PROCEEDINGS

OF

THE ROYAL SOCIETY

OF

EDINBURGH.

VOL. II.

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PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. II. 1844–5. No. 25.

Sixty-Second Session.

Monday, 2d December 1844.

Sir T. M. BRISBANE, Bart., President, in the Chair.

The following Communications were read:—

1. Account of the late Earthquake at Demerara. By W. H. Campbell, Esq. Communicated by M. Ponton, Esq.

2. On the Existence of an Electrical Apparatus in the Flapper Skate and other Rays. By James Stark, M.D., Fellow of the Royal College of Physicians, Edinburgh.

The author, after noticing the fishes in which an electrical apparatus had been discovered, stated that, with the exception of Geoffroy St Hilaire, no writer had endeavoured to shew that Skates possessed electrical organs. The organ to which that writer alluded, the author regarded as nothing else than muciparous ducts, identical with the very same organs in the Torpedo, and quite distinct from the electrical organs. The circumstance was then mentioned which directed the author's attention to the tail of the skate, and led him to suppose that an electrical organ might exist in it.

On removing the skin from the tail of a Flapper Skate, an organ was discovered occupying the place of the lateral muscles, extending from near the base to the very tip of the tail, and possessing all the anatomical characters of the electrical apparatus of fishes. It was about 14 inches in length, and about half an inch in diameter for nearly one-half of its length; was composed of columns or four-sided membranous tubes, about the twelfth of an inch in diameter, divided by cross membranous septa into compartments varying from the twentieth to the thirtieth of an inch in diameter. These compartments were filled with a transparent gelatinous matter. The columns were so arranged as to form layers, which, on a transverse section, were seen to have a concentric arrangement, but on a longitudinal section, were seen to form hollow cones placed one within the other. This arrangement allowed about three-fourths of the wide extremity of every cone-shaped layer to come in contact with the skin.

The microscope shewed the walls of the columns and their cross septa to be composed of dense fibrous and filamentous tissue, on which the blood-vessels and nerves were minutely ramified. The gelatinous matter which filled the compartments was seen to be composed of very minute cells, the walls of which were composed of a fine transparent membrane, over which the nerves were seen to ramify in the most beautiful manner. The interior of these very minute transparent cells was filled with a transparent jelly.

A rough chemical examination shewed the organ to be composed principally of gelatine, and in small part of albumen. No fibrin was observed in it.

The organ was supplied with nerves from three sources, viz., from the lateral branch of the eighth pair, and from the anterior and posterior caudo-spinal nerves.

This organ did not exist in equal development in every species of Ray. In most of them it was merely rudimentary, not being thicker than a crow-quill or common pen, and consisted of only four or five columns, similarly divided into cells by cross membranous septa.

The author formed the conclusion that this was an electrical apparatus from the following purely anatomical considerations, as he had not an opportunity of ascertaining the occurrence of electrical discharges from the organ by an examination of living specimens.

1. The structure of the organ was the same as that of the electrical apparatus of the Torpedo and Electrical Eel—consisting of membranous tubes or columns arranged in layers, each column being divided into distinct compartments filled with a gelatinous fluid.

2. All the layers of tubes communicated with the skin, and that skin was abundantly supplied with mucus.

3. The organ was supplied not only with spinal nerves, but with the descending branch of the eighth pair, which, along with a branch of the fifth pair, is the electrical nerve of the allied genus Torpedo ;

whereas, in other fishes, this nerve is solely distributed to the insignificant muscles which move the fin of the tail.

4. The microscopic structure is the same as that of the electrical organs of the Torpedo and Electrical Eel.

5. The chemical constitution is the same as that of the electrical organs of the Torpedo, consisting of gelatine and albumen, but no fibrin.

The paper was concluded with a few remarks on the probable uses to the Rays of an electrical organ; and by directing the attention of naturalists to the probable existence of electrical organs in many soft-bodied fresh and salt water animals, as Polypi, Asterias, &c.

3. Observations on the Comet, visible now or lately in the Constellation of the Whale. By C. Rumker, Esq. Communicated by Sir T. M. Brisbane, Bart.

The following Donations were announced:—

Transactions of the American Philosophical Society, held at Philadelphia, for promoting Useful Knowledge. Vol. ix., Part 1. —*By the Society,*

Bulletin de la Société Geologique de France. Tome xiii.—*By the Society.*

Memoirs and Proceedings of the Chemical Society. Parts 7, 8, 9. —*By the Society.*

Journal of the Statistical Society of London. Vol. vii., Parts 1, 2, 3.—*By the Society.*

Journal of the Asiatic Society of Bengal, 1843. No. 142.—*By the Society.*

Det Kongelige Danske Vindenskabernes Selskabs Naturvidenskabelige og Mathematiske Afhandlingar. Deels ix. and x.—*By the Society.*

A System of Mineralogy, comprising the most Recent Discoveries. By James D. Dana.—*By the Author.*

The Journal of Agriculture, and the Transactions of the Highland and Agricultural Society of Scotland, 1844, July and October. —*By the Society.*

Scheikundige Onderzoekingen, gedaan in het Laboratorium der Utrechtsche Hoogeschool. 2d Deel. 5d Stuk.—*By the Editors.*

Report of the Thirteenth Meeting of the British Association of the

Advancement of Science, held at Cork in August 1843.—*By the Association.*

The Eleventh Annual Report of the Royal Cornwall Polytechnic Society, 1843.—*By the Society.*

The Journal of the Royal Geographical Society of London. Vol. xiv., Part 1.—*By the Society.*

Proceedings of the Royal Astronomical Society. Vol. vi., Nos. 1–6. —*By the Society.*

The Electrical Magazine, conducted by Mr Charles V. Walker. Vol. i., No. 5.—*By the Editor.*

Journal of the Asiatic Society of Bengal. No. 143.—*By the Society.*

Journal of the Bombay Branch Royal Asiatic Society. Nos. 5 and 6. —*By the Society.*

Annales des Sciences Physiques et Naturelles, d'Agriculture et d'Industrie, publiées par la Société Royale d'Agriculture, &c., de Lyon. Tome vi.—*By the Society.*

Geologische Bemerkungen über die Gegend von Baden bei Rastadt. Von J. F. L. Hausmann.—*By the Author.*

Memoire sur le Daltonisme. Par Elie Wartmann.—*By the Author.*

Astronomische Nachrichten, herausgegeben, von H. C. Schumacher.—*By the Editor.*

Versuch einer objectiven Begrundung der Lehre von den drei Dimensionen des Raumes. Von Dr Bernard Bolzano.—*By the Author.*

Magnetische und Meteorologische Beobachtungen zu Prag. Von Karl Kreil—(vierter Jahrgang). *By the Author.*

Astronomical Observations made at the Royal Observatory, Greenwich, in the year 1842, under the direction of George Biddell Airy, Esq., M.A., Astronomer-Royal.—*By the Royal Society.*

Catalogue of the Places of 1439 Stars, referred to the 1st of January 1840; deduced from the Observations made at the Royal Observatory, Greenwich, from 1836, January 1, to 1841, December 31.—*By the Royal Society.*

Proceedings of the Geological Society of London. No. 98.—*By the Society.*

Memoires de la Société Geologique de France. (Deuxieme Serie). Tome i., Partie 1.—*By the Society.*

Sixth, Seventh, and Eighth Letters on Glaciers. By Professor Forbes.—*By the Author.*

Proceedings of the Zoological Society of London. Nos. 120 to 134.—*By the Society.*

Abhandlungen der Koniglichen Akademie der Wissenschaften zu Berlin—ans dem Jahre 1842.—*By the Academy.*

Bericht über die zur Bekantmachung geeigneten Verhandlungen der Konigl. Preuss. Akademie der Wissenschaften zu Berlin. Juli 1843 bis Juni 1844.—*By the Academy.*

Tijdschrift voor Natuurlijke Geschiedenis en Physiologie—Uitgegeven door J. van der Hoeven en W. H. De Vriese, M.D. Deel. xi. St. 2.—*By the Editors.*

Archief voor Geneeskunde—Uitgegeven door Dr J. P. Heije. Deel. iii, St. 4.—*By the Editor.*

Het Instituut of Verslagen en Mededeelingen, Uitgegeven door de vier Klassen van het koninklijk Nederlandsche Instituut van Wetenschappen, Letterkunde en Schoone Kûnsten over den Jare 1842, St. 4. 1843, St. 1, 2, 3.—*By the Royal Institute of Holland.*

Niewe Verhandelingen van het Bataafsch Genootschap, der Proefondervindelijke Wijsbegeerte te Rotterdam. Deel. ix., St. 1, 2, 3.—*By the Society.*

Memoire de l'Academie Imperiale des Sciences de Saint Petersbourg—(Sciences Politiques, &c.) Tome vi., Liv. 4, 5, 6. Tome vii., Liv. 1, 2, 3.

