadillos, and *Bradypus* or Sloths, are confined to South America.

Having defined the orders into which the Lissencephala were subdivided, the speaker adduced his evidences of the more truly natural character, notwithstanding the differences of form, structure, and habits, of this primary group, than could be affirmed of the 'Unguiculata' of the Linnæan and Cuvierian systems and summed up, in recapitulation, the following as amongst the more remarkable indications of their affinity to the Oviparous Vertebrata in particular orders or genera of the subclass. Such, *e.g.*, were the cloaca, convoluted trachea, supernumerary cervical vertebræ and their floating ribs, in the Three-toed Sloth; the numerous trunk-ribs in the Two-toed Sloth; the irritability of the muscular fibre, and persistence of contractile power in the Sloths and some other Bruta; the long, slender, beak-like edentulous jaws and gizzard of the Anteaters; the imbricated scales of the equally edentulous Pangolins, which have both gizzard and gastric glands like the proventricular ones in birds; the dermal bony armour of the Armadillos like that of loricated Saurians; the quills of the Porcupine and Hedgehog: the brilliant iridescent colours of the fur of the Cape-mole (*Chrysochlora aurea*); the proventriculus of the Dormouse and Beaver; the prevalence of disproportionate development of the hind limbs in the *Rodentia*; coupled, in the Jerboa, with confluence of the three chief metatarsals into one bone, as in birds; the keeled sternum and wings of the Bats; the aptitude of the *Cheiroptera*, *Insectivora*, and certain *Rodentia* to fall, like Reptiles, into a state of true torpidity, associated with a corresponding faculty of the heart to circulate carbonized or black blood:—these, and the like indications of coaffinity with the *Lyencephala* to the Oviparous air-breathing Vertebrata, had mainly prevailed with Professor Owen against an acquiescence in the elevation of different groups of the *Lissencephala* to a higher place in the Mammalian series, and in their respective association, through some single character, with better-brained orders, according to Mammalogical systems which, at different times, have been proposed by zoologists of deserved reputation. Such, *e.g.*, as the association of the long-clawed *Bruta* with the *Ungulata*, and of the shorter-clawed Shrews, Moles, and Hedgehogs, as well as the Bats, with the *Carnivora*; of the Sloths with the *Quadrumana*; of the Bats with the same high order; and of the *Insectivora* and *Rodentia* in immediate sequence after the Linnean 'Primates,' as in the latest published 'System of Mammalogy,' from a distinguished French author.

So far as their ordinal affinities are known, the most ancient Mammals, the fossil remains of which have been found in secondary strata, are either ly- or liss-encephalous, and belong either to the *Marsupialia* or the *Insectivora*.

The Mammals exemplifying the third type of brain, were called *Gyrencephala*,* in reference to the commonly unvoluted exterior of the cerebral hemispheres: but the more general character was the larger proportion of these parts, as exemplified in the small smooth-brained Monkeys and Lemurs.

The *Gyrencephala* are primarily subdivided, according to modifications of the locomotive organs, into three series, for which the Linnean terms may well be retained; viz., *Mutilata*, *Ungulata*, and *Unguiculata*, the maimed, the hoofed, and the clawed series.

These limb-characters can only be rightly applied to the gyrencephalous subclass; they do not indicate natural groups, save in that

* γυρόω, to wind about; ἐγκέφαλος, brain.

section of the Mammalia. To associate the *Lyencephala* and *Lissencephala* with the unguiculate *Gyrencephala* into one great primary group, appeared to the speaker to be a misapplication of a solitary character, akin to that which would have founded a primary division on the discoid placenta or the diphyodont dentition. No one had proposed to associate the unguiculate Bird or Lizard with the unguiculate Ape; and it was but a little less violation of natural affinities to associate the Monotremes with the Quadrumanes in the same primary (unguiculate) division of the Mammalian class.

The three primary divisions of the *Gyrencephala* were of higher value than the ordinal divisions of the *Lissencephala*; just as those orders were of higher value than the representative families of the *Lyencephala*.

The *Mutilata*, or the maimed Mammals with folded brains, are so called because their hind limbs seem as it were, to have been amputated; they possess only the pectoral pair of limbs, and those in the form of fins: the hind end of the trunk expands into a broad, horizontally flattened, caudal fin. They have large brains with many and deep convolutions, are naked, and have neither neck, scrotum, nor external ears.

The first order, called Cetacea, are either edentulous or monophyodont, and the latter have teeth of one kind and usually of simple form. They are 'testiconda,' and have no 'vesiculæ seminales.' The mammæ are pudendal; the placenta is diffused; the kidneys are much subdivided; the arterial system is remarkable for vast and complex 'piexuses,' covering the spinal chord and lining the intercostal spaces, forming a reservoir of arterial blood: the external nostrils—single or double—are on the top of the head, and called spiracle or, blowholes.' They are marine, and, for the most part, range the wide oceans; though with certain geographical limits as respects species. The 'right whale' of the northern hemisphere (*Balæna mysticetus)* is represented by a distinct species *(Balæna australis)* in the southern hemisphere: the high temperature of the waters at the equatorial zone bars the migration of either from one pole to the other. True Cetacea feed on fishes or marine animals.

The second order, called Sirenia, have teeth of different kinds, incisors which are preceded by milk-teeth, and molars with flattened or ridged crowns, adapted for vegetable food. The nostrils are two, situated at the upper part of the snout; the lips are beset with stiff bristles; the mammæ are pectoral; they are 'testiconda,' but have vesiculæ seminales. The Sirenia exist near coasts or ascend large rivers; browsing on fuci, water plants, or the grass of the shore. There is much in the organization of this order that indicates its nearer affinity to members of the suceeeding division, than to the cetaceous order. The Dugongs *(Halicore)* inhabit the Red Sea, the Malayan Archipelago, and the soundings of the Australian coasts; the Manatees *(Manatus*) frequent the shores of tropical America and Africa.

In the *Ungulata* the four limbs are present, but that portion of the toe which touches the ground is incased in a hoof, which blunts its sensibility and deprives the foot of prehensile power. With the limbs restricted to support and locomotion, the Ungulata have no clavicles: the fore leg remains constantly in the state of pronation, and they feed on vegetables.

A particular order, or suborder, of this group is indicated by fossil remains of certain South American genera, *e. g. Toxodon* and *Nesodon*, with long, curved, rootless teeth, have a partial investment of enamel, and with certain peculiarities of cranial structure: the name TOXODONTIA is proposed for this order, all the representatives of which are extinct.

A second remarkable order, most of the members of which have also passed away, is characterized by two incisors in the form of long tusks; in one genus *(Dinotherium)* projecting from the under jaw, in another genus *(Elephas)* from the upper jaw, and in some of the species of a third genus *(Mastodon)*, from both jaws. There are no canines: the molars are few, large, and transversely ridged; the ridges sometimes few and mammillate, often numerous and with every intermediate gradation. The nose is prolonged into a cylindrical trunk, flexible in all directions, highly sensitive, and terminated by a prehensile appendage like a finger: from this peculiar organ is derived the name PROBOSCIDIA given to the order. The feet are pentadactyle, but the toes are indicated only by divisions of the hoof; the placenta is annular; the mammæ are pectoral.

Elephants are dependent chiefly upon trees for food. One species now finds the conditions of its existence in the rich forests of tropical Asia; a second species in those of tropical Africa. These are all that now remain of the order *Proboscidia.*

Numerous species of Mastodon and Elephant roamed in pliocene times in warm and temperate latitudes of America and Europe. At a later or pleistocene period, a huge elephant, clothed with wool and hair, obtained its food from hardy trees, such as now grow in the 65th degree of north latitude; and abundant remains of this *Elephas primigenius* (as it has been prematurely called, since it was the last of our British elephants) have been found in temperate and high northern latitudes in Europe, Asia, and America. This, like other Arctic animals, was peculiar in its family for its longitudinal range. The Musk Buffalo was its contemporary in England and Europe, and still lingers in the northernmost parts of America.

Both the proboscidian and toxodontal orders of *UNGULATA* may be called aberrant: the dentition of the latter, and several particulars of the organization of the Elephant, indicate an affinity to the *Rodentia;* the cranium of the Toxodon, like that of the Dinothere, resembles that of the *Sirenia* in its remarkable modifications.

The typical Ungulate quadrupeds were divided by the speaker, according to the odd or even number of the toes, into PERISSODACTYLA and ARTIODACTYLA: the single hoof of the horse, the triple hoof of the tapir, exemplify the first: the double hoof of the camel, the quadruple hoof of the hippopotamus, exemplify the second.

The characters of these primary divisions were given, and illustrated in detail. The subdivisions of the orders *Perissodactyla* and *Artiodactyla* were next pointed out.

A well-marked, and at the present day extensive, subordinate group of the Artiodactyles, is called *Ruminantia*, in reference to the second mastication to which the food is subject after having been swallowed; the act of rumination requiring a peculiarly complicated form of stomach. The Ruminants have the 'cloven foot,' *i.e.* two hoofed digits on each foot forming a symmetrical pair, as by the cleavage of a single hoof: in most species there is added a pair of small supplementary hoofed toes. The metacarpals of the two functional toes coalesce to form a single 'cannon-bone,' as do the corresponding metatarsals. The camel-tribe have the upper incisors reduced to a single pair; in the rest of the ruminants, the upper incisors are replaced by a callous pad. The lower canines are contiguous to the six lower incisors, and, save in the Camel-tribe, are similar to them, forming part of the same terminal series of eight teeth, between which and the molar series there is a wide interval. The true molars have their grinding surface marked by two double crescents, the convexity of which is turned inwards in the upper and outwards in the under jaw.

Many fossil Artiodactyles, with similar molars, appear to have differed from the Ruminants chiefly by retaining structures which are transitory and embryonic in most existing Ruminants, as, *e.g.* upper incisors and canines, first premolars, and separate metacarpal and metatarsal bones; these are among the lost links that once connected more intimately the Ruminants with the Hog and Hippopotamus. The speaker, pursuing the retrospect of the twofold division of Gyrencephala as represented in the tertiary geological series, remarked that it was interesting, in relation to the needs of mankind, to find that, whilst some groups of *Ungulata*, *e.g.* the Perissodactyles and omnivorous Artiodactyles had been gradually dying out, other groups, *e.g.* the Ruminants, had been augmenting in genera and species. Most interesting also was it to observe, that in existing Ungulates there is a more specialized structure, a further departure from the general type, than in their representatives of the miocene and eocene tertiary periods: and that such later and less typical Mammalia did more effective service by virtue of their adaptively modified structures. The Ruminants, *e.g.*, more thoroughly digest and assimilate grass, and form out of it a more nutritive and sapid kind of meat, than did the antecedent more typical and less specialized non-ruminant Herbivora.

The monodactyle Horse was a better and swifter beast of draught and burthen than its tridactyle predecessor the miocene *Hipparion* could have been. The nearer to a Tapir or a Rhinoceros in structure, the further would an equine quadruped be left from the goal in contending with a modern Racer.

The geological distribution of the existing hoofed mammalia was next touched upon, and contrasted with that of the extinct forms.

The Unguiculata, as restricted in the cerebral system of the Mammalia, form the third division of the *Gyrencephala*. They enjoy a higher degree of the sense of touch than the Ungulata through the greater number and mobility of the digits and the smaller extent to which they are covered by horny matter. This substance forms a single plate, in the shape of a claw or nail, which is applied to only one of the surfaces of the extremity of the digit, leaving the other, usually the lower, surface possessed of its tactile faculty.

All the species are 'diphyodont,'* and the teeth have a simple investment of enamel.

The first order, CARNIVORA, includes the beasts of prey, properly so called. With the exception of a few Seals the incisors are $\frac{3-3}{3-3}$ in number; the canines $\frac{1-1}{1-1}$, always longer than the other teeth, and usually exhibiting a full and perfect development as lethal weapons; the molars graduate from a trenchant to a tuberculate form, in proportion as the diet deviates from one strictly of flesh, to one of a more miscellaneous kind. The clavicle is rudimental or absent; the innermost digit is often rudimental or absent; they have no vesiculæ seminales; the teats are abdominal; the placenta is zonular.

The Carnivora are divided, according to modifications of the limbs, into 'pinnigrade,' 'plantigrade,' and 'digitigrade' tribes. In the Pinnigrades (Walrus, Seal-tribe) both fore and hind feet are short, and expanded into broad, webbed paddles for swimming, the hinder ones being fettered by continuation of integument to the tail. In the Plantigrades (Bear-tribe) the whole or nearly the whole of the hind foot forms a sole, and rests on the ground. In the Digitigrades (Cat-tribe, Dog-tribe, &c.) only the toes touch the ground, the heel being much raised.

The principle of the more specialized character of actual organisations receives illustration in the genetic history of the present order.

The genera *Felis* and *Machairodus*, with their curtailed and otherwise modified dentition and their strong short jaws, become, thereby, more powerfully and effectively destructive than the eocene *Hyænodons* and miocene *Pterodons*, with their numerically typical dentition and their three carnassial teeth on each side of the concomitantly prolonged jaws, could have been.

In the most strictly carnivorous *GYRENCEPHALA* the paw is perfected as an instrument for retaining and lacerating a struggling prey by the superadded elastic structures for retracting the claws and maintaining them sharp. We next find in the unguiculate limb such a modification in the size, shape, position, and direction of the innermost digit that it can be opposed, as a thumb, to the other digits, thus constituting what is properly termed a 'hand.' Those Unguiculates which have both fore and hind limbs so modified, form the order QUADRUMANA. Most of them have $\frac{2-2}{2-2}$ incisors, $\frac{1-1}{1-1}$ canines, $\frac{3-3}{3-3}$ broad tuberculate molars, and premolars in variable numbers; all have per-

* *See* Philosophical Transactions, 1850, p. 493; and Art. 'Odontology,' Encycl. Britannica, 1858.

fect clavicles; pectoral mammæ; vesicular and prostatic glands; a discoid, sometimes double, placenta. The Quadrumana have a well-marked threefold geographical as well as structural division.

The Strepsirhines are those with curved or twisted terminal nostrils, with much modified incisors, premolars $\frac{3-3}{3-3}$ or $\frac{2-2}{2-2}$ in number, and molars with sharp tubercles: the second digit of the hind limb has a claw. This group includes the Galagos, Pottos, Loris, Aye-Ayes, Indris, and the true Lemurs; the three latter genera being restricted to Madagascar, whence the group diverges in one direction to the continent of Africa, in the other to the Indian Archipelago.

The speaker exhibited drawings by Joseph Wolf, of an Aye-Aye, (*Cheiromys Madagascariensis*) which had been recently transmitted, well preserved in spirits, to the British Museum. He pointed out, by comparison of its brain with that of a similarly sized Rodent, its combination of a type of cerebral organ truly gyrencephalous in the proportions of the hemispheres, and in the pattern of the few convolutions, with a dentition so similar to that of a Rodent as to have deceived Gmelin and Cuvier in regard to its ordinal position in the mammalian class. The equally remarkable modification of the hands was pointed out, concurring with the dentition in enabling the Aye-Aye to extract its favourite food, the larvæ of Coleoptera, burrowing in hard wood, from their hiding-places.

The Platyrhine Quadrumana are those with the nostrils subterminal and wide apart; premolars $\frac{3-3}{3-3}$ in number, the molars, with blunt tubercles; the thumbs of the fore-hands not opposable or wanting; the tail in most prehensile; they are peculiar to South America.

The Catarhine Quadrumana have the nostrils oblique and approximated below, and opening above and behind the muzzle: the premolars are $\frac{2-2}{2-2}$ in number; the thumb of the fore-hand is opposable. They are restricted to the Old World, and, save a single species on the rock of Gibraltar, to Africa and Asia. The highest organized family of Catarhines is tailless, and offers in the Orang, Chimpanzee, and Gorilla, the nearest approach to the human type.

Climate rigidly limits the range of the Quadrumana latitudinally: creational and geographical causes limit their range in longitude. Distinct genera represent each other in the same latitudes of the New and Old Worlds; and also, in a great degree, in Africa and Asia.

The fourth and highest type of the Mammalian brain rises at once, and without transitional rudiments of the hippocampus minor, hinder horn of lateral ventricle, or concomitant lobe of cerebrum protruding backward beyond the cerebellum, to that marvellous structure which is peculiar to our own species. The sole representative of the *ARCHENCEPHALA*,* is the genus *Homo*. His structural modifications, more

* ἄρχω, to over-rule; ἐγκέφαλος, brain.

especially of the lower limb, by which the erect stature and bipedal gait are maintained, are such as to claim for Man ordinal distinction on merely external zoological characters. But his psychological powers, in association with his extraordinarily developed brain, entitle the group which he represents to equivalent rank with the other primary divisions of the class *Mammalia* founded on cerebral characters. In this primary group Man forms but one genus, *Homo*, and that genus but one order, called BIMANA, on account of the opposable thumb being restricted to the upper pair of limbs. The mammæ are pectoral. The placenta is a single, sub-circular, cellulo-vascular, discoid body.

Man has only a partial covering of hair, which is not merely protective of the head, but is ornamental and distinctive of sex. The dentition of the genus *Homo* is reduced to thirty-two teeth, by the suppression of the outer incisor and the first two premolars of the typical series on each side of both jaws, the dental formula being :—

$$i.\frac{2-2}{2-2},\quad c.\frac{1-1}{1-1},\quad p.\frac{2-2}{2-2},\quad m.\frac{3-3}{3-3} = 32.$$

All the teeth are of equal length, and there is no break in the series; they are subservient in Man not only to alimentation, but to beauty and to speech.

The human foot is broad, plantigrade, with the sole, not inverted as in Quadrumana, but applied flat to the ground; the leg bears vertically on the foot; the heel is expanded beneath; the toes are short, but with the innermost longer and much larger than the rest, forming a 'hallux' or great toe, which is placed on the same line with, and cannot be opposed to, the other toes; the pelvis is short, broad, and wide, keeping the thighs well apart; and the neck of the femur is long, and forms an open angle with the shaft, increasing the basis of support for the trunk. The whole vertebral column, with its slight alternate curves, and the well-poised, short, but capacious subglobular skull, are in like harmony with the requirements of the erect position.

The widely-separated shoulders, with broad scapulæ and complete clavicles, give a favourable position to the upper limbs, now liberated from the service of locomotion, with complex joints for rotatory as well as flexile movements, and terminated by a hand of matchless perfection of structure, the fit instrument for executing the behests of a rational intelligence and a free will. Hereby, though naked, Man can clothe himself, and rival all natural vestments in warmth and beauty; though defenceless, Man can arm himself with every variety of weapon, and become the most terribly destructive of animals. Thus he fulfils his destiny as the supreme master of this earth, and lord of the lower Creation.

[R. O.]

The Discourse was illustrated with drawings of type species of quadrupeds, by Joseph Wolf, and by numerous diagrams, including the subjoined tables of classification.

TABLE OF VIVIPAROUS FOUR-FOOTED ANIMALS, ACCORDING TO RAY.

Viviparous hairy animals or quadrupeds are,—

- *Ungulate*, and these either
  - *Solidipedous*, as the HORSE, ASS, ZEBRA.
  - or *Bisulcate*, which are
    - *Ruminants* with horns, that are
      - Persistent, as in the OX, SHEEP, GOAT,
      - or
      - Deciduous, as in the STAG.
    - or *Not Ruminants*, as the HOG.
  - or *Quadrisulcate*, as the RHINOCEROS, HIPPOPOTAMUS.
- or *Unguiculate*, whose feet are either
  - *Bifid*, as in the CAMEL, or
  - *Multifid*, which are
    - With *digits* adhering together, and covered with a common integument, so that the extremities alone are visible at the margin of the foot, and are covered with obtuse nails, as in the ELEPHANT.
    - With *digits* in some measure distinct and separable from each other, the nails being
      - *Depressed*, as in APES,
      - or
      - *Compressed*, where the incisor teeth are
        - *Many*, in which group all the animals are carnivorous and rapacious, or at least insectivorous, or subsist on insects with vegetable matter:
          - *The larger ones* with the
            - *Muzzle short*, and head rounded, as the Feline tribe; or with the
            - *Muzzle long*, as the Canine tribe;
          - or *The smaller ones* with a long slender body, and short extremities, as the Weasel or Vermine* tribe;
        - or *Two very large*, of which tribe all the species are phytivorous, as the HARE.

* Genus *Vermineum*, from their worm-like form.

TABLE OF THE SUBCLASSES AND ORDERS OF THE MAMMALIA, ACCORDING TO CUVIER.

CLASS.	SUBCLASS.		ORDER.	FAMILY OR GENUS.	EXAMPLE.
MAMMALIA	UNGUICULATA	With three kinds of teeth.	BIMANA	*Homo*	Man.
			QUADRUMANA	*Catarrhina*	Ape.
				Platyrrhina	Marmoset.
				Strepsirrhina	Lemur.
			CARNARIA[1]	*Cheiroptera*	Bat.
				Insectivora	Hedgehog. Shrew. Mole.
				Carnivora	Bear. Dog. Seal.
			MARSUPIALIA	*Didelphys*	Opossum.
				Phalangista	Phalanger.
				Macropus	Kangaroo.
				Phascolomys	Wombat.
		Without canines.	RODENTIA	*Claviculata*	Rat.
				Non-claviculata	Hare.
		Without incisors.	EDENTATA	*Bradypus*	Sloth.
				Dasypus	Armadillo.
				Myrmecophaga	Anteater.
				Monotremata	Echidna. Ornithorhynchus.
	UNGULATA		PACHYDERMATA	*Proboscidia*	Elephant.
				Ordinaria	Hog. Tapir.
				Solidungula	Horse.
			RUMINANTIA		Sheep.
	MUTILATA		CETACEA	*Herbivora*	Dugong.
				Ordinaria	Whale.

1 Written *Carnassiers* by Cuvier.

TABLE OF THE SUBCLASSES AND ORDERS OF THE MAMMALIA, ACCORDING TO THE CEREBRAL SYSTEM.

CLASS.	SUB-CLASS.		ORDER.	GENUS OR FAMILY.	EXAMPLE.
MAMMALIA	ARCHENCEPHALA		BIMANA	*Homo*	Man.
	GYRENCEPHALA	Unguiculata	QUADRUMANA	*Catarhina*	Ape.
				Platyrhina	Marmoset.
				Strepsirhina	Lemur.
			CARNIVORA	*Digitigrada*	Dog.
				Plantigrada	Bear.
				Pinnigrada	Seal.
		Ungulata	ARTIODACTYLA	*Omnivora*	Hog.
				Ruminantia	Sheep.
			PERISSODACTYLA	*Solidungula*	Horse.
				Multungula	Tapir.
			PROBOSCIDIA	*Elephas*	Elephant.
				Dinotherium	
			TOXODONTIA	*Toxodon*	
				Nesodon	
		Mutilata	SIRENIA	*Manatus*	Sea-cow.
				Halicore	Dugong.
			CETACEA	*Delphinidæ*	Porpoise.
				Balænidæ	Whale.
	LISSENCEPHALA		BRUTA	*Bradypodidæ*	Sloth.
				Dasypodidæ	Armadillo.
				Edentula	Anteater.
			CHEIROPTERA	*Frugivora*	Roussette.
				Insectivora	Bat.
			INSECTIVORA	*Talpidæ*	Mole.
				Erinaceidæ	Hedgehog.
				Soricidæ	Shrew.
			RODENTIA	*Non-claviculata*	Hare.
				Claviculata	Rat.
	LYENCEPHALA		MARSUPIALIA	*Rhizophaga*	Wombat.
				Poephaga	Kangaroo.
				Carpophaga	Phalanger.
				Entomophaga	Opossum.
			MONOTREMATA	*Echidna*	Echidna.
				Ornithorhynchus	Duck-mole.

WEEKLY EVENING MEETING,

Friday, February 3, 1860.

Rev. John Barlow, M.A. F.R.S. Vice-President and Secretary, in the Chair.

Frederick Field, Esq. F.R.S.E.

On the Mineral Treasures of the Andes.

Owing to the great extent of country, the difficulty of access in many parts, and the comparatively few labourers in the field of science, much of the mineralogy of the district, bounded on the east by the Cordilleras, and on the west by the Pacific Ocean, remains to be investigated.

European enterprise and capital have, however, incidentally effected much. The miner, finding a ready purchaser in the smelter or merchant for his ores and other mineral productions, traverses the hills and mountains in quest of the treasures his country supplies, bringing them into the various ports on the coast for sale. It necessarily happens that he meets occasionially with some minerals of which he is not cognizant; but from their high specific gravity, metallic lustre, or other physical appearances, he deems them to be not wholly destitute of value. Thus, oftentimes, independent of purely scientific research, many curious natural compounds are brought to light, which otherwise perhaps might have lain for ages hidden in the deep recesses of the hills.

The chief wealth of the Andes, in a monetary point of view, consists of copper and silver minerals. Gold, although extensively disseminated, never occurs in very large quantities in one particular spot. The copper and silver mines on the contrary, yield those metals in very great abundance.

The copper ores may be divided as follows:—1. Those minerals which consist of native copper, in a nearly pure condition, or combinations of the metal with oxygen, and the oxides combined with carbonic, silicic, phosphoric, and vanadic acids, as well as with chlorine, in the mineral *atacamite.* 2. Minerals consisting essentially of copper, iron, and sulphur. 3. Combinations of copper with arsenic, or with arsenic and sulphur, or antimony and sulphur.

In the first division are found many interesting varieties. The native copper of Chili, found not only in the higher ridges of the Cordilleras, but also comparatively near the coast, is remarkable for its purity. In the district of *Andacollo*, a few leagues east of Coquimbo, it occurs in large masses, containing sometimes merely traces of foreign matter, among which gold is generally recognized. The red and black oxides are found associated with the metal, the former in larger quantity, in fine crystals belonging to the regular system, and having a clear ruby colour. The black oxide, hitherto considered a rare mineral, has quite recently been observed in the extreme north of Chili, as a fibrous mass, somewhat resembling hornblende.

The carbonates of copper abound extensively in the Andes, although no true *malachite* has yet been discovered, at least which will bear comparison with the specimens met with in Russia, Australia, and the west coast of Africa. The silicates, black, blue, and green are abundant; the two last varieties are termed *llanca*, by the miners; they do not occur crystallized, but in masses, sometimes assuming the botryoidal form. A rare double silicate of manganese and copper has also been obtained, easily soluble in hydrochloric acid, even in the cold, with evolution of chlorine. This mineral is called by the miners "*metal de carbon*," from its striking resemblance to ordinary coal.

The phosphates of copper, *Tagilite* ($4\,Cu\,O,\ P\,O_5,\ 3\,H\,O$) and *Liebethenite* ($4\,Cu\,O,\ P\,O_5,\ H\,O$) have only lately been described. They were discovered in a mine in *Tambillos*, near Coquimbo. A very rare double phosphate of lime and copper occurs in the same district $2\,(6\,Cu\,O,\ P\,O_5) + 10\,(3\,Ca\,O,\ P\,O_5 + Ca\,Cl)$; it is found associated with apatite in large blueish-green crystals.

Atacamite derives its name from the locality in which it was originally discovered, the desert of Atacama. It has been recognised as far south as Coquimbo, although only occasionally. Nearly all the mines in the district north of Copiapo, on the confines of Bolivia, yield this mineral. It consists of $Cu\,Cl,\ 3\,Cu\,O + 4\,H\,O$. Berthier analyzed a specimen from Cobija, containing six atoms of water. Atacamite was produced artificially in some smelting works in Chili, in very large quantities. Pounded oxide of copper, the product of calcined regulus in a fine state of division, gradually accumulated upon the base of a horizontal chimney close to the sea. The floor of the chimney had not been bricked, but consisted simply of sand impregnated with the alkaline chlorides and sulphates. The hot oxide of copper, partially protected by additional layers of dust from the furnace, by long contact with the sand became converted, after the lapse of some months, into sulphate and oxy-chloride of the metal. More than three tons were accidentally obtained in this manner.

Double sulphides of copper and iron :—*Disulphide of Copper* exists very often in a state of great purity, containing sometimes 79·5 of metal, with scarcely a trace of iron. The proto-sulphide is rarer, being generally associated with sulphate of lime in intimate mechanical union. The "Cobre Añilado" of the mines is a soft indigo-blue coloured mineral, which by long digestion in water loses its sulphate of lime, the residue consisting of tolerably pure proto-sulphide (Cu S). Immense masses of what is usually termed blue sulphide of copper, but which is really a double sulphide of that metal with iron, occur in Tamaga, a district lying between Valparaiso and Coquimbo. The pure mineral contains about 56 of copper, 23 of sulphur, and 21 of iron.

The ordinary yellow pyrites (Cu_2 S, Fe_2 S_3) and peacock ore abound throughout the Andes.

Arsenide of Copper—Domeykite.—This mineral is interesting, being somewhat similar to condurite, analyzed by Mr. Faraday in 1826, and subsequently by Dr. Blyth in 1848. The compound investigated by these chemists, however, was a mixture of arsenite of copper, in conjunction with the arsenide. Domeykite does not contain arsenious acid, but is simply expressed by the formula Cu 6 As.

Algodonite contains more copper than the former, and is composed of 12 atoms of copper and one of arsenic.

Guayacanite.—This mineral, very similar to enargite, is found in only one locality in Chili, very high up in the Cordilleras, the mine being for a great part of the year, quite inaccessible. It consists of 3 Cu_2 S, As S_5, or three atoms of disulphide of copper united to one atom of pentasulphide of arsenic. Arsenic is generally found combined with three atoms of sulphur, when in combination with the sulphides of most metals. The state of pentasulphide in this instance, makes the mineral peculiarly interesting.

Vanadiate of Copper, in union with vanadiate of lead, exists in the "Mina Grande," near Coquimbo. It has a brownish earthy appearance, and contains generally from 13 to 16 per cent. of vanadic acid.

Silver Minerals.—For a long time the principal, and indeed the only great source of silver in Chili was from the province of Coquimbo, the district in which the mines were situated being about fifty miles distant from the coast. The rich silver mines of Chañarcillo, in Copiapo, were discovered about the year 1835 by Juan Godoy, a wood-cutter. Sleeping on the hill, after a hard day's toil, he kindled a small fire, and in the morning he discovered underneath the embers a bright metallic surface, which he mistook for lead, but which proved to be silver. The locality has yielded enormous riches since that time. Besides native silver, the chloride, two or three varieties of chloro-bromide, the bromide and iodide have been discovered, as well as the dark and light vosiclers ($3\,Ag\,S + Sb\,S_3$) α ($3\,Ag\,S + As\,S_3$) the sulphide Ag S, Polybasite, and a variety of other interesting appearances.

Arquerite, named from Arqueros, the district in which it is found, consists of Ag 6 Hg. For a long time, with the exception of the liquid amalgam discovered by Del Rio, in Mexico, it was considered the only native amalgam of silver. M. Domeyko, to whom the mineralogy of South America owes so much, has however described at least six native amalgams.

$$\begin{array}{ll} Ag_6 & Hg. \\ Ag_7 & Hg_2 = Ag_6\,Hg + Ag\,Hg. \\ Ag_5 & Hg_3. \\ Ag. & Hg_2. \\ Ag_8 & Hg_4 = 2\,Ag\,Hg + Ag\,Hg_2. \\ Ag. & Hg. \end{array}$$

Mercury has been found pretty abundantly in many parts of the Andes, chiefly as cinnabar.

A curious combination of this metal with arsenic, sulphur, antimony, and copper also exists, and may be termed mercurial fahlerz. Resulting from the oxidation of this mineral, the singular natural combination, constituting *ammiolite*, is found, which consists essentially of oxide and sulphide of antimony with oxide and sulphide of mercury.

Time alone prevented a description of many other important and interesting minerals, the production of the Andes and the mountains in the vicinity.

[F. F.]

GENERAL MONTHLY MEETING,

Monday, February 6, 1860.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Matthew Bell, Esq.
James Butler, Esq.
Robert Lush, Esq. Q.C. and
John Morgan, Esq.

were duly *elected* Members of the Royal Institution.

Captain James Drew, Esq. and
Thomas Wilson, Esq.

were *admitted* Members of the Royal Institution.

The Special Thanks of the Members were returned to William Salmon, Esq. *M.R.I.* for the following Present:

Botanical Works of R. J. Thornton, M.D. viz.
New Illustrations of the Sexual System of Linnæus. (Many Portraits and Plates.) 2 vols. fol. 1807.
Botanical Extracts, or the Philosophy of Botany. 3 vols. fol. 1810.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same: viz.

FROM

Governor-General of India—Memoirs of the Geological Survey of India. Vol. II. Part 1. 8vo. 1859.

The French Minister of Public Instruction—Documens Inédits sur l'Histoire de France: Lettres Missives de Henri IV. (1603–6). Tome VI. 4to. 1853.
Papiers d'Etat de Cardinal de Granvelle. Tome IX. 4to. 1852.

Actuaries, Institute of—Assurance Magazine, No. 38. 8vo. 1859.

Arts, Society of—Journal for Nov. Dec. 1859; Jan. 1860. 8vo.

Asiatic Society of Bengal—Journal, No. 273. 8vo. 1859.

Astronomical Society, Royal—Proceedings, No. 2, 1860. 8vo.
Instructions for the Observation of Mars in 1860: by G. B. Airy, F.R.S. &c. 8vo. 1858.

Beauclerc, G. Esq. M.R.I.—Observations on the Report of the Commission on Harbours of Refuge Enquiry. 8vo. 1860.

Botfield, Beriah, Esq. F.R.S. M.R.I. (the Author)—Stemmata Botevilliana: Memorials of the Families of De Boteville, Thynne, and Botfield. 4to. 1858.

Boosey, Messrs. the (Publishers)—The Musical World for Nov. Dec. 1859; Jan. 1860. 4to.

Chambers, George F. Esq. M.R.I.—W. Brodie: Pitcairn Island, and the Islanders in 1850. 16mo. 1851.
Church-Rate Papers, 1858. 8vo.
State of Parties, January 1, 1860. 8vo. 1860.

Cobb, Rev. John W. B.A. (the Author)—Two Lectures on the History and Antiquities of Berkhamstead, Herts. 8vo. 1855.

Dublin Geological Society—Journal, Vol. VIII. Part 2. 8vo. 1859.

Dublin Society, Royal—Journal, Nos. 11 and 14. 8vo. 1859.

Dresser, Christopher, Esq. (the Author)—Rudiments of Botany. 8vo. 1859.

Editors—Artizan for Nov. Dec. 1859; Jan. 1860. 4to.
Athenæum for Nov. Dec. 1859; Jan. 1860. 4to.
Bulletin de la Société de Vaudoise. No. 44. 8vo. 1859.
Engineer for Nov. Dec. 1859; Jan. 1860. fol.
Horological Journal, Nos. 17, 18. 8vo. 1859–60.
Journal of Gas-Lighting for Nov. Dec. 1859; Jan. 1860. 4to.
Mechanics' Magazine for Nov. Dec. 1859; Jan. 1860. 8vo.
Medical Circular of Nov. Dec. 1859; Jan. 1860. 8vo.
Practical Mechanics' Journal for Nov. Dec. 1859; Jan. 1860. 4to.
Revue Photographique, Nov. Dec. 1859; Jan. 1860. 8vo.
St. James's Medley. No. 21. 8vo. 1859.

Faraday, Professor, D.C.L. F.R.S.—Répertoire de Chimie, par C. Barreswil et A. Wurtz. 8vo. Dec. 1859.
Königliche Preussischen Akademie, Berichte, Aug.–Nov., 1859. 8vo.

Franklin Institute of Pennsylvania—Journal, Vol. XXXVIII. Nos. 5, 6.

Horticultural Society—Proceedings, Nos. 4–8. 8vo. 1859.

Irish Academy, Royal—Transactions, Vol. XXIII. Part 2. 4to. 1859.
Proceedings, Vol. VII. Part 1–8. 8vo. 1858–9.

Jablonowski'schen Gesellschaft, Leipzig.—Preisschrifte VII. 8vo. 1859.

Linnean Society—Proceedings, Vol. III. No. 15. 8vo. 1859.

Mackie, S. J. Esq. F.G.S. (the Editor)—The Geologist, Nov. Dec. 1859; Jan. 1860.

Marcet, W. M.D. (the Author)—On Chronic Alcoholic Intoxication. 16mo. 1860.

National Lifeboat Institution—Journal, Nos. 24–26. 8vo. 1857.

Newton, Messrs.—London Journal (New Series), for Nov. Dec. 1859; Jan. 1860. 8vo.

North of England Institute of Mining Engineers—Transactions, Vol. VII. 8vo. 1859.

Novello, Mr. (the Publisher)—The Musical Times for Nov. Dec. 1859; Jan. 1860. 4to.

Petermann, A. Esq. (the Editor)—Mittheilungen auf dem Gesammtgebiete der Geographie. 1859. Heft 11, 12. 4to. Gotha, 1859.
Photographic Society—Journal, Nos. 92, 93. 8vo. 1859.
Royal Society—Proceedings, No. 37. 8vo. 1859.
Scottish Society of Arts (Royal)—Transactions. Vol. V. Part 3. 8vo. 1859.
Statistical Society—Journal, Vol. XXII. Part 4. 8vo. 1859.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Sept. und Oct. 1859. 4to.
Vienna Imperial Geological Institute—Jahrbuch X. No. 2. 8vo. 1859.
Ansprache von W. Haidinger, Nov. 22, 1859.
M. Hörnes, Fossilen Mollusken des Tertiær-beckens von Wien. 4to. 1859.
Volpicelli, Professor P. (the Author)—Sugli Elettrometri Memoria. 4to. Roma, 1858.
United Service Institution—Journal, No. 11. 8vo. 1860.
Washington Observatory, U.S.—Nautical Monographs. No. 1. 4to. 1859.
Wells, T. Spencer, Esq. M.R.I.—Eight Cases of Ovariotomy. 8vo. 1859.
Wilson, Thomas, Esq. (the Author)—Jottings on Money (and on Wages.) 8vo. 1853, and seq.
Ordonnances et Instrvction povr les Changevrs. fol. Anvers, 1633. (With Engravings of Coins).

WEEKLY EVENING MEETING,

Friday, February 10, 1860.

Sir Henry Holland, Bart. M.D. F.R.S. Vice-President, in the Chair.

Professor T. H. Huxley, F.R.S.

On Species and Races, and their Origin.

The speaker opened his discourse by stating that its object was to place the fundamental propositions of Mr. Darwin's work "On the Origin of Species by Natural Selection," in a clear light, and to consider whether, as the question at present stands, the evidence adduced in their favour is, or is not, conclusive.

After some preliminary remarks, in the course of which the speaker expressed his obligations for the liberality with which Mr. Darwin had allowed him to have access to a large portion of the MSS. of his forthcoming work, the phenomena of species in general were considered—the Horse being taken as a familiar example. The distinctions between this and other closely allied species, such as the Asses and Zebras, were considered, and they were shown to be of two kinds, structural or morphological, and functional or physiological. Under the former head were ranged the callosities on the inner side of the fore and hind limbs of the Horse—its bushy tail, its peculiar larynx, its short ears, and broad hoofs: under the latter head, the fact, that the offspring of the horse with any of the allied species is a hybrid, incapable of propagation with another mule, was particularly mentioned.

Leaving open the question whether the physiological distinction just mentioned is, or is not, a universal character of species, it is indubitable that it obtains between many species, and therefore has to be accounted for by any theory of their origin.

The species *Equus caballus*, thus separated from all others, is the centre round which a number of other remarkable phenomena are grouped. It is intimately allied in structure with three other members of the existing creation, the Hyrax, the Tapir, and the Rhinoceros; and less strait, though still definite bonds of union connect it with every living thing. Going back in time, the Horse can be traced into the Pliocene formation, and perhaps it existed earlier still; but in the newer Miocene of Germany it is replaced by the *Hippotherium*, an animal very like a true *Equus*, but having the two rudimental toes in each foot developed, though small. Further back in time, in the Eocene rocks, neither *Equus* nor *Hippotherium* have been met with, nor *Rhinoceros*, *Tapirus*, or *Hyrax*; but instead of them, a singular animal, the *Palæotherium*, which exhibits certain points of resemblance with each of the four existing genera, is found. The speaker pointed out that these resemblances did not justify us in considering the *Palæotherium* as a more generalized type, any more than the resemblance of a father to his four sons justifies us in considering him as of a more generalized type than theirs.

The geographical distribution of the *Equidæ* was next considered; and the anomalies and difficulties it offers were pointed out; and lastly the variations which horses offer in their feral and their domesticated condition, were discussed.

The questions thus shown to be connected with the species Horse, are offered by all species whatever; and the next point of the discourse was the consideration of the general character of the problem of the origin of species of which they form a part, and the necessary conditions of its solution.

So far as the logic of the matter goes, it was proved that this problem is of exactly the same character as multitudes of other physical problems, such as the origin of glaciers, or the origin of strata of marble; and a complete solution of it involves—1. The experimental determination of the conditions under which bodies having the characters of species are producible; 2. The proof that such conditions are actually operative in nature.

Any doctrine of the origin of species which satisfies these requirements must be regarded as a true theory of species; while any which does not, is, so far, defective, and must be regarded only as a hypothesis whose value is greater or less, according to its approximation to this standard.

It is Mr. Darwin's peculiar merit to have apprehended these logical necessities, and to have endeavoured to comply with them. The Pigeons called Pouters, Tumblers, Fantails, &c., which the audience had an opportunity of examining, are in his view, the result of so many long-continued experiments on the manufacture of species; and he considers that causes essentially similar to those which have given rise to these birds are operative in nature now, and have in past times been the agents in producing all the species we know. If neither of these positions can be upset, Mr. Darwin's must be regarded as a true theory of species, as well based as any other physical theory: they require, therefore, the most careful and searching criticism.

After pointing out the remarkable differences in structure and habits between the Carrier, Pouter, Fantail, Tumbler, and the wild *Columba livia*, the speaker expressed his entire agreement with Mr. Darwin's conclusion, that all the former domesticated breeds had arisen from the last-named wild stock; and on the following grounds—1. That all interbreed freely with one another. 2. That none of the domesticated breeds presents the slightest approximation to any wild species but *C. livia*, whose characteristic markings are at times exhibited by all. 3. That the known habits of the Indian variety of the Rock Pigeon (*C. intermedia*) render its domestication easily intelligible. 4. That existing varieties connect the extremest modifications of the domestic breeds by insensible links with *C. livia*. 5. That there is historical evidence of the divergence of existing breeds, *e.g.* the Tumbler, from forms less unlike *C. livia*.

The speaker then analyzed the process of selection by which the domesticated breeds had been produced from the Wild Rock Pigeon; and he showed its possibility to depend upon two laws which hold good for all species, viz., 1. That every species tends to vary. 2. That variations are capable of hereditary transmission. The second law is well understood; but the speaker adverted to the miscomprehension which appears to prevail regarding the first, and showed that the variation of a species is by no means an adaptation to conditions in the sense in which that phrase is commonly used. Pigeon-fanciers, in fact, subject their pigeons to a complete uniformity of conditions; but while the similarly used feet, legs, skull, sacral vertebræ, tail feathers, oil gland and crop undergo the most extraordinary modifications; on the other hand, the wings, whose use is hardly ever permitted to the choice breeds, have hitherto shown no sign of diminution. Man has not as yet been able to determine a variation; he only favours those which arise spontaneously, *i.e.* are determined by unknown conditions.

It must be admitted that, by selection, a species may be made to give rise experimentally to excessively different modifications; and the next question is, Do causes adequate to exert selection exist in nature? On this point, the speaker referred his audience to Mr. Darwin's chapter on the struggle for existence, as affording ample satisfactory proof that such adequate natural causes do exist.

There can be no question that just as man cherishes the varieties he wishes to preserve, and destroys those he does not care about; so nature (even if we consider the physical world as a mere mechanism) must tend to cherish those varieties which are better fitted to work harmoniously with the conditions she offers, and to destroy the rest.

There seems to be no doubt then, that modifications equivalent in extent to the four breeds of pigeons, might be developed from a species by natural causes; and therefore, if it can be shown that these breeds have all the characters which are ever found in species, Mr. Darwin's case would be complete. However, there is as yet no *proof* that, by selection, modifications having the physiological character of species (*i.e.* whose offspring are incapable of propagation, *inter se*) have ever been produced from a common stock.

No doubt the numerous indirect arguments brought forward by Mr. Darwin to weaken the force of this objection are of great weight; no doubt it cannot be proved that all species give rise to hybrids infertile, *inter se;* no doubt (so far as the speaker's private conviction went), a well conducted series of experiments very probably would yield us derivatives from a common stock, whose offspring should be infertile, *inter se:* but we must deal with facts as they stand; and at present it must be admitted that Mr. Darwin's theory does not account for all the phenomena exhibited by species; and so far, falls short of being a satisfactory theory.

Nevertheless the speaker expressed his sense of the extremely high value to be attached to Mr. Darwin's hypothesis; and, avowing his own conviction that the following it out must ultimately lead us to the detection of the laws which have governed the origin of species, he concluded his discourse in the following words, which he wishes to be added in full to the very brief preceding account of his view of Mr. Darwin's argument:—

"I have endeavoured to lay before you what, as I fancy, are the turning points of a great controversy; to render obvious the mode in which the vast problem of the origin of species must be dealt with; and so far as purely scientific considerations go, I have nothing more to say. But let me beg you still to listen to a last word respecting the unscientific objections which I constantly hear brought forward, on the part of the general public, against such doctrines as those we have been discussing. For this is a matter upon which it is of the utmost importance that men of science and the public should come to an understanding. I have heard it said, that it is presumptuous for us to attempt to inquire into such matters as these; that they are problems beyond the reach of the human understanding. Do you remember what was the reply of the old philosopher to those who demonstrated to him so clearly the impossibility of motion? 'Solvitur ambulando,' said he, and got up and walked. And so I doubt not that one of these days either Mr. Darwin's hypothesis, or some other, will get up and walk, and that vigorously; and so save us the trouble of any further discussion of this objection.

"Another, and unfortunately a large class of persons take fright at the logical consequences of such a doctrine as that put forth by

Mr. Darwin. If all species have arisen in this way, say they —Man himself must have done so; and he and all the animated world must have had a common origin. Most assuredly. No question of it.

"But I would ask, does this logical necessity add one single difficulty of importance to those which already confront us on all sides whenever we contemplate our relations to the surrounding universe? I think not. Let man's mistaken vanity, his foolish contempt for the material world, impel him to struggle as he will, he strives in vain to break through the ties which hold him to matter and the lower forms of life.

"In the face of the demonstrable facts, that the anatomical difference between man and the highest of the *Quadrumana* is less than the difference between the extreme types of the Quadrumanous order; that, in the course of his development, man passes through stages which correspond to, though they are not identical with, those of all the lower animals; that each of us was once a minute and unintelligent particle of yolk-like substance; that our highest faculties are dependent for their exercise upon the presence of a few cubic inches more or less of a certain gas in one's blood; in the face of these tremendous and mysterious facts, I say, what matters it whether a new link is or is not added to the mighty chain which indissolubly binds us to the rest of the universe? Of what part of the glorious fabric of the world has man a right to be ashamed—that he is so desirous to disconnect himself from it? But I would rather reply to this strange objection by suggesting another line of thought. I would rather point out that perhaps the very noblest use of science as a discipline is, that now and then she brings us face to face with difficulties like these. Laden with our idols, we follow her blithely—till a parting in the roads appears, and she turns, and with a stern face asks us whether we are men enough to cast them aside, and follow her up the steep? Men of science are such by virtue of having answered her with a hearty and unreserved, Yea; by virtue of having made their election to follow science whithersoever she leads, and whatsoever lions be in the path. Their duty is clear enough.

"And, in my apprehension, that of the public is not doubtful. I have said that the man of science is the sworn interpreter of nature in the high court of reason. But of what avail is his honest speech, if ignorance is the assessor of the judge, and prejudice foreman of the jury? I hardly know of a great physical truth, whose universal reception has not been preceded by an epoch in which most estimable persons have maintained that the phenomena investigated were directly dependent on the Divine Will, and that the attempt to investigate them was not only futile, but blasphemous. And there is a wonderful tenacity of life about this sort of opposition to physical science. Crushed and maimed in every battle, it yet seems never to be slain; and after a hundred defeats it is at this day as rampant, though happily not so mischievous, as in the time of Galileo.

"But to those whose life is spent, to use Newton's noble words, in picking up here a pebble and there a pebble on the shores of the great ocean of truth—who watch, day by day, the slow but sure advance of that mighty tide, bearing on its bosom the thousand treasures wherewith man ennobles and beautifies his life—it would be laughable, if it were not so sad, to see the little Canutes of the hour enthroned in solemn state, bidding that great wave to stay, and threatening to check its beneficent progress. The wave rises and they fly; but unlike the brave old Dane, they learn no lesson of humility: the throne is pitched at what seems a safe distance, and the folly is repeated.

"Surely, it is the duty of the public to discourage everything of this kind, to discredit these foolish meddlers who think they do the Almighty a service by preventing a thorough study of his works.

"The Origin of Species is not the first, and it will not be the last, of the great questions born of science, which will demand settlement from this generation. The general mind is seething strangely, and to those who watch the signs of the times, it seems plain that this nineteenth century will see revolutions of thought and practice as great as those which the sixteenth witnessed. Through what trials and sore contests the civilized world will have to pass in the course of this new reformation, who can tell?

"But I verily believe that come what will, the part which England may play in the battle is a grand and a noble one. She may prove to the world, that for one people, at any rate, despotism and demagoguy are not the necessary alternatives of government; that freedom and order are not incompatible; that reverence is the handmaid of knowledge; that free discussion is the life of truth, and of true unity in a nation.

"Will England play this part? That depends upon how you, the public, deal with science. Cherish her, venerate her, follow her methods faithfully and implicitly in their application to all branches of human thought; and the future of this people will be greater than the past.

"Listen to those who would silence and crush her, and I fear our children will see the glory of England vanishing like Arthur in the mist; they will cry too late the woful cry of Guinever:

'It was my duty to have loved the highest;
It surely was my profit, had I known;
It would have been my pleasure had I seen.'"

[T. H. H.]

WEEKLY EVENING MEETING,

Friday, February 17, 1860.

John Webster, M.D. F.R.S. in the Chair.

Professor F. Crace Calvert, F.R.S.

On the Influence of Science on the Art of Calico-Printing.

Calico printing has partaken of the general progress of the manufacturing arts; and this can be easily understood when it is remembered that it is based upon three distinct branches of knowledge—mechanics, art, and chemistry. Not being acquainted with machinery, I shall not attempt to describe the various mechanical improvements and machines which have been introduced; but shall confine myself to stating that ever since 1815, the period at which it was first extensively applied in the printworks of Lancashire, machinery has gradually supplanted hand labour, and thereby immensely decreased the cost of production, at the same time that it has improved the beauty and precision of the results obtained.

Pencilling and Block-Printing.—During the early part of this century, the production of designs upon calico was performed by means of hand-blocks, made of sycamore or peartree wood, two or three inches thick, nine or ten inches long, and about nine broad. The face of the block was either carved in relief into the desired pattern, like ordinary woodcuts; or the figure was formed by the insertion edgewise into the wood of narrow slips of flattened copper wire, and the patterns were finished by the hand labour of women with small brushes called pencillings. Owing to a strike amongst the block-printers in 1815, to resist the threatened introduction of machinery, great efforts were made on the part of the employers to render themselves independent of hand labour; and the result has been the gradual introduction of cylinder printing. Without entering into the intricate details of the steps by which the art of engraving has been carried to its present high degree of perfection, I shall simply give an outline of the successive improvements alluded to.

Engraving.—The first kind of roller used was made by bending a sheet of copper into a cylinder, soldering the joint with silver, and then engraving upon the continuous surface thus obtained.

The second improvement consisted in producing the pattern on copper cylinders obtained by casting, boring, drawing, and hammering. In this case, the pattern is first engraved in intaglio upon a roller of softened steel, of the necessary dimensions. This roller is then hardened and introduced into a press of peculiar construction, where, by rotatory pressure, it transfers its design to a similar roller in the soft state, and the *die* being in intaglio, the latter, called the "*mill*," is in relief. This is hardened in its turn, and by proper machinery is made to convey its pattern to the full-sized copper roller. This improvement alone reduced the cost of engraving on copper rollers many hundreds per cent.; and, which is of far greater importance, made practicable an infinite number of intricate engravings which could never have been produced by hand labour applied directly to the roller.

A further improvement was made by tracing with a diamond on the copper roller, covered with varnish, the most complicated patterns by means of excentrics, and then etching.

The combination of mill engraving with the tracing and etching processes naturally followed, adding immensely to the resources of the engraver and printer in production of novel designs.

Another development of this art is the tracing of patterns on the surface of rollers, which has been effected by machines constructed on the principle of the pentagraph. Although this invention dates from 1834, still it is only of late years that it has been successfully applied.

But if mechanical art has greatly assisted the engraver, chemistry has rendered him equally important services, by enabling him to abandon costly and cumbrous modes of impressing by force the designs on the cylinder, substituting for them a great number of etching processes. By some of these processes, as by every other addition to the resources of the engraver, an entirely new and beautiful class of engraving is produced, unattainable by any other known means.

A very recent improvement is highly interesting in a scientific point of view. It is the application of galvanism to the diamond tracer. By combining the galvanic action with the excentric motion, most beautiful and delicate engravings can be produced. This is effected by tracing the pattern with a varnish on a zinc cylinder, which is so placed in the engraving machine, that as a needle passes over its surface, and comes in contact with the zinc, the galvanic current is established, and by simple machinery, causes the diamond to trace the corresponding pattern on the copper roller. The communication is so rapid and so precise, that this invention of Mr. Gaiffe, of Paris, bids fair to produce very important results. Galvanism is also made use of, for producing effects on roller surfaces by depositing copper thereon.

To give an idea of the extraordinary influence which the introduction of machinery and improvements in engraving have had in cheapening the cost of printed calicoes, I may state, that large furniture patterns, such as are required for Turkish, Egyptian, and Persian markets, into which 16 colours and shades enter, would have cost formerly, from 30*s*. to 35*s*. per piece, because they would have required 16 distinct applications of as many different blocks, and would have occupied more than a week in printing; whereas the same piece can now be printed in one single operation, which takes three minutes, and costs 5*s.* or 6*s.* So rapid is the progress of one branch of manufacture in connexion with another, that it has only recently been possible to produce the rollers capable of performing this operation, that is to say, cylinders of copper 43 inches in circumferece, by 44 inches long. For light styles of printing, the time required to print a piece of 36 yards is not more than one minute.

CHEMISTRY.—But the discovery which has exercised more influence than any other on the progress of calico printing, is the application of *chlorine* gas as a bleaching agent. Previously to the employment of this gas, (chiefly as bleaching powder) the imperfect bleaching of a piece of calico required six weeks; and as it had to be exposed to the action of the atmosphere, a large surface of land was required. Further, at that time, bleachers had to use potashes imported from Canada; whereas, at the present time, thanks to the progress of chemical knowledge, not only is soda-ash manufactured in this country; but by the application of bleaching powder, calicos are much better bleached in 24 hours than they were formerly by a six weeks' exposure to the atmosphere; and even when an extra cleaning and whiteness is required, as for madder goods, only two days are necessary. The aid of machinery renders possible the continuous process; that is to say, several hundred pieces of gray calico are sewn together, end to end, and made to pass from one operation to another, without any pause, until they are bleached. So rapid and economical is this method, that the cost of bleaching a piece of calico, does not exceed one or two pence. Chlorine, again, renders a great service to the calico-printer, by enabling him, after his *madder goods* have been produced and soaped, to obtain fine whites without the necessity of exposing them for several days in the meadows to the action of the atmosphere. In fact, the discovery of *garancine* and *alizarine*, and their application to calico-printing, have facilitated the production of madder styles, at very low cost, as the whites of such goods require no soaping, and only a little bleaching or cleaning powder.

Cotton has this peculiarity, as distinguished from wool and silk, that it will not fix any organic colour, excepting indigo, without the interposition of a mordant, which is generally a metallic oxide or salt. The two most important discoveries in connexion with this necessity of calico-printing were; first, that made in 1820, by Mr. George Wood, of Bankbridge, who found out the means of preparing calicos with peroxide of tin, which enabled printers to produce a large variety of prints called *steam goods;* and secondly, that of Walter Crum, Esq., F.R.S., who in a paper presented to the British Association, at Aberdeen, in 1859, showed that the tedious process of ageing madder mordants for three or four days, might be dispensed with, by passing the goods during a quarter of an hour through a moist atmosphere, at a temperature of 80° to 100°, where the mordants absorb the required quantity of moisture, and then rapidly undergo the chemical changes necessary to fit them for producing the black, purple, lilac, red, pink, and chocolate colours, which the madder root will yield immediately in the dyebeck, according to the nature of the mordant previously fixed in the cloth.

As it is impossible, in the brief space of an hour to convey an idea how various colours are produced on prints, I shall confine my remarks to illustrating the interesting fact that abstruse science has brought to light various substances, which have lately proved valuable accessories to the resources of the calico-printer. Thus, Dr. Prout, some thirty or forty years ago, made the curious discovery, that uric acid possessed the property of giving a beautiful red colour, when heated with nitric acid and then brought into contact with ammonia. The substance thus obtained was further examined by Messrs. Liebig and Wöhler, in a series of researches which have been considered as amongst the most important ever made in organic chemistry; and this substance they called *Murexide*. In the course of these investigations, they also discovered a white crystalline substance called Alloxan. For twenty years both these substances were only to be found in the laboratory; but in 1851 Dr. Saac observed that alloxan, when in contact with the hand, tinged it red. This led him to infer that alloxan might be employed to dye woollens red; and further experiments convinced him that if woollen cloths were prepared with peroxide of tin, passed through a solution of alloxan, and then submitted to a gentle heat, a most beautiful and delicate pink colour resulted. Subsequently murexide was employed and applied successfully by Mr. Depouilly, of Paris, to dyeing wool and silk, and to printing calicos, by the aid of oxide of lead and chloride of mercury as mordants; but the great obstacle to its extensive use was the difficulty of obtaining uric acid in sufficient quantity for its manufacture. The idea soon occurred to chemists to extract it from guano; and this is the curious source whence the chief supply of uric acid is obtained, and which enables Edmund Potter, Esq., and other printers, to produce the colour called Tyrian purple.

Another example will be found in the successive scientific discoveries which have led to the discovery of the recently popular colour, *Mauve*. Lichens, which have been the subject of extensive researches on the part of Robiquet, Heeren, Sir Robert Kane, Dr. Schunck, and especially of Dr. Stenhouse, have yielded to those chemists several new and colourless organic substances, which, under the influence of air and ammonia, give rise to most brilliant colours, and amongst these are orchil and litmus. Dr. Stenhouse, in a most elaborate paper, published by the Royal Society in 1848, pointed out two important facts; first, that the colour-giving acids could be easily extracted from the weed by macerating it in lime water, from which the colouring matters were easily separated by means of an acid; and, secondly, the properties of certain colouring acids, which gave M. Marnas, of Lyons, the key which enabled him to produce commercially from lichens, a fast mauve and purple which up to 1857 had been considered impossible of attainment.

The commercial production, by Mr. W. H. Perkin, of another purple, at the same time, is not less interesting. Some thirty or forty years ago, Dr. Runge obtained from coal-tar six substances; amongst which was one called *Kyanol*, which substance was thoroughly examined by Dr. W. A. Hofmann, who proved it to be an organic alkaloid, and identical with a substance known by the name of aniline. Owing to the subsequent study of this substance by that eminent chemist, and the discovery that it yielded a beautiful purple colour when placed in contact with bleaching powder, his pupil, Mr. W. H. Perkin, was induced to make experiments, with a view to producing commercially, a fast purple, in which he succeeded, and secured it by a patent in 1857. The process devised by this chemist is exceedingly simple. It consists in oxidising aniline by means of bichromate of potash and sulphuric acid. I shall not attempt to give any further details on this subject, as they have been very ably described by Mr. Robert Hunt, in the *Art Journal.*

More recently Mr. Renard found a method of producing also from aniline, by means of chlorine eompounds, a most splendid rose colour, called by him *Fuchsiacine;* and, within the last few months, Mr. David Price has also succeeded in producing from aniline, by the employment of peroxide of lead, either a fast purple, or a pink, called by him Roseine, and a fast blue, according to the mode of operating. All these colours require special mordants to fix them on calicos or muslins; and the beautiful specimens which I have the honour to lay before you I owe to the kindness of Messrs. James Black and Co., and Messrs. Boyd and Hamel, of Glasgow, who have fixed the last-mentioned colours by means of azotised principles, such as albumen, lactarine, &c.

I cannot give a better idea of the immense magnitude of the calico-printing trade than by quoting the number of yards exported, which amounted, in 1858, to 785,666,473—and give a price value of £13,147,280.

I cannot conclude without expressing, also, my thanks to Mr. Wood, of the firm of Wood and Wright, and Mr. R. Leake, of the firm of Lockett, Sons, and Leake, Messrs. Dalglish and Faulkners, for the numerous and valuable specimens which they have kindly lent me to illustrate my discourse ; and especially to Mr. W. Grant for the loan of a most interesting book, which bears the date of 1790, containing the patterns belonging to the late firm of Sir Robert Peel, Bart.

[F. C. C.]

WEEKLY EVENING MEETING,

Friday, February 24, 1860.

Sir Henry Holland, Bart. M.D. F.R.S. Vice-President, in the Chair.

William B. Carpenter, M.D. F.R.S. F.G.S. F.L.S.

On the Relation of the Vital to the Physical Forces.

In every period of the history of Physiology, attempts have been made to identify all the forces acting in the living body with those operating in the inorganic universe. Because muscular force, when brought to bear on the bones, moves them according to the mechanical laws of lever-action, and because the propulsive power of the heart drives the blood through the vessels according to the rules of hydraulics, it has been imagined that the movements of living bodies may be explained on physical principles;—the most important consideration of all, namely, the source of that contractile power which the living muscle possesses, but which the dead muscle (though having the same chemical composition) is utterly incapable of exerting, being altogether left out of view. So, again, because the digestive process, whereby food is reduced to a fit state for absorption, as well as the formation of various products of the decomposition that is continually taking place in the living body, may be imitated in the laboratory of the chemist; it has been supposed that the appropriation of the nutriment to the production of the living organized tissues of which the several parts of the body are composed, is to be regarded as a chemical action;—as if any combination of albumen and gelatine, fat and starch, salt and bone-earth, could make a living man, without the constructive agency inherent in the germ from which his fabric is evolved.

A scarcely less unphilosophical method has been pursued by another class of reasoners, who have cut the knot which they could not untie, by attributing all the actions of living bodies for which physics and chemistry cannot account, to a hypothetical "vital principle"; a shadowy agency, that does everything in its own way, but refuses to be made the subject of scientific examination; like the electricity or the spiritual power, to which the lovers of the marvellous are so fond of attributing the mysterious movements of turning and tilting tables.

A decided advance has been made in Physiology, however, by the introduction of the dynamical ideas furnished by physics and chemistry, into the domain of Life. The phenomena that seem altogether peculiar to living bodies are now ranked in the category of *vital* actions; they are regarded as the results of a special form of force, which is as distinct from chemical affinity or from mechanical power, as these are from each other; and the scientific physiologist aims to discover the laws of its operation, just as the chemist seeks for the laws of affinity, or the physicist for those of motion.

Now, of all forms of vital action, there is none so universal, so fundamental, or so characteristic, as that by which the living organism is built up—or rather builds itself up—from the germ, by the appropriation of materials derived from external sources, and subsequently maintains itself in its typical form during its term of life; and the force which effects this has been commonly designated by some distinctive title, such as the *nisus formativus*, the *bildungstrieb*, or the *germ-force.* This force has been commonly considered to be originally inherent in the germ, and to have been derived, like the material substance of that germ, from the parental organism which produced it. In this mode of viewing the subject, all the organizing force required to build up an oak or a palm, an elephant or a whale, must be concentrated in a minute particle, only discernible by microscopic aid; and the aggregate of all the germ-forces appertaining to the descendants, however numerous, of a common parentage, must have existed in their progenitor. Thus, in the case of the successive viviparous broods of *Aphides*, a germ-force capable of organizing a mass of living structure, which would amount (it has been calculated) in the tenth brood to the bulk of 500 millions of stout men, must have been shut up in the single individual, weighing perhaps the 1-1000th of a grain, from which the first brood was evolved. A more complete *reductio ad absurdum* can scarcely be brought against any hypothesis; and we may consider it proved that, in some way or other, fresh organizing force is constantly being supplied, *ab externo*, during the whole period of the exercise of its activity.

When we carefully look into the question, we find that what the *germ* really supplies is not the force, but the *directive* agency; thus rather resembling the control exercised by the superintendent builder who is charged with working out the design of the architect, than the bodily force of the workmen who labour under his guidance in the construction of the fabric. The agency of the germ may be regarded, like Magnetism, as a *static* force; and just as magnetism requires to be combined with motion to enable it to develope electricity, so does the directive agency of the germ need the co-operation of a *dynamic* force for the manifestation of its organizing power. That dynamic force, as we learn from an extensive survey of the phenomena of life, is Heat; the influence of which upon the rate of growth and development, both animal and vegetable, is so marked as to have universally attracted the attention of physiologists, who, however, have only recognised in it a *vital stimulus*, calling forth the latent power of the germ, instead of looking upon it as itself furnishing the power that does the work. It has been from the narrow limitation of the area over which physiological research has been commonly prosecuted, that the intimacy of this relationship between heat and the organizing force has not sooner become apparent. Whilst the vital phenomena of warm-blooded animals, which possess within themselves the means of maintaining a constant temperature, were made the sole, or at any rate the chief objects of study, it was not likely that the inquirer would recognize the influence of external heat in accelerating, or of cold in retarding, their functional activity. It is only when the survey is extended to cold-blooded animals and to plants, that the immediate and direct relation between Heat and Vital energy, as manifested in the rate of growth and development, or of other changes peculiar to the living body, is unmistakeably manifested.

The action of Heat on the Vegetable kingdom, it is true, cannot be fully effective without Light; but this latter force would appear to be concerned rather in providing the materials at the expense of which the plant grows, than in converting these materials into living tissue. Its action is limited to the surface; and it is mainly exerted in decomposing the carbonic acid of the surrounding medium, and in uniting the carbon, (oxygen being set free) with oxygen and hydrogen, to form starch, chlorophyll, &c., and with nitrogen in addition to form albuminous compounds. If supplied with these by other means, plants can grow without light; as we see in the instances of the germinating seed, which lives on the store laid up by its parent in the cotyledons, and of leafless parasites and fungi, which appropriate materials previously prepared by other plants. On the other hand, the rate at which germination proceeds is entirely regulated by the degree of heat to which the seed is subjected; and the same thing is true with regard to the rate of development of the egg in cold-blooded animals. By the time that the plumula of the young plant is beginning to unfold, the store of material laid up in the seed is exhausted; and henceforth the plant is mainly dependent upon Light for the power of forming (at the expense of the materials supplied by the inorganic world) those organic compounds, without a due supply of which no further growth or development can take place; the continued action of Heat being required to turn these materials to account.

A note-worthy suggestion has lately been made by Prof. Le Conte (of South Carolina College, U.S.), with reference to the fact that in germination a certain proportion of these organic compounds is given back to the inorganic world, in that liberation of carbonic acid which is a remarkable feature of the process; and that, during the whole subsequent life of plants, a similar process is continually taking place to a certain extent, even whilst the green surfaces of the plant are most actively engaged in fixing carbon from the atmosphere, and in generating organic compounds. May not the force liberated (he asks) in the fall of a certain portion of these organic products down to the lower level of simple binary compounds, be necessary for the elevation of another portion to the rank of living tissue? That such is not unlikely to be the true view of the case, would appear from the circumstance that recent experiments have shown that, to keep up a warm-blooded animal to its full weight, a considerably larger amount of food is required than would suffice to supply the necessary waste of the body, as determined by the loss of weight which it undergoes when food is entirely withheld; from which it seems quite clear that, in some mode or other, the decomposition of a certain proportion of the food, *i.e.* its descent from a higher to a lower form of chemical combination, is necessary to the conversion of the rest into living organized tissue.

The continual decay which is going on during the life of a Plant (by the seasonal death of its leaves, as well as by the decomposition just adverted to) restores to the inorganic world, in the form of carbonic acid, water, and ammonia, a large part of the materials drawn from it in the act of vegetation; and, with the exception of those vegetable products which are consumed as food by animals, or which are preserved (like timber, flax, cotton, &c.) in a state of permanence, the various forms of decomposition which take place after death complete that restoration. But in returning, however slowly, to the condition of water, carbonic acid, ammonia, &c., the constituents of plants give forth an amount of heat equivalent to that which they would generate by the process of ordinary combustion; and thus they restore to the inorganic world not only the *materials* but the *forces*, at the expense of which the vegetable fabric was constructed. It is for the most part only in the humblest plants, and in a particular phase of their lives, that such a restoration takes place in the form of *motion;* this motion being, like growth and development, an expression of the vital activity of the "zoospores" of Algæ, and being obviously intended for their dispersion.

Hence we seem justified in affirming that the correlation between Heat and the Vital force of Plants is not less intimate than that which exists between Heat and Motion. The special attribute of the vegetable germ is its power of effecting the metamorphosis, and of utilizing the organizing force according to the plan of construction characteristic of each species.

It is a consideration of no little interest, that on this view the light, heat, and mechanical power, which we now obtain from the combustion of Coal, are really derived, through the vegetable life of the carboniferous epoch, from the light and heat communicated to the Flora of that epoch by the sun; a fact which was discerned by the genius of George Stephenson, before the general doctrine of the "correlation of forces" had been given to the world by Mr. Grove.

The results of the inquiry, therefore, so far as developed in this discourse, (the forces specially concerned in producing the phenomena of Animal life not having been included, for want of time) are in complete harmony with the great doctrine of the conservation of force, according to which it is no less true that *nil fit ad nihilum*, than that *nil fit ex nihilo.*

[W. B. C.]

WEEKLY EVENING MEETING,

Friday, March 2, 1860.

Sir Henry Holland, Bart. M.D. F.R.S. Vice-President, in the Chair.

Professor H. E. Roscoe,

On the Measurement of the Chemical Action of the Solar Rays.

Those portions of the solar rays which vibrate most slowly, and are situated near the red end of the spectrum, are those which mainly regulate the alterations of temperature on the surface of our planet. They are, *par excellence*, the heating rays. They principally produce all those motions in our atmosphere which we term winds; they effect those grand phenomena of distillation and deposits which we call rains; and the amount and distribution of those heating rays at any point on the earth's surface determines the thermal climate of that point.

On a scale, perhaps less grand, but certainly not less important as regards their effects, are the actions produced by the most rapidly vibrating portion of the sun's rays; those, namely, which are situated near the violet end of the spectrum. These rays have been called the chemical rays, because it is by these especially that the chemical action of the sunlight is effected. It is in presence of these rays alone that the plant is enabled to decompose the carbonic acid of the air, to assimilate the carbon, restoring the oxygen for the subsequent use of animals. Hence the amount and distribution of these rays at any given place regulates to a great extent the character of the fauna and flora; gives, in short, the "chemical climate" of the place.

The measurement of the quantity of this solar energy, falling at any time on a given spot upon the earth's surface, must be a subject of primary importance in the determination of the physical history of our globe. We fortunately possess a method, although it is only a comparative one, for measuring the amount of effect which the heating rays produce, that is for measuring *temperature*. No such mode of measurement for those of the solar rays which especially effect chemical action has, up to the present time, been adopted; not that meteorologists have ignored the importance of the subject, but because the difficulties which beset the establishment of a measuring instrument for chemical action were considered to be insurmountable.

The speaker remarked, that his object was to bring before his audience the principles and mode of action of a method employed for the measurement of the chemical action of light.*

As an illustration of the chemical action of light, attention was directed to the fact, that when a perfectly pure mixture of exactly equal volumes of chlorine and hydrogen gases is exposed to light, the gases combine, producing an equal volume of hydrochloric acid gas, whilst no such combination occurs in the dark. This combination may occur gradually, or with great rapidity. If the chemical activity of the light be great, the union takes place quickly, great heat is evolved, a sudden expansion takes place, and the vessel containing the mixture of chlorine and hydrogen is shattered by the explosion. The gradual or slow combination may be rendered evident by allowing the hydrochloric acid thus formed to be absorbed by water; the consequent diminution of bulk of the gas accurately representing the chemical action effected.

This mixture of equal volumes of chlorine and hydrogen is used as the sensitive substance for measuring the chemical action of light. It is evolved in the perfectly pure state by the electrolytic decomposition of strong aqueous hydrochloric acid; and it is by this method only, that it can be prepared. The gases thus evolved are in the exact proportion in which they exist in hydrochloric acid; so that, if by any means, we re-combine these gases, no trace of either substance will remain behind, the whole uniting to form hydrochloric acid.

For the purpose of measuring this chemical action, effected, not only by solar light, but also by light from many artificial sources, we require some instrument, which is to the chemical action of light what the thermometer is to the heat actions; an instrument which will show objectively the amount of chemically active light. We must be sure, in the first place, that our mode of measurement is a reliable one. That, as in the case of the thermometer, equal increments of volume, correspond to equal increments of heat, so, in the new instrument, the indications, however obtained, shall be proportional to, and represent the amount of chemical rays emanating from, any source.

This has been accomplished in the chemical photometer; by the help of which an accurate measurement of the chemical action of light is effected.

The facts upon which this mode of measurement is based, may be summed up as follows:

1. Exactly equal volumes of chlorine and hydrogen gases, when mixed, combine together on exposure to light, forming hydrochloric acid gas.
2. This combination does not occur in the dark.

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* For a detailed description of apparatus, &c. see "Photochemical Researches," Part 1. "Measurement of the Chemical Action of Light," by R. Bunsen and H. E. Roscoe.—*Phil. Trans.* 1857, p. 355.

3. The quantity of hydrochloric acid thus formed is directly proportional to the intensity of the incident light, and serves, therefore, as a measure of the chemical action produced.
4. The chemical photometer is an instrument, by help of which the quantity of hydrochloric acid thus formed, can be accurately measured.

The chemical photometer consists essentially of three parts; namely, first, the apparatus in which the sensitive gas is generated; secondly, the apparatus in which the gas is exposed to the light; and thirdly, the apparatus in which the volume of hydrochloric acid produced in a given time is read off.

When very numerous precautions in the management of the photometer are taken, it proves a most sensitive and reliable instrument. Having thus obtained an instrument by which the chemical action of light can be accurately measured, it only remains to graduate it. For this purpose we require a standard of light, from which the determination is to proceed. For this comparative measurement, the possession of a constant source of light is the first essential. This is obtained as follows:—

1. A flame of pure carbonic oxide gas, burning in the air and issuing from an opening of given size at a given rate, is employed as the *standard flame.*
2. *The unit amount of chemical action*, is that effected by such a flame upon the sensitive mixture of chlorine and hydrogen during one minute, at the distance of one metre.
3. The quantity of chemically active light producing this action is called *one chemical unit of light;* and ten thousand of such units *one chemical degree of light.*
4. The chemical photometer is graduated by observing how many of these chemical units of light correspond to one division on the scale of the instrument.

As an illustration of the mode in which this measurement of the chemical action of light is employed, the speaker described the method by which the chemical action produced by the direct solar rays has been determined.* For this purpose, it was necessary to admit a very small, but a known, portion of direct sun-light into the dark room in which the instrument was placed, and to allow the insolation vessel to be bathed in the pencil of rays thus admitted. By help of Silbermann's heliostate, the sun's image was reflected during the whole day upon one spot, a small opening of known size, in the window shutter of a dark room. The fraction of the total sun's rays thus admitted and allowed to fall upon the chemical photometer can be calculated, and the action thus effected, observed; hence the amount

* The full memoir on this subject is to be found in Poggendorff's Annal. Bd. CVIII. p. 193. In Abstract, Proceedings Royal Society, Vol. x. p. 39, 1859. Photochemical Researches, Part 4, by R. Bunsen and H. E. Roscoe.

of action can be found which the sun would have produced if directly shining upon the instrument; a condition, impossible of course to fulfil, as the action would become too rapid and the whole apparatus would be shattered by explosion.

The day chosen for observation of the sun's action must obviously be cloudless, if we wish to obtain an idea of the relation existing between the chemical action and the height of the sun. Beginning the observations as near sunrise as possible, we find, for instance, on September 15th, 1858, one of the days on which such a series of experiments was made, that at $7^{h.}$ $9^{m.}$ a.m., when the sun's zenith distance was 76° 30′, the observed action amounted to 1·52. That is, in one minute the column of water moved through 1 52 division; or the quantity of hydrochloric acid formed, when the sun stood at the height mentioned, was represented by 1·52 division on the scale.

Gradually, as the day wore on, the observed action for each minute became larger; until at $9^{h.}$ $14^{m.}$ a.m., the latest observation possible on the day in question, owing to the formation of clouds, the action reached 18·5 divisions, or was thirteen times as large as at 7^h 9^m. In the last column of the accompanying table is found the action, expressed in degrees of light, which would have been observed at the foregoing times, if the whole sunlight had been allowed to fall on the instrument.

TABLE I.

Hour.	Sun's Zenith. Distance.	Observed Action. 1 Minute.	Total Sun's Action in Degrees of Light.
7^h 9^m	76° 30′	1·52	5·54
7 26	73° 49′	4 22	15·50
7 40	71° 37′	6·09	22·43
8 0	68° 34′	7 56	27·85
8 7	67° 30′	8·38	38 87
8 26	64° 42′	12·48	45·85
8 54	60° 48′	17 09	62·59
9 14	58° 11′	18·51	67·61

This great increase in the chemical action with the rise of the sun in the heavens simply results from the fact that the solar rays, in passing through the air, are extinguished or absorbed, lost in fact as light; and that as the sun rises higher above the horizon, the column of air through which the rays pass is constantly being lessened; consequently more of the direct rays reach the earth.

Now, the law according to which the direct rays of the sun are thus absorbed in the air can be obtained from the experiments, of which the foregoing is only an example; hence, if the action which the sun produces, when at a given height, is known, it is possible to calculate the action which it would produce at any other height.

That these calculated results agree very closely with the experimental data,—with the observed action,—is seen by comparing the numbers in Table, No. II., expressing the observed and calculated action.

TABLE II.

The amount of Chemical Action effected at a point upon the Earth's Surface on any cloudless day, by the direct Solar Rays, depends alone upon the Sun's zenith distance; or upon the height of the column of air through which the Rays have to pass.

Sun's Zenith Distance at time of Observation.	Chemical Illumination of Sun's Direct Rays at the Earth's Surface expressed in degrees of Light.	
	Observed.	Calculated.
46° 8′	93·0	96·4
50° 51′	89·2	85·8
57° 35′	63·1	67·9
58° 11′	67·6	66·2
60° 48′	62·6	58·3
64° 42′	45·9	47·9
67° 30′	38·9	36·6
68° 34′	27·9	33·1
71° 37′	22·4	24·5
73° 49′	15.5	16·3
76° 30′	5·5	9·2

Probable error = $\pm$ 2·7 degrees of Light.

Knowing the law which regulates the absorption of the chemical rays, we can calculate what the action would be if there were no atmosphere to diminish the power of the rays. It is thus found that if the sun's rays were not thus weakened, by passage through the atmosphere, they would produce an illumination represented by 318 *degrees of light:* or they would effect a combination in one minute, upon an unlimited atmosphere of chlorine and hydrogen on which they fell perpendicularly, of a column of hydrochloric acid, 35·3 *metres in height.* The sun's rays having passed perpendicularly through our atmosphere to the sea's level, effect an action of only 14·4 *light metres;* or nearly two-thirds of their chemical activity has been lost by extinction and dispersion in the atmosphere.

A large number of most interesting conclusions may be drawn from the facts already noticed. Thus, for instance, we may determine the chemical action which the solar rays will produce on the various planets; for we know that the intensity of the chemical illumination varies inversely as the square of the distance of the planet from the sun. The numbers in Table III., express this chemical action in degrees of light, and in heights of columns of hydrochloric acid called *light metres.* Hence, we see how much the sun's chemical action varies on the different planets; the superior planets receiving so small a portion as to render it impossible that the kind of animal and vegetable life which we here enjoy can there exist.

TABLE III.—*Chemical Action produced by Direct Sunlight on each Planet.*

	Mean Distances.	Chemical Action in Light Degrees.	Chemical Action in Light Metres.
Mercury . .	0·387	2125·0	235.4
Venus . . .	0·723	608·9	67·5
Earth . . .	1·000	318·3	35·3
Mars . . .	1·524	137·1	15·2
Jupiter. . .	5·203	11·8	1·2
Saturn . . .	9 539	3.5	0·4
Uranus . .	19·183	1·0	0·1
Neptune . .	30·040	0·4	0·04

Interesting conclusions can be drawn from these facts, concerning the distribution of the chemical rays on the surface of our earth in different latitudes, and at different elevations above the sea's level. The farther removed a situation is from the level of the sea, the higher up in the atmosphere it is placed, the greater amount of chemical action it will receive. Thus, in the highlands of Thibet, where corn and grain flourish at a height of from 12,000 to 14,000 feet, the chemical action of the direct sunlight is 1½ times as great as in the neighbouring lowland plains of Hindostan. In the same way we can calculate for any point of the earth's surface whose latitude is known, the amount of chemical action which the direct sunlight effects at any given time of day or year. In Table IV. the numbers represent the chemical

TABLE IV.—*Chemical Action effected by Direct Sunlight in One Minute on the Vernal Equinox at*

A. Melville Island.
B. Rejkiavik, Iceland.
C. St. Petersburg.
D. Manchester.
E. Heidelberg.
F. Naples.
G. Cairo.

Hour.	A.	B.	C.	D.	E.	F.	G.
6 a.m. or 6 p.m.	0·0	0·0	0·0	0·0	0·0	0·0	0·0
7 „ 5	0·0	0·02	0·07	0·22	0·38	0·89	1·74
8 „ 4	0·07	1·53	2·88	5·85	8·02	13·31	20·12
9 „ 3	0·67	6·62	10·74	18·71	23·99	35·88	50·01
10 „ 2	1·86	13·27	20·26	32·91	40·94	58·46	78·61
11 „ 1	3·02	18·60	27·55	43·34	53·19	74·37	98·33
12 at noon	3·51	20·60	30·26	47·15	57·62	80·07	105·3

action effected by direct sunlight in one minute at the places and hours named on the 21st of March. Curves were exhibited, showing the rise of the action, with the progress of the sun through the heavens. By comparing the numbers in the table, it is seen how greatly this chemical action differs at various points on the earth's surface; and we can understand how it is, that at the latitude of Cairo, where the chemical action of the direct sunlight is twice as great as it is in that of Manchester, the whole flora and fauna assume a more tropical and luxuriant character.

The speaker stated, that he was only able briefly to notice the principles upon which the new mode of measuring the chemical aotion of light depends; adding one or two illustrations of the measurements actually made. He was unable even to refer to one of the most interesting and important applications, viz., the measurement of the chemical action effected by the diffuse daylight. This has, however, been accomplished, and we are now able to calculate the amount of chemical action produced by both diffused and direct solar light, on a cloudless day, at any place situated above the latitude of Cairo. The following table shows the results of such a calculation.

Table V.—*Total Chemical Action effected by the Solar Rays from Sunrise to Sunset on the Vernal Equinox, at*

	I.	II.	III.	IV.
Melville Island . .	1196	10590	11790	1306
Rejkiavik . . .	5964	15020	20980	2324
St. Petersburg . .	8927	16410	25340	2806
Manchester . . .	14520	18220	32740	3625
Heidelberg . . .	18240	19100	37340	4136
Naples	26640	20550	47190	5226
Cairo	36440	21670	58110	6437

I. Gives the action of direct sunlight in degrees of light.
II. „ diffuse daylight „
III. „ total light „
IV. „ „ in light metres.

Knowing the intimate connection of the chemically active solar rays with the plant and animal-producing power of a country, no one can doubt the immense importance of the establishment of a regular series of measurements of the variations of the amount of these chemical rays at different situations on the earth's surface. Such a series would open an entirely new field to the science of meteorology, and would reveal to us relations and points of difference as regards the chemical climate, at present wholly unknown.

The chemical photometer, in the hands of an experienced experimenter, is a perfectly accurate and extremely sensitive instrument; and the method described is a most valuable one for scientifically investigating the primary laws regulating the chemical action of light, and the distribution of the chemical rays. It is, however, not capable of universal application as a meteorological instrument, owing to its complicated nature, and the great care requisite in its management. At present we know of no easy, and at the same time correct, method of estimating the chemical action of light. Much time and labour has already been spent by the authors of the method described in endeavouring to prepare an instrument, which can be practically used for this purpose in meteorological observations. Persevering in their efforts, they hope ere long to overcome the numerous difficulties which beset the subject, and to describe a method which shall answer the proposed end.

[H. E. R.]

GENERAL MONTHLY MEETING,

Monday, March 5, 1860.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Thomas Farmer Baily, Esq.
George Francis Brown, Esq.
Joseph Brown, Esq.
Stephen Busk, Esq.
Charles William Franks, Esq.
John Peter Gassiot, jun. Esq.
Thomas Greg, Esq.
Rev. George Godwin Pownall Glossop, A.M.
Thomas John Kent, Esq.
Robert Morant, Esq.
John Charles Salt, Esq. and
Edward Woods, Esq.

were duly *elected* Members of the Royal Institution.

John Morgan, Esq.
Arthur Giles Puller, Esq. and
William Salmon, Esq.

were *admitted* Members of the Royal Institution.

The Secretary announced, That the following Arrangements had been made for the Lectures after Easter :—

Seven Lectures on the Structure, Habits, and Affinities of Herbivorous Mammalia, with especial reference to certain Species now living in the Zoological Society's Gardens, Regent's Park, by T. Spencer Cobbold, M.D. F.L.S.

Eight Lectures on SOME RECENT RESEARCHES IN PHYSICAL GEOGRAPHY AND GEOLOGY, by DAVID T. ANSTED, Esq. M.A. F.R.S.

Eight Lectures on SOME RESULTS OF THE ASSOCIATION OF HEAT WITH CHEMICAL FORCE, PRACTICALLY APPLIED, by F. A. ABEL, Esq. Director of the Chemical Department, Royal Arsenal, Woolwich.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same.

The Special Thanks of the Members were returned to the Lord OVERSTONE, *M.R.I.* and to ARTHUR DE NOE WALKER, Esq. *M.R.I.* for their valuable Donations, mentioned below :—

LORD OVERSTONE—A Select Collection of Scarce and Valuable Tracts on Political Economy. (Privately printed at his Lordship's expense.) Edited by J. R. McCulloch. 4 vols. 8vo. 1857–9.

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Index.

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7. D. Hume on Public Credit. 1752.
8. W. Blackstone—Account of the National Debt. (From his Commentaries, published 1765-8.)
9. R. Price, on the National Debt. 1774.
10. J. Wimpey, Extracts from "The Challenge," a letter to Price. 1772.
11. Note on the Sinking Fund, established by Mr. Pitt in 1786.
12. On the Annual Million Bill and the Sinking Fund. 1787.
13. R. Hamilton, on the National Debt. 1818.
Index.

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1. Sir Walter Raleigh on Trade and Commerce.
2. J. Evelyn—Navigation and Commerce. 1674.
3. Plan of English Commerce. 1730.
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Index.

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9. Dissertation on the Poor Laws. 1776.
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11. A. Bell, on the Prohibition of the Use of Grain in Distilleries. 1808.

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Chinese Literature:

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Portfolio Chinensis; a Collection of Chinese State Papers, illustrative of the History of the Present Position of Affairs in China. With a Translation, Notes, and Introduction by J. S. Shuck. 8vo. Macao, 1840.

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Agricultural Society of England, Royal—Journal, No. 44. 8vo. 1859.
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Athenæum for Feb. 1860. 4to.
Engineer for Feb. 1860. fol.
Journal of Gas-Lighting for Feb. 1860. 4to.
Mechanics' Magazine for Feb. 1860. 8vo.
Medical Circular for Feb. 1860. 8vo.
Practical Mechanics' Journal for Feb. 1866. 4to.
Revue Photographique, Feb. 1860. 8vo.
Faraday, Professor, D.C.L. F.R.S.—Répertoire de Chimie, par C. Barreswil et A. Wurtz. Jan. 1860. 8vo.
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Mackie, S. J. Esq. F.G.S. (the Editor)—The Geologist, Feb. 1860.
Newton, Messrs.—London Journal (New Series), for Feb. 1860. 8vo.
Novello, Mr. (the Publisher)—The Musical Times for Feb. 1860. 4to.
Photographic Society—Journal, No. 94. 8vo. 1860.
Tite, W. Esq. M.P. (the Author)—Address to the Members of the Royal Institute of British Architects, Nov. 7, 1859. 8vo. 1859.
Vincent, B. (Keeper of Library, R.I.)—Haydn's Dictionary of Dates. Ninth Edition, revised and greatly enlarged. 8vo. 1860. (Two copies.)
Wells, T. Spencer, Esq. M.R.I.—Cancer-Cures and Cancer-Curers. 16mo. 1860.

WEEKLY EVENING MEETING,

Friday, March 9, 1860.

The Lord Wensleydale, Vice-President, in the Chair.

Professor Faraday, D.C.L. F.R.S.

On Lighthouse Illumination—the Electric Light.

The use of light to guide the mariner as he approaches land, or passes through intricate channels, has, with the advance of society and its ever increasing interests, caused such a necessity for means more and more perfect, as to tax to the utmost the powers both of the philosopher and the practical man, in the development of the principles concerned, and their efficient application. Formerly the means were simple enough; and if the light of a lanthorn or torch was not sufficient to point out a position, a fire had to be made in their place. As the system became developed, it soon appeared that power could be obtained, not merely by increasing the light but by directing the issuing rays: and this was in many cases a more powerful and useful means than enlarging the combustion; leading to the diminution of the volume of the former with, at the same time, an increase in its intensity. Direction was obtained, either by the use of lenses dependent altogether upon refraction, or of reflectors dependent upon metallic reflexion; and some ancient specimens of both were shown. In modern times the principle of total reflexion has also been employed, which involves the use of glass, and depends both upon refraction and reflexion. In all these appliances much light is lost: if metal be used for reflexion, a certain proportion is absorbed by the face of the metal; if glass be used for refraction, light is lost at all the surfaces where the ray passes between the air and the glass; and also in some degree by absorption in the body of the glass itself. There is, of course, no power of actually increasing the whole amount of light, by any optical arrangement associated with it.

The light which issues forth into space must have a certain amount of divergence. The divergence in the vertical direction must be enough to cover the sea from the horizon, to within a certain moderate distance from the shore, so that all ships within that distance may have a view of their luminous guide. If it have less, it may escape observation where it ought to be seen; if it have more, light is thrown away which ought to be directed within the useful degree of divergence: or if the horizontal divergence be considered, it may be necessary so to construct the optical apparatus, that the light within an angle of 60° or 45° shall be compressed into a beam diverging only 15°, that it may give in the distance a bright flash having a certain duration instead of a continuous light,—or into one diverging only 5° or 6°, which, though of far shorter duration, has greatly increased intensity and penetrating power in hazy weather. The amount of divergence depends in a large degree upon the bulk of the source of light, and cannot be made less than a certain amount, with a flame of a given size. If the flame of an argand lamp $\frac{7}{8}$th of an inch wide, and $1\frac{1}{2}$ inches high, be placed in the focus of an ordinary Trinity-house parabolic reflector, it will supply a beam having about 15° divergence: if we wish to increase the effect of brightness, we cannot properly do it by enlarging the lamp flame; for though lamps are made for the dioptric arrangement of Fresnel, which have as many as four wicks, flames $3\frac{1}{2}$ inches wide, and burn like intense furnaces, yet if one be put into the lamp-place of the reflector referred to, its effect would chiefly be to give a beam of wider divergence: and if to correct this, the reflector were made with a greater focal distance, then it must be altogether of a much larger size. The same general result occurs with the dioptric apparatus; and here, where the four-wicked lamps are used, they are placed at times nearly 40 inches distant from the lens, occasioning the necessity of a very large, though very fine, glass apparatus.

On the other hand, if the light could be compressed, the necessity for such large apparatus would cease, and it might be reduced from the size of a room to the size of a hat: and here it is that we seek in the electric spark, and such like concentrated sources of light, for aid in illumination. It is very true, that by adding lamp to lamp, each with its reflector, upon one face or direction, power can be gained; and in some of the revolving lights, ten lamps and reflectors unite to give the required flash. But then not more than three of these faces can be placed in the whole circle; and if a fixed light be required in all directions round the lighthouse nothing better has been yet established than the four-wicked Fresnel lamp in the centre of its dioptric and catadioptric apparatus. Now the electric light can be raised up easily to an equality with the oil lamp, and if then substituted for the latter, will give all the effect of the latter; or by expenditure of money it can be raised to a five or tenfold power, or more, and will then give five or tenfold effect. This can be done, not merely without increase of the volume of the light, but whilst the light shall have a volume scarcely the 2000th part of that of the oil flame. Hence, the extraordinary assistance we may expect to obtain of diminishing the size of the optical apparatus and perfecting that part of the apparatus.

Many compressed intense lights have been submitted to the Trinity-house; and that corporation has shown its great desire to advance all such objects and improve the lighting of the coast, by spending, upon various occasions, much money and much time for this end. It is manifest that the use of a lighthouse must be never failing, its service ever sure; and that the latter cannot be interfered with by the introduction of any plan, or proposition, or apparatus, which has not been developed to the fullest possible extent, as to the amount of light produced,—the expense of such light,—the wear and tear of the apparatus employed,—the steadiness of the light for 16 hours,—its liability to extinction,—the amount of necessary night care,—the number of attendants,—the nature of probable accidents,—its fitness for secluded places, and other contingent circumstances, which can as well be ascertained out of a lighthouse as in it. The electric spark which has been placed in the South Foreland High Light, by Professor Holmes, to do duty for the six winter months, had to go through all this preparatory education before it could be allowed this practical trial. It is not obtained from frictional electricity, or from voltaic electricity, but from magnetic action.—The first spark (and even magnetic electricity as a whole) was obtained 28 years ago. (Faraday, *Philosophical Transactions*, 1832, p. 32.) If an iron core be surrounded by wire, and then moved in the right direction near the poles of a magnet, a current of electricity passes, or tends to pass, through it. Many powerful magnets are therefore arranged on a wheel, that they may be associated very near to another wheel, on which are fixed many helices with their cores, like that described. Again, a third wheel consists of magnets arranged like the first; next to this is another wheel of the helices, and next to this again a fifth wheel, carrying magnets. All the magnet-wheels are fixed to one axle, and all the helix wheels are held immoveable in their place. The wires of the helices are conjoined and connected with a commutator, which, as the magnet-wheels are moved round, gathers the various electric currents produced in the helices, and sends them up through two insulated wires in one common stream of electricity into the lighthouse lanthorn. So it will be seen that nothing more is required to produce the electricity than to revolve the magnet-wheels. There are two magneto-electric machines at the South Foreland, each being put in motion by a two-horse power steam-engine; and, excepting wear and tear, the whole consumption of material to produce the light is the coke and water required to raise steam for the engines, and carbon points for the lamp in the lanthorn.

The lamp is a delicate arrangement of machinery, holding the two carbons between which the electric light exists, and regulating their adjustment; so that whilst they gradually consume away, the place of the light shall not be altered. The electric wires end in the two bars of a small railway, and upon these the lamp stands. When the carbons of a lamp are nearly gone, that lamp is lifted off and another instantly pushed into its place. The machines and lamp have done their duty during the past six months in a real and practical manner. The light has never gone out, through any deficiency or cause in the engine and machine house: and when it has become extinguished in the lanthorn, a single touch of the keeper's hand has set it shining as bright as ever. The light shone up and down the Channel, and across into France, with a power far surpassing that of any other fixed light within sight, or any where existent. The experiment has been a good one. There is still the matter of expense and some other circumstances to be considered; but it is the hope and desire of the Trinity-house, and all interested in the subject, that it should ultimately justify its full adoption.

[M. F.]

WEEKLY EVENING MEETING,

Friday, March 16, 1860.

Sir Benjamin Collins Brodie, Bart. D.C.L. Pres. R.S. Vice-President R.I. in the Chair.

Maxwell T. Masters, Esq.

LECTURER ON BOTANY, ST. GEORGE'S HOSPITAL, ETC.

On the Relation between the Abnormal and Normal Formations in Plants.

The object of this discourse was to point out certain interesting facts, relating to the natural and abnormal development of plants, and to the impossibility of drawing any absolute distinction between the two; to show their bearing upon the theory of vegetable morphology, and on the views lately propounded by Mr. Darwin. Premising that no general law can be laid down to include all plants, as each large group has, to a certain extent, its own special organization, the speaker briefly adverted to the natural conformation of plants under the following heads: 1st, Alimentary system; 2nd, Tegumentary; 3rd, Reproductive; 4th, Fibrous; 5th, Appendicular System.

All plants possess alimentary, tegumentary, and reproductive systems, and the humblest plants have no others. All plants, but Thallogens, possess in addition a fibro-vascular system, and an axis, co-existent with which is the presence of an appendicular system, in the form of scales, leaves, and in the higher plants of sepals, petals, &c. All these organs have a common origin, and this relationship is only partially obliterated throughout life; so that there is a much closer homology between the organs of one plant, and between the organs of one class of plants, and those of another, than is the case in the animal kingdom.

In considering what is natural and what is not so, a great deal is of necessity assumed. Naturalists construct for themselves a sort of type or ideal standard of perfection, which does not of necessity exist in nature, but which enables us to gain a clearer insight into the truth. If this be not borne in mind, in speaking of "the laws" of creation, etc. we are likely to be charged with the sin of presumption, and to foster the very prevalent error, that because one hypothesis is shown to be false by arguments derived from another, that the latter is of necessity true. In natural science, that theory has the greatest claims to acceptance, which satisfactorily explains the largest number of facts, and by means of which our store of knowledge is most augmented.

For the present purpose, the speaker assumed the correctness of the heretofore generally received opinion of the existence of "species," endowed with a very variable, but a limited power of variation; and then proceeded to discuss what degree or extent of variability might be considered natural, and what unnatural. The distinction is not always easy, and in many cases it is impossible. Where the variation is slight, and apparently co-existent with a change in the conditions of growth the variation is evidently natural. Allusion was made to such facts as that of peaches and nectarines found growing on the same bough, to alterations effected by changes in climate, &c. When the variation is greater, of course greater difficulty exists in determining whether or no it be natural. Reference was made to the primrose, the cowslip, and the oxlip, which differ in many important points one from the other, but which, nevertheless seem to be variations of one form; in evidence of which, amongst other facts, are these, that all three have been raised from seeds derived from the same fruit; and that in the Hookerian herbarium there exists a specimen wherein a primrose and a cowslip spring from the same stem. Some plants are especially liable to vary; such are orchids, ferns, grasses, and especially fungi, some of which latter have no less than five different modes of reproducing themselves by as many distinct organs. The speaker was enabled, through the kindness of Professor Buckman, to exhibit specimens illustrating the very curious experiments of that gentleman in ennobling the wild oat, and in producing from the seeds of two so-called species of aquatic grasses, *Glyceria aquatica*, and *Glyceria fluitans*, when grown in a dry soil, a form unlike either of the parent plants,—a form in which the herbage of *Glyceria aquatica* was combined with the inflorescence of *Poa trivialis*. The experiments of Professor Buckman have the more value as they have been made without any reference to theoretic views.*

Another difficulty in distinguishing the abnormal from the normal in plants, arises from the fact that what is unnatural or unusual in one plant, is the common condition in another nearly allied plant. In illustration of which several instances were cited; and one in particular, which led Herr S. Reissek, in some measure, to anticipate the views of Mr. Darwin. The changes took place in a species of *Thesium*, affected by a parasitical fungus, in consequence of which apparently, the plant underwent many changes, some of which caused it to assume many of the characteristics of allied species and even genera. "Suppose," says the author, "the condition originally caused by the fungus to become constant in the course of time, the plant would, if found growing wild, be considered as a distinct species, or even as belonging to a new genus. Nature appears to have set up a fingerpost, to show the way in which species and genera may have been formed out of a previously existing type," &c.† Here, however, there is no reference to the ceaseless process of natural selection, and of very gradual change. Another circumstance which adds to the perplexity that is felt in distinguishing the normal from the abnormal in plants, is that irregularity of growth can hardly be considered abnormal, because it is in many instances a constant condition; the health of the plant is in no wise impaired, the irregularity does not exist at first, but arises during development, and it is subject to definite laws.

Certain changes may be physiologically abnormal, because they interfere with the due fulfilment of the functions of the part affected; and yet morphologically they can hardly be considered abnormal, because they do not violate any of the laws of morphology, and are caused perchance by a mere reversion to a simpler form.