Ditto ditto, Sciences Mathematiques. Tome v., Liv. 4, 5, 6. Tome vi., Liv. 1.

Recueil des Actes de la Seance Publique de l'Academie Imperiale des Sciences de Saint Petersbourg, tenue le 29. Dec. 1843.—*By the Imperial Academy.*

Nouveaux Memoires de la Société Helvetique des Sciences Naturelles. Tomes i.–vi.—*By the Society.*

Actes de la Société Helvetique des Sciences Naturelles.—*By the Society.*

Verhandlungen der Schweirischen Naturforschenden Gesellschaft bei ihrer Versammlung zu Zurich, 1841.

Ditto ditto, zu Altdorf, 1842.—*By the Association.*

Specimens of Printing-Types in the Establishment of Neill & Co., Printers, Edinburgh.—*By Messrs Neill & Co.*

Comptes Rendus Hebdomadaires des Séances de l'Academie des Sciences. Tome xviii., Nos. 15–26, and Tome xix., Nos. 1–16.—*By the Academy.*

Maps of the Irish Ordnance Survey, containing the County of Limerick, 62 sheets.—*By the Lord Lieutenant.*

Monday, 16*th December* 1844.

SIR GEO. MACKENZIE, Bart., in the Chair.

The following Communications were read:—

1. On a Possible Explanation of the Adaptation of the Eye to Distinct Vision at Different Distances. By Professor Forbes.

The idea suggested in this paper, occurred to the author three years ago, from reflecting that the destruction of spherical aberration in the eye might be effected by a modification of the curvature of the lens, as well as by the variable density which it is known to possess, and which has usually been accounted for as intended for that purpose.

The author considering the probability to be almost infinite against the sphericity of the surfaces (a necessary evil in our instruments, but inexplicable in a natural organ), a conviction which he afterwards found to be reduced to certainty by experiments which have actually been made on the figure of the lens—he conceived that the variable density of this part of the eye must have some other cause. He considered it likely that it might contribute to the focal adjustment of the eye in the following way:—The lens is composed of coats more firm and tenacious, as well as more refractive, towards the centre, and less so at the sides. These coats are also nearly spherical in the centre, forming a nucleus of considerable resistance. Hence the author supposes, that if the lens be compressed in any manner by a uniform hydrostatic pressure, it will yield most readily in a plane at right angles to the axis of vision, and hence the lens will become more spheroidal, and, consequently, more refractive; that is, adapted for the vision of objects at small distances. The hydrostatic pressure in question is believed to be conveyed from the humours of the eye, between which the lens is delicately suspended, and to originate in the compressing action of the muscles which move the eye-ball acting simultaneously on the tough sclerotic coat.

The author thus sums up the evidence which he thinks gives probability to this explanation:—

1. The form of the surfaces of the lens might have been such as to correct aberration without any variation of density whatever. But, on the contrary, it has a form which exaggerates the ordinary spherical aberration. A form which, therefore, appears to be

adapted to the rapid variation of density in the lens, which must, therefore, be presumed to have some distinct mechanical utility.

2. The effort to view near objects is accompanied in most, if not all persons, by a sensible muscular effort.

3. This theory is free from the various conclusive objections urged by Dr Young against all explanations which do not turn upon a change of figure of the lens; and it is also free from the difficulties to the admission of Dr Young's theory,—the muscularity of the lens itself.

4. When the lens is reproduced, after the operation for cataract, the power of adjustment is greatly diminished or wholly lost, since the variable elasticity will be wanting.

5. The diminution of the adjusting power of the eye in old age is explained by the increased rigidity of the lens, and consequent incompressibility.

6. The crystalline lens polarizes light in a manner similar to that exerted by glass and other uncrystallized substances in a state of constraint, that is, possessing unequal elasticity in different directions.

2. Notice of an Ancient Beach near Stirling. By Charles Maclaren, Esq.

This beach presents the appearance of a terrace, extending along the north side of the Carse of Stirling, from the foot of Abbey Craig westward to Lecropt Church, and beyond it. Its length is about two miles; its breadth, at Lecropt Church, is about 200 feet, at Airthrey Mineral Well 900 feet, and at Airthrey Castle near half a mile. Its upper surface is nearly a dead level in both directions, except where breaches have been made in it, by streams or other agents. The elevation of this surface above the Carse in about 85 feet, and above the Forth at Stirling probably 110 feet. Rock is seen under the terrace at the village of Causeyhead, in the bed of the River Allan, at the Bridge, and at the bottom of the acclivity under Lecropt Church. Everywhere else the terrace seems to consist of stratified sand and gravel, constituting a mass similar in form and materials to our present beaches. Its preservation may be attributed to the high rock of Abbey Craig, which protected it from the action of the ancient tides when the sea stood at a considerably higher level than it does at present. A remnant of a similar beach is seen on the south side of the Carse, about a mile southwest from Stirling. It consists of a hill of alluvial matter, a furlong south from Whitehouse Farm, and about 70 or 80 feet in height. A small portion of a lower terrace about 30 or 35 feet above the Carse, is found at the southwest foot of the rock of Stirling Castle; and a portion of a terrace of the same height is seen in connection with the higher terrace, extending for a short distance eastward of Lord Abercromby's south gate.

The following Donations were announced:—

Annuaire de l'Academie Royale des Sciences et Belles Lettres de Bruxelles, 1844.

Bulletin de l'Academie Royale de Bruxelles. Tome x., Nos. 8–12. Tome xi., Nos. 1–8.

Memoires Couronnés et Memoires des Savants Etrangers, publiés par l'Academie Royale des Sciences et Belles Lettres de Bruxelles. Tome xvi.

Annales de l'Observatoire Royale de Bruxelles, publiées par le Directeur, A. Quetelet. Tome iii.—*By the Academy.*

Annuaire de l'Observatoire Royale de Bruxelles, par A. Quetelet, Directeur de cet Etablissement, 1844.—*By the Author.*

Recherches Statistiques, par A. Quetelet.—*By the Author.*

Notices sur Pierre Simons, Alexis Bouvard, et Antoine Reinhard Falck, par A. Quetelet.—*By the Author.*

Bulletin de la Société Géologique de France. Tome i. Feuilles 28–33.—*By the Society.*

Novi Commentarii Academiæ Scientiarum Instituti Bononiensis. Vols. i., ii., iii., iv., v.—*By the Academy.*

Opere Edite et Inedite del Professore Luigi Galvani, raccolte e pubblicate per cura dell' Accademia delle Scienze dell' Institute di Bologna.—*By the Academy.*

On the Excision of the Eyeball in cases of Melanosis, Medullary Carcinoma, and Carcinoma, with Remarks by J. Argyll Robertson, M.D., F.R.S.E.—*By the Author.*

Monday, 6th January 1845.

Sir T. M. BRISBANE, Bart., President, in the Chair.

The following Communications were read:—

1. Farther Remarks on the Electrical Organs of the Rays. By Dr Stark.

2. Observations on the same subject. By John Goodsir, Esq.

3. Note on the Form of the Crystalline Lens. By Professor Forbes. Included in the former Abstract.

4. On the Cause which has produced the Present Form and Condition of the Earth's Surface. By Sir George Mackenzie, Bart.

The author first described, generally, the appearance of the loose materials covering the surface; and referred to some districts in the north of Scotland, especially the central one on the borders of Perth and Inverness shires, as demonstrating the effects of vast currents of water having passed over the surface. He also referred to the valleys of the river Conan and its tributaries, in Ross-shire, as presenting an epitome of all the phenomena which may be supposed to result from a vast flood gradually subsiding, and taking the direction of the valleys. He then shortly alluded to the theories proposed to account for the present condition of the surface, which appear to resolve themselves into the effects of a remote cause.

It being generally admitted that the crust of the earth now appears broken, some portions having been elevated, and some having sunk; and that this breaking up of the strata, causing them to take various positions, the broken portions being inclined at different angles, some being vertical; and that this dislocation of the strata is observed everywhere; it is obvious that a tremendous force must have been exerted to produce these effects. The elevation of the former ocean bottom, and the sinking of much of the former land, would occasion an agitation of the waters such as would have caused the waves to overtop the mountains; and as the waves subsided currents would have been directed with great violence through all the valleys, sufficient to produce all the phenomena we observe, except those which may be fairly attributed to a subsequent gradual rising of the land, and to partial convulsions.

Sir George concluded by observing that, probably, Man had not appeared on the earth previous to the great convulsion by which the order of the strata had been so greatly disturbed; for, besides no human fossil remains having yet been found, man, without such disturbance of the rocks, could not have enjoyed what external nature offers to his senses, nor have discovered the minerals and organic remains which have contributed so much to his wealth and comfort, as well as to the noblest exercise of his mental faculties.

The following Candidates were duly elected Fellows of the Society :—

Dr James Andrew, F.R.C.P.
Dr Geo. Wilson, Lecturer on Chemistry.

The following Donations were announced :—

The Journal of Agriculture, and the Transactions of the Highland and Agricultural Society of Scotland, for January 1845.—*By the Society.*

Arsberättelse om Zoologiens Framsteg under ären 1840–42. Af S. Loven.

Arsberättelse om Framstegen i Kemi och Mineralogafgiven, den 31 Mars 1844. Af Jac. Berzelius.

Arsberättelse om Botaniska Arbeten och Upptackter for är 1838. Af J. E. Wikstrom.

Kongl. Vetenskaps-Academiens Handlingar, for är 1842.

Ofversight af Kongl. Vetenskaps-Academiens Forhandlingar, 1844. Nos. 1 to 7.—*By the Academy.*

The Journal of the Royal Asiatic Society. No. 15, Parts 1, 2.—*By the Society.*

Observations Meteorologiques faites à Nijne-Taguilsh (Monts Oural) Government de Perm. Annee 1842.

Mittlere Oerter von 12,000 Fix-Sternen, von Carl Rumker. Part 1, containing pp. 1–47.—*By Sir Thomas Brisbane.*

Monday, 20th January 1845.

The Right Reverend Bishop TERROT, V. P., in the Chair.

The following Communications were read :—

1. Some Account of the Magnetic Observatory at Makerstoun, and of the Observations made there. By J. A. Broun, Esq. Communicated by Sir T. M. Brisbane, Bart.

The observatory is situated on a rising ground forming the left bank of the Tweed, and is at a distance of about fifty yards from the Astronomical Observatory. It is built of wood ; copper nails were used, and all iron carefully excluded from the building.

The plan of the observatory is rectangular, 40 feet long by 20 broad : it is divided into one large room to the north, 40 feet by 12, and two ante-rooms to the south, with the lobby and entrance doors between.

The magnetometers and telescopes in the observatory are placed on stone pillars of about 20 inches diameter, having good foundations, and being completely disconnected with the floor, so that no tremor can be communicated to the instruments by walking past them.

The instruments in the observatory are of two classes, Magnetical and Meteorological.

Belonging to the former class, there are, 1. The Declination Magnetometer, used for determining the variations and absolute values of the angle formed by the astronomical and magnetic meridians. 2. The Bifilar Magnetometer, by means of which the variations of the horizontal component of the earth's magnetic force are obtained. 3. The Balance Magnetometer, which gives the variations of the vertical component. 4. The Inclinometer, used for obtaining the magnetic dip, or the angle formed by the direction of the earth's total intensity, and a horizontal plane, namely, in the magnetic meridian. 5. An extra Declinometer in a small wooden building, at a distance from the observatory, is used for obtaining the *absolute* value of the horizontal component of the earth's magnetic force, according to the method of Gauss.

The second class of instruments is the Meteorological.

These are,—1. The Barometer, a standard by Newman, similar to that belonging to the Royal Society. The tube is 0·552 inches in diameter.

2. Thermometers. The dry and wet bulb thermometers, by Messrs Adie and Son, are placed on a revolving wooden frame opposite one of the north windows. They are always read from within the observatory, the frame being moved by means of cords and pullies. Maximum and minimum register thermometers, by Adie and Son, are placed on the north side of the observatory. Self-registering black bulb thermometers, for solar and terrestrial radiation, are placed in an enclosed space at a distance from the observatory.

3. Anemoscope and Anemometer.

4. The Rain-Gauge.

In 1841 and 1842, daily observations were made at the Göttingen hours of 8 and 11 A.M., and 2 and 5 P.M.; in 1843 at 6, 8, 10, A.M.; Noon; 2, 4, 6, 8, and 10, P.M. Another observation was made at 11 A.M., after October 1843; in 1844, at every hour of the twenty-four; and this system is being continued in 1845.

The instruments observed at these hours were, the declination, the horizontal force, and vertical force, magnetometers,—the barometer, its height and temperature,—the dry and wet bulb external thermometers, and the thermometers of the two force magnetometers. To these were added observations of the weather.

The magnetic inclination, or dip, has been observed since the commencement, with a few exceptions, twice a-week.

Since July 1841, the monthly periods, named Terms, have been regularly kept. The terms are periods of twenty-four hours, which occur once a-month, on days previously agreed upon, and during which simultaneous observations of the magnetometers are made in all the magnetic observatories. At present, the three magnetometers are observed every five minutes of the 24 hours, meteorological observations being made hourly. In this way, each term produces 388 observations of each magnetometer, comparable with the observations made at the same instant in the other observatories.

Finally, observations for the absolute declination and absolute horizontal intensity of the earth's magnetism have been made as regularly as possible at different periods of the year.

The second class of observations is irregular, and comprise all those observations of phenomena which are irregular in their periods. The most important of this class are the extra magnetic observations. Whenever the magnets are found to have assumed positions differing unusually from those at the previous observations, they are watched, and if found to be moving, extra observations are immediately commenced. When the disturbance is moderate, the three instruments are observed every five minutes, as on term days; if it be considerable, an instrument is observed every minute.

Much attention has been paid to the magnetic disturbances in 1844. Upwards of 60,000 extra-readings of the magnetometers having been made in that year, giving about 25,000 mean positions.

The aurora borealis is as carefully observed as due attention to the magnetic disturbance with which it has here been found invariably accompanied, will allow. Attention is paid to the measurement and description of halos, parhelia, and paraselenæ.

The reductions are at present in a considerable state of forwardness; and it is expected that the volume for 1843 will be ready to place in the printer's hands as soon as that for 1841 and 1842 is issued.

2. Description of a Sliding Scale for Facilitating the Use of the Moist-bulb Hygrometer. By James Dalmahoy, Esq.

The instrument described in the paper is made of German silver, and is about a foot in length, and $\frac{3}{10}$ of an inch in breadth; along the middle of it there is a groove for a slider. On the right edge of the groove is engraved a scale of inches, and on the left the degrees of temperature from 0° to 85° Fahrenheit, each being placed exactly opposite that point of the scale of inches which measures the corresponding tension of vapour. On the left edge of the slider is engraved a scale of equal parts, each $\frac{10}{87}$ of an inch; on the right edge, and having the same zero, is a vernier, applicable to the scale of inches. The lines on these scales are ten times larger than those which the symbols in the dew-point formula represent, but their numerical designations are not changed.

The instrument is to be used as follows:—Find on the slider the number which expresses the difference between the indications of the dry and moist bulb thermometers, and bring it opposite the number on the left scale, denoting the temperature of the moist-bulb; then zero on the slider will indicate, on the left scale, the temperature of the dew-point; and on the right scale, the corresponding force of vapour.

The paper concludes by shewing that the ordinary hygrometric formula, which suggested the idea of the sliding scale, indicates also a geometrical construction for finding the temperature and tension of vapour at the dew-point, which, however, would not be practically applied with convenience.

3. Account of Experiments to Measure the Direct Force of the Waves of the Atlantic and German Oceans. By Thomas Stevenson. Communicated by David Stevenson, Esq.

The author has attempted to supply a great desideratum in the practice of marine engineering, by instituting a series of experiments to ascertain what force the waves exert against opposing barriers.

For this purpose he suggests, in some peculiar situations, the use of columns of water or of air, by which the force of each wave can be ascertained; in the one case by the rise of the water-column, or in the other by a pressure-gauge, shewing the same result in atmospheres by compression. But in all the observations as yet made he has used an instrument which may be termed a "*self-registering*

Marine Dynamometer." This dynamometer is contained in an iron cylinder, which is fixed to the rock where the experiments are to be made. The instrument consists of a plate or disc attached to a powerful spring, which is lengthened by the action of the waves. In graduating the instrument, the pressure required to lengthen the spring, a given quantity is ascertained by loading the disc with weights, so that when the quantity that the spring has yielded by the action of the sea is known, the pressure due to the area exposed is known also. The discs employed varied from 3 to 9 inches in diameter, and the resistance of the springs from about 10 lb. to about 50 lb. for every $\frac{1}{8}$ inch of elongation. With a view to check the results, three instruments, of very different powers of springs, were besides placed each other on an exposed rock, for a space of about six months, and the results were found to be remarkably concordant As the action of a wave may be supposed to combine the effects of a sudden impact with a subsequent continuous pressure, an objection might be urged against estimating these effects statically; and the author has accordingly made some remarks relative to this subject, which it is not, however, necessary here to state.