Reference was then made to the classification of malformations in plants, adopted by M. Moquin Tandon, in his standard work on the subject, in order to exemplify the impossibility of drawing a line between what should and should not be deemed a malformation, for the reasons already mentioned. Even in the class of Malformations grouped under the head of "Deformity," the change was so slight as not to interfere with the physiological functions, or it was one which occurred naturally in other plants. "Peloria," or that change whereby a flower usually irregular becomes regular, may occur in two ways—one where the flower becomes regular by the increase of its irregular portions, so as to restore the symmetry, as in the Pelorian varieties of the common *Linaria*; the other, almost entirely overlooked, is where the flower preserves throughout life its original equality of proportions. The calyx of the double *Tropæolum* affords an illustration of this. The calyx of the simple form is coloured, with its upper sepal prolonged in the shape of a spur; in the double variety

* Buckman, Rep. Brit. Assoc. 1857.

† Linnæa, Vol. 17, 1843.

all five sepals are green, and of equal size. We can hardly consider a return to regularity, in whatever way it be effected, as anomalous ; and it has been before shown that irregular flowers are not necessarily monstrous. It is not requisite to go through the classification of M. Moquin Tandon, at any greater length, as the same remarks, to a greater or lesser extent, apply to all the groups.

As confessedly artificial distinctions, it may be said that a variety is some change from the ordinary condition of a plant—a change in nowise impairing the exercise of the physiological functions—a change affecting the whole, or at least several parts of the plant,—a change which is more or less constant and permanent and which is reproduced. On the other hand, in a monstrosity, there is a change which does more or less interfere with the due exercise of the functions of the organs affected—a change usually affecting one organ, or one set of organs in a plant,—a change, less constant and permanent than in a variety,—a change which is rarely reproduced.

Allusion was then made to the value of Teratology, as affording the basis on which the now generally received theory of vegetable morphology rests. No doubt the unusual conditions of plants, whether they be called varieties or monstrosities, arise frequently from the operation of that ceaseless struggle for existence in the battle of life, to which Mr. Darwin, as well as the late Dean Herbert attribute so much importance ; but we should be extremely careful in reasoning from malformations, and even from varieties, either in support of, or in opposition to Mr. Darwin's views, especially if the word species be understood in its widest acceptation. The amount of change, great as it is in certain instances, is not greater than is the diversity of form under which the same individual plant may occur : moreover, the changes on which Mr. Darwin relies are small in degree, but constantly increasing. Violent and sudden changes are disavowed by him ; for though the result of a struggle for life, yet they tend rather to the extinction of the organ or of the plant, than to the production of a new species. If Mr. Darwin's views be pushed to their fullest consequences, it would appear as if there were no limits to variation ; and it is of the highest importance to ascertain whether this be so or not. Without forgetting the necessity of caution in employing teratological facts in such a question, the speaker cited as tending to show the probability that there were limits to variation, the fact that in the malformations of what are considered to be the most highly specialized groups of plants, those whose structure is most complex, most concentrated, and furthest removed from the leaf type, as *Compositæ*, *Umbelliferæ*, &c., little or no exaltation of the type ever occurs, whereas in other orders whose structure does not so widely depart from the leaf type, such an exaltation is frequent, though always less so than the opposite process of degeneration.

The degree of constancy is very various, and most important to be considered in questions of this kind. The speaker is under obligations to his father for the following interesting facts bearing on this point.

A tree of that variety of the weeping willow, whose leaves are rolled up in a spiral coil, after retaining its character for twenty-five years, at length sent forth a shoot in an ascending direction. this shoot being clothed with flat leaves, as in the common form, There are several varieties of the sweet pea : many years of observation have shown that the white flowered sweet peas seldom, if ever, vary; but that in proportion as the flower becomes darker in colour, so is the liability to vary greater: and these changes are not confined to the colour merely, but affect the pods and other organs. So too, the yellow varieties of the hyacinth are more constant than those of other colours. On the whole the varieties, and still more the malformations, are characterized by a want of constancy and a tendency to degenerate : a tendency not overlooked by Virgil, as witness the following lines :—

> "Vidi lecta diu, et multo spectata labore,
> Degenerare tamen ; ni vis humana quotannis
> Maxima quæque manu legeret : sic omnia fatis
> In pejus ruere, ac retro sublapsa referri :
> Non aliter, quam qui adverso vix flumine lembum
> Remigiis subigit, si brachia forte remisit,
> Atque illum in præceps prono rapit alveus amni."
>
> *Georg.* i., 197.

[M. T. M.]

[The speaker takes this opportunity of expressing his obligations to several of his friends who supplied him with illustrations for his discourse, especially to his father, to Mr. Ward, Mr. Baxter, and Prof. Buckman.]

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, March 23, 1860.

THE LORD WENSLEYDALE, Vice-President, in the Chair.

NEVILL STORY MASKELYNE, Esq.

On Diamonds.

THE progress of chemical discovery in so fundamental and important a subject as the elements, has been hitherto singularly incommensurate with the rapid strides of theory and the vast accumulation of facts regarding the compounds, which those elements combine to form. Of this, the element carbon is a remarkable illustration.

Organic chemistry is the chemistry of carbon, yet even now the number of allotropic states of that element are undetermined with any certainty. Even the question of the crystalline system of graphite is not decided; and, while we are almost entirely ignorant of the real conditions requisite for the production of carbon in any of its allotropic states, we are completely in the dark about those under which the diamond, and the strange mineral kindred to it called "carbonate" have been, or may be called into existence.

The transcendant hardness of the diamond is a quality which would alone make so rare a body very costly, even if it had no value as a gem. [The apparatus employed by the diamond-cutter for cutting the diamond by means of its own dust was exhibited; consisting of the schyf or horizontal iron wheel armed with this dust, on which the diamond is pressed by weights, laid on an arm of wood, below the end of which arm the diamond is carried in a projecting bed of fusible metal.]

Another remarkable characteristic of the diamond is its adamantine lustre, arising from the reflection of so large an amount of light from its polished surface; a characteristic which is closely linked to its high refringent power. That power indeed is so great, that it is approached by no colourless solid, nor by any even of the highly refringent oils, and inorganic compounds of carbon. Thus, a ray, on passing into this substance is so bent from its course, that the new direction which it takes is at any given point $\frac{3}{5}$ nearer to the perpendicular on the surface than was the distance of a corresponding point in the course which it previously held, while traversing the air. It results from this, that the critical angle—the angle beyond which all light internally incident on the surface is totally reflected internally, and whose sine is the reciprocal of the refractive index,—is in the diamond so small an angle as 26°; a fact which enables us to cut diamonds for the purposes of jewellery, so as to produce an effect which, in any other gem, would only be produced by stones of much greater thickness. Thus, the diamond-cutter, by the instinct of an art which worked out practically this problem at a time when science had not determined "the law of sines," takes advantage of this principle in cutting the diamond into the brilliant form.

The brilliant is fundamentally a double pyramid or cone, truncated by a large plane called the table, on one end, and by a small one called the culet, at the other. The adjustment of the angles of the two pyramids has to be so made, that the girdle of the stone presents a prismatic edge, the angle of which must be as much as twice the critical angle, or as nearly so as the original form of the diamond permits. This angle, which in the diamond is 48°, would require to be 68° to 70° for the sapphire or chrysoberyl, and above 80° for glass. The adjustment of the relative sizes of the table and the culet are also very important; and the object aimed at in both these adjustments is that no light shall penetrate the diamond from above, that shall not be totally reflected internally, and so thrown back again through the summit planes, called the bezil planes, or the table. Meeting these planes at various inclinations, the light is shattered into a thousand hues, by the dispersive power of the stone. [A comparative experiment was exhibited, by which the spectrum formed by a flint glass prism was brought into comparison with one formed by the table and one of the inferior planes of a thin brilliant.]

The geographical distribution and geological association of the diamond have not as yet thrown much light on its origin. In India, Malacca, Borneo, in Brazil, Mexico, the gold states of the United States, and in the Urals it is found in beds of rivers or alluvial deposits. In Australia and in Algiers it is reported to have been found, and under similar conditions. In Brazil it has been traced to its rock-home in the itacolumite (a micaceous quartzose schist, often containing talcose minerals, and intersected by quartz veins), and also in a hornblende slate continuous with the itacolumite. But whether these are its parent rocks, or whether—as they are probably metamorphic in their nature—its origin dates from an earlier state of the materials that have become transmuted by time and the play of chemical and physical forces into itacolumite and hornblende slate, we are not in a position to declare. The companions of the diamond do not tell its history in a much less vague language. Gold seems in every diamond country to be either an associate or the not distant neighbour of the diamonds. Tourmaline, chrysoberyl, chrysolite, topaz, kyanite, oxides of titanium and of iron, quartz as jasper, and in other forms, are frequently found with them.

In the diamond, splinters of ferruginous quartz have been found. A high antiquity and an origin perhaps contemporaneous, and not improbably connected with the geological distribution of gold in quartz veins, may be inferred from these facts.

The chemist has to deal with a more general problem ; that of the methods, whether employed by nature, or open to his own ingenuity, for producing the diamond. Many solutions for this problem have been and may be proposed :—

1. The authority of Liebig supports the view of a process of eremacausis having converted organic compounds into diamonds.
2. The decomposition of binary carbon compounds by replacement of the carbon by some other elements.
3. A process of sublimation.
4. Cooling from fusion under pressure (supposing carbon otherwise to vaporise without fusion, like arsenic).
5. Deposits from voltaic currents between carbon poles.
6. Deposits on the cooling of fused metals (or other substances ?), surcharged with carbon.
7. The separation of carbon from carbonates, analogous to that of silicon from silicates, which may be effected by magnesium at a red heat, and by lithium far under red heat.

And these do not exhaust the number of possible suggestions. Of them, one (the 6th) possesses peculiar interest.

Graphitic boron and silicon are formed by the cooling of fused aluminium, surcharged with these elements ; and the same elements—in other respects so closely grouped with carbon—separate in the adamantine form from zinc, under analogous circumstances. The latter are crystallised indeed in different systems from diamond, but they possess many of its characters in a remarkable degree.

Mr. Maskelyne then adverted to some of the largest diamonds that have been recorded, and concluded with a few facts regarding the Koh-i-Nur. These had chiefly for their object, to prove that the great diamond of India, which the Emperor Baber records as having been taken at Agra, by Humayun, in May 1526, was the Koh-i-Nur, now the crown jewel of England. This was based on the identity of weight of the diamond (before it was cut) in 1851, with the eight mishkals, which Baber declared to have been its estimated weight. It is difficult to state, precisely, what the mishkal was in Baber's day. His coins and Humayun's are very scarce ; but even in their greatly worn state, these early Mogul silver coins, or dirhams, average above 71, and range up to 71·5 grains, probably corresponding to a coined value of at least 73, and perhaps even 74 grains. These probably represent the mishkal. It is not less difficult to determine what was the precise weight of the Bokhara goldsmith's mishkal, which would have been the basis of Baber's coinage, in the 16th century ; but among the old Samanian coins (of Bokhara and Samarkand, A.D. 961—1165) are some from which it would seem, that besides the old dinar, of about 66 grains, and a dirham, of 50, there were 1½ dirham pieces (corresponding in their ratios to the modern ones of Bussorah), whose weight is about 75 grains. The Ghaznavid coins of the 1st Pathân sovereign of India, Muaz-ud-din, A.D., 1193, tally with these; one of them, indeed, is inscribed as a dirham, and weighs 74 grains. The base of the modern ponderary system of Bokhara is stated by the Parsee writer, Nowrozjee Furdoonjee, to be a mishkal of 71 grains, so that the limits of range of the mishkal of Bokhara and Ghazni, were probably from 74 or 75 grains in the 10th and 11th century, to 71 in modern times, and in the days of Baber, as rendered probable from his coins, the margins of which are much worn, it was probably not less than 74 grains—which indeed is still the weight of the goldsmith's mishkal, in Persia—and corresponds to the relation recorded by Makrizi and Abu'l fuzl, as subsisting between the Syrian or Indian mishkal and the Greek dinar (the 66-grain coin of the caliphate). A mishkal of 73·69, would give the weight of the Koh-i-Nur; and is in fact, even at the present day, almost precisely the Arabian gold miscal of 73·368 grains troy. Another resource for the determination of this point, is to be found in the Indian weights, for which the Emperor Baber gives the Persian equivalent, in a remarkable passage in his memoirs, that has been apparently overlooked by numismatic writers.

The ratios he gives, are 4 Mashes (of 8 ratis) = 1 Tank,
5 " = 1 Mishkal,
12 " = 1 Tola,

and he adds, they weigh jewels and precious stones by the tank. Until the time of Shir Shah, the tola, as determined from the Pathan coins, was a weight of a minimum value of 174, and probably as high as 176 or 177 grains. In Baber's time, therefore, it may be taken at this value. The tank also, as deduced from the a'dhali of Mohammed ben Tughlak, would accord with this estimate.

Abu'l fuzl states the a'dhali to have been half a dâm, and the dâm to have been five tanks. Mohammad-ben-Tuglak's a'dhali weighs 140 grains. These must be assumed to have been issued, or to have been estimated at a mint value of 146 grains, to give the tank of 59, and a tola of 177 grains, an assumption by no means too great for coins of that sovereign. The rati, the little red and black seed of the Abrus precatorius, is far too uncertain and variable a weight to be made the basis of a calculation of the kind.

The 320 ratis of Baber, and the 319½ ratis of Tavernier, would give an error of some ten carats for every tenth of a grain error that might be made in the estimate of the value of the rati, as the unit in such a calculation.

There are two kinds of rati-weight; the goldsmith's of 8, and the jeweller's of 6 ratis, to the same masha. There is also a pearl rati. The only possible means of assigning a value to these weights is by arriving at some result with regard to the tank, the tola, the masha, or the mishkal, and deducing the value of the rati from these. Tavernier has obviously fallen into an error regarding the relative value of this little weight, as compared not only with European standards of weight, but also with those of India. It is possible that he took the pearl rati of three troy grains for his basis, and confounded it with the other varieties of this variable unit of comparison.

The speaker then noticed Tavernier's account of the diamond he saw in the possession of Aurungzebe, of which he gives the weight as 319½ ratis; and the apparent inconsistencies of his narrative, with the idea of that diamond being the 320-rati diamond of Baber, were sought to be explained from the contemporary history as given by the translator of Ferishta, and by Bernier, no less than by Tavernier himself. It seemed probable at least, that the old crown jewel of the sovereigns of Delhi, and the talisman of Indian empire, was then in the possession of Aurungzebe, and was seen as such by the French traveller; but that he has mistaken the history of that stone, and confounded it with that of another which had been found but a few years before, and had been acquired by Shah Jehaun. Shah Jehaun was then a state prisoner, and his reigning son let him retain his jewels in his captivity. Among these would doubtless have been the diamond Tavernier alludes to, and which had been in fact no crown jewel, but a private possession acquired by Shah Jehaun himself.

Tavernier indeed gave a drawing of the diamond; but his representation is a most rude one, and is as much like the Koh-i-Nur seen from one end, as it can be said to be like any large diamond known to exist now; while his description, utterly at variance with his drawing, exceedingly well characterises the Koh-i-Nur, even in its peculiarities.

The history of this diamond is one long romance from then till now; but it is well authenticated at every step, as history seems never to have lost sight of this stone of fate, from the days when Ala-ud-din took it from the Rajahs of Malwah five centuries and a half ago, to the day when it became a crown jewel of England; while tradition carries back its existence in the memory of India to the half mythic hero Bihramojeet, Rajah of Usjein and Malwa, 57 B.C.; and a still wilder legend would fain recognise in it a diamond recorded as worn by Karna, Rajah of Anga, who fell in the "great war," and first discovered near Masulipatam in the bed of the Godaveri, 5000 years ago.

[N. S.-M.]

WEEKLY EVENING MEETING,

Friday, March 30, 1860.

THE LORD WENSLEYDALE, Vice-President, in the Chair.

WILLIAM ODLING, Esq. M.B. F.R.S.

SECRETARY TO THE CHEMICAL SOCIETY.

On Acids and Salts.

IT is natural to inquire whether the doctrines of series and substitutions, which are essential for the association of organic products, may not throw some additional light upon the simpler compounds of mineral chemistry, when viewed as unitary molecules; and particularly upon the relations and properties of the mineral acids and their salts, which have hitherto constituted the strongholds of the electro-chemical, or binary, theory of combination.

The doctrine of series affirms that chemical compounds may be arranged in series, the successive members of each of which differ from one another in composition by a common increment, and are associated with one another by a certain relation of properties, the exact nature of the relation varying with the nature of the increment.

The doctrine of substitutions affirms that, in very many chemical compounds, one or more atoms may be displaced by some other atoms or groupings, and that the new bodies, resulting from this displacement, correspond in constitution with the normal bodies from which they were derived. The doctrine of substitutions affords great assistance to the doctrine of series; for when, as frequently happens, a gap exists in any series, that gap can almost always be filled up by a substitution-representative of the missing body.

(α.) There are four acid compounds of hydrogen, two volumes of each of which contain one volume of hydrogen, namely:

HF	Fluorhydric acid.
HCl	Chlorhydric acid.
HBr	Bromhydric acid.
HI	Iodhydric acids.

When two volumes of chlorhydric acid, for instance, are acted upon by a red hot iron-wire, the chlorine is absorbed by the iron, and one volume of hydrogen gas liberated. The two volumes of chlorhydric acid yield one volume of hydrogen, or the original bulk of gas is reduced to one-half by the absorption of its chlorine. The above four acids may be looked upon as substitution-representatives, one of another.

Chlorhydric acid yields the following series of oxides, convertible into each other by mutual metamorphosis.

HCl	Chlorhydric acid.
$HClO$	Hypochlorous acid.
$HClO_2$	Chlorous acid.
$HClO_3$	Chloric acid.
$HClO_4$	Perchloric acid.

When chlorhydric acid HCl, is oxidated by permanganic acid, hypochlorous acid $HClO$, is produced; and, conversely, chlorhydric acid may be reproduced by the deoxidation of hypochlorous acid. Hypochlorous acid, when heated, breaks up into chloric acid $HClO_3$, and other products. When chloric acid is deoxidated by nitrous acid, it becomes chlorous acid $HClO_2$; and, when oxidated at the positive pole of a galvanic battery, it becomes perchloric acid $HClO_4$. Here then is a series of associated acids, expressed as unitary molecules, by the simplest possible formulæ, and arranged in a series, the successive members of which differ from one another in composition by an increment of one atom, or volume, of oxygen.

(*β.*) There are four other binary compounds of hydrogen, two volumes of each of which, however, contain two volumes of hydrogen, namely:

H_2O	Water.
H_2S	Sulphydric acid.
H_2Se	Selenhydric acid.
H_2T	Tellurhydric acid.

A given volume of any one of these gases or vapours contains exactly twice the quantity of hydrogen, that the same volume of any one of the first class of gases contains. When two volumes of sulphydric acid, for instance, are acted upon by a red hot iron-wire, the sulphur is absorbed by the iron, and two volumes of hydrogen gas are liberated. The two volumes of sulphydric acid yield two volumes of hydrogen, or the abstraction of the sulphur produces no alteration in the bulk of gas. The bihydric character of water, moreover, is well shown by the experiment of its electrolytic decomposition, in which two volumes of hydrogen are produced for every one volume of oxygen.

In the sulphur series of oxygen acids we have two gaps, which, however, can be filled up by the chloro-representatives of the missing bodies, thus:

H_2S	Sulphydric acid.	Cl_2S.
H_2SO	Wanting.	Cl_2SO.
H_2SO_2	Wanting.	Cl_2SO_2.
H_2SO_3	Sulphurous acid.	
H_2SO_4	Sulphuric acid.	

The compounds Cl_2SO, and Cl_2SO_2, are obtainable from the chloro-representative of sulphydric acid Cl_2S, by successive oxidation. The first product actually afforded by the oxidation of sulphydric acid is sulphurous acid H_2SO_3, which is produced by the combustion of sulphydric acid in air or oxygen. Conversely, sulphydric acid may be obtained by deoxidating sulphurous acid with nascent hydrogen. Sulphuric acid H_2SO_4, results from the oxidation of sulphurous acid, and by deoxidation can reproduce that body, as in the ordinary process for the preparation of sulphurous acid. Here then, including the chloro-representatives, is a second series of acids associated with one another by a common increment of composition, and by mutual metamorphosis.

Sulphuric acid H_2SO_4, is the representative on the sulphur series, of perchloric acid $HClO_4$, on the chlorine series. Each contains one atom of the radicle which gives the special character to the acid, in the one case chlorine, in the other sulphur. Each contains also four atoms, or volumes, of oxygen; but whereas perchloric acid contains only one atom, or volume, of hydrogen, sulphuric acid contains two atoms, or two volumes. And this difference in composition leads to a marked difference in the properties of the two acids. Perchloric acid $HClO_4$, has only one atom of hydrogen that can be replaced. Hence it forms only one description of salt, such, for instance, as perchlorate of potassium $KClO_4$, and only one description of ether, such, for instance as perchloric ether $EtClO_4$. But sulphuric acid has two hydrogen atoms that can be replaced. Hence it can form acid salts, neutral salts, double salts, acid ethers, neutral ethers, double ethers, and saline ethers, as shown in the table.

$H_2\ SO_4$	Sulphuric acid.
$KH\ SO_4$	Acid sulphate of potassium.
$K_2\ SO_4$	Neutral sulphate of potassium.
$KNi\ SO_4$	Potassio-sulphate of nickel.
$EtH\ SO_4$	Ethylo-sulphuric acid.
$Et_2\ SO_4$	Neutral sulphate of ethyl.
$EtMe\ SO_4$	Ethylo-sulphate of methyl.
$EtK\ SO_4$	Ethylo-sulphate of potassium.

This property of forming acid and double salts, and acid and double ethers, &c., indicates a fundamental difference in character between sulphuric and perchloric acids, a difference that is satisfactorily represented by the difference in their formulæ as here written down, $HClO_4$, and H_2SO_4. Bibasic characters are manifested as decidedly by the sulphurous and sulphydric acids.

(γ.) There are four other binary compounds of hydrogen, two volumes of each of which, however, contain three volumes of hydrogen, namely:

H_3N	Ammonia.
H_3P	Phosphamine.
H_3As	Arsenamine.
H_3Sb	Stibamine.

When the two volumes of phosphamine, for instance, are acted upon by a red hot iron-wire, the phosphorus is absorbed by the iron, and three volumes of hydrogen gas are liberated. Two volumes of chlorhydric acid yield one volume of hydrogen; two volumes of sulphydric acid yield two volumes of hydrogen, while two volumes of phosphamine yield three volumes of hydrogen; and this is a most important distinction between the three classes of hydrides to which these three gases respectively belong. Again, two volumes of gaseous ammonia, when decomposed by the Ruhmkorff spark, become converted into three volumes of hydrogen and one volume of nitrogen; or the original bulk of the ammonia becomes doubled.

In the phosphorus series of oxygen acids there is but one gap, and this can be filled up by the chlorine-, or the ethyl- representative of the missing body.

H_3P	Phosphamine	Cl_3P	Et_3P
H_3PO	Wanting	Cl_3PO	Et_3PO
H_3PO_2	Hypophosphorous acid		
H_3PO_3	Phosphorous acid		
H_3PO_4	Phosphoric acid		

Brodie has ascertained that oxichloride of phosphorus Cl_3PO, may be obtained directly by passing oxygen gas through boiling terchloride of phosphorus, or trichloro-phosphamine Cl_3P. The union of tri-ethyl phosphine Et_3P, with oxygen, to form the oxide of tri-ethylphosphine Et_3PO, constituted one of Hofmann's earliest experiments on the phosphorus bases. Proceeding to the actual oxides of phosphamine, it is doubtful whether hypophosphorous acid H_3PO_2, has been obtained by the oxidation of phosphamine; but, on the other hand, phosphamine is readily obtainable by deoxidating hypophosphorous acid with nascent hydrogen; while by oxidating hypophosphorous acid, phosphorous and phosphoric acids are successively produced. Phosphorous acid H_3PO_3, results from the slow oxidation, and phosphoric acid H_3PO_4, from the rapid oxidation of phosphamine. Conversely, phosphamine may be obtained by the deoxidation of each of the two last-mentioned acids. Here again then is a series of naturally associated and mutually convertible bodies, represented by the simplest possible formulæ, by formulæ which do not express any speculative view whatever, but merely indicate the indisputable fact that these bodies, or their representatives, differ from one another in composition, by the successive increments of one, two, three, and four oxygen atoms.

Phosphoric acid H_3PO_4, is the representative on the phosphorus series, of sulphuric acid H_2SO_4, on the sulphur series, and of perchloric acid $HClO_4$, on the chlorine series ; but whereas perchloric acid contains only one atom of hydrogen, and can form only one class of salts and ethers ; whereas sulphuric acid contains only two atoms of hydrogen, and can form only two classes of salts and ethers ; phosphoric acid contains three atoms of hydrogen, and can form three classes of salts and ethers. One-third, two-thirds, or three-thirds of its hydrogen may be displaced by a metal or basic radicle, or the hydrogen may be partly or wholly displaced by two or three different metals, or by two or three different radicles, or by a mixture of metals and radicles, thus : $EtKCuPO_4$, or $H(NH_4)NaPO_4$, &c.

(*δ*.) There is yet another primary hydride to be considered, namely, that of silicon, the siliciuretted hydrogen of Wöhler. The composition of this body has not been ascertained. It has been ascertained, however, that the substance from which it is obtained by the action of chlorhydric acid, is a silicide of magnesium, represented by the formula Mg_4Si, whence the formula of siliciuretted hydrogen is assumed to be H_4Si, analogous to that of marsh gas H_4C, a conclusion strongly confirmed by the composition of chloride of silicon, which is undoubtedly Cl_4Si, that is, a chloro-representative of siliciurretted hydrogen. Each primary hydride, hitherto considered, has yielded a remarkably stable acid, formed by the addition of four atoms of oxygen to the hydride ; and hydride of silicon ought to behave in the same manner, thus :

Chlorhydric acid	$H\ Cl$	$H\ ClO_4$	Perchloric acid.
Sulphydric acid	$H_2\ S$	$H_2\ S\ O_4$	Sulphuric acid.
Phosphamine	$H_3\ P$	$H_3\ P\ O_4$	Phosphoric acid.
Hydride of Silicon	$H_4\ Si$	$H_4\ SiO_4$	Silicic acid.

Now whether or not H_4SiO_4 is the correct formula for silicic acid, it is certain that the great majority of simple and well-defined silicates may be referred to that type, as illustrated in the table.

Orthosilicates.

$Et_4\ SiO_4$	Silicic ether.	$Gl_4\ SiO_4$	Phenakite.
$Li_4\ SiO_4$	Silicate of lithium.	$Ce_4\ SiO_4$	Cerite.
$Na_2H_2\ SiO_4$	Silicate of sodium.	$Fe_4\ SiO_4$	Fayelite.
$Ca_4\ SiO_4$	Silicate of calcium.	$Fe_2Mn_2\ SiO_4$	Knebelite.
$Mg_4\ SiO_4$	Olivine, Chrysolite.	$Cu_2H_2\ SiO_4$	Dioptase.
$Ca_2Mg_2\ SiO_4$	Batrachite.	$Al_2Ca\ SiO_4$	Anorthite.
$Zn_4\ SiO_4$	Zinc glance.	$Al_2Mn\ SiO_4$	Karpholite.

This next table illustrates the general relations of the perchloric salts and ethers, to their sulphuric, phosphoric, and silicic analogues. The existence of the silicated compounds corresponding to the formulæ in italics, has not yet been established.

Acids, Salts, and Ethers.

HCl	$HClO_4$	$NaClO_4$			
		$EtClO_4$			
H_2S	H_2SO_4	Na_2SO_4	$NaHSO_4$		
		Et_2SO_4	$EtHSO_4$		
H_3P	H_3PO_4	Na_3PO_4	Na_2HPO_4	NaH_2PO_4	
		Et_3PO_4	Et_2HPO_4	EtH_2PO_4	
H_4Si	H_4SiO_4	Na_4SiO_4	*Na_3HSiO_4*	$Na_2H_2SiO_4$	NaH_3SiO_4
		Et_4SiO_4	*$EtHSiO_4$*	*$Et_2H_2SiO_4$*	*EtH_3SiO_4*

Considering the relations of ammonia and phosphuretted hydrogen, H_3N and H_3P respectively, and the relations of marsh gas, and siliciuretted hydrogen, H_4C and H_4Si respectively, there should exist nitrates and carbonates having the general formulæ M_3NO_4 and M_4CO_4 respectively, corresponding to ordinary phosphates and silicates having the general formulæ M_3PO_4 and M_4SiO_4 respectively. It is observable, however, that in addition to ordinary phosphates and silicates, there are other phosphates and silicates, known respectively as metaphosphates and metasilicates, which differ from the ordinary salts by the loss of an atom of base, and that it is these metasalts to which ordinary nitrates and carbonates correspond, thus:

Phosphate	$M_3PO_4 - M_2O =$	MPO_3	Metaphosphate.
		MNO_3	Nitrate.
Silicate	$M_4SiO_4 - M_2O =$	M_2SiO_3	Metasilicate.
		M_2CO_3	Carbonate.

But chemists are acquainted with a considerable number of carbonates and nitrates, which may be called orthocarbonates and orthonitrates respectively, that do correspond in their formulæ with ordinary silicates and phosphates, as shown in the table.

Orthocarbonates.

Ca_4CO_4	Dicarbonate of calcium.
Zn_4CO_4	Dicarbonate of zinc.
Mg_3HCO_4	Dicarbonate of magnesium.
Pb_4CO_4	Dicarbonate of lead.
Pb_3HCO_4	White lead.
Cu_4CO_4	Mysorine.
Cu_3HCO_4	Azurite.
$Bi'''HCO_4$	Dicarbonate of bismuth.

ORTHONITRATES.

$Pb_2H\ NO_4$ } of lead.
$Pb_3\ NO_4$ }
$Hg_3\ NO_4$ } of mercury.
$Hg_2H\ NO_4$ }
$Bi'''\ NO_4$ of bismuth.

The succeeding tables present lists of the principal ter-oxygen and tetra-oxygen mineral acids. Some of these acids are known only through the medium of their metal- and ethyl-representatives.

TER-OXYGEN ACIDS.		TETRA-OXYGEN ACIDS.	
Chloric . . .	$H\ Cl\ O_3$	Perchloric . .	$H\ Cl\ O_4$
Bromic . . .	$H\ Br\ O_3$	Periodic	$H\ I\ O_4$
Iodic. . . .	$H\ I\ O_3$	Permanganic . .	$H\ Mn_2O_4$
Nitric . . .	$H\ N\ O_3$	Sulphuric . . .	$H_2S\ O_4$
Metaphosphoric	$H\ P\ O_3$	Selenic	$H_2Se\ O_4$
Sulphurous .	$H_2S\ O_3$	Telluric	$H_2T\ O_4$
Selenious . .	$H_2Se\ O_3$	Oxalic	$H_2C_2\ O_4$
Tellurous .	$H_2T\ O_3$	Molybdic . . .	$H_2Mo_2O_4$
Carbonic . .	$H_2C\ O_3$	Vanadic	$H_2V_2\ O_4$
Metasilicic . .	$H_2Si\ O_3$	Tungstic	$H_2W_2\ O_4$
Titanic . . .	$H_2Ti\ O_3$	Chromic	$H_2Cr_2\ O_4$
Stannic . . .	$H_2Sn\ O_3$	Manganic . . .	$H_2Mn_2O_4$
Vanadous . .	$H_2V_2\ O_3$	Ferric	$H_2Fe_2\ O_4$
Phosphorous .	$H_3P\ O_3$	Orthonitric . . .	$H_3N\ O_4$
Arsenious . .	$H_3As\ O_3$	Phosphoric . . .	$H_3P\ O_4$
Antimonous .	$H_3Sb\ O_3$	Arsenic	$H_3As\ O_4$
Bismithous .	$H_3Bi\ O_3$	Antimonic . . .	$H_3Sb\ O_4$
Boracic . . .	$H_3B\ O_3$	Orthocarbonic .	$H_4C\ O_4$
Aluminous . .	$H_3Al_2O_3$	Silicic	$H_4Si\ O_4$

Hence the formula $H_x\ R_y\ O_z$, will represent the general type for an acid, where H_x represents the atoms of hydrogen, which, save in carbon compounds, are found to vary only from 1 to 4; where R_y represents the acid radicle, that is the chlorine, or sulphur, or phosphorus, or carbon, &c. which gives the special character to the acid, and which, save in carbon compounds, is usually confined to 1 or 2 elementary atoms; and where O_z represents the atoms of oxygen which generally range from 0 to 4, but occasionally extend to higher numbers.

[W. O.]

GENERAL MONTHLY MEETING,

Monday, April 2, 1860.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

George Bowdler Buckton, Esq. F.R.S. F.L.S. F.C.S.
Arthur Cohen, Esq. B.A. and
John Wyatt, Esq.

were duly *elected* Members of the Royal Institution.

Stephen Busk, Esq. and
John Charles Salt, Esq.

were *admitted* Members of the Royal Institution.

The Special Thanks of the Members were returned to M. ROULAND, the French Minister of Public Instruction, for his Present, on behalf of the French Government, of the following Forty-nine Volumes :—

DOCUMENTS INEDITS SUR L'HISTOIRE DE FRANCE :

Histoire de la Guerre de Navarre en 1276 et 1277, par Guillaume Anelier de Toulouse. Ed. Michel. 4to. 1856.
Recueils des Monuments de l'Histoire du Tiers Etat. (Region du Nord.) Ed. Aug. Thierry. Vols. 1, 2, 3. 4to. 1850–6.
Mélanges Historiques. Ed. Champollion-Figeac. Vols. 3, 4. 4to. 1847–8.
Lettres des Rois, Reines, &c. de France et d'Angleterre. Ed. Champollion-Figeac. Vol. 2. (1301–1515.) 4to. 1847.
Lettres Missives de Henri IV. Ed. Berger de Xivrey. Vols. 4, 5, 6, 7. (1593–1610.) 4to. 1848–58.
Correspondance Administrative sous le Règne de Louis XIV. Ed. G. B. Depping. Vol. 1, 2, 3, 4. 4to. 1851–5.
Papiers d'Etat de Cardinal de Granvelle. Ed. Ch. Weiss. Vols. 7, 8, 9. (1563–5.) 4to. 1850–52.
Lettres de Cardinal de Richelieu. Ed. Avenel. Vols. 1, 2, 3. (1608–30) 4to. 1853–8.
Chronique de Religieux de St. Denys. (1380-1422.) Ed. Bellaguet. Vol. 6. 4to. 1852.
Mémoires de Claude Haton. Ed. F. Bourquelot. 2 vols. 4to. 1857.
Mémoires Militaires relatifs à la Succession d'Espagne. Vols. 7 (et Atlas), 8, 9, 10. 4to. 1848–59.
Li Livres de Jostice et de Plet. Ed. P. Chabaille. 4to. 1850.
Les Olim : ou Régistres des Arrets rendus par la Cour du Roi. (1312–18.) Ed. Beugnot. Vol. 3. Partie 2. 4to. 1848.
Procès des Templiers. Ed. Michelet. Vol. 2. 4to. 1851.
Priviléges accordés à la Couronne de France par le Saint-Siége. (1224–1622.) Ed. A. Tardif. 4to. 1855.

Négotiations de la France avec la Toscane. Ed. A. Des Jardins. Vol. 1. 4to. 1849.
Négotiations de la France dans le Levant. Ed. Charrières. Vols. 1, 2, 3. (1515–80.) 1848–53.

Cartulaire de Notre-Dame de Paris. Ed. Guérard. 4 vol. 4to. 1850–7.
Cartulaire de l'Abbaye de St. Victor de Marseille. Ed. Guérard. 2 vols. 4to. 1857.
Cartulaires de Sévigny et d'Ainay. Ed. A. Bernard. 2 vols. 4to. 1853–6.
Cartulaire de l'Abbaye de Beaulieu en Limousin. Ed. Deloche. 4to. 1859.

Archives de Rheims. Ed. P. Varin. Vol. 3, Partie 2. Vols. 2, 3. 4to 1848–53.

Jean Palsgrave: Eclaircissement de la Langue Française (1530); suivie de le Grammaire de Giles du Guez. Ed. F. Genin. 4to. 1852.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same: viz.

FROM
Board of Trade (through Admiral Fitzroy)—Fourth Number of the Meteorological Papers. 4to. 1860.
Secretary for India—Bombay Magnetical and Meteorological Observations for 1857. 4to. 1858.
Colonial Secretary of New Zealand—Dr. Hochstetter's Lecture on the Geology of the Province of Nelson. fol. 1859.

Arts, Society of—Journal for March 1860. 8vo.
Asiatic Society, Royal—Journal, Vol. XVII. Part 2. 8vo. 1860.
Astronomical Society, Royal—Proceedings, No. 4. 8vo. 1860.
Bombay Medical and Physical Society—Transactions, New Series, No. 4. 8vo. 1859.
Boosey, Messrs. (the Publishers)—The Musical World for March 1860. 4to.
Botfield, Beriah, Esq. M.P. F.R.S. M.R.I.—Passages from the Diary of General Patrick Gordon, of Auchleuchries. (A.D. 1635–99.) 4to. 1859.
Brodie, Sir B. C. Bart. Pres. R.S. M.R.I. (the Author)—Lectures on Pathology and Surgery. 8vo. 1846.
Pathological and Surgical Observations on Diseases of the Joints. 5th ed. 8vo. 1850.
Lectures on the Diseases of the Urinary Organs. 4th ed. 8vo. 1849.
Chemical Society—Journal, No. 48. 8vo. 1860.
Civil Engineers, Institute of—Minutes of Proceedings, Vols. 7, 8, 10, 11, 12, 13, 14, 15, 16, 17. 8vo. 1848–58.
Editors—Artizan, for March 1860. 4to.
Athenæum for March 1860. 4to.
Engineer for March 1860. fol.
Horological Journal, No. 19. 8vo. 1860.
Journal of Gas-Lighting for March 1860. 4to.
Mechanics' Magazine for March 1860. 8vo.
Medical Circular for March 1860. 8vo.
Practical Mechanics' Journal for March 1860. 4to.
Everest, Col. G. F.R.S. M.R.I.—Rectification of Logarithmic Errors in the Measurement of Two Sections of the Meridional Arc of India. 8vo. 1860.
Faraday, Professor, D.C.L. F.R.S.—Königliche Preussischen Akademie, Berichte, Jan. 1860. 8vo.
Forrester, J. J. Esq. M.R.I. (the Author)—On Port Wine. 8vo. 1860.
Franklin Institute of Pennsylvania—Journal, Vol. XXXIX. Nos. 1, 2. 8vo. 1860.
Geological Institute of Vienna—Jahrbuch, 1859. No. 3. 4to.
Geographical Society, Royal—Proceedings, Vol. IV. No. 1. 8vo. 1860.
Goodchild, Thos. Esq. (the Author)—Description of his Trocheidoscope. 16to. 1860.
Granville, A. B. M.D. F.R.S. M.R.I.—The Mineral Springs of Vichy. 8vo. 1859.
Groombridge, Messrs. (the Publishers)—S. J. Mackie: First Traces of Life on the Earth; or the Fossils of the Bottom Rocks. 16to. 1860.

Halls, John James, Esq. (the Author)—Two Months in Arrah, in 1857. 16to. 1860.
Linnean Society—Proceedings. Supplement to Botany, Vol. IV. 8vo. 1860.
Mackie, S. J. Esq. F.G.S. (the Editor)—The Geologist, March, 1860.
Newton, Messrs.—London Journal (New Series), for March 1860. 8vo.
North, John, Esq. M.R.I.—Lucan's Pharsalia, by N. Rowe. fol. 1718.
Novello, Mr. (the Publisher)—The Musical Times for March 1860. 4to.
Petermann, A. Esq. (the Editor)—Mittheilungen auf dem Gesammtgebiete der Geographie. 1860. Heft 2, 3. 4to. Gotha, 1860.
Photographic Society—Journal, No. 95. 8vo. 1860.
Royal Society.—Descriptive Catalogue of the Portraits in the possession of the Royal Society, by C. R. Weld. 8vo. 1860.
Sturz, J. J. Esq. (the Author)—On Emigration to Canada. 8vo. 1860.
Statistical Society—Journal, Vol. XXIII. Part 1. 8vo. 1860.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, Nov. und Dec. 1859. 4to.

WEEKLY EVENING MEETING,

Friday, April 20, 1860.

Sir Henry Holland, Bart. M.D. F.R.S. Vice-President, in the Chair.

T. Spencer Cobbold, M.D. F.L.S.

On the Scope and Tendency of the Natural History Sciences.

[Dr. Cobbold, in consequence of illness, was prevented from giving an abstract of his discourse.]

WEEKLY EVENING MEETING,

Friday, April 27, 1860.

Colonel Philip James Yorke, F.R.S. Vice-President, in the Chair.

F. A. Abel, Esq.

DIRECTOR OF THE CHEMICAL ESTABLISHMENT OF THE WAR DEPARTMENT.

On recent Applications of Science, in reference to the Efficiency and Welfare of Military Forces.

Some introductory remarks were offered illustrative of the very numerous and important improvements which had taken place within the last few years in almost everything connected with the efficiency, comfort, and general welfare of the soldier, and which had been effected by the successful adaptations made, from time to time, of discoveries and improvements in applied sciences.

The general introduction of rifled small-arms; the great perfection and saving of cost attained in the manufacture of all implements of war; the employment of electric telegraphs in the field, may be quoted as examples of important results completely or partially attained even during the late war.

One of the most important subjects in connection with military equipment, and one which has recently received a very large share of general attention, relates to the changes which have gradually been effected in the nature of material, and the principles of construction, applied to the production of cannon.

Until very recently the materials used for cannon have been only of two kinds; cast iron and bronze, or rather the alloy of copper and tin, known as gun-metal.

Of these, the latter is by far the most ancient. Guns were cast of bronze in France and Germany about 1370, and from that period until the close of the 15th century, this material gradually replaced wrought iron, of which guns were constructed in the first instance. An examination of such iron guns, of early date, as are still in existence (such as the Mons Meg, of Scotland, the great gun of Ghent, and others), shows that the principles involved in their general construction are precisely those which have just been most successfully applied to the production of wrought iron rifled guns in this country. Those ancient guns were built up of stave-bars arranged longitudinally, upon which wrought iron rings were shrunk. The very imperfect nature of those structures, arising from the primitive condition of mechanical and metallurgic appliances at that early period, rendered their durability exceedingly uncertain; and it is therefore not surprising to find that compound guns of this class were gradually replaced by cannon cast in one piece. Even the great expense of bronze, as compared with iron, was counterbalanced by the vast amount of time and labour which must have been bestowed on the construction of the old wrought-iron guns.

Although cast iron was applied to the production of shot and other projectiles at the close of the 14th century, it was not until about 1660 that cannon were made of this material. In proportion as the facility of its production increased, its application in this direction was gradually extended; but in no country has it ever entirely superseded bronze or gun-metal, which, on account of its superior tenacity, has always been employed for the construction of light field-guns. This alloy possesses, however, some very serious defects, arising principally out of its softness and its consequent incapacity to resist the injurious effects of rapid firing. Numerous experiments have been made with alloys of copper, and, recently, with other combinations of that metal, with the object of discovering some material, at least equal to gun-metal in tenacity, and superior to it in hardness and also in uniformity.

Alloys of copper and aluminium have been proposed; but, apart from the present great cost of aluminium, the readiness with which this metal is attacked by alkaline substances, and the powerful corrosive action which portions of the products of decomposition of powder consequently exert upon it, preclude its application to the production of a substitute for gun-metal. The effect of silicon in hardening and greatly increasing the tenacity of copper has also received attention; and there appears little doubt that, the difficulty of producing on a large scale an uniform compound of copper and silicon once overcome, such a material would prove a most valuable substitute for bronze. The effects of a small quantity of phosphorus upon copper are similar to those of silicon; the metal is greatly hardened, its uniformity may be ensured, and its tenacity is also much increased. Copper containing from two to four per cent. of phosphorus will resist a strain of from 48,000 to 50,000 pounds on the square inch, while the average strain borne by gun-metal is about 35,000 pounds. Uniform compounds of phosphorus and copper can, moreover, be prepared without difficulty upon a large scale. By immersing pieces of phosphorus for a short time in a solution of sulphate of copper they become coated with a film of the metal, so that they may be safely handled, and thrust beneath the surface of liquid copper before the coating melts; thus, the phosphorus is readily combined with the copper without loss.

The great success which has recently attended the construction of malleable iron guns, appears, however, to render it doubtful whether any of the compounds above referred to, or others of a similar character, will ever receive employment as materials for cannon. Attempts have been made from time to time, for many years past, to produce forgings of malleable iron of sufficient size for conversion into cannon. The great difficulty of ensuring anything approaching uniformity of chemical composition and physical properties in cast iron, and the consequent great variation and uncertainty of the enduring power of guns made of that material, acted as powerful incentives for the prosecution of such experiments. Experience gained during the recent war was also unfavourable, partly to the employment of cast iron as the material for the heaviest pieces of ordnance, and partly to the system of casting these hitherto in use. An important series of experiments recently carried on under the United States' Government, with reference to the application of cast iron for the production of cannon, had furnished many valuable results; and the continuation of these experiments and enquiries, both in America and in this country, appeared to promise considerable improvements in the general quality and uniformity of cast-iron cannon. Meanwhile, however, the importance of securing, without loss of time, more uniformly durable guns, to which the principle of rifling could be applied with greater security than to cast iron, became an additional inducement for the renewal of experiments with the view of producing wrought iron cannon of large calibre.

The attempts made by Nasmyth and others to produce large forgings, sufficiently perfect for conversion into cannon, were, however, uniformly attended with failure, excepting in the instance of a very large gun (13 in. calibre), constructed at the Mersey Company's works, which has successfully withstood some severe trials, though even this gun is not a perfectly sound forging throughout. This want of success is ascribed partly to the difficulty of ensuring perfect welds throughout a very large forging, and partly to a change which is gradually effected in the physical structure of the metal, by its repeated exposure to a high temperature, and possibly also, in some measure, by its frequent subjection to powerful concussion. In large masses of wrought iron, which have been built up by welding, the fibrous structure of the metal is always found to have passed over, more or less perfectly, into a lamellar structure, and the strength of the mass thus becomes very considerably diminished.

While unsuccessful attempts to construct cannon of large masses of malleable iron were still in progress, Mr. Mallet, Captain Blakeley, and others, who had given the subject of the construction of cannon of large size their serious attention, and had applied mathematical reasoning to its elucidation, had arrived at the conclusion that the true system to be followed was that of constructing cannon of several parts, combined in such a manner as to render every portion of the metal available in resisting, by its tenacity and elasticity, the strain exerted upon the gun by the explosion of powder. The method of construction proposed by those gentlemen consisted in preparing, in the first instance, cylinders (or rings, to be afterwards braced together), and in shrinking upon these, other rings, of which the internal diameter was somewhat less than the external diameter of the first rings or the cylinder. The latter are thus placed in a state of compression, while the external rings are in a state of tension. Other rings are again shrunk upon the outer ones, according to the size of the gun and the strain which it has to bear. In this way, the whole of the metal composing a heavy gun or mortar is arranged in a condition most favourable to the effectual resistance of a sudden strain applied from the interior. A gun constructed on this plan, by Captain Blakeley, has exhibited very great enduring powers. Two enormous mortars have also been constructed by Mr. Mallet on the same principle; and, although the trials with one of these were only partially successful, the correctness of the principles above referred to were in no way impugned by the results obtained.*

The methods adopted for the production of the beautiful rifle-gun invented by Sir W. Armstrong, which is rapidly replacing the old

* In referring to some photographic illustrations of the construction of Mallet's mortar, allusion was made to the very numerous applications which the photographic art now received to military purposes; being not only employed as the best means of recording the results of experiments, but also furnishing the most ready and effective method of communicating to military authorities at a distance any changes introduced in the nature and arrangements of military equipments, or of giving instruction in points of drill, &c.

bronze field guns, afford an interesting illustration of the application of the above system to the construction of very light and durable cannon. This gun consists essentially of rings, partly welded together so as to produce a cylinder or barrel of sufficient length, and partly shrunk one upon another, so as to impart the requisite strength to the structure. The rings themselves are from two to three feet in length, and are formed out of long bars, which are coiled up, when at a red heat, into spiral tubes, and afterwards welded into solid rings or tubes, by a few blows from the steam-hammer, applied to one end of the heated coil, while in a vertical position. The rings are united, to form the barrel of the gun, by raising to a welding heat the closely proximate extremities of two rings, placed end to end, and then applying a powerful pressure to the cold ends of the rings. In the large guns, a second layer of rings is shrunk on to the first set, or barrel, throughout the length; but in the smaller guns, it is only behind the trunnions that two additional rings are shrunk on, one over the other. The outer ring is exactly like those already described; but the intermediate one is prepared by bending two iron slabs into a semi-cylindrical form, and then welding them together at the edges. In this way, a cylinder is obtained in which the fibre of the iron is arranged longitudinally instead of transversely, as in the other rings. This arrangement is adopted, because that part of the gun has to sustain the principal force of the thrust upon the breech, on the discharge. It is into this portion that the breech-screw (made of steel) fits, by means of which a moveable plug of steel, provided with a soft copper washer, is pressed up against the end of the barrel, when the gun has been loaded. The breech-screw being hollow, the charge is introduced through it into the gun, on the removal of the plug.

This gun, built up of so many pieces accurately welded and turned and fitted, with its 30 or 40 grooves, its neat lever-arrangement for working the breech-screw, its admirable sights for giving direction, and various other arrangements, contrived so as to render it a most complete and perfect weapon, is undoubtedly very costly as compared with the ordinary cast-iron gun. But, owing to the admirable system of manufacture and the beautiful mechanical appliances brought to bear upon the production of each part, the original cost of the gun has already been very much diminished; on comparing the price of a 12-pounder gun with that of a bronze gun of the same calibre, which it has now superseded, the latter is found to be about double the expense. The price of iron used for the manufacture of the Armstrong gun is £19 per ton; it is the best description of malleable iron, bearing a tensile strain of about 74,000 pounds on the square inch. The present cost of a 12-pounder gun (weighing 8 cwt.) is about £93. The value of gun-metal is about £125 per ton; and the cost of a 12-pounder gun of this material (weighing 19 cwt.) is £175. 10*s*. Of the latter, it may be said, that when no longer serviceable it may be re-cast, while an old Armstrong gun cannot be re-converted into a new one. But, on the other hand, the average number of rounds which can be fired from the old gun before it is unserviceable, scarcely exceeds 1000, while the limit to the powers of endurance of the Armstrong gun is not yet known. Between 5000 and 6000 rounds have been fired from one, without any vital injury to the gun.

While these important results have been obtained with guns of wrought iron, built up of rings, others, scarcely less valuable, have attended the application of materials, varying in their nature between steel and malleable iron, to the production of light guns, cast in one piece. M. Krupp, of Essen, was the first to produce masses of cast steel of sufficient size for conversion into cannon. A 12-pounder gun, cast of this material, was experimented upon in this country several years ago, and exhibited the most extraordinary powers of endurance, having withstood the heaviest proofs without bursting. Similarly good results were obtained with cast steel in France and Germany, and it is now applied to the construction of the rifled field guns in Prussia. A cast material, somewhat similar in character to this steel of M. Krupp, and to which the name of homogeneous iron has been given, has recently received most successful application in the hands of Mr. Whitworth, not only to the production of the barrels for his rifle-small-arms, but also to the manufacture of his beautiful rifle-cannon. The smaller cannon are cast in one piece, and then forged to the required form. The heavy guns (80 and 100-pounders) consist, however, of cylinders of homogeneous iron—upon which hoops of fibrous iron are forced by hydraulic pressure, the breech-portion receiving hoops of puddled steel. The small Whitworth guns undoubtedly possess the great advantage of simplicity of construction over the compound guns just described; but the present great expense of the material gives the latter the advantage in point of cost. There can be little doubt, however, that the facilities for obtaining products of this description will increase with the demand; and there appears no reason why the process of Mr. Bessemer, which has recently been applied with great success to the conversion of iron of good chemical quality into excellent cast steel, upon a very considerable scale, should not be resorted to for the production, at a moderate cost, of masses of cast steel, or a material of a similar character, of sufficient size for conversion into cannon of all sizes but those of the heaviest calibre, which it will perhaps always be found most advantageous to construct of several pieces, upon the principles just now referred to.

The improvements effected in the construction of fire-arms have rendered indispensable a careful revision of the descriptions of gunpowder hitherto used, which has already led to the modification of several important points in the manufacture of powder, whereby a greater uniformity in the action of the latter is ensured, and its explosion is regulated with special regard to the double work which it now has to perform in the greater number of rifled arms, namely that of propelling the projectile, and of expanding it into the grooves of the rifle.

The necessity of affording assistance to the removal from the gun of the residue left on the discharge of powder, the escape of which is to a great extent prevented by the destruction of windage in the rifle-gun, has led to the introduction of lubricating materials which are placed in the gun, together with the charge of powder, in the form of plugs or wads of the materials only, or of hemp, or sawdust, saturated with the fatty matter. In the case of rifled small-arms, the lubricant has to be applied to the exterior of that portion of the cartridge-paper which surrounds the bullets, and considerable difficulty was experienced in the first instance, in the selection of a suitable material for this purpose. Tallow was first employed, in admixture with sufficient beeswax to harden it somewhat, and enable it to resist the effects of warm climates. It was found, however, that the tallow, penetrating the paper, soon established a corrosive action upon the surface of the lead, which proceeded occasionally to such an extent as to cover the bullet with a hard coating of compounds of oxide of lead, thus increasing its diameter so greatly as to render its introduction into the barrel impossible. Many materials were tried, as substitutes for tallow, which did not possess a tendency to promote the corrosion of lead, and eventually beeswax, used alone, was found not only to protect the metal, but also to act as a most efficient lubricator in small arms, and to possess great permanence in all climates. In discontinuing the use of tallow, it was also found indispensable to avoid the use of any alterable oils as lubricators in the bullet-making machines, as the small film of oil remaining on the lead, was found in many instances sufficient to establish a rapid corrosion of the metal. Such an occurrence is now avoided by the use as lubricant for the machines, of the neutral and permanent oil prepared by Price's Candle Company from Rangoon Petroleum.

Considerable attention has been devoted in different continental states, during the last few years, to the application of the different forms of electricity to the discharge of mines. The many serious inconveniences attending the employment of voltaic batteries for that purpose in the field have led to the use, with considerable success, of the arrangements contrived by Ruhmkorff and others for the production of powerful electro-magnetic currents. The application of the induction-coil machine, with appropriate fuse-arrangements for the ignition of the mine by means of the spark, led to a very great reduction in the size of the battery required even for extensive operations. The necessity, however, of still using a battery, and the great liability to injury of the induction-apparatus, have rendered the advantage to be attained by their employment somewhat questionable. In Austria, very important results are said to have been obtained by the employment of frictional in the place of voltaic electricity. A very portable arrangement of a plate-electric machine, with Leyden jars, and a small stove to protect the apparatus from damp, has been employed with success in some extensive operations, as many as one hundred charges having been fired simultaneously by its means. Professor Wheatstone and Mr. Abel have carried on numerous experiments on the application of electricity in this direction; and, at the suggestion of the former, attempts were made to employ the electricity obtained, by induction, from permanent magnets. No difficulty was experienced in igniting a single charge by its agency; but it was found that the ignition of more than one charge could not be effected with certainty, by the employment even of the most powerful magnets and the use of fuses containing very sensitive compositions. Eventually, a fuse-arrangement was contrived and a composition prepared, by Mr. Abel, with the employment of which the ignition of several mines could be effected with certainty, by means of one of the small magnetic arrangements employed by Mr. Wheatstone in his portable telegraphs; and an ingenious combination of several such magnets, arranged in a form very portable and readily worked by any soldier, can be applied with equal certainty to the discharge of a considerable number of mines. The great element of success in the fuse-composition employed, is to be found in the circumstance that it combines a high degree of sensitiveness with considerable conducting power. The substitution of the magnet for the voltaic and other arrangements hitherto used will greatly facilitate mining operations; the soldier requires but little instruction in its use; with ordinary care it is not liable to derangement; it is very transportable, and ready for application at the shortest notice.

In connection with submarine operations, vulcanised india-rubber bags have become valuable substitutes for the wooden and metal receptacles hitherto employed for the charges of powder. The numerous applications which india-rubber, especially in its vulcanised form, now receives in connection with military equipment, render it a most indispensable material. Thus, it has been applied to the preparation of waterproof linings for powder-barrels, waterproof cases for cartridges, convenient holders and waterproof coatings for percussion caps; it is used in the form of springs and buffers in connection with gun-carriages and the beds of heavy mortars; ambulance waggons are supplied with efficient and easily applicable springs of india-rubber; and one of the most important additions recently made to the comfort of troops has been the general supply to them, when on active service, of waterproof clothing and covers, to be used in camp.

The protection of camp-erections from fire has also received attention with successful results. A cheap and ready mode of applying a coating of insoluble silicate of lime and soda to the surfaces of camp-huts, whereby very important protection against fire is attained, received application a few years ago; and quite recently a method has been devised by Mr. Abel of impregnating tent-cloth with silicates, to such an extent as effectually to prevent fire from spreading, when applied to any portion of it, and in such a form as to enable them to resist the solvent effect of drenching rains.

The application of soluble silicates to the preparation of very porous artificial stone has enabled Mr. Ransome to produce portable filters, by the aid of which the soldier may frequently be enabled to partake of water, which otherwise would be unfit for use. A still more efficient portable filter is now, however, prepared of carbon in a porous condition, which not only has the property of retaining the mechanical impurities of water, in its passage through it, but also will purify it to a very considerable extent from injurious organic matters and gases which it may contain.

One of the most important improvements which have yet been effected in the purification of water, and one which has already received important application in connection with the military service, is presented in the apparatus contrived by Dr. Normandy for the preparation of wholesome and pleasant water from sea or other water unfit for consumption. The apparatus consists, in the first instance, of a great improvement on the condensing arrangement contrived by Sir T. Grant, which has been for some time used in the navy. The heat abstracted from the steam first consumed is applied to the distillation of a second similar quantity of water, and the arrangement employed for condensing this second product is of such a nature as to ensure a very gradual but continuous replacement of the condensing water. In this manner the latter becomes sufficiently heated, before it passes out of the apparatus, to part with the gases which it contains in solution, and which are made to pass into the distilling apparatus and mix with the steam. The condensed product is thus thoroughly aërated; it is then, finally, made to pass through a charcoal filter, which completely deprives it of the disagreeable empyreumatic flavour always possessed by distilled water. Independently of the applications which this apparatus is receiving to the supply of ships with water, it has proved very valuable in readily and continuously producing large quantities of wholesome water for the supply of troops at stations where the only water procurable was unfit for consumption.

The important subject of the economical supply of well-cooked and palatable food to troops in barracks and on active service, which had been considerably neglected previous to the late war, has received great attention on the part of Captain Grant, and the results of his labours in this direction have been the production of most efficient cooking-ranges for barracks, and equipments for cooking in the field. By the employment of the range, with oven attached, which has been contrived by him, and is used at Aldershot, Woolwich, and other military stations, the cost of cooking for a large number of troops (800 to 1000 being supplied with food from one range) has been reduced to one halfpenny per man per week; and by further improvements, which Captain Grant is just carrying out, it will still be subject to considerable reduction. The food is, at the same time, cooked in various ways by means of the oven and other appliances. An arrangement has been devised by Captain Grant, and used by troops with great success, for cooking in the field, in long cylindrical boilers, which are so disposed over trenches dug for the purpose that, with a very small consumption of fuel, well-cooked food may be supplied from eight of them, in between two and three hours, sufficient for 800 men. These kettles are of such a form that they may also be made to serve the purpose of pontoons in the construction of bridges.

The subjects briefly discussed in this discourse can only be regarded as examples of the many directions in which every branch of science has recently received application in connection with the military service.

[F. A. A.]

ANNUAL MEETING,

Tuesday, May 1, 1860.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1859 was read and adopted.

The statement of Sums Received shows a steady and gradual increase in the yearly income. The amount of Annual Contributions of Members and Subscribers in 1859 amounted to £2140. 19*s.* 0*d.*, the Receipts from Subscriptions to Lectures were £883. 11*s.* 6*d.*; the total Annual Income amounted to £5440. 6*s.* 5*d.*: each amount being more than had been received in any previous year.

On December 31, 1859, the Funded Property was £26,583. 14*s.* 1*d.*; and the Balance £1157. 15*s.* 2*d.*, with Six Exchequer Bills of £100 each. There were no Liabilities.

A List of Books Presented, amounting in number to 243 volumes, accompanies the Report; making, with those purchased by the Managers and Patrons, a total of 670 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to Professor Faraday, for their services to the Institution during the past year.

The following Officers were unanimously elected for the ensuing year:—

President—The Duke of Northumberland, K.G. F.R.S.
Treasurer—William Pole, Esq. M.A. F.R.S.
Secretary—Rev. John Barlow, M.A. F.R.S.

WEEKLY EVENING MEETING,

Friday, May 4, 1860.

WILLIAM ROBERT GROVE, Esq. M.A. Q.C. F.R.S. Vice-President, in the Chair.

EDWIN LANKESTER, M.D. F.R.S.

On Bread-Making and Baking.

THE speaker stated that the principal object he had in view in delivering the discourse was to answer the question so often put as to what was "aërated bread." Bread, as used at table, assumed two forms dependent on its preparation, *vesiculated* and *unvesiculated*. The latter is known under the name of *unleavened* bread, and consists of such preparations of flour as biscuits, passover cakes, &c. Vesiculated bread is prepared in two ways, either by *fermentation* or *aëration*. In all cases fermented bread is made from the flour of wheat, or a mixture of this with the meal or flour of other grain. Barley, oats, maize, rye, will not alone make fermented bread. The meal of these grains is added to wheaten flour, when they are made into bread.

Wheaten flour is made from the grains of wheat, which are the fruit of the plant. Six layers of cellular tissue were described between the albumen or perisperm of the seed, and the outside of the grain :—1. The epicarp; 2. The sarcocarp; 3. The endocarp—(these belong to the fruit; 4. The testa of the seed; 5. A secondary membrane; 6. The covering of the perisperm. These layers constitute the *bran*, which is separated from the fine flour. They contain the same chemical constituents as the flour, and, so far from being objectionable, are a desirable addition to the flour. The gluten of the flour is represented in the bran by a principle, called by its discoverer *cerealin*. Like gluten, it acts as a ferment; but its power in this respect is said to be destroyed at a temperature of 150° Fahr. It is soluble in cold water, and in that state acts as a ferment. Bran tea accelerates the changes of fermentation. It is this agent, which during the fermentation of bread gives the brown colour to meal bread. Twenty-one ounces of wheat yield five ounces of bread, and sixteen ounces of fine flour. One pound of flour contains

Water	2¼	ozs.
Gluten	2	,,
Albumen	¼	,,
Starch	9½	,,
Sugar	1	,,
Gum	¼	,,
Fat	⅛	,,
Cellulose	¼	,,
Ashes	¼	,,

The gluten and albumen are flesh-forming substances, sugar and starch heat-giving. In the making of fermented bread, yeast is added to the flour, and the gluten is put into a state of change, but not decomposed. A small portion of the starch is converted into glucose, which is decomposed, and alcohol formed, and carbonic acid produced. The carbonic acid gas escaping from the mass vesiculates the bread. The quantity of starch changed in this process is very small. It is expressed by the quantity of carbonic acid gas necessary for the vesiculation of the bread, as little or none of this gas escapes in the *rising* of the bread. The conversion of starch into glucose during the fermentation of the bread does not appear to be greater than is necessary to form the carbonic acid for vesiculation. The starch during fermentation acquires the power of being more quickly converted into glucose and its subsequent products, than when heated and not exposed to this process. This is probably the great peculiarity of fermented bread, that the starch more rapidly passes into a state of change.

When the starch of wheat has already acquired this tendency to change, from the sprouting of the wheat, the flour forms a sweet, heavy, and sticky bread. In order to prevent this, alum is employed. Alum is not necessary to the making of palatable bread from "sound" flour, but it is necessary for the making of saleable bread from unsound flour made from sprouted grain. The habitual use of alum in bread is undoubtedly injurious, especially to the young.

Bread is vesiculated without being fermented by two processes:—1. By the addition of substances, which, during their decomposition give out carbonic acid, as carbonate of soda and hydrochloric acid. 2. By making the bread with water charged with carbonic acid. The first is the process recommended by the late Dr. Whiting, and sold in London under the name of Dodson's Unfermented Bread. The second process consists in mixing intimately water containing carbonic acid with flour, so that when the dough is baked the escape of the carbonic acid gas vesiculates the bread. This process is worked in London under Dr. Dauglish's patent,* and extensive machinery for making this bread has been erected by Messrs. Peek, Frean, & Co., at Dockhead. This is the "Aërated Bread." The process of making fermented bread is tedious; the time employed for making the bread varying from three to twelve hours. By the aërating process, the whole time taken from mixing the flour and carbonated water to putting the loaves into the oven is only twenty-six minutes. The necessity of handling the dough in kneading is also avoided by the use of machinery. Other advantages of this process are the saving of the starch destroyed in fermenting bread, and the absence of yeast and other substances, as potatoes, employed for facilitating the process of fermentation.

The Baking of the bread is the same in all processes. At the same time the healthy digestion of bread depends much on the way in which this process is conducted. The regularity of the temperature and the condition of the atmosphere in the oven exert a considerable influence on the wholesome character of the bread. An oven has been recently constructed by Mr. Bonthron, of Regent-street, by which steam can be turned into the atmosphere of the oven. The action of the steam prevents the charring of the crust of the bread, allows of the interior expansion of the bread by preventing the hardening of the crust, and produces a natural varnish on the outside by reducing the sugar and gum on the outside to a liquid state.

With regard to the action of the two breads on the system, there can be no doubt that either, when properly prepared and baked, is adapted for general use. The question of flavour or appearance every one will decide for himself. In certain morbid conditions of the stomach fermented bread undergoes rapid changes, which are productive of inconvenience, and which is prevented by the use of unfermented bread.

[E. L.]

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* Since the delivery of the lecture, my attention has been called to the fact that a patent was obtained by Mr. Luke Hebert in 1833, for making bread with carbonic acid gas.—E. L.

GENERAL MONTHLY MEETING,

Monday, May 7, 1860.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

The Secretary announced that His Grace the President had nominated the following Vice-Presidents for the ensuing year:

The Duke of Wellington.
The Lord Ashburton.
W. R. Grove, Esq.
Sir R. I. Murchison.
Charles Wheatstone, Esq.
Wm. Pole, Esq. *the Treasurer.*
Rev. J. Barlow, *the Secretary.*

Henry Barnett, Esq.
Edward Lewton Cox, Esq.
John Edward Cox, Esq.
William Watkiss Lloyd, Esq.
James Marshall, Esq. and
John Walter, Esq. M.P.

were duly *elected* Members of the Royal Institution.

The following Professors were re-elected:—

William Thomas Brande, Esq. D.C.L. F.R.S. as Honorary Professor of Chemistry.

John Tyndall, Esq. Ph.D. F.R.S. as Professor of Natural Philosophy.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same: viz.

From

Académie des Sciences de l'Institut, Paris—Mémoires, Tome XXV; Tome XXVII. Partie 2. et Tome XXXI. 4to. 1860.; Annuaire, 1860. 16to.
Actuaries, Institute of—Assurance Magazine, No. 39. 8vo. 1860.
Adhémar, M. J. (*the Author*)—Révolutions de la Mer: Déluges Périodiques. 2e Ed. 8vo. Paris, 1860.
Arts, Society of—Journal, April 1860. 8vo.
Asiatic Society of Bengal—Journal, No. 275. 8vo. 1860.
Astronomical Society, Royal—Proceedings, No. 5. 8vo. 1895.
Baines, Mrs. M. A. (*the Author*)—On Human and Animal Milks; and on Hiring Wet Nurses. 8vo. 1860.

Barclay, Mr. G. (the Author)—Designs for Marking Silver Plate. 8vo. 1860.
Bavarian Academy of Sciences—Festreden von Dr. W. Christ und Justus Freiherrn von Liebig. 4to. 1860.
Boosey, Messrs. (the Publishers)—The Musical World for April 1860. 4to.
Bradbury, Henry, Esq. M.R.I. (the Author)—Specimens of Bank Notes and other Engravings. 4to. 1860.
Chambers, Geo. F. Esq. M.R.I. (the Author)—Planetary Tables. 8vo. 1860.
Civil Engineers, Institute of—Proceedings in April 1860. 8vo.
Duquin, M. P. A. (the Author)—Traité de Physique. Tome III. 8vo. 1859.
Davis, Hewett, Esq. M.R.I. (the Author)—Practical Essays for the Improvement of Farming. 8vo. 1860.
Dove, Professor, F.R.S.—Meteorologische Tabellen über der Preussischen Staat. fol. 1858.
Editors—Artizan for April 1860. 4to.
Athenæum for April 1860. 4to.
Atlantis, No. 5. 8vo. 1860.
Engineer for April 1860. fol.
Horological Journal, Nos. 20, 21. 8vo. 1860.
Journal of Gas-Lighting for April 1860. 4to.
Mechanics' Magazine for April 1860. 8vo.
Medical Circular for April 1860. 8vo.
Practical Mechanic's Journal for April 1860. 4to.
St. James's Medley, No. 22. 8vo. 1860.
Faraday, Professor, D.C.L. F.R.S.—Abhandlungen der Akademie der Wissenschaften zu Berlin; 1858. 4to. 1859.
Franklin Institute of Pennsylvania—Journal, Vol. XXXIX. No. 3. 8vo. 1860.
Geological Survey of India—Memoirs, Vol. I. Part 3. 8vo. 1859.
Annual Report 1858-9. 8vo. 1859.
Halswell, Edmund, Esq.—Report of the Visitors of the County Lunatic Asylums at Hanwell and Colney Hatch for 1860. 8vo. 1860.
Harle, C. E. Esq.—Engraving of an Impression from "Darius's Seal," in the British Museum.
Mackie, S. J. Esq. F.G.S. (the Editor)—The Geologist, April 1860.
Medico-Chirurgical Society, Royal—Proceedings, Vol. III. No. 3. 8vo. 1860.
Newton, Messrs.—London Journal (New Series), for April 1860. 8vo.
Novello, Mr. (the Publisher)—The Musical Times for April 1860. 4to.
Petermann, A. Esq. (the Editor)—Mittheilungen auf dem Gesammtgebiete der Geographie. 1860. Heft 4. 4to. Gotha, 1860.
Photographic Society—Journal, No. 96. 8vo. 1860.
Royal Society of Edinburgh—Transactions, Vol. XXII. Part 1. 4to. 1857-9.
Proceedings, No. 49. 8vo. 1858-9.
St. Petersbourg, Académie Impériale des Sciences—Mémoires: VII. Série. Tome I. 4to. 1859.
Bulletin, Tome I. Nos. 1-3. 4to. 1859.
Smyth, Admiral W. H. and Dr. John Lee, F.R.S.—Cycle of Celestial Objects, continued at the Hartwell Observatory to 1859; by Admiral W. H. Smyth. 4to. 1860.
Ward, Stephen H. M.D. (the Author)—On Rational Medicine. 8vo. 1860.
Walker, James, Esq. F.R.S. M.R.I.—Brief Memoir of Capt. Joseph Huddart, and an account of his Inventions in the Manufacture of Cordage, by W. Cotton. 4to. 1855.
Webster, John, M.D. F.R.S. M.R.I.—Reports on Bridewell and Bethlem Hospitals for 1859. 8vo. 1860.

WEEKLY EVENING MEETING,

Friday, May 11, 1860.

The Duke of Wellington, K.G. D.C.L. Vice-President, in the Chair.

Thomas Mayo, M.D. F.R.S.
PRESIDENT OF THE ROYAL COLLEGE OF PHYSICIANS.

On the relations of the Public to the Science and Practice of Medicine.

The speaker opened his discourse with remarks on some peculiarities in the relations of society to the medical profession. The business of the profession is conducted among perturbations varying in kind and intensity. The medical practitioner depends upon his skill and success in dealing with a commodity, in relation to which the emotions of his heart and the exertions of his intellect are largely called forth, while the recipient of the supposed or hoped for benefit is with different degrees of intensity pained and annoyed by having to apply for it. And the relation of the practitioner to society is thus rendered more peculiar by his having to give a certain amount of satisfaction, where all those feelings of the human mind are actively at work, the most calculated to confound the judgment of those whom he has to satisfy. He is tried, *coram non judice.*

There is thus, the speaker observed, a sort of atmosphere of repugnance generated by the very nature of the case, between the profession which is presumed to confer the benefit of health, and the public which is presumed to receive it. It was one main object of this discourse to point out, that in spite of these points of antagonism, they have to co-operate, with a view to success; to co-ordinate their efforts; and that the public cannot divest itself of responsibility by placing a case in the hands of the physician, as it can, when dealing with an advocate under analogous circumstances. There are certain great physiological truths, lending themselves both to hygiene and to medicine, which it is incumbent on the public to acquire as a part of education. "If men," says M. Auguste Comte, "do not confide the study of astronomy to navigators, neither should they leave physiology to the leisure of physicians."

In this way, the public may attain another very important object. It claims, and justly claims, a right to choose between conflicting schools of medicine. This right it may exercise with more safety and advantage, by obtaining some knowledge of that great human system which is the common arena of all the professors of the healing art. Every school, normal or abnormal, must make its appeal to physiological principles. How much good may be done, how much evil may be avoided, by an enlightened application of the public mind in these directions, the speaker proceeded to point out, by directing attention to the phenomena of growth and progress in certain systems, according as the attention of the public has or has not been called to them. For this purpose, he took the allied theories of Dr. Currie of Liverpool, and M. Priessnitz, the author of what is termed hydropathy. About the end of last century, Dr. Currie, reasoning deductively from facts observed by himself and Dr. Wright, affirmed the applicability of the affusion of cold water in a large group of cases. His views have no doubt left behind them in the medical mind an increased tendency to avoid certain mischievous agents in the treatment of febrile disorders. But they have not fructified consistently with his great and well-deserved reputation. "In the first British epidemic which broke out subsequently, they were speedily abandoned by all practitioners," says Dr. Christison, writing in 1840; "and for twenty years Dr. Currie's theory has been almost unknown in the treatment of fever." Nor has any speculative question since arisen among us, as to what might possibly yet be deducible from a scientific hypothesis at first eminently successful.

Very different has been the fate of the hydropathic theory, contrived by an untutored Silesian peasant, neglected by the regular profession, and cared for by a credulous, but not an indifferent public. If, in the latter case, a rash sciolism has been largely predominant, against which a *better informed* public might have protected itself, this must at least be admitted, that an important principle of treatment has thus been kept alive by the public, though put before them in its least scientific shape. While a similar principle of treatment from which all the valuable elements of the hydropathic system might have been deduced by scientific thinkers, without its concomitant mischief, has been allowed by the medical profession to sleep and be forgotten.

With respect to the mischief concomitant to hydropathy, the speaker quoted the following passage from a work by Dr. Bence Jones—"Until Professor Liebig directed attention anew to the action of oxygen on the human body, the causes of success or failure in hydropathy were unknown. The greatest possible action of the skin is produced under hydropathy, on the system, by baths: large quantities of water are taken, and by these means the action of oxygen on the body is promoted to a very high degree, and *death ensues, if ever the system is unable to furnish matter to resist the action of oxygen.*" A fact, which, on the highest authority, indicates the expediency of an increased amount of medical and physiological knowledge in the public, who select for themselves both medical systems and medical advisers.

The speaker, after expressing his regret that the patronage of *the public* had not been extended to another hypothesis, equally neglected *by the profession**, that namely of Dr. Beddoes, whose scientific merits were as undoubted as those of Dr. Currie, proceeded to consider the question, how far his remarks in favour of popularising some amount of medical or hygienic education apply to the capabilities and position of females.

Having thus far estimated the action of the public on the medical profession, the speaker proceeded to consider the action of the profession on the public, *i.e.* the share of duties which belong to the profession in regard to it. In this point of view, he proposed, as within the limits of his allotted time, to describe the mental dynamics of the profession, whether as engaged in actual practice, or in the province of research.

The first object of medical reasoning thus contemplated, when brought to bear upon practical conclusions, whether at the bedside of the patient, or in ulterior pathological enquiry, is to reduce cases to the general law, or laws, under which they fall. Subservient to this purpose, a large fund of systematized knowledge, both in pathology and treatment, has been accumulated. Some amount of attentive observation may possibly by their means satisfy the requirements here supposed, and assign the cases to the place which they hold in the history of disease. But, owing to the points of difference by which men are individualized, combined with our still imperfect scientific knowledge, the case may seldom be thus disposed of. It is often seen to contain elements, neither referable to known causes, nor capable of being subordinated to any previous theory. Here, another faculty of a higher and more energetic character must be called into use by the practitioner or thinker. He must drift helplessly down the current of his own associations and those supplied to him by the teaching of others, unless he is able to seize some leading idea to be applied by him to the nature and conduct of the case. He must in fact extemporise an *hypothesis*. If truth more readily merges out of error than out of confusion, some process of this kind must be adopted, even though it should suggest in the case before him a temporary negation of measures. The imagination, the speculative power, and the memory must combine with the knowledge of the physician, to answer the questions here suggested; and, according to the construction of his mind on these points, the hypothesis will come out clear and practical, or fanciful and obscure. This is the analysis of an operation, which, under the influence of habit, often bears the character of an instinctive perception.

While this kind of reasoning is immediately applicable in actual practice, it enters also into the speculations of those who have most extended the boundaries of our knowledge; but with this important difference, that the physician must often be contented with an hypothesis at the bedside, which will explain the symptoms of the patient, and thus suggest grounds, where immediate practice is required; while

* The treatment of disease by Pneumatic Medicine.

he who is engaged in extending medical science by publication or teaching, must be less easily satisfied. *His* hypotheses must not only be explanatory, they must also be verifiable by induction. They have to serve a purpose, not limited to a present case, but calculated to exercise an influence over future years and generations, as involving a principle.

These doctrines the speaker could not illustrate before his audience by taking them to the bedside of the patient, in reference to the immediate practical hypothesis. But he brought forward one instance of the uses of hypothesis in research. It related to the disorder called senile apoplexy, the apoplexy of the aged. That the usual antecedents of apoplexy, a congestive state of the circulation, should occur in an *aged* brain might appear improbable ; and the more so, when it is considered that in age the brain undergoes a process of atrophy. Meanwhile, this improbability disappears under an hypothesis derived by Dr. Rokitansky from this very assumption of an atrophic state of brain in old age. Atrophy of a part implies diminution of size in that part; and this in the brain would imply a shrinking away of the substance of the brain from the inner surface of the skull ; a fact often observed after death of the aged. Now the space thus produced between the brain and the skull, being impervious to air, is so far a vacuum ; so that, atmospheric pressure continuing to exist on the rest of the body, and being removed from the surface of the brain, it must, however extenuated the subject, be replete with blood. It is needless to say that this theory of the cause of cerebral congestion in the aged is highly suggestive of a practical conclusion. The physician, being enabled to explain the state of congestion induced in the brain into conditions immediately resulting from a state of feebleness, is justified in declining depletory measures.

Having thus discussed the value of a verified hypothesis, or in technical language a theory, the speaker considered the less complete forms of hypothetical reasoning. It happens in medical research, as well as in practice, that an hypothesis, at present unverified by induction, but which would explain the case under consideration, derives from this latter circumstance certain, though limited, amount of weight. As, for instance, the occurrence of such symptoms as would be present if a poison had been given, affords a presumption that a poison has been given ; though while the poison is undetected, it remains unverified. The hypothesis of temperaments again, though unverified, is luciferous ; it may eventually obtain inductive proof, and be fitted to take its place in the history of our science.

But an hypothesis, whether for immediate practice or for purposes of research, may not only be unverified ; it may also be unexplanatory. This latter defect, occasional among medical *writers*, and frequent *in the practice* of those by whom the force of words is not justly estimated, the speaker illustrated from the work of Mr. Charles Darwin. He presumes the terms " natural selection " to constitute an instance of it ; as *seeming only* to explain his process of transmutation of animals.

Having given this account of the workings of the medical intellect, he affirmed the misapprehension involved in the use of the term "guessing," as sometimes applied to them. Truths of a high order, as leading to most important results, have been inductively established, which can be applied with great confidence to practice. But there is, he says, one modification of medical thought, *in the present day*, which certainly may deserve the name of mere guessing, and which advances no pretensions to philosophy: one, which contemns all hypotheses as to *causes*, the only legitimate subject of treatment in medicine; and which, cataloguing *symptoms* and the presumed remedies for those symptoms, prescribes from the catalogue. Such reasoning, he says, is to be found in books purporting to convey the principles of homœopathy.

As an element of the medical intellect required and developed in actual practice and in medical research, the imagination has been already noticed. Its intellectual value in enabling the mind to realise absent objects of perception, and its moral value in affording sources of comfort to the sick, if judiciously managed, was insisted on. And the speaker finished his analysis of the medical mind by strongly insisting upon a just measure of its attention being given to the various sciences, which lay claim to its pursuit; so that undue predominance may not be given to those which may happen to be most attractive, or to afford most opportunities to the student of displaying brilliancy in competitive examination.

The next subject which engaged the speaker's attention, involves a close reciprocity of action between the public and the medical profession. It is that of "specialties in practice." While on the one hand the bent of the individual mind in favour of some one group of cases and diseases for consideration and inquiry, may justly influence the mind of the public in applying for medical advice; on the other hand, this tendency must not be gratified at the expense of comprehensiveness of knowledge. The tendency of the public to render the subject of insanity or unsoundness of mind a specialty *with this result*, was commented on.

The public prevents those medical practitioners, who have gained reputation in this specialty from becoming acquainted with other conterminous disease by excluding them from attendance on other diseases; and thus necessarily diminishes their power of diagnosis in mental disease *itself*. Hence arises ignorance of practitioners both in certifying the presence of insanity, and in giving evidence on it in courts of justice.

The relations of the public and the medical professions thus far described, have been mainly intellectual. The moral duties incumbent on the profession have long been pressed upon the attention of students in medicine, by lectures and the precepts of their instructors. No corresponding enumeration of the duties owed by the public to the profession exists. In regard to one, which appeared to the speaker to have been peculiarly neglected, he thus expressed himself. "I will suppose a young man settled in a country neighbourhood as a general practitioner. He has, I may also fairly suppose, received that amount of mental discipline, which it is difficult in his profession to escape under the expanded system of antecedent and professional education, which he will have undergone. He finds his way into houses, in many of the most respectable of which there dwell those who have enjoyed no such opportunities of mental enlargement, as I may fairly suppose him to have had. While his professional intercourse with society has involved the most confidential relations, yet, when his attendance is concluded, his actual position with respect to it may be one of comparative exclusion." Not, it may be observed, without corresponding loss to society itself, which is interested in obtaining or preserving his services not only as an adviser but also as a friend.

In conclusion, the speaker reiterated a strong expression of his opinion in favour of increased acquaintance on the part of the public with medical and physiological principles.

[T. M.]

WEEKLY EVENING MEETING,

Friday, May 18, 1860.

Charles Wheatstone, Esq. F.R.S. Vice-President, in the Chair.

Professor William Thomson, F.R.S.

On Atmospheric Electricity.

[It is hoped that an Abstract of this Discourse will be given in the next number of these Proceedings.]

WEEKLY EVENING MEETING,

Friday, May 25, 1860.

Sir Roderick I. Murchison, D.C.L. F.R.S. Vice-President, in the Chair.

William Pengelly, Esq. F.G.S.

On the Devonian *Fossils of Devon and Cornwall, with special reference to the Collection presented to the Oxford University Museum, in connexion with the Burdett-Coutts Geological Scholarship.*

The speaker, in commencing, called attention to the great variety of rock-formations in Devonshire, and stated that, with the doubtful exception of the crystalline schists extending from the Start to the

Bolt, the limestones, slates, and associated sandstones, of both the south and the extreme north of the county, are the oldest rocks in it; that they belong to, what is known to geologists as, the *Devonian* age of the world; that is, they are the equivalents or contemporaries of the old red sandstones of Scotland and other districts; that Professor Sedgwick regards them as divisible into three groups, namely, the Plymouth, or oldest, the Dartmouth, and the Barnstaple groups; that only the two first occur in South Devon, whilst all three are found in the north of the county; that the second, or Dartmouth group, has not been found to contain organic remains; that the richly fossiliferous beds of South Petherwin, in Cornwall, belong to the Barnstaple series, whilst the slates of Looe, Polperro, and other parts of the same county belong to the Plymouth group.

He then said—"Doubtless you have all learned from the public prints that two geological scholarships have recently been founded at Oxford. Were this the time and place, it would have given me great pleasure to have expressed my grateful sense of the service which the foundress has rendered to geology, and through it to science generally, in thus, not only giving a stimulus to the study of it in a place so every way important as that ancient seat of learning, but also by recognising it as a desirable, if not, indeed, an essential, part of the education of the general student; 'and especially of those destined for the ministry of the church, if they would hope to maintain the position which the interests of religion require them to maintain.' * These words are a quotation from the document drawn up by the foundress, and accepted or endorsed by the University authorities. Truly the day has passed away when the prevalent feeling was echoed by the sarcasm of the poet—

> 'Some drill and bore
> The solid earth, and from the strata there
> Extract a register, by which we learn
> That He who made it and revealed its date
> To Moses, was mistaken in its age.'—*Cowper*.

"My object, however, in alluding to this subject, here and now, is just to say that a collection of the *Devonian* fossils of Devon and Cornwall, which I had the honour to make at the request of the foundress, was presented by her to the Oxford University Museum, in connexion with the scholarships. In drawing up a brief account of this collection, some of the salient facts connected with the fossils of the district under consideration were brought somewhat prominently and, in a certain degree, methodically before me; hence this evening's discourse. The fossils on the table are from my own private, not the Oxford, collection; but duplicates of many of them will be found in that series, whilst several of the corals are the specimens figured in the monograph, on the British Devonian Fossil Corals, issued by the Palæontographical Society."

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* *Times* Newspaper, February 9th, 1860.

Mr. Pengelly then proceeded to, what may be called, the statistical part of his subject,[1] remarking that the tables at present before us must be regarded as merely representing our momentary knowledge, as in all probability many new fossils would be hereafter discovered; the figures however would probably always express tolerably exact *proportions* or *relative* numbers.

The following is a brief summary of this part of the discourse. Of the 27 classes into which the existing Flora and Fauna are divided, 15 only are represented by the fossils of the *Devonian* rocks, whether found in Britain or elsewhere in the world; as is shown in the following table:—

Represented Classes.	*Unrepresented Classes.*
*Cellulares.**	Dicotyledones.
Monocotyledones.	Infusoria.
Amorphozoa.	Foraminifera.
Zoophyta.	Entozoa.
Echinodermata.	Acalephæ.
Annelida.	Myriopoda.
Cirrepedia.	Arachnida.
Crustacea.	Insecta.
Bryozoa.	Tunicata,
BRACHIOPODA.	Reptilia.
Lamellibranchiata.	Aves.
Pteropoda.	Mammalia.
Gasteropoda.	
CEPHALOPODA.	
Pisces.*	

Those printed in italics appear to have been poor in species. It must be remembered, however, that some of them are similarly characterized in the existing creation; the classes in capitals contained a greater number of species then than they do at present; those *preceded* by an asterisk do not appear to have been met with in *Devonian* rocks in any part of the British isles, and those *succeeded* by the same mark, are not recorded as having been found amongst the *Devonian* fossils of Devon and Cornwall. The district last named has yielded 340 species, of which 60 are also found in Continental Europe, 6 in America, and 1 in New South Wales. Seven of the 340 species appear to have existed in the preceding or *Silurian* age, whilst about 60 passed over to the next, or *Carboniferous*, period. Many of the fossils of Devon and Cornwall appear to have had a very limited geographical range; thus 186 species, more than half the entire series, are peculiar to South Devon, whilst there is but one common to the contemporary deposits of North and South Devon and Cornwall, and not more than six common to any two of them.

Mr. Pengelly then proceeded to give a brief description of some of the classes. There are, he thinks, many more species of sponges in

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[1] This part of the discourse was illustrated with tables suspended on the walls, which the speaker had compiled from the writings of Bronn, Professor Morris's Catalogue of British Fossils, and the Monograph of the Palæontographical Society.

Devonshire and Cornwall than have generally been recognised; some have never been described, whilst others have been regarded as corals. In 1843, Mr. Peach brought certain fossils, which Mr. Couch had then recently discovered at Polperro, in Cornwall, before the geological section of the British Association, during its meeting at Cork: they were pronounced to be ichthyolites; and this was probably the more readily believed from the fact that it was well known that the contemporary rocks of Scotland had yielded fossil fish in great numbers, and there seemed no reason for their non-appearance in Cornwall. Mr. Peach traced these fossils from near Fowey harbour to Talland sands, about two miles west of Looe. They were subsequently found by the speaker along the entire south coast of Cornwall from Talland to the Rame Head, on the banks of the river Fowey, at Bodrethen on the north coast of Cornwall, and at Mudstone Bay, near Brixham, in Devonshire. Specimens were sent to the late Mr. Hugh Miller, who at first confirmed their ichthyic claims; but subsequently, in a paper read before the Royal Physical Society of Edinburgh, he doubted "whether their true place in the scale of being had been determined," and pronounced them "the most puzzling things he had ever seen; riddles on which to exercise the ingenuity of the palæontologist." Soon afterwards, Professor M'Coy declared them to be sponges merely, established for them the new genus *Steganodictyum*, and showed that they belong to two species, *S. Cornubicum* and *S. Carteri*.

According to the authors of the Monograph on the subject, issued by the Palæontographical Society, the *Devonian* rocks of Devon and Cornwall have yielded 49 species of fossil corals, belonging to 20 genera, 6 families, and 3 sub-orders: 3 of the species are also found in Silurian rocks, but no carboniferous forms occur.

Some of the species had a greater geographical range than others. *Favorites Goldfussi*, for example, being found in *Devonian* rocks in Britain, continental Europe, North America, and New South Wales, being in fact the most decided cosmopolite amongst them; whilst *Chonophyllum perfoliatum*, like many others, appears to have been confined to Britain. It seems tolerably safe to infer from this fact that the former possessed hardier, more plastic, and (if the word is allowable), more adaptable constitutions than the latter; and were more capable of enduring such a variety of physical and thermal conditions, as in the present phase of our planet's existence may be said almost invariably to accompany removal to localities so widely separated as Europe, America, and Australia. Nor would it, probably, be considered a very unwarrantable extension of the principle here involved, to conclude that such species would be the least affected by the thermal and physical changes, which we are not without reason for believing considerable lapses of time have always produced in any one and the same given area; changes, not improbably, very much of the same character obtaining in widely separated times in the same locality, as in widely separated localities in the same time. Taking this principle as a guide, it might have been expected that species having a wide geographical distribution would be equally remarkable for their specific longevity; and that such as were confined to a limited area disappeared from the stage of existence in a comparatively short time. In the case under consideration, however, the facts do not harmonise with such conclusions: *Favorites Goldfussi*, found in the most widely separated parts of the earth, appears to have commenced and closed its existence within the limits of the *Devonian* period; whilst *Chonophyllum perfoliatum*, confined geographically, so far as is at present known, to the British area, was a member of the *Silurian* as well as of the *Devonian* fauna. The gigantic size of some of the corallums—*Favorites Goldfussi* has been found upwards of two feet in diameter—seems to imply that the climate of European latitudes was warmer, or at least that the winters were less cold, during the *Devonian* age than at present. The fossil coral reefs of Torquay and Ogwell suggest that the old *Devonian* sea was not at first of profound depth, that is if then as now the reef-building zoophytes did not labour at depths exceeding 25 fathoms; whilst the facts that some of the limestone sections reveal an aggregate thickness greatly exceeding this depth, and that every stratum in them is richly charged with fossil corals, seem explicable only on the supposition that the area was one of slow, gradual, and long continued subsidence.

The *Pterygotus*, the "seraphim" of the Scotch quarrymen, does not occur in Devonshire or Cornwall. With one exception only, the crustaceans are all trilobites. Amongst them that named *Trimerocephalus lævis* occurs under somewhat remarkable circumstances. So far as is at present known, it has been found only in one locality, namely, on the flanks of a hill called Knowles, near Newton Abbott, in South Devon, and no other fossil of any kind has been found there; on this point, our present knowledge is expressed by saying, "there is but one locality for the fossil, and but one fossil for the locality." Many hundreds of specimens of it have been found, and a very considerable number of them have passed through the hands of the speaker; he knew of but two instances in which the head was found attached to the thorax. On splitting a stone, and thereby disclosing one of these trilobites, excepting in the two cases just named, the head was not visible; or, what was very much more frequently the case, one half of the stone was found to contain the thorax and tail united, and the impression of the head; whilst in the other half were found the head and the impression of the body, and always in such a way as to show that the head had been severed from the body, removed a short distance from it, (as if drawn or pushed forward,) and inverted; there were never any indications of eyes, and not unfrequently the tail appeared somewhat truncated, as if its terminal margin were slightly folded under. It is clear that an inversion of the head might have been effected, either by a semi-rotation at right angles to the axis of the body, or in the direction of that axis; but as the anterior margin of the head was always found nearest the thorax, it is clear that the motion had been of the latter kind. The rock in which the fossil occurs has been pronounced by Mr. Sorby and others to be a volcanic ash; and this without reference to, or knowledge of, any speculations respecting the facts connected with the trilobites. Knowles hill, on the flanks of which they occur, is a mass of greenstone, and is so marked in the map published by the Geological Survey.

According to Burmeister it is probable "that these animals (trilobites) moved only by swimming; that they swam close beneath the surface in an inverted position, the belly upwards, the back downwards, and that they made use of their power of rolling themselves into a ball as a defence against attacks from above; that they lived gregariously in vast numbers, chiefly of one species."* The facts connected with this fossil, which have been described, seem capable of explanation by supposing that a shower of volcanic ashes, falling into the ancient *Devonian* sea in the Newton area, alarmed a shoal of these trilobites just then swimming by, and thereby caused them instinctively to roll themselves up for defence; that the continuation of the shower, and possibly the presence of noxious gases, killed the unfortunate crustaceans in the rolled-up posture; that their centre of gravity was so situated as to cause them all to sink to the bottom on their backs; that they were inhumed in the heap of ashes, which, by accumulating very rapidly and in great quantity, produced a pressure sufficient to flatten the body, and, with a few and very slight exceptions, the tail also, to dislocate the head (the line of union of the head and thorax being the line of least resistance) and, after the manner in which slaty cleavage in rocks is probably produced, to thrust the head some little distance in advance of the body. By such a process the head would be inverted, and in such a way that the severed parts of the creature would take the relative positions which have been described.

Though with the exception of a scale of holoptychius found, according to Professor Phillips, at Meadfoot, near Torquay, ichthyolites are not recorded as occurring in the *Devonian* rocks of Devon and Cornwall, it is nevertheless certain that fish did exist within the area, at the time under consideration. The speaker exhibited a fossil which he had found in the *Devonian* slates at Looe, in Cornwall, and which Sir Philip Egerton, and other eminent palæontologists, had pronounced to be an ichthyodorulite, or defence-spine of a fish.

In conclusion, Mr. Pengelly remarked, that the time to which he was limited had compelled him to be more hurried in his descriptions than he could have wished, to omit all mention of the echinoderms, and the various classes of shells, more or less abundant amongst the fossils he had been considering, and had only allowed him to strike certain veins of thought which it would have been desirable to have worked thoroughly.

[W. P.]

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* Burmeister's Trilobites, Ray Society, page 52.

WEEKLY EVENING MEETING,

Friday, June 1, 1860.

Sir Roderick I. Murchison, D.C.L. F.R.S. Vice-President, in the Chair.

John Tyndall, Esq. F.R.S.
PROFESSOR OF NATURAL PHILOSOPHY, ROYAL INSTITUTION.

Remarks on some Alpine Phenomena.

The discourse consisted of an account of a winter's expedition to the Alps, undertaken at the close of December 1859. The speaker remained two nights at the Montanvert; and determined with a theodolite the motion of the Mer-de-glace. This amounted to about one-half of the summer motion, and exhibited the results, as regards the quicker motion of the centre, established by measurements made in summer. He described the crystals of the snow which fell almost without intermission during the progress of the measurements. He afterwards visited the vault of the Arveiron, and found a turbid stream issuing from it, indicating that even in winter the motion of the glacier along its bed, by which the rocks over which it passes are ground, is not suspended even in winter.

The gorgeous crimson of the western heaven, as observed from the vault, gave occasion for some observations and experiments on the colour of the sky. The hypotheses of Newton and Goethe were referred to, as were also the memoirs of Clausius and Brücke. It was explained that the blue of the firmament is due to reflected light, and the morning and evening red to transmitted light. The possible action of particles suspended in the atmosphere was illustrated. A solution of mastic in alcohol recommended by Brücke was dropped into water, and the resin precipitated: light reflected by the liquid containing these particles appeared blue; while the transmitted light appeared yellow, and on increasing the precipitate deepened to orange, and became finally blood red. Professor Forbes has made the interesting observation that steam, at a certain stage of its condensation, is blue by reflected and red by transmitted, light.

The colour of milk, of the juices of many plants, and of the colour of a blue eye were also referred to as illustrative of the same action.

[J. T.]

GENERAL MONTHLY MEETING,

Monday, June 4, 1860.

Sir Roderick I. Murchison, D.C.L. F.R.S. Vice-President, in the Chair.

Charles Gibbes, Esq. and
Miss Sarah Gibbes

were duly *elected* Members of the Royal Institution.

Thomas R. Andrews, Esq. and
George B. Buckton, Esq. F.R.S.

were *admitted* Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same: viz.

From
Arts, Society of—Journal for May 1860. 8vo.
Astronomical Society, Royal—Proceedings, No. 6. 8vo. 1860.
Boosey, Messrs. (*the Publishers*)—The Musical World for May 1860. 4to.
Chemical Society—Journal, No. 49. 8vo. 1860.
Civil Engineers, Institute of—Proceedings, in May 1860. 8vo.
Editors—Artizan for May 1860. 4to.
Athenæum for May 1860. 4to.
Engineer for May 1860. fol.
Horological Journal, No. 22. 8vo. 1860.
Journal of Gas-Lighting for May 1860. 4to.
Mechanics' Magazine for May 1860. 8vo.
Medical Circular for May 1860. 8vo.
Practical Mechanics' Journal for May 1860. 4to.
Elliot, John L. Esq. M.R.I. (*the Author*)—A Few Words on the Reform Bill. 8vo. 1860.
Faraday, Professor, D.C.L. F.R.S.—
Imperial Academy of Sciences, Vienna:—
Statuten. 4to. 1859.
Almanach. 1859. 12mo.
Atlas der Hautkrankheiten: Lieferung III. fol. 1859.
Math. Nat. Classe—Denkschriften: Band XVII. 4to. 1859.
Sitzungsberichte. 1859. No. 10–28. 8vo. 1859–60.
Jahrbücher der K. K. Central-Anstalt zur Meteorologie und Erdmagnetismus. Band VI. 4to. 1859.
Franklin Institute of Pennsylvania—Journal, Vol. XXXIX. No. 4. 8vo. 1860.
Geological Society—Journal, No. 62. 8vo. 1860.
Glasgow Philosophical Society—Proceedings, Vol. III. Nos. 5, 6. 8vo. 1854–5.
Kerr, Mrs. Louisa Hay, M.R.I.—M. Koch, Beitrage zur Geschichte Tyrols. 8vo. 1850.
G. Zappert, Stab und Ruthe. 8vo. 1852.

Mackie, S. J. Esq. F.G.S. (the Editor)—The Geologist, May 1860.
Newton, Messrs.—London Journal (New Series), for May 1860. 8vo.
Novello, Mr. (the Publisher)—The Musical Times for May 1860. 4to.
Petermann, A. Esq. (the Editor)—Mittheilungen auf dem Gesammtgebiete der Geographie. 1860. Heft 5. 4to. Gotha, 1860.
Phillips, John, Esq. (the Author)—Address to the Geological Society, Feb. 17, 1860. 8vo. 1860.
Photographic Society—Journal, No. 97. 8vo. 1860.
Royal Society—Proceedings, No. 38. 8vo. 1860.
Transactions, Vol. CXLIX. Part 2. 4to. 1860.
Royal Society of Literature—Transactions, Vol. VI. Part 3. 8vo. 1859.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, Jan. und Feb. 1860. 4to.
Walker, A. De Noè, Esq. M.R.I.—A Chinese Chrestomathy in the Canton Dialect. By E. C. Bridgman. 4to. Macao. 1841.
Weale, John, Esq. (the Publisher)—Seven Volumes of his Educational Series (Classical Literature). 16to. 1859–60.
Yates, James, Esq. F.R.S. and the Rev. A. Barrett (the Authors)—Improvements in Arithmetic (by the Decimal System). 8vo. 1860.
Yorkshire Philosophical Society—Annual Report for 1859. 8vo. 1860.