The results obtained are, up to this date, 260 in number, and these embrace a continuous register of the agitations of the Atlantic (as ascertained at the Skerryvore Rocks, Argyllshire,) for the last 22 months; together with a later train of similar observations, on the German Ocean, made at the Bell Rock Lighthouse. The following is a digest of the results obtained :—

Atlantic Ocean.

Average of results for 5 *summer* months, during the years 1843 and 1844, is 611 lb. per square foot.

Average of results for 6 *winter* months during the same years is 2086 lb. per square foot, or thrice as great as in the summer months.

Greatest result yet obtained at Skerryvore, being on the 20th December 1844, is 4335 lb. per square foot.

German Ocean.

Greatest result yet obtained at the Bell Rock, being on the 9th October 1844, is 3013 lb. per square foot.

The greatest effect of the sea, which has been observed, is, therefore, that of the Atlantic, which is *equal to about* 2 *tons per square foot.*

There are also a few observations (made in April and June 1842) upon the Irish Sea, on the coast of Kirkcudbright, but the weather was unfavourable for such observations; the highest result was 840 lb. on a square foot. The communication concludes with an account of several instances of the effects of the waves in the elevation of spray, and in the transportation of heavy masses of rock. The greatest observed elevation of spray was at the Bell Rock Lighthouse, on the 20th November 1827, during a calm with a ground swell. On this occasion the spray was projected to the height of 106 feet, which shews the existence, on the large scale, of a pressure of about 3 tons. The pressure which projected this column of spray exceeds, therefore, the greatest result obtained by the Marine Dynamometer. The largest stone that is mentioned in the paper, as having been moved by the sea, is 42 tons weight. This stone, which is on the shores of one of the Hebrides, *was seen* to move under the influence of each wave.

4. A Verbal Communication in regard to Chevalier's Experiments on the Decomposition of certain Salts of Lead by Charcoal. By Dr Traill.

The following Donations were announced:—

Journal of the Royal Asiatic Society of Bengal. Nos. 144, 145.—*By the Society.*

On the Nature of the Nervous Agency. By James Stark, M.D., F.R.S.E.—*By the Author.*

Researches on the Brain, Spinal Cord, and Ganglia, with Remarks on the Mode by which a continued flow of Nervous Agency is excited in, and transmitted from, these organs. By James Stark, M.D., F.R.S.E.—*By the Author.*

Philosophical Transactions of the Royal Society of London for 1844. Part ii.

Proceedings of the Royal Society of London. No. 59.—*By the Royal Society.*

Magnetical and Meteorological Observations made at the Royal Observatory, Greenwich, in the year 1842, under the direction of George Biddell Airy, Esq., M.A., Astronomer-Royal. —*By the Royal Society.*

Outlines of Chemistry for the use of Students. By William Gregory, M.D., Professor of Chemistry in the University of Edinburgh.—*By the Author.*

Monday, 3d February 1845.

Sir T. M. BRISBANE, Bart., President, in the Chair.

The following Communications were read :—

1. On a Peculiar Modification of the Doubly Refracting Structure of Topaz. By Sir D. Brewster, K.H.

While examining, in polarised light, some of the crystals which he had discovered in Topaz, the author observed certain optical phenomena, depending on a peculiarity of structure. This peculiarity is manifested either in the depolarisation of light, when it gives rise to four quadrants of light, separated by the radii of a black rectangular cross similar to the central portion, or the tints of the first order in the uniaxal system of polarised rings, or in the unequal refraction of common light, which gives rise to the mirage of a luminous point, in the form of concentric circles surrounding the centre of force. In every case there was found a quadrangular cavity in the centre of the intersection of the cross, generally dark and opaque, but in one case having a luminous spot in the centre. These cavities are from the $\frac{1}{3000}$ to the $\frac{1}{4000}$ of an inch in diameter.

These cavities are quite distinct from all those formerly described by the author ; and from the phenomena above described, he concludes that the contents of each cavity have exerted an elastic force on the surrounding mineral while in a plastic state. In some cases fissures are seen proceeding from the central cavities, but these are supposed to have been produced after the mineral had become indurated, and had already been subjected, in the plastic state, to the pressure or force above indicated.

These cavities never accompany the cavities with two fluids, but occur in specimens containing numerous embedded crystals, differing little from Topaz in refracting power.

Since the mineral must have been plastic when it yielded to the pressure here noticed, it cannot have been formed by the aggregation of molecules having the primary form of the crystal.

These considerations, along with others connected with the crystals, which occur in the cavities of Topaz, have led the author to adopt the idea of a new and peculiar kind of crystallization, to which he will soon direct attention.

2. Extracts from Letters to the General Secretary, on the Analogy of the Structure of some Volcanic Rocks with that of Glaciers. By C. Darwin, Esq., F.R.S. Specimens were exhibited. With Observations on the same subject, made by Professor Forbes.

"I take the liberty of addressing you, knowing how much you are interested on the subject of your discovery of the veined structure of glacier ice. I have a specimen (from Mr Stokes's collection) of Mexican obsidian, which, judging from your description, must resemble, to a considerable degree, the zoned ice. It is zoned with quite straight parallel lines, like an agate; and these zones, as far as I can see under the microscope, appear entirely due to the greater or lesser number of excessively minute, flattened air cavities. I cannot avoid suspecting that in this case, and in many others, in which lava of the trachytic series (generally of very imperfect fluidity) are laminated, that the structure is due to the stretching of the mass or stream during its movement, as in the ice-streams of glaciers. * * *

"If the subject of the lamination of *volcanic* rocks should interest you, I would venture to ask you to refer to p. 65–72 of my small volume of 'Geological Observations on Volcanic Islands.'* I there

* The laminated, volcanic rocks of Ascension, consist, as described by Mr Darwin, of excessively thin, quite parallel layers of minute crystals of quartz (determined by Professor Miller) and diopside; of atoms of an oxide of iron, and of an amorphous, black angitic mineral; and, lastly, of a more or less pure feldspathic stone, with perfect crystals of feldspar placed lengthways. The following is a portion of the passage referred to:—" Several causes appear capable of producing zones of different tension in masses semiliquified by heat. In a fragment of devitrified glass I have observed layers of sphærulites, which appeared, from the manner in which they were abruptly bent, to have been produced by the simple contraction of the mass in the vessel, in which it cooled. In certain dykes on Mount Ætna, described by M. Elie de Beaumont, as bordered by alternating bands of scoriaceous and compact rock, one is led to suppose that the stretching movement of the surrounding strata, which originally produced the fissures, continued, whilst the injected rock remained fluid. Guided, however, by Professor Forbes's clear description of the zoned structure of glacier ice, far the most probable explanation of the laminated structure of these feldspathic rocks appears to be, that they have been stretched, whilst slowly flowing onwards in a pasty condition, in precisely the same manner, as Professor Forbes believes, that the ice of moving glaciers is stretched and fissured. In both cases, the zones may be compared to those in the finest agates; in both, they extend in the direction in which the mass has flowed, and those exposed on the surface are generally vertical. In the ice, the porous laminæ are rendered