WEEKLY EVENING MEETING,

Friday, June 8, 1860.

The Duke of Wellington, K.G. D.C.L. Vice-President, in the Chair.

Professor Faraday, D.C.L. F.R.S.

On the Electric Silk-Loom.

"Illustrans Commoda Vitæ,"* the motto of the Royal Institution, was made the ruling principle on this the last evening of the season; an account being given of the application by M. Bonelli of electricity, to the service of the figure weaving-loom. The astonishing condition of perfection to which M. Jacquard had brought the silk-loom, so that artists of the highest rank could not without minute inspection distinguish its results from the most perfect engraving, and the manner in which he taught the weaver to construct a series of cards, and then to use them automatically, so as to produce as often as he pleased the design which they represented, are well known. Any effect of pattern, either simple or complicated, which is produced in the woven fabric depends upon the manner in which the threads of the warp are separated before the weft is thrown, and the successive rearrangements of the warp threads which are brought about each time the shuttle is passed. A single thread of the weft therefore represents an element

* Lucretius, iii. 2.

of the design; and in the Jacquard loom, each of these required a card pierced in a certain order, which being brought against the ends of a set of horizontal rods, allowed some to remain undisturbed, whilst others were pushed on one side. By the action of the pedal the warp-threads associated with the undisturbed rods were raised and those belonging to the displaced rods were left unmoved; and to do this rightly, a separate pierced card was required for every thread that crossed the warp within the extent of the pattern. Frequently some thousands of cards are needed, and for the production of a woven portrait of M. Jacquard, in black and white silk, as many as 24,000 were employed.

After a design has been decided upon, it has to be converted into these cards, one for each thread of the weft included in the design; the preparation and piercing of them requires much care and time, after which they have to be linked together as an endless chain in their proper order. It is to replace this part of the weaving arrangements that Mr. Bonelli has applied his attention, and the peculiar power of electricity. Instead of the many pierced cards, he has but one card, or rather its equivalent, a convertible plate of brass; which being pierced with the full number of holes required (which in the loom in action was 400) can have these holes either stopped or left open so as to represent by its successive changes of condition the successive cards of the Jacquard series. To obtain this effect, tin foil is attached strongly to paper, so as to form a compound sheet. The design is then drawn upon the metallic surface with black bituminous varnish, and the sheet is made into an endless band, which being placed upon a roller, and kept in its position by stops, moves as the roller moves, being carried forward by its motion. A set of teeth rests upon the top of this roller, touching the pattern in a line; they are made of thin brass plate, so thin that 400 of them do not occupy more than 16 or 17 inches, *i.e.* the width of the design on the roller; yet so separate that each is insulated from its neighbour by little interposed teeth of ivory; and so large and therefore weighty as to fall and rest upon the pattern, making good electrical contact where the tin foil is exposed, but being insulated where the bituminous pattern intervenes.

Behind these teeth are 400 small electro-magnets fixed in a framework, parallel to each other, and insulated. The fine covered wires which constitute their helices are connected at one set of ends with the teeth just described, each with a tooth; whilst the other ends are brought together and made fast to one metallic plate and wire. Tracing this wire onwards, it comes to an interruptor or contact-maker from whence the metallic communication proceeds to a screw appointed to communicate with one end of a five-celled Bunsen battery, the other end of which communicates with a screw near the former. This screw has a wire proceeding from it to two insulated teeth, like the teeth bearing upon the pattern, but heavier; and these rest upon the uncovered edges of the tin foil at the sides of the pattern, so as to keep up a constant electric communication with it. By simple but perfect and secure mechanical arrangements, the following movements and results take place in this part of the apparatus. As the pedal descends under the weaver's foot at a certain time, the 400 teeth descend upon the pattern; then the circuit is completed at the interruptor in the single wire; the electric current passing through that wire is divided into as many portions as there are teeth touching the metal in the line of pattern under realization; it makes all the electro-magnets surrounded by these wires active, leaving the others non-magnetic; and then, as the foot is raised and the movements return in their course, the interruptor is first separated, which, causing all current to cease, the magnets lose their power, the teeth are raised from the pattern; and then the cylinder carrying it moves forward just so much as to give the new line of pattern for the teeth to search out electrically (the next time they descend) which corresponds to the next cast of the weft thread. Because the pattern never moves, whilst it is in contact with the teeth, it is not cut or worn by them: because the current is made by the interruptor after the teeth are in contact, and before they are separated, no fusion or burning of the metal occurs at the teeth; and because there is a tongue-like wiper or brush, which at the right time passes under the teeth, sustains them, and from off which they rub on to the pattern, there is never any want of cleanliness or of contact there.

Associated with these 400 magnets, and in the same line with them, are 400 cylinders of soft iron, called pistons; they are carried in a frame which moves to and fro horizontally between the magnets and the horizontal rods belonging to the suspensions of the warp threads; and they move towards the magnets at a time so adjusted as to coincide with the passage of the electricity round its circuit: they find therefore some of the magnets excited, because their teeth touch the metal of the pattern; and, as the box of pistons begins to return before the current is interrupted, such of the pistons as have touched excited magnets are retained or held back, whilst the others have returned in their course: the pistons therefore are divided into two intermixed groups, of which the one group is perhaps half an inch behind the other. Now comes in the action of the perforated brass plate, which is to be converted for the time into the equivalent of the particular Jacquard card required. It is a vertical plate, associated with the extremities of the pistons farthest from the electro-magnets: it can move up and down to a small extent: it is pierced by 400 circular holes. The 400 pistons have each a head or button, which can pass freely each through its correspondent hole when the plate is up, but is stopped at the hole when the plate is down, and then effectually closes it. Now the time is so adjusted, that when the box of pistons has moved so far forward as to cause separation of the two groups, the plate descends, and by locking such of the heads as belong to the unretained group, fills the correspondent holes, whilst the heads of the retained group, being already behind their holes, have left them open; and so the Jacquard plate is formed, and, moving a little further it acts on the horizontal rods before mentioned, and having by that arranged the suspenders of the warp threads, it then goes back, or towards the electro-magnets, to take up, under the influence of the currents of electricity through the selecting teeth, the new arrangement of apertures required for the next cast of the weft thread.

The use of electricity, for the purpose of reading off the design and conveying it into the loom involved many peculiarities, conditions, and difficulties. These were considered; and the manner in which they were either turned to advantage or overcome, was illustrated by large and separate experiments.

[M. F.]

GENERAL MONTHLY MEETING,

Monday, July 2, 1860.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

The Special Thanks of the Members were returned to His Grace the Duke of Wellington, K.G. Vice-President R.I. for his valuable present of an Electrotype from a mask taken from his late Father, F.M. the Duke of Wellington, after his decease.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same: viz.

From

Her Majesty's Government—Magnetical and Meteorological Observations at St. Helena. Vol. II. 1844–9. 4to. 1860.
The Secretary of State for India—Catalogue of Lepidopterous Insects in the East India House Museum. Vol. II. 8vo. 1858–9.
Commissioners in Lunacy—Fourteenth Report. 8vo. 1860.
Ansted, Professor D. T. (the Author)—Lecture on the Decay and Preservation of Building Materials (Stone). 1860.
Arts, Society of—Journal, June 1860. 8vo.
Asiatic Society of Bengal—Journal, No. 276. 8vo. 1860.
Astronomical Society, Royal—Proceedings, No. 7. 8vo. 1860.
Belgique, Académie Royale des Sciences—Bulletin des Sciences. 1859. 8vo. Annuaire. 1860. 16to.
Bengal, Director of Public Institution—Report for 1857–8. 8vo. 1859.
Boosey, Messrs. (the Publishers)—The Musical World for June 1860. 4to.
British Association for the Advancement of Science—Report of the Twenty-ninth Meeting, held at Aberdeen, September 1859. 8vo. 1860.
Chambers, G. F. Esq. M.R.I.—The Life of Baber, Emperor of Hindostan, by R. M. Caldecott. 8vo. 1844.
Druitt, Dr. (the Author)—Human Habitations in Relation to Health. 4to. 1860.

Editors—American Journal of Science, by B. Silliman, &c. for 1860. 8vo.
Artizan for June 1860. 4to.
Athenæum for June 1860. 4to.
Engineer for June 1860. fol.
Journal of Gas-Lighting for June 1860. 4to.
Mechanics' Magazine for June 1860. 8vo.
Medical Circular for June 1860. 8vo.
Practical Mechanic's Journal for June 1860. 4to.
Emmett, General A. M.R.I.—Army Regulations. 8vo. 1844.
Cormontaingne, Maréchal de, Œuvres Posthumes. 3 vols. 8vo. Paris, 1809.
D'Arçon, Michaud, on Fortifications. 8vo. 1815.
Mandar, C. F. Architecture des Forteresses. 8vo. Paris, 1801.
Rogniat, Baron J. Considérations sur l'Art de la Guerre. 8vo. Paris, 1816.
Journal du Siége de Bergopzoom en 1747. 16to. Amsterdam, 1750.
Rohan, H. De, Le Parfait Captaine (Abrégé des Guerres de Cesar); et l' Interest des Princes. 16to. Paris, 1641.
Montecuculi, R. De, Mémoires. 12mo. Strasbourg, 1740.
Dictionnaire Militaire. 3 vols. 12mo. Paris, 1745–6.
Blois, E. De, Traité des Bombardements. 8vo. Paris, 1848.
Griffiths, F. A., Artillerist's Manual. 16to. 1852.
Gilbert, A., Œuvres Militaires. 5 vols. 8vo. Paris, 1803.
Gay de Vernon, J., Traité de l'Art Militaire, à l'usage de l'Ecole Polytechnique. 2 vols. 4to. Paris, 1805.
Plans et Journaux des Siéges de la dernière Guerre de Flandres. 4to. 1750.
Mouzé, Major, Traité de Fortification Souterraine. 4to. Paris, 1804.
Simmons, F., On Heavy Ordnance, Hollow Shot, and Naval Gunnery, &c. 8vo. 1837–9.
Military Tracts. 2 vols. 8vo. (K. 84, 85.)
French Bulletins relating to the War in Russia. 1813.
Campagne de l'Armée Française en Portugal. 1815.
Giraud, P., Campagne de Paris en 1814. 1815.
Rogniat, Gen., Colonisation en Algérie et des Fortifications propres, &c. Paris. 8vo. 1840.
Instruction sur le Service du Génie en Campagne. 1825.
Poumet, M., Instructions sur les Effets des Bouches à feu. 1828.
French Instructions for the Defence of Fortresses in 1813; and of Open Towns in 1814; Translated. 1824.
Pasley, C. B., Method of Loading and Throwing Hand Grenades, 1821; System of Telegraphs, 1823; on Blasting under Water, 1836.
Faraday, Professor, D.C.L. F.R.S.—Monatsberichte der Akademie der Wissenschaften zu Berlin; Feb. März und April 1860. 8vo.
Franklin Institute of Pennsylvania—Journal, Vol. XXXIX. No. 5. 8vo. 1860.
Horticultural Society of London—Proceedings, Nos. 9–12. 8vo. 1860.
Linnean Society—Journal, No. 17. 8vo. 1860.
Mackie, S. J. Esq. F.G.S. (the Editor)—The Geologist, June 1860.
Manchester Literary and Philosophical Society—Memoirs (Second Series), Vol. XV. Part 2. 8vo. 1860.
Proceedings, 1858–9. 8vo.
Marcet, W. M.D. F.R.S. (the Author)—Experimental Inquiry into the Action of Alcohol on the Nervous System. 8vo. 1860.
Mendicity Society—Report, 1860. 8vo.
New Granada Society of Naturalists—Boletin, 8vo. Bogotá, 1860.
Newton, Messrs.—London Journal (New Series), for June 1860. 8vo.
Novello, Mr. (the Publisher)—The Musical Times for June 1860. 4to
Petermann, A. Esq. (the Editor)—Mittheilungen auf dem Gesammtgebiete der Geographie. 1860. Heft 6. 4to. Gotha, 1860.
Photographic Society—Journal, No. 98. 8vo. 1860.
Roma, Accademia Pontificia de' Nuovi Lincei—Atti, Anno XI. Sess. 7; Anno XII. Sess. 1, 2, 3. 4to. 1858–9.
Statistical Society—Journal, Vol. XXIII. Part 2. 8vo. 1860.
Volpicelli, Professor P. (the Author)—Sulla Polarità Elettrostatica. Roma, 4to. 1859.

Royal Institution of Great Britain.

1860.

WEEKLY EVENING MEETING,

Friday, May 18, 1860.

Charles Wheatstone, Esq. F.R.S. Vice-President, in the Chair.

Professor William Thomson, F.R.S.

On Atmospheric Electricity.

Stephen Gray, a pensioner of the Charter-house, after many years of enthusiastic and persevering devotion to electric science, closed his philosophical labours, about 130 years ago, with the following remarkable conjecture: "That there may be found a way to collect a greater quantity of the electrical fire, and consequently to increase the force of that power, which by several of these experiments, *si licet magna componere parvis*, seems to be of the same nature with that of thunder and lightning."

The inventions of the electrical machine and the Leyden phial immediately fulfilled these expectations as to collecting greater quantities of electric fire; and the surprise and delight which they elicited by their mimic lightnings and thunders, and above all by the terrible electric shock, had scarcely subsided when Franklin sent his kite messenger to the clouds, and demonstrated that the imagination had been a true guide to this great scientific discovery—the identity of the natural agent in the thunderstorm with the mysterious influence produced by the simple operation of rubbing a piece of amber, which two thousand years before had attracted the attention of those philosophers among the ancients who did not despise the small things of nature.

The investigation of atmospheric electricity immediately became a very popular branch of natural science; and the discovery of remarkable and most interesting phenomena quickly rewarded its cultivators. The foundation of all we now know was completed by Beccaria, in his observations on "the mild electricity of serene weather," nearly a hundred years ago. It was not until comparatively recent years that definite quantitative comparisons from time to time of the electric quality manifested by the atmosphere in one locality were first obtained by the application of Peltier's mode of observation with his metrical electroscope. The much more accurate electrometer, and the greatly improved mode of observation invented by Delmann, have given for the electric intensity, at any instant, still more precise results; but have left something to desire in point of simplicity and convenience for general use, and have not afforded any means for continuous observation, or for the introduction of self-recording apparatus. The speaker had attempted to supply some of these wants, and he explained the construction and use of instruments which he had planned for this purpose, which were now exhibited to the meeting.

Apparatus for the observation of atmospheric electricity has essentially two functions to perform; to electrify a body with some of the natural electricity or with electricity produced by its influence; and to measure the electrification thus obtained.

The measuring apparatus exhibited consisted of three electrometers, which were referred to under the designations of (I.) The divided ring reflecting electrometer. (II.) The common house electrometer; and (III.) The portable electrometer.

(I.) The divided ring reflecting electrometer consists of:

(1) A ring of metal divided into two equal parts of which one is insulated, and the other connected with the metal case (5) of the instrument.

(2) A very light needle of sheet aluminium hung by a fine glass fibre, and counterpoised so as to make it project only to one side of this axis of suspension.

(3) A Leyden phial, consisting of an open glass jar, coated outside and inside in the usual manner, with the exception that the tinfoil of the inner coating does not extend to the bottom of the jar, which is occupied instead by a small quantity of sulphuric acid.

(4) A stiff straight wire rigidly attached to the aluminium needle, as nearly as may be in the line of the suspending fibre, bearing a light platinum wire linked to its lower end, and hanging down so as to dip into the sulphuric acid.

(5) A case protecting the needle from currents of air, and from irregular electric actions, and maintaining an artificially dried atmosphere round the glass pillar or pillars supporting the insulated half ring and the uncoated portion of the glass of the phial.

(6) A light stiff metallic electrode projecting from the insulated half ring through the middle of a small aperture in the metal case, to the outside.

(7) A wide metal tube of somewhat less diameter than the Leyden jar, attached to a metal ring borne by its inside coating, and standing up vertically to a few inches above the level of the mouth of the jar.

(8) A stiff wire projecting horizontally from this metal tube above the edge of the Leyden jar, and out through a wide hole in the case of the instrument to a convenient position for applying electricity to charge the jar with.

(9) A very light glass mirror, about three-quarters of an inch diameter, attached by its back to the wire (4), and therefore rigidly connected with the aluminium needle.

(10) A circular aperture in the case shut by a convex lens, and a long horizontal slit shut by plate glass, with its centre immediately above or below that of the lens, one of them above, and the other equally below the level of the centre of the mirror.

(11) A large aperture in the wide metal tube (7), on a level with the mirror (9), to allow light from a lamp outside the case, entering through the lens, to fall upon the mirror, and be reflected out through the plate glass window; and three or four fine metal wires stretched across this aperture to screen the mirror from irregular electric influences, without sensibly diminishing the amount of light falling on and reflected off it.

The divided ring (1) is cut out of thick strong sheet metal (generally brass). Its outer diameter is about 4 inches, its inner diameter $2\frac{1}{4}$; and it is divided into two equal parts by cutting it along a diameter with a saw. The two halves are fixed horizontally; one of them on a firm metal support, and the other on glass, so as to retain as nearly as may be their original relative position, with just the saw cut, from $\frac{1}{10}$ to $\frac{1}{20}$ of an inch broad, vacant between them. They are placed with their common centre as nearly as may be in the axis of the case (5), which is cylindrical, and placed vertically. The Leyden jar (3), and the tube (7), carried by its inside coating, have their common axis fixed to coincide as nearly as may be with that of the case and divided ring. The glass fibre hangs down from above in the direction of this axis, and supports the needle about an inch above the level of the divided ring. The stiff wire (4), attached to the needle, hangs down as nearly as may be along the axis of the tube (7).

Before using the instrument, the Leyden phial (3) is charged by means of its projecting electrode (8). When an electrical machine is not available, this is very easily done by the aid of a stick of vulcanite, rubbed by a piece of chamois leather. The potential of the charge thus communicated to the phial, is to be kept as nearly constant as is required for the accuracy of the investigation for which the instrument is used. Two or three rubs of the stick of vulcanite once a day, or twice a day, are sufficient when the phial is of good glass, well kept dry. The most convenient test for the charge of the phial is a proper electrometer or electroscope, of any convenient kind, kept constantly in communication with the charging electrode (8).

The electrometer (II.) is to be ordinarily used for that purpose in the Kew apparatus. Failing any such gauge electrometer or electroscope, a zinc-copper-water battery of ten, twenty, or more small cells, may be very conveniently used to test directly the sensibility of the reflecting electrometer, which is to be brought to its proper degree by charging its Leyden phial as much as is required.

In the use of this electrometer, the two bodies of which the difference of potentials is to be tested are connected, one of them, which is generally the earth, with the metal case of the instrument, and the other with the insulated half ring. The needle being, let us suppose, negatively electrified, will move towards or from the insulated half ring, according as the potential of the conductor connected with this half ring differs positively or negatively from that of the other conductor (earth) connected with the case. The mirror turns accordingly in one direction or the other through a small angle from its zero position, and produces a corresponding motion in the image of the lamp on the screen on which it is thrown.

II. The common house electrometer.—This instrument consists of:

(1) A thin flint-glass bell, coated outside and inside like a Leyden phial, with the exception of the bottom inside, which contains a little sulphuric acid.

(2) A cylindrical metal case, enclosing the glass jar, cemented to it round its mouth outside, extending upwards about an inch and a half above the mouth, and downwards to a metal base supporting the whole instrument, and protecting the glass against the danger of breakage.

(3) A cover of plate glass, with a metal rim, closing the top of the cylindrical case of the instrument.

(4) A torsion head, after the manner of Coulomb's balance, supported in the centre of the glass cover, and bearing a glass fibre which hangs down through an aperture in its centre.

(5) A light aluminium needle attached across the lower end of the fibre (which is somewhat above the centre of the glass bell), and a stiff platinum wire attached to it at right angles, and hanging down to near the bottom of the jar.

(6) A very light platinum wire, long enough to hang within one-eighth of an inch or so of the bottom of the jar, and to dip in the sulphuric acid.

(7) A metal ring, attached to the inner coating of the jar, bearing two plates in proper positions for repelling the two ends of the aluminium needle when similarly electrified, and proper stops to limit the angular motion of the needle to within about 45° from these plates.

(8) A cage of fine brass wire, stretched on brass framework, supported from the main case above by two glass pillars, and partially enclosing the two ends of the needle, and the repelling plates, from all of which it is separated by clear spaces, of nowhere less than one-fourth of an inch of air.

(9) A charging electrode, attached to the ring (7), and projecting over the mouth of the jar to the outside of the metal case (2), through a wide aperture, which is commonly kept closed by a metal cap, leaving at least one-quarter of an inch of air round the projecting end of the electrode.

(10) An electrode attached to the cage (8), and projecting over the mouth of the jar to the outside of the metal case (2), through the centre of an aperture, about a quarter of an inch diameter.

This instrument is adapted to measure differences of potential between two conducting systems, namely, as one, the aluminium needle (5), the repelling plates (7), and the inner coating of the jar, and, as the other, the insulated cage (8). This latter is commonly connected by means of its projecting electrode (10), with the conductor to be tested. The two conducting systems, if through their projecting electrodes connected by a metallic wire, may be electrified to any degree, without causing the slightest sensible motion in the needle. If, on the other hand, the two electrodes of these two systems are connected with two conductors, electrified to different potentials, the needle moves away from the repelling plates; and if by turning the torsion head it is brought back to one accurately marked position, the number of degrees of torsion required is proportional to the square of the difference of potentials thus tested.

In the ordinary use of the instrument, the inner coating of the Leyden jar is charged negatively, by an external application of electricity through its projecting electrode (9). The degree of the charge thus communicated, is determined by putting the cage in connexion with the earth through its electrode (10), and bringing the needle by torsion to its marked position. The square root of the number of degrees of torsion required to effect this, measures the potential of the Leyden charge. This result is called the reduced earth reading. When the atmosphere inside the jar is kept sufficiently dry,—this charge is retained from day to day with little loss; not more, often, than 1 per cent. in the 24 hours.

In using the instrument the charging electrode (9) of the jar is left untouched, with the aperture through which it projects closed over it by the metal cap referred to above. The electrode (10) of the cage, when an observation is to be made, is connected with the conductor to be tested, and the needle is brought by torsion to its marked position. The square root of the number of degrees of torsion now required measures the difference of potentials between the conductor tested and the interior coating of the Leyden jar. The excess, positive or negative, of this result above the reduced earth reading, measures the excess of the potential, positive or negative, of the conductor tested above that of the earth; or simply the potential of the conductor tested, if we regard that of the earth as zero.

III. The portable electrometer is constructed on the same electrical principles as the house electrometer just described. The mode of suspension of the needle is however essentially different; and a varied plan of connection between the different electrical parts has been consequently adopted as more convenient. In the portable electrometer, the needle is firmly attached at right angles to the middle of a fine platinum wire, tightly stretched in the axis of a brass tube with apertures in its middle to allow the needle to project on the two sides. One end of the platinum wire is rigidly connected with this tube; the other is attached to a graduated torsion head. The brass tube carries two metal plates in suitable positions to repel the two ends of the needle in contrary directions, and metal stops to limit its angular motion within a convenient range. The conducting system composed of these different parts is supported from the metal cover or roof of the jar, by three glass stems. The torsion head is carried round by means of a stout glass bar, projecting down from a pinion centered on the lower side of this cover, and turned by the action of a tangent screw presenting a milled head, to the hand of the operator outside. The conducting system thus borne by insulating supports is connected with the outside conductor to be tested by means of an electrode passing out through the centre of the top of the case by a wide aperture in the centre of the pinion. A wire cage, surrounding the central part of the tube and the needle and repelling plates, is rigidly attached to the interior coating of the Leyden jar. It carries two metal sectors, or "bulkheads," in suitable positions to attract the two ends of the needle, which however is prevented from touching them by the limiting stops referred to above. The effect of these attracting plates, as they will be called, is to increase very much the sensibility of the instrument. The square root of the number of degrees of torsion required to bring the needle to a sighted position near the repelling plates, measures the difference of potentials between the cage and the conducting system, consisting of tube torsion-head repelling plates and needle. The metal roof of the jar is attached to a strong metal case, cemented round the outside of the top of the jar, and enclosing it all round and below, to protect it from breakage when being carried about. There are sufficient apertures in this case, opened by means of a sliding piece, to allow the observer to see the needle and graduated circle (torsion head), when using the instrument. On the outside of the roof of the jar a stout glass stem is attached, which supports a light stiff metallic conductor, by means of which a burning match is supported, at the height of two or three feet above the observer. This conductor is connected by means of a fine wire with the electrometer, in the manner described above, through the centre of the aperture in the roof. An artificially dried atmosphere is maintained around this glass stem, by means of a metal case surrounding it, and containing receptacles of gutta percha, or lead, holding suitably shaped pieces of pumice-stone moistened with sulphuric acid. The conductor which bears the match projects upwards through the centre of a sufficiently wide aperture, and bears a small umbrella, which both stops rain from falling into this aperture, and diminishes the circulation of air, owing to wind blowing round the instrument, from taking place to so great a degree as to do away with the dryness of the interior atmosphere required to allow the glass stem to insulate sufficiently. The instrument may be held by the observer in his hand in the open air without the assistance of any fixed stand. A sling attached to the instrument and passing over his left shoulder, much facilitates operations, and renders it easy to carry the apparatus to the place of observation, even if up a rugged hill side, with little risk of accident.

The burning match in the apparatus which has just been described, performs the collecting function referred to above. The collector employed for the station apparatus, whether the reflecting electrometer or the common house electrometer is used, is an insulated vessel of water, allowed to flow out in a fine stream through a small aperture at the end of a pipe projecting to a distance of several feet from the wall of the building in which the observations are made.

The principle of collecting, whether by fire or by water, in the observation of atmospheric electricity, was explained by the speaker thus:—The earth's surface is, except at instants, always found electrified, in general negatively, but sometimes positively.

"If a large sheet of metal were laid on the earth in a perfectly level district, and if a circular area of the same metal were laid upon this sheet, and after the manner of Coulomb's proof plane, were lifted by an insulated handle and removed to an electrometer within doors, a measure of the earth's electrification, at the time, would be obtained; or, if a ball, placed on the top of a conducting rod in the open air, were lifted from that position by an insulating support, and carried to an electrometer within doors, we should also have, on precisely the same principle, a measure of the earth's electrification at the time. If the height of the ball in this second plan were equal to one-sixteenth of the circumference of the disc used in the first plan, the electrometric indications would be the same, provided the diameter of the ball is small, in comparison with the height to which it is raised in the air, and the electrostatic capacity of the electrometer is small enough not to take any considerable proportion of the electricity from the ball in its application. The idea of experimenting by means of a disc laid flat on the earth, is merely suggested for the sake of illustration, and would obviously be most inconvenient in practice. On the other hand, the method by a carrier ball, instead of a proof plane, is precisely the method by which, on a small scale, Faraday investigated the distribution of electricity induced on the earth's surface, by a piece of rubbed shellac; and the same method, applied on a suitable scale for testing the natural electrification of the earth in the open air, has given in the hands of Delmann, of Creuznach, the most accurate results hitherto published in the way of electro-meteorological observation. If, now, we conceive an elevated conductor first belonging to the earth, to become insulated and to be made to throw off and to continue throwing off portions from an exposed position of its own surface, this part of its surface will quickly be reduced to a state of no electrification, and the whole conductor will be brought to such a potential as will allow it to remain in electrical equilibrium in the air with that portion of its surface neutral. In other words, the potential throughout the insulated conductor is brought to be the same as that of the particular equi-potential surface in the air, which passes through the point of it from which matter breaks away. A flame, or the heated gas passing from a burning match, does precisely this: the flame itself, or the highly heated gas close to the match, being a conductor which is constantly extending out and gradually becoming a non-conductor. The drops into which the jet issuing from the insulated conductor breaks, on the plan introduced by the writer, produce the same effects with more pointed decision, and with more of dynamical energy to remove the rejected matter with the electricity which it carries from the neighbourhood of the fixed conductor."—*Nichol's Cyclopædia, 2nd edition, article "Electricity, Atmospheric."*

After having given so much of these explanations as seemed necessary to convey a general idea of the principles on which the construction of the instruments of investigation depended, the speaker proceeded to call attention to the special subject proposed for consideration this evening.

What is terrestrial atmospheric electricity? Is it electricity of earth, or electricity of air, or electricity of watery or other particles in the air? An endeavour to answer these questions was all that was offered; abstinence from speculation as to the origin of this electric condition of our atmosphere, and its physical relations with earth, air, and water, having been painfully learned by repeated and varied failure in every attempt to see beyond facts of observation. In serene weather, the earth's surface is generally, in most localities hitherto examined, found negatively or resinously electrified; and when this fact alone is known it might be supposed that the globe is merely electrified as a whole with a resinous charge, and left insulated in space.

But it is to be remarked, that the earth, although insulated in its atmospheric envelope, being in fact a conductor touched only by air, one of the best although not the strongest of insulators, cannot with its atmosphere be supposed to be insulated, so as to hold an electric charge in interplanetary space. It has been supposed, indeed, that outside the earth's recognised atmosphere there exists something or nothing in space which constitutes a perfect insulator; but this supposition seems to have no other foundation than a strange idea that electric conductivity is a strength or a power of matter rather than a mere *non-resistance*. In reality we know that air highly rarefied by the air-pump, or by other processes, as in the construction of the "vacuum tubes," by which such admirable phenomena of electric light have recently been seen in this place, becomes extremely weak in its resistance to the transference of electricity through it, and begins to appear rather as a conductor than an insulator. One hundred miles or upwards from the earth's surface, the air in space cannot in all probability have resisting power enough to bear any such electric forces, as those which we generally find even in serene weather in the lower strata. Hence, we cannot, with Peltier, regard the earth as a resinously charged conductor, insulated in space, and subject only to accidental influences from temporary electric deposits in clouds, or air round it; but we must suppose that there is always *essentially* in the higher aërial regions a distribution arising from the self-relief of the outer highly rarefied air by disruptive discharge. This electric stratum must constitute very nearly the electro-polar complement to all the electricity that exists on the earth's surface, and in the lower strata of the atmosphere; in other words, the total quantity of electricity, reckoned as excess of positive above negative, or of negative above positive, in any large portion of the atmosphere, and on the portion of the earth's surface below it, must be very nearly zero. The quality of non-resistance to electric force of the thin interplanetary air being duly considered, we might regard the earth, its atmosphere, and the surrounding medium as constituting respectively the inner coating, the di-electric (as it were glass), and the outer coating of a great Leyden phial, charged negatively; and even if we were to neglect the consideration of possible deposits of electricity through the body of the di-electric itself, we should arrive at a correct view of the electric indications discoverable at any one time and place of the earth's surface. In fact, any kind of "collector," or plan for collecting electricity from or in virtue of the natural "terrestrial atmospheric electricity," gives an effect simply proportional to the electrification of the earth's surface then and there. The methods of collecting by fire and water which the speaker exhibited, gave definitively, in the language of the mathematical theory, the "electric potential" of the air at the point occupied by the burning end of the match or by the portion of the stream of water where it breaks into drops. If the apparatus is used in an open plane, and care be taken to eliminate all disturbance due to the presence of the electrometer itself and of the observer above the ground, the indicated effect, if expressed in absolute electrostatic measure, and divided by the height of the point tested above the ground, has only to be, (according to an old theorem of Coulomb's, corrected by Laplace,) divided by four times the ratio of the circumference of a circle to its diameter, to reduce it to an expression of the number of units in absolute electrostatic measure of the electricity per unit of area of the earth's surface at the time and place. The mathematical theory does away with every difficulty in explaining the various and seemingly irreconcilable views which different writers have expressed, and explanations which different observers have given of the functions of their testing apparatus. In the present state of electric science, the most convenient and generally intelligible way to state the result of an observation of terrestrial atmospheric-electricity, in absolute measure, is in terms of the number of elements of a constant galvanic battery, required to produce the same difference of potentials as exists between the earth and a point in the air at a stated height above an open level plane of ground. Observations with the portable electrometer had given in ordinary fair weather, in the island of Arran, on a flat open sea beach, readings varying from 200 to 400 Daniel's elements, as the difference of potentials between the earth and the match, at a height of 9 feet above it. Hence, the intensity of electric force perpendicular to the earth's surface, must have amounted to from 22 to 44 Daniel's elements, per foot of air. In fair weather, with breezes from the east or north-east, he had often found from 6 to 10 times the higher of these intensities.

Even in fair weather, the intensity of the electric force in the air near the earth's surface is perpetually fluctuating. The speaker had often observed it, especially during calms or very light breezes from the east, varying from 40 Daniel's elements per foot, to three or four times that amount during a few minutes; and returning again as rapidly to the lower amount. More frequently he had observed variations from about 30 to about 40, and back again, recurring in uncertain periods of perhaps about two minutes. These gradual variations cannot but be produced by electrified masses of air or cloud, floating by the locality of observation. Again, it is well known that during storms of rain, hail, or snow, there are great and sometimes sudden variations of electric force in the air close to the earth. These are undoubtedly produced, partly as those of fair weather, by motions of electrified masses of air and cloud; partly by the fall of vitreously or resinously electrified rain, leaving a corresponding deficiency in the air or cloud from which it falls; and partly by disruptive discharges (flashes of lightning) between masses of air or cloud, or between either and the earth. The consideration of these various phenomena suggested the following questions, and modes of observation for answering them.

Question 1. How is electricity distributed through the different strata of the atmosphere to a height of five or six miles above the earth's surface in ordinary fair weather? To be answered by electrical observations in balloons at all heights up to the highest limit, and simultaneous observations at the earth's surface.

Q. 2. Does electrification of air close to the earth's surface, or within a few hundred feet of it, sensibly influence the observed electric force? and if so, how does it vary with the weather, and with the time of day or year? The first part of this question has been answered very decidedly in the affirmative, first, for large masses of air within a few hundred yards of the earth's surface, by means of observations made simultaneously at a station near the seashore in the island of Arran, and at one or other of several stations at different distances, within six miles of it, on the sides and summit of Goatfell. After that it was found, by simultaneous observations made at a window in the Natural Philosophy Lecture Room, and on the College Tower of the University of Glasgow, that the influence of the air within 100 feet of the earth's surface was always sensible at both stations, and often paramount at the lower. Thus, for example, when in broken weather, the superficial electrification of the outside of the lecture room, about 20 feet above the ground, in a quadrangle of buildings, was found positive, the superficial electrification of the sides of the tower, about 70 feet higher, was often found negative or nearly zero; and this sometimes even when the positive electrification of the sides of the building at the lower station equalled in amount an ordinary fair weather negative. This state of things could only exist in virtue of a negative electrification of the circumambient air, inducing a positive electrification on the ground and sides of the quadrangle, but not sufficient to counterbalance the influence on the higher parts of the tower of more distant positively electrified aerial masses.

A long continuation of such systems of simultaneous observation—not in a town only, but in various situations of flat and of mountainous country, on the sea coast as well as far inland, in various regions of the world—will be required to obtain the information asked for in the second part of this question.

Q. 3. Do the particles of rain, hail, and snow in falling through the air possess absolute charges of electricity? and if so, whether positive or negative, and of what amounts in different conditions as to place and weather? Attempts to answer this question have been made by various observers, but as yet without success; as for instance by an "electro-pluviometer," tried at Kew many years ago. By using a sufficiently well insulated vessel to collect the falling particles, it is quite certain that a decided answer may be obtained with ease for the cases of hail and snow. Inductive effects produced by drops splashing away from the collecting vessel, if exposed to the electric force of the air in an open position, or inductive effects of the opposite kind produced by drops splashing away from surrounding walls or screens and falling into the collecting vessel, if not in an exposed position, make it less easy to ascertain the electrical quality of rain; but, by taking means to obviate the disturbing effects of these influences, the speaker hoped to arrive at definite results.

It would have been more satisfactory to have been able to conclude a discourse on atmospheric electricity otherwise than in questions, but no other form of conclusion would have been at all consistent with the present state of knowledge.

The discourse was illustrated by the use of the mirror electrometer reflecting a beam of light from the electric lamp, and throwing it on a white screen, where its motions were measured by a divided scale. The principle of the water-dropping collector was illustrated by allowing a jet of water to flow by a fine nozzle into the middle of the lecture-room, from an uninsulated metal vessel of water and compressed air, and collecting the drops in an insulated vessel on the floor. This vessel was connected with the testing electrode of the reflecting electrometer; and it was then found to experience a continually increasing negative electrification, when fixed positively electrified bodies were in the neighbourhood of the nozzle. If the same experiment were made in ordinary fair weather in the open air, instead of under the roof and within the walls of the lecture-room, the same result would be observed, without the presence of any artificially electrified body. The vessel from which the water was discharged was next insulated; and other circumstances remaining unvaried, it was shown that this vessel became rapidly electrified to a certain degree of positive potential, and the falling drops ceased to communicate any more electricity to the vessel in which they were gathered.

The influence of electrified masses of air was illustrated by carrying about the portable electrometer, with its match burning, to different parts of the lecture-room, while insulated spirit lamps connected with the positive and negative conductor of an electrical machine, burned on the two sides. The speaker observed the indications on the portable electrometer; but the potentials thus measured, were seen by the audience marked on the scale by the spot of light; the reflecting electrometer being kept connected with the portable electrometer in all its positions, by means of a long fine wire. It was found that when the burning match was on one side of a certain surface dividing the air of the lecture-room, the potential indicated was positive, and on the other side negative.

The water-dropping collector constructed for the self-registering apparatus to be used at Kew, had been previously set upon the roof of the Royal Institution, and an insulated wire (Beccaria's "Deferent Wire") led down to the reflecting electrometer on the lecture-room table. The electric force in the air above the roof was thus tested several times during the meeting; and it was at first found to be, as it had been during several days preceding, somewhat feeble positive (corresponding to a feeble negative electrification of the earth's surface, or rather housetops, in the neighbourhood). This was a not unfrequent electrical condition of days, such as these had been of dull rain, with occasional intervals of heavier rain and of cessation. The natural electricity was again observed by means of the reflecting electrometer during several minutes near the end of the discourse; and was found instead of the weak positive which had been previously observed, to be strong positive of three or four times the amount. Upon this the speaker quoted* an answer, which Prior Ceca had given to a question Beccaria had put to him "concerning the state of electricity when the weather clears up." "'If, when the rain has ceased (the prior said to me) a strong excessive† electricity obtains, it is a sign that the weather will continue fair for several days; if the electricity is but small, it is a sign that such weather will not last so much as that whole day, and that it will soon be cloudy again, or even will again rain.'" The climate of this country is very different from that of Piedmont, where Beccaria and his friend made their observations, but their rule as to the "electricity of clearing weather," has been found frequently confirmed by the speaker. He therefore considered, that although it was still raining at the commencement of the meeting, the electrical indications they had seen gave fair promise‡ for the remainder of this evening, if not for a longer period. There can be no doubt but that electric indications, when sufficiently studied, will be found important additions to our means for prognosticating the weather;

* From Beccaria's first letter "On Terrestrial Atmospheric Electricity during Serene Weather."—*Garzegna di Mondovi, May* 16, 1775.

† *i.e.* vitreous, or positive.

‡ At the conclusion of the meeting it was found that the rain had actually ceased. The weather continued fair during the remainder of the night, and three or four of the finest days of the season followed.

and the speaker hoped soon to see the atmospheric electrometer generally adopted as a useful and convenient weather-glass.

The speaker could not conclude without guarding himself against any imputation of having assumed the existence of two electric fluids or substances, because he had frequently spoken of the vitreous and resinous electricities. Dufay's very important discovery of two modes or qualities of electrification, led his followers too readily to admit his supposition of two distinct electric fluids. Franklin, Æpinus, and Cavendish, with a hypothesis of one electric fluid, opened the way for a juster appreciation of the *unity* of nature in electric phenomena. Beccaria, with his "electric atmospheres," somewhat vaguely struggled to see deeper into the working of electric force, but his views found little acceptance, and scarcely suggested inquiry or even meditation. The 18th century made a school of science for itself in which, for the not unnatural dogma of the earlier schoolmen "matter cannot act where it is not," was substituted the most fantastic of paradoxes, *contact does not exist*. Boscovich's theory was the consummation of the 18th century school of physical science. This strange idea took deep root, and from it grew up a barren tree, exhausting the soil and overshadowing the whole field of molecular investigation, on which so much unavailing labour was spent by the great mathematicians of the early part of our 19th century. If Boscovich's theory no longer cumbers the ground, it is because one true philosopher required more light for tracing lines of electric force.

Mr. Faraday's investigation of electrostatic induction influences now every department of physical speculation, and constitutes an era in science. If we can no longer regard electric and magnetic fluids attracting or repelling at a distance as realities, we may now also contemplate as a thing of the past that belief in atoms and in vacuum, against which Leibnitz so earnestly contended in his memorable correspondence with Dr. Samuel Clarke.

We now look on space as full. We know that light is propagated, like sound, through pressure and motion. We know that there is no substance of caloric—that inscrutably minute motions cause the expansion which the thermometer marks, and stimulate our sensation of heat—that fire is not laid up in coal more than in this Leyden phial, or this weight: there is potential fire in each. If electric force depends on a residual *surface action*, a resultant of an inner tension experienced by the insulating medium, we can conceive that electricity itself is to be understood as not an accident, but an essence of matter. Whatever electricity is, it seems quite certain that electricity in motion is *heat;* and that a certain alignment of axes of revolution in this motion is *magnetism*. Faraday's magneto-optic experiment makes this not a hypothesis, but a demonstrated conclusion. Thus a rifle bullet keeps its point foremost; Foucault's gyroscope finds the earth's axis of palpable rotation; and the magnetic needle shows that more subtle rotatory movement in matter of the earth, which we call terrestrial magnetism, all by one and the same dynamical action.

It is often asked, are we to fall back on facts and phenomena, and give up all idea of penetrating that mystery which hangs round the ultimate nature of matter? This is a question that must be answered by the metaphysician, and it does not belong to the domain of Natural Philosophy. But it does seem that the marvellous train of discovery, unparalleled in the history of experimental science, which the last years of the world has seen to emanate from experiments within these walls, must lead to a stage of knowledge, in which laws of inorganic nature will be understood in this sense—that one will be known as essentially connected with all, and in which unity of plan through an inexhaustibly varied execution, will be recognized as a universally manifested result of creative wisdom.

[W. T.]

GENERAL MONTHLY MEETING,

Monday, November 5, 1860.

Sir Roderick I. Murchison, F.R.S. and Vice-President, in the Chair.

Carl Haag, Esq.

was *elected* a Member of the Royal Institution.

The resignation (on account of ill health) of the Rev. John Barlow, M.A. F.R.S. for nearly eighteen years the Honorary Secretary, was announced from the Chair, and received with deep regret by all the Members present.

The Special Thanks of the Members were returned to M. Rouland, the French Minister of Public Instruction, for his Present, on behalf of the French Government, of the following Works:—

Documents inedits sur L'Histoire de France.

Rapports au Roi et au Ministre. 4to. 1835–39.
Instructions du Comité Historique des Arts et Monuments: Architecture et Musique. 1837–49.
De Wailly, N. Eléments de Paléographie. 2 vols. 4to. 1838.
Chronique des Ducs de Normandie, par Bénoit. Ed. F. Michel. 3 vols. 4to. 1836–44.
Chronique du Bertrand du Guesclin, par Cuvelier, Trouvère du XIV^e Siècle. Ed. E. Charrière. 2 vols. 4to. 1839.
Captivité de François I. Ed. A. Champollion-Figeac. 4to. 1847.
Journal des Etats-Généraux de France tenus à Tours en 1484. Ed. A. Bernier. 4to. 1835.
Procès-Verbaux des Etats-Généraux de 1593. 4to. 1842.

Relations des Ambassadeurs Vénitiens sur les Affaires de France au XIV[e] Siècle. Ed. N. Tommaseo. 2 vols. 4to. 1838.
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GENERAL MONTHLY MEETING,

Monday, December 3, 1860.

SIR CHARLES HAMILTON, Bart. C.B. in the Chair.

The Rev. Alexander Denny, M.A. and
Edward Snaith, Esq.

were *elected* Members of the Royal Institution.

Carl Haag, Esq.

was *admitted* a Member of the Royal Institution.

HENRY BENCE JONES, M.D. M.A. F.R.S. was elected Secretary of the Royal Institution, in the room of the Rev. JOHN BARLOW, M.A. F.R.S. resigned, who was elected a Manager.

The following Arrangements for the Lectures before Easter 1861, were announced :—

Six Lectures on the CHEMICAL HISTORY OF A CANDLE (adapted to a Juvenile Auditory)—by MICHAEL FARADAY, Esq. D.C.L. F.R.S. &c. Fullerian Professor of Chemistry, R.I.

Twelve Lectures on FISHES—by RICHARD OWEN, Esq. D.C.L. F.R.S. Fullerian Professor of Natural Physiology, R.I.

Twelve Lectures on ELECTRICITY—by JOHN TYNDALL, Esq. F.R.S. Professor of Natural Philosophy, R.I.

Ten Lectures on INORGANIC CHEMISTRY—by Dr. EDWARD FRANKLAND, Esq. F.R.S. Lecturer on Chemistry at St. Bartholomew's Hospital.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same: viz.

FROM

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Mémoires par Divers Savans. Tome IX. 4to. 1859.

Mémoires des Sciences Politiques, &c. Tome IX. 4to. 1859.
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Bulletins: Tome II. Nos. 1–3. 4to. 1860.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, Juli und Aug. 1860. 4to.

1861.

WEEKLY EVENING MEETING,

Friday, January 18, 1861.

William Robert Grove, Esq. Q.C. F.R.S. Vice-President, in the Chair.

John Tyndall, Esq. F.R.S.
PROFESSOR OF NATURAL PHILOSOPHY, ROYAL INSTITUTION.

On the Action of Gases and Vapours on Radiant Heat.

The discourse commenced by a reference to the researches of Leslie, Forbes, and Knoblauch; but more especially to the admirable investigations of Melloni on Radiant Heat. These eminent men had left the gaseous form of matter practically untouched, and to extend our knowledge into this wide region was the object of the investigation on which the present discourse was founded.

The apparatus made use of, and which was applied in the experiments of the evening, consists of the following parts:—

1. A copper cube C, containing water kept constantly boiling, and one of whose faces, coated with lamp-black, forms the source of radiant heat.

2. A brass tube, 2·4 inches in diameter, which is divided into two portions, α and β.

α. The portion of the tube intended to receive the gases and vapours; it is stopped air-tight at its two ends by plates of rock-salt, and is attached to a good air-pump, by which it can be exhausted at pleasure. The length is 4 feet.

β. An air-tight chamber between the tube α and the cube C. It is kept constantly exhausted, and the calorific rays therefore pass from the radiating plate through a vacuum into the tube, thus retaining the quality which belonged to them at the moment of emission.

To prevent the transmission of heat by conduction from the cube C to the tube α, the chamber β is partly embraced by an annular space, in which cold water continually circulates.

3. A thermo-electric pile furnished with two conical reflectors, and connected with an excellent galvanometer. One of the faces of the pile receives the rays which have passed through the tube α.

4. A second copper cube C′, also filled with boiling water, and whose rays fall upon the second face of the thermo-electric pile. The two cubes C and C′, thus radiating upon the opposite faces of the pile, tend, of course, to neutralise each other.

Between the cube C′ and the adjacent face of the pile a screen S is introduced, being attached to an apparatus of Ruhmkorff's, capable of extremely fine motion; by the partial advance or withdrawal of this screen the two sources of heat can be caused to neutralise each other perfectly.

The tube α and the chamber β, being both exhausted, the needle of the galvanometer is brought exactly to zero by means of the screen S. The gas or vapour to be experimented with is now admitted into the tube α, and if it possess any sensible absorbing power, it will destroy the previously existing equilibrium. The consequent deflection of the galvanometer, properly reduced, is the measure of the absorption. In this way the action of eight gases and thirteen vapours have been examined, and also the action of atmospheric air.

Oxygen, hydrogen, nitrogen, and atmospheric air, respectively absorb about 0·3 per cent. of the calorific rays; this is the feeblest action which has been observed.

The most energetic action is that of olefiant gas, which at the tension of one atmosphere absorbs 81 per cent. of the calorific rays. Between those extremes stand carbonic oxide, carbonic acid, nitrous oxide, and sulphuretted hydrogen.

Below a certain tension, which varies for different gases, the amount of heat absorbed is exactly proportional to the density of the gas. Above this tension, the rays on which the principal absorptive energy is exerted, become gradually exhausted, so that every augmentation of density produces a diminished effect.

In the case of olefiant gas, for example, where a unit measure $\frac{1}{50}$th of a cubic inch in capacity was made use of; for a series of fifteen such measures, the absorption was exactly proportional to the quantity of gas; subsequently, the ratios of the successive absorptions approached gradually to equality. The absorption produced by a single measure of olefiant gas of the above volume, moved the index of the galvanometer through an angle of 2·2 degrees; the tension of the gas being only $\frac{1}{1000}$th of an atmosphere.

In the case of vapours, the most energetic is that of sulphuric ether; the least energetic is that of bisulphide of carbon. Comparing small volumes and equal tensions, the absorptive energy of sulphuric ether vapour is ten times that of olefiant gas, and ten thousand times that of oxygen, hydrogen, nitrogen, or atmospheric air.

On a fair November day the aqueous vapour in the atmosphere produced fifteen times the absorption of the true air itself. It is on rays emanating from a source of comparatively low temperature that this great absorptive energy is exerted; hence the aqueous vapour of the atmosphere must act powerfully in intercepting terrestrial radiation; its changes in quantity would produce corresponding changes of climate; subsequent researches must decide whether this *vera causa* is competent to account for the climatal changes which geologic researches reveal.

Oxygen obtained from the electrolysis of water exerted four times the absorptive energy of the same substance when caused to pass through iodide of potassium; the greater action being due to the presence of ozone.

The radiative power of gases was examined by causing them to pass over a heated sphere of metal, and ascend in a column in front of the thermo-electric pile; various precautions were taken to secure accuracy in the results. It was found that the order of radiation was exactly that of absorption; that any atom or molecule which is capable of accepting motion from agitated ether, is capable in precisely the same degree of imparting motion to still ether. Films of gas on surfaces of polished metal were found to act like coats of varnish.

The speaker also investigated the physical connection of radiation, absorption, and conduction. In the foregoing experiments *free* atoms and molecules were dealt with, and upon them individually was fixed the responsibility of the effects observed. These effects are thus detached from considerations of cohesion and aggregation, which suggest themselves in the case of liquids and solids.

The reciprocity of absorption and radiation is a simple mechanical consequence of the theory of an ether.

But why is one molecule competent to stop or generate a calorific flux so much more powerfully than another? The experiments prompt the following reply:—The elementary gases which have been examined all exhibit extremely feeble powers both of absorption and radiation, in comparison with the compound ones. In the former case we have oscillating atoms, in the latter oscillating systems of atoms. Uniting the atomic theory with the conception of an ether, it follows that the *compound* molecule which furnishes *points d'appui* to the ether must be capable of accepting and generating motion in a far greater degree than the single atom, which we may figure to our minds as an oscillating sphere. Thus oxygen and hydrogen, which, taken separately, or united mechanically, produce a scarcely sensible effect, when united chemically to form oscillating systems as in aqueous vapour, produce a powerful effect. Thus also, nitrogen and hydrogen, which when separate or mixed, produce but little action, when combined to form ammonia, produce a great action. So also nitrogen and oxygen, which when mixed as in air, are feeble absorbers and radiators, when united to oscillating systems, as in nitrous oxide, are very powerful in both capacities. Comparing small volumes and equal tensions, the action of nitrous oxide is 250 times that of air; a fact which perhaps furnishes a stronger presumption than any previously existing, that air is a *mixture*, and not a compound. Carbonic oxide is about 100 times as powerful as its constituent oxygen; carbonic acid is 150 times as powerful, while olefiant gas, as already remarked, is 1000 times as powerful as its constituent hydrogen. In the case of the hydro-carbon vapours, where the atomic groups attain a higher degree of complexity, the action is even greater than that of olefiant gas.

The speaker also referred to the experiments and observations of Niepce, Angstrom, and Foucault; but more especially to the admirable researches of Kirchhoff and Bunsen, as regards the influence of the period of oscillation on the rate of absorption. He pointed out how the grouping of atoms to systems in a resisting medium must tend to make their periods of oscillation longer, and thus bring them into isochronism with the periods of the obscure radiations made use of in the experiments.

With regard to conduction, the speaker would illustrate his views by reference to two substances—rock-salt and alum. He was once surprised to observe the great length of time required by a heated mass of rock-salt to cool; but this was explained by the experiments of Mr. Balfour Stewart, who shows that rock-salt is an exceedingly feeble radiator. The meaning of this is that the molecules of the salt glide through the ether with small loss of *vis viva*. But the ease of motion which they are thus proved to enjoy must facilitate their mutual collision. The motion of the molecules, instead of being expended on the ether between them, and then communicated in part to the ether external to the mass, is transferred freely from particle to particle; or in other words, is freely conducted. This *à priori* conclusion is completely verified by the author's experiments, which prove rock-salt to be an excellent conductor. It is quite the reverse with alum. Mr. Balfour Stewart's experiments prove it to be an excellent radiator, and the author's experiments show it to be an extremely bad conductor. Thus it imparts with ease its motion to the ether, and for this very reason finds difficulty in transferring it from particle to particle; its molecules are in fact so constituted that when one of them approaches its neighbour, a swell is produced in the intervening ether; this motion is immediately communicated to the ether outside, and is thus lost for the purposes of conduction. The lateral waste prevents the motion from penetrating the alum to any great extent, and hence it is pronounced a bad conductor. These considerations seem to reduce the phenomena of absorption, radiation, and conduction to the simplest mechanical principles.

[J. T.]

WEEKLY EVENING MEETING,

Friday, January 25, 1861.

The Rev. John Barlow, M.A. F.R.S. Vice-President, in the Chair.

Dr. G. C. Wallich,

On the Nature of the Deep-sea Bed, and the Presence of Animal Life at vast Depths in the Ocean.

Our first clear glance at the floor of the ocean may be said to date from the period at which submarine telegraphy was first undertaken. For although the depth of the sea has been approximatively ascertained over widely extended areas, in the course of the various surveys conducted under the auspices of the British, the United States, and the Dutch Governments, hardly any previous attempts have been made systematically to investigate the characters and composition of its bed. In the absence of any special object, such attempts would have been far too costly and difficult to be practicable. It has been ascertained, however, that the floor of the ocean is but the reflex, as it were, of the dry land; that it is in no place unfathomable; that along its deeper portions certain muddy deposits are to be met with, in many cases made up, more or less entirely, of minute calcareous shells belonging to one of the most simple order of beings with which we are acquainted; and that together with these are also to be found, but in comparatively speaking small quantity, the minute flinty skeletons of other organisms derived both from the animal and vegetable kingdoms. But no conclusive evidence has been produced to show whether any or all of these organisms normally lived and perished, at the profound depths from whence they were obtained by the sounding lead; or whether, having inhabited distant, and perhaps shallower seas, their dead remains alone, after being transported by currents or other agencies, had gradually subsided into the deep hollows of the ocean. Taking into consideration the very important part played by these organisms in the structure of the earth's crust, that vast strata have in ages gone by been built up of them, and that similar strata are at the present time being deposited along the beds of existing seas, the investigation of these questions becomes of the highest consequence, as bearing on the successful establishment of ocean telegraphy.

The distribution of animal life in the upper waters of the sea is determined by climate, by the composition of its waters, the nature of its bed, and its depth in any given locality; the last of these items necessarily involving the relative degrees of temperature, light, aeration, and pressure, as compared with those to be met with near the surface. Of these conditions climate exercises a very powerful influence; for it is found as we advance from the equator towards the poles that a gradual diminution takes place, not only in the number of types met with, but of the varieties ranged under those types. It has been maintained, that in order to compensate for the diminution in the number of generic forms, the number of individuals of each species is much augmented. Although this law holds good as regards the higher orders, it can hardly be said to do so in the case of the lower; for the vast assemblages of these lower forms met with on the surface of the sea in the tropics, are in no wise less extensive than those met with in high latitudes. It will be found that the lower the grade of being, the more equally balanced will be its distribution, at the extremes of the globe; inasmuch as the greater range in depth commanded by these lower forms renders them less amenable to conditions which are variable from being dependent on atmospheric changes.

The composition of the waters of the ocean is well known to become much more equable at great depths; and it therefore exercises a far less marked influence on the presence of animal life than it does at the surface. The same causes which equalize the temperature in so remarkable a manner as the depth increases, are effective in equalizing the relative proportions of the various ingredients that enter into the composition of sea water, in all latitudes. For whilst the surface stratum is subject to dilution with fresh water, from various sources, the greater the depths the less subject can the waters be to this influence, and the less can it operate in modifying the distribution of the organisms that frequent them.

Oxygen is essential to the presence of animal life—without it animal life ceases. To air-breathing, as well as water-breathing creatures a due supply of this gas is indispensable; the function of respiration, no matter whether performed by lungs, as in man and the higher orders, or by a simple process of absorption and exudation through the general surface of the body, as in some of the lower forms, being, in every instance, essentially that process whereby oxygen is received into the system in exchange for carbonic acid which is given off. But although oxygen enters largely into the composition of both atmospheric air and water, the supply of this element is not obtained, in the case of creatures inhabiting the sea, under ordinary circumstances, from its decomposition, but from a certain portion of atmospheric air present in water in a state of solution. Most gases are absorbed by water. Under pressure, the quantity absorbed is much increased, as is seen in the familiar case of soda-water. It should be borne in mind, however, when the fact is applied to the occurrence of animal life at great depths in the sea, that in order to produce the absorption of atmospheric air, its contact or mixing together at the surface by the action of wind and wave is necessary, and the effect of this operation can only extend to a limited depth, unless, as has been assumed by some of our highest authorities, the lower strata of sea water, being subject to increased pressure, become capable of holding in solution a greater quantity of oxygen; and, by robbing the superincumbent strata of that which they contain, gradually become saturated with it. Should this view be correct, there must be a point at which the maximum amount of oxygen which sea water can absorb, is permanently present in it. But, inasmuch as the vegetable cell, simple though it be in structure, can eliminate carbon from the medium in which it lives, it is not unreasonable to assume that the lowest forms of animal life, even where no specialized organs are traceable, may, in like manner, be able to eliminate oxygen directly from the water around them.

The temperature of the sea is materially influenced by the climatic conditions of different latitudes; and, of course, exercises a powerful effect both on the distribution and abundance of the higher orders of living beings present in its waters. But, as has been shown, this influence is not manifest, or at all events, not so manifest in the lower orders; for at great depths the variability of the temperature is reduced within very narrow limits in all latitudes. Now the higher orders of oceanic creatures inhabit only the surface waters, never sinking down to extreme depths. In the case of some of the lower forms, on the other hand, a very extended pathymetrical range exists, putting out of the question those which constantly dwell on the sea bed itself, of which I shall presently have to speak.

In like manner, Light, or rather the absence of it, can hardly be said to determine, in any important degree, the distribution and limitation of the lower forms of animal life. Light is not essential even in the case of some of the higher orders. A large class of creatures, both terrestrial and marine, possess no true organs of vision, although there is good reason for believing that they do possess some special sensory apparatus, susceptible to the influence of light; whilst certain creatures, whose habitation is in subterranean caves or lakes, as in the Magdalena caves near Adelsberg, and the Great Mammoth caves in Kentucky, either possess no organs of vision, or possess them in so rudimentary a state, as to prove clearly that the absence or imperfect development of this sense may be compensated for by the higher development of other senses.

It is impossible at present to say to what depth light penetrates in the sea. The photographic art will, no doubt, one day solve the problem. But it is almost certain that a limit is attained, and that, moreover, long before the deep recesses gauged by the sounding machine are reached, where the light-giving portion of the ray cannot penetrate, even in its most attenuated condition; and yet, as shall hereafter be shown, creatures have been found down in those profound and dark abysses, whose colouring is as delicate and varied as if they had passed their existence under the bright influence of a summer sun!

Pressure is the last condition which has to be noticed. Although undoubtedly a highly important one, I hope to be able to prove that it is not of essential value, as has heretofore been laid down, in determining the final limit of animal life in the sea.

It is almost needless to state that at the sea level there exists a pressure of 15 lbs. on every square inch of surface, due to the weight of the atmospheric column resting upon it; and that the pressure on the successive strata of water in the sea, as the depth increases, is infinitely in excess of this, inasmuch as a column of water only 33 feet in height is capable of counterbalancing the entire atmospheric column, which extends to a height of about 45 miles. Accordingly, for every 33 feet of descent in the sea, putting out of consideration the effect of the superincumbent column in actually diminishing the bulk of the portions beneath by augmenting their density, there is an additional 15 lbs. At great depths, therefore, the aggregate pressure becomes stupendous. As is well known, pieces of light wood let down to a depth of 1500 or 2000 fathoms, become so compressed and surcharged with water as to be too heavy to float. But there is a fallacy in this experiment; for the contraction of the woody fibre and cells is a necessary consequence of their submission to an amount of pressure so enormously in excess of that under which they originated. With organisms which have been developed, from first to last, under the full operation of *any* given amount of pressure, the result would not be of this nature; for the equalization of the pressure, within and without their entire structure, although it might possibly exercise some definite effect in determining their shape, size, or even functions, cannot, I submit, operate in causing the creatures living under it to experience any more detrimental results than we experience from the 15 lbs. on every square inch, or about 14 tons, on the general surface of our bodies near the sea level.

It can scarcely be wondered at that under such apparently extraordinary conditions, the maintenance of life, even in its least developed aspects, should have been deemed absolutely impossible at extreme depths; and that it should have been almost unanimously recognized as an axiom, that at a depth of 400, or, at most, 500 fathoms, life, whether animal or vegetable, must be extinct. The fact is unquestionable that as we descend beyond the first hundred fathoms, the traces of life become more and more remote; and it is probably owing to this gradual diminution in the number of animal forms, as the depth exceeds this limit, that it has been assumed, rather as a matter of theory than of observation, that a point is speedily reached at which all the conditions essential to life are extinguished. This view has also derived support from the idea that "animal life depends on the previous existence of vegetable life." In the case of the higher orders of the animal kingdom, the law, no doubt, holds good. Not so, however, in the case of the lower. The conditions essential to the perpetuation of the one are not essential to the perpetuation of the other. Thus, light is indispensable for the healthy respiration and growth of the vegetable. The animal can, on the other hand, respire as freely in the blackest darkness as in the broad glare of day; and this is, no doubt, the reason why vegetable life in the ocean attains its final limit in depth so much sooner than animal life. And yet, considering how very unexpectedly animal life has been proved to exist deep down in the ocean—as I shall immediately show, far removed beyond those conditions which had hitherto been considered indispensable—we ought perhaps to pause before we assert that the same plastic skill which has so constituted certain creatures as to admit of their inhabiting the deep abysses of the ocean, may not, in like manner, have so constituted some of the vegetable organisms as to be capable of living under similar conditions.

The *Foraminifera* are the organisms to which reference has been made as performing so very important a part in the formation of certain strata on the earth's crust. They occur abundantly in all existing seas. They are to be met with in a fossil state, not only in chalk, but in almost all marine sedimentary strata; as, for instance, in the hard limestones and marbles. The recent Foraminifera may therefore be looked upon as the oldest living representatives of any known class of organisms.

In the mud, or "ooze" as it has been termed, which is brought up from great depths in many parts of the open sea, immense assemblages of Foraminifera are to be met with, chiefly belonging to one species however. In the absence of examinations conducted immediately on their being brought up to the surface by the sounding machine, it is not surprising that the question as to their occurrence in a living, or only in a dead state, should have remained undecided: most of the authorities who have written on the subject being of opinion that they do not live at great depths, but that their shells and remains have drifted to the positions in which they were found from shallower waters, or have subsided from the upper strata of the ocean. Professor Huxley was one of the very few who leant to the more correct opinion; he having declared, that although far from regarding it as proved that the Globigerina (the species referred to) live at these depths, the balance of probabilities seemed to him to incline in that direction. Other writers have offered surmises on the subject; but these, in the absence of anything like substantial proofs, were, of course, only estimated at what they were worth.

The difficulty is how to determine the point conclusively. For it seems legitimate to infer, that if these organisms are specially adapted to exist under conditions differing so widely from those present at or near the surface, the very circumstance of removing them from one set of conditions to the other, would inevitably destroy their vitality, and perhaps their normal structure, before it could become practicable to subject them to microscopic analysis. Nor is the difficulty an imaginary one. For, taking into consideration the entirely altered circumstances in which these creatures must find themselves placed when brought to the surface, locomotion, or even the protrusion of their filamentary appendages, could hardly be expected. The mere existence of the fleshy parts within their shells, and that too in an apparently recent condition, affords no proof, inasmuch as the great quantity of saline matter present in sea water, and especially at great depths, would of itself alone account for their perfect state of preservation.

"During the recent survey of the North Atlantic, I found that in certain localities, where the Globigerina deposit was of the purest kind and in the greatest plenty, the specimens from the immediate surface stratum of the sea-bed alone retained their normal appearance, both as regards the perfect state of the sarcodic contents of the shells and the presence of the pseudopodia. The latter organs were never seen by me in an extended condition; but in the specimens alluded to, and in those only, occurred as minute bosses, resembling in shape the rounded rivet-heads on boilers, closely appressed to the external surface of the shell; whereas, in specimens from the substratum, the colour was much duskier, and these bosses were absent. And further, in these pure deposits the shells were to be found in every gradation, from the single chamber, of microscopic minuteness, hyaline transparency, and extreme thinness, to the dense Zeolite-like structure of the many-chambered mature shells, which are large enough to be readily distinguished by the naked eye. These facts, when taken in conjunction with the entire absence of the varied remains of other organized structures found in localities where the Globigerinæ are only scantily represented, afford, as I conceive, all but the direct proof, which can only be arrived at on witnessing locomotion, or the protrusion and retraction of the pseudopodia of the organisms in question.

"Most fortunately, as it happened, this collateral evidence was rendered doubly conclusive by other proofs of a most unexpected and interesting kind. Before entering on these, I may state that the substratum, spoken of as differing in aspect from the immediate surface-layer, is nevertheless identical in composition; the difference in colour arising simply from decay. It contains no living Foraminifera; for the minute particles of matter becoming gradually condensed and aggregated together by molecular affinity, and the enormous superincumbent pressure exerting itself only in one direction, that is, vertically, its permeability by fluids is thus completely destroyed, and it is compacted into a dense mass of far too unyielding a nature to admit of its being traversed by living creatures of any kind. As the Foraminifera die off, their shells and decaying contents, together with the minute particles of amorphous matter associated with them, go to build up the calcareous strata of the earth's crust. I would mention, that in order to determine whether the *Globigerinæ* live as free floating forms in the mid strata of water, I attached a small open-mouthed bag, at about 200 fathoms from the extreme end of the sounding line, in a locality where the species was most abundant in the deposit, and brought it up through nearly 5000 feet of water without securing a single shell.

"But by far the most important and interesting discovery remains to be noticed, namely, the detection of a high order of radiate animal, in a living state, at a depth of a mile and a half below the surface of the sea.

"When we take into consideration the low position of the rhizopod in the scale of being, and the obvious probability, pointed out by Professor Huxley, that a class of creatures proved to extend so far back in time—that is, in a fossil state—must be able to maintain existence under extraordinary and variable conditions as regards light, temperature, and pressure, the sentiment engendered is rather one of wonder, that their vitality at great depths should have been so long and so stoutly maintained, than that it should now be so fully proved. But few persons were bold enough to suspect that creatures of a far higher type, namely, Radiata, could exist under similar conditions; and I freely admit, that nothing short of the most incontrovertible proof ought to be accepted in support of such a view. Fortunately, I am in a position to afford that proof.

"In sounding midway, in the direct line between Cape Farewell, the southern point of Greenland, and the north-west coast of Ireland, in lat. 59° 27′ N., and long. 26° 41′ W., the depth being 1260 fathoms (or 2520 yards), whilst the sounding apparatus itself brought up a considerable quantity of minute granular particles, looking like a fine oolite, but which was, in reality, a nearly perfectly pure Globigerina deposit, 13 star-fishes, from 2 to 5 inches in diameter from tip to tip of rays, belonging to a genus plentifully represented on our own coasts, came up adhering to the extreme 50 fathoms of sounding line. These *Ophiocomæ* were not only alive on being brought up out of the water, but some of them continued for fully a quarter of an hour to move about their long spinous arms. To render intelligible the significancy of the entire circumstances, I must mention, that in order to ensure accuracy, it is always necessary, when sounding in deep water, to ascertain the depth by one sort of apparatus, and to bring up the sample of bottom by another. In the present case, the ascertained depth was 1260 fathoms, and 50 fathoms was accordingly "paid out" in the second operation of bringing up bottom, in order to make sure that the more complicated and unmanageable apparatus required for this purpose fairly rested on the bottom.

"Now, supposing it possible that these star-fishes were drifting about in some intermediate stratum of water, between the bottom and surface, it is evident that they would have attached themselves indiscriminately to any portion of the entire 1260 fathoms of line; unless, indeed, they chanced to have been directing their course in a closely compacted column, which was transversed by the last *extra* 50 fathoms of line at the precise moment of their crossing it. Whether it be possible that they were drifting in such a column, or floating on a bed of seaweed or other substance, is immaterial, inasmuch as they could only have attached themselves as they did to the portion of line referred to under this one condition. But the very act of attachment would, I maintain, be impossible in the case of creatures whose movements are so sluggish, when the object which they had to grasp was moving upwards at the rate of two miles per hour (as it does when hauled up by the steam-engine), and without a moment's intermission.

But even assuming it to be possible that they had drifted to the position in which they were captured, from distant and less profound depths, the fact of their vitality and vigorously healthy condition would be scarcely less extraordinary; for the distance from the nearest point of land, which is a rock off Iceland, is 250 miles; whilst the next nearest land, Greenland, is distant no less than 500 miles. But it must be obvious to every one who is at all conversant with the structure of the *Ophiocomæ* and Echinoderms generally, that they are essentially creeping and crawling creatures, and of far too great specific gravity to float at all under any circumstances.

"Taking into consideration then, the circumstances under which these *Ophiocomæ* were taken, the extreme improbability of their having drifted to the locality in which they were found, from distant and shallower waters; and lastly, the peculiarities of structure, which render them wholly unfit to float or swim for even a brief period, we should have been fully warranted, I think, in believing that they existed in a living state at the bottom. In order to obtain some clue to the solution of the question, I very carefully dissected and analyzed the contents of the digestive cavity of a specimen, immediately on its being brought up; and was most amply repaid by the detection of numerous *Globigerinæ* in every stage of comminution, and with the contained sarcodic matter in greater or lesser quantity. Whilst, therefore, the detection of these organisms in the digestive cavities of the *Ophiocomæ* afforded a most conclusive proof that the Foraminifera were living on the sea bed at the profound depth from which they were obtained; the fact of the star-fishes being captured with the fresh remains of the Foraminifera in their digestive cavities, proves that their normal habitation is at the same great depth, inasmuch as it has been sufficiently established that the Globigerinæ are present *only* at the bottom. I may mention that, within the past few days, in examining a sample of the Globigerina deposit brought up by a previous sounding on the same spot, I detected some Echinoderm spines, which at once struck me as being identical with those on the Ophiocomæ; and that, on comparison, my surmise proved to be quite correct: a further and very striking proof of the vitality of the Ophiocomæ at the bottom being thus afforded."

[G. C. W.]

WEEKLY EVENING MEETING,

Friday, February 1, 1861.

The Rev. John Barlow, M.A. F.R.S. Vice-President, in the Chair.

Rev. A. J. D. D'Orsey, B.D.
English Lecturer at Corpus Christi College, Cambridge.

On the Study of the English Language as an Essential Part of a University Course.

The speaker said, that "A plea for the study of the English Language as an essential part of a University Course," was a subject which, on its first announcement, might seem unsuited to the distinguished auditory which he had the honour to address. Some might regard it as too literary to be within the range of an institution whose objects were more directly scientific. Others might consider it as a purely professional question, interesting to clergymen, college tutors, and educators of all kinds, but having no claim on the attention of the general public; and not a few might be found, whom the theme took entirely by surprise, believing it hardly possible that in the middle of the nineteenth century, it should be found necessary to plead for the introduction of the study of our own English tongue into our own English Universities. In reply, he urged that the philosophy of language might fairly claim rank as a science. Nor was the question of an extended culture of the mother-tongue one of mere professional importance, for it concerned us all to be able to say what we had to say clearly and forcibly. The absence of such culture in most of our Universities was a fact; and the results were evident in our compositions, our speeches, our sermons, our reading of the church service, and even in our conversation.

The speaker then defined language, not simply as the vehicle of our ideas, but, in Whately's words, as "the instrument of thought." It was the mysterious machinery by which thought was manufactured. Words were realities, and a knowledge of words correctly taught was a knowledge of facts, for every word was a coin in the currency of human intercourse. The abuse of a thing was no argument against its use; and if the sciolist or the pedant had divorced words from ideas, and degraded linguistic studies into mechanical taskwork, that could not fairly be adduced as a proof that such pursuits were no longer useful in a practical age like ours. Wordsworth was right when he said language was "not the dress, it was the very incarnation of thought," a union as close as that of body and soul. He felt this preface necessary to vindicate the study of words against those who fancied it was opposed to the study of things; but his business was now with our own mother-tongue. The attention of the audience was then called to a very large map of Europe, coloured "glossographically," to show the fields occupied by the great families of languages. Beside it was suspended a chart, headed "Indo-European Languages," containing the great stems—Celtic, Germanic, Græco-Roman, Scandinavian, and Sclavonic—at the heads of columns, under which were ranged the languages and dialects. The speaker said that we were in origin oriental, kindred in language even with the Hindoo. The clearest idea of the subject might be formed by the conception of waves of peoples and tongues rolling from the East, of which the Celtic was the first, its localities being now the extreme western points of Europe. The stems with which English had to do, were the Germanic and the Græco-Roman. He then pointed to another large map, similarly coloured, of the British Islands, with an accompanying table of languages and dialects. Coloured diagrams were also exhibited, showing the large proportion of Saxon in English. Out of 100,000 words, 60,000 were of Teutonic origin, 30,000 of Romanic, and 10,000 from other sources. Our best authors used far more Saxon than Latin: Shakspeare, 85 per cent. of Saxon; even Johnson, 75 per cent.; and Gibbon, our most Latinized writer, 55. The English Bible had 97 per cent. of Saxon words. The writers of our own day were showing a much greater love for good, strong, home-bred words. He now begged his hearers to look at the bare fact, that there were twice as many Saxon words as Latin ones in our tongue, and then to say whether our present school-craft was right, which utterly passed by the one and unfairly fostered the other. It was too bad that we should thus undervalue our own speech, of which Grimm had said, "The English language possesses a veritable power of expression, such as perhaps never stood at the command of any other language of man."

The speaker then asked, how was English taught? He quoted Quintilian as to the choice of nurses with good pronunciation, and censured the carelessness of English parents in allowing vulgar, uneducated servants to surround their children. He next adverted to the sad condition of English teaching in most of our National Schools, proving his assertion from the reports of the Inspectors. He had ascertained that in our great public schools, no direct attention was paid to English; no recognition of the dignity of our own language and of its idiomatic structure, so different from that of the ancient tongues; no organized English department with an able Saxon scholar at its head: most of the head-masters being of opinion that sufficient provision was made by the ordinary practice of translation, writing an occasional theme, or declaiming a passage from Shakspeare on "Speech-day." Some exceptions existed,—the City of London

School, King Edward's School at Birmingham, and the School of Bury St. Edmund's. The Universities had for ages sent forth men who had adorned the pulpit, the senate, the bar, and almost every branch of human knowledge; and one might well pause before presuming to suggest even a doubt that systems, which had produced such great and varied excellence, could be deficient in one of the chief means for qualifying the student for any department of public life. All this was readily admitted—the great value of classical and mathematical studies as disciplining the mind quite undisputed—and the Universities defended against the popular cry of their not being equal to the requirements of the age. But still the warmest supporter of the existing order of things must, if candid, be compelled to grant, that there existed some serious defect in any system of education which, while sending out a few brilliant stars, left the great mass of its men but very indifferent workers in those pursuits which were to be the business of their lives. Were it the special duty of our Universities to produce classical and mathematical professors and teachers, possessing not merely a critical, but a hypercritical knowledge of their subjects, the present system would seem admirably calculated to fulfil the end in view. When, however, it was remembered, that nearly all the Colleges bore some such designation as the "ancient and *religious* foundation of ——;" it was clear they were intended by their founders as training-schools for the clergy: and when the Universities recognized the fact, that the education given was *not simply preparatory*, but to a certain extent professional, by their institution of Divinity Professorships, and by requiring the attendance of theological students not only at lectures but at examinations, it could not but be felt as somewhat extraordinary and inconsistent, that no provision had been made for due training in that language in which the future clergyman was to address his flock —for systematic instruction in the composition and delivery of sermons, and for distinct and unaffected reading of the Church service. The present system of prizes for poems, essays, and declamations stimulated but a very small proportion of the men—the scholarship referred rather to the niceties of the classical tongues than to exercise in our own idiom; and the debating societies, to which some attached so much value, effected but partial good, from the want of a judicious criticism of the youthful orators. These meetings did, it was true, frequently develope self-reliance, but they also encouraged a flashy declamatory style, which prejudiced many thoughtful men against attempts to cultivate the art of public speaking. The College chapels, instead of being normal schools for Church reading, were practically the reverse; for the chaplains generally recited the prayers as if the object were to get the duty over in the shortest time, and the scholars, profiting by the example, read the Lessons rapidly, indistinctly, with little attention to pauses or emphases; and they were seldom corrected for mispronunciation, monotony, provincial accent, or any error, unless indeed a false quantity should have offended the ears of the classical tutor. The University and College authorities seemed to take it for granted, that men coming into residence were fully masters of the English language; that they required no course of study in so elementary a matter as the art of reading, writing, and speaking their native tongue; and that they must just do as others had done before them in preparation for the prayer-desk and pulpit,—trust to the "light of nature" in writing and delivering sermons; or, if hard pressed, they might compile, "adapt," borrow, beg, or hire! Consequently little or no encouragement was given to any proposals to supply these defects. Several Professors of Divinity had tried, but in vain, to get their men to do what was done in all the Scottish Universities,—write and deliver trial sermons. An eminent elocutionist, Mr. Plumptre (a member of the English bar), had just commenced work at Oxford; and an English Lectureship had been founded at Cambridge; but both were of origin too recent to be at present taken into account. Certain it was, that, while those in influential positions spoke of elocution as mere "spouting," while undergraduates had every inducement to devote themselves exclusively to classics and mathematics, and while the most accomplished English scholar found himself no better off in examinations than the man who sent up his papers full of errors in grammar, spelling, and punctuation, there was but little hope of any extensive measure of improvement in our University system.

The speaker then rapidly sketched the results of this imperfect teaching, as shown in the stagnant condition of the great mass of our population, urging that the cases often quoted of great intelligence were quite exceptional. Millions of our countrymen never opened a book. Of the thirty millions in these islands, fourteen millions never entered a place of worship. Many causes might operate, but he believed a great one was the want of language; the utter inability to understand what was read or spoken. And how should it be otherwise with peasants whose stock of words was limited to 350, and whose clergymen were incapable of preaching a sermon in good plain Saxon-English? In the middle and upper classes, too, the knowledge of English was very deficient. De Quincey had said, most truly, "It makes us blush to add that even grammar is so little of a perfect attainment amongst us, that, with two or three exceptions, we have never seen the writer who has not sometimes violated the accidence or the syntax of English grammar." And he adds that faults in composition "may be detected in every page of almost every book that is published." How few could write a really good letter! How many of our writers seemed to know nothing of punctuation! How many youths of good families, educated at our public schools, have been rejected at our civil service examinations from sheer ignorance of spelling! What errors in the choice of words, "lay" for "lie," "expect" for "suppose"—what harping upon pet words—what blunders in syntax—what efforts to write "fine English"—what adoption of slang and foreign terms! And what was to be said of our speech-makers? Professional orators set aside, what lamentable exhibitions were made on our platforms and hustings, at our public dinners and our wedding breakfasts!

Nominatives in vain search of missing verbs—verbs pursuing nominatives without success; plurals and singulars joined in ungrammatical wedlock; premises laid down, from which no conclusions were drawn; many conclusions, with most vehement "therefores," drawn from imaginary premises! Such speeches owed much to the mercy and the talent of reporters, who could bring order out of chaos, sense out of nonsense, and even eloquence out of the veriest platitudes. And yet these are the results of our training in grammar, which ought, in the famous words of Lindley Murray, to enable us to "read, write, and speak the English language with fluency and propriety!"

Our Universities had not much to boast of as the results of their teaching in this respect. There was sound scholarship—there were profound mathematical attainments—there was a certain amount of theology; in short, great disciplining of the powers—much storing of the mind—but no direct training in reading, writing, and speaking English—the daily business of all—the professional duty of the clergy. They might listen to the way in which the Church Service was read in nine cases out of ten as a proof (indistinct utterance, incorrect pronunciation, provincial accent, false emphasis, drawling, monotony, &c.)—to the dull, listless style in which sermons were composed and delivered, and to the painful attempts of most clergymen to do what the barrister or the member of Parliament did,—speak without paper. In Parliament, too, matters were not much better. With some dozen exceptions, in neither House were there any really good public speakers. Of the 10,000 speeches made last session, 5000 might have been spared, and the rest given in a tenth of the time, if the speakers had been trained to speak to the point, to say what they had to say in the fewest words. If volunteers must be drilled, public speaking must be taught. Three months under a sound teacher of elocution, of which there were several in London, would enable noble lords and honourable members so to articulate as not to be reported "inaudible in the gallery;" and three months more under a composition master would teach them the art of constructing an English sentence, warranted not to fall to pieces in the course of delivery.

In other countries, the native tongue was not neglected. In France, the language of the country was carefully taught through the whole course of a boy's education; the examination paper for degrees, in 1861, contained five Greek, six Latin, and *eight French* authors. When would that be matched in Oxford and Cambridge? Nothing could be better than the teaching of the mother-tongue in Germany. And the common objection raised by our exclusively classical men, that the study of English would interfere with Latin and Greek, was met by the facts, that in Germany the very best philologists were to be found; that from Germany our compilers of grammars and dictionaries were glad to draw their most valuable information; and that, while Germans were at least our equals in the languages of Greece and Rome, they were unquestionably our superiors in Oriental and European tongues. And there could be no doubt that the profounder attainments were due to the sound system of teaching the principles of grammar, based on their own mother-tongue, from the very earliest age.

At home there had long been some cheering signs of encouragement. In 1834 the Lord Provost and magistrates of Glasgow had led the way, by establishing in the High School of that city a department of the English language and literature. English professorships had been founded in King's College and University College, London; in Dublin, and recently in the Queen's Colleges. Professor Aytoun had restored the subject in Edinburgh. The theological colleges of the Church of England were moving; but the Dissenters' colleges, especially the one under Dr. Angus, seemed more practical. Another encouraging symptom was the demand for a far superior class of books —the writings of Whately, Richardson, Bosworth, Smart, Max Müller, Latham, Trench, Dasent, Masson, Craik, Hunter, Morell, Demaus, and last not least, Farrar.

The press, too, was on our side. *The Times* not only by its own racy idiomatic English was the composition-master for all England, but had done good service by calling attention to the "something rotten in the state" of school-work, as shown by the competitive examinations. The *Saturday Review*, too, was unsparing in its criticism; and even our facetious friend *Punch* defended purity of language from the contamination of slang, foreign terms, and the vulgarity of "fine English." An admirable article, by Mr. Robinson, of York, in *Macmillan's Magazine*, must not be unnoticed.

The remedies suggested were:—1. Training schools for nursery governesses. 2. Greater care in giving National Schoolmasters a thorough knowledge of English, spoken and written. 3. Encouragement to really good men to become and to remain National masters, by rewarding distinguished veterans with School-Inspectorships, instead of limiting such offices, as at present, to young clergymen and barristers. 4. The appointment by the Committee of Council of a Government Lecturer in each county, whose duty it should be to lecture on the principles of English teaching, and to instruct the schoolmasters. 5. The introduction of a thoroughly accomplished scholar as English master in every great public school, not a mere educational drudge, a "general utility gentleman," to look after the "small boys," but one of equal rank with the classical masters. 6. The endowment of at least one Professor of English in every University; his duty being to give men, during their undergraduate career, a critical knowledge of the language, supplying deficiencies, correcting errors in speech and writing, suggesting courses of reading, drilling the future barrister and legislator in accurate and fluent oratory, and training candidates for the ministry to read distinctly and unaffectedly, and to compose and deliver sermons in a clear, impressive, and attractive style. 7. Making English take its place with Latin and Greek in every examination for degrees, as it does in the India Civil Service examinations, and giving substantial rewards (Scholarships and Fellowships) for distinction in English, as for eminence in Classics and Mathematics. 8. The co-operation of the Bishops in exacting from all candidates for Holy Orders proofs of competency in reading the Church Service, composition, delivery of sermons, and extemporaneous speaking. The speaker was perfectly aware of the objections that would be raised against such proposals; but if these, or most of these, were carried into effect, he believed the greatest benefits would result, not merely in a literary, but in a moral and religious point of view. He concluded by expressing his regret, that he had been only able, in the limited time allowed him, to throw out a few practical suggestions; and he thanked the audience for the very kind and marked attention with which they had listened to his observations.

[A. J. D. D.]

GENERAL MONTHLY MEETING,

Monday, February 4, 1861.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Nicholas P. Leader, Esq.

was *elected* a Member of the Royal Institution.

With reference to the Minutes of the Meeting on November 5 last, the following Resolution was moved by PROFESSOR FARADAY, seconded by MR. G. MACILWAIN, and carried unanimously:—

RESOLVED,—That the Members now assembled, desiring to record in their Minutes the deep sense they entertain of the long, devoted, and effectual services of the Rev. JOHN BARLOW, whilst their Honorary Secretary for a period of eighteen years, which they esteem as a service of love to the Royal Institution and to science, do, for themselves and the general body, offer him their best and sincerest thanks on the occasion of his retirement, and the heartiest wishes for his health and happiness.

SIR HENRY HOLLAND, Bart. M.D. F.R.S. was elected a Manager of the Royal Institution, in the room of SIR CHARLES FELLOWS, deceased.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same: viz.

From

Secretary of State for India—Bombay Magnetical and Meteorological Observations, 1858. 4to. 1859.

Actuaries, Institute of—The Assurance Magazine, No. 42. 8vo. 1861.

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WEEKLY EVENING MEETING,

Friday, February 8, 1861.

Sir Roderick I. Murchison, F.R.S. Vice-President, in the Chair.

Professor T. H. Huxley, F.R.S.

On the Nature of the Earliest Stages of the Development of Animals.

The lecturer commenced by giving a general description of the structure and singular properties of the animal organism, termed *Pyrosoma Giganteum*, a specimen of which, taken by Capt. Callow in the North Atlantic, had been forwarded to him by Admiral Fitzroy, in the autumn of 1859.

Not only had his investigations enabled the speaker to verify the most important of the statements made in his memoir on *Pyrosoma*, published in the *Philosophical Transactions* for 1851; but they had revealed peculiarities in the mode of reproduction of the animal, of great interest from their bearing on some of the most difficult questions of embryology.

In order to render the importance of these new facts obvious, it was necessary to premise a concise statement of our present knowledge with regard to the early stages of animal development. To this end the structure of the fowl's egg was described, and the effects of incubation were traced, so far as was necessary to prove that the chick takes its origin from the cicatricula, or blastoderm.

It was next pointed out, that we owe the discovery of this important fact to the great Harvey, who, in his "Exercitationes de Generatione Animalium," demonstrated with perfect clearness, firstly, that the chick is developed from the cicatricula, and not, as had been supposed, from the chalazæ, or other parts; and secondly, that the process of development is an "epigenesis," or gradual addition of new parts to those already formed.

In virtue of these discoveries, Harvey has as much right to be regarded as the originator of modern embryology, as, in virtue of his discovery of the circulation, he has to be considered the founder of scientific physiology: but his embryological views met with a less fortunate reception than his physiological doctrines; and for a century and a half, the strange dogmas of the evolutionists, supported by the vast authority of Haller and of Cuvier, were allowed almost completely to override and weigh down the sounder teachings of the great Englishman.

With the publication of Caspar F. Wolff's "Theoria Generationis," in the middle of the last century, however, a new epoch commenced; and partly by the labours of that eminent observer, and still more largely by those of Pander, Von Bär, Rathke, and Reichert, Harvey's doctrine has been rehabilitated, and has taken its place among the firmly ascertained verities of science.

For want of proper microscopes and other appliances, neither Harvey nor C. F. Wolff could trace the origin of the germ further back than the blastoderm; still less could they obtain any just conception of the essential structure of the ovum. But in the course of the last thirty-five years, thanks to the labours of Purkinje, Von Bär, Wagner, Bischoff, Wharton Jones, Prevost, Dumas, Coste, and others, vast advances have been made.

It has been ascertained that the ovum of every animal primarily consists of a germinal vesicle, containing its so-called spot, and enclosed within a yelk, or vitellus; and that, in the great majority of cases, the first changes which follow upon impregnation consist in the disappearance of the germinal vesicle as such, and the regular division of the yelk into smaller and smaller masses, out of which, in one way or another, the blastoderm, of which the embryo is a modification, arises. Such yelk division, however, has not yet been observed among the higher *Annulosa*, nor in certain *Entozoa*, nor does it occur in *Pyrosoma*.

So much being definitely ascertained, there is yet one question upon which embryologists are widely divided, viz. What is the relation between the germinal vesicle and the cells, or structural elements, of which the blastoderm is composed? Three answers have been given to this question:—

1. According to the late Dr. Barry, the blastoderm arises from a modification of the germinal vesicle, in a manner particularly de-

scribed by him. No other observer, however, has been able to discover a trace of this process; and it may be regarded as tolerably certain that its describer was mistaken.

2. According to Bischoff, Kölliker, and the majority of embryologists, the germinal vesicle and its contents disappear, and have no direct connection with the cells of the blastoderm.

3. According to observations of the late Johannes Müller, of Gegenbaur and others, the germinal vesicle may give rise directly, by division, to the cells of the blastoderm.

The study of the development of the embryo of *Pyrosoma* yields results in close conformity with the last view. The ovum of this animal is, in fact, composed, at first, like all others, of germinal vesicle, germinal spot, and vitellus; but, in the course of development, the vitellus disappears, probably becoming liquefied, and the germinal vesicle is laid bare, so that it becomes comparatively easy to watch the subsequent changes in its interior. These consist in the deposit of a somewhat opaque matter and the division of the germinal spot, so as to give rise to the endoplasts, or "nuclei" of the blastoderm, which is thus primarily formed within the interior of the germinal vesicle.

The speaker concluded by observing, that it is not improbable that the process thus traced, is similar to that by which the blastoderm of the higher *Annulosa* arises, and that it will probably furnish the key to the signification of the multiple germinal spots observed in so many of the lower *Vertebrata;* while, by proving the direct descent of some of the histological elements of the progeny from those of the parent, it combines the theories of the pre-existence of germs with that of epigenesis.

[T. H. H.]

WEEKLY EVENING MEETING,

Friday, February 15, 1861.

Sir Charles Hamilton, C.B. in the Chair.

Henry F. Chorley, Esq.

On English Poetry in reference to Music.

The speaker maintained, as a general principle, that every poet must be born a musician, though many instances to the contrary had been cited;—Sir Walter Scott, among others, who was said to care for nothing save ballads for the sake of their words. This might arise from the taste not having received timely development; or from physical impediments—it being not possible that Scott should have written his poetical romances, his lyrics, and prose passages in his works, without an instinct for musical form and cadence. So, too, every orator who moves his audience by eloquence as distinguished from convincing it by argument, must have musical feeling: else would his periods halt.—On the other hand, there have been poets skilled in music, whom the excess of their knowledge has led into license, and seeming irregularity of versification, only to be overcome by the most exquisite adroitness on the part of the reader. Mr. Browning and Leigh Hunt are instances.

As an art, Music has always had conditions and caprices of its own as remarkable as its connection with other arts. Melody, as we understand it, is modern. The music of the Hebrews and the Greeks is, so far as we know, semi-barbarous. Even the music produced in Italy during the great era of Italian painting bears no proportion to the perfection of the other art.

Some of the requisites of the poetry fitted for music, as an art, were named. There must be beauty of thought and imagery without super-subtlety. Passion must be not too much interrupted; description not overlaid by superfluity of detail. Language must be clear, sonorous, avoiding alike bombast and familiarity—the sentences intelligible as they pass, and the phrases, however varied, bearing a proportion one to the other. These rules apply to the poetry of all nations; but the nationalities of Italy, Germany, and France, have influenced their application characteristically.—In Italy, the great classical poets are not susceptible of musical treatment. The minor ones, and those who wrote for the stage, have arranged the commonest sentiments in the simplest manner; regarding the vowels more than the ideas, and the display of the voice rather than the expression of original sentiment. The language of Italian comic opera admits and encourages positive dissonance of sound pronounced rapidly.—In Germany, so soon as the school of national art began to separate itself from that of Italy, a desire for something deeper in poetry, and something more precisely expressive in music, began to exert itself; sometimes, however, pedantically, in a *bit-by-bit* expression of every word as it rose, which is false in taste, and tending towards cumbrousness.—In France, all the fine arts are singularly self-consistent; largely indebted to foreigners, who have been, nevertheless, obliged, one and all, to conform, in order to hold their ground. As in their architecture, painting, and drama, point and piquancy, sometimes at the expense of beauty and simplicity, are indispensable; the compound as a whole being complete, however artificial.

In England the poets, from Chaucer downwards, are a richer, various, and more numerous choir than those of any other country. Not so its musicians. The national melodies of Great Britain and Wales are full of beauty and interest; but these, and the ballad poetry recited to them, are not so much works of art, as materials for art.

Instances were given to prove to how many different uses the same tune might be turned. The first union of poetry with regular music, with which it is expedient to deal, is to be found in the works of the Elizabethan madrigalists. Here the music is better than the poetry. The latter is too euphuistic; but with this character of conceit and quaintness the music also may be charged. Allowing for effects, inevitable to the intricate style of composition, the English madrigals are as tunable and sonorous as the Italian madrigals of the same period.

Shakspeare's lyrics are models of words for music, complete in themselves; and, when musically read by a speaker of refinement (Mrs. Fanny Kemble instanced), entirely contenting the ear;—yet lending themselves, without the slightest loss of freedom or sense of incumbrance, to a clothing as exquisite and ingenious as that given by Mendelssohn to the "Midsummer Night's Dream."—Milton's poetry is no less admirable in this respect; more symmetrical even, perhaps, than Shakspeare's, owing to Milton's musical training—and furnishing, as in "Comus," "Samson Agonistes," and "L'Allegro," a more continuous text. Thus Milton's verse was sought as text by two of the greatest foreign musicians—Handel and Haydn. Ben Jonson's lyrics, though excellent, full of rare fantasy and largely sought by English musicians, are less eligible than those of Milton and Shakspeare, from their being more far-fetched in meaning and imagery. Some of them, however, set by Horsley, are among the best specimens of English music that we possess.—Cowley was touched on as having given to music some verses of rare sweetness.—Dryden, at a time of decadence and tawdry taste, wrote notably for music; some of his stage lyrics, as set by Purcell, are full of animation and colour; and his "Alexander's Feast," set a second time by Handel, is the finest ode for music in honour of St. Cecilia existing; and Handel's setting of it, the best *Cantata* ever written.—Congreve, Gay, Carey, all wrote well for the musician; though, after Purcell's date, while the art of music was enriching itself, and taking new forms abroad, its nationality was on the decline at home, owing to the overwhelming splendour of foreign talents, and the disrespect into which it had fallen among the wits of the eighteenth century. Yet, even at that time, when poetry was more didactic and satirical, and later, when the Johnsonian influences prevailed, the book of English poetry was never without its good inspirations for the refined musician. Gray and Mason contributed—Cowper even tried his hand.—The canzonets set by Jackson, of Exeter, were instanced as bearing comparison with those of any other country.

Among the poets flourishing at the close of the last and the commencement of the present century, Burns was passed with a word; since of his tunefulness there had never been any question at home or abroad.—Coleridge, though among the most richly musical of poets, is not therefore a good poet for the musician's use; his verse being too mellifluous, too thoroughly charged with sweetness of its own, to bear a single additional touch.—Southey, too, is ineligible for music, owing to that certain coldness which has stood between him and a wide and genial popularity. Yet the unrhymed verse of "Thalaba," by the artful variety of its structure, and the sonority of its words, is a model for recitative.—Wordsworth is too meditative, too calm, to invite any union with an art that renders pondering on the part of the hearer impossible without damage.—The rank of Byron among modern poets for music has hardly been rated high enough. Not merely his songs, but the choruses in his lyrical dramas, and the descriptive passages in his poems, are everywhere suggestive and lend themselves readily to illustration.—Shelley's poetry, though much sought after, is less excellent for the use in question; his lyrics are generally too mystical, too dreamy, and when clearer in expression, as the "Ode to the Skylark," too thickly crowded with gorgeous and changeful imagery to endure any addition of music, without conclusion and loss of power resulting. But Shelley's dreaminess was voluntary; since of vigorous, direct language, a more forcible example could not be cited than his tragedy of the "Cenci," from its first to its last line.—Campbell was a great and versatile lyrist for music: witness his "Mariners of England"—witness his "Exile of Erin;" both become standard poems unequalled, in their way, by English or Irish poets. His longer poems, too, have yielded much good text for the musician, —even in so gloomy and solemn a lyric as "The Last Man."—Joanna Baillie is prominent among poetesses, not merely for the passion and power in certain of her tragic scenes, but as a writer for music. Her drama of "The Beacon" claims express and respectful study, with regard to the subject under treatment.—Moore was a born musician, and one through whose every work the influences of the art were felt. With faults of taste, tending to what is too artificial and ornate, his artistic organization enabled him to perform feats with our language which passed unnoticed, such as a liberal use of the letter S, and the writing of verse full of meaning and poetry, with a syllable to a note, —no effect of heaviness being thereby produced.

Living writers being left without mention, it remained to be pointed out how false ideas on the subject of English poetry as ineligible for music had been perpetuated and spread.—The English musicians, as a body, have been too imitative, too careless in selection, too regardless in treating their words;—the English singers, as a body, have delivered the latter too unintelligibly and inelegantly, owing to a neglect of the study of the English language. The discourse of Professor D'Orsey, given February 1st, was referred to, in corroboration of the speaker's views, and foreign singers from four countries were instanced, who, owing to the necessity imposed upon them of attentively studying their words before they sang the same, pronounced with a purity, elegance, and clearness too seldom attained by our native professors.—The speaker, in conclusion, bespoke indulgence for the imperfection hardly to be escaped from when treating a subject so full of speculation and fact within the limits of a single discourse. [H. F. C.]

WEEKLY EVENING MEETING,

Friday, February 22, 1861.

Sir Roderick I. Murchison, D.C.L. F.R.S. Vice-President, in the Chair.

Professor Faraday, D.C.L. F.R.S.

On Platinum.

The discourse was founded on the recent investigations of MM. Henri Ste-Claire Deville and H. Debray regarding the characters and conditions of the platiniferous metals, and the new process of working the ore which they have established on their results. Wherever platinum occurs, it is usually, if not always accompanied by five other remarkable metals; namely, Ruthenium, Osmium, Iridium, Rhodium, and Palladium: and in addition, by other substances, as iron, copper, gold, silver, and sand. Being washed, the heavy particles are left as the general ore of platinum; this metal constituting by far the largest part of the substances.

The six metals, when obtained apart and purified, form two groups of three each; each group having an equivalent number very different from that of the other group, as appears in this table:—

Equivalent number, 95·5.			Equivalent number, 53.		
1. Osmium .	Spec. grav.	21·40	2. Ruthenium .	Spec. grav.	11·3
3. Iridium	„	21·15	4. Rhodium	„	12·1
5. Platinum . . .	„	21·15	6. Palladium . . .	„	11·8

The three in the first group have the same equivalent number, and nearly the same specific gravity; but osmium takes the place of platinum as the heaviest of bodies. The equivalent number of the second group is alike for all, but it is little more than half that of the former group. The specific gravity also of the group is little more than half that of the former group: from which it results that an equivalent of any of these will have very nearly the same volume as an equivalent of any one of the heavier group.

There are certain analogies between 1 and 2; 3 and 4; 5 and 6; platinum is more like palladium than like the other metals. These numbers also represent the order of fusibility. Osmium has not as yet been fused; the rest have, in the order given. Platinum appears among them as a comparatively easily fusible metal. They are all volatile at very high temperatures, even osmium disappearing whilst the mass remains solid.