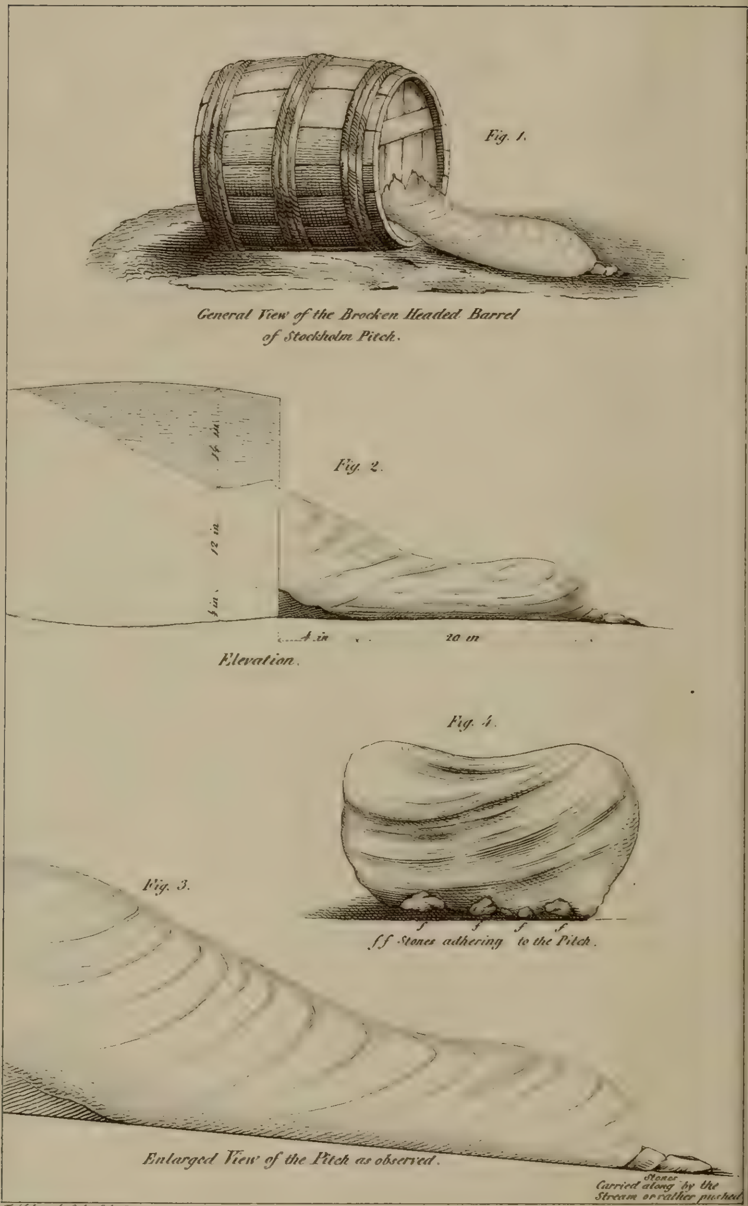
throw out the idea, that the structure in question may perhaps be explained by your views on the zoned structure of glacier ice, the layers of less tension being, in the case of the Ascension obsidian-rocks, rendered apparent, chiefly by the crystalline and concretionary action superinduced in them, instead of, as in zoned ice, by the congelation of water. * * *

"How singular it at first appears, that your discoveries in the structure of glacier ice should explain the structure, as I fully believe they will, of many volcanic masses. I, for one, have for years been quite confounded whenever I thought of the lamination of rocks which have flowed in a liquified state. Will your views throw any light on the primary laminated rocks? The laminæ certainly seem very generally parallel to the lines of disturbance and movement. Believe me, &c. C. DARWIN."

To Professor FORBES.

Professor Forbes confirmed the previous remarks by others, made by himself on the specimens transmitted to him by Mr Darwin, and on specimens from Lipari and Iceland in the collection of the Royal Society, as well as by direct observations made by himself on the lava streams of Ætna.

distinct by the subsequent congelation of infiltrated water; in the stony feldspathic lavas by subsequent crystalline and concretionary action. The fragment of glassy obsidian in Mr Stokes's collection, which is zoned with minute air-cells, must strikingly resemble, judging from Professor Forbes's description, a fragment of the zoned ice; and if the rates of cooling and the nature of the mass had been favourable to its crystallization, or to concretionary action, we should here have had the finest parallel zones of different composition and texture. In glaciers, the lines of porous ice and of minute crevices seem to be due to an incipient stretching, caused by the central parts of the frozen stream moving faster than the sides and bottom, which are retarded by friction. Hence, in glaciers of certain form, and towards the lower end of most glaciers, the zones become horizontal. May we venture to suppose that, in the feldspathic lavas with horizontal laminæ, we see an analogous case? All geologists who have examined trachytic regions have come to the conclusion, that the lavas of this series have possessed an exceedingly imperfect fluidity; and as it is evident that only matter thus characterized would be subject to become fissured, and to be formed into zones of different tensions, in the manner here supposed, we probably see the reason why augitic lavas, which appear, generally, to have possessed a higher degree of fluidity, are not, like the feldspathic lavas, divided into laminæ of different composition and texture. Moreover, in the augitic series, there never appears to be any tendency to that kind of concretionary action, which, we have seen, plays an important part in the lamination of rocks of the trachytic series, or, at least, in rendering that structure apparent."



Fr. Schenck Lith. Edinr.

3. Professor Forbes then read the following Letter from Professor Gordon, of Glasgow, also on the subject of the Viscous Theory of Glaciers.

GLASGOW, *January* 31. 1845.

* * * When you requested me to give you a memorandum of what appeared to me to be the *very glacier-like motion* and appearance of Stockholm pitch flowing from a barrel, I considered my observation to have been too casual to be worth writing, and having foreseen that I could arrange an experiment at Gateshead in the beginning of the year, I delayed giving you the memorandum you wished. I had hoped to have been able to inspect and report on my experiment about this time ; but I cannot go to Gateshead for some time to come, nor have I had any report of the progress of my pitch glacier since the 6th January, when I was informed it had not moved since the day after I left it, on the 28th December. Your note of yesterday induces me to offer you the following still perfectly vivid impressions of the analogy between *ice* and *Stockholm pitch.*

Allow me, in the first place, to mention that I read your travels in the Alps, in May last. That on the 24th of June I spent almost 20 hours on the glaciers of the Grindewald. I went up by the lower glacier, prepared with poles to prove the motion, and actually observed a progress of about 12 inches in the course of 13 hours, from 6 A.M. to 7 P.M. I traced the "dirt bands" on the surface. I was let down into several crevasses, one of them to a depth of 30 feet and could trace the *slaty structure* of the ice. The alternate clear blue thin veins, and the transition to opaque grey or even white. I descended from the glacier with a much better appreciation of the theory of glaciers than I had had, and a strong conviction that the facts I had observed, could not be otherwise accounted for than by the mechanical theory you have given. In passing through Gateshead in August, a broken headed barrel of Stockholm pitch at the Wire Rope Factory, attracted my attention. Its general appearance is represented in Fig. 1.

A mass of Stockholm pitch *broken* from a barrel in August (at the time of the observations I am about to mention) presented a dark-brown colour, a glassy lustre, translucent edges. The substance is fragile, fracture conchoidal, and very uniform. A mass, Fig. 4., which was brought to me by the workman having charge of this department, and which he had broken from the end of such a *stream* as I have represented coming from the barrel, presented generally the same appearance as a mass broken from an entire barrel,* but had this remarkable peculiarity, that there were lines —structural lines, *a a a a*—whose texture and colour were different from the general colour of the mass recognisable on such points as *b b b*, between any two such structural lines.

Fig 2. is an elevation of the stream of pitch, shewing pretty nearly the dimensions and outward appearance of the stream. The striated *slaty* structure appears here on the outside, as is more distinctly (intended to be) shewn in Fig. 3. There were certain well-defined lines, and on either side of these for some little distance, other small lines or cracks (but not *open* cracks or fissures), and then a space of smooth glassy-looking pitch.

I am strongly impressed with the idea, that the structural lines are a *result of the motion*, and that they correspond with the *veins* of glaciers. The lines incline most when the surface is steepest, as at *h*, Fig. 3., and are very faint and nearly horizontal at *i*, where the surface of the stream is nearly so too. I left Gateshead without having an opportunity of getting a sectional view of this stream. I can get no real Stockholm pitch in Glasgow, else I should have made the experiment you have incited me to attempt here. I am, &c.

LEWIS GORDON.

To Professor FORBES.

The following Candidates were duly elected Fellows of the Society:—

Dr John Burt, F.R.C.P., Edinburgh.

Dr Thomas Anderson, Lecturer on Chemistry.

The following Donations were announced:—

The Electrical Magazine, conducted by Mr Charles V. Walker, for October 1844.—*By the Editor.*

Memoir of Francis Baily, Esq., D.C.L., Oxford and Dublin. By Sir John F. W. Herschel, Bart.—*By the Royal Astronomical Society.*

Inest de Stella Lyræ variabili Disquisitio. Per F. G. A. Argelander.—*By the Royal Astronomical Society.*

Description of Bones, &c., found near the River Ohio, 1786, with an Engraving, and Observations on the Annual passage of Herrings. By Mr John Gilpin. From the Columbian Magazine, December 1786.—*Anonymous.*

Three Volumes in the Chinese Character on Astronomy and Geography.—*By Professor Forbes.*

* The pitch is *fragile* at the same time that it *flows*.—L.G.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. II. 1844–5. No. 26.

Sixty-Second Session.

Monday, 17th February 1845.

Right Reverend BISHOP TERROT in the Chair.