The platinum has usually been obtained from these ores (after they have been well washed, sifted, and mechanically separated) by the action of nitro-muriatic acid; which, bringing the platinum into solution, supplies a fluid which, on the addition of muriate of ammonia, &c., throws down a precipitate of ammonio-chloride of platinum. This, washed, dried, and heated, gives spongy metallic platinum; which being then pressed, heated, and hammered, yields massive platinum; the aggregation of the particles taking place entirely by adhesion and welding. Instead of forming a solution by acids, Deville proposes to employ a heat fluxion process; and instead of welding, to fuse the metal together at the last by intense heat, obtained by the use of the oxy-hydrogen or the oxy-coal-gas blowpipe. The ore, properly prepared, is mixed with its weight of galena, or native sulphuret of lead, and half its weight of metallic lead; it is then heated and well stirred together, the iron and some other metals are taken up by the sulphur of the galena, the platinum and other metals are taken possession of by the lead, and when the action is well effected, the access of air is adjusted until the remaining part of the sulphuret is decomposed, and only platiniferous lead left at the bottom part of the crucible or furnace, with scoriæ upon it. The former is separated, and then heated, exposed to air until much of the lead is oxidized; which, escaping as litharge, leaves at last an alloy of lead and platinum, containing not more than 10 or even 5 per cent. of lead. Such an alloy of platinum requires a very high temperature to fuse it, and this is therefore attained and applied in furnaces constructed of chalk-lime, heated by the insertion of gas blowpipes. The heat first melts the alloy, and being combined with oxygen in a little excess, the remaining lead is rapidly oxidized and dissipated in fumes, and then being raised and continued, any gold, copper, osmium, or other metals, except iridium and rhodium, are also converted into vapour and driven off. The platinum remaining is at last heated to a still higher degree, and is either cast into flat cakes or granulated; and this has been done with quantities weighing even as much as 40 lbs.

The resulting metal contains some iridium and some rhodium, being in fact an alloy of platinum; but it is an alloy which being harder than platinum, and even less liable than it to the chemical action of acids and other chemical agents, is as useful as the pure substance in the ordinary applications of the metal. As iridium and rhodium have no employment at present better than that of alloying platinum, their quantity has been purposely increased until it has made as much as 25 per cent. of the mass.

A mixed process has been devised by MM. Deville and Debray, which gives a platinum purer than any heretofore obtained. It is then as soft and ductile as silver. But for this process, for general directions and minute particulars, and for most interesting matter about all the metals of the platinum group, the reader is referred to Volumes LVI. and LXI. of the *Annales de la Chimie.*

[M. F.]

WEEKLY EVENING MEETING,

Friday, March 1, 1861.

SIR HENRY HOLLAND, Bart. M.D. F.R.S. in the Chair.

HENRY ENFIELD ROSCOE, Esq.

PROFESSOR OF CHEMISTRY IN OWEN'S COLLEGE, MANCHESTER.

On Bunsen and Kirchhoff's Spectrum Observations.

THE speaker commenced by stating that the researches of Bunsen and Kirchhoff, which he had the honour of bringing before his audience, marked a new era in the science of Analytical Chemistry; that by means of these discoveries the composition of terrestrial matter becomes revealed to us with a degree of accuracy and delicacy as yet unheard of, so that chemical elements supposed to be of rare and singular occurrence, are shown to be most commonly and widely distributed, and on the first practical application of this new method of analysis two new and hitherto undetected alkaline metals have been discovered.

The importance of these researches becomes still more strikingly apparent, when we hear that the conclusions derived from them outstep the bounds of our planet, enabling us to determine with all the certainty of definite experiment the actual presence of a number of elementary bodies in the sun.

The colours which certain bodies impart to flame, have long been used by chemists as a test for the presence of such bodies. Thus soda brought into a colourless flame produces a bright yellow light, and substances containing soda in any form give this yellow colour. Potash gives a violet flame, lithia and strontia impart to flame a crimson colour, whilst salts of barium tinge it green. These colours are produced by the incandescence or luminosity of the heated vapour of the various bodies placed in the flame. It is only because these substances are volatile, or become gases at the temperature of the flame, that we observe the peculiar colour. If any substance, such as platinum, which is not volatile at the temperature of the flame, be placed in it, no coloration is observed. The higher the temperature of the flame into which the same substance is placed, the greater will be the luminosity; and the more volatile the salt of the same metal, the more intense is the light produced.

Heated to the point of incandescence in any other manner, the vapours of these metals and their salts give out the same coloured light. Thus, if we burn gun-cotton, or gun-paper, steeped in solutions of these various salts, we get the characteristic colours. The well-known coloured fires owe their peculiar effects to the ignition of the vapour of some particular substance. Thus, in red fire we have strontium, in green fire we have barium salts present in the state of luminous vapour.

These facts have long been known and applied; but it was reserved for Bunsen and Kirchhoff to place these beautiful phenomena in their true position, to apply to them the modern methods of exact research; and thus to open out a new and rich field for most important investigations. This they accomplished in a most simple and beautiful manner, by examining these coloured flames, not by the naked eye, but by means of a prism or an apparatus for separating, decomposing, or splitting up the light produced by the incandescent vapour into its different constituent parts.

If we pass white sun-light through a prism, we get the well-known solar spectrum discovered by Newton. The red, or least refrangible rays appear at one end, and we pass through all gradations of colour—noticing on our way certain dark lines or spaces, showing the absence in solar light of some particular rays, lines with which we shall have much to do—until we arrive at the violet, or most refrangible end of the spectrum. If instead of using white sun-light, we pass the rays from the yellow soda flame through the prism, we get the soda spectrum; and we find that instead of a continuous spectrum, all we see is one bright yellow line, showing that every kind of light except that bright yellow ray, is absent in the soda flame; or that the soda flame gives out only one *kind* of light.

And as each metal, sodium, potassium, lithium, calcium, strontium, barium, &c., communicates a distinct tint to flame, so each gives a distinct and characteristic spectrum, consisting of certain bright coloured lines, or bands of light of the most peculiar form and tint.

The actual spectra of these metals can be beautifully seen in the simple apparatus designed by Bunsen and Kirchhoff.

In each spectrum of these metals, the form, number, position, colour, and tone of the bright lines remain perfectly constant and unvarying, so that from the presence or absence of one of these lines, we may with absolute certainty draw conclusions respecting the presence or absence of the particular metal, as we know of no two substances which produce the same bright lines. None of the bright lines produced by any one metal interfere in the least with those of any other, and in a mixture of all these metallic salts together, each ingredient can thus be easily detected.

As an example of the exactitude with which a very small quantity of a most complicated mixture can thus be analyzed, the speaker quoted Bunsen's words. "I took," says Bunsen, "a mixture of chloride of sodium, chloride of potassium, chloride of lithium, chloride of calcium, chloride of strontium, chloride of barium, containing at most $\frac{1}{10000}$ part of a grain of each substance. This mixture I put into the flame, and observed the result. First, the intense yellow sodium line appeared, on a background of a pale continuous spectrum; as this began to be less distinct, the pale potassium lines were seen, and then the red lithium line came out, whilst the barium lines appeared in all their vividness. The sodium, lithium, potassium, and barium salts were now almost all volatalized, and after a few moments the strontium and calcium lines came out, as from a dissolving view, gradually attaining their characteristic brightness and form."

We can thus detect the most minute traces of any one of these bodies, if mixed with the largest quantities of any other substance. The delicacy and accuracy of these reactions is without parallel, as is seen from the following statements:—

1. *Soda* $\frac{1}{3,000,000}$ part of a milligramme, or $\frac{1}{180,000,000}$ part of a grain of soda can be detected. Soda is always present in the air. All bodies exposed to air show the yellow sodium line. If a book be dusted near the flame the soda light can be seen.

2. *Lithia* $\frac{1}{100,000}$ part of a milligramme, or $\frac{1}{60,000,000}$ part of a grain of lithia can easily be detected. Lithium was only known to occur in four minerals. It is now found by spectrum analysis to be one of the most widely distributed elements. It exists in almost all rocks; it has been found in 3 cubic inches of sea, river, and Thames water; in the ashes of tobacco, and most plants; in milk, human blood, and muscular tissue.

3. *Strontia* $\frac{6}{100,000}$ of a milligramme, or $\frac{1}{1,000,000}$ parts of a grain of strontia can easily be detected.

4. *Lime* $\frac{1}{100,000}$ of a milligramme, or $\frac{1}{1,000,000}$ of a grain may be easily detected.

In examining the spectra of the alkalies obtained from certain mineral waters, Bunsen observed the occurrence of two bright blue lines which he had not seen before, when he examined alkalies from other sources. Hence he concluded that these bright lines must be produced by a new, hitherto undetected, alkaline metal. Subsequent search proved the validity of the supposition. The new metal was found and isolated. The analogy between this discovery and a celebrated one in another branch of physical science, will be at once understood. As Adams and Leverrier discovered Neptune, so Bunsen discovered "*Cæsium*" by the perturbations produced in the spectra of the other alkaline metals.

This is, however, not all. A few days ago the speaker received a letter from Bunsen, which contains the following most interesting information:—"The substance which I sent you as impure tartrate of Cæsium contains *a second* new alkaline metal. I am at present engaged in preparing its compounds. I hope soon to be able to give you more detailed information concerning it. The spectrum of the new metal consists of two splendid red lines situated beyond the red line K α in the ultra red portion of the solar spectrum. Hence I propose to call the new metal '*Rubidium.*'"

That this same method of investigation can be extended to all the metallic elements is more than probable, for Kirchhoff writes—"I have assured myself that even the metals of the rarest earths, as yttrium, erbium, and terbium, can be most quickly and certainly determined by help of the spectrum analytical method."

Experiments are being carried on with the view of making this mode of examination practically applicable to all metals.

To turn, now, to the second, and, if possible, to the more interesting part of the subject, namely, the conclusions drawn from these observations respecting the composition of the sun's atmosphere. The solar spectrum invariably contains a large number of dark lines, or spaces, or shadows. These have been called Fraunhofer's Lines, from the name of their discoverer. They show us that in the sun's light certain kind of rays are wanting; and as these lines are always present, exactly in the same position, we see that certain kinds of rays are always absent in solar light. There are many thousands of these lines in the whole length of the spectrum. Only a few have been, as yet, mapped and named.

What is the cause of these constant dark lines? And we must remember that it is in sunlight alone that these particular lines occur; in the light of the fixed stars, as well as in artificial lights, other lines are found. It is the discovery of this cause by Kirchhoff which gives the subject such peculiar interest, as it enables us to draw conclusions respecting the composition of the sun's atmosphere. The points of the case are put as concisely as possible under the following heads:—

1. The solar spectrum invariably contains certain fixed *dark* lines, called Fraunhofer's Lines.

2. The spectra produced by the luminous vapour of all metals contain certain fixed *bright* lines, invariable, and distinct for each metal.

3. All and each of the bright lines thus produced by certain metals—viz. sodium, potassium, magnesium, and iron—are found to coincide exactly with certain of the dark lines of the solar spectrum.

4. Hence there must be some connection between the bright lines of the metal, and the dark solar lines.

5. The connection is as follows:—Each of the dark fixed lines in the solar spectrum is caused by the presence in the sun's atmosphere of the luminous vapour of that metal which gives the coincident bright line.

By taking a special case we may more easily understand the matter. Let us examine the question why it is to be concluded that *Sodium* occurs in the sun's atmosphere? In the following sentences the reasoning on this subject is rendered clear:—

1. The light emitted by luminous sodium vapour is homogeneous. The sodium spectrum consists of one double bright yellow line.

2. This bright double sodium line is exactly coincident with Fraunhofer's dark double line D.

3. The spectrum of a Drummond's Light (like that of all incandescent solids) is continuous. It contains no dark lines or spaces.

4. If between the prism and the Drummond's Light a soda flame be placed, a dark double line identical with Fraunhofer's dark double line D is produced.

5. If instead of using Drummond's Light we pass sunlight through the soda flame, we see that the line D becomes much more distinct than when sunlight alone is employed.

6. The sodium flame has, therefore, the power of absorbing the same kind of rays as it emits. It is opaque for the yellow "D" rays.

7. Hence we conclude that luminous sodium vapour in the sun's atmosphere causes Fraunhofer's dark double line D. The light given off from the sun's solid body producing a continuous spectrum.

8. In a similar manner the presence in the solar atmosphere of potassium, iron, magnesium, nickel, and chromium has been proved.

Kirchhoff's own words may perhaps render this matter still more plain. "The sun," says Kirchhoff, "consists of a glowing gaseous atmosphere, surrounding a solid nucleus which possesses a still higher temperature. If we could see the spectrum of the solar atmosphere without that of the solid nucleus, we should notice in it the bright lines which are characteristic of the metals it contains. The more intense luminosity of the internal nucleus does not, however, permit the spectrum of the solar atmosphere to become apparent; it is *reversed* according to my newly discovered proposition; so that, instead of the *bright* lines which the luminous atmosphere by itself would have shown, *dark* ones appear. We do not see the spectrum of the solar atmosphere itself, but a negative image of it. This case, however, with an equal degree of certainty serves to detect the metals present in the sun's atmosphere. All that we require for this purpose is a very accurate knowledge of the solar spectrum, and of the spectra of the individual metals."

Kirchhoff is at present engaged in continuing these observations; and although only eighteen months have elapsed since the first discovery was made, he has already mapped more than seventy lines in the solar spectrum, between D and E, which are produced by iron. He has shown that the well-known group in the green, known as *b*, is caused by magnesium, whilst other coincident lines prove the presence of nickel, chromium, potassium, and sodium in the solar atmosphere.

The speaker regretted that he was unable to show even a drawing of these coincident lines, as no representation of them has yet been completed.

The lines produced by many metals possessing very distinctly marked spectra are seen to coincide with *none* of the dark solar lines;

and hence the conclusion is drawn, that these metals—for instance, silver, copper, zinc, aluminium, cobalt, lead, and antimony—do not occur at all, or at any rate occur only in very small quantities in the sun's atmosphere.

The speaker said that he should not soon forget the impression produced on his mind when visiting his friends in Heidelberg last autumn, by seeing the splendid spectacle of the coincidence of the bright lines of the iron spectrum with the dark solar lines. In the lower half of the field of the telescope were at least seventy brilliant iron lines of various colours, and of all degrees of intensity and of breadth; whilst in the upper half of the field, the solar spectrum, cut up, as it were, by hundreds of dark lines, exhibited its steady light. Situated *exactly* above each of the seventy bright iron lines was a dark solar line. These lines did not only coincide with a degree of sharpness and precision perfectly marvellous, but the intensity and breadth of each bright line was so accurately preserved in its dark representative, that the truth of the assertion that iron was contained in the sun, flashed upon the mind at once.

The speaker concluded by remarking that these researches are still in their earliest infancy; that the dawn of a new stellar and terrestrial chemistry has been announced, thus opening out for investigation a bright prospect of vast fields of unexplored truth.

[H. E. R.]

GENERAL MONTHLY MEETING,

Monday, March 4, 1861.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

H.R.H. The Count of Paris.
H.R.H. The Duke of Chartres.
Wm. C. Baker, Esq.
Henry Wollaston Blake, Esq. F.R.S.
Charles Buxton, Esq. M.P.
Charles Sedgfield Crowley, Esq.
John Clerk, Esq.
Henry Gueneau De Mussy, M.D.
Joseph S. Earle, Esq.
Sir Philip De Malpas Grey Egerton, Bart. M.P. F.R.S.
John Fleming, Esq.
The Rev. Charles L. Forster, M.A.
Richard Clewin Griffith, Esq
Sir John Edward Harington, Bart.
Alexander Mackintosh, Esq.
William Nichols, Esq. B.A.
Henry Alfred Pitman, M.D.
Mrs. Fanny Pitman.
W. H. Stone, Esq.
Wm. E. M. Tomlinson, Esq.
Thomas Harrington Tuke, M.D.
John Waring, Esq.

were *elected* Members of the Royal Institution.

With reference to certain Resolutions passed at the previous meeting, the following Letter from the Rev. JOHN BARLOW, the late Secretary, was read:—

5, BERKELEY STREET,
Feb. 4*th*, 1861.

DEAR DR. BENCE JONES,

In Mrs. Barlow's name, as well as my own, I request that you will express to the Members of the Royal Institution our grateful acknowledgment of the kind act to her, and the not less kind words to myself, by which they have unanimously recorded their estimation of my past services. The Members have referred to those services as effectual. Let me assure them, in all sincerity, that very much of this efficiency is owing to themselves. But for the ever willing co-operation of the Professors, the unvarying support of the Managers and Visitors, and the uniform confidence reposed in me by the Members, the best endeavours I could exert in their service must have been fruitless. I thankfully accept the cordial wishes of the Members for my health and happiness. It is, indeed, a subject of happy reflection, that I am conscious of not having altogether laboured in vain to carry out the principles on which the Royal Institution has acted for more than sixty years. These principles are not likely to be abandoned. Possessed of increasing means and influence, this Institution will, under the guidance of my successor, be more conspicuous than ever, for that promotion of science, and that co-operation and sympathy with those who devote themselves to the pursuit of science, which have already obtained for our society a distinguished pre-eminence over all similar associations in the world.

Believe me, dear Dr. Bence Jones,
Very truly yours,
JOHN BARLOW.

DR. BENCE JONES,
Secretary, Royal Institution.

The following Arrangements for the Lectures after Easter 1861, were announced:—

Three concluding Lectures on FISHES—by RICHARD OWEN, Esq. D.C.L. F.R.S. Fullerian Professor of Natural Physiology, R.I.

Three concluding Lectures on ELECTRICITY—by JOHN TYNDALL, Esq. F.R.S. Professor of Natural Philosophy, R.I.

Six Lectures on MODERN MUSIC—by JOHN HULLAH, Esq.

Six Lectures on the DEVONIAN AGE OF THE WORLD—by WILLIAM PENGELLY, Esq. F.G.S.

Nine Lectures on the SCIENCE OF LANGUAGE—by MAX MÜLLER, Esq. Taylorian Professor, Oxford.

Two Lectures on MUSICAL ACOUSTICS and on the PHYSIOLOGICAL and PSYCHOLOGICAL CAUSES of MUSICAL HARMONY and DISCORD, by PROFESSOR HELMHOLTZ, of Heidelberg.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same: viz.

FROM

Board of Admiralty—(through J. Russell Hind, Esq.)—The Nautical Almanack, for 1861, 1862, 1863, and 1864. 8vo.

Amussat, M. Alph.—Recherches sur l'Introduction Accidentelle de l'Air dans les Veines. 8vo. 1839.
Arts, Society of—Journal, Feb. 1861. 8vo.
Botfield, Beriah, Esq. M.P. F.R.S. M.R.I. (the Editor)—Præfationes et Epistolæ Editionibus Auctorum Veterum præpositæ. 4to. 1861.
Chemical Society—Journal, No. 52. 6vo. 1861.
Editors—American Journal of Science, by B. Silliman, &c. for Feb. 1861. 8vo.
Artizan for Feb. 1861. 4to.
Athenæum for Feb. 1861. 4to.
Chemical Gazette for Feb. 1861. 4to.
Engineer for Feb. 1861. fol.
Horological Journal, No. 30. 8vo. 1861.
Journal of Gas-Lighting for Feb. 1861. 4to.
Mechanics' Magazine for Feb. 1861. 8vo.
Medical Circular for Feb. 1861. 8vo.
Practical Mechanic's Journal for Feb. 1861. 4to.
Faraday, Professor, D.C.L. F.R.S.—Mémoires de l'Institut de France; Académie des Sciences Morales, &c. Tome X. 4to. 1860.
Imperial Academy of Sciences, Vienna: Math.-Nat. Hist. Classe: Sitzungsberichte. 1860. Nos. 6, 13–21. 8vo.
Franklin Institute of Pennsylvania—Journal, Vol. XLI. Nos. 1, 2. 8vo. 1861.
Geological Society—Proceedings, Feb. 1861. 8vo.
Quarterly Journal, No. 65. 8vo. 1861.
Horticultural Society of London—Proceedings, No. 21. 8vo. 1861.
Institut Impérial de France—Mémoires de l'Académie des Sciences. Tome XXVIII. 4to. 1860.
Lankester, Dr. E. F.R.S. M.R.I. (the Author)—Lectures on Food. Part 1. 16to. 1861.
Lubbock, John, Esq. F.R.S. M.R.I. (the Author)—On Sphærularia Bombi and on the Daphnidæ. 8vo. 1861.
Macilwain, George, Esq. M.R.I. (the Author)—Remarks, Logical and Physiological, on Vivisection. 8vo. 1860.
Mackie, S. J. F.G.S. (the Editor)—The Geologist, Feb. 1861.
Manning, Frederick, Esq. M.R.I.—A Series of Views illustrative of the Boscobel Tracts. 4to. 1861.
Russell, J. Scott, Esq. F.R.S. (the Author)—The Fleet of the Future: Iron or Wood? 8vo. 1861.
Newton Messrs.—London Journal (New Series) for Feb. 1861. 8vo.
Photographic Society—Journal, No. 106. 8vo. 1861.
Royal Agricultural Society of England—Journal, No. 46. 8vo. 1861.
Vernon, W. F. Esq. M.R.I. (the Author)—Memorial of Admiral Vernon. 16to. 1861.

WEEKLY EVENING MEETING,

Friday, March 8, 1861.

The Rev. John Barlow, M.A. F.R.S. Vice-President, in the Chair.

Edward Frankland, F.R.S.

On some Phenomena attending Combustion in Rarefied Air.

The investigation forming the subject of this discourse had its origin in some experiments which the speaker made upon the summit of Mont Blanc, in the autumn of 1859, for the purpose of ascertaining the effect of atmospheric pressure upon the amount of combustible matter consumed by a common candle. He found, as the average of five experiments, that a stearin candle diminished in weight 9·4 grammes, when burnt for an hour at Chamonix; whilst it consumed 9·2 grammes, when ignited for the same length of time on the summit of Mont Blanc. This close approximation in the quantity of combustible matter consumed under such widely different atmospheric pressures, goes far to prove that the rate of combustion is entirely independent of the density of the atmosphere. This result was subsequently confirmed by a repetition of the experiments in air, artificially rarefied, until it supported a column of only 9 inches of mercury.

In burning the candles upon the top of the mountain, it was noticed, in the subdued light of the tent in which the operation was performed, that their luminosity was much less than usual. The lower and blue portion of the flame, which, under ordinary circumstances, scarcely rises to within a quarter of an inch of the apex of the wick, now extended to the height of one-eighth of an inch above the cotton, thus greatly reducing the size of the luminous portion of the flame; and, on subsequently repeating the experiments in artificially rarefied atmospheres, and measuring the amount of light emitted in each case, it was found that as the rarefaction proceeded, the blue or non-luminous portion of the flame gradually extended upwards until it finally expelled, as it were, the yellow or luminous part even from the apex of the flame. During the progress of the rarefaction, the flame became somewhat enlarged, assumed an ellipsoidal shape, and ultimately became almost globular, whilst a large external shell of bluish pink flame gradually came into view as the last portion of yellow light was disappearing from the apex of the flame, which had alone been previously visible. It is scarcely necessary to add, that during these changes in the flame, the light underwent a rapid diminution; the rate of its decrease, however, was subject to considerable irregularities from the heating of the apparatus surrounding the candle, and the consequent guttering and unequal combustion of the latter. For the accurate measurement of the diminution of light, therefore, recourse was had to coal gas, which, although also liable to certain disturbing influences, yet yielded results, during an extensive series of experiments, exhibiting sufficient uniformity to render them worthy of confidence.

By passing the gas through a "governor," uniformity of pressure in the delivery tubes could be secured; and by other appropriate arrangements a uniform amount of gas, viz. 0·65 cubic feet per hour, was made to burn in each experiment within the atmosphere of varying density. This experimental flame was placed at one extremity of a Bunsen's photometer; whilst, as a standard for comparison, a similar jet of gas, surrounded by a glass shade, and burning freely in the air with a uniform consumption, was fixed at the opposite end of the photometer. In the case of the experimental flame, the products of combustion were completely removed, and a steady supply of fresh air constantly supplied.

The following table contains a summary of the results of these determinations, the illuminating power given under each pressure being the average of twenty closely accordant observations. In each series the maximum illuminating effect—that is, the light given by the experimental flame when burning under the full atmospheric pressure—is taken at 100.

FIRST SERIES.

Pressure of Air in inches of Mercury.	Illuminating Power of Experimental Flame.	
	Observed.	Calculated.
29·9	100	100
24·9	75·0	74·5
19·9	52·9	49·0
14·6	20·2	22·0
9·6	5·4	− 3·5
6·6	·9	−18·8

SECOND SERIES.

Pressure of Air in inches of Mercury.	Illuminating Power of Experimental Flame.	
	Observed.	Calculated.
30·2	100	100
28·2	91·4	89·8
26·2	80·6	79·6
24·2	73·0	69·4
22·2	61·4	59·2
20·2	47·8	49·0
18·2	37·4	38·8
16·2	29·4	28·6
14·2	19·8	18·4
12·2	12·5	8·2
10·2	3·6	−2·0

An inspection of these results indicates that even the natural oscillations of atmospheric pressure cause a considerable variation in the amount of light emitted by gas flames. In order to determine these variations, the following special series of experiments was made, the pressures being very accurately ascertained by means of a water-gauge.

THIRD SERIES.

Pressure of Air in inches of Mercury.	Illuminating Power of Experimental Flame.	
	Observed.	Calculated.
30·2	100	100
29·2	95·0	94·9
28·2	89·7	89·8
27·2	84·4	84·7

It is thus evident that the combustion of an amount of gas which would give a light equal to 100 candles, when the barometer stands at 31 inches, would afford a light equal to only 84·4 candles if the barometer fell to 28 inches.

The results of these three series of observations taken together, show, that beginning at atmospheric pressure, and with 100 units of light, a decrease of almost exactly 5·1 units of light is the result of each diminution of mercurial pressure to the extent of one inch, until the barometer stands at 14 inches, below which the diminution of light takes place in a less rapid ratio. One of the columns headed "Calculated," in the above tables, exhibits the illuminating power calculated from the constant just given, and it will be seen, that these calculated numbers nearly coincide in most cases with the observed amount of light.

In explaining the cause of the above phenomena, the speaker referred to the conditions upon which the light of ordinary flames depends. He showed, that in these flames there are two sources of light, viz. incandescent gaseous matter, and incandescent solid matter; but that, practically, 99 per cent. of the light of such flames owed its origin to the second of these sources. In gas, candle, and oil flames, the incandescent solid matter consisted of carbon in a minute state of division. The amount of light emitted by these flames depended, within certain limits, first, upon the quantity of solid particles of carbon existing, at any given moment, within the flame; and secondly, upon the temperature to which these carbon particles were heated. Now, the temperature of a flame might be affected by imperfect combustion in rarefied air; but it had been proved, by the analysis of the products, that combustion was equally complete in the above experiments under all pressures; in fact, it was found that complete combustion could be far more easily secured in rarefied air, than in air at the ordinary atmospheric pressure. Other experiments also showed, that the temperature of a flame was not materially affected by the pressure of the air in which it was burning; consequently, it was inferred that the diminution of luminosity in rarefied atmospheres, was not due either to imperfect combustion, or to reduction of temperature.

The diminution of light must therefore arise from the decrease of the amount of solid carbon separated within the flame; and this the speaker believed to be due to the admission of oxygen in larger quantities into the interior of the flame when the atmosphere was rarefied. It was shown by experiment, that the admission of a comparatively small amount of air, and consequently of oxygen, into the interior of a gas flame, immediately reduced the illuminating power of the latter to a very marked extent; the carbon particles, instead of being separated as such in the interior of the flame, being at once oxidized to carbonic oxide. This increased access of oxygen to the interior of a flame burning in rarefied air, was believed to be due to the greater mobility of the particles of expanded gases, which enabled the gases of the flame and the circumambient air to commingle more rapidly than at ordinary atmospheric pressure.

The cause of the less rapid decrease of the light of flames burning in atmospheres below 14 inches of mercurial pressure was due to the comparative prominence assumed by the light of the incandescent gaseous matters of the flame at such high stages of rarefaction; this gaseous illumination being affected by pressure to a much less extent than that afforded by incandescent carbon particles.

In his celebrated researches on flame, Davy had not overlooked the diminution of light by decrease of pressure, but he had not determined the diminution quantitatively nor indicated its cause.

The speaker stated in conclusion, that he had only yet imperfectly extended his inquiry to pressures higher than that of the atmosphere; but, so far as these experiments went, they appeared to indicate that the law which had been elicited for lower pressures, also held good for pressures above that of the atmosphere.

[E. F.]

EXTRA EVENING MEETING.

Monday, March 18, 1861.

Sir Roderick I. Murchison, D.C.L. F.R.S. Vice-President, in the Chair.

M. F. P. Du Chaillu.

Personal Narrative of his Travels in Western Central Africa.

[No Abstract given.]

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, March 15, 1861.

SIR HENRY HOLLAND, Bart. M.D. F.R.S. in the Chair.

LATIMER CLARK, Esq.

On Electrical Quantity and Intensity.

THE modifications of the strength of the electric current in dynamic electricity, and in the amount of charge in static electricity, are at present usually defined by the terms Quantity and Intensity. The speaker pointed out that the expression intensity, as ordinarily understood, really involved two perfectly distinct qualities, and dwelt on the advantage which would accrue to electrical science by the habitual separation of the complex idea of intensity into its two component parts, viz. that of *tension,* as propounded by Ohm in his celebrated mathematical investigation of the galvanic circuit; and that of *quantity,* as developed by Faraday in those valuable researches in which he established the definite quantitative character of electro-chemical decomposition and the action of electricity on the galvanometer. The term "tension," as here used, is intended to convey the same idea as the expression *electromotive force*, or as the term "electric potential," employed by Green and other mathematicians, and is entirely dissociated from the idea of quantity; both terms are equally applicable to electricity at rest or in motion.

The *quantity* of electricity, both in its static condition and in its motion through conductors, usually varies directly as the tension, and hence their joint effects have been ordinarily confounded together and attributed to one cause under the name of intensity; but since the tension and quantity do not, under all circumstances, vary in the same ratio, there exists an absolute necessity for their clear separation before any numerical reasoning can be founded on them. Cases of the independent variation of tension and quantity were shown, and it was pointed out that all the most striking properties of electricity, such as the decomposition of water and salts, the combustion of metals, the deflection of the galvanometer, the attraction of the electro-magnet, and the physiological effects of the current were really dependent, as regards their magnitude and energy, solely on the quantity of electricity passing. Their greater energy when the tension was increased, was an indirect effect, due not to that tension, but to the increased quantity which passed in a given time by reason of the increased tension. A galvanometer wound with a few turns of thick wire was shown to be deflected as powerfully by one cell as by six, or even by 600 cells of the same size, because by reason of its shortness the wire conveyed freely the whole quantity which one cell could produce, which was the same as that produced by the whole 600; but any alteration in the size of the cell produced a consequent change in the quantity and in the deflection of the galvanometer. On the other hand, a galvanometer with many thousand turns of fine wire gave the same deflection with a battery formed of a small gun-cap, as with one of twenty square feet of surface, because the quantity in this case was regulated and limited not by the size of the plates, but by the power of conduction of the wire; the quantity being therefore the same in both cases. In every case the deflection was dependent solely on the quantity of electricity actually passing through the instrument without reference to its tension.

The combustion of metals was shown to be a phenomenon dependent on quantity, and not on tension; one cell of Grove's battery ignited a certain length of platina wire; and whatever its size it would ignite no greater length; but two, three, or more cells were shown to ignite two and three times the original length, the quantity passing in the greater length being under the higher tension, precisely the same as in the original length. This explained Faraday's oft misunderstood remark, that the same quantity of electricity which would ignite an inch of wire, would ignite a foot or a mile.

The pain and shock experienced on touching a powerful battery, or shocking coil, or Leyden jar, were proportionate to the quantity of electricity passing through the system, and not to the tension. A carrier ball or minute Leyden jar charged to the highest tension, would produce no sensation if the quantity were absent, and the same was the case with a Zamboni's pile. Sparks nearly eighteen inches long were received from an electrical machine; but although of this high tension, they produced no violent physiological effects, owing to their deficiency in quantity. Long sparks, nevertheless, produced greater effects than short ones, because at double the striking distance the tension is doubled, and the quantity is therefore twice as great. The quantity contained in a Leyden jar or battery is comparatively great, and the effect on the system proportionately violent. Two conditions were necessary for these effects: first, that the quantity present should be considerable; and secondly, that the tension should be sufficiently great to make it pass through the system. A battery of two or three cells, which could readily fuse platina wire, was shown to produce no painful sensation on the tongue, because, although the quantity was abundant, the tension was low; while another battery of 600 cells, which produced the most intolerable shock to the system, had, from its deficiency in quantity, scarcely any power to fuse wire. The

Ruhmkorff coil combined very high tension with considerable quantity, and its physiological effects were therefore very violent.

A frictional machine was exhibited by Mr. Varley, constructed on a plan of Dr. Winter's; the plate was of vulcanite, or vulcanized India rubber, about three feet in diameter, excited by amalgam in the usual way; its peculiarity was a large and lofty wooden ring, with a metallic rod in its interior, which, by its overshadowing inductive influence, increased the length of the sparks from six or seven inches to nearly eighteen.

The forces of electrical attraction and repulsion are sometimes stated to vary as the square of the intensity, sometimes as the square of the quantity, and sometimes as the square of the distance; but it was contended, that these effects were due to the circumstance that the quantity usually varies in the same ratio as the tension, and as the distance; and that all the phenomena were more rationally explained by the assumption that electrical attraction and repulsion vary in the simple ratio of the quantity and of the tension, and of the distance inversely.

The instances in which the quantity present is not simply dependent on the tension, are those in which other electrified bodies are present, which, by their inductive influence, affect the quantity present in all bodies in their vicinity without necessarily affecting their tension. An insulated cylinder was connected with the positive pole of a Daniell's battery of 600 cells, its negative pole being connected with the earth; so that the cylinder was in a condition to give off a powerful and visible current to another wire connected with the ground; in this condition a positively electrified disc was approached to it, and by its inductive influence was shown to render one end of the cylinder electrically negative, so that a carrier ball applied to that end showed it to have a negative charge, thus presenting the apparent paradox of a negative electrified body giving off a positive current to the earth, or *vice versâ*. One end was negatively electrified, and the other end positively, but the tension was the same everywhere.

According to the ordinary way of regarding this class of phenomena, it was usual to state that the ends of the cylinder acquired a state of positive or negative *intensity*, or that they had their intensity changed: it was contended that this gave an inaccurate idea of the real nature of the change, and that the approach of an electrified body, however near or however violently it might be excited, could not in the slightest degree affect the tension of a conducting body, which was in connection with the earth: the only influence it could have would be to alter the *quantity* in the second body, by driving a portion of its electricity downwards to the earth. It might be assumed as a law that *the tension of the electricity in every part of a conducting body of moderate dimensions was the same*, notwithstanding the vicinity of other electrified bodies. If a positively electrified body were brought near an insulated conductor, the distribution of the electricity in the second body was changed, and its whole tension was raised, but the tension remained everywhere uniform, and was as high at the negative as at the positive end.

The fall of tension in electricity was always accompanied by its conversion into heat; the ignition of wire by the voltaic current, the intense heat of the voltaic arc, and the heat and light of the electric discharge and of the spark, were all cases of the evolution of heat consequent on the fall of tension, and the quantity of heat evolved was apparently *directly proportionate to the fall of tension within a given space and to the quantity of electricity passing.*

In the case of electric telegraph conductors and submarine cables, it was shown, from a carefully conducted and extensive series of experiments, that the tension falls with the most perfect regularity from the positive pole of the battery to the end in connection with the earth, in accordance with the law of Ohm; and since the quantity of electricity held under induction varies in the same ratio as the tension, the distribution of the charge in a cable follows precisely the same law. It results from this, that if a cable with a current flowing through it be divided into any number of equal sections, and the quantity in the section connected with the earth be taken as unity, the quantities in all the other parts, whatever their number, will be in the ratio 1, 3, 5, 7, 9, 11, &c. So that if a cable be divided into two halves, the quantities will be in the ratio of one to three.

The speaker stated that he had ascertained that in the voltaic battery, the presence of two metals was not an essential condition—the negative metal was not necessary for the formation of the electric current, but only for its after detection and exhibition. A simple mass of copper, iron, zinc, or any oxidisable metal, when laid on the moist earth, formed a complete battery in itself, giving positive electricity to the earth, and quickly assuming a negative tension, which it would communicate to any other body resting on it or in contact with it, as, for instance, a length of submarine cable. If a cable thus charged were afterwards removed and applied to any more electro-negative metal, such as platinum, or to carbon, the charge would return to the earth; and it was found by measurement that the charge thus acquired by a cable was exactly the same as if the two metals had been employed simultaneously in the ordinary form of a galvanic couple. If the mass of zinc were permanently connected with the non-oxidisable or less oxidisable metal, and thus with the earth, the tension, being constantly destroyed and as constantly renewed, would form a constant current, becoming in fact a voltaic couple. If the connection with the earth, instead of being made through another metal, were made through any inert conducting substance or liquid, the same constant current would be produced, thus forming the well-known case of a voltaic battery with one metal and two liquids.

After the conclusion of the discourse, Mr. Clark stated, with reference to Faraday's discovery of specific inductive capacity, that in the course of some investigations conducted in conjunction with Professor Hughes, they had observed that every different dielectric possessed its own specific law of variation of inductive capacity with respect to distance. With air it varied as the distance, inversely; but with gutta-percha it was more nearly as the square root; with India-rubber and white wax it was intermediate between the two ratios, and the law of variation was different with every substance tried. From which it would result that observers who deduced the specific inductive capacity of any material from experiments on half-inch plates, would arrive at very different results from others who operated on inch plates.

[L. C.]

WEEKLY EVENING MEETING,

Friday, March 22, 1861.

The Rev. JOHN BARLOW, M.A. F.R.S. Vice-President, in the Chair.

PROFESSOR H. D. ROGERS, F.R.S. F.G.S. &c.

On the Origin of the Parallel Roads of Lochaber (Glen Roy), Scotland.

THE speaker prefaced his account of these curious features in the scenery of Lochaber, by stating that he was induced to recall attention to them from having, during four recent visits to the ground, discovered certain phenomena not hitherto noticed or theoretically considered by any of the able and distinguished observers who have preceded him. Though nearly all the more prominent peculiar characters of the scene have been very skilfully described and discussed by Dr. MacCulloch, Sir Thomas Dick Lauder, Charles Darwin, Esq., David Milne Home, Esq., Professor Agassiz, Sir George S. Mackenzie, Robert Chambers, Esq., and others, Professor Rogers has been led by a careful study of the structure of the so-called Parallel Roads, and a perusal of the views of those eminent geologists, to reject all the hypotheses thus far offered in explanation of the terraces as inadequate, and to recognize in the facts about to be developed, a key to a solution of the problem of their origin, which he thinks may prove satisfactory.

The geographical area of the parallel roads may be defined as embraced between Loch Laggan and Loch Lochy, east of the Great Caledonian Valley. They are chiefly restricted indeed to Glen Spean, Glen Roy, and two or three immediately adjacent smaller glens. One belt of them ranges from near Spean Bridge up the Spean Valley, to beyond the head of Loch Laggan; another up Glen Roy to the watersheds at its very head, and a third through Glen Gluoi to its head.

The "Roads," or Shelves, themselves, are of various heights above the sea, the lowest of the three conspicuous ones in Glen Roy having an elevation of about 850 feet, the middle one a height of about 1060 feet, and the highest a level of nearly 1140 feet. Other much fainter, still more elevated shelves, are discernible in Glen Gluoi, but all hitherto seen lie below a horizon of 1500 feet above the ocean. These Parallel Roads, as they are called, are apparently level, and therefore parallel, but further instrumental measurements are necessary before the question of their absolute horizontality can be regarded as satisfactorily settled.

They constitute a most impressive feature in the scenery of the lonely, treeless glens containing them. Winding into all the recesses and round the shoulders of the mountains which they imprint, they present at first view a striking likeness to a succession of raised beaches deserted by their waters.

Seen in profile, as when looked at horizontally, they resemble so many artificial hill-side cuttings, the back of each terrace lying within the general profile of the mountain slope, while the front or outer edge is protuberant beyond it. Each is indeed a nearly level, wide, deep groove, in the easily eroded boulder drift, or diluvium, which to a greater or less thickness everywhere clothes the sides of these mountains. They vary greatly in their relative distinctness, being in some places vaguely discernible, while in other spots they indent the surface very plainly, just as they happen to be narrow and to coincide in slope with the hill, or to be broad and apparently level from front to back. Where most indistinct they are frequently not discernible at all when we stand upon them; though we may in a favourable light have detected their position and course from the opposite side of the glen, or, better still, from the bed of the valley. The conditions which influence this fluctuation in distinctness, promise, if carefully observed, to dispel much of the obscurity which has hitherto invested the origin of the terraces. The modifying circumstances seem to be all referable to one general condition, that of exposure to a current or inundation, supposed by the speaker to have rushed through these glens from their mouths to their heads, or upper ends. Thus it would appear: 1st, With scarcely an exception, that each terrace or shelf is most deeply imprinted in the hill-side, and is broadest where the surface thus grooved has its aspect *down the glen* on towards the Atlantic, and is faintest where the ground fronts towards the head of the valley on the German Ocean. 2nd, While conspicuous on the open sides and the westward sloping shoulders of the hills, the terraces *disappear altogether* in the recesses or deeper corries which scollop the flanks of the mountains. 3rd, Each shelf, or "road," grows usually more and more distinct as it approaches the head of its own special glen, until those of the two opposite sides meet in a round spoon-like point.

A fact obviously material to a true theory of the origin of the terraces, is that each of them coincides accurately in level with some watershed or notch in the hills leading out from its glen into some other glen or valley adjoining, a coincidence suggestive of the notion that they were formed by the grooving agency of a flood pouring through the glens while it was embayed at the respective levels of these natural waste weirs. In confirmation of this view that they were transiently caused by erosive currents held successively at the heights of the barriers on whose levels the terraces terminate, we have as another interesting general feature, a remarkable ruggedness of the bed of each *external* glen just outside the water-shed or barrier closing the glen which contains the terrace. These rough and deep ravines, contrasting strikingly with the smooth spoon-like terminations of the terrace-lined glens which head against them, strengthen the suggestion already awakened by the marks of horizontal erosion in the terraces themselves, that the notches or passes which determined the grooving of the hill sides on their one hand were externally the sites of so many stupendous cataracts.

The internal structure or disposition of the matter composing each terrace, affords a further and striking corroboration of this hypothesis of the passage of an erosive flood. It consists in an "oblique lamination," or slant bedding of the constituents of the shelves; viz. the layers of gravel, sand, and other sediment, such as geologists familiarly recognize as the result of a strong *current* pushing forward the fragmentary material which it is depositing, and which is held by them to indicate in the direction towards which the laminæ dip, the direction towards which the current has moved. Now it is a most suggestive peculiarity in the oblique bedding of these terraces, that the "dip," or downward slant, is almost invariably *up* the glen, or towards its *head*, and *not down the glen*, or towards the Atlantic, as we must suppose it would have been, had the glen been a bay of the sea, and these materials but portions of ordinary sea beaches. Indeed, this feature is of itself enough to suggest an origin due to a strong current sweeping inward from the Atlantic, and across the water-shed of the island to the opposite sea.

The speaker next proceeded to examine the hypotheses of his predecessors in this inquiry respecting the origin of the Parallel Roads. They all assume the agency, in one form or another, of *standing* water, either the ocean in its ordinary state of repose, or lakes pent within the glens.

The notion that a quietly resting sea has fashioned these level shelves is refuted by the fact, that they are not true marine beaches; they exhibit none of the distinctive features of genuine sea-shores, not a vestige of any marine organic remains, no rippled sands, no shingle, and no sea cliffs. They display in like manner a total absence of the distinctive marks of lake sides; not one lacustrine organism, neither fresh-water plant, nor animal having ever been discovered imbedded in them. A further difficulty attends the lake-hypothesis in the necessity it imposes of discovering a feasible cause of blockage of the glens at different stations above their mouths, to pond the waters to the respective heights of the terraces. Though much ingenuity has been expended upon this part of the problem, no suggestions yet offered of barriers of gravel, accumulated by currents or glaciers from Ben Nevis, can be regarded as admissible, inasmuch as there are no traces of any such in any of those localities where alone we can assume them to have existed to produce the required embaying of the waters. In this entire absence of all remnants of the supposed natural dams across the glens, it is most unphilosophical to take for granted their total obliteration, where no cause has or can be assigned which can have so effaced them.

On the other hand the hypothesis of successive "sea margins," or sea levels, is overthrown by the now well-established deduction from the speaker's own recent measurements, that none of the several shelves, or "roads," of Glen Roy correspond in level with any of those seen in the adjacent valley Glen Gluoi, a marked discrepancy separating the two groups of terraces into two independently produced systems. It can be shown, moreover, that these discordances of interval between the shelves of the glens respectively, are such as cannot be accounted for on any supposition of "faults," or dislocations of the earth's crust, in the ground between the two glens. Equally incompatible are all the facts of the relative levels of the shelves, with the notion that they are possibly sea beaches which may have undergone an *unequal* amount of elevation by an oblique secular rise of the land, such as is known to be very gradually taking place on some coasts at the present day. The individual terraces are too nearly level to admit of this explanation; since so wide a warping of the crust from horizontality within so limited a space as separates the two glens, would have left them conspicuously sloping. Besides, the two systems of shelves are wholly insulated from each other, and the notion of their origin as sea beaches gradually elevated implies a continuity between them, together with certain agreements in their directions of derivation from levelness which we wholly fail to perceive.

In conclusion, the speaker proceeded to sketch the action to which he ascribes the formation of all these shelves or parallel roads. He supposes the several terraces to have been cut or grooved in the sides of the hills by a great inundation from the Atlantic, engendered by some wide earthquake disturbance of the ocean's bed, and forced against the western slope of Scotland. The features of the country indicate that, while a portion of such a vast sea-tide entering the Firth of Linnhe rushed straight across the island through the deep natural trench, Glen Mor, or the great Caledonian valley, a branch current was deflected from this, and turned by the Spean valley and its tributary glens, Glen Roy and Glen Gluoi, into the valley of the Spey, and so across to the German Ocean. In this transit, the deflected waters first embayed in these glens, and then filling and pouring through them, would, upon rising to the levels of the successive watersheds, or low passes, which open a way to the eastern slope of the island, take on a swift current through each notch, and as long as the outpour nearly balanced the influx, this current, temporarily stationary in height, would carve or groove the soft "drift" of the hill-side. But the influx increasing, the stationary level and grooving power of the surface stream would cease, and would only recommence when the flood rising to the brim of another natural dam, a new temporary equilibrium would be established, a new horizontal superficial current set in motion, and a second shelf or terrace begin to be eroded at the higher level. So each of the parallel roads is conceived to have been produced in the successive stages of the rising of one vast steady incursion of the sea. The lapsing back of the waters, unaccompanied by any sharp localized surface currents, through the passes, could imprint no such defined marks on the surface, nor accomplish more than a faint and partial obliteration of the terraces just previously excavated during their incursion. This procedure was elucidated by likening it to what takes place when we allow a steady but *gradually increasing* jet of water to flow into a tank, perforated laterally with several orifices at successive elevations, the outlets permitting a somewhat less rapid rate of discharge than is equivalent to the influx. If such a tank be smeared internally with soft clay, the inpour can be so regulated in respect to its acceleration, that the water, as it rises successively to the levels of the several orifices will take on a horizontal motion or current, through, first the lower hole, and then the second and so on, and, remaining approximately stationary for a brief while on the level of each, will groove the soft clay as it passes out, until it swells above the orifice to reach the next. Some such process as this at the notches which terminate the glens will, it is believed, account for the terraces and all the features which belong to them.

[H. D. R.]

GENERAL MONTHLY MEETING,

Monday, April 1, 1861.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President.

William Rutherford Ancrum, Esq.
Stephen Jennings Goodfellow, M.D.
William Newmarch, Esq.

were *elected* Members of the Royal Institution.

Rev. Alexander Denny, M.A.
Rev. Charles Forster, M.A.
Henri Gueneau De Mussy, M.D.
W. E. M. Tomlinson, Esq.

were *admitted* Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same : viz.

American Academy of Arts and Sciences—Memoirs, Vol. VII. 4to. 1860.
Arts, Society of—Journal, March 1861. 8vo.
Bache, A. D. Professor (the Author)—Report of the United States' Coast Survey, 1858. 4to. 1859.
Boston Society of Natural History, U.S.—Journal, Vol. VII. No. 1. 8vo. 1859.
Proceedings, Vol. VII. Nos. 10–15. 8vo. 1860.
Clifford, C. Esq. (the Author)—How to Lower Ships' Boats. 8vo. 1859.
Editors—Artizan for March 1861. 4to.
Athenæum for March 1861. 4to.
Chemical Gazette for March 1861. 4to.
Engineer for March 1861. fol.
Horological Journal, No. 31. 8vo. 1861.
Journal of Gas-Lighting for March 1861. 4to.
Mechanics' Magazine for March 1861. 8vo.
Medical Circular for March 1861. 8vo.
Practical Mechanic's Journal for March 1861. 4to.
Technologist, Nos. 1–9. 8vo. 1860.
Faraday, Professor, D.C.L. F.R.S.—Monatsberichte der Akademie der Wissenschaften zu Berlin, Nov. und Dec. 1860. 8vo.
Ferrel, W. Esq. (the Author)—The Motions of Fluids and Solids relative to the Earth's Surface. 4to. 1860.
Geological Society—Proceedings, March 1861. 8vo.
Horticultural Society of London—Proceedings, No. 22. 8vo. 1861.
Lubbock, Sir John W. Bart. F.R.S. (the Author)—The Discovery of the Planet Neptune. 8vo. 1861.
Mackie, S. J. Esq. F.G.S. (the Editor)—The Geologist, March 1861.
Newton, Messrs.—London Journal (New Series), for March 1861. 8vo.
Photographic Society—Journal, No. 107. 8vo. 1861.
Ratcliff, Charles, Esq. M.R.I.—Report of the Conference on Ragged Schools. 8vo. 1861.
Roma, Accademia de' Nuovi Lincei—Atti, Anno XIII. Sess. 2. 4to. 1860.
Smithsonian Institution—Contributions to Knowledge, Vol. XI. 4to. 1860.
Statistical Society of London—Journal, Vol. XXIV. Part 1. 8ov. 1861.
Storer, Frank, Esq. (the Author)—On the Alloys of Copper and Zinc. 4to. 1860.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, Nov. und Dec. 1860. 4to.
Volpicelli, Professor P. (the Author)—Teorica della Compensazione de' Pendoli Memoria. 4to. 1860.

WEEKLY EVENING MEETING,

Friday, April 12, 1861.

William Robert Grove, Esq. M.A. Q.C. F.R.S. Vice-President, in the Chair.

Professor Helmholtz, F.R.S.

On the Application of the Law of the Conservation of Force to Organic Nature.

The most important progress in natural philosophy by which the present century is distinguished, has been the discovery of a general law which embraces and rules all the various branches of physics and chemistry. This law is of as much importance for the highest speculations on the nature of forces, as for immediate and practical questions in the construction of machines. This law at present is commonly known by the name of "the principle of conservation of force." It might be better perhaps to call it, with Mr. Rankine, "the conservation of energy," because it does not relate to that which we call commonly *intensity* of force; it does not mean that the intensity of the natural forces is constant: but it relates more to the whole amount of power which can be gained by any natural process, and by which a certain amount of work can be done. For example: if we apply this law to gravity, it does not mean, what is strictly and undoubtedly true, that the intensity of the gravity of any given body is the same as often as the body is brought back to the same distance from the centre of the earth. Or with regard to the other elementary forces of nature—for example, chemical force: when two chemical elements come together, so that they influence each other, either from a distance or by immediate contact, they will always exert the same force upon each other—the same force both in intensity and in its direction and in its quantity. This other law indeed is true; but it is not the same as the principle of conservation of force. We may express the meaning of the law of conservation of force by saying, that every force of nature when it effects any alteration, loses and exhausts its faculty to effect the same alteration a second time. But while, by every alteration in nature, that force which has been the cause of this alteration is exhausted, there is always another force which gains as much power of producing new alterations in nature as the first has lost. Although, therefore, it is the nature of all inorganic forces to become exhausted by their own working, the power of the whole system in which these alterations take place is neither exhausted nor increased in quantity, but only changed in form. Some special examples will enable you better to understand this law than any general theories. We will begin with gravity; that most general force, which not only exerts its influence over the whole universe, but which at the same time gives the means of moving to a great number of our machines. Clocks and smaller machines, you know, are often set in motion by a weight. The same is really the case with water-mills. Water-mills are driven by falling water; and it is the gravity, the weight of the falling water, which moves the mill. Now you know that by water-mills, or by a falling weight, every machine can be put in motion; and that by these motive powers every sort of work can be done which can be done at all by any machine. You see, therefore, that the weight of a heavy body, either solid or fluid, which descends from a higher place to a lower place is a motive power, and can do every sort of mechanical work. Now if the weight has fallen down to the earth, then it has the same amount of gravity, the same intensity of gravity; but its power to move, its power to work, is exhausted; it must become again raised before it can work anew. In this sense, therefore, I say that the faculty of producing new work is exhausted—is lost; and this is true of every power of nature when this power has produced alteration. Hence, therefore, the faculty of producing work, of doing work, does not depend upon the intensity of gravity. The intensity of gravity may be the same, the weight may be in a higher position or in a lower position, but the power to work may be quite different. The power of a weight to work, or the amount of work which can be produced by a weight, is measured by the product of the height to which it is raised and the weight itself. Therefore our common measure is foot-pound; that is, the product of the number of feet and the number of pounds. Now we can by the force of a falling weight raise another weight; as, for example, the falling water in a water-mill may raise the weight of a hammer. Therefore it can be shown that the work of the raised hammer, expressed in foot-pounds, that is, the weight of the hammer multiplied by the height expressed in feet to which it is raised, that this amount of work cannot be greater than the product of the weight of water which is falling down, and the height from which it fell down. Now we have another form of motive power, of mechanical motive power; that is, velocity. The velocity of any body in this sense, if it is producing work, is called *vis viva*, or living force, of that body. You will find many examples of it. Take the ball of a gun. If it is shot off, and has a great velocity, it has an immense power of destroying; and if it has lost its velocity, it is quite a harmless thing. The great power it has depends only on its velocity. In the same sense, the velocity of the air, the velocity of the wind, is motive power; for it can drive windmills, and by the machinery of the windmills it can do every kind of mechanical work. Therefore you see that also velocity in itself is a motive force.

Take a pendulum which swings to and fro. If the pendulum is raised to the side, the weight is raised up; it is a little higher than when it hangs straightly down, perpendicular. Now if you let it fall, and it comes to its position of equilibrium, it has gained a certain velocity. Therefore, at first, you had motive power in the form of a raised weight. If the pendulum comes again to the position of equilibrium, you have motive power in the form of *vis viva*, in the form of velocity, and then the pendulum goes again to the other side, and it ascends again till it loses its velocity; then again, *vis viva* or velocity is changed into elevation of the weight: so you see in every pendulum that the power of a raised weight can be changed into velocity, and the velocity into the power of a raised weight. These two are equivalent.

Then take the elasticity of a bent spring. It can do work, it can move machines or watches. The cross-bow contains such springs. These springs of the watch and cross-bow are bent by the force of the human arm, and they become in that way reservoirs of mechanical power. The mechanical power which is communicated to them by the force of the human arm, afterwards is given out by a watch during the next day. It is spent by degrees to overpower the friction of the wheels. By the cross-bow, the power is spent suddenly. If the instrument is shot off, the whole amount of force which is communicated to the spring is then again communicated to the shaft, and gives it a great *vis viva*.

Now the elasticity of air can be a motive power in the same way as the elasticity of solid bodies; if air is compressed, it can move other bodies; let us take the air-gun; there the case is quite the same as with the cross-bow. The air is compressed by the force of the human arm; it becomes a reservoir of mechanical power; and if it is shot off, the power is communicated to the ball in the form of *vis viva*, and the ball has afterwards the same mechanical power as is communicated to the ball of a gun loaded with powder.

The elasticity of compressed gases is also the motive power of the mightiest of our engines, the steam-engine; but there the case is different. The machinery is moved by the force of the compressed vapours, but the vapours are not compressed by the force of the human arm, as in the case of the compressed air-gun. The compressed vapours are produced immediately in the interior of the boiler by the heat which is communicated to the boiler from the fuel.

You see, therefore, that in this case the heat comes in the place of the force of the human arm, so that we learn by this example, that heat is also a motive power. This part of the subject, the equivalence of heat as a motive power, with mechanical power, has been that branch of this subject which has excited the greatest interest, and has been the subject of deep research.

It may be considered as proved at present, that if heat produces mechanical power, that is, mechanical work, a certain amount of heat is always lost. On the other hand, heat can be also produced by mechanical power, namely, by friction and the concussion of unelastic bodies. You can bring a piece of iron into a high temperature, so that it becomes glowing and luminous, by only beating it continuously with a hammer. Now, if mechanical power is produced by heat, we always find that a certain amount of heat is lost; and this is proportional to the quantity of mechanical work produced by that heat. We measure mechanical work by foot-pounds, and the amount of heat we measure by the quantity of heat which is necessary to raise the temperature of one pound of water by one degree, taking the centigrade scale. The equivalent of heat has been determined by Mr. Joule, of Manchester. He found that one unit of heat, or that quantity of heat which is necessary for raising the temperature of a pound of water one degree centigrade, is equivalent to the mechanical work by which the same mass of water is raised to 423½ metres, or 1389 English feet. This is the mechanical equivalent of heat.

Hence, if we produce so much heat as is necessary for raising the temperature of one pound of water by one degree, then we must apply an amount of mechanical work equal to raising one pound of water 1389 English feet, and lose it for gaining again that heat.

By these considerations, it is proved, that heat cannot be a ponderable matter, but that it must be a motive power, because it is converted into motion or into mechanical power, and can be either produced by motion or mechanical power. Now, in the steam-engine we find that heat is the origin of the motive power, but the heat is produced by burning fuel, and therefore the origin of the motive power is to be found in the fuel, that is, in the chemical forces of the fuel, and in the oxygen with which the fuel combines.

You see from this, that the chemical forces can produce mechanical work, and can be measured by the same units and by the same measures as any other mechanical force. We may consider the chemical forces as attractions, in this instance, as attraction of the carbon of the fuel for the oxygen of the air; and if this attraction unite the two bodies, it produces mechanical work just in the same way as the earth produces work, if it attract a heavy body. Now the conservation of force, of chemical force, is of great importance for our subject to-day, and it may be expressed in this way. If you have any quantity of chemical materials, and if you cause them to pass from one state into a second state, in any way, so that the amount of the materials at the beginning, and the amount of the materials at the end of this process be the same, then you will have always the same amount of work, of mechanical work or its equivalent, done during this process. Neither more nor less work can be done by the process. Commonly, no mechanical work in the common sense is done by chemical force, but usually it produces only heat; hence the amount of heat produced by any chemical process must be independent of the way in which that chemical process goes on. The way may be determined by the will of the experimenter as he likes.

We see, therefore, that the energy of every force in nature can be measured by the same measure, by foot-pounds, and that the energy of the whole system of bodies which are not under the influence of any exterior body must be constant; that it cannot be lessened or increased by any change. Now the whole universe represents such a system of bodies endowed with different sorts of forces and of energy, and therefore we conclude from the facts I have brought before you, that the amount of working power, or the amount of energy in the whole system of the universe must remain the same, quite steady and unalterable, whatever changes may go on in the universe. If we accept the hypothesis of Laplace, that in the first state the universe was formed by a chaos of nebulous matter, spread out through infinite space, then we must conclude, that at this time the only form of energy existing in this system was the attraction of gravitation, and it was therefore the same sort of energy as is possessed by a raised weight. Afterwards, astronomers suppose, this nebulous matter was conglomerated and aggregated to solid masses. Great quantities of this nebulous matter, possibly from a great distance, fell together, and thus their attraction, or the energy of their attraction was destroyed, and hence heat must have been produced; and the facts we know at present are sufficient to enable us to calculate the amount of this heat, that is, of the whole heat which must have been produced during the whole process of conglomeration. This amount of heat is immensely great, so that it surpasses all our ideas and all the limits of our imagination. If we calculate this quantity of heat, and suppose that the sun contained at the same time the whole heat, and that the sun had the same specific heat as water, the sun would be heated to twenty-eight millions of degrees, that is, to a temperature surpassing all temperatures we know on earth; however, this temperature could not exist at any time in the sun, because the heat which was produced by the aggregation of the masses, must also be spent partially by radiation into space. I give only the result of these calculations, in order that you may see from it what a great amount of heat could be produced in this way. The same process goes on also at present in the falling stars and meteors which come down to the earth from planetary spaces. Their velocity is destroyed by the friction of the air and by the concussion with the surface of the earth, and we see how they become luminous, and if they are found on the earth, we find them hot.

The sun also at present is hotter than any heated body here on the earth. That is shown by the latest experiments made by Professors Kirchhoff and Bunsen, of Heidelberg, on the spectrum of the sun, by which it is proved, that in the atmosphere of the sun, iron and other metals are contained as vapours which cannot be changed into vapours by any amount of heat on the earth.

Our earth contains a great amount of energy in the form of its interior heat. This part of its energy produces the volcanic phenomena; but it is without great influence upon the phenomena of the surface, because only a very small amount of this heat comes through. It can be calculated that the amount of heat which goes from the interior to the surface cannot raise the temperature of the surface any higher than the thirteenth part of a degree.

We have another power which produces motion on the surface of the earth. I mean the attraction of the sun and of the moon producing the tides.

All the other phenomena on the surface of the earth are produced by the radiation of the sun, by the sunbeams; and the greater part of those changes which occur on the surface of our earth, are caused by the heat of the sun. As the heat of the sun is distributed unequally over the surface, some parts of the atmosphere become heated more than other parts; the heated parts of the atmosphere rise up, and so winds and vapours are produced. They come down at first as clouds in the higher parts of the atmosphere, and then as rain upon the surface of the earth; they are collected as rivers, and go again down into the sea. So you see that all the meteorological phenomena of our earth are produced by the effect of the solar beams by the heat of the sun.

The light of the sun is the cause of another series of phenomena, and the principal products of the light of the sun are plants, because plants can only grow with the help of the sun-light. It is only by the help of the sun-light, that they can produce the inflammable matter which is deposited in the bodies of plants, and which is extracted from the carbonic acid and the water contained in the atmosphere, and in the earth itself.

This may give you an idea of the sense and bearing of the general principle on which I purpose to speak. As many English philosophers have been occupied with working out the consequences of this most general and important principle for the theory of heat, for the energy of the solar system, for the construction of machines, you will hear these results better explained by your own countrymen; I shall abstain from entering farther into this part of the subject. At the same time that Mr. Grove showed that every force of nature is capable of bringing into action every other force of nature, Mr. Joule, of Manchester, began to search for the value of the mechanical equivalent of heat, and to prove its constancy, principally guided by the more practical interests of engineering. The first exposition of the general principle was published in Germany by Mr. Mayer, of Heilbron, in the year 1842. Mr. Mayer was a medical man, and much interested in the solution of physiological questions, and he found out the principle of the conservation of force guided by these physiological questions. At the same time also, I myself began to work on this subject. I published my researches a little later than Mr. Mayer, in 1845. Now, at first sight, it seems very remarkable and curious, that even physiologists should come to such a law. It appears more natural, that it should be detected by natural philosophers or engineers, as it was in England; but there is, indeed, a close connection between both the fundamental questions of engineering and the fundamental questions of physiology with the conservation of force. For getting machines into motion, it is always necessary to have motive-power, either in water, fuel, or living animal matter. The constructors of machines, instruments, watches, within the last century, who did not know the conservation of force, were induced to try if they could not keep a machine in motion without any expenditure for getting the motive power. Many of them worked for a long time very industriously to find out such a machine which would give perpetual motion, and produce any mechanical work which they liked. They called such a machine a perpetual mover. They thought they had an example of such a machine in the body of every animal. There, indeed, motive-power seemed to be produced every day without the help of any external mechanical force. They were not aware that eating could be connected with the production of mechanical power. Food they believed was wanted only to restore the little damages in the machine, or to keep off friction, like the fat which made the axles of wheels to run smoothly. Now at first by the mathematicians of the last century, the so-called principle of the conservation of *vis viva* was detected, and it was shown that by the action of the purely mechanical powers, it was not possible to construct a perpetual mover; but it remained still doubtful if it would not be possible to do so by the interposition of heat, or electricity, or chemical force. At last, the general law of conservation of force was discovered, and stated, and established; and this law shows that also by the connection of mechanical powers with heat, with electricity, or with chemical force, no such machine can be constructed to give a perpetual motion, and to produce work from nothing.

We must consider the living bodies under the same point of view, and see how it stands with them. Now if you compare the living body with a steam-engine, then you have the completest analogy. The living animals take in food that consists of inflammable substances, fat and the so-called hydrocarbons, as starch and sugar, and nitrogenous substances, as albumen, flesh, cheese, and so on. Living animals take in these inflammable substances and oxygen; the oxygen of the air, by respiration. Therefore, if you take, in the place of fat, starch, and sugar, coals or wood, and the oxygen of the air, you have the substances in the steam-engine. The living bodies give out carbonic acid and water; and then if we neglect very small quantities of more complicated matters which are too small to be reckoned here, they give up their nitrogen in the form of urea. Now let us suppose that we take an animal on one day, and on any day afterwards; and let us suppose that this animal is of the same weight the first day and the second day, and that its body is composed quite in the same way on both days. During the time—the interval of time—between these two days the animal has taken in food and oxygen, and has given out carbonic acid, water, and urea. Therefore, a certain quantity of inflammable substance, of nutriment, has combined with oxygen, and has produced nearly the same substances, the same combinations, which would be produced by burning the food in an open fire, at least, fat, sugar, starch, and so on; and those substances which contained no nitrogen would give us quite in the same way carbonic acid and water, if they are burnt in the open fire, as if they are burnt in the living body ; only the oxidation in the living body goes on more slowly. The albuminous substances would give us the same substances, and also nitrogen, as if they were burnt in the fire. You may suppose, for making both cases equal, that the amount of urea which is produced in the body of the animal, may be changed without any very great development of heat, into carbonate of ammonia, and carbonate of ammonia may be burnt, and gives nitrogen, water, and carbonic acid. The amount of heat which would be produced by burning urea into carbonic acid and nitrogen, would be of no great value when compared with the great quantity of heat which is produced by burning the fat, the sugar, and the starch. Therefore we can change a certain amount of food into carbonic acid, water, and nitrogen, either by burning the whole in the open fire, or by giving it to living animals as food, and burning afterwards only the urea. In both cases we come to the same result.

Now I have said that the conservation of force for chemical processes requires a fixed amount of mechanical work, or its equivalent, to be given out during this process; and the amount is exactly the same in whatever way the process may go on. And therefore we must conclude that by the animal as much work must be done, must be given out—the same equivalent of mechanical work—as by the chemical process of burning. Now let us remark that the mechanical work which is spent by an animal, and which is given to the external world, consists, firstly, in heat; and secondly, in real mechanical work. We have no other forms of work, or of equivalent of work, given out by living animals. If the animal is reposing, then the whole work must be given out in the form of heat; and therefore we must conclude that a reposing animal must produce as much heat as would be produced by burning its food. A small difference would remain for the urea; we must suppose that the urea produced by the animal is also burnt, and taken together with the heat immediately produced by the animal itself. Now we have experiments made upon this subject by the French philosophers Dulong and Desprez. They found that these two quantities of heat—the one emitted by burning, the other by the living animal—are nearly identical; at least, so far as could be established at that time, and with those previous researches which existed at that time. The heat which is produced by burning the materials of the food is not quite known even now. We want to have researches on the heat produced by the more complicated combinations which are used as food. Dulong and Desprez have calculated the heat according to the theoretical supposition of Lavoisier—which supposition is nearly right, but not quite right—therefore there is a little doubt as to the amount of the heat, but experiments show that at least to the tenth part of that heat the quantities are really equal; and we may hope if we have better researches on the heat produced by burning the food, that these quantities will also be more equal than they were found to be by Dulong and Desprez.

Now if the body be not reposing, but if muscular exertion take place, then also mechanical work is done. The mechanical work is very different according to the different kinds of muscular exertion. If we walk only on a plane surface, we must overpower the resistance of friction and the resistance of the air; but these resistances are not so great that the work which we do by walking on a plane is of great amount. Our muscles can do work in very different ways. By the researches of Mr. Redtenbacher, the director of the Polytechnic School of Carlsruhe, it is proved that the best method of getting the greatest amount of work from a human body is by the treadmill, that is, by going up a declivity. If we go up the declivity of a hill we raise the weight of our own body. In the treadmill the same work is done, only the mill goes always down, and the man on the mill remains in his place.

Now we have researches on the amount of air which is taken in and of carbonic acid given out during such work in the treadmill, made by Dr. Edward Smith. He found that a most astonishing increase of respiration takes place during such work. Now you all know that if you go up a hill you are hindered in going too fast by the great frequency and the great difficulty of respiration. This, then, becomes far greater than by the greatest exertion of walking on a plain, and really the difficulty is produced by the great mechanical work which is done in the same time. Now, partly from the experiments of Dulong and Desprez, and partly from the experiments of Dr. Edward Smith, we can calculate that the human body, if it be in a reposing state, but not sleeping, consumes so much oxygen, and burns so much carbon and hydrogen, that during one hour so much heat is produced that the whole body, or a weight of water equal to the weight of the body, would be raised in temperature one degree and two-tenths centigrade (two degrees and two-tenths Fahrenheit). Now Dr. Edward Smith found that by going in the treadmill at such a rate that if he went up a hill at the same rate, he would have risen during one hour 1712 feet, that during such a motion he exhaled five times as much carbonic acid as in the quiet state, and ten times as much as in sleeping. Therefore the amount of respiration was increased in a most remarkable way. If we now calculate these numbers we find that the quantity of heat which is produced during one hour of repose is one degree and two-tenths centigrade, and that these are nearly equivalent to rising 1712 feet, so that therefore the amount of mechanical work done in a treadmill, or done in ascending a hill at a good rate, is equivalent to the whole amount of heat which is produced in a quiescent state. The whole amount of the decomposition in the living body is five times as great as in a reposing and wakeful state. Of these five quantities, one quantity is spent for mechanical work, and four-fifths remain in the form of heat. Always in ascending a hill, or in doing great mechanical work, you become hot, and the production of heat is extremely great, as you well know, without making particular experiments. Hence you see how much the decomposition in the body is increased by doing really mechanical work.

Now these measurements give us another analogy. We see that in ascending a mountain we produce heat and mechanical work, and that the fifth part of the equivalent of the work which is produced by the chemical process is really gained as mechanical work. Now if we take our steam-engine, or a hot-air engine, or any other engine which is driven by heat in such a way that one body is heated and expands, and by the expansion other bodies are moved,—I say, if we take any thermo-dynamic engine, we find that the greatest amount of mechanical work which can be gained by chemical decomposition or chemical combination is only an eighth part of the equivalent of the chemical force, and seven-eighths of the whole are lost in the form of heat; and this amount of mechanical work can only be gained if we have the greatest difference of temperature which can be produced in such a machine. In the living body we have no great difference of temperature; and in the living body the amount of mechanical work which could be gained if the living body were a thermo-dynamic engine, like the steam-engine or the hot-air engine, would be much smaller than one-eighth. Really, we find from the great amount of work done, that the human body is in this way a better machine than the steam-engine, only its fuel is more expensive than the fuel of steam-engines.

There is another machine which changes chemical force into mechanical power; that is, the magneto-electric machine. By these magneto-electric machines a greater amount of electrical power can be changed into mechanical work than in our artificial thermo-dynamic machines. We produce an electric current by dissolving zinc in sulphuric acid, and liberating another oxidizable matter. Generally it is only the difference of the attraction of zinc for oxygen compared with the attraction of copper or nitrous acid for oxygen. In the human body we burn substances which contain carbon and hydrogen, and therefore the whole amount of attraction of carbon and hydrogen for oxygen is put into action to move the machine; and in this way the power of the living body is greater and more advantageous than the power of the magneto-electric machine.

Let us now consider what consequences must be drawn when we find that the laws of animal life agree with the law of the conservation of force, at least as far as we can judge at present regarding this subject. As yet we cannot prove that the work produced by living bodies is an exact equivalent of the chemical forces which have been set into action. It is not yet possible to determine the exact value of either of these quantities so accurately as will be done ultimately; but we may hope that at no distant time it may be possible to determine this with greater accuracy. There is no difficulty opposed to this task. Even at present I think we may consider it as extremely probable that the law of the conservation of force holds good for living bodies.

Now we may ask, what follows from this fact as regards the nature of the forces which act in the living body?

The majority of the physiologists in the last century, and in the beginning of this century, were of opinion that the processes in living bodies were determined by one principal agent which they chose to call the "vital principle." The physical forces in the living body they supposed could be suspended or again set free at any moment, by the influence of the vital principle; and that by this means this agent could produce changes in the interior of the body, so that the health of the body would be thereby preserved or restored.

Now the conservation of force can exist only in those systems in which the forces in action (like all forces of inorganic nature) have always the same intensity and direction if the circumstances under which they act are the same. If it were possible to deprive any body of its gravity, and afterwards to restore its gravity, then indeed we should have the perpetual motion. Let the weight come down as long as it is heavy; let it rise if its gravity is lost; then you have produced mechanical work from nothing. Therefore this opinion that the chemical or mechanical power of the elements can be suspended, or changed, or removed in the interior of the living body, must be given up if there is complete conservation of force.

There may be other agents acting in the living body, than those agents which act in the inorganic world; but those forces, as far as they cause chemical and mechanical influences in the body, must be quite of the same character as inorganic forces, in this at least, that their effects must be ruled by necessity, and must be always the same, when acting in the same conditions, and that there cannot exist any arbitrary choice in the direction of their actions.

This is that fundamental principle of physiology which I mentioned in the beginning of this discourse.

Still at the beginning of this century physiologists believed that it was the vital principle which caused the processes of life, and that it detracted from the dignity and nature of life, if anybody expressed his belief that the blood was driven through the vessels by the mechanical action of the heart, or that respiration took place according to the common laws of the diffusion of gases.

The present generation, on the contrary, is hard at work to find out the real causes of the processes which go on in the living body. They do not suppose that there is any other difference between the chemical and the mechanical actions in the living body, and out of it, than can be explained by the more complicated circumstances and conditions under which these actions take place; and we have seen that the law of the conservation of force legitimizes this supposition. This law, moreover, shows the way in which this fundamental question, which has excited so many theoretical speculations, can be really and completely solved by experiment.

[H. H.]

WEEKLY EVENING MEETING,

Friday, April 19, 1861.

Sir Roderick I. Murchison, D.C.L. F.R.S. Vice-President, in the Chair.

John Ruskin, Esq.

On Tree Twigs.

The speaker's purpose was to exhibit the development of the common forms of branch, in dicotyledonous trees, from the fixed type of the annual shoot. Three principal modes of increase and growth might be distinguished in all accumulative change; namely:—

1st. Simple aggregation, having no periodical or otherwise defined limit; and subject only to laws of cohesion and crystallization, as in inorganic matter.

2nd. Addition of similar parts to each other, under some law fixing their limits and securing their unity.

3rd. Enlargement, or systematic change in arrangement, of a typical form, as in the growth of the members of an animal.

The growth of trees came under the second of these heads. A tree did not increase in stem and bough as the wrist and hand of a child increased to the wrist and hand of a man; but it was built up by additions of similar parts, as a city is increased by the building of new rows of houses.

Any annual shoot was most conveniently to be considered as a single rod, which would always grow vertically if possible.

Every such rod or pillar was, in common timber trees, typically either polygonal in section, or rectangular.

If polygonal, the leaves were arranged on it in a spiral order, as in the elm or oak.

If rectangular, the leaves were arranged on it in pairs, set alternately at right angles to each other.

Intermediate forms connected each of these types with those of monocotyledonous trees. The structure of the *arbor vitæ* might be considered as typically representing the link between the rectangular structure and that of monocotyledons; and that of the pine, between the polygonal structure and that of monocotyledons.

Every leaf during its vitality secreting carbon from the atmosphere, with the elements of water, formed a certain quantity of woody tissue, which extended down the outside of the tree to the ground, and farther, to the extremities of the roots. The mode in which this descending masonry was added, appeared to depend on the peculiar functions of cambium; and (the speaker believed) was as yet unexplained by botanists.

Every leaf, besides forming this masonry all down the tree, protected a bud at the base of its own stalk. From this bud, unless rendered abortive, a new shoot would spring next year. Now, supposing that out of the leaf-buds on each shoot of a pentagonal tree, only five at its extremity or on its sides were permitted to develope themselves, even under this limitation the number of shoots developed from a single one in the seventh year would be 78,125. The external form of a healthily-grown tree at any period of its development was therefore composed of a mass of sprays, whose vitality was approximately distributed over the *surface* of the tree to an equal depth. The branches beneath at once supported, and were fed by, this orbicular field, or animated external garment of vegetation; from every several leaf of which, as from an innumerable multitude of small green fountains, the streams of woody fibre descended, met, united as rivers do, and gathered their full flood into the strength of the stem.

The principal errors which had been committed by artists in drawing trees had arisen from their regarding the bough as ramifying irregularly, and somewhat losing in energy towards the extremity; whereas the real boughs threw their whole energy, and multiplied their substance, towards the extremities, ranking themselves in more or less cup-shaped tiers round the trunk, and forming a compact united surface at the exterior of the tree.

In the course of arrival at this form, the bough, throughout its whole length, showed itself to be influenced by a force like that of an animal's instinct. Its minor curves and angles were all subjected to one strong ruling tendency and law of advance, dependent partly on the aim of every shoot to raise itself upright, partly on the necessity which each was under to yield due place to the neighbouring leaves, and obtain for itself as much light and air as possible. It had indeed been ascertained that vegetable tissue was liable to contraction and expansion (under fixed mechanical conditions) by light, heat, moisture, &c. But vegetable tissue in the living branch did not contract nor expand under external influence alone. The principle of life manifested itself either by contention with, or felicitous recognition of, external force. It accepted with a visible, active, and apparently joyful concurrence, the influences which led the bough towards its due place in the economy of the tree; and it obeyed reluctantly, partially, and with distorted curvatures, those which forced it to violate the typical organic form. The attention of painters of foliage had seldom been drawn with sufficient accuracy to the lines either of branch curvature, or leaf contour, as expressing these subtle laws of incipient volition; but the relative merit of the great schools of figure-design might, in absence of all other evidence, be determined, almost without error, by observing the precision of their treatment of leaf curvature. The leaf-painting round the head of Ariosto by Titian, in the National Gallery, might be instanced.

The leaf thus differed from the flower in forming and protecting behind it, not only the bud in which was the germ of a new shoot like itself, but a piece of permanent work; and produced substance, by which every following shoot would be placed under different circumstances from its predecessor. Every leaf laboured to solidify this substance during its own life; but the seed left by the flower matured only as the flower perished.

This difference in the action and endurance of the flower and leaf, had been applied by nearly all great nations as the type of the variously active or productive states of life, among individuals or commonwealths. Chaucer's poem of the "Flower and Leaf" is the most definite expression of the mediæval feeling in this respect, while the fables of the rape of Proserpine and of Apollo and Daphne embody that of the Greeks. There is no Greek goddess corresponding to the Flora of the Romans. Their Flora is Persephone, "the bringer of death." She plays for a little while in the Sicilian fields, gathering flowers; then, snatched away by Pluto, receives her chief power as she vanishes from our sight, and is crowned in the grave. Daphne, on the other hand, is the daughter of one of the great Arcadian river gods, and of the earth; she is the type of the river mist filling the rocky vales of Arcadia; the sun, pursuing this mist from dell to dell, is Apollo pursuing Daphne;—where the mist is protected from his rays by the rock shadows, the laurel and other richest vegetation spring by the river-sides, so that the laurel-leaf becomes the type, in the Greek mind, of the beneficent ministry and vitality of the rivers and the earth, under the beams of sunshine; and therefore it is chosen to form the signet-crown of highest honour for gods or men; honour for work born of the strength and dew of the earth, and informed by the central light of heaven;—work living, perennial, and beneficent.

[J. R.]

WEEKLY EVENING MEETING,

Friday, April 26, 1861.

THE DUKE OF NORTHUMBERLAND. K.G. F.R.S. President, in the Chair.

PROFESSOR OWEN, F.R.S., &c.

FULLERIAN PROFESSOR OF PHYSIOLOGY, R.I.

On the Scope and Appliances of a National Museum of Natural History.

ANNUAL MEETING,

Wednesday, May 1, 1861.

THE DUKE OF NORTHUMBERLAND, K.G. F.R.S. President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1860 was read and adopted.

The statement of Sums Received continues to show a gradual increase in the yearly income. The amount of Annual Contributions of Members and Subscribers in 1860 amounted to £2967. 6*s.*, the Receipts and Subscriptions to Lectures were £946. 11*s.* 6*d.*; the total Annual Income amounted to £4808. 2*s.*

On December 31, 1860, the Funded Property was £27,750. 9*s.* 8*d.*; and the Balance at the Bankers, £1182. 12*s.* 7*d.*, with Six Exchequer Bills of £100 each. There were no Liabilities.

A List of Books Presented accompanies the Report, amounting in number to 386 volumes; making, with those purchased by the Managers and Patrons, a total of 758 volumes (including Periodicals) added to the Library in the year.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to Professor Faraday, for their services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:—

PRESIDENT—The Duke of Northumberland, K.G. F.R.S.
TREASURER—William Pole, Esq. M.A. F.R.S.
SECRETARY—Henry Bence Jones, M.A. M.D. F.R.S.

MANAGERS.

John George Appold, Esq. F.R.S.
The Rev. John Barlow, M.A. F.R.S.
John J. Bigsby, M.D. F.G.S.
William Bowman, Esq. F.R.S.
Warren De la Rue, Esq. Ph.D. F.R.S.
George Dodd, Esq. F.S.A.
Col. Sir George Everest, C.B. F.R.S.
John Peter Gassiot, Esq. F.R.S.
John Hall Gladstone, Esq. Ph.D. F.R.S.
Sir Henry Holland, Bart. M.D. D.C.L. F.R.S.
Richard Jennings, Esq. M.A.
John Lubbock, Esq. F.R.S.
Lewis Powell, M.D. F.S.A.
The Lord Stanley, M.P. F.R.S.
The Lord Wensleydale.

Visitors.

Neil Arnott, M.D. F.R.S.
George J. Bosanquet, Esq.
Archibald Boyd, Esq.
John Charles Burgoyne, Esq.
George Busk, Esq. F.R.S. F.L.S.
Rev. Charles John Fynes Clinton, M.A.
Edward Enfield, Esq.
Gordon Willoughby James Gyll, Esq.
John MacDonnell, Esq.
Edmund Macrory, Esq. M.A.
James Nasmyth, Esq.
Henry Minchin Noad, Esq. F.R.S.
George Stodart, Esq.
The Viscount Templetown.
Arthur De Noè Walker, Esq.

WEEKLY EVENING MEETING,

Friday, May 3, 1861.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

Professor Faraday, D.C.L. F.R.S.

On Mr. Warren De la Rue's Photographic Eclipse Results.

The speaker commenced by drawing attention to the sun as the great source of light and heat to the planets of our system; and to the phenomena which occur from time to time when the earth and the moon are brought by their orbital revolutions nearly or absolutely in the same plane. The sun, casting shadows of the moon and of the earth in an opposite direction to their illumined sides, there would always be produced a total eclipse of the sun, or the moon, when these bodies were situated in the same line on the same side of the sun, if the distances of the earth or moon invariably admitted of the one falling within the shadow of the other. In consequence, however, of the elliptical form of the orbits of the earth and moon, the distances of these planets from each other and the sun are constantly varying, and sometimes the shadow of the earth does not reach the moon, or that of the moon does not reach the earth. We might consequently have, in the case of the sun, either a partial eclipse when the sun, moon, and earth were not exactly in the same plane, or an annular or a total eclipse when they were so situated. A total eclipse might be only just total, or be of a shorter or longer duration as the apparent diameter of the moon exceeded by little or much the apparent diameter of the sun; no eclipse of the sun is so great, however, as to shut off the light of the sun from the whole hemisphere of the earth; on the contrary, the shadow of the moon can never cover more than a very small extent of the earth's surface. On the 18th of July of last year, it happened that under unusually favourable circumstances there occurred a total solar eclipse; the sun was nearly at his greatest possible distance from us, and therefore had almost a minimum apparent diameter, and the moon was nearly at her least possible distance, and therefore had a nearly maximum apparent diameter, so that the breadth and duration of the total eclipse were nearly at a maximum; moreover, the shadow of the moon passed over a country easily accessible to European astronomers. The speaker, after pointing out the course of the eclipse, from sun rising in Greenland, across the Atlantic, across Spain, the Mediterranean, and Africa, stated that for a belt of 60 miles broad, the duration of the eclipse in Spain was fully three minutes, and about three minutes and a half in the central line of that belt. Such favourable circumstances were not likely to occur again within the probable lifetime or opportunities of the observers, who went out to Spain to observe the eclipse. Hence the desirability of placing before scientific men the means used by various persons to record and measure the appearances visible at total eclipses, with a view to facilitate the labours of future observers.

Curious and paradoxical as it might at first appear, it is necessary to shut out the sun in order to see it; for example, said the speaker, look on this electric light, could any one conjecture from its aspect what it is? One sees a brilliant centre surrounded by rays, but one cannot see the two charcoal points which are producing it; and if it were magnified and projected against a screen by means of lenses, although the ignited points would be rendered evident, still there are matters about them which would remain invisible. So it is with the sun; one can so reduce his light, by means of dark glasses, or other contrivances, as to enable us to scrutinize by means of telescopes his photosphere; or we might project his image on to a screen, and thus examine it: but we should not see the sun, that is the whole sun. His mottled surface, his bright markings, his dark spots would undoubtedly be thus shown; but we should fail to discern those curious appendages which were first observed a few years back on the occasion of a total eclipse. These so-called red flames, about the nature of which many conjectures have been made, are, as it now appears, true belongings of the sun, and are not subjective phenomena, produced, as some philosophers suppose, by a deflection or diffraction of the sun's light in passing through the valleys on the moon's profile.

The object of the speaker being, as before stated, to bring under the notice of the Members of the Institution, Mr. De la Rue's photographic results: he now described the Kew Photoheliograph. A photograph of the instrument and temporary observatory, taken in Spain, being projected on the screen by the electric lamp. The heliograph consists of a telescope, the tube of which is square in section, and which can be made to follow the sun accurately by means of clock-work. The optical part consists of an object glass (actinically corrected), and a secondary combination of lenses, situated near the focus of the object glass, for the purpose of magnifying the sun's image to four inches in diameter. The image so magnified together with position wires are depicted on the collodion plate placed in the telescope. In taking ordinary sun pictures, or pictures of the partial phases of the eclipse, the light is allowed to act on the sensitive plate by the passage of a narrow slit in a brass plate drawn with great rapidity across the secondary magnifying lens. By this contrivance, the sun's image is allowed to act for a very small fraction of a second of time. Thirty-one pictures of the various partial phases of the eclipse were obtained in this way by Mr. De la Rue and his assistants. Several of the most interesting were projected on to the screen by means of the electric lamp. The two totality pictures were, however, obtained in another way; the brass plate with the slit was drawn aside, and the picture of the protuberances allowed to fall for a whole minute on the collodion plate. The first picture was procured exactly from the commencement of the totality and during the minute precisely succeeding it. The second picture from about a minute preceding the reappearance of the sun until just before he reappeared.

These pictures were shown on the screen by means of the electric lamp, and it was seen that the luminous prominences extended for a long distance beyond the moon's dark limb. In the first picture, some prominences were completely detached, and were at some considerable distance from the moon's limb; these, in the second picture, were reached by the moon, which during the interval had been travelling across the sun's disc. Fresh prominences had come into view in the second picture on the western limb, while some of the prominences on the eastern limb had been shut off by the moon's motion.

The speaker then referred to diagrams, which had been enlarged from actual photographs, etched upon glass by hydrofluoric acid, and graduated in accordance with the data furnished by the images of position wires on the photographs. These diagrams showed that a luminous prominence situated at a *right angle* to the path of the moon's motion across the solar disc had hung back in reference to the moon's centre an angular distance of about $5\frac{1}{2}°$, while prominences situated *in the direction* of the moon's path had not shifted angularly, but were covered and uncovered to an extent of about 93″ during the period of totality. Such results were in accordance with the hypothesis that the prominences belong to the sun, and opposed to the theory that they are subjective phenomena produced by the deflection of the sun's light.

In order to render evident the relative positions of the whole of the protuberances visible during the eclipse, attention was drawn to a diagram enlarged from an etched photograph of the sun, on which were etched also the protuberances visible in the first and second totality —photographs which coincided exactly when superposed in respect of those parts visible in both. It was seen on the diagram, that in consequence of Rivabellosa not being exactly in the centre of the shadow path, the moon's centre was depressed below the sun's centre, and thus at the nearest approach of the two centres they were distant about 14″.

This had the effect of rendering visible a little more of the prominences on the northern limb, and of shutting off a portion on the southern limb of the sun.

The speaker drew attention to the heights of many of the prominences; to the circumstance of their brilliancy in some cases being greatest in those parts nearest the sun, while in others the brightest part was on that edge most distant from the sun. One prominence, upwards of 70,000 miles distant from the sun's limb, was particularly pointed out—this had not been seen by human eyes, but there was its image fixed and recorded by the heliograph in both the pictures. Photography could therefore render evident to us phenomena of the sun which the human eye could not discern; and here we had another of the many proofs of the importance of varying our means of observation. This prominence was not the less real because we could not see it; it existed and emitted a radiant force; invisible it was true, but still nevertheless a force, and even possibly a greater chemical force than that of the visible prominences. In order to render this apparent, a spectrum was produced by means of the electric light and projected on to a collodion plate recently made sensitive, and placed in full view of the audience; during the thirty seconds of exposure, marks were made by scratching through the collodion film to indicate the position of red, yellow, green, and violet bands; on developing the picture and projecting the image on to the screen, it was seen, by reference to the scratches, that up to the violet band very little effect had been produced, but that the invisible rays beyond the violet had produced a very intense image, to an extent equal to the breadth of the visible spectrum, consisting of several well-marked bands of varying intensity.

The speaker now drew attention to the corona: on Mr. De la Rue's photographs the corona to some extent was visible, but recourse was had to a photograph of a drawing showing the whole phenomena, which was also projected against the screen. It was pointed out that observations in Spain had proved that the corona polarized light, and as light coming direct from a luminous body is not polarized, but that after reflection it is so; the fact of polarization tended to show that the corona must be a consequence of an atmosphere around the sun reflecting the sun's light.

The speaker concluded by drawing attention to some phenomena connected with the sun's spots, their rotation, the sudden bursting out of a brilliant light observed by two astronomers distant from each other; and also to a curious foliated appearance in the sun's spots, observed by Mr. Nasmyth. What are these vast masses which reach to such enormous distances beyond the sun, as we see him under ordinary circumstances? One, we perceive, extends nearly as far from the sun as three times the entire circumference of the earth, and another is suspended away from the sun's limb about once that distance! Modern science places at our disposal methods of determining the nature of some of the constituents of the sun's atmosphere, with a degree of certainty equal, perhaps, to any of our laboratory methods, could we bring away a sample and analyse it by chemical means. Employing the principles and methods of Bunsen and Kirchhoff, which Professor Roscoe recently brought under our notice, might we not, suggested the speaker, on the occasion of another solar eclipse give some answer to this question, and add another proof of the reality of these prominences by revealing something as to the nature of their constituent particles.

Lastly, there was exhibited a photograph of the moon, obtained by Mr. De la Rue at his own observatory, for the double object of showing the great beauty and perfection of the pictures, and in illustration of the light and shadow being different in proportion from the visible image, in consequence of the actinic force of the light reflected by different parts of the moon not according precisely with its luminosity.

[M. F.]

GENERAL MONTHLY MEETING,

Monday, May 6, 1861.

SIR HENRY HOLLAND, Bart. F.R.S. M.D. Vice-President, in the Chair.

The Hon. Lieut.-Gen. Thomas Ashburnham, C.B.
Lieut.-Colonel W. MacGeorge, and
William Reid, Esq.

were *elected* Members of the Royal Institution.

H.R.H. The Count of Paris,
H.R.H. The Duke of Chartres,
Stephen Goodfellow, Esq. M.A. and
Alexander Mackintosh, Esq.

were *admitted* Members of the Royal Institution.

The following Professors were re-elected :—

WILLIAM THOMAS BRANDE, Esq. D.C.L. F.R.S. Hon. Professor of Chemistry.

JOHN TYNDALL, Esq. F.R.S. Professor of Natural Philosophy.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same : viz.

FROM

Actuaries, Institute of—Assurance Magazine, No. 43. 8vo. 1861.
Anonymous—The Squire : a Biographical Sketch. 16mo. 1861.
Arts, Society of—Journal, April 1861. 8vo.
Asiatic Society of Bengal—Journal, No. 279. 8vo. 1860.
Astronomical Society, Royal—Monthly Notices, March 1861.
Bombay, Medical and Physical Society of—Transactions for 1859. 8vo. 1860.

Chemical Society—Journal, No. 53. 8vo. 1861.
Clifford, Mr. C.—Samuel How's Sermon on the Sufficiency of the Spirit's Teaching without Human Learning: first published in 1639. With a Sketch of his Life. 16to. 1835.
Editors—American Journal of Science, by B. Silliman, &c. for April 1861. 8vo.
Artizan for April 1861. 4to.
Athenæum for April 1861. 4to.
Chemical News for April 1861. 4to.
Engineer for April 1861. fol.
Horological Journal, No. 32. 8vo. 1861.
Journal of Gas-Lighting for April 1861. 4to.
Mechanics' Magazine for April 1861. 8vo.
Medical Circular for April 1861. 8vo.
Practical Mechanic's Journal for April 1861. 4to.
St. James's Magazine, No. 26. 8vo. 1861.
Faraday, Professor, D.C.L. F.R.S.—Monatsberichte der Academie der Wissenschaften zu Berlin: Register 1836–58. 8vo. 1860.
Imperial Academy of Vienna: Sitzungsberichte, Math-Nat. Classe, 1860. No. 22–26. 8vo. 1860.
Franklin Institute of Pennsylvania—Journal, Vol. XLII. No. 3. 8vo. 1861.
Geographical Society, Royal—Proceedings, Vol. V. No. 1. 8vo. 1861.
Geological Society—Proceedings, April 1861. 8vo.
Gladstone, John Hall, Esq. Ph. D. F.R.S. M.R.I.—Report of the Commissioners appointed to inquire into the Condition and Management of Lights, Buoys, and Beacons: with Appendix, &c. 2 vols. fol. 1861.
Granville, A. B. M.D. F.R.S. (the Author)—On Propagation in Females of the Industrial Classes in the Metropolis. 8vo. 1861.
Herbert, the Lord, Secretary of State for War—Report of the Explosion of Gunpowder by Electricity. By C. Wheatstone and F. Abel. fol. 1861.
Horticultural Society of London—Proceedings, Nos. 23, 24. 8vo. 1861.
Jones, H. Bence, M.D. F.R.S. Sec. R.I. (the Author)—On Sugar in the Urine. 8vo. 1861.
Linnean Society—Transactions, Vol. XXIII. Part 1. 4to. 1861.
Proceedings, No. 19. 8vo. 1860.
Locke, John, Esq. (the Author)—Polar Explorations: a Lecture. 8vo. 1861.
Longman, William, Esq. M.R.I. (the Author)—Suggestions for the Exploration of Iceland. 8vo. 1861.
Lubbock, John, Esq. F.R.S. M.R.I. (the Author)—On the Generative Organs of the Annulosa. 8vo. 1861. (Proceedings R.S.)
Mackie, S. J. Esq. F.G.S. (the Editor)—The Geologist, April 1861.
Newton, Messrs.—London Journal (New Series) for April 1861. 8vo.
Photographic Society—Journal, No. 108. 8vo. 1861.
Petermann, A. Esq. (the Editor)—Mittheilungen auf der Gesammtgebiete der Geographie. 1861. No. 3. 4to. 1861.
Royal Society of London—Proceedings, No. 43. 8vo. 1861.
St. Bartholomew's Hospital, the Governors of—Statistical Tables of St. Bartholomew's Hospital, 1860. By G. N. Edwards. 8vo. 1861.
Upsal Royal Society of Sciences—Nova Acta. 3a Series. Vol. I. et II. 4to. 1855-8. Arsskrift I. 8vo. 1860.
Upton, W. Esq. (the Author)—The Japetic Philosophy, and Physioglyphics or Natural Philology. 8vo. 1861.
Wilson, Thomas, Esq. M.R.I.—C. Fullbrook, on Wet and Dry Seasons of England, 1846 to 1860. 8vo. 1861.
Yorkshire Philosophical Society—Annual Report, 1860. 8vo.

Samwell, Francis B. Esq. M.R.I.—Ancient Egyptian Inscription, cut in chalk.
Ancient Greek Inscription, cut in sandstone.
A Fine Specimen of Native Crystallized Carbonate of Lead.
A Good Specimen of Crystallized Sulphuret of Lead.

WEEKLY EVENING MEETING,

Friday, May 10, 1861.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

William Scovell Savory, Esq. F.R.S.

On the Relation of the Vegetable and Animal to the Inorganic Kingdom.

After some introductory remarks to show that Plants and Animals, when viewed in the relation in which they stand to Inorganic substances, may fairly be regarded as together constituting one great kingdom of nature—the organized, the speaker proceeded to consider the relation of organized to inorganic bodies, in structure, in composition, and in function. He challenged the truth of the cell doctrine, and asserted that its leading propositions were not tenable. Not only are some of the simplest tissues of the most complex plants and animals formed without the direct agency of cells, but also that elaborate tissue, striated muscular fibre. He referred to the common Amœba or Proteus, as an example of a living form, simpler than an unicellular organism, inasmuch as it exhibits no distinction of cell wall and cell contents; and to Actinophrys, as an illustration of a structure apparently cellular, produced by simple reticulation of a structureless amorphous substance. Vegetable cellular tissue has been generally supposed to be formed by the multiplication of distinct cells, but there is better evidence of its production by a similar process of reticulation of protoplasm. The simplest forms of life, and the simplest tissues of the highest, reveal no difference in structure,—no distinction of parts to the severest scrutiny, and cannot, therefore, as heterogeneous bodies, be distinguished from homogeneous inorganic matter.

Chemistry draws no line of demarcation between the organized and inorganic kingdoms. All questions of composition are of degree only, not of kind.

Life constitutes the grand distinction. The difference is infinitely greater between living and dead organic matter than between dead organic and inorganic substances. Without attempting to describe it, life may be distinguished by its effects. In life, when reduced to its simplest terms, and separated from all those elaborate details which invest it in the more complex forms, there exists a definite relation between destruction and renewal, a regulated adjustment between waste and repair whereby the condition is maintained, notwithstanding constant change.

Life is not a state of resistance. The proofs of this are clear and complete. Waste or destruction is a necessary, an inevitable condition of its manifestation. It is involved in every vital act. And the power of compensating for this waste or change, the repair or reproduction necessary to the continuance of life, involves that of assimilation,—the power of converting foreign matters into the structure of the organism: in other language, the power of appropriating food.

We cannot conceive life without including these two conditions—consumption and supply. Life is not a state of change only, as opposed to stability; for this is simply a question of degree everywhere, and dependent on the conditions to which bodies are exposed. Neither dead organic nor inorganic bodies are immune from change. Nor is life peculiar as a process of repair only; for this may occur in inorganic bodies, as crystals, under favourable circumstances. But in life there is the constant and concurrent operation of these two processes, whereby it is distinguished from mere change on the one hand, and from repair on the other.

It does not appear that we can at present safely venture further than this. If we attempt to define the vital process of nutrition, distinctions fail us.

The investigation of the phenomena of life has not been in any way assisted, our knowledge of the vital processes has not been in any measure advanced, by the assumption of what has been styled a "vital principle;"—an empirical term, which, like some others when employed in physiology, is, even at the best, equivalent to nothing more than the final letters of the alphabet in an algebraical formula: for it is, when used in its least objectionable sense, a mere expression of something unknown. But the assumption of such an agent or principle, however designated, annihilating or suspending the operation of forces acting elsewhere, has not proved altogether harmless in its influence upon the progress of knowledge. By referring all vital actions to this obscure agency, while nothing was thereby explained, inquiry was to a great extent, and for a long while, checked. Many, dazzled by the idea that the nature of vital phenomena was exalted by thus associating them with some mysterious and peculiar principle apart from, and opposed to those agencies which act elsewhere, missed the grander conception, that even in the vital functions may be recognized the operation of forces, some of which, at least, are common to both kingdoms of nature; while, between these and others, which appear to be peculiar to living tissues, it is probable that a relation may exist like that which prevails between the chemical and physical forces.