The following Communications were read:—

1. On the Existence of peculiar Crystals in the Cavities of the Topaz. Part I. By Sir D. Brewster.

2. On the Use of Colourless Ink in Writing. By Sir George Mackenzie.

Many years ago, the author had attempted to separate the component parts of common ink, with the view to get rid of its inconvenience in soiling everything with which it came in contact, by committing some of the parts to paper, and some to the pen. Working with solutions, he found that, in all his trials, the paper was, sooner or later, discoloured more or less, so as to unfit it for the market, and he abandoned the attempt. He afterwards tried salts of iodine, but failed to fix the colour which they yielded. After another interval, the subject again recurring, he was led, by an accidental circumstance, to think he might attain the long sought-for object by using dry powders for the paper, instead of solutions. The first trial satisfied him that he was, at length, to succeed; and, after persevering a considerable time, he brought certain processes so far as to yield good paper. With an almost colourless ink, prepared with permuriate of iron, traces were instantaneously produced, dark enough for ordinary purposes. The powder introduced into the machine for preparing the paper, is compounded of galls, anhydrous ferro-prussiate of potassium, and carbonate of lime, so diluted with rice flour, that enough, and no more of the powder than enough, remains among the fibres of the paper. The paper is sized before being passed through the machine, and is afterwards finished in the usual manner. Specimens of different qualities of paper were laid on the table, and written on with the colourless ink by the members present.

3. On the Use of Metallic Reflectors for Sextants, and on the Determination of the Errors arising from Non-Parallelism in the Mirrors and Sun-Shades of Reflecting Instruments. By John Adie, Esq.

The object of this communication was to shew that, by the use of metallic reflectors for sextants, greater accuracy was obtained in the observed angles, and also that larger angles could be observed. Objects were seen reflected by metallic mirrors, which could not be seen when glass was used; and that when the alloy was formed of pure metals, it was not subject to rust or tarnish, even when exposed to action of the sea air.

The author then exhibited a sextant fitted with these mirrors, which had been employed for a season in the survey of the north coast of Scotland, under Mr Mossman, and read extracts of letters from that gentleman.

In the second part, he describes methods by which the non-parallelism in mirrors and shades may be determined with great accuracy, before they are applied to reflecting instruments.

A Ballot then took place for the following Candidates, recommended by the Council at last Meeting for filling places in the Foreign Honorary list:—

MM. Cauchy,........................ Paris,
Charpentier, ... Bex,
Ehrenberg, ... Berlin,
Elie de Beaumont,............ Paris,
Guizot, ... Paris,
Hansteen, ... Christiania,
Jacobi, ... Königsberg,
Lamont, ... Munich,

Liebig, ... Giessen,
Melloni, ... Naples,
Neander, ... Berlin,

all of whom were declared to have been duly elected Foreign Honorary Members of the Society.

The following Donations were presented :—

List of Specimens of Birds in the Collection of the British Museum. Parts 1. and 3. Accipitres, Gallinæ, Grallæ, and Anseres.

List of the Specimens of Lepidopterous Insects in the Collection of the British Museum. Part 1.

List of the Specimens of Myriapoda in the Collection of the British Museum.

Catalogue of the Tortoises, Crocodiles, and Amphibænians, in the Collection of the British Museum.—*By the Trustees of the British Museum.*

The Electrical Magazine, conducted by Mr Charles W. Walker. Vol. I. No. 7.—*By the Editor.*

Tijdschrift voor Naturlijke Geschiedenis en Physiologie—Uitgegeven door I. van der Hoeven, M.D. en W. H. De Vriese, M.D. Deel XI. Stuks 3, 4.—*By the Editors.*

Cast of the Bust of the late Professor Playfair, which was executed by the late Sir Francis Chantrey.—*By Sir George Mackenzie, Bart.*

Fifteenth Report of the Scarborough Philosophical Society.—*By the Society.*

Monday, 3d *March* 1845.

Sir T. M. BRISBANE, Bart., President, in the Chair.

The following Communications were read :—

1. On the Existence of peculiar Crystals in the Cavities of Topaz. Part II. By Sir D. Brewster, K.H.

The author, after alluding to his former papers on the fluids in topaz, described, in Section I., the form and position of the strata in the minerals in which the cavities occur. They generally occur in immense numbers, occupying extensive strata, and injuring the transparency of the mineral. These strata occupy every possible position, and have every possible curvature; their shape is equally irregular; and it is probable that, in every case, some edge or angle of the stratum touches the surface. The cavities are sometimes concentrically arranged, and sometimes occur in parallel straight lines. In one specimen, they radiate from a centre. When different strata occur in the same specimen, they generally differ in the character of the cavities, one stratum containing flat, another deep cavities, &c. The whole facts lead to the conclusion, that the strata of cavities have been formed under the influence of forces propagated through a plastic mass, carrying with them gases and vapours, which came to a position of rest previous to the crystallization of the mineral. In Section II. the author describes some new observations on the two fluids formerly discovered by him. In some cases of flat cavities, the faces of which are parallel to the planes of easy cleavage, the application of heat forced the fluid between the laminæ of the crystal to a distant part. In one specimen, a white ball was seen to be projected from one cavity to the edge of the specimen, as in a case formerly described. In other specimens, where the dense fluid was accompanied by a bubble of some gas, the application of heat increased the size of the bubble, which then threw off a smaller to a distant spot. On cooling, the latter disappeared, and the former recovered its original size. Apparently the gas was here absorbed by the liquid on cooling. While the bubble expanded by heat, the liquid was forced into minute tubes or slits, from which, on cooling, it returned. In Section III. he described the form and position of the crystals in the cavities of topaz. They are both fixed and moveable, and often beautifully formed. They are very numerous, and occur in several different forms, which are enumerated, a very frequent one being the cube. In Section IV. he treated of the physical properties of these crystals. Many of them melt or dissolve in the fluid in a gentle heat, others with difficulty, some not at all. Those which melt are commonly reproduced, on cooling, of their original form, with modifications. The tessular crystals have no action on polarized light; but there seem to be two substances in this form, as some tessular crystals melted, while others were found infusible in any heat used. The doubly refracting crystals would appear to be of three kinds, as some melted easily, others with difficulty, others not at all. They did not depolarize white light, or the highest order of colours. One crystal melted, and was reproduced, without any fluid being present. In another cavity, several crystals, when heated, darted across the cavity, while others rotated rapidly round their middle point. Too strong a heat often bursts the cavities, separating the laminæ of the topaz. The volatile fluid, escaping, leaves a crystalline residue; the dense fluid disappears entirely, and is probably a condensed gas. In one specimen, the faces of the cavities formerly filled with the volatile fluid, are corroded, as if by a solvent, developing crystalline structure; an appearance analogous to that which has been observed on the external surface of topazes. In Section V. the author described crystals embedded in the mass of the topaz, some of which can only be detected by polarized light, as they do not affect the transparency of the crystal by common light. He concluded by describing cavities lined with a doubly refracting crust or shell, with optical and crystallographic axes—a phenomenon which has no parallel in mineralogy. These cavities have the *appearance* of embedded crystals, but are detected by depolarizing a uniform tint with a variable thickness of crystal.

2. On the Extraction of pure Phosphoric Acid from Bones, and on a New and Anomalous Phosphate of Magnesia. By Dr Gregory.

The author, after explaining the methods hitherto proposed for purifying the phosphoric acid of bone earth, and pointing out their inconveniences, mentioned that the chief difficulty was the separation of the magnesia always present in bone earth. The lime, it has been for some time known, may be entirely removed by means of sulphuric acid. In repeating the process of Liebig, which did not succeed in his hands, and which requires the use of alcohol, he found that, after the separation of the lime, there is obtained, by evaporation and heating, a clear and colourless glass, containing all the magnesia: that this glass dissolved completely by boiling in water; but that the solution, if again evaporated and heated to 600° for a quarter of an hour in a platinum capsule, became turbid, and deposited the whole magnesia as an insoluble salt. When water was added, so as to dissolve the phosphoric acid, and the insoluble salt separated by the filter, the filtered liquid was found absolutely free from magnesia, and was a solution of pure phosphoric acid.

The insoluble salt of magnesia above mentioned is new. It is remarkable for its insolubility in water and acids; and its composition, as ascertained by several concurring analyses, is—

Magnesia, ...	16.54
Phosphoric acid,.........	83.46
	100.00

It contained no water, and must therefore be composed according to the formula, $2\,MgO+3\,P_2O_5$. This formula, the author shewed, could not be reconciled with the prevalent views of the constitution of phosphates, and the existence of this salt might therefore lead to some modification of these views.