Again, it is needful to beware how we create artificial distinctions. Is there not much assumption involved in the confession that we are unable to construct the simplest form of living tissue? Men sometimes talk as if their powers were limited only by life. But can we construct a crystal any more than a nucleated cell? We may fulfil certain conditions, under which, as we have learned from experience, crystals are formed; but what is our share in the act itself? In like manner we may take a seed or an egg, and place them under circumstances in which they will develope. In either case we are acquainted with the necessary conditions, and we fulfil them. We can do no more.

The speaker concluded with some remarks on the fact that the tendency of advancing knowledge is to efface the lines of demarcation which have been hitherto drawn between the natural kingdoms.

[W. S. S.]

WEEKLY EVENING MEETING,

Friday, May 17, 1861.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

Professor J. Clerk Maxwell,

On the Theory of Three Primary Colours.

The speaker commenced by showing that our power of vision depends entirely on our being able to distinguish the intensity and quality of colours. The forms of visible objects are indicated to us only by differences in colour or brightness between them and surrounding objects. To classify and arrange these colours, to ascertain the physical conditions on which the differences of coloured rays depend, and to trace, as far as we are able, the physiological process by which these different rays excite in us various sensations of colour, we must avail ourselves of the united experience of painters, opticians, and physiologists. The speaker then proceeded to state the results obtained by these three classes of inquirers, to explain their apparent inconsistency by means of Young's Theory of Primary Colours, and to describe the tests to which he had subjected that theory.

Painters have studied the relations of colours, in order to imitate them by means of pigments. As there are only a limited number of coloured substances adapted for painting, while the number of tints in nature is infinite, painters are obliged to produce the tints they require by mixing their pigments in proper proportions. This leads them to regard these tints as actually compounded of other colours, corresponding to the pure pigments in the mixture. It is found, that by using three pigments only, we can produce all colours lying within certain limits of intensity and purity. For instance, if we take carmine (red), chrome yellow, and ultramarine (blue), we get by mixing the carmine and the chrome, all varieties of orange, passing through scarlet to crimson on the one side, and to yellow on the other; by mixing chrome and ultramarine we get all hues of green; and by mixing ultramarine with carmine, we get all hues of purple, from violet to mauve and crimson. Now these are all the strong colours that we ever see or can imagine: all others are like these, only less pure in tint. Our three colours can be mixed so as to form a neutral grey; and if this grey be mixed with any of the hues produced by mixing two colours only, all the tints of that hue will be exhibited, from the pure colour to neutral grey. If we could assume that the colour of a mixture of different kinds of paint is a true mixture of the colours of the pigments, and in the same proportion, then an analysis of colour might be made with the same ease as a chemical analysis of a mixture of substances.

The colour of a mixture of pigments, however, is often very different from a true mixture of the colours of the pure pigments. It is found to depend on the size of the particles, a finely ground pigment producing more effect than one coarsely ground. It has also been shown by Professor Helmholtz, that when light falls on a mixture of pigments, part of it is acted on by one pigment only, and part of it by another; while a third portion is acted on by both pigments in succession before it is sent back to the eye. The two parts reflected directly from the pure pigments enter the eye together, and form a true mixture of colours; but the third portion, which has suffered absorption from both pigments, is often so considerable as to give its own character to the resulting tint. This is the explanation of the green tint produced by mixing most blue and yellow pigments.

In studying the mixture of colours, we must avoid these sources of error, either by mixing the rays of light themselves, or by combining the impressions of colours within the eye by the rotation of coloured papers on a disc.

The speaker then stated what the opticians had discovered about colour. White light, according to Newton, consists of a great number of different kinds of coloured light which can be separated by a prism. Newton divided these into seven classes, but we now recognize many thousand distinct kinds of light in the spectrum, none of which can be shown to be a compound of more elementary rays. If we accept the theory that light is an undulation, then, as there are undulations of every different period from the one end of the spectrum to the other, there are an *infinite* number of possible kinds of light, no one of which can be regarded as compounded of any others.

Physical optics does not lead us to any theory of three primary colours, but leaves us in possession of an infinite number of pure rays with an infinitely more infinite number of compound beams of light, each containing any proportions of any number of the pure rays.

These beams of light, passing through the transparent parts of the eye, fall on a sensitive membrane, and we become aware of various colours. We know that the colour we see depends on the nature of the light; but the opticians say there are an infinite number of kinds of light; while the painters, and all who pay attention to what they see, tell us that they can account for all actual colours by supposing them mixtures of three primary colours.

The speaker then next drew attention to the physiological difficulties in accounting for the perception of colour. Some have supposed that the different kinds of light are distinguished by the time of their vibration. There are about 447 billions of vibrations of red light in a second; and 577 billions of vibrations of green light in the same time. It is certainly not by any mental process of which we are conscious that we distinguish between these infinitesimal portions of time, and it is difficult to conceive any mechanism by which the vibrations could be counted so that we should become conscious of the results, especially when many rays of different periods of vibration act on the same part of the eye at once.

Besides, all the evidence we have on the nature of nervous action goes to prove that whatever be the nature of the agent which excites a nerve, the sensation will differ only in being more or less acute. By acting on a nerve in various ways, we may produce the faintest sensation or the most violent pain; but if the intensity of the sensation is the same, its quality must be the same.

Now, we may perceive by our eyes a faint red light which may be made stronger and stronger till our eyes are dazzled. We may then perform the same experiment with a green light or a blue light. We shall thus see that our sensation of colour may differ in other ways, besides in being stronger or fainter. The sensation of colour, therefore, cannot be due to one nerve only.

The speaker then proceeded to state the theory of Dr. Thomas Young, as the only theory which completely reconciles these difficulties in accounting for the perception of colour.

Young supposes that the eye is provided with three distinct sets of nervous fibres, each set extending over the whole sensitive surface of the eye. Each of these three systems of nerves, when excited, gives us a different sensation. One of them, which gives us the sensation we call red, is excited most by the red rays, but also by the orange and yellow, and slightly by the violet; another is acted on by the green rays, but also by the orange and yellow and part of the blue; while the third is acted on by the blue and violet rays.

If we could excite one of these sets of nerves without acting on the others, we should have the pure sensation corresponding to that set of nerves. This would be truly a primary colour, whether the nerve were excited by pure or by compound light, or even by the action of pressure or disease.

If such experiments could be made, we should be able to see the primary colours separately, and to describe their appearance by reference to the scale of colours in the spectrum.

But we have no direct consciousness of the contrivances of our own bodies, and we never feel any sensation which is not infinitely complex, so that we can never know directly how many sensations are combined when we see a colour. Still less can we isolate one or more sensations by artificial means, so that in general when a ray enters the eye, though it should be one of the pure rays of the spectrum, it may excite more than one of the three sets of nerves, and thus produce a compound sensation.

The terms simple and compound, therefore, as applied to colour-ensation, have by no means the same meaning as they have when applied to a ray of light.

The speaker then stated some of the consequences of Young's theory, and described the tests to which he had subjected it :—

1st. There are three primary colours.

2nd. Every colour is either a primary colour, or a mixture of primary colours.

3rd. Four colours may always be arranged in one of two ways. Either one of them is a mixture of the other three, or a mixture of two of them can be found, identical with a mixture of the other two.

4th. These results may be stated in the form of colour-equations, giving the numerical value of the amount of each colour entering into any mixture. By means of the Colour Top,* such equations can be obtained for coloured papers, and they may be obtained with a degree of accuracy showing that the colour-judgment of the eye may be rendered very perfect.

The speaker had tested in this way more than 100 different pigments and mixtures, and had found the results agree with the theory of three primaries in every case. He had also examined all the colours of the spectrum with the same result.

The experiments with pigments do not indicate what colours are to be considered as primary; but experiments on the prismatic spectrum show that all the colours of the spectrum, and therefore all the colours in nature, are equivalent to mixtures of three colours of the spectrum itself, namely, red, green (near the line E), and blue (near the line G). Yellow was found to be a mixture of red and green.

The speaker, assuming red, green, and blue as primary colours, then exhibited them on a screen by means of three magic lanterns, before which were placed glass troughs containing respectively sulphocyanide of iron, chloride of copper, and ammoniated copper.

A triangle was thus illuminated, so that the pure colours appeared at its angles, while the rest of the triangle contained the various mixtures of the colours as in Young's triangle of colour.

The graduated intensity of the primary colours in different parts of

* Described in the Trans. of the Royal Society of Edinburgh, Vol. XXI., and in the Phil. Mag.

the spectrum was exhibited by three coloured images, which, when superposed on the screen, gave an artificial representation of the spectrum.

Three photographs of a coloured ribbon taken through the three coloured solutions respectively, were introduced into the camera, giving images representing the red, the green, and the blue parts separately, as they would be seen by each of Young's three sets of nerves separately. When these were superposed, a coloured image was seen, which, if the red and green images had been as fully photographed as the blue, would have been a truly-coloured image of the ribbon. By finding photographic materials more sensitive to the less refrangible rays, the representation of the colours of objects might be greatly improved.

The speaker then proceeded to exhibit mixtures of the colours of the pure spectrum. Light from the electric lamp was passed through a narrow slit, a lens and a prism, so as to throw a pure spectrum on a screen containing three moveable slits, through which three distinct portions of the spectrum were suffered to pass. These portions were concentrated by a lens on a screen at a distance, forming a large, uniformly coloured image of the prism.

When the whole spectrum was allowed to pass, this image was white, as in Newton's experiment of combining the rays of the spectrum. When portions of the spectrum were allowed to pass through the moveable slits, the image was uniformly illuminated with a mixture of the corresponding colours. In order to see these colours separately, another lens was placed between the moveable slits and the screen. A magnified image of the slits was thus thrown on the screen, each slit showing, by its colour and its breadth, the quality and quantity of the colour which it suffered to pass. Several colours were thus exhibited, first separately, and then in combination. Red and blue, for instance, produced purple; red and green produced yellow; blue and yellow produced a pale pink; red, blue, and green produced white; and red and a bluish green near the line F produced a colour which appears very different to different eyes.

The speaker concluded by stating the peculiarities of colour-blind vision, and by showing that the investigation into the theory of colour is truly a physiological inquiry, and that it requires the observations and testimony of persons of every kind in order to discover and explain the various peculiarities of vision.

[J. C. M.]

WEEKLY EVENING MEETING,

Friday, May 24, 1861.

The Duke of Northumberland, K.G. F.R.S., President, in the Chair.

John O. Westwood, Esq. M.A. F.L.S.
HOPE PROFESSOR OF ZOOLOGY IN THE UNIVERSITY OF OXFORD.

On the Metamorphoses of Insects.

The marvellous in all ages has taken precedence in the human mind over the everyday phenomena of the routine of existence, and every thing not exactly in accordance with our ordinary ideas has become invested with something of the marvellous. Hence the metamorphoses sung by Ovid have been admired and extolled from the Roman age to our own, and old and young alike enjoy our Christmas pantomimes as much on account of the transformations effected by the wand of Harlequin, as for the gorgeous scenery with which they are accompanied. In nature, however, "truth is stranger than fiction," as manifested in the extraordinary changes of the animals which formed the subject of the discourse. To suppose a creature existing at one period of its life as an egg; then as a serpent; next, as burying itself in the ground, and encasing itself in a solid tomb, in which it remains for a time swathed up like an Egyptian mummy, and from which it ultimately bursts forth a changed and glorious being, flitting on delicately-constructed wings from flower to flower, would be regarded as the height of imagination without the truth were before us. And yet these are the transformations which, year by year and day by day, are being effected before our eyes in our fields and gardens.

There are two leading operations to which every living thing is constantly subjected—the taking in of new, and the rejection of already worked-up materials. Breathing and exhaling are instances of these operations, and it is mainly owing to these operations that growth and the accompanying rejection of portions of the animal frame (constituting what has been termed metamorphoses, in their most striking forms) are effected. In the vertebrated animals the fleshy covering is increased by constant depositions, but in animals encased in an external skeleton there is a restriction and obstruction offered by it to their growth, and the hard outer case must consequently be periodically got rid of. And it is this periodical "casting off" the outer envelope which results in the transformations of insects. A gourmand, after his dinner, unbuttons his waistcoat and takes his nap. Just the same is done by insects. The "Museum beetle," for example, having eaten all it wants, splits open its waistcoat along the back, and goes to sleep as a pupa, creeping out of its covering when it assumes the final state.

The relations of the Crustacea and Arachnida with the true insects was then dwelt upon, and it was stated that since the days of Linnæus (when it was supposed that the latter only underwent transformations) it had been discovered that equally striking changes of form are undergone by many of the wingless articulated animals having articulated legs. Even the barnacle (after the rejection of the old fancy that it was transformed into a goose), which had been supposed to be a mollusc, had been discovered to be subject to real transformations, its earliest state proving its crustaceous character, being extremely active and furnished with natatory limbs. These changes, first discovered by Mr. Vaughan Thompson, had been fully proved by Mr. Darwin, whose monograph on the Cirrhopoda was one of the most masterly works ever published. In like manner Mr. Vaughan Thompson was also the discoverer of the changes to which most of the true Crustacea are subject, the eggs of crabs, shrimps, &c., being developed into minute transparent animals, unlike their parents, with long spines projecting from the front and sides of their shell, and which swim about by the assistance of a long jointed tail and natatory limbs, which, in a later state, become reduced in size and altered in function, forming the foot-jaws of the adult animals, the real locomotive limbs being then developed, although previously they only existed in a rudimentary state beneath the carapace of the young animal.

The centipedes and millepedes, forming the class Myriapoda, were also subject to a series of changes, by which additional segments were from time to time developed out of the penultimate ring of the body, to which the name of the germinal segment had been applied. At each moulting, also, a fixed number of additional legs were developed in double pairs, attached to those segments, which had been produced at a previous moulting. For the most accurate knowledge of the changes of this class we are indebted to the labours of the late Mr. George Newport, whose early death was much to be lamented as a great loss to science.

The speaker then passed on to the true Insecta, which in their full development acquired wings. The great consumption of material here takes place in the larva state; the perfect insects are rarely voracious. The gnat, it is true, stings, and takes food in the shape of blood extracted from its victim, and so some others; but many perfect insects have only rudimental mouths, and all their sustenance has been taken in during their antecedent caterpillar state. The larvæ hatched from one ounce of silkworms' eggs, require 1609½ lbs. weight of leaves for their food; but as their digestive powers are not so strong as those of the higher animals, only about 771 lbs. weight of the pure leaves are digested, from which 120 lbs. of silk cocoons are produced. A single silkworm consumes, within thirty days, about 60,000 times its primitive weight; one hundred worms, just hatched, weighing about one grain. In their passage to the final winged state, a great difference is, as might be expected, manifested in the size and condition of the stomach. In the larva it occupies nearly the whole of the interior of the body, but in the subsequent changes it becomes gradually reduced to a very minute sac, a honey-stomach being developed, and becoming proportionately extended in size.

In the changes of insects there are two principal differences to be observed; some being active during their whole lives, without undergoing any deathlike or inactive state in their existence: whilst the others pass through an inactive pupa state, in which latter the metamorphosis is said to be complete, the perfect insect being quite unlike the earlier stages of its existence.

Of the former class, the mole cricket was selected as one example, and the habit of the female in burying its eggs in an oval cell in the soil, and the adaptation of the fore-legs of the parent to the operation of burrowing, were described. The larva has no wings; but in its general form and the structure of its legs it agrees with the perfect insect. In the pupa state the rudiments of wings are developed in the shape of scales upon the back, whilst in the perfect state the wings are of large size, of the most delicate gauze-like texture, and which, in order to protect them from injury underground, are capable of being folded up into a fan-like mass, scarcely thicker than a large pin, lying upon the back, thus forming no impediment to the subterranean progress of the insect. The larva and pupa states of the dragon-fly, and its aquatic predaceous habits, and the remarkable structure of its mouth, were next described.

Of the insects which undergo a deathlike pupa state, the common cockchafer served as an example, the larvæ of which make oval cells in the ground, in which they are transformed to inactive pupæ, with the limbs lying upon the breast. The grubs acquire their full size and undergo their pupa state at the spring season of the year, and are turned up in hundreds in the tillage of the ground required at that season, forming a great attraction to the rooks, which are to be seen following the plough in search of them as it passes along, their young being then newly hatched, and most ravenous in their demands for food. Here, therefore, we have an exemplification of those "Harmonies" which exist everywhere throughout nature.

The ant-lion, with its funnel-shaped pit for trapping its prey—the ichneumon fly, depositing its eggs in the bodies of other insects—the humble-bee, with its pollen brushes and pollen baskets—the common gnat, with its boat-like raft of eggs floating on the surface of the water—the cheese-hopper, with its hooked jaws and its two catches at its tail, by which it is able to concentrate the force for effecting its spring, and the common flea, were then described in detail as to their changes and habits, the whole being illustrated with large highly-magnified figures.

Some remarks were also made on silkworms, and the disease termed Muscardine, by which they are destroyed in vast numbers in the South of France and North of Italy. Through the breathing-pores at the sides of the body of the worms, the sporules or seeds of a very minute white fungus are inhaled, and the development taking place within the worm, the fungus plants grow out through the orifices and segments in the form of white mould. In New Zealand, one species of fungus attacks an underground species of caterpillar, and, when fully grown, the entire plant extends to the length of five or six inches.

The metamorphoses of insects were most complete in the Lepidoptera, or scaly-winged tribes (butterflies and moths); in the cabbage butterfly, for instance, out of the egg comes the caterpillar, which, after living its time of feasting, attaches itself by cords across its body, and then assumes the chrysalis state; bursting forth from this, the butterfly assumes its aerial condition. No wonder, then, that poets have seized upon this final transformation as a striking simile to express the release of the soul of man from its earthly bondage, and to typify its passage through the darkness of the grave to the bright realms of happiness and heaven.

[J. O. W.]

WEEKLY EVENING MEETING,

Friday, May 31, 1861.

THE LORD WENSLEYDALE, Vice-President, in the Chair.

DR. AUGUSTUS WALLER, F.R.S.

On the Nutrition and Reparation of Nerves.

[In consequence of Dr. WALLER's illness, the discourse was given by Dr. F. BOND.]

THE speaker commenced by pointing out that a knowledge of the conditions by which the structure and vitality of nerves were maintained in health, and their repair effected when injured by accident or disease, was not only of great interest to the physiologist, as enabling him to examine the distribution and functions of various portions of the nervous system, with which he must otherwise have remained unacquainted; but was also of the highest importance in a practical point of view, as providing us with the means of availing ourselves of those conservative powers of the animal economy which medical science was daily seeking more actively to utilize. It was not at all improbable that the practical applications which had been founded on recent researches as to the reproductive power of bone, might be rivalled by similar ones based upon the reproductive energy of nerve tissue.

With the view of explaining the nature of the researches which Dr. Waller had undertaken on this subject, the speaker gave a general outline of the structure and relations of the various parts of the nervous system, pointing out the distinction between the nervous centres and trunks, and comparing the structure of the more common form of nerve fibre,—the *white*, to a wax taper inserted in a tightly-fitting cylinder of paper, of which the wick represented the central *axis filament*, the paper cylinder *the investing nerve sheath*, and the wax the intervening *medullary substance;* the *grey* variety of nerve-fibre differing from the white in the absence of this medullary substance. The structure of nervous ganglia was also referred to, and the relations of the cells they contained to the fibres that passed through them was pointed out, as well as the important fact, first discovered by Sir C. Bell, that the sensory fibres of the ordinary spinal nerves alone pass through tne ganglia, and are connected with its cells.

In order to become acquainted with the means by which the nutrition of healthy nerves was maintained and their injuries repaired, it was necessary to study the effects which were produced in a nerve by separating it from its centre. Fontana and Michaelis, who were induced to examine the subject by Hunter, found that when a nerve was divided it reunited, and that new nerve-fibres were produced at the point of the union. Neither they, nor subsequent observers, however, had paid any attention to the lower portion of the divided nerve, into the condition of which Dr. Waller was the first to examine. The results of his researches had shown that the changes which took place here were of a most important character, consisting, in brief, of a complete disorganization of the medullary substance of nerve-fibre, which was then gradually removed, and afterwards re-created, whilst the axial filament and investing membrane remained intact.* Hence, if we examined the fibres of the lower end of a divided nerve a short time after section, we should find that they all appeared to be undergoing disorganization, and their medullary substance to be breaking up. But if we examined it at a much later period, the nerve-fibres, though rather smaller in size than before section, would have all the appearance of being perfectly healthy. This fact, which Dr. Waller had first attributed to a complete destruction of the old fibres and a formation of new ones, he was now satisfied was owing only to the changes he had just mentioned, that is, to the regeneration of the medullary substance alone, and not of the whole fibre.

This process of disorganization was accompanied by a complete loss of functions in the nerve, which were only slowly recovered after

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* Diagrams, illustrating these changes, as well as most of the other facts alluded to in the discourse, were exhibited; and microscopic specimens of disorganized nerve were afterwards shown in the Library.

the fibres had been reconstituted and re-united to their central organ. This real loss of function was to be distinguished from the apparent loss in which paralysis consists; for a motor nerve that was paralyzed by section, might still be stimulated to discharge its functions by galvanism for some days after it had been separated from its centre. No change, however, took place in the fibres of the *upper* portion of a divided nerve. The slowness with which the regenerated nerve acquired its normal functions, was probably due to the small size and imperfect conducting power of the new fibres in the cicatrix.

Some of the results which had followed from a knowledge of this process were then pointed out. In the first place, we learned from it how essential the medullary substance of the nerve-fibres is to their vitality, since its destruction and removal is followed by a complete abolition of their functions. Was it, it was suggested, that it bore to the less destructible axial filament and investing membrane a relation analogous to that which the exciting fluid in a voltaic cell does to the metallic poles with which it is in contact? In either case, the removal of the white substance, or the exciting fluid, caused a loss of power, which was restored by their renewal. The nutritive energy of the white substance, that is, the facility with which it underwent nutritive changes, was also dwelt upon, as indicating that it was a source of power, just as the chemical activity of the fluid in the voltaic cell regulated its potential energy. The readiness which the fluid element, in a voltaic cell, exhibits to undergo chemical change, and to enter into new combinations is, to a great extent, the gauge of the voltaic power which the cell of which it is a constituent is capable of developing. So the readiness with which the medullary constituent of the nerve-fibre undergoes decomposition, when compared with the more stable axis cylinder and investing membrane, which indicate a corresponding activity of nutrition, may be assumed to indicate that it is in some way a reservoir of power for the nerve-fibre, or, at least, that the decomposition which its nutrition involves is a condition of the development of nerve force in the fibre.

Another result was, the facility with which an application of this process of disorganization had enabled us to trace the distribution of various important nerves. Thus, Dr. Waller had been enabled by it to separate the fibres of the spinal accessory nerve from those of the vagus, and to trace each of them to their distribution in the lungs, heart, and stomach. It had also enabled him to determine the direction of the fibres of the sympathetic nerve in the neck. In fact, it provided physiologists with a means of tracing the course of nerves analogous to that which they had long possessed for tracing vessels, by injecting them with coloured fluids. He had traced in this way the minute distribution of the nerves in the tongue. Thus, we were provided with a means of research, the want of which an old anatomist (Rusych) had expressed, when he said that "he would have nothing further to wish for than to inject nerves, as he had succeeded in injecting blood-vessels."

The speaker then referred to the important conclusions to which the knowledge of the functions of these nerves so obtained had led MM. Claude Bernard and Brown-Sequard, and Dr. Waller, more especially as to the influence exerted by the sympathetic nerve over the circulation in the blood-vessels; and he mentioned a most interesting result which the latter had quite recently discovered, viz., that a powerful influence could be exerted on the functions of the heart, lungs, stomach, &c., by pressure on the vagus nerve in the neck; and that this could be carried so far as to produce complete insensibility. He considered this fact to be not only of much physiological interest, but also of great practical importance, as it provided us with a new means of producing anæsthesia; and he also believed that it might be made very useful in facilitating the diagnosis of certain nervous affections.

The researches of MM. Philippeaux and Vulpian, two French physiologists, which were in continuation of Dr. Waller's, had shown that the regenerative powers of nerves were even greater than he had mentioned; for they had found that a nerve could be regenerated, though unconnected with its centre, and even when transplanted to another part of the body.

The speaker then referred to the influence over the nutrition of nerves, which Dr. Waller's researches had shown their ganglia to possess, and to the corroboration which they gave to the theory of the *centrical* action of the ganglia and of the cells of which they were composed. If a nerve that was attached to a ganglion was divided a little way from its point of attachment, all the fibres below the section were decomposed, whilst those in connection with the ganglion retained their healthy structure, even though the connection of the ganglion itself with the great centres (the brain or spinal cord) was broken off. This showed that the ganglion maintained and regulated the vitality of the fibres which were attached to its cells. A ganglion, therefore, was to the fibres connected with it what a fountain was to the rivulet that trickled from it, *a source of nutritive energy.*

In conclusion, he drew attention to the extensive results which often flowed from very simple observations, instancing the discoveries above mentioned which had all more or less followed from his observance of the simple fact that a nerve-fibre when separated from its centre became disorganized; and he impressed upon his audience that the knowledge of the functions of the body in health was the only sure basis of treatment of its derangements in disease.

[A. W.]

GENERAL MONTHLY MEETING,

Monday, June 3, 1861.

The Rev. John Barlow, M.A. F.R.S. Vice-President, in the Chair.

The Secretary announced that His Grace the President had appointed the following Vice-Presidents for the ensuing year:—

The Lord Wensleydale.
The Lord Stanley, M.P. F.R.S.
Sir Henry Holland, Bart. D.C.L. D.D. F.R.S.
The Rev. John Barlow, M.A. F.R.S.
John Peter Gassiot, Esq. F.R.S. and
William Pole, Esq. F.R.S. *the Treasurer*

Frederick Augustus Burgett, Esq.
John Dobie, Esq.
Henry Wells Foote, Esq.
William W. Gull, M.D.
The Rev. John Philip Malleson, B.A. and
John Wells Wainwright, M.D.

were *elected* Members of the Royal Institution.

Henry Alfred Pitman, Esq., was *admitted* a Member of the Royal Institution.

The cordial thanks of the Members were returned to Sir Henry Holland, Bart. for his present of Forty Pounds, being the second annual continuation of his Donation in 1859.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same: viz.—

From
Secretary of State for War—Col. Sir Henry James's Instructions for taking Meteorological Observations. 8vo. 1860.
Board of Trade (through Mr. E. A. Bowring)—Fourth Report of the Commissioners for the Exhibition of 1851. 8vo. 1861.
Arts, Society of—Journal, May 1861. 8vo.
Astronomical Society, Royal—Monthly Notices, April 1861.

Bavarian Academy, Royal—Sitzungsberichte; 1860. Heft 4, 5. 8vo.
Cambridge Observatory (*the Syndicate*)—Astronomical Observations, Vol. XIX. (1852–4). 4to. 1861.
Cobbold, T. S. (*the Author*)—On the Scope, Tendency, and Value of the Natural History Sciences. 8vo. 1860.
Dance, Rev. H. A.—Dion Cassius Nicæus; Ælius Spartianus; Julius Capitolinus; Ælius Lampridius; Vulcatius Gallicanus. Ed. J. B. Egnatius. 12mo. Parisiis, 1544.
Editors—American Journal of Science, by B. Silliman, &c. for May 1861. 8vo.
Artizan for May 1861. 4to.
Athenæum for May 1861. 4to.
Chemical News for May 1861. 4to.
Engineer for May 1861. fol.
Horological Journal, Nos. 33, 34. 8vo. 1861.
Journal of Gas-Lighting for May 1861. 4to.
Mechanics' Magazine for May 1861. 8vo.
Medical Circular for May 1861. 8vo.
Practical Mechanic's Journal for May 1861. 4to.
Faraday, Professor, D.C.L. F.R.S.—Monatsberichte der Academie der Wissenschaften zu Berlin: January 1861. 8vo.
Jahrbücher der K.K. Central-Anstalt für Meteorologie und Erdmagnetismus. Von K. Kreil. Band VII. 4to. Ween, 1860.
Académie Royale de Belgique: Bulletins des Séances de la Classe des Sciences. 8vo. 1860. Annuaire: 1861. 16to.
A. Quetelet: Sur le Congrès International de Statistique, tenu à Londres, 1860. 4to.
Observations des Phénomènes Périodiques: 1859. 4to.
Physique du Globe, en Belgique. 8vo. 1861.
Franklin Institute of Pennsylvania—Journal, Vol. XLII. No. 4. 8vo. 1861.
Geographical Society, Royal—Proceedings, Vol. V. No. 2. 8vo. 1861.
Geological Society—Proceedings, May 1861. 8vo.
Journal, No. 66. 8vo. 1861.
Hamel. Dr. J.—On the Cochinelle from Mount Ararat [in Russian]. 8vo. 1835.
Irish Academy, Royal—Transactions, Vol. XXIV. Part 1. 4to. 1860.
Londesborough, the Lord—Illustrated Descriptive Catalogue of Antique Silver Plate: by F. W. Fairholt. 4to. 1860.
Mackie, S. J. Esq. F.G.S. (*the Editor*)—The Geologist, May 1861.
Medical and Chirurgical Society of London, Royal—Proceedings, Vol. III. No. 5. 1861.
Newton, Messrs.—London Journal (New Series) for May 1861. 8vo.
Photographic Society—Journal, No. 109. 8vo. 1861.
Petermann, A. Esq. (*the Editor*)—Mittheilungen auf der Gesammtgebiete der Geographie. 1861. No. 4. 4to. 1861.
Plateau, J. (*the Author*)—Recherches sur les Figures d'Equilibre d'une Masse liquide sans Pesanteur. 5e Série. 4to. 1861.
Roscoe, Professor H. E. (*the Author*)—On the Alleged Practice of Arsenic-eating in Styria. 8vo. 1861.
United Service Institution, Royal—Journal, No. 15. 8vo. 1861.
Webster, John, M.D. F.R.S.—On Iron, as a Material for Ship building, by a F.R.S. 8vo. 1861.

EXTRA EVENING MEETING,

Monday, June 3, 1861.

THE DUKE OF NORTHUMBERLAND, K.G. President, in the Chair.

C. T. NEWTON, Esq.
KEEPER OF CLASSICAL ANTIQUITIES AT THE BRITISH MUSEUM.

On the Mausoleum of Halicarnassus.

THE Mausoleum was originally constructed about the year B.C. 353, in honour of her husband Mausolus and as his tomb, by Artemisia, Queen of Halicarnassus. Two Greek architects, Satyrus and Phiteus, were employed on its erection; the most renowned sculptors of the age, Bryaxis, Timotheus, Leochares, and Scopas, were chosen to adorn its four sides with sculpture; while a fifth, Pythis, executed the marble quadriga which surmounted it. Several authors of antiquity have left records of the general character of this building. Thus, Pliny states that it was, in shape, a parallelogram, the northern and southern sides of which were, respectively, 63 feet long, while those to east and west were somewhat shorter; that it was surrounded by 36 columns, the support of a pyramid of 24 steps, which tapered towards the top; and that its total altitude was 140 feet. In Martial, it is described as hanging in the air, in allusion, probably, to its peculiar structure; a description which recalls to memory the tale of the Hanging Gardens of Babylon. Lucian, in one of his Dialogues, introduces Mausolus, who speaks of his tomb as rich in sculpture of men and horses of the choicest workmanship and material; and lastly, Pausanias dwells upon the fact of its great size, and declares that the Romans admired it so much, that they called all subsequent great tombs after its name, *Mausolea.*

It is clear, then, that it was of old regarded as an edifice of singular magnificence. Subsequently to the fall of the Roman empire, there are but scanty notices of it, yet these are enough to show that it still existed intact. Thus, in the fourth century, Gregory of Nazianzus alludes to it, as do also Constantinus Porphyro-Gennetus, and Eudocia in the tenth and eleventh; till, finally, in the twelfth, Eustathius says of it, "It was and is a wonder:" from which we may reasonably infer that it was at that time standing. From this period there is no record of it for some centuries, nor any means of ascertaining how far it had become a ruin, or when, indeed, the earthquake, to which its final overthrow may be most probably attributed, took place. In A.D. 1399, however, the Christian knights of Rhodes took possession of Halicarnassus, and fortified it with a citadel, which they called the Castle of St. Peter, and Fontanus, the historian of the siege of Rhodes, with which he was contemporary, tells how a German knight, Henry Schlegelholt, commenced building this fortress out of the ruins of the tomb of Mausolus, a fact which, Mr. Newton adds, is completely confirmed by his own examination of the present state of the site. It is also mentioned that this castle was twice subsequently repaired —first in A.D. 1482, and again in A.D. 1522. Of this last occasion and of the discovery of what, Mr. Newton, with reason, supposes to have been the actual tomb of Mausolus, a remarkable account has been published by M. Guichard, in 1581, who states that he heard this story from Dalechamp, the editor of Pliny, who received it from M. de la Tourrette, who was himself present at the last demolition of the Mausoleum, the object of which barbarous act was to procure squared stone for the construction of the castle.

Mr. Newton read an extract from this most curious narrative, and expressed his opinion that the gold ornaments, &c., said to have been seen by the knights within the tomb, were probably analogous with those discovered in the famous sepulchre of Koul-Oba, at Kertch.

Having given this slight sketch of what was previously known of the Mausoleum, Mr. Newton proceeded to describe the course of his own researches, which have had the remarkable success, not only of satisfactorily proving the position of this famous monument, above the Agora, in the centre of the ancient city, but also of determining the style of its art and the general character of its structure. He stated that he was first led to make inquiries about the Mausoleum by the arrival, in 1846, at the British Museum, of twelve sculptured slabs, which had been obtained from the walls of the castle of St. Peter through the influence of Lord Stratford de Redcliffe, then British ambassador at Constantinople; and that he was further stimulated to make excavations, with the view of finding the actual site of the tomb, by having noticed, in 1855, several lions' heads still projecting from the walls of this castle, which, like the slabs previously procured, he had no doubt had once belonged to the Mausoleum.

In 1856 he commenced excavating, and at intervals pursued his researches till the spring of 1858, proceeding gradually, step by step, by mining under ground covered by modern Turkish houses and gardens, which he had to buy up. He began near a spot where, many years ago, Professor Donaldson had noticed the remains of a superb Ionic edifice, and where he had himself observed many fragments of Ionic columns, the walls of the fields and the houses around being, for the most part, built of fragments of sculpture and architecture in Parian marble. He soon came upon a portion of the body of a colossal lion built into a modern wall; and shortly after, on several fragments of frieze in high relief, and many architectural mouldings. Not long after this, he met with the torso of a colossal equestrian figure in an Asiatic dress, and four slabs of the frieze in the finest condition; after clearing out the site of the building itself, he discovered on the north the Peribolus wall on this side, in an almost perfect state, and beyond it many fragments of statues, which, on being reunited in England, have produced a male and female figure of exquisite workmanship (the former, doubtless, a representation of Mausolus himself, and the latter of a goddess who must have stood near him in the quadriga), together with all the architectural portions required for the determination of the Order, viz. drums of columns, bases, capitals, the two stones of the architrave, the bed-mould of the cornice, and the cornice itself. Besides these, great portions of two colossal horses, unquestionably those of the marble quadriga, executed by Pythis, were discovered, and a number of slabs which there is reason to believe formed the steps of the pyramid, together with portions of the felly, spokes, and the outer rim of one of its wheels.

By the middle of 1857, Mr. Newton succeeded in tracing out the base lines of the original building (nearly every fragment of which had been removed by the Knights, or subsequently), and had proved that the area wherein the edifice had stood was a parallelogram, the western side of which was 110 feet long and the southern 126. The whole of this arena was cut out of the native rock, to depths varying from two to sixteen feet below the surface of the surrounding fields.

Mr. Newton then proceeded to discuss the evidences as to the character of the design of the Mausoleum, as determinable from the fragments he had excavated, and pointed out the difficulties which had beset earlier inquirers in their attempt to reconstruct the Mausoleum from the descriptions of the ancients. He remarked, that architects had been prone to imagine corruptions in the texts of the old writers, whenever the numbers given by them did not happen to square with their modern theories; but, that in this case, a recent collation of the MSS. had shown that there was no important variation in the readings; that Pliny's smaller dimensions of 63 feet must be taken to be the measurement of the *cella* of the building; and that his "*totus circuitus*" of 411 feet must relate to the entire area occupied by the thirty-six columns which surrounded this *cella*. Mr. Newton further showed that, by the dimensions afforded by the treads of the steps, this circumference could be shown to be 412 feet, a coincidence of numbers with that given by Pliny too remarkable to be accidental.

Further elements for calculation were also provided by the happy discovery of the piece of the rim of the chariot wheel; for, by means of this, it was easy to strike the curve, and to ascertain that the total diameter of the wheel must have been 7 feet 7 inches. The length of the horses was about 10 feet, and the entire length of the platform might thus be easily calculated. In the same way the half-diameter of the wheel combined with that of the statue of Mausolus gave the means of calculating the height of the chariot group.

Mr. Newton then went on to show that it might be further calculated from existing remains, that the height of the order was the same as that of the pyramidal portion it supported, and that, therefore, of the 140 feet of total height, 75 would be occupied by the columns, architrave, and pyramid, leaving 65 unaccounted for. This was a puzzle which the theoretical restorers had failed hitherto to resolve; but a comparison with the tombs still existing in the adjoining country, in Caria itself, at Mylasa in Lycia, and at Souma in Algeria, shows that it was not unusual to erect such monuments on very lofty basements. He added, that the scattered composition of the frieze and the elongated proportions of the figures on it were adapted to be seen at a great height above the eye, as would be the case in the proposed restoration.

Mr. Newton concluded his discourse by stating that he had, he believed, discovered the very stone which had closed the entrance to the original sepulchre of the king,—a huge block of marble, weighing ten tons, carefully grooved at the edges and then lowered by machinery into sockets, like a portcullis. Close to it was also a staircase, which he believed was made to enable the body of Mausolus to be lowered into its resting-place. The numerous statues of lions which had been met with, must have been placed round the tomb to guard it. Lastly, Mr. Newton added, that the whole of the sculptures had once been painted, the flesh generally a dun colour, with an ultramarine background.

[C. T. N.]

WEEKLY EVENING MEETING,

Friday, June 7, 1861.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

John Tyndall, Esq. F.R.S.

PROFESSOR OF NATURAL PHILOSOPHY, ROYAL INSTITUTION.

On the Physical Basis of Solar Chemistry.

Omitting all preface, the speaker drew attention to an experimental arrangement intended to prove that gaseous bodies radiate heat in different degrees. Behind a double screen of polished tin was placed an ordinary ring gas-burner; on this was placed a hot copper ball, from which a column of heated air ascended: behind the screen, but so placed that no ray from the ball could reach the instrument, was an excellent thermo-electric pile, connected by wires with a very delicate galvanometer. The thermo-electric pile was known to be an instrument whereby heat was applied to the generation of electric currents; the strength of the current being an accurate measure of the quantity of the heat. As long as both faces of the pile were at the same temperature, no current was produced; but the slightest difference in the temperature of the two faces at once declared itself by the production of a current, which, when carried through the galvanometer, indicated by the deflection of the needle both its strength and its direction.

The two faces of the pile were in the first instance brought to the same temperature; the equilibrium being shown by the needle of the galvanometer standing at zero. The rays emitted by the current of hot air already referred to were permitted to fall upon one of the faces of the pile; and an extremely slight movement of the needle showed that the radiation from the hot air, though sensible, was extremely feeble. Connected with the ring-burner was a holder containing oxygen gas; and by turning a cock, a stream of this gas was permitted to issue from the burner, strike the copper ball, and ascend in a heated column in front of the pile. The result was, that oxygen showed itself, as a radiator of heat, to be quite as feeble as atmospheric air.

A second holder containing olefiant gas was also connected by its own system of tubes with the ring-burner. Oxygen had already flowed over the ball and cooled it in some degree. Hence, as a radiator in comparison with oxygen, the olefiant gas laboured under a disadvantage. It was purposely arranged that this should be the case; so that if, notwithstanding its being less hot, the olefiant gas showed itself a better radiator, its claim to superiority in this respect would be decisively proved. On permitting the gas to issue upwards, it cast an amount of heat against the adjacent face of the pile sufficient to impel the needle of the galvanometer almost to its stops at 90°. This experiment proved the vast difference between two equally transparent gases with regard to their power of emitting radiant heat.

The converse experiment was now performed. The thermo-electric pile was removed and placed between two cubes filled with water kept in a state of constant ebullition; and it was so arranged that the quantities of heat falling from the cubes on the opposite faces of the pile were exactly equal, thus neutralizing each other. The needle of the galvanometer being at zero, a sheet of oxygen gas was caused to issue from a slit between one of the cubes and the adjacent face of the pile. If this sheet of gas possessed any sensible power of intercepting the thermal rays from the cube, one face of the pile being deprived of the heat thus intercepted, a difference of temperature between its two faces would instantly set in, and the result would be declared by the galvanometer. The quantity absorbed by the oxygen under those circumstances was too feeble to affect the galvanometer; the gas, in fact, proved sensibly transparent to the rays of heat. It had but a feeble power of radiation: it had an equally feeble power of absorption.

The pile remaining in its position, a sheet of olefiant gas was caused to issue from the same slit as that through which the oxygen had passed. No one present could see the gas; it was quite invisible, the light went through it as freely as through oxygen or air; but its effect upon the thermal rays emanating from the cube, was what might be expected from a sheet of metal. A quantity so large was cut off, that the needle of the galvanometer, promptly quitting the zero line, moved with energy to its stops: thus the olefiant gas, so light and clear and pervious to luminous rays, was a most potent destroyer of the rays emanating from an obscure source. The reciprocity of action established in the case of oxygen comes out here; the good radiator is found by this experiment to be the good absorber.

This result, which was exhibited before a public audience this evening for the first time, was typical of what had been obtained with gases generally. Going through the entire list of gases and vapours in this way, we should find radiation and absorption to be as rigidly associated as positive and negative in electricity, or as north and south polarity in magnetism. The gas which, when heated, is most competent to generate a calorific ray, is precisely that which is most competent to stop such a ray. If the radiation be high, the absorption is high; if the radiation be moderate, the absorption is moderate; if the radiation be low, the absorption is low; so that if we make the number which expresses the absorptive power the numerator of a fraction, and that which expresses its radiative power, the denominator, the result would be, that, on account of the numerator and denominator varying in the same proportion, the value of that fraction would always remain the same, whatever might be the gas or vapour experimented with.

But why should this reciprocity exist? What is the meaning of absorption? what is the meaning of radiation? When you cast a stone into still water, rings of waves surround the place where it falls; motion is radiated on all sides from the centre of disturbance. When the hammer strikes a bell, the latter vibrates; and sound, which is nothing more than an undulatory motion of the air, is radiated in all directions. Modern philosophy reduces light and heat to the same mechanical category. A luminous body is one with its particles in a state of vibration; a hot body is one with its particles also vibrating, but at a rate which is incompetent to excite the sense of vision; and, as a sounding body has the air around it, through which it propagates its vibrations, so also the luminous or heated body has a medium, called ether, which accepts its motions and carries them forward with inconceivable velocity. Radiation, then, as regards both light and heat, is *the transference of motion from the vibrating body to the ether in which it swings:* and, as in the case of sound, the motion imparted to the air is soon transferred to the surrounding objects, against which the aerial undulations strike, the sound being, in technical language, *absorbed;* so also with regard to light and heat, absorption consists in *the transference of motion from the agitated ether to the particles of the absorbing body.*

The simple atoms are found to be bad radiators; the compound atoms good ones: and the higher the degree of complexity in the atomic grouping, the more potent, as a general rule, is the radiation and absorption. Let us get definite ideas here, however gross, and purify them afterwards by the process of abstraction. Imagine our simple atoms swinging like single spheres in the ether; they cannot create the swell which a group of them united to form a system can produce. An oar runs freely edgeways through the water, and imparts far less of its motion to the water than when its broad flat side is brought to bear upon it. In our present language the oar, broad side vertical, is a good radiator; broad side horizontal, it is a bad radiator. Conversely the waves of water, impinging upon the flat face of the oar-blade, will impart a greater amount of motion to it than when impinging upon the edge. In the position in which the oar radiates well, it also absorbs well. Simple atoms glide through the ether without much resistance; compound ones encounter this, and yield up more speedily their motion to the ether. *Mix* oxygen and nitrogen mechanically, they absorb and radiate a certain amount. Cause these gases to *combine* chemically and form nitrous oxide, both the absorption and radiation are thereby augmented 250 times!

In this way we look with the telescope of the intellect into atomic systems, and obtain a conception of processes which the eye of sense can never reach. But gases and vapours possess a power of choice as to the rays which they absorb. They single out certain groups of rays for destruction, and allow other groups to pass unharmed. This is best illustrated by a famous experiment of Sir David Brewster's, modified to suit the requirements of the present discourse. Into a glass cylinder, with its ends stopped by discs of plate-glass, a small quantity of nitrous acid gas was introduced; the presence of the gas being indicated by its rich brown colour. The beam from an electric lamp being sent through two prisms of bisulphide of carbon, a spectrum seven feet long and eighteen inches wide was cast upon a screen. Introducing the cylinder containing the nitrous acid into the path of the beam as it issued from the lamp, the splendid and continuous spectrum became instantly furrowed by numerous dark bands, the rays answering to which were struck down by the nitric gas, while it permitted the light which fell upon the intervening spaces to pass with comparative impunity.

Here also the principle of reciprocity, as regards radiation and absorption, holds good; and could we, without otherwise altering its physical character, render that nitrous gas luminous, we should find that the very rays which it absorbs are precisely those which it would emit. When atmospheric air and other gases are brought to a state of intense incandescence by the passage of an electric spark, the spectra which we obtain from them consist of a series of bright bands. But such spectra are produced with the greatest brilliancy, when, instead of ordinary gases, we make use of metals heated so highly as to volatilize them. This is easily done by the voltaic current. A capsule of carbon was filled with mercury, which formed the positive electrode of the electric lamp; a carbon point was brought down upon this; and on separating one from the other, a brilliant arc containing the mercury in a volatilized condition passed between them. The spectrum of this arc was not continuous like that from the solid carbon points, but consisted of a series of vivid bands, each corresponding in colour to that particular portion of the spectrum to which its rays belonged. Copper gave its system of bands; zinc gave its system; and brass, which is an alloy of copper and zinc, gave a splendid spectrum made up of the bands belonging to both metals.

Not only, however, when metals are united like zinc and copper to form an alloy, is it possible to obtain the bands which belonged to them. No matter how we may disguise the metal—allowing it to unite with oxygen to form an oxide, and this again with an acid to form a salt; if the heat applied be sufficiently intense, the bands belonging to the metal reveal themselves with perfect definition. Holes were drilled in a cylinder of retort carbon, and these being filled with pure culinary salt, the carbon was made the positive electrode of the lamp: the resultant spectrum showed the brilliant yellow lines of the metal sodium. Similar experiments were made with the chlorides of strontium, calcium, lithium,* and other metals; each salt gave the bands due to the metal. Different salts were then mixed together, and rammed into the holes in the carbon; a spectrum was obtained which contained the bands of them all.

The position of these bright bands never varies, and each metal has its own system. Hence the competent observer can infer from the bands of the spectrum the metals which produce it. It is a language addressed to the eye instead of the ear; and the certainty would not be augmented if each metal possessed the power of audibly calling out, "I am here!" Nor is this language affected by distance. If we find that the sun or the stars give us the bands of our terrestrial metals, it is a declaration on the part of these orbs that such metals enter into their composition. Does the sun give us any such intimation? Does the solar spectrum exhibit bright lines which we might compare with those produced by our terrestrial metals, and prove either their identity or difference? No. The solar spectrum, when closely examined, gives us a multitude of fine dark lines instead of bright ones. They were first noticed by Dr. Wollaston, were investigated with profound skill by Fraunhofer, and named from him Fraunhofer's lines. They have been long a standing puzzle to philosophers. The bright lines which the metals give us have been also known to us for years; but the connection between both classes

* The vividness of the colours of the lithium spectrum is extraordinary: it contained a blue band of indescribable splendour. It was thought by many, during the discourse, that I had mistaken strontium for lithium, as this blue band had never before been seen. I have obtained it many times since; and my friend Dr. Miller, having kindly analyzed the substance made use of, pronounces it chloride of lithium.—J. T.

of phenomena was wholly unknown, until Kirchhoff, with admirable acuteness, revealed the secret, and placed it at the same time in our power to chemically analyze the sun.

We have now some hard work before us; hitherto we have been delighted by objects which addressed themselves rather to our æsthetic taste than to our scientific faculty. We have ridden pleasantly to the base of the final cone of Etna, and must now dismount and march wearily through ashes and lava, if we would enjoy the prospect from the summit. Our problem is to connect the dark lines of Fraunhofer with the bright ones of the metals. The white beam of the lamp is refracted in passing through our two prisms, but its different components are refracted in different degrees, and thus its colours are drawn apart. Now the colour depends solely upon the rate of oscillation of the particles of the luminous body; red light being produced by one rate, blue light by a much quicker rate, and the colours between red and blue by the intermediate rates. The solid incandescent coal-points give us a continuous spectrum; or in other words they emit rays of all possible periods between the two extremes of the spectrum. They have particles oscillating so as to produce red; others, to produce orange; others, to produce yellow, green, blue, indigo, and violet respectively. Colour, as many of you know, is to light what *pitch* is to sound. When a violin-player presses his finger on a string he makes it shorter and tighter, and thus, causing it to vibrate more speedily, augments the pitch. Imagine such a player to move his finger slowly along the string, shortening it gradually as he draws his bow, the note would rise in pitch by a regular gradation; there would be no gap intervening between note and note. Here we have the analogue to the continuous spectrum, whose colours insensibly blend together without gap or interruption, from the red of the lowest pitch to the violet of the highest. But suppose the player, instead of gradually shortening his string, to press his finger on a certain point, and to sound the corresponding note; then to pass on to another point more or less distant, and sound its note; then to another, and so on, thus sounding particular notes separated from each other by gaps which correspond to the intervals of the string passed over; we should then have the exact analogue of a spectrum composed of separate bright bands with intervals of darkness between them. But this, though a perfectly true and intelligible analogy, is not sufficient for our purpose; we must look with the mind's eye at the very oscillating atoms of the volatilized metal. Figure these atoms connected by springs of a certain tension, and which, if the atoms are squeezed, together push them asunder, or if the atoms are drawn apart, pull them together, causing them, before coming to rest, to quiver at a certain definite rate determined by the strength of the spring. Now the volatilized metal which gives us one bright band is to be figured as having its atoms united by springs all of the same tension, its vibrations are all of one kind. The metal which gives us two bands may be figured as having some of its atoms united by springs of one tension, and others by a second series of springs of a different tension. Its vibrations are of two distinct kinds; so also when we have three or more bands, we are to figure as many distinct sets of springs, each set capable of vibrating in its own particular time and at a different rate from the other. If we seize this idea definitely, we shall have no difficulty in dropping the metaphor of springs, and substituting for it mentally the forces by which the atoms act upon each other. Having thus far cleared our way, let us make another effort to advance.

Here is a pendulum,—a heavy ivory ball suspended from a string. I blow against this ball; a single puff of my breath moves it a little way from its position of rest; it swings back towards me, and when it reaches the limit of its swing I puff again. It now swings further; and thus by timing my puffs I can so accumulate their action as to produce oscillations of large amplitude. The ivory ball here has absorbed the motions which my breath communicated to the air. I now bring the ball to rest. Suppose, instead of my breath, a wave of air to strike against it, and that this wave is followed by a series of others which succeed each other exactly in the same intervals as my puffs; it is perfectly manifest that these waves would communicate their motion to the ball and cause it to swing as the puffs did. And it is equally manifest that this would not be the case if the impulses of the waves were not properly timed; for then the motion imparted to the pendulum by one wave would be neutralized by another, and there could not be that accumulation of effect which we have when the periods of the waves correspond with the periods of the pendulum. So much for the kind of impulses absorbed by the pendulum. But such a pendulum set oscillating in air produces waves in the air; and we see that the waves which it produces must be of the same period as those whose motions it would take up or absorb most copiously if they struck against it. Just in passing I may remark, that if the periods of the waves be double, treble, quadruple, &c., the periods of the pendulum, the shocks imparted to the latter would also be so timed as to produce an accumulation of motion.

Perhaps the most curious effect of these timed impulses ever described was that observed by a watchmaker, named Ellicott, in the year 1741. He set two clocks leaning against the same rail; one of them, which we may call A, was set going; the other, B, not. Some time afterwards he found, to his surprise, that B was ticking also. The pendulums being of the same length the shocks imparted by the ticking of A to the rail against which both clocks rested were propagated to B, and were so timed as to set B going. Other curious effects were at the same time observed. When the pendulums differed from each other a certain amount, A set B going, but the re-action of B stopped A. Then B set A going, and the re-action of A stopped B. If the periods of oscillation were close to each other, but still not quite alike, the clocks mutually controlled each other, and by a kind of mutual compromise they ticked in perfect unison.

But what has all this to do with our present subject? They are mechanically identical. The varied actions of the universe are all modes of motion; and the vibration of a ray claims strict brotherhood with the vibrations of our pendulum. Suppose ethereal waves striking upon atoms which oscillate in the same periods as the waves succeed each other, the motion of the waves will be absorbed by the atoms; suppose we send our beam of white light through a sodium flame, the particles of that flame will be chiefly affected by those undulations which are synchronous with their own periods of vibration. There will be on the part of those particular rays a transference of motion from the agitated ether to the atoms of the volatilized sodium, which, as already defined, is absorption. We use glass screens to defend us from the heat of our fires; how do they act? Thus:—The heat emanating from the fire is for the most part due to radiations which are incompetent to excite the sense of vision; we call these rays obscure. Glass, though pervious to the luminous rays, is opaque in a high degree to those obscure rays, and cuts them off, while the cheerful light of the fire is allowed to pass. Now mark me clearly. The heat cut off from your person is to be found in the glass, the latter becomes heated and radiates towards your person; what then is the use of the glass if it merely thus acts as a temporary halting-place for the rays, and sends them on afterwards. It does this:—It not only sends the heat it receives towards you, but scatters it also in all other directions round the room. Thus the rays which, were the glass not interposed, would be shot directly against your person, are for the most part diverted from their original direction, and you are preserved from their impact.

Now for our experiment. I pass the beam from the electric lamp through the two prisms, and the spectrum spreads its colours upon the screen. Between the lamp and the prism I interpose this snapdragon light. Alcohol and water are here mixed up with a quantity of common salt, and the metal dish that contains them is heated by a spirit-lamp. The vapour from the mixture ignites and we have this monochromatic flame. Through this flame the beam from the lamp is now passing; and observe the result upon the spectrum. You see a dark band cut out of the yellow,—not very dark, but sufficiently so to be seen by everybody present. Observe how the band quivers and varies in shade as the amount of yellow light cut off by the unsteady flame varies in amount. The flame of this monochromatic lamp is at the present moment casting its proper yellow light upon that shaded line; and more than this, it casts, in part, the light which it absorbs from the electric lamp upon it; but it scatters the greater portion of this light in other directions, and thus withdraws it from its place upon the screen, as the glass, in the case above supposed, diverted the heat of the fire from your person. Hence the band appears dark; not absolutely, but dark in comparison with the adjacent brilliant portions of the spectrum.

But let me exalt this effect. I place in front of the electric lamp the intense flame of a large Bunsen's burner. I have here a platinum capsule into which I put a bit of sodium less than a pea in magnitude. The sodium placed in the flame soon volatilizes and burns with brilliant incandescence. Observe the spectrum. The yellow band is clearly and sharply cut out, and a band of intense obscurity occupies its place. I withdraw the sodium, the brilliant yellow of the spectrum takes its proper place: I reintroduce the sodium and the black band appears.

Let me be more precise:—The yellow colour of the spectrum extends over a sensible space, blending on one side into orange and on the other into green. The term "yellow band" is therefore somewhat indefinite. I want to show you that it is the precise yellow band emitted by the volatilized sodium which the same substance absorbs. By dipping the coal-point used for the positive electrode into a solution of common salt, and replacing it in the lamp, I obtain that bright yellow band which you now see drawn across the spectrum. Observe the fate of that band when I interpose my sodium light. It is first obliterated, and instantly that black streak occupies its place. See how it alternately flashes and vanishes as I withdraw and introduce the sodium flame!

And supposing that instead of the flame of sodium alone, I introduce into the path of the beam a flame in which lithium, strontium, magnesium, calcium, &c., are in a state of volatilization, each metallic vapour would cut out its own system of bands, each corresponding exactly in position with the bright band which that metal itself would cast upon the screen. The light of our electric lamp then shining through such a composite flame would give us a spectrum cut up by dark lines, exactly as the solar spectrum is cut up by the lines of Fraunhofer.

And hence we infer the constitution of the great centre of our system. The sun consists of a nucleus which is surrounded by a flaming atmosphere. The light of the nucleus would give us a continuous spectrum, as our common coal-points did; but having to pass through the photosphere, as our beam through the flame, those rays of the nucleus which the photosphere can itself emit are absorbed, and shaded spaces, corresponding to the particular rays absorbed, occur in the spectrum. Abolish the solar nucleus, and we should have a spectrum showing a bright band in the place of every dark line of Fraunhofer. These lines are therefore not absolutely dark, but dark by an amount corresponding to the difference between the light of the nucleus intercepted by the photosphere, and the light which issues from the latter.

The man to whom we owe this beautiful generalization is Kirchhoff, Professor of Natural Philosophy in the university of Heidelberg; but, like every other great discovery, it is compounded of various elements. Mr. Talbot observed the bright lines in the spectra of coloured flames. Sixteen years ago Dr. Miller gave drawings and descriptions of the spectra of various coloured flames. Wheatstone, with his accustomed ingenuity, analyzed the light of the electric spark, and showed that the metals between which the spark passed determined the bright bands in the spectrum of the spark. Masson published a prize essay on these bands; Van der Willigen, and more recently Plücker, have given us beautiful drawings of the spectra, obtained from the discharge of Ruhmkorff's coil. But none of these distinguished men betrayed the least knowledge of the connection between the bright bands of the metals and the dark lines of the solar spectrum. The man who came nearest to the philosophy of the subject, was Ängström. In a paper translated from Poggendorff's "Annalen" by myself, and published in the "Philosophical Magazine" for 1855, he indicates that the rays which a body absorbs are precisely those which it can emit when rendered luminous. In another place, he speaks of one of his spectra giving the general impression of *reversal* of the solar spectrum. Foucault, Stokes, and Thomson, have all been very close to the discovery; and, for my own part, the examination of the radiation and absorption of heat by gases and vapours, some of the results of which I placed before you at the commencement of this discourse, would have led me in 1859 to the law on which all Kirchhoff's speculations are founded, had not an accident withdrawn me from the investigation. But Kirchhoff's claims are unaffected by these circumstances. True, much that I have referred to formed the necessary basis of his discovery; so did the laws of Kepler furnish to Newton the basis of the theory of gravitation. But what Kirchhoff has done carries us far beyond all that had before been accomplished. He has introduced the order of law amid a vast assemblage of empirical observations, and has ennobled our previous knowledge by showing its relationship to some of the most sublime of natural phenomena.

[J. T.]

GENERAL MONTHLY MEETING,

Monday, July 1, 1861.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

William Beckett, Esq.
Alexander John Ellis, Esq.
Hardinge Giffard, Esq. and
Joseph Neuberg, Esq.

were *elected* Members of the Royal Institution.

John Dobie, Esq.
W. H. Stone, Esq. and
John Wells Wainwright, M.D.

were *admitted* Members of the Royal Institution.

The Secretary announced that the Fullerian Professorship of Physiology was vacant, and that the Managers would appoint a Professor on May 12, 1862.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same: viz.

FROM

Board of Trade (through Capt. D. Galton)—Report of Commissioners on Submarine Telegraph Cables. fol. 1861.
British Museum Trustees—The Cuneiform Inscriptions of Western Asia. Vol. I. Prepared for Publication by Sir H. C. Rawlinson and E. Norris, Esq. fol. 1861.
Arts, Society of—Journal, June 1861. 8vo.
Asiatic Society, Royal—Journal, Vol. XVIII. Part 2. 8vo. 1861.
Asiatic Society of Bengal—Journal, No. 280. 8vo. 1860.
Astronomer Royal—Report on Greenwich Observatory. 1861.
Astronomical Society, Royal—Monthly Notices, May 1861.
Bavarian Academy, Royal—Sitzungsberichte, 1861; Heft 1. 8vo.
Commissioners in Lunacy—Eighteenth Report. 8vo. 1861.
Cornwall Polytechnic Society, Royal—Twenty-eighth Annual Report. 8vo. 1860.
Editors—Artizan for June 1861. 4to.
Athenæum for June 1861. 4to.
Chemical News for June 1861. 4to.
Engineer for June 1861. fol.
Horological Journal, No. 35. 8vo. 1861.
Journal of Gas-Lighting for June 1861. 4to.
Mechanics' Magazine for June 1861. 8vo.
Medical Circular for June 1861. 8vo.
Practical Mechanics' Journal, for June 1861. 4to.
Franklin Institute of Pennsylvania—Journal, Vol. XLII. Nos. 5, 6. 8vo. 1861.
Geographical Society, Royal—Proceedings, Vol. V. No. 2. 8vo. 1861.
Geological Society—Proceedings, May 1861. 8vo.
Geological Survey of India—Memoirs. Vol. II. Part 2. 8vo. 1860.
Annual Report. 1859-60. 8vo.
Gill, Joseph, Esq. (the Author)—Essay on the Thermo-Dynamics of Elastic Fluids. 8vo. 1860.
Horticultural Society, Royal—Proceedings, No. 25. 8vo. 1861.
Linnean Society—Proceedings, No. 20, and 2nd Supplement to Botany. 8vo. 1861.
Mackie, S. J. Esq. F.G.S. (the Editor)—The Geologist, June 1861.
Newton, Messrs.—London Journal (New Series) for June 1861. 8vo.
Petermann, A. Esq. (the Editor)—Mittheilungen auf der Gesammtgebiete der Geographie. 1861. No. 5. 4to. 1861.
Photographic Society—Journal, No. 110. 8vo. 1860.
Poey, Andrès (the Author)—Catalogue Chronologique des Tremblements de Terre ressentis dans les Indes Occidentales de 1530 à 1858. 8vo. 1858.
Various Meteorological Papers (in French). 8vo and 4to. 1859-60.
Relation Historique et Théorie des Images Photo-électriques de la Foudre. (A.D. 360-1860.) 16to. 1861.
Séguin, M. (the Author)—Origine et Propagation de la Force, Paris. 8vo. 1859.
Considérations sur les Lois qui président à l'Accomplissement des Phénomènes Naturelles, rapportés à l'Attraction Newtonienne. 8vo. Paris, 1861.
Statistical Society—Journal, Vol. XXIV. Part 2. 8vo. 1860.
Vincent, B. Keeper of Library R.I. (the Editor)—Haydn's Dictionary of Dates for Universal Reference. 10th Edition. 8vo. 1861. (2 *Copies.*)
Whitecross, James W. Esq. (the Author)—Sketches and Characters; or, the Natural History of Human Intellects. 8vo. 1853.

Royal Institution of Great Britain.

1861.

GENERAL MONTHLY MEETING,

Monday, November 4, 1861.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

The Rev. Wm. H. A. Wentworth Bowyer, M.A.
was *elected* a Member of the Royal Institution.

Alexander J. Ellis, Esq.
was *admitted* a Member of the Royal Institution.

The Secretary announced that the following arrangements had been made for the Lectures during the ensuing season:—

Christmas Lectures.—Professor Tyndall—Six Lectures on Light (adapted to a Juvenile Auditory).

Before Easter, 1862.—Professor J. Marshall, F.R.S.—Twelve Lectures on the Physiology of the Senses.

Professor Tyndall, F.R.S.—Twelve Lectures on Heat.

Rev. A. J. D'Orsey—Five Lectures on the English Language.

H. F. Chorley, Esq.—Four Lectures on National Music.

Professor H. E. Roscoe—Three Lectures on Spectrum Analysis.

After Easter.—C. T. Newton, Esq.—Four Lectures on the History of Ancient Art; illustrated by Examples in the British Museum.

Rev. G. Butler—Three Lectures on the Art of the Last Century.

Professor Lyon Playfair, C.B. F.R.S.—Six Lectures on some of the Chemical Arts, with reference to their Progress between the Two Great Exhibitions of 1851 and 1862.

Professor T. Anderson, M.D. F.R.S.E.—Seven Lectures on Agricultural Chemistry.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same: viz.

From

Her Majesty's Government (through Sir R. I. Murchison)—Memoirs of the Geological Survey:—
Mineral Statistics for 1858. Part 2; and for 1860. 8vo. 1860–1.
Geology of Parts of England and Scotland. 10 Parts. 8vo. 1860.
Geology of Trinidad. 8vo. 1860.
Iron Ores of Great Britain. Part 3. 8vo. 1861.
Secretary of State for War (through Sir H. James)—Abstracts of the Principal Lines of Spirit-levelling in England and Wales. By Sir H. James. 2 vols. 4to. 1861.
Meterological Department of the Board of Trade (through Admiral Fitzroy)—Meteorological Papers, Nos. 3, 5, 6, 7, 8, 9, 10. 4to and 8vo. 1861.
Compiled by Admiral Fitzroy:—
Barometer and Weather Guide. 8vo. 1861.
Barometer Manual. 8vo. 1861.
Passage-Table and Sailing Directions. (L 13) 8vo. 1861.
Secretary of State for India—Report of Three Surveys of India. (P 5) fol. 1861.
Governor of the State of Arkansas, U.S.—Second Report of the Geological Reconnaissance of Arkansas. 8vo. 1860.
Actuaries, Institute of—Assurance Magazine, No. 44. 8vo. 1861.
Agricultural Society, Royal—Journal, No. 47. 8vo. 1861.
American Academy of Arts and Sciences—Proceedings, Vol. IV. Nos. 32–55. Vol. V. Nos. 1–29. 8vo. 1859–60.
American Philosophical Society—Proceedings, No. 63. 8vo. 1860.
Arnott, Neil, M.D. F.R.S. M.R.I. (the Author)—Survey of Human Progress, from the Savage State to the Highest Civilization yet attained. 8vo. 1861.
Arts, Society of—Journal, July to October 1861. 8vo.
Asiatic Society of Bengal—Journal, No. 281. 8vo. 1861.
Asiatic Society, Royal—Journal, Vol. XIX. Part 1. 8vo. 1861.
Astronomical Society, Royal—Monthly Notices, No. 9. 1861.
Memoirs, Vol. XXIX. 4to. 1861.
Bavarian Academy, Royal—Sitzungsberichte, 1861; Heft 2, 3. 8vo.
Boston Natural History Society, U.S.—Proceedings, Vol. VII. Nos. 16–28. Vol. VIII. Nos. 1–4. 8vo. 1860–1.
Botfield, Beriah, Esq. M.P. F.R.S. M.R.I. (the Author)—Notices of Libraries. 4to. 1861.
British Architects, Royal Institute of—Papers read in Session 1860–1. 4to. 1861.
British Association for the Advancement of Science—Report of the 30th Meeting, held at Oxford, 1860. 8vo. 1861.
Corporation of the City of London—Supplement to the Library Catalogue. 8vo. 1860.
Chemical Society—Quarterly Journal, Nos. 54, 55. 8vo. 1861.
Denison, E. B. Esq. Q.C. F.R.S.—Appendix to his Treatise on Clocks and Watches. 12mo. 1861.
Dove. H. (the Author)—Meteorological Papers on North Germany. 4to. 1859–61.
Editors—American Journal of Science, by B. Silliman, &c. for July and September 1861 8vo.
Artizan for July to October 1861. 4to.
Athenæum for July to October 1861. 4to.
Chemical News for July to October 1861. 4to.
Engineer for July to October 1861. fol.
Horological Journal, No. 36–39. 8vo. 1861.
Journal of Gas-Lighting for July to October 1861. 4to.

Mechanics' Magazine for July to October 1861. 8vo.
Medical Circular for July to October 1861. 8vo.
Practical Mechanics' Journal for July to October 1861. 4to.
St. James's Medley, No. 27. 1861. 8vo.
Technologist, July to October 1861. 8vo.

Faraday, Professor, D.C.L. F.R.S.—Akademie der Wissenschaften zu Berlin: Abhandlungen, 1859, 1860. 4to. 1861.
Akademie der Wissenschaften, Wien: Math.-Nat. Classe: Denkschriften. Band XIX. 4to. 1861.
Sitzungsberichte, 1861. Nos. 1, 2. 8vo.

Franklin Institute of Pennsylvania—Journal, Vol. XLIII. Nos. 1, 2, 3. 8vo. 1861.

Geographical Society, Royal—Proceedings, Vol. V. Nos. 3, 4. 8vo. 1861.

Geological Society—Journal, No. 67. 8vo. 1861.

Gibb, G. D., M.D. (the Author)—On Canadian Caverns. (K 87) 8vo. 1861.

Glasgow Philosophical Society—Proceedings, Vol. V. No. 1. 8vo. 1860.

Goodfellow, S. J., M.D. M.R.I. (the Author)—Lectures on Diseases of the Kidney and on Dropsy. 12mo. 1861.

Greenwich Observatory, Royal—Greenwich Observations for 1859. 4to. 1861.

Guidi, M. Luigi (the Author)—Lavori dell' Accademia Agraria di Pesaro nell' ultimo quinquennio. (K 87) 8vo. 1861.

Horticultural Society, Royal—Proceedings, Nos. 26–29. 8vo. 1861.

Lankester, Edwin, M.D. F.R.S. M.R.I. (the Author)—Lectures on Food. Second Course. 16to. 1861.

Linnean Society—Proceedings, No. 21. 8vo. 1861.

Locke, John, Esq. (the Author)—Remarkable Discoveries in Central Australia. 8vo. 1861.

Lubbock, Sir John W. Bart. F.R.S. (the Author)—On the Theory of the Moon, &c. Part 10. 8vo. 1860.

Mackie, S. J. Esq. F.G.S. (the Editor)—The Geologist, July to October 1861.

Medical and Chirurgical Society of London, Royal—Proceedings, Vol. III. No. 6. 1861.

Montagu, M. Esq. M.R.I. (the Author)—Fifty more Sonnets. 8vo. 1861.

Müller, Max, M.A. (the Author)—Lectures on the Science of Language: delivered at the Royal Institution of Great Britain in 1861. 8vo. 1861.

Murchison, Sir R. I., D.C.L. F.R.S. M.R.I. (the Author)—Address to the Royal Geographical Society. May 27, 1861. 8vo. 1861.

Newton, Messrs.—London Journal (New Series) for July to October 1861. 8vo.

Petermann, A. Esq. (the Editor)—Mittheilungen auf der Gesammtgebiete der Geographie. 1861. Nos. 6–9. 4to. 1861.

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Royal Society of London—Proceedings, Nos. 44, 45, 46. 8vo. 1861.

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Bulletins, Tome II. et Tome III. Nos 1–5. 4to. 1860–1.

Savory, W. S. Esq. F.R.S. (the Author)—Relation of the Vegetable and Animal to the Inorganic Kingdom. A Lecture at the Royal Institution. 8vo. 1861.

Saxon Society of Sciences, Royal—Abhandlungen. 6 Parts. 4to. 1860–1.
Berichte, 6 Parts. 8vo. 1860–1.

Smith, Edward, M.D. F.R.S. (the Author)—Physiological Papers:—On Respiration; and on Diet, Tea, Alcohol, &c. 8vo. 1857–61.

Smithsonian Institution, Washington, U.S.—Smithsonian Contributions to Knowledge. Vol. XII. 4to. 1860.
Smithsonian Reports, 1859. 8vo. 1860.

Statistical Society—Journal, Vol. XXIV. Part 3. 8vo. 1860.

Taylor, Rev. W., F.R.S. M.R.I. (the Translator)—J. G. Knie, Guide to the Education and Management of Blind Children. 16to. 1861.

Tyndall, Professor (the Author)—On the Absorption and Radiation of Heat by Gases. (Phil. Trans. 1861.) 4to. 1861.
On Experimental Physics. A Lecture. 8vo. 1861.
United Service Institution, Royal—Journal, Nos. 16-18. 8vo. 1861.
United States Patent Office.—Report for 1859. Mechanics. 2 vols. 8vo. 1860.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, März und April, Mai und Juni. 4to. 1861.
Weale, Mr. John (the Author)—Statistical Notices of Works on the Fine and the Constructive Arts, published by him. 1822-61. 8vo. 1861.
Webster, John, M.D. F.R.S. (the Author)—On the Leper Hospital at Granada. 8vo. 1860.
Report on Murray's Lunatic Asylum, Perth. 8vo. 1861.
Report on Sussex County Lunatic Asylum. 8vo. 1861.
Westminster, the Dean of (the Editor)—Journal of a Visit to Germany in 1798-1800. (Not published.) 16to. 1861.

GENERAL MONTHLY MEETING,

Monday, December 2, 1861.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

James Bass, Esq.
Rev. George Bowes Macilwain, B.A.
David Ricardo, Esq.

were *elected* Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same: viz.

FROM
Actuaries, Institute of—Assurance Magazine, No. 45. 8vo. 1861.
Bidder, George P. Esq. Jun. (the Author)—On National Defences. (From Vol. XX. of the Proceedings of Civil Engineers.) 8vo. 1861.
De la Rue, Warren, Esq. F.R.S. M.R.I. (the Author)—Two Engravings of the Planet Mars, as seen by a Newtonian Equatorial, April 20, 1856.
Editors—American Journal of Science, by B. Silliman, &c. for November 1861. 8vo.
Artizan for November 1861. 4to.
Athenæum for November 1861. 4to.
Chemical News for November 1861. 4to.
Engineer for November 1861. fol.
Horological Journal, No. 40. 8vo. 1861.
Journal of Gas-Lighting for November 1861. 4to.
Mechanics' Magazine for November 1861. 8vo.
Medical Circular for November 1861. 8vo.
Practical Mechanics' Journal for November 1861. 4to.
St. James's Medley, No. 28. 1861. 8vo.
Technologist for November 1861. 8vo.

Faraday, Professor, D.C.L. F.R.S.—Academie der Wissenschaften. Wien: Math.-Nat. Classe. Sitzungsberichte, 1860. No. 29. 1861. Abtheilung I. No. 3, 4, 5. Abtheilung II. No. 3, 5, 6, 7. 8vo.
Almanach für 1861. 12to.
Memorie della Reale Accademie delle Scienze di Torino. II^e^ Serie. Tome XIX. 4to. 1861.
Franklin Institute of Pennsylvania.—Journal, Vol. XLIII. No. 4. 8vo. 1861.
Geographical Society, Royal—Proceedings, Vol. V. No. 5. 8vo. 1861.
Geological Society—Journal, No. 68. 8vo. 1861.
Hamel, Dr. J. (the Author)—Bishop Watson and the Electric Telegraph. (O 10) 16to. 1861.
Hamilton, Sir Charles J., Bart. C.B. M.R.I.—Lieut.-Col. W. Congreve on Mounting Naval Ordnance. 4to. 1811.
Horticultural Society, Royal—Proceedings, No. 30. 8vo. 1861.
Jervis, Swynfen, Esq. M.R.I. (the Author)—Proposed Emendations of the Text of Shakespeare's Plays. (K 87) 8vo. 1861.
Kupffer, A. T. (the Director)—Annales de l'Observatoire Physique Central de Russie: 1858. 2 vols. 4to. 1861.
Comptes Rendus, 1859, 1860. 4to.
Mackie, S. J. Esq. F.G.S. (the Editor)—The Geologist for November 1861.
Macrory, Edmund, Esq. M.A. M.R.I. (the Author)—A Few Notes on the Temple Organ. 2nd Edition. 12mo. 1861.
Mechanical Engineers' Institution, Birmingham—Proceedings, 1849–61. 8vo.
Newton, Messrs.—London Journal (New Series) for November 1861. 8vo.
North of England Institute of Mining Engineers—Transactions, Vol. IX. 8vo. 1861.
Paris, H.R.H. the Count of, M.R.I. (the Author)—Damas et le Liban. Extraits du Journal d'un Voyage en Syrie au printemps de 1860. 8vo. 1861.
Petermann, A. Esq. (the Editor)—Mittheilungen auf der Gesammtgebiete der Geographie. 1861. No. 10. 4to. 1861.
Philadelphia, Academy of Natural Sciences—Journal, Vols. VI. and VII. 8vo. 1828–37.
Proceedings for 1857–61. 5 vols. 8vo.
Dr. W. Ruschenberger—Notice of the Origin, Progress, and Present Condition of the Academy. 8vo. 1860.
Act of Incorporation of the Academy. 8vo. 1857.
Catalogue of Human Crania in the Society's Museum. By J. A. Meigs. 8vo. 1857.
Photographic Society—Journal, No. 115. 8vo. 1860.
Royal Society of London—Philosophical Transactions for 1860, Part II.; for 1861, Part I. 4to. 1861.
Royal Society of Tasmania—Results of Twenty Years' Meteorological Observations at Hobarton, 1841. 4to. 1861.
Upsal Royal Society of Sciences—Nova Acta. 3^e^ Serie. Vol. III. 4to. 1861.
Arsskrift. 8vo. 1861.

1862.

SPECIAL GENERAL MEETING,

Monday, January 13, 1862.

The Rev. JOHN BARLOW, M.A. F.R.S., Vice-President, in the Chair.

THE following Address to Her Majesty the QUEEN, in reference to the decease of H.R.H. the PRINCE CONSORT, Vice-Patron of the Royal Institution, on December 14th, 1861, was read and unanimously adopted :—

"TO THE QUEEN'S MOST EXCELLENT MAJESTY :

"May it please Your Majesty,

"WE, the Members of the Royal Institution of Great Britain, respectfully desire to express to Your Majesty our grief for the loss which has fallen upon the Kingdom, upon our Institution, and, with exceeding weight, upon Your Majesty personally.

"May it please GOD, who grants consolation in HIS own due time, to give it to Your Majesty, even while Your thoughts are directed towards Him that is gone, and may the recollection of our Prince's doings whilst in life, have an abiding influence for good upon the many millions who have heard of and rejoiced in His name."

WEEKLY EVENING MEETING,

Friday, January 17, 1862.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. Vice-President, in the Chair.

JOHN TYNDALL, F.R.S. &c. &c.

PROFESSOR OF NATURAL PHILOSOPHY AT THE ROYAL INSTITUTION.

On the Absorption and Radiation of Heat by Gaseous Matter.

RESUMING with a new apparatus his experiments on the influence of Chemical Combination on the Absorption and Radiation of Heat by Gases, the speaker, in the investigation of which the evening's discourse would be a *résumé*, first examines the deportment of chlorine as compared with hydrochloric acid, and of bromine as compared with hydrobromic acid, and finds that the act of combination which in each of these two cases notably diminishes the density of the gas and renders the coloured gas perfectly transparent to light, renders it more opaque for obscure heat. He also draws attention to the fact that sulphur, which is partially opaque to light, is transparent to 54 per cent. of the rays issuing from a source of 100 C, while its compound, heavy spar, which is sensibly transparent to light, is quite opaque to the rays from a source of 100 C. He demonstrates, in confirmation of Melloni, the transparency of lampblack in thin layers; but shows how irreconcilable its deportment to radiant heat is with the idea generally prevalent at the present day, that lampblack absorbs heat of all kinds with the same intensity.

All his experiments with gases have been repeated with a different source of heat, and he finds the result still more pronounced than formerly, that the compound gases far transcend the elementary ones in absorptive power. Taking air as unity, ammonia, at 30 inches tension, is 1195, this latter figure representing *all the heat* that issued from the source. A layer of ammonia, 3 feet long, is *perfectly black* to heat emanating from an obscure source. The coloured gases, chlorine and bromine, though much superior in absorptive power to the transparent elementary gases, are exceeded in this respect by every compound gas that has been hitherto examined. When, instead of tensions of 30 inches, we compare tensions of 1 inch, the differences between the gases come out still more strikingly. At this tension, for example, the absorption of sulphurous acid is eight thousand times that of air.

The speaker also referred to a new and extensive series of experiments on the Absorption of Radiant Heat by Vapours. The least energetic, as before, he finds to be bisulphide of carbon; the most energetic, boracic ether. He shows that the absorption of the latter vapour (which is quite transparent) at 0·1 of an inch of tension is 600 times the absorption of the densely coloured vapour of bromine, while in all probability it is 186,000 times that of air.

The speaker was led by a series of perplexing experiments, which are fully described in a Memoir recently presented to the Royal Society, to the solution of the following remarkable and at first sight utterly paradoxical problem—"*To determine the absorption and radiation of a gas or vapour without any source of heat external to the gaseous body itself.*"

When air enters a vacuum it is heated by the stoppage of its motion; when a vessel containing air is exhausted by an air-pump, chilling is produced by the application of a portion of the heat of the air to generate *vis viva*. Let us call the heating in the first case dynamic heating, and the chilling in the second case dynamic chilling. Let us further call the radiation of a gas which has been heated dynamically, dynamic radiation, and the absorption of a gas which has been chilled dynamically, dynamic absorption. Placing a thermo-electric pile at the end of his experimental tube, the latter being exhausted, the gas to be examined is permitted to enter the tube; the gas is heated, and if it possess any sensible radiative power, the pile will receive its radiation, and the galvanometer connected with the pile will declare it.

Proceeding in this way with gases, Professor Tyndall found that the radiation thus manifested, and which was sometimes so intense as to urge the needle of the galvanometer through an arc of more than sixty degrees, followed the exact order of the absorptions which he had already determined. After the heat of the radiating column of gas had wasted itself, the air-pump was worked at a certain rate, the rarefied gas within the tube became chilled, and the face of the pile turned towards the chilled gas became correspondingly lowered in temperature. The dynamic absorptions of various gases were thus determined, and they were found to go strictly hand in hand with the dynamic radiation.

In the case of vapours the following method was pursued. A quantity of the vapour sufficient to depress the mercury column 0·5 of an inch was admitted into the tube, and this was heated dynamically by allowing dry air to enter till the tube was filled. The radiation of the vapours thus determined followed exactly the same order as the absorption which had already been measured. The dynamic absorption of the vapour was obtained by pumping out in the manner just described, and it was found to follow the same order as the dynamic radiation. In these experiments the air bore the same relationship to the vapour that a polished silver surface does to a coat of varnish laid over it. Neither the silver nor the air, both of which are elements or mixtures of elements, possesses the power of agitating in any marked degree the luminiferous ether. But the motion of the silver being communicated to the varnish, and the motion of the air being communicated to the vapour, molecules are agitated which have the power of disturbing, in a very considerable degree, the ether in which they swing.

The speaker finds by strict experiments that the dynamic radiation of an amount of boracic ether vapour, possessing a tension of only $\frac{1}{10125000000}$th of an atmosphere is easily measurable. He also shows and explains the fact that with a tube 33 inches long, the dynamic radiation of acetic ether considerably exceeds that of olefiant gas; while in a tube 3 inches long, the dynamic radiation of olefiant gas considerably exceeds that of the ether. Aqueous vapour has been subjected to a special examination, and Professor Tyndall finds it a common fact for the aqueous vapour contained in the atmosphere to exercise 60 times the absorption of the air itself. The further he has pursued his attempts to obtain perfectly pure and dry air, the more has the air approached the character of a vacuum. He further points to the possibility of determining the temperature of space by direct experiment.

Scents of various kinds have been examined. Dry air was passed over bibulous paper moistened by the essential oils, and carried into the experimental tube. Small as the amount of matter here entering the tube is known to be, it was found that the absorption of radiant heat by those odours varies from 30 times to 372 times that of the air which formed the vehicle. The speaker remarked that the absorption of terrestrial rays by the odour of a flower-bed may exceed in amount that of the entire oxygen and nitrogen of the atmosphere above the bed.

Ozone has also been subjected to examination. The substance was obtained by the electrolysis of water, and from decomposing cells containing electrodes of various sizes. Calling the action of the ordinary oxygen, which entered the experimental tube with the ozone unity, the absorption of the ozone itself was in six different experiments,—21, 36, 47, 65, 85, 136. The augmenting action of the ozone accompanied *the diminution of the size of the electrodes* used in the decomposing cells. Professor Tyndall points out the perfect correspondence of these last results with those of M. Meidinger by a totally different method of experiment.

[J. T.]

WEEKLY EVENING MEETING,

Friday, January 24, 1862.

The Rev. John Barlow, M.A. F.R.S. Vice-President, in the Chair.

George Rolleston, M.D.
LINACRE PROFESSOR OF PHYSIOLOGY, OXFORD.

On the Affinities and Differences between the Brain of Man and the Brains of certain Animals.

The speaker having commenced by giving a short explanation of his diagrams of human and other brains, proceeded to enumerate the several sets of opinions which men might bring with them to an investigation of his subject. It was possible to combine either view of the origin of species with either of the two creeds of the idealist or of the materialist; and to the four sets of opinions thus made up, a fifth—that of Positivism—must be added. It was not asserted that these conflicting theories could all be true simultaneously; but the facts to be detailed were elastic enough to bear compression within any one of those formulæ.

Beginning with the internal anatomy of the brains which he had to compare and contrast, the speaker said that the question as between man and the ape might be stated thus :—Has the ape such a biradiate, two-horned ventricular cavity within its brain as has the dog, or has it not rather such a one as has man himself, triradiate and three-horned? By the aid of drawings of dissected brains of the dog, of an old-world and of a new-world monkey, and of man, it was seen that the interior of the simious brain was even more pre-eminently a three-horned cavity than was that of the human brain; and that the new-world monkey contrasted with man to even greater advantage in this, and the disputed point of the closely-allied hippocampus minor, than did the much more anthropomorphous old-world ape. Tiedemann's retractation of his error as to the *processus digitati* of the greater hippocampus was alluded to; the speaker insisting that though such discoveries and rectifications might seem of weight and consequence to persons imbued, as was Tiedemann, with materialistic views, they possessed no anthropological interest whatever for the idealist.

Certain anatomical plates of Eustachius', published some 150 years ago, were shown to give representations of the interior of the human brain which coincided in all points with figures of the interior of the brain of the orang, which had been published within the current month by two Dutch anatomists, in the English 'Natural History Review.'

Passing, then, from the anatomy of the internal to that of the external surface of the brain, the speaker said that the points of agreement and of difference upon which he should have to dwell could be arranged under two heads—either they were such as the eye could judge of even though its owner were not an anatomist *ex professo*, depending as they did upon general outline and configuration; or they were such as a deeply-going analysis of the convolutions alone could elicit.

Under the first head were enumerated the more elegantly ovoidal and tapering shape, the more accurate semicircularity of the superior, and the irregularity of the inferior boundary line, as signs of defect and diminishment in the ape's brain; but the outcropping of the cerebellum from beneath the overlying cerebral hemispheres, which had been so much insisted upon as a distinctive mark of the inferiority of the simious encephalon, was shown to depend largely upon the changes of relative position which the several masses of nervous matter, comprised under the one term "encephalon," undergo when they are removed from their supporting brain-case.

The absolute necessity of comparing the configuration and proportions of brains preserved in spirits with the configuration and proportions of plaster-casts of the cavities they occupied during life, was dwelt upon with special reference to Mr. Marshall's observations upon this point in the 'Natural History Review' for July, 1861. It was in the gorilla alone of the Simiadæ that M. Gratiolet ('Comptes Rendus,' 1860, p. 803) had found the posterior cerebral lobes doing otherwise than "recouvrant complètement le cervelet;" and it was this peculiarity, together with other characteristics of its encephalon and other structures, which had induced him to speak of it as "the last, the most degraded of all the anthropomorphous apes;" and to class it with the baboons, whilst he ranked the chimpanzee with the macaques, and the orang with the gibbons.

The last point of general configuration and measurement in which the simious was contrasted with the human brain was that of their several altitudes; and it was shown that whilst men differed but little *inter se* as to the height of their brains, it was precisely in this very dimension that they differed, perhaps more widely than in any other, from all apes whatsoever.

After expressing his sense of the obligations which anatomy owed to M. Gratiolet's analysis of the cerebral convolutions, the speaker proceeded to give in detail the points of resemblance and of contrast which that analysis had enabled us to detect as subsisting between human or simious brains. The chief points in which, under this head, the human was seen to contrast to advantage with the ape's brain were two. First: The absence in man of "the external perpendicular fissure," or, in other words, the filling up in him of what is more or less of a chasm in the ape, by a large quadrangular mass of convolutions. Second: The much greater size and complexity of the frontal lobes. But it was shown that these differences affected what have been called "secondary" and "tertiary" convolutions, and indeed the latter of these chiefly, whilst the "primary" convolutions, the great typical lines and ridges, were the same in both classes of brains. The apparatus for the mechanical, (and possibly also physiological,) unification of the hemispheres, which is known as the *corpus callosum*, was stated to have in man just double the sectional area which it had in the apes; whilst the very lowest weight which an adult and healthy human encephalon was recorded to have fallen to, was yet double, and more than double, of the very highest which had ever been attained in the weighing of an ape's brain.

The results of the anatomical investigation were summed up thus. "This doubly and more than doubly greater weight, the doubly greater corpus callosum, that subquadrate lobule, lettered α and β in the diagram, those complexly convoluted frontal lobes, 1, 2, and 3, are, I believe, the four great points in which the human brain asserts its superiority over that of the ape."

The metaphysical or anthropological bearings of the investigation might be summed up thus. How similar soever the simious might be shown to be to the human brain, the argument which Bossuet drew thence for the essential difference between mind and matter, would but be rendered the stronger. If organs are common to man and to brutes, one is necessarily forced to the conclusion that intelligence is not attached to organs; and the cogency of this argument, M. St. Hilaire remarks, increases as the number of organs, common to the two subjects of comparison, becomes more numerous and their resemblance more striking.

The anatomist, however, though not obliged to concede, could yet afford to argue upon, the assumption that mind and matter always vary concomitantly. For, granting this, it by no means followed, that, of the two terms of the comparison, mind was the second, body the first. The effects of prolonged mental states of different natures, the operation of education in marring or in elevating the physical features, the instinctive value which we all give to physiognomy, whether before us in actuality, or reproduced and preserved for us by art, as affording indications of character, were glanced at as lines of evidence to show that the mind might modify, whilst the body was adapted; that the immaterial might fashion, whilst the corporal was conformed into accordance with it. "All alike, when coldly and dispassionately viewed as concomitantly varying phenomena, lead us to hold that our higher and diviner life is not a mere result of the abundance of our convolutions. How harmony may have come to exist between them our faculties are incompetent either to decide or to discover; but this shortcoming of man's intelligence affects neither his duties nor his hopes, neither his fears nor his aspirations."

[G. R.]

WEEKLY EVENING MEETING,

Friday, January 31, 1862.

William Pole, M.A. F.R.S. Treasurer and Vice-President, in the Chair.

William Hopkins, Esq. M.A. F.R.S. &c.

On the Motion of Glaciers.

In the introductory part of this discourse Mr. Hopkins insisted on the necessity of a more exact definition of terms, and more accurate modes of mechanical reasoning than those which had too often characterized the discussion of glacial phenomena. Nor had careful experimental investigations respecting the properties of ice been adequately appreciated in laying the foundations of theories of glacial motion, till the experiments of Mr. Faraday and Dr. Tyndall reminded us how defective and erroneous might be our conceptions on this subject without the guidance of such careful research. These experiments had revealed to us a property of ice, that of *regelation*, of which we were previously entirely unconscious, though it is now probably recognized by nearly all glacialists as the fundamental property by means of which a glacier preserves both the continuity of its motion and that of its mass in a degree with which the external conditions under which it is placed might, at first sight, appear to be totally inconsistent. Attempts had been made to confound the explanation of glacial motion which rests on the property of regelation, with that afforded by the property of the assumed viscosity of ice according to the viscous theory. The speaker first endeavoured to remove this vague and erroneous impression by a more careful definition of the term *viscous*, as expressing a determinate property of the aggregates of material particles which constitute *bodies*. He remarked that any body might, in a greater or less degree, be extended by tension, compressed by pressure, or angularly distorted by forces acting in couples. The same might be asserted of every indefinitely small element of the body, the volume of which would be increased, and its density diminished by tension, and the opposite effects would be produced by pressure, while the forces acting as couples would distort or twist the element without changing its volume or density. It might be said in general terms that, if the force required to produce any of these changes of volume or form in the elements composing the body were comparatively large, the body was *solid;* if the required force were much smaller, the body might be termed *plastic*, or if smaller still, *viscous* or *semifluid*. *Rigidity* would be the limit of solidity, and *perfect fluidity*, that of imperfect fluidity, two limits which of course were never attained in nature, since in the first case an infinite force would be required to produce any relative displacements of the constituent particles; and in the second, such displacement might be produced by a force which should be infinitely small. Such definitions, however, would be far too vague and indeterminate for our immediate purpose. If we would designate by any of these terms determinate properties of a body, we must endeavour to give to the definitions of them a degree of determinateness corresponding to that required in the properties to be expressed. When a body, or each of its component elementary portions, was affected by pressures or tensions as above described, such pressures or tensions, whether acting externally or internally, were called *forces of displacement*. If such forces were of sufficient magnitude, they would of course dislocate the mass on which they acted; but in these definitions they were not supposed sufficient to produce any kind of dislocation, but only a distortion from the forms which the body or its component elements would have, if acted on by no external forces whatever. When thus distorted, every element of the mass would exert a certain force to regain its undistorted form and position. This force was usually termed the *force of restitution;* in its greatest limit it would be equal to, but was in fact always less than the force of displacement. The ratio (less than unity) which it bore to the former force, measured what is termed the *elasticity* of the mass. But it was to be observed that this property of a body might not only be different at different points of it, if the body were not homogeneous, but that it might be different at the same point for different modes of displacement. Thus if the body, or any of its elements, were compressed, the elasticity developed might not be the same as if it were extended, or as if it were twisted or contorted without a change of volume. Thus hard bodies, like wood, metal, stone, &c., would usually exhibit nearly the same amount of elasticity whether they were compressed or extended, provided the compression or extension should not exceed certain small limits. On the contrary, semifluid masses generally exert a large force of restitution when under compression, but a very small one when extended, *i. e.* in the former case the elasticity developed is very large, in the latter it is very small. And again, if a semifluid mass, such as soft paste for instance, or any portion of it, were angularly distorted without alteration of volume, it would exhibit but a small amount of elasticity, and little or no tendency to regain its original form and position. Moreover, in the class of substances first mentioned, such as wood, metal, &c., the *cohesive power* was very great, while it was manifestly small in the others.

With the preceding preliminary observations, it was easy to define with the required degree of exactness the terms *solid* and *plastic*, or *viscous*. When a substance admitted of only a small extension without being dislocated, and of only a small compression without being crushed, and having its structure destroyed; when its cohesion as opposing direct tension or torsion was comparatively great, as well as its power of resistance to direct pressure ; and also, when the elasticity developed by each of the three displacements or changes of form above described—then might such a substance be emphatically called *solid*. To define the term plastic, it was most simple to conceive the form of the mass, or of its elements to be changed by angular distortion alone without any change of volume. In such case, if the cohesive power resisting this change should be small, and the elasticity developed by it should become sensibly = zero ; and if, moreover, the substance should be able to bear any number of repetitions of this change of form, and therefore an indefinite elongation without rupture and dislocation—then might it be emphatically termed *plastic*. In all such substances in nature, the force of displacement for the change of form here described would always be small. Viscosity might be regarded, so far as we were directly concerned with these properties, as a higher degree of plasticity. A viscous body would differ from a plastic one only so far as the required force of displacement should be smaller. Tar, soft paste, and fluid lava afforded obvious examples of plastic or viscous substances. When the relative positions of their component particles were disturbed they exhibited no sensible elasticity, and made no effort to regain the position from which they had been disturbed. They admitted of any degree of elongation without change of volume or discontinuity in their mass.

The most important character of plasticity was the absence of all elasticity when the particles of a body were displaced without change of volume, and the consequent capability of an indefinite extension of the mass without dislocation. So far this property was as definite as that of fluidity. In nature the cohesive power of plastic bodies was generally comparatively small, but this was a less definite and less important character than the other. With respect to ice, the question before us was this : Is glacial ice a *solid* or a *plastic* substance, according to the definitions which had just been given of those terms? Whether others might agree to those definitions or not was immaterial, for they were here used to denote the exact properties which they had been defined to express. Taking them in this sense, it would seem to be impossible to say that ice was not distinctly *solid.* It breaks into an indefinite number of fragments when crushed, and will bear only a small elongation without fracture. But these were the properties of a solid, and those which are directly antagonistic to the properties which could alone justify our designating any proposed substance as plastic, without a violation of all the strictness of scientific language.

It would seem not improbable, Mr. Hopkins proceeded to state, that the advocates of the Viscous Theory had reserved to themselves, as it were, a more comprehensive, and consequently a more vague and indeterminate meaning for the terms viscous and plastic. The defect of that theory was, in fact, that it gave no definition of the term by which it designated the property of ice on which it was founded. When such definition was demanded, it was said that glacial ice must be viscous, because a glacier was capable of adapting itself to the inequalities of the valley containing it, *as if* it were viscous. This was equivalent to the assumption that ice possessed no other property by virtue of which a glacier might change its form, without losing its continuity, and thus adapt itself to external conditions *as if* it were viscous. Dr. Tyndall's experiments showed at once that ice did possess such a property—that of *regelation*—by virtue of which a glacier, after being crushed and broken by any unusual compression to which it might be exposed, perfectly regained by pressure and contact its continuity as a vitreous or crystalline, hard, and brittle mass. It was this phenomenon, apparently so inexplicable, that formed till recently the great stumbling-block in our glacial theories. It was to meet this difficulty, that the author of the Viscous Theory introduced the fundamental idea of that theory, the viscosity of ice. No attempt was made to establish this property by experimental investigation, in any sense in which it might be applied. It was only founded, as had been already stated, on an induction, the fallacy of which was immediately proved by Dr. Tyndall's beautiful and decisive experiment. By that experiment the theory of glacial motion was made to rest on a property of ice, established by clear and determinate experimental evidence, instead of resting on a property which was never proved, and which, if it meant anything definite, appeared to be in opposition to the direct evidence of our senses.

Still it might be said that there must be substances possessing properties similar to those which had been designated by the terms solid and plastic, but intermediate to them. This was doubtless the case;

but Mr. Hopkins contended that there was not the slightest evidence that ice was characterized by any such ambiguous property. It might bear somewhat more or less of extension or compression without dislocation; experiment had not yet determined that point with exactness. But it might at all events be deemed certain, that ice possesses no such capability of elongation as really plastic substances possessed, nor such as would be sufficient to account in the least degree for that elongation of the marginal parts of a glacier which must necessarily result from the more rapid motion of its central portion.

Again, to say that the capability of a glacier to move, as had just been stated, was due to the breaking, bruising, and tearing of the mass, was not to attribute that capability to viscosity or any other distinctive property of ice. This *modus operandi* belonged to solid and not to viscous masses; and was, in fact, a part of the process required by the regelation theory, according to which the ice of a glacier must be dislocated by tearing or crushing as above described, immediately regaining the continuity of its mass and of its crystalline structure by regelation.* In this process there was not the slightest reference to any property which could, in any received meaning of the term, be called viscosity.

But, the speaker proceeded to observe, although regelation was unquestionably the property of ice on which the molecular mobility of a glacial mass conjoined with the preservation of its continuity depended, it was not the physical cause of the general motion of the glaciers. That cause was gravity, tending, first, to make the whole mass *slide* bodily down the containing valley; and, secondly, to make the upper portions of the mass move faster than the lower; in consequence of the retarding action on its base, and the central portions to move faster than the lateral ones, in consequence of the retarding action of the sides of the valley. De Saussure had been the first to suggest the sliding of glaciers along the bottoms of their valleys; but when a more careful attention was subsequently given to glacial phenomena by M. Agassiz, Professor Forbes, and others, grave and apparently insurmountable objections were urged against this view of glacier motion. It was said to be most improbable, if not impossible, that a glacier should descend in this manner down a surface inclined to the horizon at an angle not unfrequently less than 5°, at the same time overcoming numerous obstacles to its motion; and, moreover, that if the mass did so move at all, it must necessarily be with an *accelerated motion*, like that with which a body ordinarily slides down an inclined plane, and then be finally discharged from the mouth of its valley like an avalanche. But whatever apparent force might belong to these objections, they arose from an erroneous conception of the real nature of the mechanical problem which the motion in question presented to us. In the common case of a body sliding down an inclined plane, it was found

* Dr. Tyndall had the kindness to elucidate this process, by repeating his experiment on regelation before the audience.

by experiment that the retarding force of friction was independent of the body's velocity, which consequently went on constantly increasing; but this result involved the condition that the surfaces of the body and of the plane which were in contact, should not be affected by their action against each other. Now this condition was not fulfilled in the case of the glacier. The temperature of its whole mass beyond very small depths was found by observation and theory to remain constantly at the freezing point, and in that state the cohesive power of ice was found to be considerably more feeble than at lower temperatures. In fact, it must necessarily, at the lower surface of the glacier, be constantly in a state of disintegration from its thawing. Hence, as each successive thin layer at the bottom of the mass is converted into water, the mass of the glacier is *let down*, as it were, and advances through a corresponding small space along its inclined bed. At the same time this advance must depend on the rate at which the ice melts, and as that rate is very *slow* and *uniform*, the motion of the glacier must be slow and uniform likewise.

This important conclusion was not left to mere abstract mechanical reasoning. Mr. Hopkins had verified it by a very simple experiment. He placed a lump of ice on a rough paving slab, which could easily be placed at different inclinations to the horizon. When the temperature of the surrounding atmosphere and the slab were below that of freezing, the lump of ice remained stationary on the slab at all inclinations less than about 20°; at greater inclinations it moved down the slab, as in ordinary cases, with an accelerated motion. But when the temperature of the slab and of the air was above the freezing temperature, the ice descended with a very slow and uniform motion, even at inclinations as small as half a degree.* The greater the inclination of the plane and the greater the weight of the sliding mass, the greater was the uniform velocity with which it moved. Mr. Hopkins regarded this experiment as supplying a complete answer to the objections above stated against the sliding motion of glaciers.

That glaciers do slide over their beds was indeed established on indubitable evidence. It was easily seen, on examining almost any glacial valley, that its glacier had been formerly continued beyond the position of its present extremity, far below which, along the sides and bottoms of the valley, were observed the rounded and striated surfaces frequently so conspicuous at the in mediate termination of the glacier, where they were manifestly due to the sliding of its general mass. All valleys in which ancient glaciers were believed to have existed likewise bore the same testimony. But the most direct evidence of this fact is to be found in the observations of Professor Forbes, and those of Dr. Tyndall, from which it appeared that, of the whole observed motion of the upper surfaces of the glaciers at the points where the observations were made, about one-half was due to the sliding of the general mass, and the other

* The experiment was exhibited during the discourse.

to its molecular mobility as above explained. No glacialist, Mr. Hopkins conceived, could at present doubt the fact of the sliding motion.

And yet, though nearly the same evidence had existed of the fact in question, and the above explanation of it had been given upwards of fifteen years ago, both the fact and the explanation had, till a recent period, been almost entirely ignored.

But the sliding of a glacier was not only important as an integral part of its motion, but it also increased the internal pressures and tensions, which were thus enabled to overcome the cohesive power of the mass, to dislocate it, and leave it free to move onwards when its motion might otherwise have been arrested. The action of the bed of the valley on the bottom of the glacier had this apparently anomalous property—that while it prevented the mass from moving with more than a very slow uniform velocity, it exerted a force to hold it absolutely at rest little greater than if the plane had been perfectly smooth, and the glacier had moved with perfect freedom. It was so far like the force of resistance afforded by water to the descent of a heavy body within it. Such resistance, while it had no power to hold the body at rest, soon increased with the motion sufficiently to counteract almost entirely the accelerating effect of gravity, and to reduce the body very approximately to a uniform velocity which it could never exceed. And such was the analogous case of the glacier. To hold the heavy body in the water at rest would require a force equal to the excess of the weight of the body over an equal volume of water; and so to hold any portion of a glacier at rest would require a force nearly equal to the resolved part of its weight along the inclined plane of its valley: whereas, if its motion were impeded by a large amount of friction on its bed, it would require a far less force to hold it at rest, and it would be capable of exerting only a far less force on any obstacle impeding its onward progress. Thus, suppose the motion of the glacier to be more or less impeded by the narrowing of its valley, an enormous pressure would be produced on the impeded portion of the mass, by the tendency of the less impeded portion behind to press forward, and the more freely this latter portion should slide over its bed, the greater would manifestly be the pressure which would be thrown on the impeded portion before it. We might thus see how enormously the sliding of the glacier might increase the fracturing power of the internal pressures and tensions, and help to preserve the continuity of the process of alternate dislocation and regelation on which the continuity of the mass and structure of the glacier and that of its motion depend.

According to the views which Mr. Hopkins had been endeavouring to develope, if we conceived a glacier to be placed in its valley at rest and in a state of no constraint, it would begin to move, partly by sliding on its bed and partly by virtue of that small relative displacement of the molecules of the mass of which all bodies are susceptible in some degree, without dislocation. The whole glacier would thus become a continuous mass in a state of constraint, and therefore subject to internal pressures and tensions. These internal strains might become, from the sliding of the glacier, of enormous intensity. When they became large enough to overcome the cohesion of the particles to each other, the mass, supposed always to be *solid* in the sense which had been defined, would be broken into open fissures, or crushed into numerous solid fragments. Its state of internal constraint would be thus relieved, its onward motion would be continued, and the continuity of its mass and of its crystalline structure immediately restored by regelation. The continuous motion of the glacier would result from the repetition of this process, and the approximate uniformity of the motion of the whole mass, notwithstanding the opposition of local obstacles, would result from the fact that those obstacles, considered with reference to the whole glacier, would at all different times be acting in the same manner.

The two important points in this theory were the sliding of the glacier, and its dislocation and regelation. The fact of the sliding had been established by observation, and the nature of the motion resulting from it had been explained by means of a simple and accurate experiment. The property of regelation in ice, regarded as a brittle crystalline substance, also rested on the clearest experimental evidence. What more could be required to complete our theory of the general and ordinary phenomena of the *motion of glaciers?* The experimental results on which this view of the subject was founded were clear and certain; while the Viscous Theory was founded on an assumed property of ice, which, so far from being proved, was never even clearly defined. That theory the speaker doubted not had effectively passed away, however some of its advocates might still be disposed to cling to it. At the same time he should be sorry not to do justice to the claims which its author had established to the best thanks of all those who interest themselves in glacial phenomena. By the number of Alpine objects and phenomena which he had observed, and the force and vigour with which he had delineated them, he had been the first to excite in this country an interest in the subject of glaciers, which was still maintained both in a popular and scientific point of view. Mr. Hopkins had always been a declared opponent, and he hoped a candid one, of the Viscous Theory. He had never been satisfied with the principle on which it rested, and had always contended that much of its mechanical reasoning, and the conclusions drawn therefrom, were essentially erroneous. At the same time he considered Principal Forbes, though not absolutely the first to observe certain fundamental characters in the motion of glaciers, was undoubtedly the first glacialist whose mind was fully imbued with a sense of the importance of taking account of the molecular mobility within the mass of a glacier in any theory of its motion; and though the theory which he was thus led to put forth was, in Mr. Hopkins's opinion, erroneous, it had had the effect of leading the thoughts of other glacialists in the same direction, and in this manner could hardly have failed to have its influence in advancing the science, and in conducting to the discovery of the real physical cause which accounts for the preservation of continuity in the mass of a glacier, notwithstanding the molecular mobility of its particles.

Time would not allow Mr. Hopkins to speak of other phenomena which appeared to be essentially connected with the motion of glaciers, but of which the theory was far more imperfect than that of the general motion of their masses.

[W. H.]

GENERAL MONTHLY MEETING,

Monday, February 3, 1862.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Robert Russell Carew, Esq.
William Whitaker Collins, Esq.
John Parnell, Esq. M.A. and
Major-General Edward Sabine, R.A. D.C.L. President of the Royal Society.

were *elected* Members of the Royal Institution.

The following letter to the President was read:—

"Whitehall, 19*th Jan.*, 1862.

"My Lord Duke,

"I have the honour to acknowledge the receipt of the loyal and dutiful Address of the Members of the Royal Institution of Great Britain, on the occasion of the death of His Royal Highness the Prince Consort; and to inform your Grace that I will take an early opportunity of laying the Address before Her Majesty.

"I have the honour to be,
"My Lord Duke,
"Your Grace's obedient Servant,
"G. Grey.

"His Grace the Duke of Northumberland,
&c. &c. &c."

The special Thanks of the Members were returned to Warren De la Rue, Esq. Ph.D. F.R.S. M.R.I. for his valuable present of an Electric Lamp, by Duboscq, with its accompanying Microscope.

The PRESENTS received since the last Meeting were laid on the able, and the thanks of the Members returned for the same: viz.

FROM

Secretary of State for India—Report on Survey of India. (P 6.) fol. 1858-9.
Anderdon, J. L. Esq. (*the Author*)—The Messiah. 8vo. 1861.
Anonymous—J. Dumesnil-Marigny; The Free Trade Question. Translated by J. Akerson. (L 13) 8vo. 1861.
Asiatic Society, Royal—Journal, Vol. XIX. Part 2. 8vo. 1861.
Astronomical Society, Royal—Monthly Notices, Nov. and Dec. 1861. 8vo.
Bavarian Academy, Royal—Sitzungsberichte, 1861; Heft 4, 5. 8vo.
  Abhandlungen, Band IX. 1ste Abtheilung. 4to. 1861.
  Annalen der Kön. Sternwarte bei München. Band XII. 8vo. 1860.
Bombay Medical Department—Deaths in Bombay in 1860. 8vo. 1861.
Browning, W. Ernst, Esq. M.R.I. (*the Author*)—The Practice and Procedure of the Court for Divorce and Matrimonial Causes. 16to. 1862.
Chemical Society—Quarterly Journal, No. 56, 8vo. 1861.
Civil Engineers' Institution—Proceedings, Dec. 1861 and Jan. 1862. 8vo.
Dublin Geological Society—Journal, Vol. IX. Part 1. 8vo. 1861.
Editors—American Journal of Science, by B. Silliman, &c. for Nov. 1861. 8vo.
  Artizan for Dec. 1861 and Jan. 1862. 4to.
  Athenæum for Dec. 1861 and Jan. 1862. 4to.
  Chemical News for Dec. 1861 and Jan. 1862. 4to.
  Engineer for Dec. 1861 and Jan. 1862. fol.
  Horological Journal, No. 41. 8vo. 1861.
  Journal of Gas-Lighting for Dec. 1861 and Jan. 1862. 4to.
  Mechanics' Magazine for Dec. 1861 and Jan. 1862. 8vo.
  Medical Circular for Dec. 1861 and Jan. 1862. 8vo.
  Practical Mechanics' Journal for Dec. 1861 and Jan. 1862. 4to.
  St. James's Medley, No. 29. 8vo. 1862.
  Technologist, Dec. 1861 and Jan. 1862. 8vo.
Franklin Institute of Pennsylvania—Journal, Vol. XLIII. Nos. 5, 6. 8vo. 1861.
Geneva, Société de Physique—Mémoires: Tome XVI. Partie 1. 4to. 1861.
Geographical Society, Royal—Proceedings, Vol. VI. No. 1. 8vo. 1862.
Geological Institute, Imperial, Vienna—Jahrbuch, 1860. No. 2. 8vo.
Greenwich Royal Observatory (*through the Royal Society*)—Greenwich Observations for 1858. 4to. 1860.
Gyll, Gordon Willoughby James, Esq. M.R.I. (*the Author*)—History of the Parish of Wraysbury, Ankerwycke Priory, and Magna Charta Island; with the History of Horton and the Town of Colnbrook, Bucks. 4to. 1862.
Held, Dr. Joseph (*the Author*)—System des Verfassungsrechts der Monarchischen Staaten Deutschlands, mit besonderer Rücksicht auf den Constitutionalismus. 2 vols. 8vo. Würzburg, 1856-7.
Horticultural Society, Royal—Proceedings, 1861, No. 31; 1862, No. 1.
Jablonowsky Society, Leipsic—Preisschriften, VIII. und X. 8vo. 1861.
Ladd, Mr. W. (*the Publisher*)—Dr. H. M. Noad: The Improved Induction Coil. 16to. 1861.
Lubbock, John, Esq. F.R.S. M.R.I.—On the Lake Habitations of Switzerland. (From Nat. Hist. Rev. Vol. II.) 1862.
Mackie, S. J. Esq. F.G.S. (*the Editor*)—The Geologist for Dec. 1861; Jan. 1862. 8vo.
Mechanical Engineers' Institution, Birmingham—Proceedings, May and Aug. 1861.
Medical and Chirurgical Society, Royal—Medico-Chirurgical Transactions. Vol. XLIV. 8vo. 1861.
Newton, Messrs.—London Journal (New Series) for Dec. 1861; Jan. 1862. 8vo.
Parkers and Bourn, Messrs. (*the Publishers*)—J. Hullah: The History of Modern Music; a Course of Lectures delivered at the Royal Institution of Great Britain, in 1861. 16to. 1862.
Petermann, A. Esq. (*the Editor*)—Mittheilungen auf der Gesammtgebiete der Geographie. 1861. Nos. 11, 12; Ergänzungsheft 7. 4to. 1861.

Photographic Society—Journal, Nos. 116, 117. 8vo. 1861–62.
Radcliffe Trustees, Oxford—Professor H. W. Acland's Report on the Transfer of the Radcliffe Library to the Oxford University Museum. 8vo. 1861.
Royal Society of Edinburgh—Transactions, Vol. XXII. Part 3. 4to. 1861.
Proceedings, No. 53. 8vo. 1860–1.
San Fernando, Royal Academy of—D. José Amador de los Rios: El Arte Latino-Bizantino en España y las Coronas Visigodas de Guarrazar. 4to. Madrid, 1861.
Scottish Society of Arts, Royal—Transactions, Vol. VI. Part 1. 8vo. 1861.
St. Petersburg Academy of Sciences, Imperial—Mémoires, Tome III. Nos. 10–12. 4to. 1861. Bulletins, Tome III. Nos. 6–8; Tome IV. Nos. 1, 2. 4to. 1861.
Statistical Society—Journal, Vol. XXIV. Part 4. 8vo. 1860.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, Juli und August. 4to. 1861.

WEEKLY EVENING MEETING,

Friday, February 7, 1862.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. Vice-President, in the Chair.

Professor T. H. Huxley, F.R.S.

On Fossil Remains of Man.

The purpose of the discourse was to give an explanation of the interest attaching to two casts upon the table—the one that of a skull, discovered and described by Professor Schmerling, from the Cave of Engis, in Belgium; the other, discovered by Dr. Fuhlrott and described by Professor Schaaffhausen, from a cave in the Neanderthal, near Düsseldorf—the former being the oldest skull whose age is geologically definable, the latter the most aberrant and degraded of human skulls.

The nature and extent of the cranial modifications exhibited by the man-like apes and by man were discussed; and their modifications were shown to depend upon variations in the capacity and in the form of the cranium, in the greater or less development of its ridges, and in the size and form of the face. In respect of such differences, skulls have been called dolichocephalic and brachycephalic, orthognathous and prognathous, &c.

Neither orthognathism or prognathism are necessarily correlated with brachycephaly or dolichocephaly. But the most extreme prognathism is accompanied by a dolichocephalic cranium, while perfect orthognathism may occur with extreme brachycephalism.

The known varieties of the skull have a certain geographical distribution, which may be broadly expressed by drawing a line upon a map of the world from Russian Tartary to the Gulf of Guinea, and by regarding the two ends of that line as ethnological poles, while another line, drawn at right angles to it, from Western Europe to Hindostan, may be called the ethnological equator.

At the north-eastern pole are situated the people with the most eminently brachycephalic and orthognathous skulls; at the south-western pole, those people who have the most eminently dolichocephalic and prognathous skulls; while along the ethnological equator the races of men are, for the most part, oval-headed, or, if dolichocephalic, they are orthognathous. Passing from the ethnological poles, in either direction, there is a tendency to the softening down of the extreme types of skull. Turning from this general view of cranial modification, which was expressly stated to be open to many exceptions in detail, the question was next raised whether the distribution of cranial forms had been the same in all periods of the world's history, or whether the older races, in any locality, possessed a different cranial character from their successors.

No evidence of the existence of such older and different races has yet been obtained from Northern Asia, from Africa beyond the shores of the Mediterranean, or from Australia; it may be that the Alfourons and the mound-builders of the Mississippi valley are to be regarded as ancient stocks which preceded modern immigration; but definite evidence is wanting with regard to these and similar cases. In Northern and Western Europe, however, there is little doubt that several races, different in cranial conformation and in civilization, have succeeded one another. Below and beyond the traces of Roman civilization, archæologists find evidence, first, of people who used iron, then of those who employed bronze, and then of those who were acquainted only with stone and flint (or bone) weapons and implements. How far these various weapons may have been used at different epochs by the same people, is a question yet to be decided; but that in some parts of Europe, at any rate, they characterize people of different cranial structure, appears to be tolerably well made out.

The remarkable crania from tumuli of the stone period at Borreby, in Denmark, figured by Mr. Busk, were cited as authentic examples of the skulls of people of the epoch in which stone axes ground to an edge were the chief weapons.

The evidence of the antiquity of these people afforded by the peat bogs of Denmark, and the probability of their contemporaneity with the makers of the "refuse-heaps" of Denmark, and of the pile-works of Switzerland, were next considered. Ancient as the Borreby race may be, they peopled Denmark subsequently to its assumption of its present physical geography, and since its only great quadrupeds were the urus, the bison, and deer.

The Engis skull, on the other hand, is of a date antecedent to the last great physical changes of Europe, and its owner was a contemporary of the mammoth, the tichorine rhinoceros, the cave bear, and the cave hyæna, so that a vast gulf of time separates him from the Borreby men. The skull was shown, however, by all its measurements, to be nearly as well developed as that of an average European.

The Neanderthal skull, whose age is not exactly known, on the contrary, is the lowest and most ape-like in its characters of any human skull yet discovered, though it presents certain points of resemblance to the Borreby skulls.

Great as are the differences between the Engis, the Borreby, and the Neanderthal skulls, the speaker stated that it would not be justifiable to assign them even to distinct races of men; for by a careful examination of the crania of one of the purest of living races of men, —the Australian,—it is possible to discover skulls which differ from one another in similar characters, though not quite to the same extent, as the ancient ones.

Thus it appears that the oldest known races of men differed comparatively but little in cranial conformation from those savage races now living, whom they seem to have resembled most in habits; and it may be concluded that these most ancient races at present known were at least as remote from the original stock of the human species as they are from us.

[T. H. H.]

WEEKLY EVENING MEETING,

Friday, February 14, 1862.

Rev. John Barlow, M.A. F.R.S. Vice-President, in the Chair.

William Odling, M.B. F.R.S.

SEC. CHEM. SOC.

On Mr. Graham's Researches on Dialysis.

Vial Diffusion.—When an open vial, filled with a solution of some salt or other substance, is introduced into a jar of water, a portion of the dissolved salt passes gradually from the vial into the external water. This portion is known as the *diffusate.* The diffusates yielded by different substances under precisely the same circumstances vary greatly. Thus, common salt yields double the amount of diffusate that Epsom salt yields, while Epsom salt diffuses twice as rapidly as gum-arabic. Every substance has its own definite rate of diffusibility in the same liquid medium, dependent upon the nature of the medium,—

whether water or alcohol, for instance. The diffusion of any particular salt is scarcely affected by the presence of a different salt, though materially affected by the presence of the same salt in the external liquid. The general law seems to be, that the velocity with which a dissolved salt diffuses from a stronger into a weaker solution is proportional to the difference of concentration, *quoad* that salt, between the contiguous solutions. The phenomena of liquid diffusion are manifested most simply and uniformly when dilute solutions only are employed.

Jar Diffusion.—When the solution of the salt to be diffused, instead of being placed in a vial, is conveyed by means of a pipette to the bottom of a jar of water, the dissolved salt gradually rises through the superincumbent water to a height or extent proportional to its diffusibility. The results of jar diffusion bear out generally those of vial diffusion. They show, moreover, the absolute rate or velocity of the diffusive movement. Thus, during a fourteen days' aqueous diffusion from ten per cent. solutions of gum-arabic, Epsom salt, and common salt respectively, the gum-arabic rose only through $\frac{7}{14}$ths of the superincumbent water, or to a height of 55·5 millimetres; while the Epsom salt rose through the whole $\frac{14}{14}$ths of the superincumbent water, or to a height of 111 millimetres; while the common salt not only rose to the top, but could have risen much higher, seeing that the uppermost or 14th stratum of water into which it had diffused, contained about fifteen times as much salt as was contained in the uppermost or 14th stratum of water into which the Epsom salt had diffused.

Diffusive Separations.—If a solution containing equal weights of common salt and gum arabic be poured into a jar of water, the ratio of salt to gum in the dilute liquid in the jar will be the same as in the original solution. But if the original solution be poured into a diffusion vial from which the dissolved compounds can diffuse into a jar of water, for every 100 milligrammes of salt, about 22½ milligrammes of gum will pass out into the external water, or the ratio of salt to gum in the dilute liquid in the jar will be as 100 to 22½, instead of as 100 to 100, or a separation of the gum from the salt to the extent of 77½ per cent. will have been effected. Again, when a solution containing 5 per cent. of common salt and 5 per cent. Glauber's salt (the diffusive rates of which salts are to one another as 100 to 70) is submitted for seven days to the process of jar diffusion, the upper half, or $\frac{7}{14}$ths of superincumbent water, will be found to contain 380 milligrammes of common salt and only 53 milligrammes of Glauber's salt, or the ratio of common salt to Glauber's salt on the upper half of the liquid will be as 100 to 14, instead of as 100 to 100, or a separation of the Glauber's salt from the common salt to the extent of 86 per cent. will have been effected. Not only the partial separation of mixed bodies, but the partial decomposition of definite chemical compounds may be effected by diffusion. Thus when alum, which is a double sulphate of the two metals, potassium and aluminium, is allowed to diffuse from its aqueous solution, the diffusive tendency of potassium compounds being much greater than that of aluminium compounds, a portion of sulphate of potassium actually breaks away from the sulphate of aluminium with which it was in combination, in order to diffuse into the external or superincumbent water more freely than the sulphate of aluminium can diffuse.

Crystalloids and Colloids.—Diffusibility does not seem to be associated in any definite way with chemical composition. Thus, complex organic bodies, like picric acid and sugar, have much the same diffusion-rates as common salt and Epsom salt respectively. Isomorphous compounds, however, are for the most part equi-diffusive, but the groups of equi-diffusive are larger than those of isomorphous bodies. The common salt group, for instance, includes not only chloride, bromide, and iodide of sodium, which are similar in composition and isomorphous in form, but also nitrate of sodium, which is dissimilar in composition and heteromorphous in form. But in comparing highly diffusive substances on the one hand, with feebly diffusive substances on the other, one broad dissimilarity becomes apparent, namely, that highly diffusive substances affect the crystalline state, while feebly diffusive substances are amorphous, and, in particular, are characterized by a capability of forming gelatinous hydrates. Hence the distinction established by Mr. Graham between highly diffusive bodies, or *crystalloids*, and feebly diffusive bodies, or *colloids*. There are very many compounds which can exist, both in the crystalline and gelatinous states, and which present two distinct diffusion-rates corresponding respectively thereto.

Nature of Diffusion.—Liquid diffusion may be attributed to a self-repulsion of the particles of the salt or other body which diffuses on the one hand, and of the particles of water or other liquid into which it diffuses on the other; or it may be attributed to a particular kind of mutual attraction existing between the particles of the salt and of the water. Assuming the truth of this latter hypothesis, it is clear that the diffusive attraction of crystalloid particles for water is greater than that of colloid particles; and, recognizing this superior diffusive attraction of saline particles, it is conceivable that a salt should be able to unite diffusively, not only with free water, but also with water that is already in a low form of combination, as it exists in a soft solid, for instance, such as jelly: and this is found to be the case. Common salt, for example, diffuses into a mass of solid jelly almost as easily and extensively as into a similar bulk of free water. But although the introduction of a gelatinous substance does not interfere in any appreciable way with the diffusion of a crystalloid, it arrests almost completely the diffusion of a colloid body. The colloid has but very little tendency to unite diffusively, even with free water, and is quite incapable of abstracting and uniting diffusively with water that is in any state of combination, however feeble.

Dialysis.—Although a simple diffusion into water can effect the partial separation of a highly diffusible from a feebly diffusible substance, yet a much better result may be obtained by causing the diffusion to take place, not into free water, but into or through the combined water of a soft solid, which, as before observed, scarcely affects the diffusion of crystalloid, but almost arrests that of a colloid body. When a piece of vellum, or of membrane, or of parchment-paper, or even a layer of mucus, is interposed between a colloid solution and a quantity of water, the colloid, in order to get to the free water, must pass through the membrane, that is to say, must unite diffusively with the combined water of the membrane, which however it is incapable of doing. The crystalloid body can pass through, but the colloid cannot. This constitutes *dialysis*, which means the separation of crystalloid from colloid bodies, through a membrane that will allow the passage of crystalloid, but will not allow that of colloid particles; because the crystalloid particles, having a highly diffusive power, can unite diffusively with the combined water of the soft solid septum, so as to reach the external free water; whereas the colloid particles, having a very feebly diffusive power, cannot. The process of dialysis is altogether different from that of filtration. Filtration refers to the passage of masses through appreciable pores, but in a dialytic septum there are no pores, and the movement is not molar but molecular. The dialytic septum will allow chemical action to take place, or that low form of chemical action which constitutes diffusion; but, being quite impervious to the mechanical passage of liquid, it will not allow of filtration.

Colloid Solutions.—The solution of various colloids has been heretofore effected by means of crystalloid chemicals, comprising acids, bases, and salts. By dialyzing these liquids, the crystalloid reagents diffuse away, and leave the colloid bodies in simple aqueous solution. Mr. Graham has thus obtained pure colloidal solutions in water of numerous mineral and organic substances, such, for instance, as silica, tin-stone, alumina, hæmatite, chrome, Prussian blue, prussiate of copper, sucrate of copper and other sucrates, tannin, gum, caramel, albumin, &c. These colloidal solutions are for the most part unstable. Either spontaneously, or on the addition of a very minute quantity of some or other crystalloid reagent, they pectize, or become converted into solid jellies. Hence Mr. Graham speaks of two colloidal states, the *peptous*, or dissolved, and the *pectous* or gelatinized. Colloid bodies are characterized by their non-crystalline habit, by their low diffusibility, by their chemical inertness, by their high atomicity, and above all by their mutability. All these properties are exceedingly well manifested by colloid silica, or co-silicic acid.

[W. O.]

WEEKLY EVENING MEETING,

Friday, February 21, 1862.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

James Fergusson, Esq.

On the Site of the Holy Sepulchre at Jerusalem.

The speaker commenced by stating that this was perhaps the most interesting subject that could be addressed to a Christian audience, being neither more nor less than the re-discovery of the true site of the Holy Sepulchre, after a knowledge of it had been lost for eight centuries; and it was the more interesting, as having been made by a process of direct philosophical deduction in archæology—a new science, which had as yet few such triumphs to record.

The question commenced necessarily with the undoubted fact that the Emperor Constantine erected a magnificent church at Jerusalem, over the site of the Sepulchre; and the assumption that since his time the church in the middle of the city always had been, and still was, *his* "Church of the Sepulchre," coupled with the erroneous supposition that it had replaced the original building of Constantine.

In carefully studying the subject of Saracenic Architecture, from its remains in India, Egypt, Persia, Constantinople, Spain, &c., he had always been unable to account for the peculiarities of one remarkable building at Jerusalem—that generally known as the "Mosque of Omar." This, however, was evidently a misnomer; for the building violated the invariable and essential principles of mosque architecture. The precept of the Koran being, that all true believers should turn their faces towards Mecca when they prayed, a mosque was really only an indicator of the direction of the Holy City, whereas the building in question had its principal entrance towards the South, so that the worshipper on entering turned his back on Mecca—a sacrilege too horrible to be conceived, and which is not to be found in any mosque in the whole Mahomedan world.

The building in question being within the Haram area, was not generally accessible to travellers; and, until it had been surveyed, measured, and drawn, in the year 1833, by Mr. Catherwood and Mr. Arundale, no adequate representations of it had ever been made. On examining these drawings, he had been at once so much struck with the architectural and other features they displayed, that he immediately pronounced the building to be the identical church erected by Constantine; the internal evidence proved it to be a Christian sepulchral building of that age; and there could be no other answering that description than the original church of the Holy Sepulchre.

With this conviction he published his "Essay on the Ancient Topography of Jerusalem," in the year 1847; and the result of fourteen years' consideration, notwithstanding his views had met with much opposition, had only confirmed his original opinion.

Since the classification and nomenclature of the different kinds of Gothic architecture, by Rickman, the successive periods of that style had become fixed and universally recognized. The date of a Gothic structure, whether Norman, Early-English, Decorated, or Perpendicular, was irrevocably determined by the evidence of its style alone. Classical architecture, since this mode of classification had been invented, was abused as Pagan, Heathen, sensuous, and sensual, and accordingly neglected: but his own studies assured him that the same regular progression which was admitted to mark Gothic architecture, was to be traced in every other true style; indeed there was not a single building in the world anterior to the cinque-cento period which did not bear upon it the impress of its age, and to which the educated student could not assign a date.

In the decadence of classical architecture, the gradual conversion of the noble sculptural frieze of the Corinthian order into a mere rounded moulding, could be clearly traced, through successive stages, in edifices the dates of which were known; and in like manner the entablature itself was seen gradually tending towards a curvilinear form, until the arch became the real constructive feature, although a trabeated or beam form was still for some time retained merely as an ornament. Of this modification of classic architecture, the Palace of Diocletian at Spalatro furnished a striking example; and the so-called Mosque of Omar presented architectural characteristics equally marked and peculiar. So clear was this progressive modification, that it might be taken absolutely that no arch was to be met with as an essential decorative feature in any building older than the year 300; nor any entablature in a building erected later than the year 400; and hence the architectural evidence clearly proved the so-called Mosque of Omar to have been erected in the age of Constantine.

Again, its form and arrangement distinctly marked it, not as a mosque, but as a sepulchral building. It presented the strictest analogy in these respects to the Baptistry of the Lateran, built by Constantine, it is said to contain his own tomb; and that of Sta. Agnese, erected by him, to contain the tomb of his daughter, Sta. Costanza; as also to other early sepulchral buildings in Italy, as well as to all the Mahomedan tombs.

Moreover, it was the only church or building in the world having in its centre *a large and prominent Rock*, rising some height above the floor, filling the space beneath the dome. The Rock, forming the Holy Sepulchre, was expressly referred to by Eusebius, who described the church which Constantine erected over it; and the rock in this building had one cave in it exactly as described by this author. Every word, in fact, which Eusebius uses in his narrative of the discovery of the Holy Sepulchre, and of the building erected over it by Constantine, is so completely borne out by what is now found here, and is so essentially confirmed by the architecture, that it is impossible to escape the conviction that what we now look upon is the identical building to which he refers.

It may be added, that although the proposition has now been for more than fourteen years before the public, the architectural evidence has never been disputed by any competent authority, notwithstanding the opposition which the conclusions drawn from it have been met with on other grounds; and until it is refuted, the argument must be considered as proved.

The description which Eusebius gave of the Basilica of Constantine, in the immediate vicinity of this sepulchral church, pointed to the "Golden Gateway," now forming part of the city wall, and further tended to fix the site of the sepulchre. This gateway was evidently not originally a city gate; it had no carriage-way, and was not fortified; and its architecture, as in the case of the Dome of the Rock, bore unquestionable evidence of the age of Constantine.

Besides the direct evidence derived from the architectural character of these two buildings, there was almost as strong, though negative evidence, derived from the examination of the Mosque el Aksah situated in the same inclosure. That building was undoubtedly erected by Abd El Malek in the first century of the Hegira, or about A.D. 695, and showed such a difference of style, such a progress towards mediævalism, that some centuries must have elapsed between its erection and that of the buildings just referred to.

Another curious coincidence was, that Justinian had erected his Mary Church so near this spot, that most travellers contended that the Aksah must be the identical building. There was abundance of evidence to prove that that was not so. But the fact of that Emperor having come to this corner of the city, so near to the accursed temple of the Jews to erect his church, proved that the great group of sacred localities must have been in this neighbourhood.

In concluding this part of his subject, the speaker contended that the historical, local, and architectural evidence connected with these four buildings made together so clear and tangible a proof of his proposition, that no direct refutation of it had been attempted, and, in so far as he could judge, none was possible.

An objection to this theory had been raised, that the church of Constantine was not within the city wall, whereas the Mosque of Omar was. The difficulty was, however, at once removed by the knowledge that the wall which now runs along the eastern side of the Haram area was erected by Herod Agrippa twelve years after the crucifixion, and consequently the locality was at that time certainly without the walls. It was also clear that the site of this building was the Hill of Zion;

and that the Tombs of the Kings of Judah were always in that locality; and there were many other tombs in the neighbourhood. The speaker illustrated this branch of the argument by reference to the site of the Temple of the Jews, as described by Josephus—the position of the Jews' Wailing Place—the erection of the "El Aksah," avowedly on the site of the temple, by the Mahomedans (a true mosque, pointing towards Mecca) about the year 695, and to a small mosque really built by Omar, which still exists attached to the aisle of the Aksah, and overhanging the southern wall, and to the desecration of the Aksah by the Knight Templars, who resided in and took their name from it. While, on the other hand, the Crusaders reverenced the Dome of the Rock, under the title of "Templum Domini," and considered it as sacred, if not more so than the "Sepulchrum Domini." All which proved that from the time of Constantine to the Crusades the site and extent of the Jewish temple were perfectly well known, and that Jews, Christians, and Moslems knew perfectly well that this building was outside it, and built by Christians, though for what purpose in that uncritical age they did not care to inquire.

Another difficulty was the break in the tradition as to the true site of the Sepulchre. Down to the time of the Crusaders, the Mosque of Omar, or rather the "Dome of the Rock,"—for that was the only name by which it was known to the Mahomedans—was recognized as the Church of the Sepulchre; but after that period the church of the middle of the city assumed that title, without any record being preserved of the change. The difficulty was startling, if judged by the standard of the nineteenth century; but to those who were familiar with the depth of the darkness that prevailed in the eleventh, and what strange things were then passed unnoticed, the difficulty would hardly occur. Even in the present day, the jealousy of the Greek Christians bid fair to establish a rival to the true Garden of Gethsemane in the Valley of Kedron. The cause of the change was the fierce and dreadful persecution of the Christians by the Caliph El Hakim, about the year 1000. Every Christian was driven from Jerusalem for a period of forty-eight years—long enough to impair their traditions. The Basilica of Constantine was destroyed, but the Dome of the Rock was respected, for the Mahomedans reverence Christ as the Sixth Prophet in descent from Adam, Mahomet being the Seventh. On their return, the Christians built a sepulchral church in the city for the celebration of the Easter rites, similar to the round churches of this and all other Christian countries; and this became the resort of pilgrims when pilgrimages again became a fashion. These pilgrims were led by the priest to believe their church marked the Sepulchre; and the delusion was maintained with the Crusaders. Indeed, he believed at that period no great importance was attached to the question of the exact site.

The church in the city was rebuilt by the Crusaders as it now exists, and no portion of it was older than the year 1100.

Some mediæval accounts of Jerusalem were quoted, in proof that the Dome of the Rock was early regarded as the site of the Sepulchre. About A.D. 530, a pilgrim wrote that under the rock there was a well, that by placing the ear to it the sound of water might be heard below, and that if an apple were thrown into the cavity it would float onward underground to the pool of Siloam, where it would be found. It was most remarkable that all these conditions applied to the rock in the Mosque of Omar at this day; the sound of running water could be heard, and M. Pierrotti had actually traced its current to Siloam. On the contrary, in the church in the city there was no rock, no cave, no well, and no communication with Siloam. Other references by mediæval writers tended to confirm this view.

In the church in the city, instead of the natural rock, there was a tabernacle of marble, built on the pavement, in the middle ages; and the Golgotha, under the same roof, was built up of marble and granite: the stone of the country being limestone.

Although not of vital importance in a religious point of view, still it was desirable that the Bible narrative should be reconciled with local knowledge. The Dome of the Rock answered all the requirements of the Bible narrative, and of Eusebius; the so-called Church of the Sepulchre answered none. It was true the main argument rested upon architectural evidence; but this, to the educated mind, was perfectly conclusive. It might not be agreeable to have the faith disturbed, or old traditions upset, but the speaker felt the utmost confidence that it would and must be ultimately admitted that the building called the Dome of the Rock, or the Mosque of Omar, was the identical church which Constantine the Great built over, what he at least believed to be, the Sepulchre of Christ.

The discourse was illustrated by a map of Jerusalem, and plans and drawings of the buildings referred to; including Mr. Carl Haag's elaborate drawing of the rock and interior of the church.

[J. F.]

WEEKLY EVENING MEETING,

Friday, February 28, 1862.

SIR HENRY HOLLAND, Bart. D.C.L. F.R.S. Vice-President, in the Chair.

A. E. DURHAM, Esq.

On Sleeping and Dreaming.

[Abstract Deferred.]

GENERAL MONTHLY MEETING,

Monday, March 3, 1862.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

John Birkett, Esq. F.R.C.S. F.L.S.
Jonathan Sparrow Crowley, Esq. F.G.S.
Major-General Charles James Green.
Alexander Henderson Macdougall, Esq.
The Rev. George Musgrave Musgrave, M.A. Oxon.
A. C. Brisbane Neill, M.D.
Francis Pirie, Esq.
Robert Pryor, Esq.
Sir Joshua Rowe, C.B.
Samuel Scott, Esq.
Edward Henry Sieveking, M.D.
Oswald Augustus Smith, Esq.
Alexander John Sutherland, M.D. F.R.S.
James Thomas White, Esq.
Herbert George Yatman, Esq.

were *elected* Members of the Royal Institution.

Robert R. Carew, Esq.
William Whitaker Collins, Esq. and
John Parnell, Esq.

were *admitted* Members of the Royal Institution.

The following Letter from the Comte de Paris to the Secretary, was read:—

"Washington, *February 2nd*, 1862.

"Sir,

"I have just received notification that the Members of the Royal Institution, among whom my brother and I had the honour of being admitted last year, were to meet on the 13th of January, to draw up an Address to the Queen on occasion of the sudden and irreparable loss of the Prince Consort.

"While loyal England was offering to her afflicted Sovereign the tribute of an unanimous and touching sympathy, every man in the world felt the loss of the Prince whose intelligence, energy, and untiring exertions had been, during a too short career, exclusively devoted to the progress of civilization and the benefit of mankind. But the extent of that loss can only be fully appreciated by those who had many opportunities of witnessing that domestic happiness so cruelly interrupted and who shall always keep, with as much gratitude as regret, the vivid remembrance of one who proved not only a kind relation but a friend to all their family.

"We, therefore, deeply regret not to have heard of that Meeting in time to send from this distant country our sincere adhesion to the homage paid to his respected memory by the illustrious Society over which he so often presided, and not to have been able to avail ourselves of our privilege as Members of the Royal

Institution precisely in the circumstance where we should have been most anxious to do so.

"I beg you, Sir, to convey to the President the expression of these regrets, and I remain,

"Very truly yours,
"LOUIS-PHILIPPE D'ORLEANS,
"COMTE DE PARIS."

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same: viz.

FROM

Asiatic Society of Bengal—Journal, No. 283. 8vo. 1861.
Astronomical Society, Royal—Monthly Notices, Jan. 1862. 8vo.
Barclay, Mr. G. (the Author)—Monograms: D. 8vo. 1862.
Basel, Naturforschender Gesellschaft—Verhandlungen: Theil III. Heft 1, 2. 8vo. 1861.
Bombay Branch of the Royal Asiatic Society—Journal, No. 21. 8vo. 1862.
Chemical Society—Quarterly Journal, Nos. 57, 58. 8vo. 1862.
Civil Engineers' Institution—Proceedings, February, 1862. 8vo.
De la Rue, Warren, Esq. Ph.D. F.R.S. M.R.I. (the Author)—Engraving of the Great Comet of 1861, as seen by a Newtonian Equatorial of 13 inches, aperture, July 3, 1861.
D'Orsey, Rev. A. J. B.D. (the Author)—Practical Grammar of Portuguese and English. 16to. 1860.
Dublin Society, Royal—Journal, Nos. 20–23. 8vo. 1861.
Editors—American Journal of Science, by B. Silliman, &c., for Jan. 1862. 8vo.
Artizan for Feb. 1862. 4to.
Athenæum for Feb. 1862. 4to.
Chemical News for Feb. 1862. 4to.
Engineer for Feb. 1862. fol.
Horological Journal, No. 42. 8vo. 1862.
Journal of Gas-Lighting for Feb. 1862. 4to.
Mechanics' Magazine for Feb. 1862. 8vo.
Medical Circular for Feb. 1862. 8vo.
Practical Mechanics' Journal for Feb. 1862. 4to.
Technologist for Feb. 1862. 8vo.
Fergusson, James, Esq. (the Author)—Notes on the Site of the Holy Sepulchre at Jerusalem. (K 88) 8vo. 1861.
Franklin Institute of Pennsylvania—Journal, Vol. XLIV. No. 1. 8vo. 1862.
Geological Society—Quarterly Journal, No. 69. 8vo. 1862.
Horticultural Society, Royal—Proceedings, 1862. No. 2.
Hungarian Academy of Sciences—Transactions, Proceedings, and Reports (in Hungarian). 4to. and 8vo. 23 vols. 1833–61.
Mackie, S. J. Esq. F.G.S. (the Editor)—The Geologist for Feb. 1862. 8vo.
Mailly, E. (the Author)—Essai sur les Institutions Scientifiques de la Grande-Bretagne et de l'Irlande. II. 16to. (O 13) 1862.
Mitchell, George, Esq. (the Author)—Letters to Earl Russell, respecting the late Events at Warsaw. (K 88) 8vo. 1862.
Newton, Messrs.—London Journal, (New Series), for Feb. 1861. 8vo.
Pratt, Henry F. A. M.D. (the Author)—The Genealogy of Creation, newly translated from the Unpointed Hebrew Text of the Book of Genesis. 8vo. 1861.
Petermann, A. Esq. (the Editor)—Mittheilungen aus dem Gesammtgebiete der Geographie. 1862. No. 1. 4to.
Photographic Society—Journal, No. 118. 8vo. 1861.
Swanwick, Miss Anna, M.R.I. (the Translator)—Selections from the Dramas of Goethe and Schiller. 8vo. 1843.
United Service Institution, Royal—Journal, No. 19. 8vo. 1861.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, Sept. und Oct. 4to. 1861.
Beavan, Hugh J. C. Esq. (Annual Subscriber)—Spanish Bandarilla, Knife, and Cap, and Minerals and Fossils, from Funchal, Madeira, and other places.

WEEKLY EVENING MEETING,

Friday, March 7, 1862.

THE REV. JOHN BARLOW, M.A. F.R.S. Vice-President, in the Chair.

PROFESSOR D. OLIVER,

OF UNIVERSITY COLLEGE, LONDON.

On the Distribution of Northern Plants.

THE discourse referred primarily to the botanical evidence bearing upon the hypothesis advanced by Professors Unger * and Heer † of an Atlantic communication between Europe and America at some period of the tertiary epoch. The close analogy which is to be traced between the miocene flora of central Europe and the existing flora of the Eastern American States, these authors conceive can only be explained by assuming such direct overland connection of the two Continents.

The speaker explained the basis upon which comparisons between two recent floras and between a recent and a fossil flora should rest, referring to the peculiar conditions which affect the latter comparison owing to the imperfect and partial character of the fossil element. The general character of the tertiary flora of central Europe was described. In the tertiary beds of Switzerland, according to Professor Heer,‡ about 800 species of Phanerogamia have been discovered, referable to 197 genera (exclusive of *Phyllites*, *Carpolithes*, &c.), of which number 154 still exist. Of these genera—

76 are common to the Swiss tertiary, and to the present flora of . .	Europe.
77	Japan.
88	Ditto States, America.
120	Europe and Asia (taken together, and including Japan).

It is to be noted that the 77 of Japan include 26 not occurring in Europe: amongst them several forms highly characteristic of the ter-

* 'Die versunkene Insel Atlantis.'
† 'Flora Tertiaria Helvetiæ.'
‡ The Tertiary data were throughout derived from the 'Flora Tertiaria Helvetiæ' of Professor Heer.

tiary, as *Glyptostrobus*, numerous *Fici*, coriaceous-leaved oaks and *Lauraceæ*, *Juglandeæ*, *Liquidambar*, &c.

The genera, common to the Swiss tertiary and the United States, which are not found also in the old world, are *Sabal*, *Taxodium*, *Bumelia*, *Liriodendron*, *Ceanothus*, *Ptelea*, and *Carya*. But in respect to these 7 it was observed that at least 5 were very doubtful determinations. The 9 largest orders of the 'Flora Tertiaria Helvetiæ' are *Leguminosæ*, *Amentaceæ*, *Cyperaceæ*, *Proteaceæ*, *Lauraceæ*, *Gramineæ*, *Coniferæ*, *Compositæ*, and *Aceraceæ*. Of these Orders 3 are included in the 9 largest of Europe, 4 in the 9 largest of the United States, and 6 in the 9 largest of Japan, while the remaining 3 of the tertiary, not included in the 9 largest orders of Japan, are much more largely developed in Japan than in the United States. They are *Lauraceæ*, *Aceraceæ*, and *Proteaceæ*.

The proportion of ligneous to herbaceous species in the above floras was alluded to. Heer estimates ligneous plants to have formed about 66 per cent. of the phanerogamic vegetation of the tertiary in Switzerland. The speaker considered this estimate as too high, believing that sufficient allowance had not been made for the advantages that ligneous plants, which are often tall-stemmed, possess over herbaceous species in securing access of their leaves and debris to the waters in which they had been floated, and ultimately preserved. He admitted, however, that ligneous species were relatively very numerous in the vegetation of the tertiary period. The proportion of ligneous plants he estimates in the existing flora of Japan at near 40 per cent., in the Southern States 22, Northern States 18, Europe 9 to 12.

The intimate relationship traceable between the tertiary and Japanese floras in the numerous characteristic types common to both; the issue of the ordinal and generic comparisons given above; the larger proportion of ligneous species in the Japanese than in the Eastern American flora; and the number of types peculiar, at the present day, to Eastern America and Eastern Asia, compared with the few restricted to Europe and America, the speaker contends, favour the view advanced by Professor Asa Gray in reference to plants and by Mr. Darwin as to animals, viz.—that the migration of forms to which is due the community of types in the Eastern States of North America and the miocene of Europe, took place to the north of the Pacific; an overland communication it may be supposed, having existed during the tertiary time somewhere about Behring's Straits or the line of the Aleutian Islands. This view is confirmed by the occurrence of miocene vegetable remains in North-west America (including genera yet growing in Japan but lost to America), which prove, further, the temperature of these latitudes to have been at that time sufficiently high to have permitted their existence so far north.

The evidence in favour of the 'Atlantis' hypothesis might, moreover, be expected to have been more marked in the existing vegetation of the Atlantic Islands than is the case. Professor Heer points out the genera *Clethra*, *Bystropogon*, *Cedronella*, and *Oreodaphne* as common to the Atlantic Islands and America. Japanese species, however, have been described of *Clethra* and *Cedronella;* and Messrs. Webb and Berthelot limit *Bystropogon* to Atlantic Island species. *Oreodaphne* occurs in South Africa and adjacent islands.

A connection between these Islands and Europe, at perhaps a late period of the tertiary, may be considered as highly probable from the predominance of Mediterranean forms in their flora. The few genera characteristic of the tertiary which they possess may have been derived during this connection; but the hypothesis that a continent should have extended westward as far as America, the speaker considered the available botanical evidence did not in the least substantiate.

Through the kindness of Dr. Hooker the speaker was enabled to exhibit numerous specimens,—living, dried, and fossil, illustrative of his observations.

[D. O.]

WEEKLY EVENING MEETING,

Friday, March 14, 1862.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. Vice-President, in the Chair.

William Scovell Savory, Esq. F.R.S.

On Motion in Plants and Animals.

Vital motion is at once associated with our idea of an animal. We speak of Voluntary motion as emphatically an animal function. And this truly and naturally: for it is in the animal kingdom that we are accustomed to witness motion which depends on the vital property of a structure; and those movements with which we are most familiar—those seen in ourselves and in the higher animals—we know to be usually under the influence of a will. But while the power of producing motion, and motion too which depends on the vital property of a structure, is an attribute of the vegetable as well as of the animal kingdom, voluntary motion in the proper acceptation of the term is restricted to a portion of the animal kingdom.

Voluntary motion is therefore an animal function, inasmuch as there is no evidence whatever that any movement in plants is voluntary; at the same time it must be remembered that there is not a tittle more evidence that the motion exhibited by far the largest number of animals is voluntary.

Nor is it possible to say when or where the earliest indications of voluntary motion appear—that is to say, a movement under the influence of a will: but undoubtedly the application of the phrase should be restricted within much narrower limits than it commonly is.

We are apt to confound the term 'voluntary' with the visible adaptation of a movement to an obvious purpose; but a passing consideration of some of our own functions will render this error manifest. For example, what movements are more obviously adapted to a definite purpose than those of the muscles of respiration—the muscles by whose action the respiratory movements are effected? Yet we know that neither the will nor even consciousness is at all times concerned in them, for they work most efficiently during the profoundest sleep.

Again, the evident purpose and adaptation manifested in the movements of animals—the cold-blooded for example—when beheaded, must be admitted to be independent of will or consciousness, or we are driven to the extravagant assumption that these faculties may remain after the brain is removed.

Pass at once to the other extreme of animal life and watch the movements of Infusoria. What actions do they exhibit more entitled to be called voluntary than those just mentioned? When we observe the movements which these active atoms accomplish, we are prone, from our preconceived ideas of motion as we commonly witness it in the higher animals and in ourselves, to consider it voluntary without sufficient evidence.

Nor is it only in the lowly organized Infusoria that we seek in vain for any evidence of the existence of a will. There are no good grounds for concluding that the actions of creatures far beyond these are in their nature voluntary.

Be it remembered that there is no more evidence of the existence even of nerve-force in the simplest animals than in plants. No trace of nerve-structure has yet been demonstrated.

The visible adaptation of a movement to an obvious purpose, is therefore not necessarily evidence that it is voluntary. It by no means implies even consciousness; far less of itself does it indicate a will.

Still more closely is Locomotion associated with our idea of the nature of an animal. And although, when the higher individuals or extreme forms of the two kingdoms are compared, the power of locomotion is undoubtedly a striking and distinctive attribute of an animal, yet while on the one hand vast numbers of the lower animals are fixed to one spot, on the other, many of the lowest forms of vegetable life possess the power of locomotion. Amongst the lower members of either kingdom many are locomotive in the earlier, and fixed in the later, periods of their existence. Some others again are locomotive only during the later stages of their existence.

The movements which are exhibited by living beings are not only very various in themselves, but obviously dependent on widely different causes. They may, I think, be thus classified:*

Motion due to extraneous causes.

Molecular movement—the peculiar movement visible in the minutest granules or molecules of matter when placed in a fluid which is not too tenacious, and viewed by the highest powers of the microscope—is an example of this kind of motion. It is due, in great part at least, simply to vibration.

Movements which are due to osmotic currents and other hygroscopic changes.

In the vegetable kingdom the expulsion of the seeds from the ripe fruit of the spirting cucumber (*Momordica Elaterium*) may be selected as a remarkable example. Although we cannot bring forward from the animal kingdom such striking illustrations of osmotic force as this one, yet there is every reason to believe that it is constantly playing a most important part in the transference of matter.

Movement due to the physical properties of a tissue.

Elastic tissue plays an interesting and important part in various movements of animals; and this simply by means of its physical property of elasticity which it possesses in perfection. This elastic tissue is of course passive, until either extended or compressed by some force—and this force is usually muscular action—when as the force is remitted it recoils with proportionate power. Thus we commonly consider its use in relation to muscular action. It may either assist muscular action—be accessory to it, or it may be opposed to muscular action—acting in an opposite direction. Elastic tissue is commonly rendered active by stretching, sometimes however it is compressed. As illustrations of these several modes and purposes of action may be mentioned the functions of the *Ligamentum Nuchæ*, of the spring and ligament at the hinge of the shell of bivalves, of the elastic ligaments attached to the claws of *Felidæ*, and of the vocal cords. Again, movements produced by the elastic recoil of stretched or distended tissues in plants are common enough. To elasticity the bursting of seed cases and anthers is without doubt to be in great measure referred.

Movements due to a structure possessing the vital property of contractility—vital motion.

By vital contractility is to be understood the power which certain structures possess of altering their form, of approximating distant parts by an inherent force.

Examples of vital contractility in animals are found everywhere.

* From the causes of motion I exclude the changes which are involved in nutrition; for no attempt to review these could be brought within the scope of this discourse.

To certain structures in which this property is most plainly and strongly marked the term muscle is applied; but these structures are so various in their characters, that the only common distinctive feature they possess is contractility. Moreover, other forms of contractile tissue occur in the animal kingdom, as certain cell-walls, or cell contents or globules. Nay more, a substance, presenting no structure or any definite form whatever, may yet possess this power. Now, although the term muscle is not commonly applied to all these forms of contractile substance, but usually restricted to those varieties which assume a definite shape, and appear as fibres, yet an extended examination of the various forms of contractile substance which occur in the animal kingdom will show that this distinction, although perhaps better than any other, and sufficient for ordinary purposes, is nevertheless an arbitrary one, and cannot be applied in all cases. The several forms of contractile substance pass so insensibly into each other, that it is not practicable to point out when or where distinct and isolated fibres first appear; so that those who insist upon the distinction are compelled to admit the existence of transitional forms: so numerous and gradual are the changes by which special parts—fully-formed muscular fibres, arise out of the general structure—amorphous, contractile substance. It is at least clear that the various structures, commonly called muscles, are not connected by any physiological characters which distinguish them from other forms of contractile tissue. We can but say of the muscles, as of all forms of contractile tissue, that they are distinguished only by the possession of a peculiar power.

But a structure endowed with vital contractility is not peculiar to the animal kingdom. Although much more restricted in its occurrence and extent, in the degree to which it is developed, and proportionally in its power of action, and never, so far as we know, appearing in the form of striated fibre in the vegetable kingdom; yet a tissue possessing vital contractility is found in plants. A substance endowed with this power occurs in the vegetable kingdom, which, in structure and function, cannot be distinguished from the simplest form of animal contractile tissue.

The substance called 'Protoplasm' exhibits changes of form and other movements which cannot be explained by any physical property, or by any extraneous influences. These movements are most remarkably shown at times in the spaces of young cellular tissue. The movement termed rotation or gyration, which is often seen in the contents of young "cells," and which, in some form or other, are probably of general occurrence, may depend on the contractility of protoplasm. They are said by those who have studied them to present a close resemblance to those of Amœba and its allies. No one has yet shown a distinction of importance between protoplasm of the vegetable and sarcode of the animal kingdom.

But there are other movements in plants, the cause of which is less equivocal. Such movements are not confined to the lowest plants,—as the *Oscillatoriæ*,—but are met with amongst the most highly organized members of the vegetable kingdom. The movements of sensitive plants, various species of Mimosa, of Dionæa Muscipula, of certain tropical species of Desmodium, of the stamens of Berberry, &c., can be referred only to vital contractility of certain of their tissues. Whatever obscurity may hang over these, let it be remarked that there is the same evidence of the nature of this vital contractility in plants as in animals. It is dependent on life, and not, like any physical property, retained so long as the structure itself is not destroyed. So, also, these movements either occur spontaneously, or may be excited by various stimuli—touch for example. If these motions depend upon elasticity or hygroscopic changes, or any other physical cause which elsewhere operates, how could stimuli act to produce them? Moreover, they appear to be governed by the same laws that regulate their action in the animal kingdom. Their energy varies with the vigour of the plant. Excessive exercise produces exhaustion, but the power is restored during subsequent repose. This evidence, thus clear and satisfactory, receives a remarkable and most interesting confirmation from the effects produced by the vapour of chloroform.

Now, it would be startling to talk of the muscles of plants, but, nevertheless, we see that a structure endowed with vital contractility occurs in them. This is simply a question of the relation of the term muscle to contractile tissue. The term must either be limited to certain forms only of contractile tissue in animals—and it may be doubted whether it be practicable even thus arbitrarily to define its application—or it must be extended to the contractile tissues of plants. At all events it is obvious that we can draw no physiological distinction between the contractile tissues of plants and animals.

The contractile tissues of the higher animals are indeed capable of being excited to activity by a peculiar stimulus—the nerve-force; but this special stimulus does not exclude the operation of others, and, what is nearer to our purpose, there is no more evidence of the operation of nerve-force exciting vital contractility in the substance of the simplest animals than in plants.

After excluding from both kingdoms all those movements which are due either to extraneous causes, or simply to the physical properties of a tissue, there still remains in the vegetable, as in the animal kingdom, motion which unquestionably depends on vital contractility, and this is perhaps the most universal attribute of living beings.

[W. S. S.]

WEEKLY EVENING MEETING,

Friday, March 21, 1862.

The Rev. John Barlow, M.A. F.R.S. Vice-President, in the Chair.

F. A. Abel, Esq. F.R.S.
DIRECTOR OF THE CHEMICAL ESTABLISHMENT OF THE WAR DEPARTMENT.

On some of the Causes, Effects, and Military Applications of Explosions.

A glance was taken at the general nature and causes of the phænomena termed Explosions, and attention was then specially directed to those explosions which are due to chemical agency.

In all instances of chemical action accompanied by an explosion, the production and violence of the latter are either entirely or principally due to the sudden and very considerable development of heat, which results from the disappearance, for the time, of chemical activity. The violence of such explosions is therefore regulated by the energy of the chemical action, or the degree of rapidity with which the chemical change takes place. There are instances in which the change of state (*e. g.* the conversion of solids into vapours and gases), resulting from chemical action and the suddenness with which this transformation occurs, would suffice to produce explosive effects, quite independently of the effects of heat developed by the change ; but in all such instances the sudden increase in volume of the matter, resulting simply from the chemical change, is insignificant as compared with the expansive effect exerted, at the same time, by the heat developed in consequence of the sudden and violent disturbance of chemical equilibrium. Thus, the actual volume of gas produced on the decomposition of gunpowder, though very considerable in comparison with that of the original solid, is but small when compared with the volume which it occupies at the moment of its production, when under the influence of the intense heat resulting from the chemical change.

Explosions are occasionally produced by energetic chemical combination between elementary substances. Thus, potassium combines with bromine with explosive violence, in consequence of the powerfully expansive effect of the heat resulting from the intense and sudden chemical action between the two elements. Again, the union of hydrogen with oxygen or chlorine is so energetic, that the resulting water or hydrochloric acid is suddenly and enormously expanded by the heat developed; a powerfully explosive effect being consequently produced.

Explosions are much more frequently the result of chemical decomposition. Several classes of compounds are known, the unstable character of which endows them with explosive properties. Thus the compounds known as the chloride, iodide, and bromide of nitrogen are highly susceptible of instantaneous decomposition; the very slightest disturbing causes sufficing to destroy the chemical equilibrium which exists between their component particles. Compounds of silver and gold with nitrogen, hydrogen, and oxygen (fulminating silver and gold), and of silver and mercury with a peculiar organic group, generally known as fulminic acid (the fulminates of mercury and silver), are also highly susceptible of sudden, and therefore violently explosive, decomposition. By the action of nitric and nitrous acids upon several organic bodies, compounds of highly explosive characters are produced, their formation resulting from the abstraction (by oxidation) of a proportion of hydrogen-atoms from the original body, and the introduction, in their place, of a high oxide of nitrogen. The products of the action of nitric acid upon starch and cotton, in different forms, are the best known of these; among others, the substances known as nitromannite (obtained by the action of nitric acid upon mannite) and nitroglycerine, or glonoïne (the product of the action of nitric acid at low temperature upon glycerine), are remarkable for the violence with which they explode when submitted to friction or concussion. One of the most recently-discovered and curious of these explosive organic bodies is the nitrate of diazobenzol, obtained by the action of nitrous acid at a low temperature upon aniline. This substance explodes at least as violently as iodide of nitrogen and fulminate of silver, if exposed to a heat approaching that of boiling water; it is, however, far less sensitive to friction than those two bodies. Similarly explosive substances have been quite recently obtained by Dr. Hofmann from derivatives of the interesting and important base, rosaniline, the salts of which furnish some of the most beautiful of the colours now obtained from aniline.

Explosions are most readily produced by establishing chemical action between certain substances, greatly opposed to each other in their properties, and brought together in an intimate state of mixture. The substances applicable to the production of such mixtures are, on the one hand, bodies remarkable for their great affinity for oxygen; and, on the other, compounds containing that element in abundance, and partly, or entirely, in a loose state of combination. To the first class belong the elements carbon, sulphur, and phosphorus, and compounds of the last two, with readily oxidisable metals; the second class includes a few of the higher metallic oxides (such as the higher oxides of manganese and lead) and combinations of metals with nitric, chloric, and perchloric acids. Mixtures produced with these two classes of bodies readily ignite, or afford explosions, either upon the direct application of heat, or by submitting them to friction, percussion, or concussion; and, in a few instances, by establishing chemical action in a small portion of the mixture, with the aid of some other compound. These explosive mixtures vary greatly in the ease with which chemical action is established in them, and in the rapidity and violence of their transformation; their properties are naturally regulated by the chemical and physical characters of their constituents, and by the degree of intimacy of their mixture.

The variation in their explosive properties, and the great extent to which the characters of any particular mixture may be modified, are very important elements in their application to practical purposes; while the comparatively instantaneous nature of the decomposition of explosive compounds, and the facility with which it is brought about, present very great, and in many cases insuperable, obstacles to their employment as explosive agents. By the comparatively gradual decomposition of an explosive mixture, such as gunpowder (when employed as a charge in a gun), the force exerted, by the gases generated in the confined space, discovers, before it attains its maximum, that portion of the chamber enclosing the powder (*i. e.* the projectile) which is separated from the remainder. By the motion which it immediately imparts to this, the *smaller* mass, the strain upon the *larger* mass, forming all but one side of the chamber (*i. e.* the breech of the gun), is at once relieved, while the force continues, to the close of its development, to act in the direction of the mass which has once yielded to its influence, and thus propels the projectile. The explosion of a charge of a fulminate, on the other hand, in the chamber of a gun, is so instantaneous that the maximum of force is at once developed, and the strain thus exerted within the chamber, at the same time that it overcomes the inertia of the projectile (or the movable side of the chamber), will also overwhelm the cohesive force which maintains the mass of the chamber entire, and the breech of the gun will therefore be shattered. Enclosed in a shell, a charge of a fulminate will produce a much greater shattering effect than gunpowder upon the metal enveloped, reducing it to a much larger number of fragments; but the pieces of the shell, produced by employing gunpowder as the bursting agent, will be propelled with much greater violence, because there is still a development of force after the rupture of the shell, while, with the fulminate, the entire force is at once expended upon the bursting of the shell.

The very great extent to which the rapidity of explosion of gunpowder may be modified to suit different applications, is one of the most important properties possessed by this material. A very rapidly burning powder is necessary in many instances; for example, in shrapnel shells, in which the charge of powder is required to break open the shell without interfering, by any great dispersive effect, with the flight of the enclosed bullets or fragments of metal. In mortars, and short guns also, a quickly burning powder is required, as they afford a comparatively limited space for the combustion of the charge.

If a slowly burning powder be employed in such arms, a portion of the unexploded charge is expelled together with the projectile, the period between the first ignition of the powder, and the expulsion of the shot or shell from the gun, being insufficient for the combustion of the entire charge. In long guns and in rifled cannon it is very important, on the other hand, that the ignition of the charge of powder should take place gradually, so that the pressure exerted thereby upon the gun and the projectile should, after the first ignition, be as far as possible uniformly continuous during the passage of the shot or shell along the principal portion, if not the entire length, of the gun's bore. With the gunpowder which has been, until quite recently, in general use for large cannon, the actual explosion of a charge is almost entirely accomplished before the projectile has passed beyond the trunnions of the gun. Hence the rear portion of the weapon is subject to a strain which is enormous as compared to that sustained by the front part of the cannon. Numerous important advantages naturally result from a more uniform distribution of the pressure over the interior of the gun; for instance, the necessity of constructing the part reaching from the breech to the trunnions of very much greater strength than the remainder (a measure which, in the production of cast-iron cannon, involves considerable difficulties) is greatly diminished, and the risk of fracture of guns, or of their serious injury from submission to excessive strain, is considerably lessened. The explosive action of gunpowder may, it need hardly be observed, be easily regulated by the introduction of modifications in the proportions of the carbon, sulphur, and saltpetre employed in its manufacture, and in the degree of intimacy with which the ingredients are mixed. Both of these expedients interfere, however, with the extent of force ultimately exerted by a given weight of the gunpowder; since, in either case, the chemical action between the ingredients would be modified. The rapidity of combustion of gunpowder may, however, be admirably regulated, without introducing any alteration in its composition or in the perfection of its manufacture, simply by increasing or diminishing the size of the particles or grains constituting a charge; and also by modifying the degree of compression to which the gunpowder is subject before, or at the time of, its conversion into grains or pellets.

By combining the application of uniform and accurately regulated pressure with modifications in the composition of gunpowder, and by thoroughly confining the material within a case or receptacle, so that, if ignited, it can only burn in one direction, admirable and valuable arrangements (known as Fuzes and Time-Fuzes) are obtained for igniting charges of gunpowder in shells at any period, during their flight, which may have been determined upon previous to the loading of the gun. By simple mechanical arrangements, regulating the amount of the compressed gunpowder which shall burn before the flame reaches the charge in the shell, the time of explosion is readily adjusted with the greatest nicety (subject, however, to variations depending upon the degree of density of the atmosphere, as recently shown by Dr. Frankland's researches). The principle of regulated compression, and of combustion in one direction, is applied to the preparation of rockets, signals, and numerous pyrotechnic arrangements, other explosive mixtures being, in some instances, substituted for the gunpowder.

The advantages offered by materials of a much more powerfully or rapidly explosive character than gunpowder, when employed simply as destructive agents (for instance, in many classes of mining operations), have led to repeated attempts at the application, as substitutes for gunpowder, of highly explosive mixtures, readily obtainable in large quantities, in which chlorate of potassa is employed, in the place of a nitrate, in conjunction with very oxidisable materials, such as the sulphides of arsenic and antimony, and compounds containing carbon and hydrogen (Callow's mining powder and white or German gunpowder are examples of such compounds). All attempts to manufacture and employ such mixtures have, however, invariably terminated in more or less disastrous results, in consequence of the comparatively low temperature at which chlorate of potassa exerts its oxidizing power. Very slight friction or percussion suffices to inflame many of these mixtures, and the violence of their explosive action is, in many instances, as difficult to control as that of explosive chemical compounds. Even in the manufacture and employment of comparatively so safe an agent as gunpowder, which may be subjected, without ignition, to tolerably powerful friction or percussion, and to the direct application of any temperature below that which suffices to ignite sulphur (about 550° Fah.), the neglect of strict precautions, for excluding the possibility of a particle of the powder being subjected to sudden and powerful friction, may, and frequently does, lead to accidental explosions. The occasional accidents in gunpowder manufactories are generally enveloped in mystery, in consequence of their fearfully destructive effects; in all cases, however, where it has been possible to trace the causes of such explosions, they have been found in the wilful or accidental neglect of simple precautionary measures, indispensable to the positive safety of the works and operators.

The more highly explosive mixtures, and some few explosive compounds, though inapplicable as substitutes for gunpowder, on account of their great sensitiveness to the effects of heat, have, in consequence of this very quality, received important applications in numerous ingenious contrivances for effecting the ignition of gunpowder. Well-known instances of such applications are:—The employment of fulminate of mercury in percussion-caps; of a mixture of chlorate of potassa and sulphide of antimony, in arrangements for firing cannon by percussion and by friction, and for exploding shells by percussion or concussion; and of the same mixture, exploded at will, by being brought into contact with a drop of strong sulphuric acid, for the ignition of submarine mines or of signals.

Other mixtures, combining a high degree of explosiveness with power of conducting electricity, have been successfully applied to the simultaneous ignition of numerous charges of gunpowder by electricity of high tension: by means of one of them, recently discovered, many mines may be simultaneously discharged, even by the employment of small magneto-electric machines; the necessity for the employment of voltaic arrangements in mining operations being thus entirely dispensed with.

One of the most highly explosive mixtures at present known, consisting of chlorate of potassa and amorphous phosphorus, has been most ingeniously applied by Sir William Armstrong to the ignition of his time-fuzes, and to the production of concussion and percussion-fuzes, remarkable for the great ease with which they are exploded. The above mixture may be ignited by the application of a gentle heat, or by submission to moderate pressure; if it is made up into a hard mass by mixture with a little shellac-varnish, the friction resulting from the rapid insertion of a pin's point into the material suffices to ignite it, even when it is well covered with varnish. Thus, in Armstrong's time-fuze, which, when fixed in its place in the head of the shell, cannot, like ordinary fuzes employed in smooth-bore guns, be ignited by the flame of the exploding charge of powder (as the shell accurately fits the bore of the gun), the fuze-composition is inflamed, immediately upon the firing of the gun, in the following manner:—A small quantity of the phosphorus-mixture is deposited at the bottom of a cylindrical cavity in the centre of the fuze, and over it is fixed a small plug of metal, with a pin's point projecting from its lower end. This plug is held in its place by a pin of soft metal, which by reason of the *vis inertiæ* of the plug, is broken when the gun is fired, and the pin then instantly pierces the pellet of detonating mixture, which, by its ignition, sets into action the time-fuze. The distance between the pin's point and the phosphorus-mixture, before the explosion, is only one-tenth of an inch. This arrangement exemplifies in a striking manner the delicacy of action which may be obtained by a judicious combination of simple mechanical arrangements and highly explosive materials.

The variety of work accomplished by the explosion of a charge of powder in an Armstrong gun loaded with a shell—no less than five distinct and important operations being thereby effected before the shell leaves the gun—affords a most interesting illustration of the progress made in the application of explosives, and of the comparatively great control which may be exercised over the operations of those destructive agents.

[F. A. A.]

WEEKLY EVENING MEETING,

Friday, March 28, 1862.

JOHN PETER GASSIOT, Esq. F.R.S. Vice-President, in the Chair.

REAR-ADMIRAL FITZ-ROY, F.R.S.

An Explanation of the Meteorological Telegraphy, and its Basis, now under trial at the Board of Trade.

THE telegraphic communication of meteorological changes from distant stations to a central position, whence occasional warnings of impending storms might be given, which has been organized and tried by Government, had its origin at a meeting of the British Association in 1859, at Aberdeen, under the presidency of that deeply lamented Prince whose short life was wholly devoted to the most useful objects. It was then resolved by their Council that application should be made to Her Majesty's Government for an organization and trial of a plan by which the approach of storms might be telegraphed to distant localities. At two meetings in Buckingham Palace, early the following year (1860), minutes were authorized on this subject, and correspondence ensued which resulted in establishing a telegraphic communication of meteorological facts between twenty home stations, besides foreign ones—and daily with Paris.

The Aberdeen meeting had only just terminated, when public attention was startled by the loss of the 'Royal Charter.' It so happened that the storm which caused the destruction of that iron ship (notwithstanding power of steam additional to that of sails—and while a *sailing* ship, managed differently, was bearing its brunt uninjured within a few miles distant *)—that storm, completely cyclonic, passed over the middle of England, and could be more fully investigated than *any* storm hitherto, because in every direction observers happened to be ready, who recorded ample statical facts, and many valuable dynamical results.

Advancing gradually, the first cautionary or storm-warning signals were made early in 1861, but on that occasion were unhappily disregarded in the Tyne, and on the following days awful losses of life were witnessed on the north-east coasts. From that time to the present similar warnings have been given there and elsewhere,—with increasingly advantageous effects, it appears—if one may judge, in the first

* 'The Cumming.'

instance, by applications since sent officially from all the principal ports, and from the chief associations of Underwriters, in addition to Admiralty approval, and the co-operation of the Coast-guard.

In August, 1861, the first *published* "forecasts" of weather were tried; and after *another* half-year had elapsed for gaining experience by varied tentative arrangements, the *present* system was established. Twenty reports are now received each morning (except Sundays), and ten each afternoon, besides five from the Continent. Double forecasts (*two* days in advance) are published, with the full tables (on which they *chiefly* depend), and are sent to six *daily* papers, to one weekly,—to Lloyds'—to the Admiralty,—and to the Horse Guards, besides the Board of Trade.

These forecasts add almost nothing to the pecuniary expense of the system,—while their usefulness, practically, is said to be more and more recognized.* Warnings of storms arise out of them; and (scarcely enough considered) the satisfaction of knowing that no very bad weather is imminent, may be very great to a person about to cross the sea. Thus their negative evidence may be actually little less valuable than the positive.

Prophecies or predictions they are not:—the term forecast is strictly applicable to such an *opinion* as is the result of a scientific combination and calculation, liable to be occasionally, though rarely, marred by an unexpected "downrush," † of southerly wind, or by a rapid electrical action not yet sufficiently indicated to our extremely limited sight and feeling. We shall know more and more by degrees. At present it is satisfactory to know that the measures practised daily in these proceedings do not depend solely on one *individual.* They are the results of facts exactly recorded,—and deductions from their consideration, for which rules have been given. An assistant has been practised and is able to share their responsibility. Others are also advancing in the subject of dynamical meteorology.

Air currents sometimes flow side by side, though in opposite directions, as "parallel streams," for hundreds or even thousands of miles. Sometimes they are more or less superposed—occasionally, indeed *frequently*, crossing at various angles; sometimes combining, and by the *composition* of their forces and *qualities* causing those varieties of weather that are experienced as the wind veers more toward or from the equator or the nearest pole; and sometimes so antagonistic in their angular collision as to cause those large circling eddies or rotatory storms called cyclones (in modern parlance), which are really like the greater storms in all parts of the world, although they do *not* quite assimilate to those local whirlwinds, dust-storms, and other commotions

* At a recent meeting of the shareholders of the Great Western Docks, at Stonehouse, Plymouth, it was stated officially that "the deficiency (in revenue) is to be attributed chiefly to the absence of vessels requiring the use of the graving docks for the purpose of repairing the damages occasioned by storms and casualties at sea."—(Feb. 24, 1862.)

† Herschel.

of atmosphere which seem to be more *electrical* in their characteristics, if not in their origin.

Whenever a polar current prevails at any place, or is *approaching*, the air becomes heavy, and the barometer is high or rises. When the opposite (equatorial or tropical) prevails or approaches, the mercury is low or falls, because the air is, or is *becoming*, specifically lighter, and these changes take place *slowly*.

Whenever, from any causes—electrical, chemical, or simply mechanical—either current, or any combination of currents, ceases to press onward *without being opposed*, a *gradual* lightening of the atmosphere, through a greater or less area of hundreds, or perhaps thousands of miles occurs, not suddenly, but very gradually, and the barometer falls. There is less tension.

To restore equilibrium, the nearest *disposable* body of air (so to speak) or most moveable, advances first; but an impulse, at the same time, may be given to other and greater masses that—though later in arriving—may be stronger—last longer, and cause greater pressure mechanically as well as by combination. Air, like water, mingles very slowly, either from above or laterally.

Taking, with Dové, north-east and south-west (*true*) as the "wind-poles," all intermediate directions are found to be more or less assimilated to the characteristics of those extremes; while all the variations of pressure, or *tension*, many of those caused by temperature, and all varieties of winds, may be clearly and directly traced to the operations of two constant principal currents—equatorial or tropical, and polar—our north-east and south-west.

Great distinction should be marked between those ever alternate and often conflicting main currents—tropical and polar, and the *local effects* of their union, or antagonism, namely mixed winds—whether westerly or easterly, with occasional cyclones or circulating eddies, on a large or small scale.

Considering that the lower current does not ordinarily extend far upward (only a few thousand yards, or even feet), and that high land, mountains, especially *ranges* of mountains, alter and impede its progress, a variety of eddy winds, or as it were streams of wind, with local and apparently anomalous effects, must be frequently caused.

Electrical action, condensation of vapour into hail, snow, rain, or fog, causing *heat;* or its other changes, namely, evaporation, rarefaction, and expansion—absorbing heat, and therefore causing *cold*—immediately affect currents of air in a degree proportional to such influence; inducing horizontal motion.

The polar current always *advances* from the polar quarter, while *laterally* moving eastward (like a ship making lee-way), being pressed towards the east by the tropical flow which advances from the *south-westward*, usually above and at an angle with the polar stream or current of air, often mixing with it, but at times *separately* penetrating downward, then sweeping and warming the earth's surface, uncombined with the polar current, even while feeling its approaching influence: and thus, as it were, forcing passages between streams of chilling polar air that at the same time are moving in opposite and nearly parallel directions.

Sometimes their opposition is so equal, and equilibrium is so complete, that a *calm* is the result, no sensible movement *horizontally* along the earth's surface being perceptible.

Self-registering barometers show the alterations in tension, or, so to speak, the *pulsations*, on a large scale, of atmosphere, by hourly marks; and the diagram expresses to a practised observer what the "indicator card" of a steam cylinder shows to a skilful engineer, or a stethoscope to a physician.

Our own Islands have very peculiar facilities for meteorological communication by telegraph, between outlying stations on the sea coast and a central place—all being at nearly the same level, and nearly all comparatively uninfluenced by mountain ranges.

And now, the results are, that, having daily knowledge of weather (including ordinary facts of a meteorological nature), at the extreme limits and centre of our British Islands, we are warned of any *great change* taking place; the greater atmospherical changes being measured by days—rather than by hours. Only local changes, however violent they may be occasionally (and dangerous in proportion to their suddenness and violence),—only such changes are unfelt at a distance, and do not influence great breadths,—say, hundreds of miles in area,—of atmosphere.

Extensive changes, showing differences of pressure, above or below the normal or mean level, amounting to an inch or thereabouts, are certain to be followed by a marked commotion of the elements in the course of a few days. If the fall has been sudden, or the rise very rapid,—swift, but brief, will be the resulting elementary movement; if slow, or gradual,—time will elapse before the change, and the altered state of weather will take place more gradually, but last longer.

Notice may thus be obtained, and given, a few hours, or a day, or even some days, before any important change in the weather actually occurs.

Having such knowledge, it obviously follows that telegraphic warning may be sent in any direction reached by the wires, and that occasionally, on the occurrence of very ominous signs, barometric and other—including always those of the heavens—such cautions may be given before storms as will tend to diminish the risks and loss of life so frequent on our exposed and tempestuous shores.

It has been proved also, lately, that storms, indeed all the greater circulations of atmosphere between the tropics and polar regions, have an eastward motion, bodily, while circulating around a centrical area. Within the tropics it is otherwise, or westward, till they *recurve*.

In answer to a question from the Royal Commissioners on Lights, Buoys, and Beacons, Sir John Herschel stated that—"the most important meteorological communication which could be telegraphed, would be information *just fresh received by telegraph*, of a cyclone actually in progress at a great distance, and working its way towards the locality. There is no doubt that the progress of a cyclone may be telegraphed, and might secure many a ship from danger by forewarning."

Successive, or rather, *consecutive* gyrations, circuits, or cyclones often affect one another, acting as temporary mutual checks, until a combination and joint action occurs; their union causing even *greater* effects: as may be seen even in water currents,—as well as in the atmosphere.

Between the tropics and the polar regions, or in temperate zones, the main currents are incessantly active, while more or less antagonistic, from the causes above mentioned: besides which, wherever considerable changes of temperature, development of electricity, heavy rain, or these in combination, cause temporary disturbance of atmospheric equilibrium (or a much altered *tension* of air) these grand agents of nature, the two great currents, speedily move by the *least resisting lines*, to restore equilibrium, or fill the comparative void. One current arrives, probably, or acts *sooner* than the other,—but invariably collision occurs, of some kind or degree, usually occasioning a circuit, a cyclonic or ellipsonic gyration; however little *noticed* when gentle, or moderate in force.

As there must be resistance to moving air (or conflicting currents), to cause gyration, and as there are no such causes, on a large scale, near the equator, there are no storms (except local squalls) in very low latitudes.

It is at some distance, from about five to twenty degrees, from the equator that hurricanes are occasionally felt in their violence.

They originate in or near those hot and densely-clouded spaces, sometimes spoken of as the "*cloud-ring*," where aggregated aqueous vapour is at times condensed into heavy rain (partly with vivid electrical action) and a comparative vacuum is suddenly caused, towards which air rushes from all sides. That which arrives from a higher latitude has a westwardly, that from a lower an eastwardly tendency, due to the earth's rotation, and to the change of latitude, whence a chief cause of the cyclone's invariable rotation in one direction, as above explained.

The hurricane or cyclone is impelled to the *west*, in *low* latitudes, because the tendency of *both* currents there is to the westward, along the surface: although one, the tropical, is *much less so*, and becomes actually easterly *near* the tropic, after which its equatorial centrifugal force is more and more evident, while the *westwardly* tendency of the polar current diminishes; and therefore, at that latitude, hurricane cyclones cease to move westward (re-curve), go then eastwardly, and toward the polar quarter.

Great and important changes of weather and wind are *preceded*, as well as accompanied, by notable alterations in the state of the atmosphere. Such changes, being indicated at *some* places sooner than at others around the British Islands, give frequent premonitions; and therefore, great *differences* of pressure (or tension) shown by barometer of temperature, of dryness, or moisture, and direction of wind, should be considered as *signs of changes, likely to occur soon.*

It will be observed, on any continued comparison of weather reports, that during the stronger winds a far greater degree of uniformity and regularity is shown than during the prevalence of moderate or light breezes: and this should be remembered.

When neither of the greater and more extensive atmospheric currents is sweeping across the British Islands,—currents of which the causes are remote, and on a large scale,—the nature or character of our winds approaches, and is rather like that of land and sea breezes in low latitudes; especially in summer.

Either the cooler sea wind is drawn in, over land heated by the summer sun; or cold air from frosty heights, snow-covered lands, or chilly valleys, moves towards the sea, which is so *uniform in temperature* for many weeks together, changing so *slowly*, and but little, in comparison with land, during the year. These light *variables* may at such times be numerous, simultaneously, around the compass, on the various coasts of the British Islands.

Frequently it has been asked, "In this country, how much rise or fall of the glasses may foretell remarkable change, or a dangerous storm?"

To which can now be replied,—great changes or storms are *usually* shown by falls of barometer exceeding an inch; and by differences of temperature exceeding about fifteen degrees. A tenth of an inch an hour is a fall indicating a storm or very heavy rain. The more rapidly such changes occur, the more risk there is of dangerous atmospheric commotion.

As all barometric instruments often, if not usually, show what may be expected a day or even days in advance, rather than the weather of the present or next few hours, and as wind, or its *direction*, affects them much more than rain or snow, due allowance should always be made for days as well as for hours to come.

The general effect of storms is felt unequally in our islands, and less *inland* than on the coasts. Lord Wrottesley has shown, by the anemometer at his observatory in Staffordshire, that wind is diminished or checked by its passage over land. The mountain ranges of Wales and Scotland, rising two to four thousand feet above the ocean level, must have great power to alter the direction, and probably the velocity of wind, independently of alterations caused by changes of temperature.

"It not unfrequently happens that a series of cyclones follow closely upon each other for several weeks, the preceding members of the series being often overtaken and interfered with by those succeeding. It is, however, important to remark, that amidst all the complexity necessarily occasioned by such combinations—the greater and more violent storms, and particularly that portion of them which is most dangerous and destructive, exhibit almost invariably the *simple* cyclonic character. It is thus with the 'Law of Storms' as with the 'Law of

Gravitation;' the grand results of both are exceedingly simple, but the minor details become more and more complicated in proportion to their minuteness." *

Consecutive storms, at the meetings of main currents, in zones of latitude, at certain periods, have had appearances of continuity. The familiar instance of the 'Charles Heddle' has so often been adduced as proof of continuing circuitous action, or gyration, that it may seem injudicious to doubt the *evidence;* but knowing how frequently circuits, or cyclones, succeed each other *rapidly;* and how unreliable are some of the earlier logs of events in a storm, *written after its cessation,* especially respecting directions of wind and courses steered, when waves and storm blasts were the guides, not the oscillating compass (if indeed *that* had not been washed away, as in the 'Charles Heddle's' case); it does not appear accordant to experience, and enlarged acquaintance with the subject, to imagine that such atmospheric eddies are, *sui generis,* erratic, and so considerably independent, as to cross a wide ocean.

When opposing currents meet, their masses must *continue* in motion a certain time, either rotating, or ascending, or going onward horizontally in *combination.*

Masses of air, either of polar or tropical origin, so to speak, *returning,* when driven back by stronger opposition,—at first, and for a certain time, retain the characteristics of their peculiar and very different natures.

In our latitudes there is a continuous alternation of air currents—each specifically different, and denoting approach by marked characteristics: and we have proved by successive series of simultaneous statical observations, over a wide range,—embracing Scotland, Ireland, all England, and adjacent islands,—that while these alternating or circuitously moving currents are thus incessantly passing—the whole body of atmosphere filling our temperate zone is moving gradually towards the east—at an *average* rate of about five geographical miles an hour.

During strong westerly winds this eastward motion is greatly increased; and in easterly gales it is proportionally diminished, as measured by its passage along a horizontal surface of earth or ocean. Knowing these circumstances, and having accurate statical observations of these various currents, at selected outlying stations,—showing pressure (or tension), temperature, and relative dryness, with the direction and estimated horizontal force of wind at each place simultaneously, the dynamical consequences are already measurable approximately, on geometrical principles; and, judging by the past, there appears to be reasonable ground for expectation that, soon, meteorological dynamics will be subjected to mathematical analysis and accurate formulas. The facts now weighed and measured mentally—in what may be correctly called "forecasting" weather—are—the direction and force of *each* air current, or wind—reported telegraphically to the

* W. Stevenson, of Dunse, 1853.

central station in London, from many distant stations; their respective tension and temperature, moisture or dryness, and their changes since former recent observations.

These show whether any or either movement or change is on the increase, or decrease—whether a polar current is moving *laterally* off —passing from our stations towards Europe—or approaching us from the Atlantic—whether moving *direct* towards the south-westward with great velocity, or with slow progress. If moving *fast*, in the direction of its length, it will approach England more from the east—its speed *direct* being twenty to fifty or eighty miles an hour—while its *constant* lateral or easterly tendency (like a ship's leeway, in a current) being only five miles an hour is then insensible to us—(though clearly deducible from other facts ascertained), and is that much in alteration of actual *direction*, as well as of what would *otherwise* be the velocity of the polar current.

With the opposite principal current—the equatorial or south-westerly, more briefly and correctly *tropical*—similar but opposite results occur—the direct motion from a south-*westerly* quarter is *accelerated*—sensibly to our perception—by *part* of the *eastward* constant (about five miles hourly)—and therefore a body of air approaches us *sooner* (other things being equal) from the westward, than it does from the eastward.

To seamen accustomed to navigate in ships making leeway, while in currents setting variously over the ground, such movements, complicated as they may appear, are familiar. Another important consideration is the disposal or progress of bodies of air united, or mixed, or contiguous to each other, after their meeting—either directly opposed or at an angle—on the earth's (or ocean's) surface. They do not vanish:—They cannot go directly upwards—against gravitation;—Westward they cannot generally go when there is collision or meeting, because the momentum, elasticity, and extent of the tropical "anti-trade"* usually overpowers any direct polar current, or rises over it, and more or less affects the subordinate below, by the friction of its eastward pressure. Downward there is no exit—eastwardly (towards the east) the accumulating air must go—and this tendency continued causes the *varieties* of wind from the westward—being more or less mixed—more or less purely polar or tropical as either one prevails in combination.

After a body of air has passed, and gone to some distance southward or northward, it may be stopped by an advancing and more powerful mass of atmosphere which is moving in a direction contrary to, or diagonally across its line of force. If their appulse be gradual and gentle, only a check occurs—and the weaker body is pushed back until its special qualities, respecting temperature and moisture, are so masked by those of its opponent as to be almost obliterated. But, if these currents meet with energy—at very different temperatures and

* Sir John Herschel's excellent term.

tension, rapid changes are noticed as the wind shifts—and circuitous eddies, storms, or cyclones occur.

Otherwise—when their meeting is, as first mentioned, *gradual*—there is the *return* of a portion of either current (which previously prevailed) either direct or deflected—deflected even through more than one quadrant of a circle—by its advancing opponent—and retaining for some considerable time its own previous characteristics.

Thus we have, for short times, cold dry Winds from the south-west, instead of the usual warm and moist ones;—or winds of this latter kind from the north, instead of cold ones. The circuitous tendency of air in motion—and the numerous impediments to its horizontal progress, such as land, ranges of mountains, hills, or even cliffs—induce many a deviation from normal directions, extremely puzzling to the student of this subject; but so retentive is air of its tension and temperature, for a time, that, like currents in the ocean, each may be traced by its characteristics as long as within our island web of stations. When the polar current is driven back by a tropical advancing from a southerly direction gradually—their action united becomes south-easterly (from the south-eastward), and as the one or other prevails, the wind blows more from one side of east or from the other.

So retentive of temperature are oceanic currents, that when H.M.S. 'Nile' was going from Halifax to Bermuda, in May, 1861, Admiral Milne found the temperature 70° at the bow,—while only 40° at the stern, as he entered the Gulf Stream.

Time is required to produce motion in the air—horizontally—*time* is indispensable for its gradual cessation from movement. *Statical* effects are noticed, at observatories—or by careful observers anywhere—hours, or days, before dynamical consequences occur.

The present daily forecasts, or premonitions of weather, are drawn up on the following arrangement. Districts are thus assumed:—

1. North Britain (including from the Moray Firth to the middle of Northumberland) along the coast.
2. Ireland—generally—around the coasts.
3. Central (Wales to the Solway) coast-wise.
4. East Coast (from Northumberland to the Thames).
5. South England (from the Thames round to Wales) by the coast.

As our space is very limited, and as *some* words are used in *different senses* by different persons, extreme care is taken in selecting those for such brief, general, and yet *sufficiently definite* sentences, as will suit the purpose.

Such words as are on *published* scales of force, or nature of wind and weather, are *generally* understood, and therefore used in preference to others.

In saying, on any day, what the probable character of the weather will be to-morrow, or the day after, at the foot of a table showing its observed nature that very morning,—a *limited* degree of information is offered, for about two days in advance, which is as far as may be yet trusted generally, on an average, though at times a longer premonition *might* be given, with sufficient accuracy to be of *occasional* use.

Minute, or special details, such as showers at particular places, or merely local squalls, are avoided; but the general or average characteristics, those expected to be principally prevalent (with but few exceptions) the following day, and the next after it, including the nights—not those of the weather actually *present*—are *cautiously* expressed, after careful consideration.

It may now be seen, after many months' trial, whether tolerably correct forecasts of ordinary weather can be formed here *sooner* than at distant, *isolated* places, where the published *general* Reports arrive a day or two later; and whether they are practically useful as conclusions available for the public.

Ordinary variations of *cloudiness*, or clear sky, or rain, of a *local*, or only temporary character, are not noticed usually.

A broad *general average*, or *prevalence*, is kept in view, referring to a day, or more, in *advance*, and to a *district*, rather than only to *one* time or place; should be remembered.

The great practical difficulty is in separating the effect, on the mind, of *present* states of air, weather, and clouds, from abstract considerations of what may be *expected* on the morrow, or next following day.

When in doubt, distrusting the indications, or inferences from them (duly considered on purely scientific principles, and checked by experience), the words "*Uncertain*," or "*Doubtful*," may be used, without hesitation.

As meteorological instruments usually foretell important changes by at least a day, or much longer, we have to consider what wind and weather may be expected from the morning observations, compared with those of the days immediately previous, as indicative of the morrow's weather, and of the day after, at *each* place—to take an *average* of those *expectations*, for each district, collectively, *in groups*; and then to estimate dynamical effects.

Outline maps, with movable windmarkers, and cyclone glasses or horns, are useful in forecasting weather: and full consideration should be given to the probable position, direction, extent, and degree of progress of that centrical area, or node, round which the principal currents usually circulate, or turn, as they meet and alter, combine with, or succeed one another.

Here dynamical considerations, with comprehensive comparisons of statical facts, are most important; and to treat them even approximately well, with such quick despatch as is requisite, demands aptitude and experience.

Those who are most concerned about approaching changes, who are going to sea, or on a journey, or a mere excursion; those who have gardening, agricultural, or other out-door pursuits in view—may often derive useful *cautionary* notices from these published *expectations* of weather—although (from the nature of such subjects) they can be but *scanty*, and imperfect, under present circumstances.

Objection has been taken to such forecasts, because they cannot be always exactly correct,—for all places in one district. It is, however, considered by most persons that general, comprehensive expressions, in aid of local observers, who can form independent judgments from the tables and *their own instruments*, respecting their immediate vicinity, *though not so well for distant places*, may be very useful, as well as interesting: while to an unprovided or otherwise uninformed person, an idea of the kind of weather thought *probable* cannot be otherwise than acceptable, provided that he is in no way *bound* to act in accordance with any such views, against his own judgment.

Like the storm signals, such notices should be merely *cautionary* —to denote anticipated disturbance *somewhere* over these islands,— without being in the least degree compulsory, or interfering arbitrarily with the movements of vessels or individuals.

Certain it is, that although our conclusions may be incorrect—our judgment erroneous—the laws of nature, and the signs afforded to man, are invariably true. Accurate interpretation is the real deficiency.

Seamen know well the marked characteristics of the two great divisions of wind, in all parts of the world, and do not care to calculate the *intermediate* changes, or combinations, to two or three points. They want to know the *quarter* whence a gale may be expected—whether northerly or southerly.

Every seaman will admit, that however useful, and therefore desirable, it would be to know exactly the *hour* of a storm's commencement—as our acquaintance with meteorology does not enable such times to be fixed—the next best thing is to have limits assigned for extra vigilance and due precaution, which limits *are* clearly stated, in all the printed popular instructions, to be from the *time* of hoisting the signal until *two* or *three days afterwards.*

But, say some, and justly—are ships to remain waiting to avoid a gale that, after all, may not happen? Are fishermen and coasters to wait idle and miss their opportunities? By no means. All that the cautionary signals imply is—"Look out." "Be on your guard." "Notice your glasses and the signs of the weather." "The atmosphere is much disturbed."

Many remarkable cases have occurred, which show the value of such warnings, or cautionary notices. Some have been published in newspapers, and need not be repeated; others have been communicated only by private letters; and one or two of them may be mentioned now as instances.

Admiral Evans wrote that on one evening, after a warning had been given at Liverpool, such a sudden (though brief) storm swept over the Mersey as would have done much harm, had not the harbour-master made due preparations, because of the signal.

A gentleman intending to cross the Irish Channel with an invalid lady, was warned to *wait*, though the weather *then* looked beautiful in London. That night it blew a "hurricane" on the west of Ireland, and a gale in the Irish Sea which lasted the following day.

Three ships-of-war were lying in Plymouth Sound, ready to sail for the West Indies. They waited two days, being cautioned, and then put to sea, in the intervening lull (as it happened), between two gales, the first of which was blowing while they were detained; and the second was a violent cyclone, that crossed France, the Netherlands, and Denmark, of which the northern semicircle swept our south and south-eastern coasts, but was *just avoided* by Her Majesty's squadron, or, rather, utilized by them, as they steered to the westward, nearly before its easterly wind, for some hours, and so passed out clear into the Atlantic.

Full warning was given along our eastern coasts, of that storm, in which the Prussian corvette 'Amazon' was totally lost; and so struck were the Prussian authorities by the facts of that period—taken in connection with other known cases—that an official application was soon afterwards made to the Board of Trade for information, with the view of enabling a similar system to be organized in the Baltic, communicating, if possible, with England.

On the 12th of November, 1861, a warning was sent to Yarmouth, in the afternoon. Being nearly dusk—and having then no night signals—nothing was done till next day, *after all* the fishing boats had gone far out to sea—having started very early in the morning. That afternoon there was a storm; and to save their own lives, the fishermen were obliged to cut from and abandon some 40,000*l.* worth of nets and gear. Night signals might have saved that loss, and the imminent risk of many lives. Such means are ready now.

On Friday, the 7th of March, the warning drum was hoisted all day at Plymouth. *Saturday* was so fine, in *appearance*, that the caution was not appreciated, and mackerel boats went to *a distance* —as usual. That afternoon *another* signal was made—South Cone under drum—to show that a heavy southerly gale was coming soon. It was a *beautiful* afternoon. No one anticipated the sequel, except those who, spider like, could "feel along the lines." Before midnight there was a storm—which lasted much of the next day. One of the boats was lost with eight men. "A more *dangerous* gale had not been known," was written by an officer of experience and good judgment, in his letter to a friend.

Perhaps sufficient thought has not always been given to the consideration of mere pecuniary *loss* by wear and tear, risk, accident, delay, and demurrage,—caused by a gale at sea;—balanced against the results of waiting for a tide or two, perhaps *once in two months*, when cautioned by a storm signal.

But be this as it may, with coasters, short traders, or even screw colliers—the question is entirely different with ordinary over-sea or foreign-going ships—especially when starting from a southern, or from a western port. To such vessels a gale in the Channel, or even during the first day or two after clearing the land, must always be very prejudicial. Officers and men are mutually strange. Things are not in their places; often not secured—and the ship perhaps is untried at sea. Of course, however, these remarks are inapplicable to fine first-class ships—and to powerful, well-managed steamers, independent of wind and weather, which start at fixed hours.

In conclusion, it appears that if due attention be paid on the coasts to cautionary signals—and at the central office—to the telegraphed reports—no very dangerous storm need be anticipated, without more or less notice of its approach being generally communicated around the British Islands; or to those coasts which are likely to be most affected by its greatest strength.

[R. F.]

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, April 4, 1862.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. Vice-President, in the Chair.

Mr. Commissioner Hill,

RECORDER OF BIRMINGHAM.

On the Post-office.