The following Donations were presented :—

The Journal of the Royal Agricultural Society of England. Vol V. Part 2.—*By the Society.*

Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce. Vol. LV.—*By the Society.*

Memoirs and Proceedings of the Chemical Society. Part 11.—*By the Society.*

The Journal of Agriculture, and the Transactions of the Highland and Agricultural Society of Scotland, for March 1845.—*By the Society.*

Fifteenth Report of the Scarborough Philosophical Society, for the year 1844.—*By the Society.*

Monday, 17*th March* 1845.

Sir GEORGE S. MACKENZIE Bart. in the Chair.

The following Commmunications were read :—

1. On the Improvement of Navigation in Tidal Rivers. By David Stevenson, Esq.

Three compartments are pointed out as existing in all rivers, when viewed in connection with the sea, possessing different characteristics, and requiring different classes of works for their improvement. These are, *first*, The "sea proper," characterised by the presence of unimpaired tidal phenomena, and including all works connected with the improvement of bars. *Second*, The "tidal compartment of the river," characterised by the modified flow of the tide, produced by the inclination of the bed, and embracing works connected with the straightening, widening, or deepening, of the beds of rivers ; the formation of new cuts; the erection of walls for the guidance of tidal currents, and the shutting up of subsidiary channels. And the *third* compartment is the "river proper," which is characterised by the absence of all tidal influence, the improvement of which is generally effected by means of dams erected in the bed of the river, and forming stretches of canal communicating with each other by means of locks in the dams. The practical remarks are confined to the improvement of the tidal compartment only, which possesses sufficient importance to entitle it to form the subject of a distinct communication; the prosperity of the ports of London, Bristol, Newcastle, Glasgow, and many other places, being intimately connected with it.

The author shews, that, owing to the smallness of the rivers of this comparatively narrow country, they can be advantageously navigated only while their waters are deepened by the influx of the tide, and proposes, as the surest means of effecting improvement, such works as produce an *increase in the duration of tidal influence.*

Instances of the success of these works are given. The rise is the bed of the Tay from Newburgh to Perth (8·56 miles), in consequence of works that have been executed, has been *reduced* from *four* to *two* feet. The time occupied in the passage of the tidal wave between these places has been *decreased fifty minutes*, and the speed with which it travels *increased* 1⅝ *of a mile per hour.* The duration of flood-tide at Perth has been *increased fifty minutes*, and the time during which the river at that place is uninfluenced by the tide, has been *decreased forty-five minutes.* It is also calculated that an additional quantity of sea water, amounting, on an average, to 760,560 tons, is propelled into, and again withdrawn from, that part of the river extending above Newburgh every tide. At the Rubble in Lancashire, similar benefits have resulted from similar operations; the tidal range at Preston having been *increased* between *three and four feet*, and the propagation of the tidal wave *accelerated about forty minutes.*

The following are the practical inferences which the author draws from the facts brought forward:—

First, That owing to the comparatively contracted country from the drainage of which our rivers derive their supplies, it is chiefly from *increased duration of tidal influence* that we must expect improvement in tide navigation, the regulation of the fresh water stream being an operation of secondary importance, but not, on that account, to be overlooked.

Second, That the whole tidal phenomena of the navigation to be improved ought to be ascertained, in order that the engineer may be enabled to discover in what part of the river the most prejudicial retardations of the tidal wave, and obstructions of the current, take place.

Third, That, in tracing these retardations to the proper cause, and suggesting means for their removal, works should be adopted which do not injuriously abstract tidal water from the sea channels.

Fourth, That the works best suited for attaining the desired end consist chiefly in lowering the bed of the river, and removing all natural or artificial obstructions, and in erecting low rubble walls for the direction of the currents.

Fifth, That although *general* views of the nature of these operations may be given, the precise details of such works as shall be best suited to particular localities can, in the present state of our information, be determined only by Engineering experience. And,

Lastly, That, by the execution of works designed in accordance with these general views, very beneficial results have been, and may be, produced, for a comparatively small expenditure.

2. On the Solvent Action of Drainage Water on Soils. By John Wilson, Esq., F.G.S. Communicated by Dr Gregory.

The author, being resident for a time in East Lothian, in order to study the system of agriculture, it occurred to him that the very extensive and complete drainage must materially affect the soil by removing large quantities of its soluble ingredients.

He was disappointed, owing to an accident, in examining, quantitatively, the water which had been first collected for the purpose; but on examining, qualitatively, some that was collected after the drain had been running very copiously for 36 hours, he found it to contain 18.4 grains of soluble matter per gallon. This was chiefly the usual salts of lime and organic matter.

He examined the surface and subsoils of the field, and found them to contain, besides silica and alumina, iron, lime, and traces of magnesia, with organic matter. The iron in the surface soil was in the state of protoxide, but in the subsoil it was found peroxidised.

The author concludes that the drainage water carries off a very large quantity of the soluble matter of the soil, which he calculates as possibly amounting to 775 lbs. per acre in the year, a quantity equivalent to a good dose of manure. He recommends the adoption of some means to prevent this great loss, and promises to continue his researches, and bring forward more precise results.

The following Donations were presented:—

Anatomical and Pathological Observations. By John Goodsir, F.R.S.E., and Harry D. S. Goodsir, M.W.S.—*By the Authors.*

The American Journal of Science and Arts, conducted by Professor Silliman, for January 1845.—*By the Editor.*

Monday, 7th April 1845.

Sir T. M. BRISBANE, Bart., President, in the Chair.

The following communications were read:—

1. Observations on the Temperature of the Earth at Trevandrum, in Lat. 8° 30′ 32″. By John Caldecott, Esq. Reduced, with some Remarks, by Professor Forbes.

The present notice is a sequel to one presented to the Royal Society on 1st May 1843, containing the results of Mr Caldecott's Observations on the Temperature of the Earth in India, made by means of thermometers sunk to depths of 3, 6, and 12 French feet, corresponding to those employed in the three stations near Edinburgh, of which the results are already before the Society.

The position of the instruments is mentioned in the former notice (*Proceedings*, vol. i. page 432).

A later note from Mr Caldecott, dated 10th August 1843, accompanies the detailed observations made upon the three thermometers four times a-day (every 6 hours) for the whole year, from July 1842 to July 1843, Sundays excepted.

The most remarkable circumstance connected with the observations, is the extraordinary excess uniformly observed of the earth temperature above the air temperature. The index error of the instruments is so large, as might have excited a suspicion of some inaccuracy in its determination, but for the known experience and skill of Mr Caldecott. I shall therefore quote what his letter says on this point. "The readings of the thermometers, as given in these tables [containing the observations *at large*], require a correction in order to reduce them to those of the standard thermometer. This correction, derived from a comparison of them with the standard, made every two hours for rather more than a month, before they were put into the ground, is as follows :—

No. 1—12 feet long, requires the addition of ...... $\overset{\circ}{2}$.133.
2 — 6 .... 2.172.
3 — 3 .... 2.922."

This detail is even too minute to render a considerable mistake at all likely ; we must therefore take the results as we find them, which are contained in the following table, where the numbers are *all corrected*, and compared with the mean temperature of the air, and the quantity of rain. It will be observed, that several of the observations of the warmer months have been lost, owing to the large index error having misled the maker as to the range which would be required, and consequently the spirit rose above the scale.

Abstract of Observations of Terrestrial Temperature at Trevandrum.
Lat. 8° 30′ 32″ N. Long. 5^h 7^m 59^s E. of Greenwich.

	No. 1. 12 feet Thermometer.	No. 2. 6 feet Thermometer.	No. 3. 3 feet Thermometer.	Mean Temperature of Air.	Rain.
1842.	°	°	°	°	Inches.
May	86.805	87.349	86.742	80.09	14.513
June	...	86.742	84.977	79.32	8.747
July	86.938	85.789	83.901	78.73	5.951
August	86.383	84.940	83.147	77.90	4.424
September	85.930	85.052	84.237	78.28	7.723
October	85.843	85.237	84.437	79.10	5.492
November	85.783	84.899	83.307	77.82	8.805
December	85.535	85.057	84.507	78.96	0.164
1843.					
January	85.783	86.212	85.759	79.05	1.154
February	86.085	86.809	87.047	80.09	0.033
March	86.643	88.579	89.457	82.36	1.721
April	...	...	89.114	81.58	9.274
May	...	88.224	87.202	80.62	15.989
June	...	85.739	83.549	78.21	16.932
July	86.043	83.879	81.777	77.29	10.899

The Thermometers are corrected for index error.

With a view to deduce more carefully the results of this curious, and hitherto unique series of observations, I have had the observations for the year projected in the form of curves, on the same scale as those formerly submitted to the Society, of the earth temperatures at Edinburgh. These curves are now submitted to the Society, and the results may be thus briefly stated.