The first indication of a letter post is stated by German authors to have been found in the republic of the Hanse Towns as early as the thirteenth century. Shortly afterwards it was adopted by the Teutonic knights, who, like him of the Canterbury Tales, made war on the infidels in Lithuania and the adjacent districts eastward. We may next trace a line of posts in the Tyrol, laid down in the reign of the Emperor Maximilian, who naturally desired to connect Lombardy with his Austrian dominions. The merit of this project is given by historians to the Lombard princes of the house of Thurn and Taxis, as they were designated after their removal to Germany. Under the Emperor Charles V. they established a line from Vienna to Brussels, thus connecting the Empire with its outlying possessions in Flanders. Until the reign of Henry VIII., letters in England were conveyed by special messengers—sometimes on foot, sometimes on horseback—and, again, by carriers who, as we learn from Shakespeare, had no relays, the same horse either carrying its pack or drawing its cart from day to day. How far England was behind some other nations in very important social arrangements is indicated by the circumstance that long before we had a post for inland letters, the foreign merchants resident here enjoyed a stated interchange of correspondence with the Continent. This undertaking had its origin during the reign of Henry VIII., or prior to its commencement. In that of James I., from a complaint by the English merchants that the foreign postmaster delayed their letters, the King took the appointment into his own hands. It might be fairly presumed, even in the absence of direct evidence, that the postboy who carried despatches or accompanied the traveller during a stage of his journey to take back the post-horse, would be induced now and then to carry a private letter, so that in the course of years a usage profitable to all parties would grow up which would engraft a letter post (of a rude kind, perhaps,) on a system which the law intended only for despatches and for travellers. The hypothesis is confirmed by history, and this practice eventually became so extensive as to attract the attention of the Government. The years 1635 and 1637 witnessed the issue by Charles I. of his famous proclamation establishing our post-office on its present foundation, and directing that it should extend to Scotland and Ireland. The merit of this enterprise would seem to belong to Thomas Witherings, who was appointed first Inland Postmaster-General, he being already one of the masters of the foreign post. Soon afterwards the ever-memorable troubles of that unhappy reign broke out. The proclamations had claimed for the Crown a right of monopoly. It will create no surprise to learn that this prerogative was questioned by Parliament, nor that when the Houses became paramount over the King, they confirmed the monopoly, transferring it, however, to themselves, and closed a rival post-office, which, after Parliament had contested the King's right, had been set on foot by the city of London. As the conflict between the Parliament and the city (no such unequal combatants in those days as they would be in the present) would call for legal knowledge on the part of the Postmaster-General, his office was united to that of Attorney-General in the person of Mr. Prideaux. He claimed, probably with justice, the credit of having so improved and expanded the system as to make it not only self-supporting, but even to yield a profit. Taught by the success of the city enterprise he lowered the rates of postage and increased the frequency of despatches, thus evincing that he not only apprehended, but acted upon principles which, although they have ever since received lip-homage, have too often been disregarded in practice, official men preferring immediate petty gain to large profits in the not distant future. In spite of great deficiencies in the service, the revenue of the Post-office, says Lord Macaulay, was from the first increasing. In the year of the Restoration the net receipts were estimated at about 20,000*l.* At the close of the reign of Charles II., the net receipts were little short of 50,000*l.*, gross about 70,000*l.* But the proceeds came partly from the monopoly of post-horses, which was a considerable source of profit. About the year 1683, Robert Murray, an upholsterer of London, set up a penny post, which delivered letters and parcels six or eight times a day in the busy and crowded streets near the Exchange, and four times a day in the outskirts of the capital, the Royal Post-office having made no provision for correspondence between one part of London and another. This undertaking he assigned to William Dockra; but as soon as it became clear that the speculation would be lucrative, the Duke of York, on whom the whole net revenue of the Post-office had been settled by his brother, complained of the penny post as an infraction of his monopoly, and the courts of law decided in his favour. Murray's invention was thus wrested from Dockra, and its profits went to swell the income of the Duke. The fusion of the two systems was imperfect, the letter-carriers belonging to each being still confined to their former duties, so that they would often be found in the same street, and not seldom would meet at the same door; whereas if one had handed over his letters to the other, and had then retired, the work might have been performed just as well. It would hardly obtain belief, but for the notoriety of the fact, that this waste of labour, which to the public was a waste of money constantly increasing, survived to the year 1854. So dear, from long association, had this absurdity become to official men, that although it was attacked in the report of the Commissioners of Inquiry as early as 1829, and although its abolition was an object of earnest desire with the author of Penny Postage, who included the change in his proposal when he submitted it to the nation and to Parliament, yet it was not until fourteen years after he entered upon the administration of the Post-office that he was enabled to overcome the impediments which the usage of nearly two centuries had accumulated in the way of this obvious improvement. Until 1720 the lines of postal communication had been radial from each metropolis of the three kingdoms, the number of cross-posts being comparatively few. But in that year the well-known Ralph Allen, then at the head of the Bath Post-office, made a contract with the Government to establish a cross-post between the cities of Exeter and Chester, by way of Bristol, Gloucester, and Worcester, thus connecting the West of England with the mail route to Ireland, and giving postal intercommunication with many towns of importance. His terms were—to bear himself all the cost of the service, to pay a fixed rent, and to retain the surplus. This contract was renewed and extended from time to time, so as to include other branches of road, and terminated only with his death in 1764. Mr. Palmer (a great name in the annals of the Post-office) says the net profits of this contract to its holder amounted to 12,000*l.* a year, or in the total to rather more than half a million! This is the only instance of unclouded good fortune in the career of postal reformers. Eighteen years after the death of Allen appears John Palmer, proprietor and manager of the theatres of Bristol and Bath. The most obvious feature of his plans was the substitution of mail-coaches for boys on horseback, or for mail-carts, though he introduced many other improvements. He encountered much opposition, but the Minister, the younger Pitt, adopted his plans, and Mr. Palmer was employed to carry them into effect. His opponents bided their time, and two years afterwards, when his plans were yet only in partial operation, he had to encounter another struggle, and was defeated. The Minister, although he gave up the inventor, retained the invention. It had been agreed that Palmer was to have 1500*l.* a year, and 2½ per cent. upon all excess of revenue beyond a fixed sum. When ejected from the Post-office, not only did his salary terminate, but instead of his 2½ per cent. he was obliged to accept a life annuity of 3000*l.* This, even at that early date, was below the proceeds of his percentage, while the rapidly advancing revenue soon made the difference far wider. He appealed against this injustice, and eventually obtained a Parliamentary grant of 50,000*l.*; an amount, however, wholly inadequate to satisfy his claim.

To what foster-parents the young system was consigned on the loss of its father may be gathered from certain criticisms proffered by the gentlemen of the Post-office on Palmer's proposals even after they had had some brief and partial trial. Mr. Draper objects to mail coaches as running *too fast.* He declares that the post cannot travel with the expedition of chaises and diligences on account of business needing to be done at the Post-office in each town through which it passes—the fearful velocity which Mr. Draper deprecates rising possibly to six, or, in some cases, even to seven miles an hour. Be it remembered, however, that prior to Mr. Palmer's innovations, the average rate of the mail, including stoppages, was only three miles and a half per hour, which, in the opinion of the office, left nothing to be desired! In truth, speed appears to have been looked upon with great suspicion. Palmer had maintained that the post should outstrip all other conveyances; but the judicious Mr. Hodgson says, "I do not see why the post *should* be the swiftest conveyance. Personal conveyances, I apprehend, should be much more, and particularly with people travelling on business." Palmer found the net annual revenue of the Post-office about 150,000*l.* By the year 1814, in the face of an enhanced tariff, it had risen tenfold, namely, to 1,500,000*l.*—an augmentation chiefly attributable to the greater speed and punctuality secured by his improvements, though aided unquestionably by the national advancement in population and wealth. But thenceforward, until the epoch of penny postage, the impulse given to the increase of letters by the causes already pointed out, and indeed, by all others—especially by Macadam's admirable invention for bettering our roads, which enabled the mails to attain a rate, including stoppages, of ten miles an hour—proved to have become exhausted. For twenty years the number of letters passing through the Post-office remained stationary, amidst the rapid development of our manufactures and our commerce, the concentration of the national mind on the arts of peace, the consequent expansion of correspondence, and the innumerable facilities for its distribution which had been thus created and had been necessarily displayed before the slumberous eyes of the postal authorities. The augmentation of correspondence had broken through the monopoly of the Post-office, guarded although it was by high penalties, rigidly enforced. The mail coaches were outstripped by the improved stage coaches, which set the penal laws at defiance and carried an enormous number of contraband letters. But in spite of harsh laws harshly executed, of a straitened service, and of exorbitant rates, the Post-office still remained a popular and respected institution. At length, however, it discovered that it had traded too long on its reputation. Murmurs were heard among the people, and the discontented found a champion in the late Mr. Wallace, M.P. for Greenock, who frequently called the attention of the House to the preposterous rates of our postage. The desire for change grew with a steady growth.

In the year 1837 Mr. Rowland Hill, then filling the position of secretary to the Commissioners for managing the affairs of South Australia, a person scarcely known beyond the circle of his family and his friends, put forth a scheme of postal reform which, being named after its most striking feature, was called Penny Postage. He proposed the uniform rate of a penny for all letters under half an ounce, to whatever part of the United Kingdom they might be carried. Hitherto, if a letter consisted of two pieces of paper, however small, it was charged double postage. Treble letters paid treble postage, quadruple letters and all other multiples paid according to weight, but on a scale still increasing in proportion to distance. Taking all matters into account he struck an average, whence it appeared that by his plan the public might command for 1*d.* as much postal service as could be had on the then established rates for 9*d.* The people at large, the manufacturing and mercantile classes, the clergy—who witnessed every day the privations endured by the poor for want of a post-office within their means to use—all united in loud and earnest prayers to the Legislature to confer upon them the boon which had been held up before their eyes. On the other hand the heads of both the great parties in the State were impressed with the fiscal dangers of the proposed experiment; many believing that the project involved not merely an extinguishment of all revenue from letters, but, in addition, a ruinous subsidy to defray the expenses of the service. Not that Mr. Hill had left his plan unsupported by allegations of fact and by arguments, which in the event of the facts being sustained in proof, showed that the attractive results promised might be achieved without any ultimate diminution of the net revenue to a more serious extent than from 1,500,000*l.*, at which it then stood, to 1,200,000*l.* For a long series of years prior to 1837 the state of the Post-office had been a favourite subject of inquiry both by Royal Commissioners and Parliamentary committees, although the only very conspicuous product of these investigations was a formidable pile of blue books. To Mr. Hill, however, who had never entered a post-office in his life, these books were a mine of knowledge enabling him to frame a set of queries, to some of which he succeeded in procuring answers. But these were neither abundant nor accurate. For instance, it was essential that he should ascertain within certain limits the number of chargeable letters passing through the British post-offices in each year. No satisfactory information on this head was the Post-office able to afford. Upon the best data within his reach he computed the annual number at about 88½ millions; but after some time, having to a certain extent been able to correct his data, he revised his estimate, which he finally settled at 79½ millions. The Post-office estimated the number at 42 or 43 millions, then at 58 millions, next at 67 millions, and subsequently at 70 millions. But the Committee of 1838, after a most laborious and searching scrutiny, conducted with untiring zeal and with a degree of ability which cannot be too highly appreciated, arrived at the conclusion that the real number was 77½ millions. Eventually it was admitted by the Post-office itself to amount to 76 millions, which number was finally adopted. As the scheme of Penny Postage was based on the understanding that the ultimate loss to the revenue would not exceed 300,000*l.* per annum, a tolerably accurate computation of the real number of letters was one of the problems regarding the amount of increase in correspondence required to fulfil this condition, it being self-evident that if the Post-office had been correct in its estimate of 43 millions, the required multiplication must be very much larger than if the then existing number, as was now conceded, amounted to 76 millions. The Postmaster-General contended that a twelvefold increase would be necessary, while Mr. Hill calculated that a fivefold increase would suffice. To enable the Committee to form a judgment of the sources of increase, he began by adducing evidence to prove the vast multitude of contraband letters which, if postage could be reduced to a penny, there would be no temptation to transmit through a surreptitious medium. He then proceeded to show that the number of contraband letters, great as it was, must sink into insignificance when compared with that which the high tariff prevented from being written at all. It is believed that on the institution of Penny Postage contraband transmission ceased altogether, and yet the first year added but 93 millions of letters to the 76 millions of the old system, while some portion of this 93 millions must clearly be placed to the account of letters which, but for the reduction in postage, would not have come into existence. The augmentations of subsequent years have exceeded the limits of the wildest aspirations. But Mr. Hill did not depend altogether on the effect to be produced by swelling the grand total of letters. He laid great stress on diminishing to the Post-office the expense of the service: the cost per letter, not the total expense. *That* by the expected great increase of letters was sure to be enhanced. This important end he proposed to attain by the combination of two expedients. One was uniformity of postage. The other the relief of the office, by the employment of stamps, from the onerous duty of collecting postage. That both these changes must be highly economical is obvious. The taxation of letters, as it was then called, meaning thereby the task of ascertaining the amount of postage for each letter, and registering it upon the letter itself, was a slow and complex process, the greater part of which uniform postage did away with. But the principal item of cost had always been the delivery of letters from house to house. Under the old system the all but universal usage was for the sender to post his letter unpaid. The inevitable consumption of time thus caused in the collection of postage would be fresh in the memory of a large portion of the audience. Neither of the two branches of postal service thus cheapened presented any obstacle to the application of the principle of uniformity; but the third, namely, the journey which the letter made from the office of reception to that of destination, would appear at first sight of necessity to demand different rates of remuneration. No one was prepared to believe that the transit of a letter from St. Martin's-le-Grand to Barnet, the first stage on the road to Edinburgh, would cost practically the same as the whole journey, and yet Mr. Hill found on laborious investigation, that such was the fact—the sum for the whole journey only amounting to one-ninth of a farthing! Thus, it is clear that strict justice, to say nothing of convenience to the Post-office, which means economy, is more closely approached by making no variation of charge in respect of greater or smaller distances of conveyance than could be attained by acting on any differential scale imaginable, unless, indeed, we had a coinage descending far below farthings. By the result of this investigation, which I think I am justified in calling a discovery, all objections to adopting the principle of uniformity were fully answered, and Mr. Hill's case was complete. The Committee reported in his favour; the project was embodied in a Bill; passed the Legislature the next session, and, at the commencement of the year 1840, was carried into operation.

And here time warns me to break off my narrative. I will conclude with a brief comparison of postal affairs as they stood at the publication of Mr. Rowland Hill's plan with their present state; premising that the results which I have now to exhibit could not have been obtained without hearty and intelligent co-operation on the part of many gentlemen in the Post-office, who in the discharge of their respective duties have laboured with fidelity and devotion to promote the new system to the best of their ability. I cannot bring myself to pass their exertions by in utter silence, although I have no space for a more explicit notification of their services. As late as the year 1838, of the 2100 districts of the registrars of births, deaths, and marriages in England and Wales, about 400, then containing 1,500,000 inhabitants, were destitute of a single post-office. The average extent of each district was nearly 20 square miles. Several of these postal deserts were considerably larger than the county of Middlesex! The average population of the chief place of each district was 1400, and its average distance from the nearest post-office between four and five miles. Yet the ramifications of our postal system pervaded England far more thoroughly than they did the remaining divisions of the United Kingdom. Many other English and Welsh districts, though possessing post-offices, were yet so scantily supplied with them in proportion to their area, that in all probability 4,000,000 of the population of England and Wales, amounting at that date to one-quarter of the whole, must be held to have been destitute of postal accommodation. The great extent of the deficiency might be also gathered from the single fact that, while England and Wales contain about 11,000 parishes, the total number of their post-offices of all descriptions was only 3000. At the present day the comparison stands thus. The number in England and Wales has increased to 11,000, making it scarcely possible that any one of the registrars' districts should now remain unsupplied with a post-office. The offices in Scotland and Ireland have also received considerable augmentation, the number in the United Kingdom having risen from 4518 to 14.358. In 1838 Mr. Hill suggested the institution of day mails to facilitate the despatch of letters. Now a mail by day as well as by night is despatched to most of the towns in England and Ireland. A large number have the advantage of two day mails, and some have even three or more. In the metropolis under the old system there were but six deliveries *per diem*. There are now eleven. Several of the suburban districts have six deliveries a day. The measure which rendered these improvements practicable was the division of the metropolis into ten postal districts. It came into partial operation iu 1856, and is now almost complete. Each of these districts is treated as a separate post town. This arrangement has been followed by a vast and rapid increase of letters posted and delivered within the London district. During the five years preceding 1856 the average annual advance in the number of metropolitan letters was only 3¾ per cent. By 1858 it had risen to 12 per cent. These letters far exceed the total number from all sources home or foreign — delivered throughout the whole island of Great Britain (London included) in 1839. They amount to 68 millions, being only 8 millions less than the grand total of the United Kingdom for that year. Subdividing London has also effected a considerable acceleration in the first delivery of each day. Postage to and from the colonies and foreign parts has been much lowered while the transit has been greatly quickened. In 1839 the number of newspapers delivered in the United Kingdom was about 44½ millions. At that date every copy, by law, bore a stamp, which, however, had the advantage of franking it when sent by post. This privilege furnished a strong motive to proprietors and newsmen to distribute their impressions through that channel ; whereas at present, if copies are sent by any other means than through the post no expense is incurred in stamps. Nevertheless the number despatched through the Post-office last year reached 72½ millions.

The privilege of sending books and works of art by post at a reasonable charge is one of Mr. Hill's improvements. Medicines, watches, patterns, botanical specimens, seeds, and many other articles now pass largely through the office, to the convenience of all, but more especially of residents in the country : for it is not undervaluing the great benefits we have derived from the multiplication of railways to remark that they do not, and probably never can without the aid of the Post-office, distribute parcels even to all our towns much less to our villages and single houses; whereas the proportion of letters and other posted packets delivered by the letter carriers at the houses of those to whom they are addressed is now probably not less than 95 per cent. of the total number despatched. Of late years the rapid development of the book-post, which dates from 1848, has been remarkable. In 1854 the number of such packets was only 750,000, yet last year it had swollen to twelve millions.

An important branch of the service, largely developed since the institution of Penny Postage, is the system of money-orders. In 1839, the total number issued for the United Kingdom was 188,921, and the amount of money 313,124*l.* In 1861, the number reached 7,580,455, and the amount was 14,616,348*l.* And during that interval, although the prices of money-orders has been reduced to one-half, the growth of the system has been accompanied by a change most advantageous to the department. In the first years the service entailed a loss, which for 1847 amounted to 10,000*l.* In 1860, it brought a profit of 28,000*l.* Intercommunication of every kind told upon the increase of letters, and no doubt the last addition to the benefits conferred by the Post-office, namely, its Savings-banks, now rapidly spreading over the land, will be followed by similar consequences.

From the various causes thus co-operating to the increase of letters, I pass to effects. The number of chargeable letters delivered from the British offices in the last complete year before the reduction of postage was, as I have said, taken at 76 millions. The number in 1861 had risen to the stupendous amount of 593 millions, being nearly an eightfold multiplication of the former number. Let us study the proportion of letters to population at the two extremes. In 1839, it stood thus:—In England and Wales four letters per annnm to each individual, in Ireland one, in Scotland three, being an average of three to each person in the United Kingdom. In 1861, it had grown in England and Wales to twenty-four per head, in Ireland to nine, in Scotland to nineteen, being an average of twenty per head for the United Kingdom. This enormous increase might be placed in still another light. The total weight of letters, exclusive of newspapers and other matter, during the year 1839, was 758 tons. In 1861, it had risen to 4300 tons. The increase of the average daily mileage of the mails is very striking. It was estimated that in 1839 it did not exceed 54,000 miles per diem; whereas in 1861 it had risen to 149,000, being six times the circumference of the globe. The staff of officers of all ranks and both sexes constantly employed in the labours of the Post-office was in 1839, by rough estimate, about 8000. In 1861, it was by exact enumeration, 25,473. In addition to this force many persons are engaged for a portion of their time. The gross revenue in 1838 was 2,346,278*l.*; in 1861, upwards of three millions and a half. Hence it appears that notwithstanding the wonderful reduction, whereby the public now obtains far more of the article postage for the same price than it did formerly, yet that its expenditure in postage exceeds by more than one-half the amount so spent under the old exorbitant rates. The net revenue for the last year exceeds 1,500,000*l.*, so that, as regards both gross and net revenue, the facts have gone beyond Mr. Hill's original estimate. Nor is the promise for the future less brilliant than the experience of the past. Correspondence is still advancing by rapid strides. One feature in this vast accession cannot but give rise to sanguine expectations. Whatever the vicissitudes in our harvests—whatever the fluctuations of our commerce—whether we are in the enjoyment of peace or suffer the privations of war—each revolving year adds to the mass of our correspondence. The tide of our letters, like that from the Pontic to the Propontic Sea, knows no ebb. 1861, though by no means a year of unclouded prosperity, added an influx of twenty-nine millions—an addition even beyond the average of former years. Such, then, is the success of Penny Postage, and such are its prospects. Still, though no peril can be discerned, the instinctive feelings of mankind and the teachings of history warn us always to be prepared, if not for reverses, yet for some interruption in this course of unexampled prosperity. That, should checks occur, they will be transient and casual, we may reasonably expect, since correspondence does not flourish or fade with the changes of manners or fashions. Its growth is governed by causes not peculiar to any one country, but common to all. This is demonstrated by the rapid spread of the new system throughout the civilized world. One source of danger is dried up. The Post-office no longer assumes to be perfect, and its conductors have renounced their claims to infallibility. Suggested improvements, if they sustain the necessary test of rigid scrutiny, are welcomed, and not, as of old, frowned away. The department acts under the conviction that to thrive it must keep ahead of rivals—that it must discard the confidence heretofore placed in legal prohibitions, and seek continuance of success only by deserving it.

[M. D. H.]

GENERAL MONTHLY MEETING,

Monday, April 7, 1862.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Alfred Denison, Esq.
Alexander Staveley Hill, Esq. D.C.L.
William Martin, Esq.
Daniel George Rees, Esq.
Augustus Thorne, Esq. F.R.G.S.
John Tyndall, Esq. F.R.S. Prof. Nat. Phil. R.I.

were *elected* Members of the Royal Institution.

Jonathan Sparrow Crowley, Esq.
Major-General Charles James Green.
Rev. George Musgrave Musgrave, A.M.
A. C. Brisbane Neill, M.D.
Sir Joshua Rowe, C.B.
Edward Henry Sieveking, M.D.

were *admitted* Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same: viz.—

FROM

Actuaries, Institute of—Assurance Magazine, No. 47. 8vo. 1862.
Agricultural Society of England, Royal—Journal, No. 48. 8vo. 1862.

Asiatic Society, Royal—Journal, Vol. XIX. Part 3. 8vo. 1862
Astronomical Society, Royal—Monthly Notices, February 1862. 8vo.
Bakewell, F. C. Esq. (the Author)—On the Figure of the Earth in Relation to Centrifugal Force. (K 88) 8vo. 1862.
Bavarian Academy, Royal—Sitzungsberichte; 1861. Band II. Heft 1, 2. 8vo.
Bollaert, Wm. Esq. (the Translator)—Expedition of Pedro de Ursua and Lope de Aguirre in Search of Eldorados and Omagua. 8vo. 1861.
Calcutta Council of Education—Report on Public Instruction in Bengal. 8vo. 1859–60.
Chemical Society—Quarterly Journal, No. 59. 8vo. 1862.
Civil Engineers' Institution—Proceedings, March 1862. 8vo.
Editors—American Journal of Science, by B. Silliman, &c. for March 1862. 8vo.
Artizan for March 1862. 4to.
Athenæum for March 1862. 4to.
Chemical News for March 1862. 4to.
Engineer for March 1862. fol.
Horological Journal, No. 43, 44. 8vo. 1862.
Journal of Gas-Lighting for March 1862. 4to.
Mechanics' Magazine for March 1862. 8vo.
Medical Circular for March 1862. 8vo.
Practical Mechanics' Journal for March 1862. 4to.
Technologist for March 1862. 8vo.
Faraday, Professor, D.C.L. F.R.S.—Kais. Akademie der Wissenschaften, Wien: Sitzungsberichte, 1861. 5 Parts.
Jahrbuch der K. K. Central-Anstalt für Meteorologie und Erdmagnetismus. Band VIII. 4to. 1861.
Forbes, Alexander C. Esq.—Memoir of Sir John Forbes, M.D. 8vo. 1862.
Franklin Institute of Pennsylvania—Journal, Vol. XLIV. Nos. 2, 3. 8vo. 1862.
Geological Survey of India—Memoirs, Vol. III. Part 1. 8vo. 1861.
Gould, John, Esq. F.R.S. (the Author)—Introduction to the Birds of Australia. 8vo. 1848.
Introduction to the Trochilidæ, or Family of Humming-Birds. 8vo. 1861.
Horticultural Society, Royal—Proceedings, 1862. Nos. 3, 4. 8vo.
Linnean Society—Journal of Proceedings, No. 22. 8vo. 1862.
Transactions, Vol. XXIII. Part 2. 4to. 1861.
Locke, John, Esq. (the Author)—Discoveries in Central Australia. (K 88) 8vo. 1862.
Lubbock, John, Esq. F.R.S. M.R.I.—Notes on the Generative Organs, and on the Egg in the Annulosa. (From the Phil. Trans. R.S., 1862.) 4to.
Mackie, S. J. Esq. F.G.S. (the Editor)—The Geologist for March 1862. 8vo.
Madrid Royal Academy of Sciences—Memorias: Tomos III. IV. V. 4to. 1859–62.
Resumen de las Actas. 1853–9. 8vo.
Marlborough, The Duke of, M.R.I.—Catalogue Raisonné of the Pictures in Blenheim Palace, with Notes by G. Scharf: Part 1. 8vo. 1862.
Mechanical Engineers' Institution, Birmingham—Proceedings, July, Aug. and Nov. 1861.
Newton, Messrs.—London Journal (New Series) for March 1861. 8vo.
Nystrom, John W. C.E. (the Author)—Project of a New System of Arithmetic, to be called 'The Tonal System.' 8vo. Philadelphia, 1862.
Petermann, A. Esq. (the Editor)—Mittheilungen aus dem Gesammtgebiete der Geographie. 1862. No. 2. 4to.
Philadelphia Academy of Natural Sciences—Proceedings, 1861. 8vo.
Photographic Society—Journal, No. 119. 8vo. 1861.
Royal Society of London—Proceedings, No. 47. 8vo. 1860–1.
Scrope, G. Poulett, Esq. M.P. F.R.S. (the Author)—Volcanoes: the Character of their Phenomena, &c. 2nd edition. 8vo. 1862.
Statistical Society—Journal, Vol. XXV. Part 1. 8vo. 1861.
Treadwell, D. Esq. (the Author)—On the Construction of Improved Ordnance. (K 88) 8vo. 1862.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, Nov. und Dec. 4to. 1861.
Vienna, Imperial Geological Institute—Jahrbuch, 1861–2. 8vo.

Parnell, John, Esq. M.R.I.—A Bottle excavated near Old London Wall, Basinghall-Street.

WEEKLY EVENING MEETING,

Friday, April 11, 1862.

JOHN PETER GASSIOT, Esq. F.R.S. Vice-President, in the Chair.

Dr. A. W. HOFMANN, F.R.S. Pres. C.S.

On Mauve and Magenta.

THE fact of the beautiful colouring matters known by these fanciful terms being substances derived from coal, must, I presume, be familiar to every one of you. But there may be many unacquainted with the means by which this transformation is accomplished. It is to them that I address myself this evening.

Coal to become colour, has to pass through a series of stages of transition, each of which claims our attention for a moment. Briefly expressed, the aim of this address may be said to be, to show you the way from *coal* to *colour*. Now let me at once tell you this way is rather long; we may have to travel over country rough and intricate, and now and then to pass through territories which—I confess it with an appeal to the ladies—may not, perhaps, be altogether remarkable for their fragrance and sweetness. But on such occasions we shall accelerate our steps, and, on the whole, I venture to hope that we shall arrive at our journey's end without too much inconvenience.

Colour is intimately associated with light; without light there is no colour. This remark applies in a double sense to the colours derived from coal; for it is to the introduction of gas-light for illuminating our streets and houses, that we are indebted for the acquisition of these colours. This statement may appear strange, for nearly half a century has elapsed during which we have been in the possession of gas, whilst the transformation of coal into colouring matters has been achieved only recently under our own eyes. But you will immediately appreciate the truth of my assertion, if I tell you that these substances are obtained from a secondary product, generated in the manufacture of gas, a product long used for a variety of purposes, but which, only within the last few years, the researches of chemists have proved to be an inexhaustible mine of wealth and interest.

The starting-point then for the production of Mauve and Magenta, is the manufacture of coal-gas; but this is so well known as not to need any detailed description. Let me briefly remind you of the principal features of the distillation of coal, by directing your attention to the two large diagrams representing the "retort house" and the "condensers" of a gas-work. You observe how the coal is heated in stupendous retorts, five or seven of which are generally associated in one furnace. The gas ascends from these retorts in vertical tubes, the bent ends of which dip into a large horizontal pipe, partly filled with water, called the *hydraulic main*, a considerable amount of the oily and tarry substances generated with the gas being separated by the water. The gas, so far purified, passes on through the condensers—immense vertical iron pipes constantly cooled by a current of cold water which surrounds their external surface. In these condensers an additional quantity of oily matter is separated, which, together with the oily substances deposited by the gas during its passage through the hydraulic main, is collected in appropriately placed cisterns. The gas, having traversed the condensers, passes through a series of further purifications before it is delivered into the mains of our streets; but these, unconnected as they are with our subject, must no longer occupy our attention.

The distillation of coal being the fundamental operation in the manufacture of Mauve and Magenta, it is but fair that it should not remain without an experimental illustration. In this tubulated retort of hard glass, I am heating fragments of coal. The beak of the retort is inserted into a three-necked glass globe, the lower neck of which terminates in a tube, communicating with a glass vessel for the collection of the "oily products;" while the third neck is provided with a delivery-tube for the discharge of the gas, which finds its way into a glass gas-holder. The coal has been heated only for a few minutes, and the gas already begins to be freely evolved; already I may light it at the orifice of the delivery-tube, which, for this purpose, I have removed from the gas-holder; already it burns with the characteristic luminous flame of coal-gas. In the meanwhile, you observe, a considerable quantity of the "oily products" has accumulated in the receiver. Their formation continues as long as the gas is evolved. Ultimately the coal is entirely resolved into *gas* and *oily products*, a non-volatile residue, the *coke*, remaining behind in the retort.

It is in the oily products, the so-called "*coal-tar-oil*," that our interest is centred. To my mind this coal-tar-oil * is one of the most wonderful productions in the whole range of chemistry. That may be rather a one-sided view, but having in younger years spent much time in the investigation of this substance, I have acquired quite an affection for it. Nor can you fail to appreciate the interest which coal-tar presents to the chemist when you look at the diagram in which I have endeavoured to arrange synoptically the various substances which have been eliminated from it.

* A large specimen of coal-tar-oil was here exhibited.

PRODUCTS OF THE DESTRUCTIVE DISTILLATION OF COAL.

Name.	Formula.	Boiling Points.
Hydrogen	$H\ H$	
Marsh gas, or Hydride of methyl	$C\ H_3,\ H$	
Hydride of Hexyl	$C_6\ H_{13},\ H$	
Hydride of Octyl	$C_8\ H_{17},\ H$	
Hydride of Decyl	$C_{10}\ H_{21},\ H$	
Olefiant gas, or Ethylene	$C_2\ H_4$	
Propylene, or Tetrylene	$C_3\ H_6$	
Caproylene, or Hexylene	$C_6\ H_{12}$	55°
Œnanthylene, or Heptylene	$C_7\ H_{14}$	
Paraffin	$C_n\ H_{2n}$ (?)	
Acetylene	$C_2\ H_2$	
Benzol	$C_6\ H_6$	84°
Parabenzol	$C_6\ H_6$	
Toluol	$C_7\ H_8$	114°
Xylol	$C_8\ H_{10}$	126°
Cumol	$C_9\ H_{12}$	150°
Cymol	$C_{10}\ H_{14}$	175°
Naphtalin	$C_{10}\ H_8$	212°
Paranaphtalin, or Anthracen	$C_{14}\ H_{10}$	
Chrysen	$C^{12}\ H_4$ (?)	
Pyren	$C_{30}\ H_4$	
Eupion	(?)	
Water	$\left.\begin{matrix}H\\H\end{matrix}\right\}O$	100°
Hydrosulphuric acid	$\left.\begin{matrix}H\\H\end{matrix}\right\}S$	
Hydrosulphocyanic acid	$\left.\begin{matrix}H\\(C\ N)\end{matrix}\right\}S$	
Carbonic oxide	$C\ O$	
Carbonic anhydride	$C\ O_2$	
Disulphide of carbon	$C\ S_2$	47°
Sulphurous anhydride	$S\ O_2$	
Acetic acid	$\left.\begin{matrix}H\\(C_2\ H_3\ O)\end{matrix}\right\}O$	120°
Phenylic acid, or alcohol, *Phenol*	$\left.\begin{matrix}H\\(C_6\ H_5)\end{matrix}\right\}O$	188°

PRODUCTS OF THE DESTRUCTIVE DISTILLATION OF COAL—*continued.*

Name.	Formula.	Boiling Points.
Cresylic acid, or alcohol, Cresol .	$\left.\begin{matrix} H \\ (C_7 H_7) \end{matrix}\right\} O$.	203°
Phlorylic acid, or alcohol, Phlorol	$\left.\begin{matrix} H \\ (C_8 H_9) \end{matrix}\right\} O$	
Rosolic acid		
Brunolic acid	(?)	
Ammonia	$\left.\begin{matrix} H \\ H \\ H \end{matrix}\right\} N$	
Aniline	$\left.\begin{matrix} C_6 H_5 \\ H \\ H \end{matrix}\right\} N$.	182°
Cespitine	$(C_5 H_{13})'''N$.	96°
Pyridine	$(C_5 H_5)'''N$.	115°
Picoline	$(C_6 H_7)'''N$.	134°
Lutidine	$(C_7 H_9)'''N$.	154°
Collidine	$(C_8 H_{11})'''N$.	170°
Parvoline	$(C_9 H_{13})'''N$.	188°
Coridine	$(C_{10} H_{15})'''N$.	211°
Rubidine	$(C_{11} H_{17})'''N$.	230°
Viridine	$(C_{12} H_{19})'''N$.	251°
Chinoline, or Leucoline	$C_9 H_7 N$ . .	235°
Lepidine	$C_{10} H_9 N$ . .	260°
Cryptidine	$C_{11} H_{11} N$	
Pyrrol	$C_4 H_5 N$(?)	
Hydrocyanic acid	H C N	

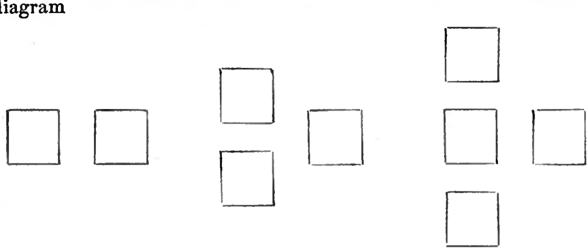
This is rather a formidable list of compounds; their names, too, are not always remarkable for smoothness and melodious character, although, I should not omit to state, they are tame and domestic when compared with some of the terms which chemists of late have been under the painful necessity of inventing and inflicting. You need not be afraid, however, that I shall trouble you with many details about these substances. Most of them, though highly interesting for more than one reason, more especially when considered from a purely scientific point of view, are of no importance for our present subject, and need not therefore specially be noticed. In fact, the only coal-derivatives which, in connection with Mauve and Magenta, claim our attention, are *Benzol*, *Phenol*, and *Aniline*; those certainly we must by-and-by examine somewhat more in detail.

But before doing so, you legitimately expect that I should endeavour to give you some idea of the nature of the process, in which this endless variety of compounds is generated from coal. Were I to tell you simply that coal consists of *Carbon*, *Hydrogen*, *Nitrogen*, *Oxygen*, and *Sulphur*, not to mention the ash which is left after combustion, and that you may therefore look upon coal as a sort of magazine of these several elements, capable, under the influence of heat, of associating in an infinity of forms and proportions, you would have learnt comparatively little. Let me attempt to convey to you a somewhat more precise idea of the processes involved in the distillation of coal. For this purpose you must allow me to remind you of some of the general results elaborated by the researches of chemists during the last ten years, which, at the first glance, appear but little connected with Mauve and Magenta.

The infinite number of substances, mineral, vegetal, or animal, which form our planet, variously as they are composed, may be referred,—chemists now pretty generally agree,—to a comparatively small number of *types of construction*. Opinions are divided respecting the actual number of these types, and even the choice of typical bodies is still a subject of discussion among chemists. But whatever the special views of particular schools may be, the number of types is always small, and among them almost invariably figure *Hydrogen*, *Water*, and *Ammonia*. The comprehension of the meaning attached by chemists to the term *types* may perhaps be facilitated to you by a glance at three models which I have had constructed for this purpose, and which for the sake of convenience I may be allowed to designate as *type-moulds*.*

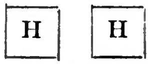
Chemists assume that the smallest particle of hydrogen, which

* These type-moulds consisted essentially of wire frames, presenting the out lines of cubes, associated, two, three, or four of them, in the manner indicated in the diagram

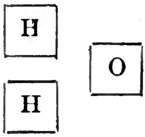


and capable of receiving zinc-cubes variously painted and marked, representing elementary and compound atoms.

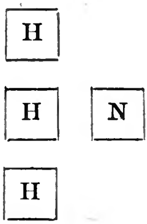
exists in the free state, or, to use the chemical phrase, the molecule of hydrogen, consists of two atoms of hydrogen. The first of our type-moulds then, charged as it is with *one* atom (one volume) of hydrogen, associated with *another* atom (one volume) of hydrogen, represents the *molecule of hydrogen.*



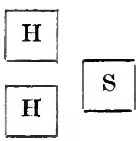
In water, as you know, we have *two* atoms (two volumes) of hydrogen, associated with *one* atom (one volume) of oxygen.* You are reminded of this fact by our second type-mould, which represents the *molecule of water.*

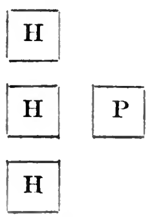


In ammonia, lastly, you have *three* atoms (three volumes) of hydrogen, united with *one* atom (one volume) of nitrogen, a form of construction which is recorded in our third type-mould representing the *molecule of ammonia.*



Nothing is easier now than to trace the derivation of other substances from hydrogen, from water, from ammonia. Let me remove from our three type-moulds one atom respectively of hydrogen, oxygen, and nitrogen, and fill the places thus vacated with atoms of chlorine, sulphur, and phosphorus, and I have, without giving you the slightest inconvenience, converted hydrogen into *hydrochloric acid*, water into *sulphuretted*, and ammonia into *phosphoretted hydrogen.*



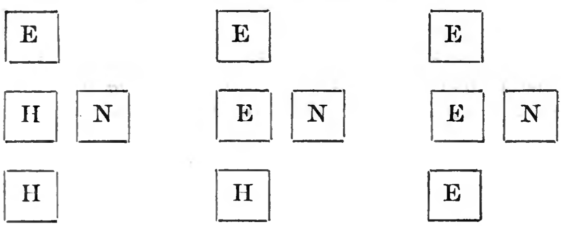


* Equivalents used: H=1; O=16; S=32; C=12; N=14; Cl=33·5; &c.

You observe the molecules of hydrochloric acid, of sulphuretted and of phosphoretted hydrogen respectively contain the same number of atoms which are present in the molecules of hydrogen, of water, and of ammonia. We have thus indicated that hydrochloric acid is constructed upon the hydrogen-type, sulphuretted hydrogen upon the water-type, phosphoretted hydrogen, lastly, upon the type of ammonia. The three bodies just considered were formed by the insertion of *elementary* atoms; but our type-moulds receive *compound* atoms with the same facility. Let me take as an illustration the compound atom *ethyl*, consisting of two atoms of carbon and five of hydrogen, ($C_2 H_5 = E$), which is familiar to the members of the Royal Institution. By inserting *one* or *two* ethyl-atoms into the hydrogen-mould I generate the molecules of *ethylated hydrogen*, or *ethylated ethyl* (free *ethyl*).

In a similar manner, by introducing either *one* or *two* ethyl-atoms into water, I convert the molecule of water into the molecules of the two *ethylated waters*, *alcohol* and *ether*.

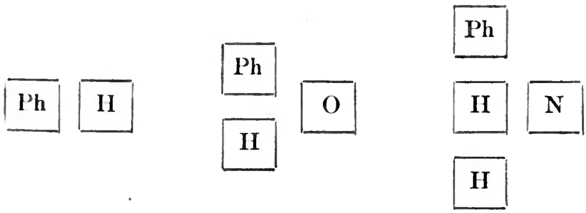
Displace, lastly, *one*, *two*, or *three* hydrogen-atoms in ammonia, by *one*, *two*, or *three* ethyl-atoms, and you give rise to the formation of the molecules of the three *ethylated ammonias*,



better known as *ethylamine*, *diethylamine*, and *triethylamine*.

At the risk of exhausting your patience, I repeat some of these changes with another compound atom of a composition differing from that of ethyl. These mauve-coloured cubes may represent a compound atom, containing six atoms of carbon and five of hydrogen ($C_6 H_5 = Ph$), to which chemists have given the name of *phenyl*.

Charge each of our type-moulds with an atom of phenyl, and you accomplish the construction of *phenylated hydrogen*, *phenylated water*, and *phenylated ammonia*,



substances better known as *benzol*, *phenol*, and *aniline;** and the existence of which in coal-tar-oil I have already pointed out to you.

But it is time for us to return to the point from which we started. What has the recognition of our types to do with the distillation of coal? In what manner do they explain the formation of the variety of substances generated in this process. In coal we have the elements of the three types of matter, and we find that hydrogen, water, and ammonia are in fact produced to a very appreciable extent during its distillation. The quantity of free hydrogen is generally small; moreover, mixed as it is with the carbonetted hydrogens of coal gas, its presence among the products of distillation of coal is not easily demonstrated by experiment. Water and ammonia, on the other hand, are abundantly generated, and nothing is easier than to exhibit their production. In fact, the coal-tar-oil which we have produced in our distillation-experiment, is covered, as you observe, with a layer of water, and the application of test-papers to the latter shows that it contains a large amount of ammonia. Now consider that our types are generated from coal in the presence of large quantities of carbon and hydrogen, two elements which, in proportions varying to an almost unlimited extent, may aggregate under the influence of heat to compound atoms similar to ethyl and phenyl; remember, moreover, that these atoms are capable of displacing, partly or entirely, the hydrogen of our types, and you will realize without difficulty the number of compounds which may be formed by the distillation of coal; I say which may be formed, for the diagram which I have exhibited to you enumerates only the bodies which have actually been obtained; but every day brings forth new substances. It is obvious that the nature of the compound atoms generated must, in a measure, depend upon the composition of the coal distilled. The composition of coal, however, varies between very considerable limits. In the subjoined diagram I give you a synopsis of the results obtained in the analysis of several specimens of coal.

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* Large specimens of these three substances were exhibited upon the table.

ANALYSIS OF DIFFERENT COALS.

Locality of Coal.	100 PARTS OF DRY COAL						
	Contain					Leave	
	Carbon.	Hydrogen.	Nitrogen.	Sulphur.	Oxygen.	Ash.	Coke.
Anthracite, Wales . . .	91·44	3·36	0·21	0·79	2 58	1·52	92·20
" " . . .	90·39	3·28	0·83	0·91	2·97	1·61	92 10
Caking Coal, Newcastle .	81·41	5·83	2·05	0·75	7·90	2·07	66·70
Cannel Coal, Wigan . .	80·07	5·53	2·12	1·50	8·09	2·70	60·36
Coal, Wolverhampton . .	78·57	5·29	1·84	0·39	12·88	10·30	57·21
Wallsend, Elgin	76·09	5·22	1·41	1·53	5·05	10·70	58·40
St. Helen's, Lancashire .	75·80	5·21	1·92	0·90	11·89	5·17	65·50
Methill Brown Coal . .	65·96	7·78	0·96	0·75	9·23	15·32	
Bohemian Brown Coal . .	55·59	4·16	19·06			21·19	

A glance at this diagram shows you that the carbon in the several specimens varies by more than 30 per cent., being 91·4 in Welsh anthracite and 55·5 in Bohemian brown coal. Similar, though less marked, discrepancies are perceptiblé in the other constituents. If you recollect, in addition, that the nature of the compound atoms generated in the distillation of coal must be influenced, moreover, by the temperature, which again oscillates between limits widely apart, you cannot fail to perceive that the destructive distillation of coal must be an almost inexhaustible source of new compounds.

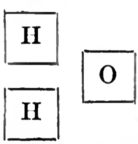
The separation of the individual substances from the complex mixture called coal-tar-oil appears, at the first glance, to present almost insurmountable obstacles. But the principles made use of for this purpose are very simple. The individual compounds contained in coal-tar-oil may be separated in a great measure by distillation, their boiling points varying, as may be seen by a glance at the diagram, to a considerable extent. But additional means of purification offer themselves in the different deportment which these substances exhibit under the influence of chemical agents. I could not perhaps, in this respect, bring under your notice a more instructive illustration than the behaviour with acids and bases of the three coal-tar-oil-constituents, repeatedly quoted. Benzol, phenol, and aniline may thus easily be separated. To demonstrate this point experimentally, two glass cylinders have been half-filled with benzol, two others with phenol, and two further ones with aniline; a solution of litmus having, moreover, been added, each of the three substances is treated in one cylinder with acid, in the other with alkali. In the case of benzol, you observe, the indifferent hydrocarbon, insoluble both in acid and alkali, floating colourless upon the coloured liquid; phenol, being an acid water-derivative, is not acted upon by the acid, but readily dissolves in the alkali; aniline, lastly, being a well-defined ammonia-derivative, exhibits the converse deportment, resisting the action of the alkali and forming a homogeneous solution with the acid.

Each of the three coal-tar-oil-constituents which I have mentioned, and of which you have characteristic specimens upon the lecture-table, has received important applications in the arts and manufactures. Benzol is the most convenient solvent for caoutchouc; as an agent for removing oil and grease it has become an ordinary household article; phenol, when treated with nitric acid, yields us a beautiful yellow dye, called by chemists carbazotic acid; but the practical interest attached to phenol you will more immediately appreciate if I tell you that this compound presents the greatest analogy with creosote, a substance, I am afraid, but too well known to most of us,—a considerable portion of the creosote of commerce being in fact simply phenol; aniline, lastly, is the source of Mauve and Magenta, and must therefore claim our attention more particularly this evening.

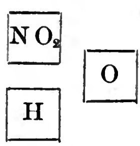
The amount of aniline which exists in coal-tar is very limited; a preparation from this source upon a sufficiently large scale could never be attempted. Fortunately, chemists are in the possession of a series of processes by which aniline may be produced in any quantity. Benzol, the phenylated hydrogen, may readily be converted into aniline, the phenylated ammonia. Let us examine this transformation experimentally.

Benzol is readily attacked by fuming nitric acid; it dissolves in it, producing a liquid of a deep-red colour. On addition of water this liquid deposits a heavy yellow oil, collecting at the bottom of the cylinder, perfectly different from benzol, which floats on the surface of the water. The reaction will be intelligible to you, if I remind you that nitric acid when referred to our types, must be viewed as a water-derivative; it is water in which, for one of the elementary hydrogen-atoms, there has been substituted a compound atom, consisting of nitrogen and oxygen.

Water.



Nitric Acid.



Modern chemistry, you observe, returns to the conceptions of former ages, which in the name *aqua fortis* appear to have anticipated in a measure our present notions.

When nitric acid acts on benzol, an interchange takes place between the elementary atom of the latter and the compound atom of the former, *nitrobenzol*, the heavy yellow liquid, which we have produced, and water being formed:—

Benzol. Nitric Acid. Nitrobenzol. Water.

$$(C_6\,H_5)\,H + \left.\begin{matrix}NO_2\\H\end{matrix}\right\}O = (C_6\,H_5)\,(NO_2) + \left.\begin{matrix}H\\H\end{matrix}\right\}O$$

The transformation of benzol into nitrobenzol, discovered by Mitscherlich, is only a preparatory operation for the production of aniline. The method of converting nitrobenzol into aniline was discovered by Zinin. It consists in submitting nitrobenzol to the action of nascent hydrogen. Under the influence of this agent, the compound atom $N\,O_2$, which in nitrobenzol is associated with phenyl, is decomposed; its oxygen is converted into water, the residue of nitrogen and phenyl assimilating the necessary quantity of hydrogen to form phenylated ammonia, or aniline.

$$\underset{\text{Nitrobenzol.}}{(C_6H_5)\,N\,O\,O} + \underset{\text{Hydrogen.}}{H\,H + H\,H + H\,H} = \underset{\text{Water.}}{\left.\begin{matrix}H\\H\end{matrix}\right\}O + \left.\begin{matrix}H\\H\end{matrix}\right\}O} + \underset{\text{Aniline.}}{\left.\begin{matrix}(C_6H_5)\\H\\H\end{matrix}\right\}N}$$

The hydrogen necessary for this transformation may be furnished by numerous processes. The most convenient method for our purpose consists in submitting nitrobenzol to the action of metallic iron and acetic acid, a process first proposed by M. Béchamp. I mix the three bodies in a glass retort, and on application of a gentle heat you observe how immediately a most powerful reaction manifests itself. Let us hasten to connect the retort with a condenser; I have removed the gas-burner from the retort; nevertheless the reaction continues, and a considerable quantity of water, covering an oily layer, has already accumulated in the receiver. This oily liquid is *aniline.* We recognize it at once by its peculiar deportment with a solution of chloride of lime. On pouring a single drop of our distillate into this beaker which contains a solution of chloride of lime, a splendid purple cloud is almost instantaneously diffused throughout the liquid. You perceive we are approaching our subject. The beautiful colour which aniline strikes with a solution of chloride of lime has been long known. A solution of bleaching powder has always been used as a test for aniline; indeed it was by this colour-reaction that the presence of our compound in coal-tar-oil was first pointed out, a fact recorded in the name *Kyanol* (blue oil), originally given to aniline prepared from coal-tar. Several other oxidizing agents, chromic acid for instance, were likewise known to produce coloured compounds from aniline; but all the colours thus obtained were of an highly ephemeral character. Observe how the purple cloud which I produced by means of chloride of lime has rapidly changed to a dingy reddish precipitate. It was Mr. W. Perkin who had first the happy idea of investigating the circumstances under which this beautiful purple might be prepared in a form permanent and applicable for the purposes of the dyer. He succeeded in isolating this colour by submitting, under appropriately selected circumstances, aniline to the action of bichromate of potassium and sulphuric acid.

Here then you have, step by step, the development of this new and important branch of chemical industry.

Through the kindness of my friend, Mr. Perkin, I am enabled to exhibit to you magnificent specimens of his aniline purple, or Mauve, in the dry state and in solution. This brown lump, with the remarkable coppery lustre, is Mauve in the solid state; its extraordinary tinctorial powers will be appreciated, if I tell you that this beautiful violet-coloured solution contains not more than $\frac{1}{10}$ of a grain of Mauve in one gallon of alcohol; you will also understand the considerable commercial value of this substance. Weight for weight, I am told by Mr. Perkin, this colouring matter, when pure, is sold at the price of metallic platinum.

Very little is known regarding the chemical nature of Mauve; its composition is not yet made out, and as a matter of course the process by which it is formed from aniline remains as yet perfectly unexplained.

Magenta is one of the fancy names given to the splendid crimson, which is likewise generated from aniline by the action of oxidizing agents. This substance was first observed in purely scientific researches, and more especially in the action of tetrachloride of carbon upon aniline. To a French chemist, M. Verguin, the merit is due of having for the first time obtained this substance on a larger scale; he produced it by the action of tetrachloride of tin on aniline. Numerous other processes were subsequently suggested, among which treatment of aniline with chloride or nitrate of mercury, with arsenic acid, and many other substances may be mentioned. Magenta, often called fuchsine, roseine, &c., soon became an article of large consumption. A great impetus to this new branch of industry was given in France by Messrs. Renard and Franc, who were the first to manufacture the new article on a commercial scale; in this country, very soon afterwards, Messrs. Simpson, Maule, and Nicholson engaged with great spirit in the manufacture of this splendid dye, the production of which has attained already colossal proportions. To Mr. E. C. Nicholson more especially belongs the credit of having developed this new industry to an unprecedented degree of perfection.

Before proceeding, however, let me show you the formation of Magenta by experiment. Among the many processes which I might adopt for this purpose I select the action of corrosive sublimate upon aniline, not because I consider this process superior to the others,—it is, in fact, inferior to many,—but because it is, perhaps, the best adapted for a lecture-experiment. This white powder is chloride of mercury (corrosive sublimate); a small portion of this salt I mix in a test-tube with perfectly colourless aniline. Let us stir the mixture with a glass rod until it is converted into a perfectly homogeneous liquid paste. This paste is still colourless; but on gently heating it by a gas-burner, it instantaneously assumes a splendid crimson of the greatest intensity, a single drop of the liquid being capable of deeply colouring a large beaker filled with alcohol.

In all the processes which convert aniline into colouring matters, a considerable number of secondary products are generated, which it is rather difficult to separate from the principal product of the reaction. These difficulties have been most perfectly overcome by Mr. Nicholson, who has succeeded in obtaining Magenta in a state of absolute purity. Chemists have thus been enabled to analyze this substance, and to lift, at all events, the corner of the veil which still covers the mysterious formation of the coloured derivatives of aniline.

In the pure state Magenta is a fine crystalline, and, remarkably enough, perfectly colourless, or only slightly tinted body, which is represented by the formula—

$$C_{20}\,H_{21}\,N_3\,O = C_{20}\,H_{19}\,N_3,\,H_2\,O.$$

Rosaniline (this is the name by which chemists designate the colourless body) is a base, or ammonia-derivative, which forms a series of splendid salts. With hydrochloric acid, for instance, it produces a beautifully crystalline salt of the formula—

$$C_{20}\,H_{19}\,N_3,\,H\,Cl.$$

It is in the state of saline combination that rosaniline acts as a crimson dye. Into this shallow porcelain dish I have thrown a few crystals of rosaniline, which at a distance you scarcely perceive; I now pour upon these crystals a small quantity of acetic acid, when on gently heating the dish the crimson colour instantaneously appears. But it is only *in solution* that even the salts of rosaniline are crimson-coloured; on slowly *evaporating* their solution, the red colour entirely *vanishes*, and a splendid green crystalline substance remains, presenting in an extraordinary manner the beautiful metal-lustre which distinguishes the wings of the rose-beetle. Together with all the products involved in the manufacture of aniline and aniline-dyes, my friend Mr. Nicholson has placed before you the finest series of rosaniline-salts which has ever been produced; and not content with this display, he was kind enough to send us a specimen of acetate of rosaniline, such as no mortal eye has ever seen before. The specimen may be literally called the *Crown of Magenta.** Crowns are always expensive articles, and often are the cost and trouble of getting them greater than their actual value. This remark applies in a measure to the Crown of Magenta. For the benefit of those who are fond of big figures—and who is not a little afflicted with this weakness?—I may state that the crown was grown in a vessel containing not less than 8000*l.* worth of Magenta, the crown itself being worth upwards of 100*l.*

Having now explained the several stages of transition through which coal has to pass before it becomes either Mauve or Magenta, it may be of some interest to you to know the proportion which the finished dye bears to the coal from which it is derived. A set of specimens for which I am likewise indebted to Mr. Nicholson, is most instructive in this respect. Observe, it commences with a large mass of coal, weighing not less than 100 lbs.; the bottles which follow contain the coal-tar-oil, naphtha, benzol, nitrobenzol, and aniline,

* An immense aggregate of sparkling green octohedral crystals,—some of them as much as an inch in diameter,—deposited upon a large wire frame having the shape of an elegant crown, was here exhibited.

obtainable in succession from 100 lbs. of coal; remark how they gradually diminish in size, and how small, I might almost say insignificant, appears the bulk of Magenta finally obtained. But compare the bulk of wool which this minute quantity will dye. It approximates to the bulk of coal with which we started. This comparison evinces perhaps sufficiently the extraordinary tinctorial power which this class of dyes possesses; but a very simple experiment may possibly convey to you this idea even in a more impressive manner. The white paper which covers this large frame has been dusted over with a minute quantity of Mauve; a second one is treated in a similar manner with Magenta. The quantity of colouring matter is so small, that the paper has retained its original white colour, but observe how it changes, when I dash a beaker full of spirit against these squares; immediately the lovely purple of Mauve is developed upon one of them, whilst the other one exhibits the dazzling crimson of Magenta.

But let us now proceed to illustrate the mode of dyeing. For this purpose I introduce silk and wool, both unspun and woven, in succession, into solutions of Mauve, of Magenta, and lastly, of a splendid new Purple, lately discovered by Mr. Nicholson. Observe the extraordinary facility with which the coal-tar-colours are fixed both on wool and silk. These materials require no previous preparation, being dyed, in fact, simply by dipping, without the aid of any mordant. Silk and wool are animal substances; vegetal materials, such as cotton and linen, unless previously submitted to a special treatment, are scarcely affected by these dyes. This fact admits of being beautifully illustrated by dyeing linen fabrics on which ornaments have been embroidered in silk ribbon. The articles, when coming out of the bath, appear uniformly dyed; but by washing, first in pure water and then in dilute ammonia, the colour rapidly vanishes from the linen ground, leaving the silk embroidery in brilliant colours. This extraordinary predilection of the aniline-colours for animal substances is, moreover, strikingly illustrated by the condition of my hands, which by this time have acquired a thoroughly Magentic appearance. Fortunately, the coal-tar-colours are unable to resist the action of chloride of lime, and I have therefore only to immerse my hands for a moment into a solution of bleaching powder.

Already the colour on my hand, you observe, has gone, but with the colour, I am afraid, my time too. Let me endeavour to bring this lecture to a conclusion. I have fulfilled in a measure the promise which I gave you at the commencement of this discourse. We have crossed together the extensive field which stretches between *coal* and *colour*. I am impressed, deeply impressed with the clumsiness of my performance as your guide, but I hope that the interest attached to the territory we have explored may, to some extent at all events, have indemnified you for the imperfection of my explanations, and that you leave the Royal Institution this evening with the kind of feeling every one of us has more than once experienced after travelling in similar company over a beautiful country—the guide is forgotten, but the impression of the scenery remains.

Having gone thus far, you may think that it is fully time for me to make my bow. But I venture, even at this late hour, to dwell for a moment on the *moral* of the story which I have told you, though you may feel disposed to consider this story rather a highly coloured one.

The material which I had to condense, I might almost say to force, into the short space of an hour, has been overwhelming; and whilst explaining the formation of the various substances which I had to describe, whilst illustrating their properties by experiment, I have scarcely had time to glance at the history of our subject. This history is not without interest. You readily perceive that a branch of industry like the one I have endeavoured to sketch could not possibly have risen like Minerva from the head of Jupiter—a sudden inspiration happily realized. The time, the toil, the thought of a host of inquirers were necessary to accomplish so remarkable an achievement. You cannot expect me at this late hour to examine minutely into this part of the subject, but I must not take leave of you without alluding to some facts which cannot fail to rivet the lively interest of the Members of this Institution. Let me tell you then that Mauve and Magenta are essentially Royal Institution colours; the foundation of this new industry was laid in Albemarle Street. Benzol, which I have so repeatedly mentioned,—benzol, which may be looked upon as the raw material, capable, under the influence of chemical agents, of assuming such wonderful shapes,—benzol is the discovery of our great master, may I not add of our kind friend, Mr. Faraday. This volume, 'The Philosophical Transactions for 1825,' contains the description of his experiments. In 1825, thirty-seven years ago, the laboratory of the Royal Institution witnessed the birth of this remarkable body. Yesterday, under the auspices of Mr. Anderson, I invaded the same laboratory, a diligent search was made, and in my hand I hold the trophies of our expedition, the original specimens of benzol which Mr. Faraday prepared. In thus reminding you of one of the early labours of Mr. Faraday,—which, owing to the number and vastness of his subsequent discoveries, appears almost to have escaped from his memory like a tradition of years gone by,—I have opened a glorious page in the glorious history of the Royal Institution. Benzol has furnished us Mauve and Magenta, but it has done much more than this. Ever since chemistry became endowed with this wonderful body, benzol has been the carrier of many of the leading ideas in our science. In the hands of Mitscherlich, Zinin, Gerhardt and Laurent, in the hands of Charles Mansfield—never to be forgotten by his friends—and many others, benzol has been a powerful lever for the advancement of chemical science. Benzol and its derivatives form one of the most interesting chapters in organic chemistry, the progress of which is intimately allied with the history of this compound.

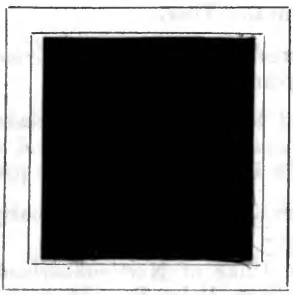
But what has the history of benzol to do with the moral of Mauve and Magenta? Well, ladies and gentlemen, ask Mr. Faraday; ask him what in 1825 was his object in examining benzol. I have perhaps no right to answer this question in Mr. Faraday's presence; but I venture to say that we owe his remarkable inquiry to the pure delight he felt in the elaboration of truth. It was in the same spirit that his successors continued the work. Patiently they elicited fact after fact; observation was recorded after observation; it was the labour of love performed for the sake of truth; ultimately, by the united efforts of so many ardent inquirers, exerted year after year in the same direction, the chemical history of benzol and its derivatives had been traced. The scientific foundation having thus been laid, the time of application had arrived, and by one bound, as it were, these substances, hitherto exclusively the property of the philosopher, appear in the market-place of life.

Need I say any more? The moral of Mauve and Magenta is transparent enough. I read it in your eyes,—we understand each other. Whenever in future one of your chemical friends, full of enthusiasm, exhibits and explains to you his newly-discovered compound, you will not cool his noble ardour by asking him that most terrible of all questions, "What is its use? Will your compound bleach or dye? Will it shave? May it be used as a substitute for leather?" Let him quietly go on with his work. The dye, the lather, the leather will make their appearance in due time. Let him, I repeat it, perform *his* task. Let him indulge in the pursuit of truth,—of truth pure and simple,—of truth not for the sake of Mauve, not for the sake of Magenta—let him pursue truth for the sake of truth!

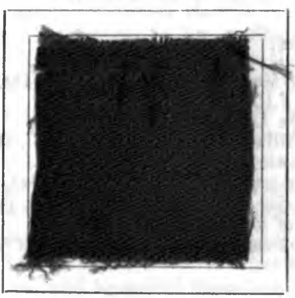
[A. W. H.]

Illustrations of Aniline Colours.

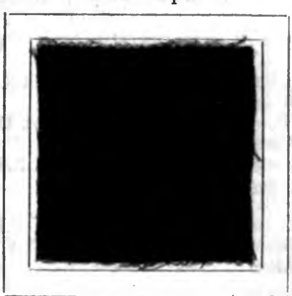
Mauve.



Magenta.



New Purple.



ANNUAL MEETING,

Thursday, May 1, 1862.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1861 was read and adopted.

The amount of Contributions of Members and Subscribers in 1861 amounted to £3013. 10*s.*, the Receipts for Subscriptions to Lectures were £740. 11*s.* 6*d.*; the total Income for the year amounted to £4693. 9*s.*

On Dec. 31, 1861, the Funded Property was £28,655. 17*s.* 2*d.*; and the Balance at the Bankers, £968. 16*s.* 8*d.*, with Six Exchequer Bills of £100 each.

A List of Books Presented accompanies the Report, amounting in number to 524 volumes; making, with those purchased by the Managers and Patrons, a total of 524 volumes (including Periodicals) added to the Library in the Year.

Sixty-three Lectures and Twenty-one Evening Discourses were delivered during the year 1861.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to Professor Faraday, for their services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:—

President—The Duke of Northumberland, K.G. F.R.S.
Treasurer—William Pole, Esq. M.A. F.R.S.
Secretary—Henry Bence Jones, M.A. M.D. F.R.S.

Managers.

The Rev. John Barlow, M.A. F.R.S.
William Bowman, Esq. F.R.S.
Sir Benjamin Collins Brodie, Bart. D.C.L. F.R.S.
Warren De la Rue, Esq. Ph.D. F.R.S.
George Dodd, Esq. F.S.A.
The Earl of Ducie, F.R.S.
John Hall Gladstone, Esq. Ph.D. F.R.S.
William Robert Grove, Esq. M.A. Q.C. F.R.S.
Sir Henry Holland, Bart. M.D. D.C.L. F.R.S.
The Lord Lovaine, M.P.
William Frederick Pollock, Esq. M.A.
Lewis Powell, M.D. F.S.A.
Robert P. Roupell, Esq. M.A. Q.C.
Major-Gen. Edward Sabine, R.A. Pres. of Royal Society.
Colonel Philip James Yorke, F.R.S.

VISITORS.

Neill Arnott, M.D. F.R.S.
Hon. and Rev. Samuel Best.
George J. Bosanquet, Esq.
Archibald Boyd, Esq.
Bernard Edward Brodhurst, Esq.
John Charles Burgoyne, Esq.
George Frederick Chambers, Esq.
Hon. Sir Charles Crompton, Justice of Queen's Bench.
Edward Enfield, Esq.
Captain Frederick Gaussen.
The Duke of Manchester.
John MacDonnell, Esq.
Colonel William Pinney, M.P.
George Stodart, Esq.
Hon. Sir James P. Wilde, Baron of the Exchequer.

His Grace the President nominated the following Vice-Presidents for the ensuing year:—

William Pole, F.R.S. Esq. *the Treasurer.*
The Rev. John Barlow, M.A. F.R.S.
Sir B. C. Brodie, Bart. D.C.L. F.R.S
W. R. Grove, Esq. M.A. Q.C.
Sir H. Holland, Bt. M.D. D.C.L. F.R.S.
Major-Gen. Edward Sabine, Pres. R.S.

WEEKLY EVENING MEETING,

Friday, May 2, 1862.

THE REV. JOHN BARLOW, M.A. F.R.S. Vice-President, in the Chair.

R. MONCKTON MILNES, ESQ. M.P.

On the International Exhibition of 1862.

THE speaker stated that the Managers of the Institution had arranged that some discourses should be there delivered, on the most important natural products to be exhibited at the Great International Festival, and had thought it advisable that these special addresses should be prefaced by a few considerations of the nature and scope of this wonderful congregation of the industries and intelligences of the world. He was much flattered at being selected to perform this duty, and could assure the members present that he should come into no competition with the eminent persons that would follow him, but should confine himself to those generalities and common-places, which are not always the more displeasing for being in some sort the reflection of their own minds.

It was the habit of this Society to deal rather with facts than speculations, and he would therefore direct their attention to the geographical and political conditions which alone rendered possible such an event as this. It had been written with sufficient accuracy for verse, that—

"The total surface of this spherèd earth
Is now surveyed by philosophic eyes;
Nor East nor West conceals a secret worth—
In the wide ocean no Atlantis lies:
Nations and men, that would be great and wise,
Thou knowest, can do no more than men have done;
No wond'rous impulse, no divine surprise,
Can bring this planet nearer to the sun,—
Civilization's prize no royal road has won.

The accessibility of the ocean-waters of the globe was a first necessity to this end, and this had been now accomplished from the ice-bound fires of Mount Erebus to the grave of Franklin. We could not say quite as much of our knowledge of the land of the world, but we perfectly understood the limits of our ignorance, and could fairly assume that there was no position of the earth yet unsurveyed which could in any notable degree add to our physical science, or extend our observation of the habits and destinies of mankind.

Although great continents are represented in our Exhibition only by their fringes, we can hardly contemplate any such conversion of nature or man as should people the sandy spaces of Africa, the vast pastoral steppes of central Asia, or those huge fields of the unlimited liberty of animal and vegetable life which stretch in South America from the tropics to the polar snows, with the higher forms of industry, art, and civilization. It is enough that no longer can Tartar hordes swoop down on richer and fairer lands, and that the sage and saleratus prairies of North America cannot check the enterprising outgrowth of the Anglo-Saxon race.

And this brings us to another necessary condition of our Exhibition, the security of the seas, and the general facility of commercial intercourse. The exceptional piracy which obstructs the trade of the waters of Oceania, and which the energy of Sir James Brooke has done much to repress, was once the custom of the world, and carried with it no notions of cruelty or disgrace. This evil was partially remedied by placing commerce under the safeguard of religion. Where the modern state establishes a factory or a free port, the old state built a temple. Thus the Tyrian Hercules linked together the trade of Greece and Phœnicia in a common worship: thus the fane of Jupiter Ammon was the great resting-place and protection of the caravans of the desert: thus the lines of the chief Catholic pilgrimages were the paths not only of all travellers but of all merchants in the middle ages. The interchange of the gifts of God was sanctioned by Pagan and by Christian piety, and the notion of connecting trade with any inferiority of social station or intellectual power is a perverted remnant of the feudal system, where the jealousy between town and country tended to discredit labour and to idealize brute force.

The speaker proceeded to draw the distinction between ancient and modern trade. In the old Asiatic nations, where influence is still palpable among mankind on the score of authority and the bond of religion, the ideas of free trade and competition would have been incomprehensible. The exclusion of foreigners from the internal navigation of the several countries was universal, and none were permitted even to enter foreign ports, except with the *tessera hospitalis*, or some other symbol of a commercial treaty. Bars were thrown across the mouths of some rivers, as by the Persians across the Tigris after their conquest of Babylon; traces of which impediments to navigation still remain. And in modern Europe the growth of liberal commerce has been slow indeed, and it is one of the happiest privileges of our time, that as regards ourselves at least, we have come to see its consummation. In Sir Dudley North's 'Discourse on Trade,' published in 1691, the principle is laid down "that the whole world as to trade is but as one nation or people, and therein nations are as persons." But the Hollander and the Portuguese long remained the objects of a commercial animosity, which did not prevent the one from occupying our fisheries up to the very coast, and the other from sharing with us the dominion of India.

The social and political conditions represented by our Exhibition next occupied the attention of the speaker. The whole of this marvellous combination of energy and art is the result of free labour—of the spontaneous industry of mankind. It is not the mere application of local nature to local designs, but the collation and transmutation of most diverse and distinct elements to the use and benefit of our race: the juxta-position of our coal and iron have suggested the manufactures of Sheffield, but it is the borax of Tuscany which assists the ingenious labourers of Colebrooke Dale. It is the sign and symbol of the general education of the world, which renders it impossible that discoveries can be neglected or arts be lost. The ignorance and superstition which kept mankind in unnecessary physical pain after the invention of the "spongia somnifera" of the 12th century, can no longer check the annæsthetic powers of a beneficial nature, nor would it require a Harvey to revive, however he might be required to develope, the knowledge that perished with the ashes of Servetus.

But besides the intercommunication of nations in space, the speaker remarked, our Exhibition surely owes much to what he would call the trade of time, the thoughts, the feelings, the interests, that pass from generation to generation; the arts of Greece, the laws of Rome, the religion of the Semitic peoples, the triple elements of modern civilization. The silent East gave the alphabetic character which has transmitted all the speeches and varied literature of the West; the Brahmin preserves the sacred language in which the linguistic science of modern times traces the mother-tongue of all the Indo-Germanic dialects that pass from mouth to mouth beneath these lofty domes.

The singularity of the circumstance that England should be the scene of this meeting of the nations was next alluded to. It was an illustration of the advantage of our insular position, which being combined with sufficient territory, gave us at once the best political conditions of external power and domestic independence. Our greatest danger in history has been not our own conquest, but the conquest of France, which must have absorbed us into the continental system. Now, the peril of our power lay in the rapid political and moral elevation of the other European nations, but we could well afford to sacrifice some individual superiority to the common gain of mankind.

The speaker concluded with noting some of the probable effects of this great jubilee of commerce. Large congregations of men had always vividly struck the imagination, and the jubilee of Pope Boniface so occupied the mind of Dante that he illustrates by it one of his supernatural pictures, and fixed it as the date of his spiritual journey. Such assemblies have always been looked on as harbingers of peace, and we know what were the expectations of 1851. But though that hope has proved delusive, we may yet feel thankful that, with the exception of the American calamity, all the disturbances of the world since that time have been the conflicts of a lower against a higher civilization, in which the higher has had the mastery. The materials here brought together must impress on the spectators the mutual dependence of nations, and the interests of amity. One of the chief objects of interest would be the various applications of art to industry; advantages perhaps somewhat balanced by the injury of the application of industry to art. As art becomes mechanical, it loses the spontaneous dignity which makes it most divine, and it seems impossible to diffuse and repeat it, without some diminution of its highest faculties. But this qualification does not extend to the relations between industry and science, there the moral is as certain as the material profit; intelligent labour is substituted for the mere exertion of brute strength; the supply of comforts is extended from the luxurious classes even to the necessitous; the diseases consequent on physical hardship are diminished, and the average longevity of man increased. To the progress of scientific education not only the philosopher but the statesman looks for the diffusion of public happiness and the permanence of modern civilization. If the states that now rule the world are to escape the doom of Babylon and Rome, of Egypt and of Greece, it is in that they have not made their science the monopoly of a caste or a priesthood, but they have placed it more or less within the reach of the individual intelligence of the humblest citizen. Let the education that enables mankind to apprehend and value truth proceed commensurately with the discoveries of science, and the community will gradually but continuously absorb into itself that knowledge which makes decay impossible, and our country may boldly and confidently meet whatever destiny remains for it in the inscrutable designs of the Creator and Ruler of the universe.

[R. M. M.]

GENERAL MONTHLY MEETING.

Monday, May 5, 1862.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Harry Footner, Esq.
Claud Hamilton, Esq.
Hugh Mair, Esq.
Miss Margaret Laurie.
Capt. Edward Southwell Sotheby, R.N. C.B.
James Spedding, Esq.
William Vansittart, Esq. M.P.

were *elected* Members of the Royal Institution.

Major-Gen. Edward Sabine, R.A. President R.S.
was *admitted* a Member of the Royal Institution.

The following Professors were re-elected :—

WILLIAM THOMAS BRANDE, Esq. D.C.L. F.R.S. Hon. Professor of Chemistry.

JOHN TYNDALL, Esq. F.R.S. Professor of Natural Philosophy.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same : viz.

FROM

Board of Trade, Meteorological Department (by Admiral FitzRoy, the Superintendent)—Report for 1862. 8vo.
1st, 2nd, and 3rd Reports of the Liverpool Compass Committee. 1855–56, fol. 1857–62.
Arnott, Neil, M.D. F.R.S. M.R.I. (the Author)—Supplement to his 'Survey of Human Progress.' 8vo. 1862.
Astronomical Society, Royal—Monthly Notices, March 1862. 8vo.
Belgique, Académie Royale des Sciences, &c.—Bulletin, Année 1861. 8vo.
Annuaire. 1862. 16to.
Brodie, Sir B. C. Bart. D.C.L. F.R.S. M.R.I. (the Author)—Psychological Inquiries. Part II. 16to. 1862.
Chambers, G. F. Esq. M.R.I.—How did they get there ? or, The Nonconforming Ministers of 1662, by the Rev. G. Venables. (O 13) 12mo. 1862.
Popular Inquiry into the Moon's Rotation on her Axis. By J. Von Gumpach. 8vo. 1856.
Warning to Churchmen. (O 10) 18mo. 1862.

Chemical Society—Quarterly Journal, No. 60. 8vo. 1862.
Civil Engineers' Institution—Proceedings, April 1862. 8vo.
Editors—American Journal of Science, by B. Silliman, &c. for March 1862. 8vo.
Artizan for April 1862. 4to.
Athenæum for April 1862. 4to.
Chemical News for April 1862. 4to.
Engineer for April 1862. fol.
Horological Journal, No. 45. 8vo. 1862.
Journal of Gas-Lighting for April 1862. 4to.
Mechanics' Magazine for April 1862. 8vo.
Medical Circular for April 1862. 8vo.
Practical Mechanics' Journal for April 1862. 4to.
St. James's Medley for May 1862. 8vo.
Technologist for April 1862. 8vo.
Foster, Frank (*the Author*)—Number One; or, The Way of the World; to which is added a Colonial Directory. 8vo. 1862.
Franklin Institute of Pennsylvania.—Journal, Vol. XLIV. No. 4. 8vo. 1862.
Geographical Society, Royal—Proceedings, Vol. VI. No. 2. 8vo. 1862.
Granville, A. B. M.D. F.R.S. M.R.I.—Account of the Kissingen Effervescing Saline Chalybeates, &c. (K 88) 8vo. 1862.
Mackie, S. J. Esq. F.G.S. (*the Editor*)—The Geologist for April 1862. 8vo.
Medical and Chirurgical Society, Royal—Proceedings, Vol. IV. No. 1. 8vo. 1862.
Newton, Messrs.—London Journal (New Series) for April 1862. 8vo.
Petermann, A. Esq. (*the Editor*)—Mittheilungen auf dem Gesammtgebiete der Geographie. 1862. No. 3. 4to.
Photographic Society—Journal, No. 120. 8vo. 1862.
Reddie, James, Esq. (*the Author*)—Vis Inertiæ Victa; or, Fallacies affecting Science. (K 88) 8vo. 1862.
Royal Society of London—Proceedings, No. 48. 8vo. 1862.
Scharf, G. Esq. (*the Secretary*)—Catalogue of the National Portrait Gallery. 8vo. 1862.
South, J. F. Esq. (*the Author*)—Observations on the Present Condition and Future Prospects of St. Thomas's Hospital. (K 88) 8vo. 1862.
Tyndall, John, Esq. F.R.S. M.R.I. (*the Author*)—Mountaineering in 1861. 8vo. 1862.
Ladd, Mr. W.—Electric Machine (by McCulloch, London), which formerly belonged to the Princess Charlotte.
Paper showing the effect produced by the burning various metallic wires, by means of the very powerful Electric Machine and Leyden Battery, formerly at the Panopticon, Leicester Square.

WEEKLY EVENING MEETING,

Friday, May 9, 1862.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

William Fairbairn, Esq. F.R.S.

On the Properties of Iron, and its Resistance to Projectiles at High Velocities.

We have no correct record as to the exact time when wrought-iron plates were first employed for the purpose of building vessels. It is, however, certain that iron barges were in use on canals at the close of the last century. In 1824 Mr. Manley, of Staffordshire, built an iron steam-boat for the navigation of the river Seine, and this was the first iron vessel that attempted a sea voyage. She was navigated from this country to Havre, by the late Admiral Sir Charles Napier, and although constructed for shallow rivers, she nevertheless crossed the channel in perfect safety. From that time to 1830, no attempt was made to build iron vessels, and nothing was done towards ascertaining the properties of iron as a material for ship-building.

A series of experiments instituted by the Forth and Clyde Canal Company in 1829-30, to ascertain the law of traction of light boats at high velocities on canals led to the application of iron for the construction of vessels, and the lightness of these new vessels, combined with their increased strength, suggested the extended application of the material in the construction of vessels of much larger dimensions, and ultimately to those of the largest class both in the war and the mercantile navy. Considerable difficulty, however, existed with regard to the navy; and although the principle of iron construction as applied to merchant vessels and packets was fully established, it was nevertheless considered inapplicable, until of late years, for ships of war. It is true that until the new system of casing the sides of vessels, first introduced by the Emperor of the French in 1854, was established, the iron ship was even more dangerous under fire than one built entirely of wood. Now, however, that thick iron plates are found sufficiently strong, under ordinary circumstances, to resist the action of guns, not exceeding 120-pounders, for a considerable length of time, the state of the navy and the minds of our naval officers have entirely changed. We must, therefore, now look to new conditions, new materials, and an entirely new construction, if we are to retain our superiority as *mistress of the seas*. There yet remain amongst us those who contend for the wooden walls, but they are no longer applicable to the wants of the state; and I am clearly of opinion that we cannot afford to trifle with so important a branch of the public service as to fall behind any nation, however powerful and efficient they may be in naval construction. Having satisfied ourselves that this desideratum must be attained, at whatever cost, I shall now endeavour to point out such facts as in my opinion relate to the changes that are now before us, and simply endeavour to show—

1st. The description of iron best calculated to secure strength and durability in the construction of ships of war.

2nd. The distribution and best forms of construction to attain this object; and,

Lastly. The properties of iron best calculated to resist the penetration of shot at high velocities.

Properties of Iron.—If we are desirous to attain perfection in mechanical, architectural, or ship-building construction, it is essential that the engineer or architect should make himself thoroughly acquainted with the properties of the materials which he employs. It is unimportant whether the construction be a house, a ship, or a bridge. We must possess correct ideas of the strength, proportion, and combination of the parts, before we can arrive at satisfactory results; and to effect these objects the naval architect should be conversant with the following facts relating to the resisting powers of malleable and rolled iron to a tensile strain.

The resistance in tons per square inch of—

Yorkshire Iron is	. . .	24·50 tons.
Derbyshire ,,	. . .	20·25 ,,
Shropshire ,,	. . .	22·50 ,,
Staffordshire ,,	. . .	20·00 ,,

Strength of Rivetted Joints.—The architect having fortified himself with the above facts, will be better able to carry out a judicious distribution of the frames, ribs, and plates of an iron ship, so as to meet the various strains to which it may be subjected, and ultimately to arrive at a distribution where the whole in combination presents uniformity of resistance to repeated strains, and the various changes it has to encounter in actual service.

There is, however, another circumstance of deep importance to the naval architect, which should on no account be lost sight of, and that is, the comparative values of the rivetted joints of plates to the plates themselves. These, according to experiment, give the following results:—

Taking the cohesive strength of the plate at .	100
The strength of the double-rivetted joint was found to be	70
And the single-rivetted joint . . .	56

These proportions apply with great force to vessels requiring close rivetting, such as ships and boilers that must be water-tight, and in calculation it is necessary to make allowances in that ratio.

Strength of Ships.—Of late years it has been found convenient to increase the length of steamers and sailing vessels to as much as eight or nine times their breadth of beam, and this for two reasons; first, to obtain an increase of speed by giving fine sharp lines to the bow and stern; and second, to secure an increase of capacity for the same mid-ship section, by which the carrying powers of the ship are greatly augmented. Now, there is no serious objection to this increase of length, which may or may not have reached the maximum. But, unfortunately, it has hitherto been accomplished at a great sacrifice to the strength of the ship. Vessels floating on water and subjected to the swell of a rolling sea,—to say nothing of their being stranded or beaten upon the rocks or sand banks of a lee shore,—are governed by the same laws of transverse strain as simple hollow beams, like the tubes of the Conway and Britannia tubular bridges. Assuming this to be true, and indeed it scarcely requires demonstration, it follows that we cannot lengthen a ship with impunity without adding to her depth or to the sectional area of the plates in the middle along the line of the upper deck.

If we take a vessel of the ordinary construction, or what some years ago was considered the best—300 feet long, 41 feet 6 inches beam, and 26 feet 6 inches deep—we shall be able to show how inadequately she is designed to resist the strains to which she would be subjected. To arrive at these facts we shall approximate nearly to the truth by treating it as a simple beam; and this is actually the case, to some extent, when a vessel is supported at each end by two waves, or when rising on the crest of another, supported at the centre with the stem and stern partially suspended. Now in these positions the ship undergoes, alternately, a strain of compression and of tension along the whole section of the deck, corresponding with equal strains of tension and compression along the section of the keel, the strains being reversed according as the vessel is supported at the ends or the centre. These are, in fact, the alternate strains to which every long vessel is exposed, particularly in seas where the distance between the crests of the waves does not exceed the length of the ship.

It is true that a vessel may continue for a number of voyages to resist the continuous strains to which she is subjected whilst resting on water. But supposing in stress of weather, or from some other cause, she is driven on rocks, with her bow and stern suspended, the probability is that she would break in two, separating from the insufficiency of the deck on the one hand, and the weakness of the hull on the other. This is the great source of weakness in wrought-iron vessels of this construction, as well as of wooden ones, when placed in similar trying circumstances.*

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* *See* Vol. I. of the 'Transactions of the Institution of Naval Architects,' on the Strength of Iron Ships.

Changes in Progress.—Having directed attention to the strength of ships, and the necessity for their improved construction, we may now advert to the changes by which we are surrounded and to the revolution now pending over the destinies of the navy, and the deadly weapons now forging for its destruction. It is not for us alone, but for all other maritime nations, that these Cyclopean monsters are now issuing from the furnaces of Vulcan; and it behoves all those exposed to such merciless enemies to be upon their guard, and to have their 'Warriors,' 'Merrimacs,' and 'Monitors' ever ready, clothed in mail from stem to stern to encounter such formidable foes. It has been seen, and every experiment exemplifies the same fact, that the iron ship with its coat of armour is a totally different construction to that of the wooden walls which for centuries have been the pride and glory of the country. Three-deckers, like the 'Victory' and the 'Ville de Paris' of the last century, would not exist an hour against the sea-monsters now coming into use.

The days of our wooden walls are therefore gone; and instead of the gallant bearing of a 100-gun ship, with every inch of canvas set, dashing the spray from her bows and careering merrily over the ocean, we shall find in its place a black demon, some five or six hundred feet long, stealing along with a black funnel and flag-staff on her mission of destruction and scarcely seen above water, excepting only to show a row of teeth on each side, as formidable as the immense iron carcass that is floating below. This may, with our present impressions, be considered a perspective of the future navy of England,—probably not encouraging,—but one on which the security of the country may ultimately have to depend, and to the construction of which the whole power and skill of the nation should be directed. I have noticed these changes, which are fast approaching, from the conviction that the progress of the applied sciences is not only revolutionizing our habits in the development of naval constructions, as in every other branch of industry, but the art of war is undergoing the same changes as those which have done so much for the industrial resources of the country in times of peace. It is therefore necessary to prepare for the changes now in progress, and endeavour to effect them on principles calculated, not only to ensure security, but to place this country at the head of constructive art. It is to attain these objects that a long and laborious class of experiments have been undertaken by the Government, to determine how the future navy of England shall be built; how it should be armed; and under what conditions it can best maintain the supremacy of the seas. This question does not exclusively confine itself to armour-plated vessels, but also to the construction of ships which, in every case, should be strong and powerful enough to contend against either winds and waves or to battle with the enemy. It is for these reasons that I have ventured to direct attention to the strength of vessels, and to show that some of our mercantile ships are exceedingly weak, arising probably from causes of a mistaken economy on the one hand, or a deficiency of knowledge or neglect of first principles on the other.

Now, it is evident that our future ships of war of the first class must be long and shallow; moreover, they must contain elements of strength and powers of resistance that do not enter into the construction of vessels that are shorter and nearly double the depth. If we take a first-rate ship of the present construction, such as the 'Duke of Wellington,' and compare it with one of the new or forthcoming construction, carrying the same weight of ordnance, we should require a vessel nearly twice the length and little more than half her depth. Let us, for example, suppose the 'Duke of Wellington' to be 340 feet long and 60 feet deep, and the new construction 500 feet long and 46 feet deep; we should then have for the resistance of the 'Duke of Wellington' to a transverse strain tending to break her back,

$$W = \frac{a\,d\,c}{l}$$

Taking 60 as the constant, and the area of the bottom and upper deck as 1060 square inches, we have

$$W = \frac{1060 \times 60 \times 60}{340} = 11{,}223 \text{ tons},$$

as the weight that would break her in the middle. Let us now take the new ship, and give her the same area top and bottom, and again we have

$$W = \frac{1060 \times 46 \times 60}{500} = 5851 \text{ tons},$$

which is little more than half the strength. From this it is obvious—if we are correct in our calculations—that the utmost care and attention is requisite in design and construction to ensure stability and perfect security in the build of ships.

Mechanical Properties of Iron.—It is unnecessary to give more examples in regard to strength, and the proportions that should be observed in the construction of our future navy. I have simply directed attention to it as a subject of great importance, and one that I am satisfied will receive careful consideration on the part of the Admiralty and the Comptroller of the Navy.

The next question for consideration is, the properties of iron best calculated to resist the penetration of shot at high velocities, and in this I am fortunate in having before me the experiments of the Committee on Iron Plates, which may be enumerated as under:—

Specific Gravity.	Tensile Strength in Tons per Square Inch.	Compression per Unit of Length in Tons.	Statical Resistance to Punching in Tons; 1-inch Plate.
7·7621	24·802	14·203	40·1804

Remarks.—The specimens subjected to compression gradually squeezed down to one-half their original height, increasing at the same time in diameter till they attained 90 tons on the square inch.

In these experiments, four descriptions of iron were selected, marked A, B, C, D: the two first and last were taken from rolled and hammered iron plates, excepting C, which was homogeneous, and gave higher results to tension and dead pressure than the others.

In density and tenacity they stood as follows:—

Mark on Plates.	Density.	Tenacity in Tons.	Remarks.
A Plates	7·8083	24·644	
B Plates	7·7035	23·354	
C Plates, homogeneous .	7·9042	27·032	
D Plates	7·6322	24·171	

Here it will be observed, that the strengths are in the ratio of the densities, excepting only the B plates, which deviate from that law.

On the resistance to compression, it will be seen that in none of the experiments was the specimen actually crushed; but they evidently gave way at a pressure of 13 to 14 tons per square inch, and were considerably cracked and reduced in height by increased pressure.

From the experiments on punching, we derive the resistance of A, B, C, D plates to a flat-ended instrument forced through the plate by dead pressure, as follows:—

Mark on Plates.	Shearing Strain in Tons per Square Inch.	Ratio, taking A as Unity.
A Plates . .	19·511	1·000
B Plates . .	17·719	0·907
C Plates . .	27·704	1·168
D Piates . .	17·035	0·873

Here may be noticed, that the difference between the steel plates of series C, and the iron plates of series A, is not considerable, though in all the others the steel plates exhibit a superiority in statical resistance.

Having ascertained, by direct experiment, the mechanical resistance of different kinds of iron and steel plates to forces tending to rupture, it is interesting to observe the close relation which exists between not only the chemical analysis as obtained by Dr. Percy, but how nearly they approximate to the force of impact, as exhibited in the experiments with ordnance at Shoeburyness.

Dr. Percy, in his analysis, observes, that of all the plates tested at Shoeburyness, none have been found to resist better than those lettered

A, B, C, D, with the exception of C. The iron of plate E contained less phosphorus than either of the three, A, B, D; and it is clearly established that phosphorus is an impurity which tends in a remarkable degree to render the metal "cold short," *i.e.* brittle when cold.

The following table shows the chemical composition of these irons:—

Mark.	Carbon.	Sulphur.	Phosphorus.	Silicon.	Manganese.
A	0·01636	0·104	0·106	0·122	0·28
B	0·03272	0·121	0·173	0·160	0·029
C	0·023	0·190	0·020	0·014	0·110
D	0·0436	0·118	0·228	0·174	0·250
E	0·170	0·0577	0·0894	0·110	0·330

Comparing the chemical analysis with the mechanical properties of the irons experimented upon, we find that the presence of 0·23 per cent. of carbon causes brittleness in the iron; and this was found to be the case in the homogeneous iron plates marked C; and although it was found equal to A plates in its resistance to tension and compression, it was very inferior to the others in resisting concussion or the force of impact. It therefore follows, that toughness combined with tenacity is the description of iron plate best adapted to resist shot at high velocities. It is also found that wrought-iron, which exhibits a fibrous fracture when broken by bending, presents a widely different aspect when suddenly snapped asunder by vibration. or by a sharp blow from a shot. In the former case the fibre is elongated by bending, and becomes developed in the shape of threads as fine as silk, whilst in the latter the fibres are broken short, and exhibit a decidedly crystalline fracture. But, in fact, every description of iron is crystalline in the first instance; and these crystals, by every succeeding process of hammering, rolling, &c., become elongated, and resolve themselves into fibres. There is, therefore, a wide difference in the appearance of the fracture of iron when broken by tearing and bending, and when broken by impact, where time is not an element in the force producing rupture.

If we examine with ordinary care the state of our iron manufacture as it existed half-a-century ago, we shall find that our knowledge of its properties was of a very crude and most imperfect character. We have yet much to learn, but the necessities arising from our position as a nation and the changes by which we are surrounded, will stimulate our exertions to the acquisition of knowledge and the application of science to a more extended investigation of a material destined, in course of time, to become the bulwark of the nation. It is, therefore, of primary importance, that we should make ourselves thoroughly acquainted, not only with the mechanical and chemical properties of iron, but we should moreover be able to apply it in such forms and conditions as are best calculated to meet the requirements of the age in which we live.

Entertaining these views, I cheerfully commenced with my talented colleagues the laborious investigations in which we are now engaged, and looking at the results of the recent experiment with the 300-pounder gun on the one hand, and the resisting targets on the other, there is every prospect of an arduous and long-continued contest.

From the Manchester experiments, to which I have alluded, we find that with plates of different thicknesses, the resistance varies directly as the thickness, that is, if the thickness be as the numbers 1, 2, 3, &c., the resistance will be as 1, 2, 3, &c.; but those obtained by impact at Shoeburyness show, that up to a certain thickness of plate, the resistance to projectiles increases nearly as the square of the thickness. That is, if the thickness be as the numbers 1, 2, 3, 4, &c., the resistance will be as the numbers 1, 4, 9, 16, &c. respectively. The measure therefore of the absolute destructive power of shot is its *vis viva*, not its momentum as has been sometimes supposed, but the work accumulated in it varies directly as the weight of the shot multiplied into the square of the velocity.

There is therefore a great difference between statical pressure and dynamical effect; and in order to ascertain the difference between flat-ended and round-ended shot, a series of experiments were undertaken with an instrument or punch exactly similar in size and diameter and precisely corresponding with the steel shot of the wall piece ·85 diameter employed in the experiments at Shoeburyness. The results on the A, B, C, and D plates are as follows:—

Character of Plates.		Resistance in lbs.	
		Punch Flat-ended.	Punch Round-ended.
Half-inch thick .	A Plates . .	57,956	61,886
	B Plates . .	57,060	48,788
	C Plates . .	71,035	85,524
	D Plates . .	49,080	43,337
Three-quarter-inch thick . . .	B Plates . .	84,587	98,420
	D Plates . .	82,381	98,571
	Mean . . .	67,017	72,754

These figures show, that the statical resistance to punching is about the same whether the punch be flat-ended or round-ended, the mean being in the ratio of 1000 : 1085 or 8½ per cent. greater in the round-ended punch. It is, however, widely different, when we consider the depth of indentation of the flat-ended punch and compare it with that produced by the round-ended one, which is 3½ times greater. Hence, we derive this remarkable deduction, that whilst the statical resistance of plates to punching is nearly the same, whatever may be the form of the punch, yet the dynamic resistance or work done in punching is twice as great with a round-ended punch as with a flat-ended one. This of course only approximately expresses the true law; but it exhibits a remarkable coincidence with the results obtained by ordnance at Shoeburyness, and explains the difference which has been observed in these experiments, more particularly in those instances where round shot was discharged from smooth-bored guns at high velocities. To show more clearly the dynamic effect or work done by the weight of shot which struck some of the targets at different velocities, the following results have been obtained.

Target.	Weight of Shot striking Target; lbs.	Work done on Target.	
		Total Foot lbs.	Per Square Foot. Foot lbs.
Thorneycroft 8-inch Shield .	1253	—	29,078,000
Thorneycroft 10-inch Embrasure	1511	—	37,140,000
Roberts's Target	946	822,000	19,726,000
Fairbairn's Target	1024	324,000	23,311,000
Warrior Target	3229	312,000	62,570,000
The Committee's Target . .	6410	—	124,098,780

From the above, it will be observed, that the two last targets have sustained in work done what would, if concentrated, be sufficient to sink the largest vessel in the British navy.

We are all acquainted with the appearances and physical character of artillery, but few are conversant with the nature of the operations and the effects produced by shot on the sides of a ship or on resisting forts and targets.

The shot of a gun—to use the expression of my colleague, Mr. Pole—is simply the means of transferring mechanical power from one place to another. The gunpowder in the gun developes by its combustion a certain quantity of mechanical force, or work as it is now called, and the object of the shot is to convey this work to a distance, and apply it to an object supposed to be otherwise inaccessible. The effect of this, according to Mr. Pole's formula, is—

$W =$ weight of the shot in lbs.
$V =$ its velocity in feet per second.

Then, by the principle of *vis viva*, the quantity of work stored up by the moving mass, measured in lbs. one foot high, is—

$$= \frac{W\,V^2}{2g}$$

g being the force of gravity $= 32\frac{1}{6}$.

Thus, if we have a shot, like that recently used against the 'Warrior' target, 156 lbs., moving at the rate of 1700 feet per second, the work done will be—

$$= \frac{156 \times (1700)^2}{64\frac{1}{3}} = 7{,}008{,}238 \text{ one foot high.}$$

Showing at once the immense power that this small body is able to deliver on every resisting medium tending to arrest its course and bring its particles to a state of rest. Or, in other words, it is equivalent to raising upwards of 3000 tons a foot high in the air.

The Application of Iron for Purposes of Defence.—Having examined in a very condensed and cursory manner the present state of our knowledge in regard to iron, and its application to the purposes of shipbuilding, let us now consider in what form and under what circumstances it can best be applied for the security of our vessels and forts. To the latter the answer is, Make the battery shields thick enough : but a very different solution is required for the navy, where the weight and thickness of the plates is limited to the carrying powers of the ship. It has been observed with some truth that we have learnt a lesson from the recent naval action on the American waters ; but it must be borne in mind that neither of the vessels engaged nor the ordnance employed were at all comparable to what have been used at Shoeburyness.

To those who, like myself, have gone through the whole series of experiments, the late engagement will appear instructive, but not calculated to cause any great alarm, nor yet effect any other changes than those primarily contemplated by the Government, and such as have been deduced from our own experiments. It is, nevertheless, quite evident that our future navy *must be entirely of iron;* and judging from the last experiment with the Armstrong smooth-bore gun, it would almost appear as a problem yet to be solved, whether our ships of war are not as safe without iron armour as with it. If our new construction of ships are strong enough to carry armaments of 300-pounder guns, which is assumed to be the case, our plating of 6 or 7 inches thick would be penetrated, and probably become more destructive to those on board than if left to make a free passage through the ship. In this case we should be exactly in the same position as we were in former days with the wooden walls; but with this difference, that if built of iron the ship would not take fire and might be made shell proof. It is, however, very different with forts, where weight is not a consideration, and those I am persuaded may be made sufficiently strong to resist the heaviest ordnance that can be brought to bear against them. In this statement I do not mean to say that ships of war should not be protected ; but we have yet to learn in what form this protection can be effected to resist the last powerful ordnance, and others of still greater force which are *looming in the distance*, and are sure to follow.

A great outcry has been raised about the inutility of forts ; and the Government, in compliance with the general wish, has suspended those at Spithead : I think improperly so, as the recent experiments at Shoeburyness clearly demonstrate that no vessel, however well protected by armour-plates, could resist the effects of such powerful artillery; and instead of the contest between the 'Merrimac' and the 'Monitor,' and that of the 300-pounder gun being against, they are to every appearance in favour of forts. Should this be correct, we have now to consider how we are to meet and how resist the smashing force of such powerful ordnance as was levelled against the 'Warrior' target.

During the whole of the experiments at Shoeburyness I have most intently watched the effects of shot on iron plates. Every description of form and quality of iron has been tried, and the results are still far from satisfactory; and this is the more apparent since the introduction of the large 300-pounder, just at a time when our previous experiments were fairly on the balance with the 40, 68, 100, and 126-pounders. They now appear worthless, and nothing is left but to begin our labours again *de novo*.

It has been a question of great importance, after having determined the law of resistance and the requisite quality of the iron to be used as armour-plates, how these plates should be supported and attached to the sides of the ship. Great difference of opinion continues to exist on this subject,—some are for entirely dispensing with wood; probably the greater number contend for a wood backing, the same as the 'Warrior' and the 'Black Prince.' I confess myself in the minority on this question; and, judging from the experiments, I am inclined to believe from past experience that wood combined with iron is inferior to iron and iron in its power of resistance to shot; and I am fully persuaded that ultimately the iron armour-plates must be firmly attached to the side, technically called the skin, of the ship. It must, moreover, form part of the ship itself, and be so arranged and jointed as to give security and stability to the structure.

The experiments instituted by the Committee on Iron Plates have been well considered and carefully conducted: they commenced with a series of plates selected from different makers of varying thicknesses, and these have been tested both as respects quality and their powers of resistance to shot. They have, moreover, been placed at different angles and in a variety of positions, and we had just arrived at the desired point of security, when the thundering 300-pounder smooth bore upset our calculations and levelled the whole fabric with the ground. We are, however, not yet defeated; and true to the national character, we shall, like the knights of old, resist to the last—

"And though our legs are smitten off,
We'll fight upon our stumps."

And thus it will be with the Iron Committee and the Armstrong and the Whitworth guns.

In conclusion, allow me to direct attention to a drawing of the 'Warrior' target, with wood backing and its compeer entirely of iron. The first underwent a severe battering, previous to the attack from the 300-pounder, but the other sustained still greater, with less injury to the plates, notwithstanding the failure of the bolts in the first experiment. It must, however, be admitted that plates on wood backing have certain advantages in softening the blow, but this is done at the expense of the plate, which is much more deflected and driven into the wood, which, from its compressibility, presents a feeble support to the force of impact. Again, with wood intervening between the ship and the iron plates, it is impossible to unite them with long bolts so as to impart additional strength to it; on the contrary, they hang as a dead weight on her sides with a constant tendency to tear her to pieces. Now, with iron on iron we arrive at very different and superior results. In the latter, the armour-plates, if properly applied, will constitute the strength and safety of the structure; and, notwithstanding the increased vibration arising from the force of impact of heavy shot, we are more secure in the invulnerability of the plates and the superior resistance which they present to the attack of the enemy's guns. In these remarks I must not, however, attempt to defend iron constructions where they are not defensible, and I am bound to state that in constructions exclusively of iron there is a source of danger which it is only fair to notice, and that is, that the result of two or more heavy shot, or a well concentrated fire, might not only penetrate the plates but break the ribs of the ship. This occurred in the last experiment on my own target, where a salvo of six guns concentrated four on one spot, not more than 14 inches diameter, went through the plates and carried away a part of the frame behind. The same effect might have taken place on the 'Warrior' target; and certainly 9 inches of wood is of little value when assailed by a powerful battery of heavy ordnance and a well concentrated fire.*

In closing these remarks, I have every confidence that the skill and energy of this country will keep us in advance of all competitors, and that a few more years will exhibit to the world the *Iron Navy of England, as of old with its Wooden Walls, unconquerable on every sea.*

[W. F.]

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* Since the above was written, another experiment has been made on the 'Warrior' target with the 300-pounder smooth-bore gun. From this it appears that the wood backing between the armour plates and the skin of the ship cannot safely be dispensed with, and that some compressible or softer substance than iron and iron is necessary to deaden the blow, and absorb the fragments of the shot and the broken plates, which in this instance lodged in the wood, and did not perforate, but only cracked, the skin of the target. From this fact it cannot be denied that this experiment is more satisfactory than those on the iron on iron targets; and however desirous it may be to realize a more effective construction as regards the strength of the ship, it cannot be doubted, in so far as the security of the ship and the lives of those on board are concerned, that a vessel with wood backing is safer in action than one composed entirely of iron. In the present state of our knowledge the experiments are therefore against iron and iron, as regards security from the effects of shot, but they are unfavourable as respects the strength of the ship.

WEEKLY EVENING MEETING,

Friday, May 16, 1862.

Sir Henry Holland, Bart. M.D. D.C.L. F.R.S. Vice-President, in the Chair.

John Scott Russell, Esq. F.R.S.

On the Iron Walls of England.

It was not the first time the speaker had been allowed the honour of expounding such truths as had been the object of his special study, but he had never treated on one of so great national importance. He was somewhat rash, perhaps, in accepting from the Managers the title of this address,—rash because the subject was *then* in a state of transition. It was even worse now, for it had come to what geologists had called a "slip;" he might almost say he found himself at "fault." What he had to say *now* was as different as possible from what he should have said when he made the promise. Six or eight months ago he should have met here a formidable phalanx of adversaries—amongst them nearly all the naval officers—arrayed against him as the advocate of iron ships of war, and he should have had to argue every point as he proceeded. But unfortunately *now* we were all on one side; the pugilistic encounter which might *then* have entertained his audience could not come off. Twelve months ago he had written a pamphlet showing that the end of wooden men-of-war was at hand, and that it was a sin and a shame to send our sailors to sea in them; but the authorities of that day brought their *guns* to bear upon him and completely demolished him. Since then, however, he had got up again; and his heterodoxy had become orthodoxy, and he thought there would be no opponent of "iron walls" for the future. About the beginning of the year we were on the eve of war with a people who, whatever their faults, have never hesitated to adopt for war the fittest weapons, —who, long before rifles were introduced into our army, were celebrated for their use of them and for their manufacture,—to whom we are indebted for the revolvers we found so useful in India, and which, whether they invented them or not, they brought to perfection. That people excelled also in ships; for while the English people, priding themselves on the beautiful "wave lines" on which their fast steamers were built, were slow to perceive the advantage of the same lines for sailing ships; the Americans adopted them for their sailing vessels, and came over and beat our fleetest yachts in our own waters. It was the Americans, too, who first built ships of large size, and carried off our best freights in their large wave-line clippers. When going to war with such a powerful nation it became necessary to take stock of our fighting material. The Government did take stock of your fleet; and the extent of your navy, fit for a naval battle, at the beginning of the present year—as announced in a powerful leader in the 'Times'—was *one ship* of the line. At the present moment we have *two* ships of the line fit for service, the 'Warrior' and the 'Black Prince,' and no more. This serious point is no longer a matter of speculation. It is now universally accepted as a fact,—and accepted by us on a very small naval engagement in American waters, the contest of the 'Merrimac' and 'Monitor,'—that an iron vessel of war is better than a wooden one; while the battle of the 'Merrimac' with the 'Congress' and 'Cumberland' has settled the point in dispute eight or nine months ago, *viz.* that a wooden vessel could not sustain the attack of a ship of war in iron armour. Sir John Hay, the chairman of the naval commission, is quoted in an excellent article in the 'Quarterly Review,' as using this expression,—"The man who goes into action in a wooden vessel is a fool, and the man who sends him there is a villain."

Let us now inquire how this revolution has come about. How is it that our brave sailors ought no longer to face our enemies from behind our wooden walls? This revolution has been chiefly brought about by the introduction in artillery of *horizontal shell-firing.* A certain General Paixhans, a Frenchman, contributed more than any one else to this result. He made cannon of eight to ten inches bore, by which explosive shells—which previously had been fired up in the air and had to come down again upon their object—could then be fired straight at the mark, especially at a wooden ship, which was as good a target as an enemy could possibly desire. This horizontal firing was for a long time a favourite idea with artillerists; but they had very little opportunity of trying it in practical war. Sir Howard Douglas, speaking of its effects, says, "a shell exploding between decks acts in every direction; under the deck it would blow up all above it; on deck it would make a prodigious breach below it, at the same time that it would act laterally." The shell which accidentally exploded in the 'Medea,' on the lower deck, killed the bombardier and several of the crew, knocked down all the bulkheads, and threw the whole squadron into consternation; and the like effect was to be expected from an enemy's shell lodged before its explosion had taken place. The first experiment on a large scale in actual war was at the commencement of the Russian war. The Russian fleet, sneaking about the Black Sea, put into Sinope, and in a very short space of one morning sank and burnt the Turkish squadron. This battle was the entire effect of horizontal shell-firing. The true nature of this horizontal fire has had another illustration. You were all astonished, and wanted to know why Sir Charles Napier did not take Cronstadt, and that our other fleet did not take Sebastopol. It was well known to professional men then why we did not, and there is now no reason why the secret should be kept. Our enemies know it, so why not our friends? Our sailors were not fools enough to stand to their guns in wooden ships exposed to horizontal shell-firing. The speaker had read a letter from Lord Dundonald, one of the bravest sailors that ever trod the deck, written by him to Napier off Cronstadt, in which he expresses the greatest apprehension that Sir Charles would be goaded on to try the attack with what he called combustible ships. We tried Sebastopol—or rather we tried to "make-believe." We drew up our fleet a *great way off*, and one or two brave sailors did go in closer. But the Russian gunners were trained to horizontal shell-firing, and they soon found out it was best to be farther off. The admiral was to be considered the wisest man on board the fleet, for *he* anchored his ship the *farthest* off. Those ships that ventured in were rendered by these shells incapable of continuing the action, and it is not now considered a disgrace to those sailors to say that after three shells had exploded in one ship it was not possible to find men "fools" enough to stand to the guns. "Now, you know why we did not take Cronstadt; and why you did not know it sooner, was because the Government did not wish you should fail to believe in the wooden walls. At last, however, the 'Monitor' and 'Merrimac' have let out the secret, and I am here to tell you the whole truth. It need not be said that those shells at Sinope and Sebastopol were not the perfect weapons we have now—the Armstrong shells are much more precise, and will scatter greater destruction around them. How *much* more I may not tell.

Attention has, therefore, since 1854 till now, been strongly directed to inventions for protecting ships from the effects of shells—and shot also, but chiefly shells. Men will stand against shot, but not against shells; they will run the risk of being hit, but will not face the certainty of being blown up. The invention of iron armour took place fifty or sixty years ago. He was not prepared to name the first inventor; but long before we thought of using it in our navy, Mr. R. L. Stevens, a celebrated engineer, of New York, the builder of some of the fastest steam-vessels on the Hudson, was, he thought, the inventor. Certainly Mr. Stevens, between 1845 and 1850, gave him a full account of experiments made in America, partly at his own and partly at the State's expense, and found that six inches thickness of iron-plate armour was sufficient to resist every shot and shell of that day. In 1845, he (Mr. Stevens) proposed to the American Government to construct an iron-plated ship, and in 1854 the ship was begun. This ship is in progress, but not yet finished. Mr. Stevens is therefore the inventor of iron armour; but no doubt the first man who applied it practically for warfare was the Emperor of the French. In 1854 he engaged in the Russian war, and being a great artillerist, he felt deeply what his fleet could not do in the Black Sea, and we could not do in the Baltic, and so he put his wise head to work to find out what could be done. In 1854, the Emperor built some floating batteries—four or five; we simply took his design, and made five or six.

He had called the introduction of iron-armour ships, Stevens's and the Emperor's; but something he laid claim to for ourselves. Stevens used thin flat plates one over the other; but Mr. Lloyd, of the Admiralty, being consulted at that time, did express his opinion that solid 4½-inch plates would be more effectual than the six inches of thickness in a congeries of plates. Mr. Lloyd has some of the merit as well as the Emperor for the adoption of this kind of armour. The speaker exhibited a model of the first iron batteries. The form, he said, was not very handsome; in short, they were not only not good sea-boats, but in a sea good for nothing. They did, however, in smooth water, some good work; at least three of the French Emperor's did. We never got so far. They went to the Black Sea—to Kinburn; and when they came back they were covered with the marks of shot, but not one of them was seriously damaged. This proved the value of these coated vessels, and so convinced the Emperor, that he *wisely* determined the fleet of France in future should be an iron fleet. We all know with what decision, what success, what economy he has carried that idea out. "I have here," said the speaker, "the means of showing you what this armour is. Now to tell the secret of the efficacy of an armour plate. First, as a matter of fact, it *stops* the shot, as an anvil stops a hammer, and stops it outside the ship; and so, therefore, the armour acts practically as an anvil. When these plates were made they were made to resist 8-pounders, and 4½ inches thickness was ample; but now they were firing shot very much larger. When a round ball, or a round shell, strikes the iron plate, the first thing done is, that it stops the *bit* of the ball that first touches the armour; next, the bits round it rush on until they too get stopped by the armour; and so this *little* (!) ball makes a dent for itself; the remainder of the crushed ball seems, as Mr. Faraday says, to be 'squermed' out of shape. I stole the word, it is so capitally expressive. The shape is not like the original ball,—it is an entirely new form altogether. I call it 'Faraday's squerm.' But we have not the full weight of mettle here. We have only a part of the shot left, the remainder is dispersed in numerous fragments. This is all that remains—a beautiful smooth, polished cone; the rest has gone everywhere. What meanwhile has happened to the armour? The plate first gets a dent; if Sir William Armstrong hits it twice in the same place the dent gets deeper; and if he hits it again in the same hollow, as he so maliciously does, the dent parts company with the plate and starts on a voyage of exploration for itself. But if this ball (150-pounder) were used, I am sure that at the first hit it would take a piece of its own size away with it. Now, if this occurs with a solid shot, what would happen with a hollow ball made to explode, and fired at the ship? Fortunately we know what would happen. We have seen it fired, and it not only got smashed to pieces, but it *forgot to explode;* and the only excuse that can be made for this is that it had not time to do so. I do not know if you know what takes place inside of a gun; but artillerists know it takes some 4 or 5-1000ths of a second for the explosion to go from one end of the charge to the other. Explosion in a shell also takes time, and what happens with the shell striking the armour is that it gets shattered to pieces and the powder scattered about *before* it has time to explode; and this not only with four-inch iron, but with plates a great deal thinner." This power of annihilating shell is one of the advantages which iron bestows on a ship, and for which wood is powerless; and upon this very fortunate fact the new principle of naval construction is based, for whatever armour will do against shot, it will infallibly keep out the shell. What kind of armour is best against shell and what against shot is still a subject of discussion. The most important results were being worked out by the committee on iron plates as to the best adaptation of armour for the purposes we want.

To the speaker's mind, the best kind of armour and the best kind of ship was that combined in the 'Warrior.' There was one gun-deck, in which a battery of guns of the heaviest calibre was placed, and that battery was entirely covered with iron plates, backed with eighteen inches of wood lying between them and the iron skin of the ship. A great effort was now being made to get rid of this wooden backing, which was liable to rot and contributed no strength to the vessel. When an effective iron backing was constructed, the last improvement would be got that was looked for in the construction of an armour-plated ship. He then explained what were the great difficulties to contend with in the construction of the new fleet. There was no difficulty in the armour; we know we can keep out the shell and the shot; for if Sir William Armstrong pushes us too hard, we know how much more iron will keep him out. What we have to do that is difficult, is to build a ship that will not merely keep out shell and resist shot, but also *possess speed with good sea-going qualities*—a monstrous difficulty. The problem was purely one of naval architecture. The difficulty arose in this way: the iron armour placed a very great weight in a very bad place; it tended to make the ship top-heavy, and "crank." Now such a vessel rolls, and a very heavy roll might roll her upside under—an event to be avoided as long as possible. The puzzle was, therefore, to make a stable ship that should stand under this great top-weight of armour, and be a good sea-going vessel. The first iron batteries were totally devoid of this quality. They were not "ship-shape," but "sea-chest" shape. Those we sent out to the Black Sea—and one was under a very good captain—never got there, or, if they did, they never did anything but come back again. He referred to them because they were a class of ships that were now being agitated for. The question was now being entertained, in the highest quarters, as to whether our new fleet of vessels should be fit for long voyages and able to encounter heavy seas, such as were necessary for the protection of our colonies and commerce; or whether they should be made unseaworthy slow vessels, incapable of following the enemy if he ran away, still less of catching him. They were only adapted for staying at home; and, in order to hurt the enemy, the enemy must come to them to be hurt.

Mr. Scott Russell then went into the details of what he advocated as the best class of shot-proof vessel—the improved 'Warrior' class. This class was 58 feet wide, 400 feet long, and more than 7000 tons in size, and cost, fully armed and fitted for sea, not much short of half-a-million. The distinguishing quality of the 'Warrior' was, that she had proved a very excellent sea-going vessel. He was happy to say that four more of this class were building, and two already built. Her armour consisted of 4½-inch iron plates, and extended over the whole length to be protected, and came down about 5 feet below water. This arrangement of armour was such, that its centre of gravity was brought to 6 feet above the water. Now, for a comfortable ship it was held, that the centre of gravity should be near the water-line, and this was therefore a problem of some difficulty; but the ship had turned out, nevertheless, a faster man-of-war than any other, and also an easy, good sea-boat.

This difficulty of top-weight was got over, in Stevens's early armour vessel, by a different method from the 'Warrior.' Giving up the problem of a sea-going ship, he took to smooth water, and built his vessel much on the mid-ship section of a London barge; the sides sloped outwards under water, and sloped inwards above water, so as to form a narrow upper deck, carrying seven guns, the angles of the sides being usually a little above water, but capable of being sunk to the level of it during action. So little, however, was she adapted for a sea-going ship, that a false side was obliged to be put up to make her at all seaworthy; and he would only ask our naval officers if such vessels were fit to protect our trade and our possessions on the wide ocean? The Stevens battery is as long as the 'Warrior,' is to have as high a speed, and carry a central, shot-proof platform, with seven large guns mounted on turn-tables, and worked below decks by machinery. The guns were pointed downwards for loading, and were returned to their positions, and worked thus by men and machinery below the iron deck, and wholly under cover. There were points of this battery so like some recently proposed to be constructed in this country, that it was difficult to conceive the secret had not transpired. This battery was begun in 1854, and is now about to be finished. The Stevens battery is a favourable specimen of a ship built for action in the smooth waters of America. But it is our duty to construct quite a different class of ships, and the 'Warrior' is the type of that class. No one can help seeing the superiority, for our uses, of having such vessels only as can go anywhere and do anything, and are faster, more powerful, more enduring, and more seaworthy than any other steamships of any other navy.

The 'Merrimac,' one of the most beautiful of the American frigates that first set the pattern which has been followed in so many of our own noble vessels, was cut down by the Southerners, and said to have been covered with rails; but, in reality, covered with one coating of plates, six inches broad, and an inch and a half thick, laid diagonally, and a second coating two inches and a half thick in an opposite direction, over a backing of wood. By this simple means she was converted into the formidable vessel that attacked so victoriously the 'Congress' and 'Cumberland,' and disabling them by the shells poured in, as much as by her power as a ram, destroyed them in a short encounter. The 'Monitor,' improvised by Ericsson in three months, is 160 feet long, 40 wide, and six feet deep, and below this upper body is another propelled by steam. She carries a revolving iron tower of six inches thick, containing two heavy guns. Now the upshot of the contest of these two vessels has decided two points for us. 1. That wooden men-of-war are worthless in presence of iron-coated ships; for the 'Merrimac' sank two of them without the slightest difficulty. 2. That wooden ships, even coated with iron, are ineffective against iron ships coated with iron armour; for after a long contest the 'Merrimac' failed to injure the 'Monitor,' and had to retire.

Capt. Coles's shield vessel was next described. His plans were submitted to the Admiralty in 1859, long prior to the construction of Ericsson's battery. These shields and the 'Monitor's' are much alike in principle, but Capt. Coles's vessel is a far better sea-boat than the 'Monitor,' and carries twelve guns instead of one as in that vessel. Coles's shield has a conical roof, and carries one or two Armstrong 100-pounders fixed in slides, which are parts of the interior of the shield, that moves round on a central pivot, and the men working the guns are turned round in it entirely under cover. The construction of the shield ship designed by the Admiralty is altogether better than the 'Monitor's'. The speaker does not wish, however, to see our war-ships replaced by vessels of this class, but by those worthy of ourselves —a fleet of 'Warriors.'

Mr. Scott Russell hoped he had now shown how it had come to pass that we had got a useless navy of wooden ships, and only two iron ones ready for service. There were two more nearly ready, not of the 'Warrior' class, about which the less he said the more he should praise them. The Government had, however, laid down the lines for four more enlarged 'Warriors,' and this was an atonement for the two he would not say anything about. We must then look to a long time before we shall have more than two ships of the 'Warrior' class. He considered this delay deplorable. When the Duke of Somerset was asked in the House why he had not sooner built more iron ships, he said, "the House of Commons had been in no particular hurry." And when he was asked about his tardy adoption of Capt. Coles's plan, he replied, "he delayed until he had consulted the House of Commons about it." Now the serious difficulty was this, while the French Emperor had been making rapid use of his experience of iron batteries, we had not. In 1854, his were at Kinburn and up to their work. In 1856, Capt. Halsted made application to have one of our batteries made the subject of experiment, in order to see if she would resist shot and shell, with a view then to make an iron navy. The Admiralty did have the 'Trusty' made ready; and had her out. Then they took fright and sent her back again; and so we lost two years' start. He would now mention a fact of which there was no longer any grounds for concealment. In 1855 he submitted to the surveyor of the navy a drawing and model of the 'Warrior' class of ships. That model was now on the table, and exhibited all the important features of construction of the 'Warrior' class. But the Admiralty delayed the construction of the first ship of the class till 1859; and so we lost our just claim to the original design of iron ships in armour, with sea-going qualities and speed united. It was Sir John Pakington who, in 1858, first ordered an iron fleet to be commenced, on a joint design of himself, Mr. Scott Russell, and the Surveyor of the Navy. But the French Emperor had already commenced the 'Gloire;' so that instead of being, as we might have been, three years ahead of the French Emperor, our delay had given him the lead, and deprived us of our true priority. He concluded by expressing a hope, that the delays and doubts of the Admiralty might now end; that a fleet of enlarged 'Warriors' would speedily be constructed, fit to carry English sailors on every sea where our colonies and commerce required their protection; and that no more of our time or money would be wasted in the consideration or construction of inferior classes of vessel, unfit for ocean navigation, and good only to stay at home until the enemy should choose to come and be hurt. We had now proved our 'Warrior' class to be sound, wholesome sea-going ships, and to be unparalleled in speed. Of course, improvements would in future be made, and changes introduced. But when our constructions truly embodied the best knowledge and experience of their time, our responsibility was fulfilled, and at present we know of no match for the enlarged 'Warrior' class of 7000 tons, and therefore there can no longer remain any excuse for continuing in our present inefficient condition.

[J. S. R.]

WEEKLY EVENING MEETING,

Friday, May 23, 1862.

The Duke of Northumberland, K.G. F.R.S., President, in the Chair.

Warington W. Smyth, Esq. F.R.S.

On Coal.

The speaker commenced by proposing to select one portion only of a very large subject; and, neglecting chemical and statistical and mining particulars with reference to this important mineral, to confine himself to the physical conditions under which it is found to occur. The enormous value of the coal of this country might be understood from the simple facts that nearly 300,000 of our fellow-subjects find their employment in the coal-mines ; and that the total quantity raised in 1860 amounted to no less than eighty-four millions of tons.

Mr. Smyth then proceeded to describe the nature of the various substances with which the coal is associated, referring to specimens on the table from the field of South Yorkshire. Comparison was made between the total thickness of carboniferous rocks or coal measures of different districts, as well as between the total thickness of coal (in the aggregate of the seams); and hence, it was shown, we have one reason for not estimating the value of a coal-field merely by its area, as we find it laid down in a geological map. Thus, the well-known Durham field, with a thickness of measures of about 2000 feet, has a total thickness of coal of 50 feet. The Derbyshire, 2000, and almost twice the thickness of coal ; the North Staffordshire, 6000 feet of measures, and 130 of coal ; whilst the South Welsh and Saarbrücken fields exhibit thicknesses of 12 to 15,000 feet, with a proportionate increase (especially in the latter) of coal.

A second reason for mistrusting area as a criterion of the importance of a coal-district, is the various forms into which the coal measures have been thrown or moulded by agencies operating at a later date in the earth's crust, whence some districts may exhibit by outcrops an indication of the full amount of their entire contents, whilst in others the beds pass with a gradual inclination beneath newer formations, through which they may nevertheless be accessible. As instances of this were quoted, the vast accession of mineral wealth added, even in the last twelve years, to the Westphalian coal-field, by the explorations carried out through the covering of cretaceous rocks which clothe the northern side of the coal-field, and the remarkable pit lately completed by the Duke of Newcastle, at Shireoak, which, commenced at a distance of several miles from any visible coal-measures, pierced the new red sandstone and magnesian limestone, and reached the "top-hard" coal at 515 yards in depth.

Mr. Smyth then described certain physical features produced in the coal seams subsequently to their consolidation, such as the *cleat* and *backs*, or various nearly vertical divisions, often more or less filled with carbonate of lime or iron pyrites, which add greatly to the amount of ash and clinker.

In referring afterwards to the principal families of plants which are found either in, or associated with, the coal, he wished to show that their occurrence throws a light on the origin of the coal-seams, which again becomes an important guide in enabling us to judge of the continuity of various fields, a question fraught with vital importance, in consequence of the rapid rate at which some of them are being exhausted. Thus the position of the stigmaria in the under-clay or floor of the seam, and of the stems of sigillaria, lepidodendron, calamites, &c., in the roof strata, point to the probability of the growth of the vegetable matter *in situ.* The existence of numerous upright stems, and especially those occurring so often and so dangerously to the miners in the roof of certain coals, is a strong confirmation of the gradual depression of the tract in which these plants grew; and Göppert has shown that the careful examination of a number of seams proves the existence in the coal itself of every family of plant which has been met with in the coal measures.

Thus much had referred to the true carboniferous period, in which it is commonly supposed that a vigorous vegetation first arose, but the speaker described his finding, a few months since, in the Laxey lead and copper mine, in the Isle of Man, at 120 fathoms deep, a seam of anthracite coal, three to four inches thick, in the midst of ancient schists, probably Lower Silurian. He then referred to coaly and lignitic beds in newer formations, especially to the tertiary brown-coal, which in continental, and especially in Southern Europe, attains to great importance. The excellent preservation of the vegetable remains in the lignite has enabled Unger and Heer to make accurate comparison with existing floras, and to show that the tertiary flora had nothing in common with our present flora in Europe, but an extraordinary resemblance to that of modern North America. This was especially to be noticed in closely similar species of the genera *Liquidambar*, *Liriodendron*, *Pavia*, *Nyssa*, *Robinia*, *Taxodium*, *Sequoia*, *Juglans*, *Glycyrrhiza*, *Cercis*, *Laurus*, *Rhododendron*, *Cissus*, and certain oaks and pines. There was hence no retreating from the conclusion, that at this portion of the tertiary period a land communication must have existed between America and Europe. Fragments of that land, with relics of the same tertiary flora, still exist in Iceland and the Azores, with their *surturbrand* and lignites; and thus, that Atlantis, which is generally set down as a dream of the poets, is brought again into solid existence by the studies of the geologist. A relation of this kind at a comparatively recent period, throws a light on the causes of phenomena belonging to an earlier epoch, and will enable us to form conclusions, if not upon the absolute contemporaneity of certain beds or groups of coal measures, at all events upon the physical connection within a given period of the agencies which were forming coal not only in the various fields of Europe, but also in North America: and the speaker concluded by pointing out that the reasoning on the continuity among one another of our British coal-fields, or of them with those of Belgium and North France, depends on somewhat complex data which scientific investigation can alone afford.

SPECIMENS *of* COAL *and* SHALE *from* STRAFFORD COLLIERIES.

No.	Order of Minerals from Surface.	Thickness.			Depth below Surface at Strafford Colliery.
		Yds.	Ft.	In.	
1	Black Shale above Joan Coal.	5	2	3	30½ yards.
2	Joan Coal	..	1	8	
3	Bottom of Joan Coal . . .	..	..	..	
4	Spavin—Floor of Joan Coal .	..	2	0	

SPECIMENS *of* COAL *and* SHALE—*continued.*

No.	Order of Minerals from Surface.	Thickness. Yds.	Ft.	In.	Depth below Surface at Strafford Colliery.
5	Black Rust.	1	0	0	44¾ do.
6	Shale—Roof of Flockton Coal	..	..	6	
7	„ Top of do. . .	..	2	5	53½ do. (6–8)
8	Spavin—Floor of do. . .	..	2	8	
9	Roof of Cannel—Bastard Shale	..	..	8	
10	Top of Cannel Coal . . .	..	1	7	84 do.
11	Coal and Spavin	..	1	2	86 do.
12	Black Spavin	..	1	2	86 1 2 do.
12*a*	Cank Stone	..	..	..	103 do.
13	Shale 3 ft. above Fenton Coal.				
13*a*	Shale 2 ft. above Fenton.				
14	Do. 14 in. above do. . .	..	..	..	
15	Spavin—Roof of Fenton Coal	1	0	4	107 do. (14–16)
16	Fenton Coal (top)	1	0	0	
17	Roof of Parkgate Coal . . .	..	..	..	
18	Shale at top of do. . . .	..	..	..	
19	Top part of do.	0	2	7	
20	Bottom part of Parkgate, Top Bed	..	..	..	131½ do. (17–22)
21	Spavin—Middle of do. . .	..	1	0	
22	Parkgate bottom Bed . . .	..	1	3	
23	Black Bind	..	1	7	216½ do.
24	Ironstone — "Silkstone Black Mine"	2	..	..	220 do. (24–25)
25	Coal—"Black Mine". . . .	..	..	5	
26	Stone Bind—top of Silkstone.	9	0	4	
27	Brass Band and top of Silkstone Coal	..	..	..	
28	Top part of do.	..	2	11½	234 do. (26–31)
29	Dirt from middle of do. . .	..	..	4	
30	Bottom Bed—Silkstone . .	..	2	7½	
31	Stone Bind—floor of do. . .	..	..	..	

[W. W. S.]

WEEKLY EVENING MEETING,

Friday, May 30, 1862.

George Dodd, Esq. F.S.A. in the Chair.

Thomas Bazley, Esq. M.P.

A Plea for Cotton and for Industry.

The fact of the cotton trade in this country being dependent upon chiefly one source of supply for its raw material, has been at all times the cause of anxious solicitude to the thoughtful observer of the nation's progress; but the dilemma in which that great industry is now placed by that sole dependence deserves the consideration alike of the statesman, of the economist, of the merchant, of the employers of labour, and of the humane and patriotic public. Between cotton and labour there was formed in Lancashire, three-quarters of a century ago, an alliance which, combining mechanical with manufacturing skill, has created an industry unparalleled and unsurpassed in any other country.

Little more than a century since the clothing comforts of the masses of the people were few in this country, and the abundant luxuries which now prevail were to them almost unknown. The prepared skins of animals were, up to that recent date, largely used in the clothing of the peasant, and in every house and hamlet the distaff and spindle, and the weaving loom, ministered to the supply of linens, woollens, and their mixtures, in aid of domestic wants. In the reign of Elizabeth her subjects were only equal in number to the inhabitants of Lancashire and Yorkshire at the present time, and greatly below the people of those two counties with Cheshire added. The British people under Elizabeth were powerful, and in splendour and position ranked with the highest nations of the earth; yet her army, navy, aristocracy, court, and people did not exceed that portion of Queen Victoria's subjects who directly and indirectly subsist upon the toils, industry, and capital of the cotton trade. The kingdoms of Belgium, Portugal, Holland, and Hanover, do not separately contain populations as extensive as the cotton trade supports in Great Britain; hence this industry of five millions of dependents, sustained by no separate regal power, and hitherto happy and prosperous as a portion of the subjects of our gracious Sovereign, may claim to be at least of some national importance. About three centuries ago the whole people of this country might be equal to the five millions who now subsist by the manipulations, products, and commerce of cotton; but at this moment the population of the United Kingdom may be regarded as thirty millions, yet the same circumscribed and limited extent of land only exists to afford them the means of labour and to yield them its fruits which supported their predecessors. Even in Elizabeth's reign the people were deemed to be too numerous for the extent of land in her British dominions, and restrictions upon the building of dwellings in proportion to the areas of districts were enacted. If, therefore, the augmentation within the period now mentioned, of from five to thirty millions of people, be considered, it is self-evident that new sources of industry have had to be developed to supply increasing wants. Mineral and agricultural products have, in past ages, furnished scanty exports to pay for foreign articles of necessity and luxury; but, with a constantly increasing population, the yield of the soil has been absorbed by the enlarged consumption at home; and now foreign supplies of corn, of other food, and of luxuries are required for, and may be equal to the subsistence of one-third of the entire population of the United Kingdom. But whilst supplies of food have been needed for this increasing population, the other concomitants of comfort have also been required, and all these necessaries of life could only be obtained in this country by the magic power of skill and labour.

A sea-girt land with navigable rivers, thus possessing egress and ingress, seems to invite foreign intercourse, and to be the first essential to a great mercantile and manufacturing district; but when such a country is found to be immeasurably rich in its mines of coals and metals, when it possesses a temperate and healthy clime, and, above all, when its inhabitants are hardy, sagacious, toil-loving, free, and untiring, we may infer that the decree of Providence has ordained that the people with these advantages shall be blest with plenty, and shall contribute of their abundance to the families of mankind. To no country, however, has exclusive advantages been given; but, wisely, mutual dependence appears to be the pacific bond intended to promote the welfare of the common brotherhood. Probably beyond every other people the British possess the elements of successful trading and commercial industry; but beyond the direct necessaries of life, which their labour ought to enable them to buy, they need raw materials whereon that labour can be employed. Sheep's wool and flax, Great Britain can, in part, produce towards the demand for them; but still large quantities of them are required from foreign countries, and silk, cotton, and other productions of the warmer regions must always be imported as contributions in aid of the manufacturing industry of the United Kingdom. Textile fabrics afford in their production the most extensive means of employment, and have become the indispensable clothing comforts of the people of every country. The fabrics and manufactures of cotton are, however, among the most useful, convenient, elegant, and economical productions of labour. From the quilt or bed-cover to the finest and most filmy muslin, from the fustian garments of the poor to the decorations of lace worn by the rich, and in the snow-white gift of the bleacher to the rainbow colours of the printer, cotton is prolific of comfort and of ornament.

Cotton, almost the chief of the fibrous gifts of Providence, might be grown in excess of the power of man to fabricate and consume. On each side of the equator is a belt of twenty-five degrees of latitude, encircling the earth, and these two belts form immense fields, on which cotton could be produced; consequently it might be obtained to an almost illimitable extent. With, therefore, the natural facilities possessed by the people of this country for entering upon a manufacturing career in cotton, and with ample means existing for cultivating and producing the raw material, no surprise need be felt that an energetic and industrious people should avail themselves of their skill and inventive powers to initiate a great industry founded upon their native talent, and in reliance of an abundant supply of the foreign fibres of cotton being always attainable.

The persecutions of the Duke of Alva had banished from their homes the Flemish weavers who took refuge in Britain. These skilful and ingenious workmen became valuable acquisitions in a country commencing the transition from the labours of the field to those of the loom; and the domestic manufactures of our country began to indicate the progress and perfection which they were destined to attain. The dawn of a great industry was perceptible. Industry was honoured, and labour inculcated as the foundation of the nation's coming distinction and prosperity. Even more than two centuries ago, when steam-engines were unknown, canals not having been formed, nor large manufacturing establishments erected, and while deer-forests surrounded this vast city, there were merchants who promulgated sound economical principles, and who taught lessons of wisdom to the possessors of regal power. In London in 1641, Roberts, a son of commerce, published an enlightened pamphlet, entitled "Treasures of Traffick," and in proof of the soundness of his views the following extract cannot fail being interesting and welcome. He said,—"Some princes are not satisfied with those materials that grow among themselves, and in their own countries, but they covet by all industry to draw others from their neighbours, or foreign nations, to employ their subjects, and to put their people on work, by this means much enriching themselves, and honouring their country; and adding a great help to the public traffick thereof, selling and venting them thus wrought, even to those nations who many times have sold and furnished them with the very first materials of the said manufactories." "Manchester, in Lancashire, must also herein be remembered, and worthily, and for their industry commended, who buy yarn of the Irish in great quantity and weaving it, return the same again in linen to Ireland to sell. Neither doth the industry rest here, for they buy cotton wool in London that comes first from Cyprus and Smyrna, and at home work the same and perfect it into fustians, vermillions, dymities, and such other stuffs, and then return it to London where the same is vented and sold, and not seldom sent to foreign parts." Thus at a period in English history when Charles the First was surrounded with troubles, discord and distress prevailing, Roberts, in beautiful simplicity of language, uttered the first plea for cotton and for its industry, thereby throwing a lustre upon his own name and upon the seventeenth century.

In the middle of the eighteenth century a considerable home manufacture had arisen. Cotton was spun by hand and afterwards blended in the loom with linen or woollen, thus producing a mixed fabric. The supply of cotton was then inconsiderable and was obtained from Turkey and the Levant, and from the West Indies. Mechanical science now escaped from the libraries and traditions of the learned, and offered practical aid to the infant industry. Such a galaxy of talent and inventive genius as then stood forth to develope new methods of increasing the comforts both of the palace and of the cottage the world had not seen. By an almost mysterious combination of efforts, Hargreaves, Whyatt, Arkwright, and Crompton were devising their several systems of spinning cotton; Watt was rendering available the majestic power of vapour, directing, controlling, and dooming it to become the universal drudge of man; Scheele and Berthollet, with their oxygenated muriatic acid blanched the calico and the cambric; and the Mauvillions, Nixons, and Peels gave their coloured tints to print these new fabrics; and as if inspired by the inventions which sprang from the east of the Atlantic, the Anglo-Saxon in the United States of America originated the cultivation of cotton in that great territory: but in giving this boon, the production of slave labour, he conferred the bane whence the vast cotton industry now suffers in the deprivations inflicted upon labour and capital. In the year 1700, when mechanical appliances were comparatively unknown in the manufacture of cotton, the consumption of this material might be one million pounds weight; but in 1860 the quantity had culminated in the consumption of one thousand million pounds weight in that year. Cotton began to arrive from America in 1787 in sufficient quantity to prove the power of the States to produce it, and in that year Crompton triumphed over his mechanical difficulties and completed the mule, this machine being the great agent at the present time for the production of coarse as well as of fine yarns; but spinning by rollers and Arkwright's throstle spinning frame had been invented twenty years previously; hence the history of the modern and mechanical cotton trade may be dated from this period.

Eighty years ago the cotton industry of our country was thus initiated, and from that time to the present, progress, improvements, and extension have characterized it. The science, skill, and invention which have accompanied its development are wonderful. It has afforded employment, comfort, and prosperity to many millions of the people of this country during that period, and it has contributed very largely to the national revenue. During the great struggle with the first Napoleon our men were able to leave their country for the strife of war, and yet the steam-engine and the mechanical agencies which existed at home more than compensated for their physical loss; but here was the waste of the nation's strength. Wiser would it have been had these new resources been developed for the moral, mental, and social improvement and comfort of the people at large. Most probably the cotton trade and the development of new mechanical powers, have enabled the people of this country to sustain a system of taxation which, without that trade and those treasures, could not have been borne, and have supported a national expenditure alike extravagant and injurious. The state, therefore, has participated in the contributions of all who have promoted and sustained this industrial fabric.

The capitalists of this trade have now two hundred million pounds sterling invested in it, in fixed and floating property; and the people directly and indirectly employed in it being now five millions, we arrive at the important deduction that not only does the national exchequer derive great benefit from it, but we have capitalists and labourers supported by it as numerous as are the people of several European kingdoms of the present time. Indifferent spectators of the abundance which has happily prevailed in this country since the introduction of the liberal commercial policy which is now established, rarely reflect upon the obligations this vast industry has conferred in aid of the elements of social comfort. Of late years the exports in cotton manufactures have been about fifty millions sterling per annum, or about one-third of the gross exports of the United Kingdom. Well then, as cotton exports constitute one-third of the whole, it becomes evident that cotton buys one-third of the imports; hence, as gold, silver, gems, coffee, tea, sugar, tobacco, wine, oil, and the fruits of sunny climes, as well as corn and other food brought hither, are foreign products largely imported into the United Kingdom, we must claim the merit for the cotton trade of having bought and paid for one-third of these exotic and foreign supplies. In 1860, the last year of active and full employment for the whole of the cotton trade, its manufactured products exceeded eighty millions sterling in value, something more than fifty of which were exported, leaving about thirty millions as the value of the home consumption of cotton manufactures; but as this latter sum will about equal the cost of the raw cotton imported, the beneficial interest of the country in the cotton industry will be represented by its export trade of upwards of fifty millions sterling. That so extensive and prosperous an industry should have been founded upon the supply of a foreign product, is not the least wonderful fact of its history; but that cotton should have been almost exclusively, as it has been, obtained from almost adverse sources, is a great reproach to the British nation.

Of the 2,523,000 bags of *cotton* consumed in this country in the year 1860, 85 per cent. consisted of the growth of the United States, 8 per cent. of the growth of Egypt, Brazil, and other foreign districts; whilst of cotton from the British East and West Indies the consumption was only seven per cent.! In consequence of the convulsion in the States of America, the consumption of cotton in Great Britain, in 1861, resulting from its contracted supply and the loss of the American markets for its manufactured products, diminished 10 per cent.; and whilst of American and other foreign cotton the consumption became only 85 per cent. against 93 per cent. in the previous year, the consumption of East Indian cotton was 15 per cent. against the previous 7 per cent.; but of the present diminished consumption probably 75 per cent. may be East Indian. A very rapid increase has been effected in the consumption of East India cotton, which, in 1860, was 3500 bags per week, in 1861, 7000 bags, and in this year is proceeding at the rate of 15,000 bags or more per week, showing the increase to be 100 per cent. per annum upon each successive year. The actual power of consuming cotton in the United Kingdom is 55,000 bags per week; but lacking the requisite supply, the present total consumption cannot exceed 25,000 bags per week.

Such then having been the rise, progress, productive and consuming power of the cotton trade, are we blameless for allowing this immense industry to exist and extend upon the frail basis of slavery upon which it has largely depended? Not only do prudential and political principles condemn us, for the almost sole dependence for an essential raw material upon a country which has often withheld from us the rights which a free people should in the spirit of justice extend to its neighbour, but a moral inconsistency has beclouded both the nation and the cotton trade. To the honour and advantage of our own country cotton has been received here free from all import duty, but upon the very manufactured goods containing only the untaxed cotton grown in the States of America the Government of those States has exacted a duty of twenty-five per cent. Now whilst as a people we abhor slavery, and our Government has been expending a million per annum in its pretended suppression, we have been receiving yearly large supplies of slave-grown cotton, tobacco, and sugar, requiring the thraldom of a greater number of our fellow-creatures than by our costly hypocrisy we have liberated. A lesson of justice, humanity, and of economical prudence is now being taught the people depending upon cotton for labour and profit; and the Government of this country must not shield itself from the charge of culpability in having neglected to encourage the cultivation of cotton in the vast possessions of Great Britain. A warning voice has often been heard; and ten years ago when the lamented late Prince Consort presided on the occasion of my speaking upon this subject at the Society of Arts, I inquired, "Whence comes the supply of the raw material for this wonderful trade? Is it not the duty of the statesman to ask this question from the interest which not only so large a portion of the community has in obtaining subsistence from it, but also from the general weal and a large amount of public revenue being involved in it? Is it not the duty of the merchant and manufacturer who is directly interested in this great trade to make the same inquiry? And is it not the duty of the philanthropist and the patriot to also ask whether the supply of this raw material be providently secured and, whence do come, and shall come, supplies of cotton wool to sustain this immense national industry?" I added,—"May not an epidemic of disease or of REVOLT momentarily destroy the cotton-fields of America?" These first questions I now reiterate, and I grieve that they are practically necessary; for when first uttered they were only prospectively and prudentially put forth. How fearful is the contemplation of a people, whose labours, directed by intelligence and right principles, having supported them with abundance, and still able and willing to work, being deprived of the material on which their industry has been advantageously engaged? The deprivations in this great industry have become lamentably severe. With less than half a supply of raw material, and at the enhanced cost of a whole supply, only half employment can be afforded, and consequently only half wages or less can be earned. Already the working classes of the cotton trade are subjected to diminished earnings of a million pounds sterling per month. Generally the mills are working half-time, but many are wholly stopped whilst a very few continue to give full employment, but the average time now worked will be the half-time now stated; and the consumption of cotton is only 25,000 bags weekly instead of the 55,000 bags capable of being consumed; but in this latter quantity is included the probable consumption of many new mills which have not begun to work. Of the consumption of cotton at the present moment, the East Indies supply 75 per cent.,—12½ per cent. is America,—and 12½ per cent. other foreign kinds. Last year the East Indies were exhausted of the stocks of cotton usually held there; and it is doubtful whether the million bales then received can be repeated this year. No efforts to obtain cotton from new fields commensurate with the necessity are being made. Past monitions have been disregarded. The Chamber of Commerce at Manchester has constantly recommended that cotton should be obtained from British foreign possessions, and from every country capable of supplying it. Having been a member of that Chamber for more than a quarter of a century, my own attention during that time has been unceasingly devoted to the urgent necessity of procuring new supplies of cotton; and in 1840, when my honourable friend, Mr. J. B. Smith, the present member for Stockport, was the President of the Chamber, I was so deeply impressed with the importance of efforts being made to obtain superior cotton from the British East and West Indies that I grew, as an incentive and example, some of the most perfect and beautiful cotton ever produced, my cotton plantation having been formed in the attic of one of my mills in Manchester, and specimens of my efforts are now before you. In 1848 my honourable friend, Mr. Bright, M.P. for Birmingham, proved by his parliamentary committee the capability of the East Indies to grow and supply abundantly most excellent cotton. With many men of experience I gave evidence before that committee; but apathy in the Government, in the trade, and in the public mind, has caused to be neglected the admonitory facts then elicited. The negro was doomed to enduring wrongs, the cotton industry was left dangerously dependent upon one chief source of supply for its raw material, the national resources remained undeveloped, and our costly colonial system perpetuated for patronage, but comparatively without profitable results.

Essentially Great Britain possesses the monopoly of the best land found in the world for the growth of cotton. With the proof that the finest and best cotton ever produced has been artificially grown in Manchester, why should not the mere trifling difficulties of the colonies be surmounted where soil, climate, and natural advantages exist? In the East Indies the policy under the rule of a nominally commercial company has been absurdly political and despotic. The material prosperity of the people was neglected; navigation, by improving the rivers, has been discouraged; few canals have been formed; roads have scarcely existed; ample means for irrigation have been withheld; quays are almost unknown; and the land held by Governmental feudal power has been largely unproductive. By the small water supplies of Colonel Sir Arthur Cotton, immense benefits have been derived in Madras, and by the recent changes in land tenure great improvements will doubtless result. Railways are now being established, and the general indications of the great dependency are becoming favourable for the extension of cotton and other agriculture, and for trade and commerce. For many years the improved navigation of the Godavery has been a subject of contention and of hope deferred. This river ought to connect the great cotton-fields of Berar with Coringa, and other ports, in Madras. The rocky barriers of the Godavery should be either removed, or they should be avoided by the aid of short links of canals or by tramways. To what extent the works of the Godavery have proceeded we are ignorant, but the advantages which would accrue from their completion cannot be overstated. Its fertile valley would yield immense supplies of excellent cotton and other products, the markets to ten or twelve millions of people being opened would yield double advantages alike to a home and a foreign trade. On the banks of that river, at Ingelhaut, cotton of most acceptable quality to the British spinner is already grown; and in its vicinity, as also in Berar, cottons could be cultivated which would equal, if they did not surpass, the productions of New Orleans. Of the power of the East Indies to produce superabundant supplies of most excellent cotton, no doubt need be entertained. The communications within that vast dependency being effectively extended to its sea-board in every direction, its agriculture being industriously conducted by the aid of practical science, and the government of it becoming wise and just, benefits would flow from and to it, fructifying and enriching the whole empire. In 1860, the imports of cotton from the East Indies were 561,200 bags, of which two-thirds were exported, and in 1861 there came 986,600 bags, or nearly double the previous year's supply; but though the importers of this enlarged import have derived very great profits by the advance which has taken place in the price of cotton, the ryot, or farmer, in India, has not yet importantly obtained any advantage from the increased value of his produce; but if the communication with the interior of India, both as to intelligence and the conveyance of cotton, be facilitated, then the ryot will be stimulated by compensating and increased rewards to extend the cultivation of cotton, and to improve the quality of it. As now stated, ample proof exists that India can grow most excellent cotton, and many supplies of very useful qualities have been thence received; and I now have the satisfaction of placing before this meeting a sample of superior cotton sent by Dr. Short, from Chingleput, in Madras. I am also enabled to display some very good yarns, of above the average fineness, being 60*s*. warp and 80*s*. weft, spun from it by Mr. Kirkpatrick at his mills near Manchester. India has in truth been slandered, both as to its power to produce excellent cotton, and the adaptability of it to the manufactures of Europe; but the cotton and yarn now shown afford all the evidence necessary to prove the fact that our great dependency can supply cotton equal to that obtained at New Orleans. It has also been asserted that Indian cotton would neither bleach nor receive colours so perfectly as American, but here again I am prepared to refute the error and misrepresentation, for I have the pleasure of exhibiting cloth as good made from Indian cotton, and as white and beautiful in the colours of the bleacher and calico printer as a similar fabric would be made from American cotton. Here we possess the foretaste of the now dormant means which would regenerate India, and resuscitate the drooping cotton industry of the United Kingdom. Apathy must cease, and remedial measures be applied to the evils which retard the prosperity of India and embarrass our home trade and commerce. There are duties to be performed by the mercantile community, as well as obstacles to be removed by Government; and without the performance of these duties prolonged disappointment will ensue.

A Cotton Supply Association was formed in Manchester a few years ago, and its labours are constantly directed to obtain corrective measures for the evils of India, and to promote the growth of cotton wherever the soil and climate of any country will enable it to be produced. This association has impelled a wiser policy for India, and has rendered valuable services to that dependency and to other countries, having made grants to upwards of 400 places of cotton seeds and of cleaning gins, besides other agricultural implements. By the exertions of this body small supplies of cotton have been received from many new fields of cultivation. Cotton growing is being slowly resumed in the British West Indies, whence encouraging supplies are now received; but if the proprietors of estates in those islands did their duty to themselves and to their country, an enlarged production of excellent cotton would compensate them and contribute to the nation's prosperity. The fine island of Jamaica, which could produce very large quantities of superior cotton, is a territorial wreck; but see the capability of this island by this sample of cloth made from its cotton. Demerara and other neighbouring possessions can produce more cotton than the United Kingdom could manufacture. From the Cape of Good Hope to Port Natal cotton can be abundantly produced. Africa has of late years sent small, but valuable supplies of cotton of qualities quite equal to the produce of New Orleans, but her Egyptian cotton has, from the time of Mehemet Ali to the present moment, been a large and most welcome contribution. If the million per annum which our fleet for the suppression of slavery costs had been devoted in our own colonies, or even in Africa, to the encouragement of the growth of cotton, sugar, and other products, which the labour of the negro in slavery has yielded, then that disgraceful traffic in human beings might have been annihilated, and our own pursuits untainted with the wrongs inflicted upon the coloured race. Australia, however, has amazing powers for the production of cotton, and in sections of that great country, Queensland, Victoria, and New South Wales, cotton of every class, from the lowest to the highest, might be cultivated and produced beyond the wants of all the world. Queensland has sent small lots of cotton of unsurpassed beauty and excellence, and from this colony and from New South Wales samples of cotton may be seen in the great International Exhibition of qualities adapted to the production of the finest muslins and laces which any skill could manipulate, as may be seen by this beautiful specimen of lace made from it. Labour appears to be almost alone the sole requisite for obtaining supplies of cotton of incalculable extent from Australia. On referring to the cultivation of cotton in America, we learn that there one million of negroes can produce cotton of nearly twice the extent of its consumption in Great Britain, consequently half-a-million of labourers would suffice to produce the cotton needed by the latter, and the question arises whether it would not be an act of prudence and of wisdom to induce this number of Chinese men, women, and children to become cotton growing labourers in Australia. In the States of America only about one quarter of the negro population is there engaged in cotton agriculture, the large majority being employed in producing tobacco, rice, sugar, Indian corn, and in handycraft and domestic pursuits or occupations. The Emperor of France has wisely offered great inducements for the growth of cotton in Algiers, whence very superior cotton is already supplied. That French colony is within a single week's sail of this country, and some eminent men of business here and in France are endeavouring to extend the cultivation of cotton in it, which, if judiciously carried out, cannot fail becoming of vast advantage to all the relations of both countries.

With these facts the power abundantly to produce cotton, not only in British possessions, but in many foreign states, is beyond all doubt. Cotton may also be profitably as well as abundantly grown, and free labour might be more economically employed than has been the labour of the slave. Great injury has been done in delaying efforts to grow cotton in new fields of cultivation by the unwarrantable assumption that slave labour was so amazingly cheap that free labour could not compete with it. This has been a discouraging delusion, for the labour of the slave has actually, in the cotton states of America, been more costly than the free-skilled labour of Europe, and cotton has not been cultivated in countries capable of growing it, from a false conviction that those American States possessed a monopoly of the means of producing it; but it is evident that Providence has not designed that this valuable material should be only raised by the bondage of the negro, for, on the contrary, proofs abound that cotton can be adequately and most profitably produced in the many countries now mentioned alike with advantage to the capitalist and to the free labourer.

By the general neglect of cotton agriculture an aggregation of evils now exists which can only be contemplated with profound grief and apprehension. Probably 100,000 labourers, who have usually shared the employment afforded in the cotton trade, are now totally idle and penniless. 300,000 more are working short time, and in the sympathetic branches much deprivation prevails. The losses of the labouring classes are one million pounds sterling per month, or twelve millions per annum; whilst the employing classes are, by loss of rents, interest of money on stagnant capital, and in suspended operations, without computing anything for loss of profit, sustaining a loss equal to eight millions per annum, thus making the certain loss, in labour and capital, into twenty millions sterling per annum; but the great infliction of double price for cotton, which adds eighteen millions per annum to its normal market cost, subjects the trade to a drain of nearly forty millions per annum, and to an exhaustion tending almost to extermination. Cotton-spinning and manufacturing in Great Britain are equal in extent to those pursuits carried on in America and upon the continent of Europe. Consequently in the British and foreign cotton trades the losses and disadvantages will be double the extent stated for this country alone. A plea, then, for cotton and for industry becomes a duty and a necessity.

Seeing, therefore, the extent of distress existing in the districts of the cotton manufacture, and that increased deprivations may inflict deeper misery upon the labouring classes, whilst many capitalists may be on the verge of total ruin, and seeing also that the people of the United Kingdom generally sympathize with the sufferings in the cotton trade, what assistance can be rendered to this apparently decaying industry is a question that many benevolent individuals will ask. This great industry has been sanctioned by the legislature, and has been a large contributor to the public revenue; the great body of the people have viewed this industry almost with envy, but certainly with approval, though none of sufficient power have stood forth to obtain a supply for it of its requisite raw material from free labour and from sources so numerous that would have averted the existing calamity. Subscriptions for relief have begun, but the wealth and predominant kindly spirit which yet prevail in the manufacturing districts will be unsparingly expended and exercised to diminish the evils which threaten starvation to meritorious men, women, and children, who are able and willing to work if the accustomed material were forthcoming from which they have hitherto earned their daily bread, and whose labours have enriched their country. Shall pauperism be even contemplated for an energetic and labour-loving people, or rather shall not profitable employment be afforded as in days past, and *without having recourse* to that impoverishing alternative of banishing by emigration those sons of toil who have also contributed to the elevation of their order among the nations of the earth? Will not the British Government and people best remove present and future evils by assisting to develope the resources of their colonies, which, by the introduction of cotton cultivation upon a large scale, would be rendered productive and prosperous?

The colonies would be enabled to trespass less upon the tax-paying people at home, and colonial and British revenues would both obtain beneficial relief. If the British public desires to assist the distressed and suffering cotton trade it will be wise to attack the evil at its root, and to promote the cultivation of cotton in new countries, and thereby prevent the possibility of a repetition of similar disasters, and as cotton-growing is profitable the advantage derived from it would be the recompense of the effort. Shall no attempts, commensurate with the wants of this great industry, be made to obtain adequate supplies of cotton, and at the same time to benefit the dependencies and colonies of Great Britain? No charity can compensate for the losses now sustained, and effectual relief can only proceed from an abundant supply of good and cheap cotton.

A moral truth has now been taught the world, that slavery and tyranny shall not permanently yield prosperity, and the wrongs of the oppressed, crying for justice, indicate that retribution is the corrective of iniquity. Experience and the physical construction of the earth both tell us that without adequate exertions there can be no beneficial results; and consequently, in this great country, when dangers are threatening extinction to any portion of the community, efforts must be called forth to sustain the social and industrial fabric, to contribute to the means of labour, to promote commerce, to extend civilization, and still to raise our national aims in the cause of humanity and of universal justice, that should prosperity again shine upon our country, there may be in our distant intercourse and relations, no leading into captivity, and at home no complaining in our streets.

[T. B.]

GENERAL MONTHLY MEETING,

Monday, June 2, 1862.

The Rev. John Barlow, M.A. F.R.S. Vice-President, in the Chair.

Mrs. Henry Bischofsheim.
The Rev. W. R. Tilson Marsh, M.A. and
Major Roger North.

were *elected* Members of the Royal Institution.

The Managers reported, That in pursuance of the Deed of Endowment, they had appointed John Marshall, Esq. F.R.S. to be Fullerian Professor of Physiology.

The cordial thanks of the Members were returned to Sir Henry Holland, Bart., for a Present of Forty Pounds, being his fourth Annual Gift to the Institution for the purchase of Philosophical Apparatus.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same: viz.

From

Asiatic Society of Bengal—Journal, No. 283. 8vo. 1862.
Astronomical Society, Royal—Monthly Notices, April 1862. 8vo.
Chemical Society—Quarterly Journal, No. 61. 8vo. 1862.
Civil Engineers' Institution—Proceedings, May 1862. 8vo.
Editors—Artizan for May 1862. 4to.
Athenæum for May 1862. 4to.
Chemical News for May 1862. 4to.
Engineer for May 1862. fol.
Journal of Gas-Lighting for May 1862. 4to.
Mechanics' Magazine for May 1862. 8vo.
Medical Circular for May 1862. 8vo.
Practical Mechanics' Journal for May 1862. 4to.
St. James's Medley for May 1862. 8vo.
Technologist for May 1862. 8vo.
Faraday, Professor, D.C.L. F.R.S. M.R.I.—Kais. Akademie der Wissenschaften, Wien: Math. Nat. Classe: Denkschriften. Band XX. 4to. 1862. Sitzungberichte. 1862. 8vo.
Franklin Institute of Pennsylvania—Journal, Vol. XLIV. No. 5. 8vo. 1862.
Geological Institute, Imperial, Vienna—M. Hörnes, Fossilen Mollusken des Tertiærbeckens von Wien. 4to. 1862.
Geological Society—Journal, No. 70. 8vo. 1862.
Transactions, Vol. VII. Nos. 1, 3, 4. 4to. 1845-56.
Guerin-Méneville, M. F. (the Author)—Rapports, &c., sur les Vers à Soie de l'Ailante, &c. 8vo. 1860-2.

Hill, Sir Rowland, K.C.B.—Eighth Report of the Postmaster-General. 8vo. 1862.
Horticultural Society, Royal—Proceedings, 1862. No. 5. 8vo.
Liharzik, M. F. P. (the Author)—The Law of Increase and the Structure of Man. Vienna. 4to. 1862.
Linnean Society—Journal of Proceedings, No. 23. 8vo. 1862.
Mackie, S. J. Esq. F.G.S. (the Editor)—The Geologist for May 1862. 8vo.
Moigno, Abbé (the Editor)—Annuaire du Cosmos. 4e Année. 16to. 1862.
Newton, Messrs.—London Journal (New Series) for May 1862. 8vo.
Perigal, H. Esq.—Ninth and Tenth Reports of the British Meteorological Society, 1859, 1860. 8vo.
Petermann, A. Esq. (the Editor)—Mittheilungen aus dem Gesammtgebiete der Geographie. 1862. No. 4. 4to.
Photographic Society—Journal, No. 121. 8vo. 1861.
Richardson, Mr. J. (the Author)—Studies from the Antique. (K 88) 8vo. 1862.
Statistical Society—Journal, Vol. XXV. Part 2. 8vo. 1862.
United Service Institution, Royal—Journal, No. 20; and Proceedings of Annual Meeting. 8vo. 1862.

Smily, W. R. Esq. M.R.I.—View of Bonaparte's Tomb at St. Helena, from a Drawing by Commissary Ibbotson, in 1823.
Guerin-Méneville, M. F.—Specimens of the Ailante Silkworm and its Products.

WEEKLY EVENING MEETING,

Friday, June 6, 1862.

THE DUKE OF NORTHUMBERLAND. K.G. F.R.S. President, in the Chair.

JOHN TYNDALL, Esq. F.R.S.

PROFESSOR OF NATURAL PHILOSOPHY, ROYAL INSTITUTION.

On Force.

THE existence of the International Exhibition suggested to our Honorary Secretary the idea of devoting the Friday evenings after Easter of the present year to discourses on the various agencies on which the material strength of England is based. He wished to make iron, coal, cotton, and kindred matters, the subjects of these discourses; opening the series by a discourse on the Great Exhibition itself; and he wished me to finish the series by a discourse on "Force" in general. For some months I thought over the subject at intervals, and had devised a plan of dealing with it; but three weeks ago I was induced to swerve from this plan, for reasons which shall be made known towards the conclusion of the discourse.

We all have ideas more or less distinct regarding force; we know in a general way what muscular force means, and each of us would less willingly accept a blow from a pugilist than have his ears boxed by a lady. But these general ideas are not now sufficient for us; we must learn how to express numerically the exact mechanical value of the two blows; this is the first point to be cleared up.

A sphere of lead weighing 1 lb. was suspended at a height of 16 feet above the theatre floor. It was liberated, and fell by gravity. That weight required exactly a second to fall to the earth from that elevation; and the instant before it touched the earth, it had a velocity of 32 feet a second. That is to say, if at that instant the earth were annihilated, and its attraction annulled, the weight would proceed through space at the uniform velocity of 32 feet a second.

Suppose that instead of being pulled downward by gravity, the weight is cast upward in opposition to the force of gravity, with what velocity must it start from the earth's surface in order to reach a height of 16 feet? With a velocity of 32 feet a second. This velocity imparted to the weight by the human arm, or by any other mechanical means, would carry the weight up to the precise height from which it has fallen.

Now the lifting of the weight may be regarded as so much mechanical work. I might place a ladder against the wall, and carry the weight up a height of 16 feet; or I might draw it up to this height by means of a string and pulley, or I might suddenly jerk it up to a height of 16 feet. The amount of work done in all these cases, as far as the raising of the weight is concerned, would be absolutely the same. The absolute amount of work done depends solely upon two things: first of all, on the quantity of matter that is lifted; and secondly, on the height to which it is lifted. If you call the quantity or mass of matter m, and the height through which it is lifted h, then the product of m into h, or mh, expresses the amount of work done.

Supposing, now, that instead of imparting a velocity of 32 feet a second to the weight we impart twice this speed, or 64 feet a second. To what height will the weight rise? You might be disposed to answer, "To twice the height;" but this would be quite incorrect. Both theory and experiment inform us that the weight would rise to four times the height: instead of twice 16, or 32 feet, it would reach four times 16, or 64 feet. So also, if we treble the starting velocity, the weight would reach nine times the height; if we quadruple the speed at starting, we attain sixteen times the height. Thus, with a velocity of 128 feet a second at starting, the weight would attain an elevation of 256 feet. Supposing we augment the velocity of starting seven times, we should raise the weight to 49 times the height, or to an elevation of 784 feet.

Now the work done—or, as it is sometimes called, the *mechanical effect*—as before explained, is proportional to the height, and as a double velocity gives four times the height, a treble velocity nine times the height, and so on, it is perfectly plain that the mechanical effect increases as the square of the velocity. If the mass of the body be represented by the letter m, and its velocity by v, then the mechanical effect would be represented by $m v^2$. In the case considered, I have supposed the weight to be cast upward, being opposed in its upward flight by the resistance of gravity; but the same holds true if I send the projectile into water, mud, earth, timber, or other resisting material. If, for example, you double the velocity of a cannon-ball, you quadruple its mechanical effect. Hence the importance of augmenting the velocity of a projectile, and hence the philosophy of Sir William Armstrong in using a 50 lb. charge of powder in his recent striking experiments.

The measure then of mechanical effect is the mass of the body multiplied by the square of its velocity.

Now in firing a ball against a target the projectile, after collision, is often found hissing hot. Mr. Fairbairn informs me that in the experiments at Shoeburyness it is a common thing to see a flash of light, even in broad day, when the ball strikes the target. And if I examine my lead weight after it has fallen from a height I also find it heated. Now here experiment and reasoning lead us to the remarkable law that the amount of heat generated, like the mechanical effect, is proportional to the product of the mass into the square of the velocity. Double your mass, other things being equal, and you double your amount of heat; double your velocity, other things remaining equal, and you quadruple your amount of heat. Here then we have common mechanical motion destroyed and heat produced. I take this violin bow and draw it across this string. You hear the sound. That sound is due to motion imparted to the air, and to produce that motion a certain portion of the muscular force of my arm must be expended. We may here correctly say, that the mechanical force of my arm is converted into music. And in a similar way we say that the impeded motion of our descending weight, or of the arrested cannon-ball, is converted into heat. The mode of motion changes, but it still continues motion; *the motion of the mass is converted into a motion of the atoms of the mass;* and these small motions, communicated to the nerves, produce the sensation which we call heat. We, moreover, know the amount of heat which a given amount of mechanical force can develope. Our lead ball, for example, in falling to the earth generated a quantity of heat sufficient to raise the temperature of its own mass three-fifths of a Fahrenheit degree. It reached the earth with a velocity of 32 feet a second, and forty times this velocity would be a small one for a rifle bullet; multiplying $\frac{3}{5}$ths by the square of 40, we find that the amount of heat developed by collision with the target would, if wholly concentrated in the lead, raise its temperature 960 degrees. This would be more than sufficient to fuse the lead. In reality, however, the heat developed is divided between the lead and the body against which it strikes; nevertheless, it would be worth while to pay attention to this point and to ascertain whether rifle bullets do not, under some circumstances, show signs of fusion.

From the motion of sensible masses, by gravity and other means, the speaker passed to the motion of atoms towards each other by chemical affinity. A collodion balloon filled with a mixture of chlorine and hydrogen was hung in the focus of a parabolic mirror, and in the focus of a second mirror 20 ft. distant a strong electric light was suddenly generated; the instant the light fell upon the balloon, the atoms within it fell together with explosion, and hydro-chloric acid was the result. The burning of charcoal in oxygen was an old experiment, but it had now a significance beyond what it used to have; we now regard the act of combination on the part of the atoms of oxygen and coal exactly as we regard the clashing of a falling weight against the earth. And the heat produced in both cases is referable to a common cause. This glowing diamond, which burns in oxygen as a star of white light, glows and burns in consequence of the falling of the atoms of oxygen against it. And could we measure the velocity of the atoms when they clash, and could we find their number and weight, multiplying the mass of each atom by the square of its velocity, and adding all together, we should get a number representing the exact amount of heat developed by the union of the oxygen and carbon.

Thus far we have regarded the heat developed by the clashing of sensible masses and of atoms. Work is expended in giving motion to these atoms or masses, and heat is developed. But we reverse this process daily, and by the expenditure of heat execute work. We can raise a weight by heat; and in this agent we possess an enormous store of mechanical power. This pound of coal, which I hold in my hand, produces by its combination with oxygen an amount of heat which, if mechanically applied, would suffice to raise a weight of 100 lbs. to a height of 20 miles above the earth's surface. Conversely, 100 lbs. falling from a height of 20 miles, and striking against the earth, would generate an amount of heat equal to that developed by the combustion of a pound of coal. Wherever work is done by heat, heat disappears. A gun which fires a ball is less heated than one which fires blank cartridge. The quantity of heat communicated to the boiler of a working steam-engine is greater than that which could be obtained from the re-condensation of the steam after it had done its work; and the amount of work performed is the exact equivalent of the amount of heat lost. Mr. Smyth informed us in his interesting discourse, that we dig annually 84 millions of tons of coal from our pits. The amount of mechanical force represented by this quantity of coal seems perfectly fabulous. The combustion of a single pound of coal, supposing it to take place in a minute, would be equivalent to the work of 300 horses; and if we suppose 108 millions of horses working day and night with unimpaired strength, for a year, their united energies would enable them to perform an amount of work just equivalent to that which the annual produce of our coal-fields would be able to accomplish.

Comparing the energy of the force with which oxygen and carbon unite together, with ordinary gravity the chemical affinity seems almost infinite. But let us give gravity fair play; let us permit it to act throughout its entire range. Place a body at such a distance from the earth that the attraction of the earth is barely sensible, and let it fall to the earth from this distance. It would reach the earth with a final velocity of 36,747 feet in a second; and on collision with the earth the body would generate about twice the amount of heat generated by the combustion of an equal weight of coal. We have stated that by falling through a space of 16 feet our lead bullet would be heated three-fifths of a degree; but a body falling from an infinite distance has already used up 1,299,999 parts out of 1,300,000 of the earth's pulling power, when it has arrived within 16 feet of the surface; on this space only $\frac{1}{1300000}$ths of the whole force is exerted.

Let us now turn our thoughts for a moment from the earth towards the sun. The researches of Sir John Herschel and M. Pouillet have informed us of the annual expenditure of the sun as regards heat; and by an easy calculation we ascertain the precise amount of the expenditure which falls to the share of our planet. Out of 2300 million parts of light and heat the earth receives one. The whole heat emitted by the sun in a minute would be competent to boil 12,000 millions of cubic miles of ice-cold water. How is this enormous loss made good? Whence is the sun's heat derived, and by what means is it maintained? No combustion, no chemical affinity with which we are acquainted would be competent to produce the temperature of the sun's surface. Besides, were the sun a burning body merely, its light and heat would assuredly speedily come to an end. Supposing it to be a solid globe of coal, its combustion would only cover 4600 years of expenditure. In this short time it would burn itself out. What agency then can produce the temperature and maintain the outlay? We have already regarded the case of a body falling from a great distance towards the earth, and found that the heat generated by its collision would be twice that produced by the combustion of an equal weight of coal. How much greater must be the heat developed by a body falling towards the sun! The maximum velocity with which a body can strike the earth is about 7 miles in a second; the maximum velocity with which it can strike the sun is 390 miles in a second. And as the heat developed by the collision is proportional to the square of the velocity destroyed, an asteroid falling into the sun with the above velocity would generate about 10,000 times the quantity of heat generated by the combustion of an asteroid of coal of the same weight. Have we any reason to believe that such bodies exist in space, and that they may be raining down upon the sun? The meteorites flashing through the air are small planetary bodies, drawn by the earth's attraction, and entering our atmosphere with planetary velocity. By friction against the air they are raised to incondescence and caused to emit light and heat. At certain seasons of the year they shower down upon us in great numbers. In Boston 240,000 of them were observed in nine hours. There is no reason to suppose that the planetary system is limited to "vast masses of enormous weight;" there is every reason to believe that space is stocked with smaller masses, which obey the same laws as the large ones. That lenticular envelope which surrounds the sun, and which is known to astronomers as the Zodiacal light, is probably a crowd of meteors; and moving as they do in a resisting medium they must continually approach the sun. Falling into it, they would be competent to produce the heat observed, and this would constitute a source from which the annual loss of heat would be made good. The sun, according to this hypothesis, would be continually growing larger; but how much larger? Were our moon to fall into the sun it would develope an amount of heat sufficient to cover one or two years' loss; and were our earth to fall into the sun a century's loss would be made good. Still, our moon and our earth, if distributed over the surface of the sun, would utterly vanish from perception. Indeed, the quantity of matter competent to produce the necessary effect would, during the range of history, produce no appreciable augmentation in the sun's magnitude. The augmentation of the sun's attractive force would be more appreciable. However this hypothesis may fare as a representant of what is going on in nature, it certainly shows how a sun might be formed and maintained by the application of known thermo-dynamic principles.

Our earth moves in its orbit with a velocity of 68,040 miles an hour. Were this motion stopped, an amount of heat would be developed sufficient to raise the temperature of a globe of lead of the same size as the earth 384,000 degrees of the centigrade thermometer. It has been prophesied that "the elements shall melt with fervent heat." The earth's own motion embraces the conditions of fulfilment; stop that motion, and the greater part, if not the whole, of her mass would be reduced to vapour. If the earth fell into the sun, the amount of heat developed by the shock would be equal to that developed by the combustion of 6435 earths of solid coal.

There is one other consideration connected with the permanence of our present terrestrial conditions, which is well worthy of our attention. Standing upon one of the London bridges, we observe the current of the Thames reversed, and the water poured upward twice a-day. The water thus moved rubs against the river's bed and sides, and heat is the consequence of this friction. The heat thus generated is in part radiated into space, and then lost, as far as the earth is concerned. What is it that supplies this incessant loss? The earth's rotation. Let us look a little more closely at the matter. Imagine the moon fixed, and the earth turning like a wheel from west to east in its diurnal rotation. Suppose a high mountain on the earth's surface; on approaching the moon's meridian, that mountain is, as it were, laid hold of by the moon, and forms a kind of handle by which the earth is pulled more quickly round. But when the meridian is passed the pull of the moon on the mountain would be in the opposite direction, it now tends to diminish the velocity of rotation as much as it previously augmented it; and thus the action of all fixed bodies on the earth's surface is neutralized. But suppose the mountain to lie *always* to the east of the moon's meridian, the pull then would be always exerted against the earth's rotation, the velocity of which would be diminished in a degree corresponding to the strength of the pull. *The tidal wave occupies this position*—it lies always to the east of the moon's meridian, and thus the waters of the ocean are in part dragged as a brake along the surface of the earth; and as a brake they must diminish the velocity of the earth's rotation. The diminution, though inevitable, is, however, too small to make itself felt within the period over which observations on the subject extend. Supposing then that we turn a mill by the action of the tide, and produce heat by the friction of the millstones; that heat has an origin totally different from the heat produced by another mill which is turned by a mountain stream. The former is produced at the expense of the earth's rotation, the latter at the expense of the sun's radiation.

The sun, by the act of vaporization, lifts mechanically all the moisture of our air. It condenses and falls in the form of rain,—it freezes and falls as snow. In this solid form it is piled upon the Alpine heights, and furnishes materials for the glaciers of the Alps. But the sun again interposes, liberates the solidified liquid, and permits it to roll by gravity to the sea. The mechanical force of every river in the world as it rolls towards the ocean, is drawn from the heat of the sun. No streamlet glides to a lower level without having been first lifted to the elevation from which it springs by the mighty power of the sun. The energy of winds is also due entirely to the sun; but there is still another work which he performs, and his connection with which is not so obvious. Trees and vegetables grow upon the earth, and when burned they give rise to heat, and hence to mechanical energy. Whence is this power derived? You see this oxide of iron, produced by the falling together of the atoms of iron and oxygen; here also is a transparent gas which you cannot now see—carbonic acid gas—which is formed by the falling together of carbon and oxygen. These atoms thus in close union resemble our lead weight while resting on the earth; but I can wind up the weight and prepare it for another fall, and so these atoms can be wound up, separated from each other, and thus enabled to repeat the process of combination. In the building of plants carbonic acid is the material from which the carbon of the plant is derived; and the solar beam is the agent which tears the atoms asunder, setting the oxygen free, and allowing the carbon to aggregate in woody fibre. Let the solar rays fall upon a surface of sand; the sand is heated, and finally radiates away as much heat as it receives; let the same beams fall upon a forest, the quantity of heat given back is less than the forest receives, for the energy of a portion of the sunbeams is invested in building up the trees in the manner indicated. Without the sun the reduction of the carbonic acid cannot be effected, and an amount of sunlight is consumed exactly equivalent to the molecular work done. Thus trees are formed; thus the cotton on which Mr. Bazley discoursed last Friday is formed. I ignite this cotton, and it flames; the oxygen again unites with its beloved carbon; but an amount of heat equal to that which you see produced by its combustion was sacrificed by the sun to form that bit of cotton.

But we cannot stop at vegetable life, for this is the source, mediate or immediate, of all animal life. The sun severs the carbon from its oxygen; the animal consumes the vegetable thus formed, and in its arteries a reunion of the severed elements take place, and produce animal heat. Thus, strictly speaking, the process of building a vegetable is one of winding up; the process of building an animal is one of running down. The warmth of our bodies, and every mechanical energy which we exert, trace their lineage directly to the sun. The fight of a pair of pugilists, the motion of an army, or the lifting of his own body up mountain slopes by an Alpine climber, are all cases of mechanical energy drawn from the sun. Not, therefore, in a poetical, but in a purely mechanical sense, are we children of the sun. Without food we should soon oxidize our own bodies. A man weighing 150 lbs. has 64 lbs. of muscle; but these, when dried, reduce themselves to 15 lbs. Doing an ordinary day's work, for 80 days, this mass of muscle would be wholly oxidized. Special organs which do more work would be more quickly oxidized: the heart, for example, if entirely unsustained, would be oxidized in about a week. Take the amount of heat due to the direct oxidation of a given amount of food; a less amount of heat is developed by this food in the working animal frame, and the missing quantity is the exact equivalent of the mechanical work which the body accomplishes.

I might extend these considerations; the work, indeed, is done to my hand—but I am warned that I have kept you already too long. To whom then are we indebted for the striking generalizations of this evening's discourse? All that I have laid before you is the work of a man of whom you have scarcely ever heard. All that I have brought before you has been taken from the labours of a German physician, named Mayer. Without external stimulus, and pursuing his profession as town physician in Heilbronn, this man was the first to raise the conception of the interaction of natural forces to clearness in his own mind. And yet he is scarcely ever heard of in scientific lectures, and even to scientific men his merits are but partially known. Led by his own beautiful researches, and quite independent of Mayer, Mr. Joule published his first Paper on the 'Mechanical Value of Heat,' in 1843; but in 1842 Mayer had actually calculated the mechanical equivalent of heat from data which a man of rare originality alone could turn to account. From the velocity of sound in air Mayer determined the mechanical equivalent of heat. In 1845 he published his Memoir on 'Organic Motion,' and applied the mechanical theory of heat in the most fearless and precise manner to vital processes. He also embraced the other natural agents in his chain of conservation. In 1853 Mr. Waterston proposed, independently, the meteoric theory of the sun's heat, and in 1854 Professor William Thomson applied his admirable mathematical powers to the development of the theory; but six years previously the subject had been handled in a masterly manner by Mayer, and all that I have said on this subject has been derived from him. When we consider the circumstances of Mayer's life, and the period at which he wrote, we cannot fail to be struck with astonishment at what he has accomplished. Here was a man of genius working in silence, animated solely by a love of his subject, and arriving at the most important results, some time in advance of those whose lives were entirely devoted to Natural Philosophy. It was the accident of bleeding a feverish patient at Java in 1840 that led Mayer to speculate on these subjects. He noticed that the venous blood in the tropics was of a much brighter red than in colder latitudes, and his reasoning on this fact led him into the laboratory of natural forces, where he has worked with such signal ability and success. Well, you will desire to know what has become of this man. His mind gave way; he became insane, and he was sent to a lunatic asylum. In a biographical dictionary of his country it is stated that he died there: but this is incorrect. He recovered; and, I believe, is at this moment a cultivator of vineyards in Heilbronn.

June 20th, 1862.

While preparing for publication my last course of lectures on Heat, I wished to make myself acquainted with all that Mayer had done in connection with this subject. I accordingly wrote to two gentlemen who above all others seemed likely to give me the information which I needed. Both of them are Germans, and both particularly distinguished in connection with the Dynamical Theory of Heat. Each of them kindly furnished me with the list of Mayer's publications, and one of them was so friendly as to order them from a bookseller, and to send them to me. This friend, in his reply to my first letter regarding Mayer, stated his belief that I should not find anything very important in Mayer's writings; but before forwarding the memoirs to me he read them himself. His letter accompanying the first of these papers, contains the following words:—"I must here retract the statement in my last letter, that you would not find much matter of importance in Mayer's writings: I am astonished at the multitude of beautiful and correct thoughts which they contain;" and he goes on to point out various important subjects, in the treatment of which Mayer had anticipated other eminent writers. My second friend, in whose own publications the name of Mayer repeatedly occurs, and whose papers containing these references were translated some years ago by myself, was, on the 10th of last month, unacquainted with the thoughtful and beautiful essay of Mayer's, entitled "Beiträge zur Dynamik des Himmels;" and in 1854, when Professor William Thomson developed in so striking a manner the meteoric theory of the sun's heat, he was certainly not aware of the existence of that essay, though from a recent article in 'Macmillan's Magazine' I infer that he is now aware of it. Mayer's physiological writings have been referred to by physiologists —by Dr. Carpenter, for example—in terms of honourable recognition. We have hitherto, indeed, obtained fragmentary glimpses of the man, partly from physicists and partly from physiologists; but his total merit has never yet been recognized as it assuredly would have been had he chosen a happier mode of publication. I do not think a greater disservice could be done to a man of science, than to overstate his claims: such overstatement is sure to recoil to the disadvantage of him in whose interest it is made. But when Mayer's opportunities, achievements, and fate are taken into account, I do not think that I shall be deeply blamed for attempting to place him in that honourable position, which I believe to be his due.

Here, however, are the titles of Mayer's papers, the perusal of which will correct any error of judgment into which I may have fallen regarding their author. "Bemerkungen über die Kräfte der unbelebten Natur," Liebig's Annalen, 1842, Vol. 42, p. 231; "Die Organische Bewegung in ihrem Zusammenhange mit dem Stoff-wechsel;" Heilbronn, 1845; "Beiträge zur Dynamik des Himmels," Heilbronn, 1848; "Bemerkungen über das Mechanische Equivalent der Wärme," Heilbronn, 1851.

[J. T.]

WEEKLY EVENING MEETING,

Friday, June 13, 1862.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

Major-Gen. Sir Henry C. Rawlinson, K.C.B. D.C.L. F.R.S.

On Cuneiform Writing, and the Way to Read it.

[Abstract Deferred.]

WEEKLY EVENING MEETING,

Friday, June 20, 1862.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

M. Faraday, Esq. D.C.L. LL.D. F.R.S.

FULLERIAN PROFESSOR OF CHEMISTRY, R.I.

On Gas-Furnaces, &c.

The subject of the evening was gas-glass furnaces, and having arisen almost extemporaneously, it resolved itself chiefly into an account of the manner in which Mr. Siemens has largely and practically applied gas, combined with the use of his heat regenerator, to the ignition of all kinds of great furnaces. Gas has been used to supply heat, even upon a very large scale, in some of the iron blast furnaces, and heat which has done work once has been carried back in part to the place from whence it came to repeat its service; but Mr. Siemens has combined these two points, and successfully applied them in a great variety of cases—as the potter's kiln—the enameller's furnace—the zinc-distilling furnace—the tube-welding furnace—the metal-melting furnace—the iron-puddling furnace—and the glass furnace, either for covered or open pots—so as to obtain the highest heat required over any extent of space, with great facility of management, and with great economy (one half) of fuel. The glass furnace described had an area of 28 feet long and 14 feet wide, and contained eight open pots each holding nearly two tons of material.

The gaseous fuel is obtained by the mutual action of coal, air, and water at a moderate red heat. A brick chamber, perhaps 6 feet by 12 and about 10 feet high, has one of its end walls converted into a fire grate, *i.e.* about halfway down it is a solid plate, and for the rest of the distance consists of strong horizontal plate bars where air enters; the whole being at an inclination such as that which the side of a heap of coals would naturally take. Coals are poured, through openings above, upon this combination of wall and grate, and being fired at the under-surface, they burn at the place where the air enters; but as the layer of coal is from 2 to 3 feet thick, various operations go on in those parts of the fuel which cannot burn for want of air. Thus the upper and cooler part of the coal produces a larger body of hydro-carbons; the cinders or coke which are not volatilized, approach, in descending, towards the grate; that part which is nearest the grate burns with the entering air into carbonic acid, and the heat evolved ignites the mass above it; the carbonic acid, passing slowly through the ignited carbon, becomes converted into carbonic oxide, and mingles in the upper part of the chamber (or gas-producer) with the former hydro-carbons. The water, which is purposely introduced at the bottom of the arrangement, is first vaporized by the heat, and then decomposed by the ignited fuel and rearranged as hydrogen and carbonic oxide; and only the ashes of the coal are removed as solid matter from the chamber at the bottom of the fire-bars.

These mixed gases form the gaseous fuel. The nitrogen which entered with the air at the grate is mingled with them, constituting about a third of the whole volume. The gas rises up a large vertical tube for 12 or 15 feet, after which it proceeds horizontally for any required distance, and then descends to the heat-regenerator, through which it passes before it enters the furnaces. A regenerator is a chamber packed with fire-bricks, separated so as to allow of the free passage of air or gas between them. There are four placed under a furnace. The gas ascends through one of these chambers, whilst air ascends through the neighbouring chamber, and both are conducted through passage outlets at one end of the furnace, where mingling they burn, producing the heat due to their chemical action. Passing onwards to the other end of the furnace, they (*i.e.* the combined gases) find precisely similar outlets down which they pass; and traversing the two remaining regenerators from above downwards, heat them intensely, especially the upper part, and so travel on in their cooled state to the shaft or chimney. Now the passages between the four regenerators and the gas and air are supplied with valves and deflecting plates, some of which are like four way-cocks in their action; so that by the use of a lever these regenerators and air-ways, which were carrying off the expended fuel, can in a moment be used for conducting air and gas into the furnace; and those which just before had served to carry air and gas into the furnace now take the burnt fuel away to the stack. It is to be observed, that the intensely heated flame which leaves the furnace for the stack always proceeds downwards through the regenerators, so that the upper part of them is most intensely ignited, keeping back, as it does, the intense heat: and so effectual are they in this action, that the gas which enters the stack to be cast into the air is not usually above 300°F. of heat. On the other hand, the entering gas and air always passes upwards through the regenerator, so that they attain a temperature equal to white heat before they meet in the furnace, and there add to the carried heat that due to their mutual chemical action. It is considered that when the furnace is in full order, the heat carried forward to be evolved by the chemical action of combustion is about 4000°, whilst that carried back by the regenerators is about 3000°, making an intensity of power which, unless moderated on purpose, would fuze furnace and all exposed to its action.

Thus the regenerators are alternately heated and cooled by the outgoing and entering gas and air, and the time for the alternation is from half an hour to an hour, as observation may indicate. The motive power on the gas is of two kinds—a slight excess of pressure within is kept up from the gas-producer to the bottom of the regenerator to prevent air entering and mingling with the fuel before it is burnt; but from the furnace, downwards through the regenerators, the advance of the heated medium is governed mainly by the draught in the tall stack, or chimney.

Great facility is afforded in the management of these furnaces. If, whilst glass is in the course of manufacture, an intense heat is required, an abundant supply of gas and air is given; when the glass is made, and the condition has to be reduced to working temperature, the quantity of fuel and air is reduced. If the combustion in the furnace is required to be gradual from end to end, the inlets of air and gas are placed more or less apart the one from the other. The gas is lighter than the air; and if a rapid evolution of heat is required as in a short puddling furnace, the mouth of the gas inlet is placed below that of the air inlet; if the reverse is required, as in the long tube-welding furnace, the contrary arrangement is used. Sometimes, as in the enameller's furnace, which is a long muffle, it is requisite that the heat be greater at the door end of the muffle and furnace, because the goods, being put in and taken out at the same end, those which enter last and are withdrawn first, remain, of course, for a shorter time in the heat at that end; and though the fuel and air enters first at one end and then at the other, alternately, still the necessary difference of temperature is preserved by the adjustment of the apertures at those ends.

Not merely can the supply of gas and air to the furnace be governed by valves in the passages, but the very manufacture of the gas fuel itself can be diminished, or even stopped, by cutting off the supply of air to the grate of the gas-producer; and this is important, inasmuch as there is no gasometer to receive and preserve the aeriform fuel, for it proceeds at once to the furnaces.

Some of the furnaces have their contents open to the fuel and combustion, as in the puddling and metal-melting arrangements; others are enclosed, as in the muffle furnaces and the flint-glass furnaces. Because of the great cleanliness of the fuel, some of the glass furnaces, which before had closed pots, now have them open, with great advantage to the working and no detriment to the colour.

The economy in the fuel is esteemed practically as one-half, even when the same kind of coal is used either directly for the furnace or for the gas-producer; but, as in the latter case, the most worthless kind can be employed—such as slack, &c., which can be converted into a clean gaseous fuel at a distance from the place of the furnace, so, many advantages seem to present themselves in this part of the arrangement.

It will be seen that the system depends, in a great measure, upon the intermediate production of carbonic oxide from coal, instead of the direct production of carbonic acid. Now, carbonic oxide is poisonous, and, indeed, both these gases are very deleterious. Carbonic acid must at last go into the atmosphere; but the carbonic oxide ceases to exist at the furnace, its time is short, and whilst existing it is confined on its way from the gas-producer to the furnace, where it becomes carbonic acid. No signs of harm from it have occurred, although its application has been made in thirty furnaces or more.

The following are some numbers that were used to convey general impressions to the audience. Carbon burnt perfectly into carbonic acid in a gas-producer would evolve about 4000° of heat; but, if burnt into carbonic oxide, it would evolve only 1200°. The carbonic oxide, in its fuel form, carries on with it the 2800° in chemical force, which it evolves when burning in the real furnace with a sufficient supply of air. The remaining 1200° are employed in the gas-producer in distilling hydro-carbons, decomposing water, &c. The whole mixed gaseous fuel can evolve about 4000° in the furnace, to which the regenerator can return about 3000° more.

[M. F.]

GENERAL MONTHLY MEETING,

Monday, July 7, 1862.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

William Radford, M.D. and
William Ranger, Esq. C.E.

were *elected* Members of the Royal Institution.

The Special Thanks of the Members were returned to WARREN DE LA RUE, Esq. F.R.S. M.R.I. for the following Presents:

Apparatus for showing the total Eclipse of the Sun with Clock-work, adapted also to the Pneumatic Bell Experiments.
Apparatus used in Photographing the Sun.
Mount for holding Photographs for the Electric Lantern.
Apparatus for throwing Reflected Images on the Screen.
The same for the Corona.
A Screen for the Electric Lamp, 12 ft. by 6ft. in size.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same: viz.—

FROM

Secretary for War (through Sir H. James, the Editor)—Abstracts of the Meteorological Observations taken at the Stations of the Royal Engineers, 1853–9. 4to. 1862.

British Museum, Trustees—Description of Ancient Marbles. By S. Birch. With Engravings. Part XI. 4to. 1861.
Select Papyri. Part II. Plates 1–19. fol. 1860.
Catalogue of the Birds of the Tropical Islands of the Pacific. By G. R. Gray. 8vo. 1859.
Catalogue of Acanthopterygian Fishes. By A. Günther. Vols. I.–III. 8vo. 1859–61.
Catalogue of Bones of Mammalia. By E. Gerrard. 8vo. 1862.
Specimen of a Catalogue of Lycænidæ. By W. C. Hewitson. 4to. 1862.
List of Lepidoptera. Parts 19–24. 12mo. 1859–62.
Catalogue of Halticidæ. Part 1. By Hamlet Clark. 8vo. 1860.
List of British Diatomaceæ. By W. Smith. 12mo. 1859.
Guide to Exhibition-rooms (Natural History and Antiquities). 8vo. 1862.
Description of Reading-room. 16to. 1860.
Guide to Printed Books. 8vo. 1862.
——— Autograph Letters, &c. 8vo. 1862.
——— Drawings and Prints. 8vo. 1862.
——— Collection of Minerals. 8vo. 1862.

Actuaries, Institute of—Assurance Magazine, No. 48. 8vo. 1862.

Asiatic Society of Bengal—Journal, No. 284. 8vo. 1862.

Astronomical Society, Royal—Monthly Notices, May, 1862. 8vo.
Memoirs, Vol. XXX. 4to. 1862.
Baude, Baron—Notices sur les Modèles, Cartes et Dessins relatifs aux Travaux Publics (de l'Empire Français). 8vo. Paris, 1862.
Bavarian Academy, Royal—Sitzungsberichte; 1861. Band II. Heft 3. 8vo.
Beavan, H. J. C. Esq. (the Author)—Six Weeks in Ireland. 16to. 1862.
Chemical Society—Quarterly Journal, Nos. 62, 63. 8vo. 1862.
Civil Engineers' Institution—Minutes of Proceedings, Vol. XIX. 8vo. 1859-60.
Clark, Latimer, Esq. (the Author)—Experimental Investigation on the Propagation of the Electric Current, on Submarine Telegraph Cables. fol. 1861.
Editors—American Journal of Science, by B. Silliman, &c. for May, 1862. 8vo.
Artizan for June, 1862. 4to.
Athenæum for June, 1862. 4to.
Chemical News for June, 1862. 4to.
Engineer for June, 1862. fol.
Horological Journal, No. 46. 8vo. 1862.
Journal of Gas-Lighting for June, 1862. 4to.
Mechanics' Magazine for June, 1862. 8vo.
Medical Circular for June, 1862. 8vo.
Practical Mechanics' Journal for June, 1862. 4to.
Technologist for June, 1862. 8vo.
Genève, Société de Géographie de—Mémoires et Bulletins. Tome II. 8vo. 1861.
Gloesener, M. (the Author)—Traité des Applications de l'Electricité, Tome I. 8vo. Paris. 1861.
Hambleton, Rev. Joseph, M.R.I.—Notice de l'Invention du Laryngoscope (du Dr. Czermak), par P. Richard; et Observations sur la Voix Humaine, par M. Garcia. (K 89) 8vo. 1861.
Horticultural Society, Royal—Proceedings, 1862. No. 6. 8vo.
Mackie, S. J. Esq. F.G.S. (the Editor)—The Geologist for June, 1862. 8vo.
Manning, Frederick, Esq. M.R.I.—G. Hogarth: The Philharmonic Society of London, 1813-1862. 8vo. 1862.
Notices concerning the Queen's Concert Rooms, Hanover Square. (L 13) 8vo. 1862.
Mechanical Engineers' Institution, Birmingham—Proceedings, Jan. 1862. 8vo.
Musson, Eugène (the Author)—Letter to Napoleon III. on Slavery in the Southern States. (K 89) 8vo. 1862.
Newton, Messrs.—London Journal (New Series) for June, 1861. 8vo.
Petermann, A. Esq. (the Editor)—Mittheilungen aus dem Gesammtgebiete der Geographie. 1862. No. 5. 4to.
Photographic Society—Journal, No. 122. 8vo. 1861.
Royal Society of London—Proceedings, No. 9. 8vo. 1862.
Greenwich Observations in 1860. 4to. 1862.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, Jan. und Feb. 1862. 4to.
United Service Institution, Royal—Journal, Nos. 21, 22. 8vo. 1862.

Karsten, M. G.—Thermometer designed by Schumacher, for the late Professor F. Daniell.
Smyth, Warington W. Esq. F.R.S.—Coal Specimens from the Grace Mary Pit, Oldbury, and other places
Varley, Cromwell F. Esq. M.R.I.—Specimens of Submarine Electric Telegraph Cable, about to be laid between Lowestoft and Zandvoort, on the coast of Holland.

GENERAL MONTHLY MEETING,

Monday, November 3, 1862.

The Rev. JOHN BARLOW, M.A. F.R.S. Vice-President, in the Chair.

August F. Andresen, Esq.
Thomas R. Williams, Esq. and
The Hon. William Warren Vernon.

were *elected* Members of the Royal Institution.

The decease of Sir B. C. BRODIE, Bart., Manager of the Royal Institution, was announced from the Chair.

The following Lecture Arrangements have been made for the Ensuing Season :—

Christmas Lectures, 1862.

Prof. FRANKLAND, F.R.S.—Six Lectures, 'On Air and Water.'
(*Adapted to a Juvenile Auditory.*)

Before Easter, 1863.

Prof. J. MARSHALL, F.R.S.—Twelve Lectures, 'On Physiology.'
Prof. E. FRANKLAND, F.R.S.—Ten Lectures, 'On Chemistry.'
W. SAVORY, Esq. F.R.S.—Four Lectures, 'On Life and Death.'
Prof. MAX MÜLLER.—Twelve Lectures.

After Easter.

Prof. TYNDALL, F.R.S.—Seven Lectures.
D. T. ANSTED, Esq. F.R.S.—Nine Lectures, 'On Geology.'
Prof. WM. THOMSON, F.R.S.—Three Lectures, 'On Electric Telegraphy.'

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same : viz.—

FROM

Her Majesty's Government (*through Henry Cole, C.B.*)—Catalogue of the Special Exhibition of Works of Art on Loan at the South Kensington Museum. 8vo. 1862.

Commissioners in Lunacy—Sixteenth Report. 8vo. 1862.

Governor-General of India—Memoirs of Geological Survey of India :— Palæontologica Indica. No. 1. 4to. 1861.

Actuaries, Institute of—Assurance Magazine, No. 49. 8vo. 1862.

Agricultural Society, Royal—Journal, Vol. XXIII. 8vo. 1862.

Airy, G. B. F.R.S. (*the Astronomer Royal*)—Errata in Hansen's Lunar Tables. 8vo. 1862.

Anonymous—Thoughts on Natural Phenomena. 8vo. 1862.

Antiquaries, Society of—Archæologia. Vol. XXXVIII. Part 2. 4to. 1862. Proceedings. Vol. I. Nos. 2-7. 8vo. 1860-2.

Asiatic Society, Royal—Journals, Vol. XIX. Part 4; and Vol. XX. Part 1. 8vo. 1862
Asiatic Society of Bengal—Journal, No. 285. 8vo. 1862.
Astronomical Society, Royal—Monthly Notices, June, 1862. 8vo.
Basel Natural History Society—Verhandlungen. Theil III. Heft 3. 8vo. 1862.
Bavarian Academy, Royal—Abhandlungen. Band IX. Abth 2. 4to. 1862.
Sitzungsberichte, 1862. Band I. Heft 1, 2, 3. 8vo.
British Architects, Royal Institute of—Papers read in Session 1861-2. 4to.
H. Roberts: Essentials of a Healthy Dwelling. (K 89) 8vo. 1862.
British Association for the Advancement of Science—Report of the Thirty-first Meeting, held at Manchester in September, 1861. 8vo. 1862.
Chambers, G. F. Esq. M.R.I.—Addresses of the Hungarian Diet of 1861, &c. (L 13) 8vo. 1862.
Chemical Society—Quarterly Journal, Nos. 64, 65, 66. 8vo. 1862.
Cornwall Polytechnic Society Royal—Report for 1861. 8vo. 1862.
De la Rive, Professor (the Author)—Sur les Aurores Boréales et Australes. (From Mém. de la Soc. de Phys. de Genève.) Tome XVI. 4to. 1862.
Dresser, Christopher, Esq. (the Author)—The Art of Decorative Design. 8vo. 1862.
Dublin Society, Royal—Journal, Nos. 24, 25. 8vo. 1862.
Editors—American Journal of Science, by B. Silliman, &c. for July and September, 1862. 8vo.
Artizan for July to October, 1862. 4to.
Athenæum for July to October, 1862. 4to.
Chemical News for July to October, 1862. 4to.
Engineer for July to October, 1862. fol.
Horological Journal, Nos. 47-51. 8vo. 1862.
Journal of Gas-Lighting for July to October, 1862. 4to.
Mechanics' Magazine for July to October, 1862. 8vo.
Medical Circular for July to October, 1862. 8vo.
Practical Mechanics' Journal for July to October, 1862. 4to.
St. James's Medley, Nos. 31, 32. 8vo. 1862.
Technologist for July to October, 1862. 8vo.
Faraday, Professor, D.C.L. F.R.S.—V. Regnault: Expériences pour déterminer les Lois et les Données Physiques nécessaires au Calcul des Machines au Feu. Tome II. 4to. 1862.
Vienna, Imperial Academy of Sciences: Sitzungsberichte. Jan.-April, 1862.
Abhandlungen der Kön. Akademie der Wissenschaften. 1861. Berlin. 4to. 1862.
Farrer, James W. Esq. M.R.I.—James Farrer, M.P.: Notice of Runic Inscriptions discovered recently during Excavations at Maes Howe, in the Orkneys. 4to. 1862. (Privately printed.)
Franklin Institute of Pennsylvania—Journal, Vol. XLIV. Nos. 1-4. 8vo. 1862.
Geographical Society, Royal—Proceedings, Vol. VI. Nos. 3, 4. 8vo. 1862.
Journal, Vol. XXXI. 8vo. 1862.
Geological Institute, Vienna—Jahrbuch, 1861-2. No. 2. 8vo. 1862.
Geological Society—Quarterly Journal, No. 71. 8vo. 1862.
Griffith, C. Darby, Esq. M.P. M.R.I. (the Author)—Colour to Sculpture: is it applicable? (K 89) 8vo. 1862.
Hood, W. C. M.D. (the Author)—Statistics of Insanity. 1846-60. 8vo. 1862.
Horticultural Society, Royal—Proceedings, 1862. Nos. 7-11. 8vo.
Irish Academy, Royal—Transactions, Vol. XXIV. Part 2. 4to. 1862.
Kerr, Mrs. Louisa Hay, M.R.I.—Vocabulary of the English and Malay Languages. (K 89) 8vo. Singapore, 1854.
Linnean Society—Journal of Proceedings, No. 24. 8vo. 1862.
Lubbock, John, Esq. F.R.S. M.R.I. (the Author)—On the Antiquity of Man, &c. 8vo. 1862.
Mackie, S. J. Esq. F.G.S. (the Editor)—The Geologist for July, 1862. 8vo.
Manchester Literary and Philosophical Society—Memoirs. Third Series. Vol. I. 1862.
Proceedings. Vol. I. No. 15; and Vol. II. 1860-62.

Marcet, W. M.D. F.R.S. (the Author)—On Chronic Alcholic Intoxication. 2nd ed. 16to. 1862.
Medical and Chirurgical Society, Royal—Proceedings, Vol. IV. No. 2. 8vo. 1862.
Mechanical Engineers' Institution, Birmingham—Proceedings, April, July. 8vo. 1862.
Melbourne, University of, Council, Victoria, Australia—Catalogue of the Melbourne Public Library. 8vo. 1861.
Statistical Register of Victoria, and Calendar for 1855. By W. H. Archer. 8vo. 1854.
Catalogue of the Victoria Exhibition, with Prefatory Essays on the Progress of the Colony. 8vo. 1861.
The Victoria Government Prize Essays. 1860. 8vo. 1861.
Statistical Notes of the Progress of Victoria. By W. H. Archer. 4to. 1861.
Milberg, J. H. (the Author)—The Worthlessness of Iron-cased Ships. (L 13) 8vo. 1862.
Das Wahre Sonnensystem. (L 13) 8vo. 1862.
Newton, Messrs.—London Journal (New Series) for July to October, 1862. 8vo.
Packe, Edmund, Esq. M.R.I.—Charles Packe: Guide to the Pyrenees. With Maps, &c. 16to. 1862.
Petermann, A. Esq. (the Editor)—Mittheilungen aus dem Gesammtgebiete der Geographie. Nos. 6, 7, 8, 9. 4to. 1862.
Photographic Society—Journal, Nos. 123-126. 8vo. 1862.
Pratt, Henry F. M.D. (the Author)—On the Eccentric and Centric Force: a New Theory of Projection. 8vo. 1862.
Reddie, James, Esq. (the Author)—The Mechanics of the Heavens. (K 89) 8vo. 1862.
Roma, Accademia Pontificia de' Nuovi Lincei—Atti: Anno XIII. Sessioni 5, 6, 7. 4to. 1860.
Royal Society of London—Proceedings, Nos. 49, 50. 8vo. 1862.
Savory, W. S. Esq. F.R.S.—Descriptive Catalogue of the Anatomical Museum of St. Bartholomew's Hospital, 3 vols. 8vo. 1846-62.
Saxon Society of Sciences, Leipsic—Abhandlungen und Berichte. 4to and 8vo. 1862.
Sedgwick, J. Bell, Esq. M.R.I.—E. Schwarz: Anthropometrical Investigations. 4to. 1862.
Statistical Society—Journal, Vol. XXV. Part 3. 8vo. 1862.
Thrupp, Joseph W. Esq. M.R.I.—Rev. Joseph Francis Thrupp: Introduction to the Use and Study of the Psalms. 2 vols. 8vo. 1860.
United Service Institution, Royal—Journal, No. 23. 8vo. 1862.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, Marz zu Juni, 1862. 4to.
Volpicelli, Professor P. (the Author)—Sulla Electricità dell' Atmosfera. 2ª e 3ª Note. 4to. 1861.
Watson, J. Forbes, M.D.—Catalogue of the Contributions from India to the Exhibition of 1862. 4to. Calcutta. 1862.
Yates, James, Esq. F.R.S. M.R.I. (the Author)—On the Excess of Water in the Region of the Earth about New Zealand: its Causes and its Effects. (From Edin. Philos. Journ. Oct. 1862.) 8vo. 1862.

INDEX TO VOL. III.

END OF VOL. III.