I. The annual curve of temperature at Trevandrum is a very irregular one, but has only one great well-marked inflexion, giving a maximum temperature in the beginning of April, corresponding exactly to the period of commencement of the rainy season. The temperature of the air goes through its whole range in the course of the following three months ; but there is no decided minimum in the annual curve.

II. The mean annual temperature of the air appears to be under 80°, whilst that of the earth reaches 85°—a remarkable difference.

III. The accidental, as well as the main annual fluctuations of temperature of the atmosphere, are faithfully reproduced in the curve of temperature at a depth of three feet. They are diminished in intensity, and have a slight retardation in point of time, not exceeding ten or twelve days, at the depth of three feet.

IV. *The range at three feet is nearly* 2° *greater than at the surface.* This is a very remarkable circumstance ; and arises apparently from the atmospheric maximum being prematurely checked by the approach of the rainy season.

V. At six and twelve feet, the range is rapidly diminished, and the casual fluctuations almost entirely disappear, leaving maximum in spring well marked. Notwithstanding the deficiency of some of the maximum temperatures, it may be inferred, that the range at six feet is $5\frac{1}{2}$° ; at twelve feet, $2\frac{1}{4}$° ; the range at three feet being 8°.

VI. The retardation of epoch is also plainly indicated, although, from the deficiencies just mentioned, it cannot be so accurately determined. According to the best guess I can form, the three feet maximum occurs about the 8th April ; the six feet maximum about the 20th April ; and that at twelve feet about the 11th May ; giving a pretty regular rate of progress of the heat downwards, of one foot in three and a-half or four days.

From these facts, it is easy to deduce, generally, that the phenomena of the propagation of that into the ground near the equator, resemble perfectly those in our own latitudes, though modified in extent. Even at a depth of 12 French feet, the annual variation has not nearly vanished, nor would so, even approximately at more than double the depth. Mr Caldecott's experiments conclusively establish the error of the doctrine of Boussingault (at least in the eastern hemisphere), that the annual temperature near the equator remains unchanged at the depth of a foot below the surface in the shade. This mistake is the more important to correct, because M. Poisson has attempted to confirm his own mathematical theories of heat, by applying them to this alleged fact.*

Mr Caldecott's experiments appear, farther, to prove a considerable excess of the temperature of the earth above that of the air at Trevandrum. This result is in opposition to the opinion of Kupffer, which supposes the earth temperature to be less than that of the air at the equator; and of Boussingault, which supposes them the same.

The results of Mr Caldecott are confirmed in both particulars by Lieutenant Newbold, of the Madras Army, in a paper lately read to the Royal Society of London.†

2. Miscellaneous Observations on Blood and Milk. By Dr John Davy.

The author first treats of the state of combination of the alkali in the blood. Enderlin, from his recent analysis of the ashes of the blood, has inferred that its alkaline reaction is not owing to the presence of carbonate, but of the tribasic phosphate of soda. The author, even admitting the accuracy of Enderlin's results, questions the propriety of applying them to the condition of the alkali in the liquid blood. Carbonate of soda, he observes, is decomposed when heated with phosphate of lime; added in small quantity to blood, it is not to be detected in its ashes. This may account for its not having been found in its ashes. Were the opinion referred to correct, an acid added to blood or its serum, after the action of the air-pump, ought not, on re-exhaustion, to occasion a farther disengagement of air; but he finds that it does. This, with other results, induces him to give the preference to the conclusion, that blood contains the sesquicarbonate of soda.

He next considers the viscid quality of the blood particles, and their tendency in consequence to adhere together in groups distinct from their aggregation in piles, and to adhere as well to other objects. Under the microscope, using the compressor, the quality in question is still exhibited; when a cluster of blood corpuscles is broken up, and its parts set in motion, some of them, while adhering to each

* Theorie de la Chalced. p. 508. † Phil. Mag. No. 5, xxiv. 461.

other, and only then, are drawn out almost to a fibre; and yet the instant the adhesion is broken, the detached particles, now solitary, recover their circular outline. This viscid property of the blood corpuscles appears to be distinct from that of coagulable lymph; lymph being viscid, not in its liquid state, when it attenuates even the serum, but in its transition state, just before and when in the act of coagulating.

The third subject treated of, is the tendency of fibrin in coagulating to a certain arrangement of its particles. In proof of this, he adduces the instance of the investing pellicle or membrane of the buffy-coat; the tubes of the fibrin formed as a cast, when blood is stirred with a rod in the act of coagulating. The cyst-like cavities occasionally met with in fibrinous concretions, whether filled with the serum or puruloid fluid, found after death in the heart and great vessels;—in all which a kind of *nisus formativus* is displayed, and an arrangement more or less regular; and which may be applicable, he believes, to account for the cysts of aneurisms speedily following punctured wounds of arteries, and for the sacs of false aneurisms, continuous with, and hardly to be distinguished from, the lining membrane of the vessel.

The last subject treated of, is the effect of serum in promoting the coagulation of milk—a property which serum possesses in common with the white and yolk of the egg, on the application of heat. The results of trials of mixtures of serum and milk in different proportions are stated, from which it appears, that 1 part of the former heated with 5 of the latter, will occasion its coagulation, and even when mixed with a third more. From analogy, the author infers, that serum and white of egg may have a like effect on vegetable juices containing albuminous matter similar to casein. The action of one animal fluid, and those so like as serum and milk, he refers to as a curious subject for speculation, and as deserving of attention, not only in relation to culinary and some manufacturing processes; but also, it may be, in connexion with physiology, and perhaps pathology.

3. The Secretary then gave an account of some of Mr Bain's applications of Electricity, as a moving power to Clocks.

The following Donations were presented.

Scheikundige Onderzoekingen, gedaan in het Laboratorium der Uttreschtshe Hoogeschool. 2de Deel 6de Stuk.—*By the Editors.*

The Journal of the Royal Geographical Society of London. Vol. XIII. Part 2. and Vol. XIV. Part 2.—*By the Society.*

The London University Calendar 1845.—*By the Council of the University.*

Account of the Northumberland Equatorial Dome attached to the Cambridge Observatory.—*By the Duke of Northumberland.*

Observations made at the Magnetical and Meteorological Observatory at Toronto in Canada, printed by order of Her Majesty's Government under the superintendence of Lieut.-Col. Edward Sabine, of the Royal Artillery.—*By the British Government.*

Memoir of Thomas Henderson, Esq., Professor of Practical Astronomy in the University of Edinburgh. By Thomas Galloway, Esq.—*By the Author.*

The Grasses of Britain. Part 2. By Richard Parnell, M.D., F.R.S.E.—*By the Author.*

On the Chemical Constitution of the Bones of the Vertebrated Animals. By James Stark, M.D., F.R.S.E.—*By the Author.*

Memoirs and Proceedings of the Chemical Society. Part 12.—*By the Society.*

The Fifth and Ninth Letters on Glaciers. By Professor Forbes, F.R.S.S.L. & E.—*By the Author.*

On the Medicinal properties of Bebeerine. By Douglas Maclagan, M.D., F.R.S.E.—*By the Author.*

Remarks on the Improvements of Tidal Rivers. By David Stevenson, M.D., F.R.S.E.—*By the Author.*

On a possible explanation of the Adaptation of the Eye to distinct vision at different distances. By Professor Forbes, F.R.S.S.L. & E.—*By the Author.*

Monday 21*st April* 1845.

Sir G. S. MACKENZIE, Bart., V.P., in the Chair.

The following communications were read :—

1. On Dr Wollaston's Argument from the limitation of the Earth's Atmosphere as to the Finite Divisibility of Matter. By Dr George Wilson.

The author commenced by stating at some length, the nature of Wollaston's argument, the object of which was to affirm that the limitation of the earth's atmosphere justified the conclusion, that the air consists of indivisible particles or true atoms. He then discussed the opinions which have been offered by Daubeny, Dumas, Whewell, and others, as to the validity of Wollaston's conclusion, and stated that the special object of his communication was to shew that the inference from the existence of a limit to the atmosphere, that matter is only finitely divisible, is quite unwarranted.

Wollaston, he observed, had only succeeded at the utmost in establishing, that the atmosphere consists of a finite number of mutually repelling molecules, without supplying, or even offering any proof, that these molecules were true atoms. The author urged that the repelling molecules of the carbonic acid and water in the atmosphere, are certainly not atoms, but groups of several particles; and that, for anything we can prove to the contrary, the molecules of oxygen may be equally or even more complex; and farther, that even if it could be shewn that oxygen and nitrogen are chemically homogeneous, it would not entitle us to assume that their repelling molecules were single atoms instead of groups of several, since we have no means of estimating what the complexity of a gaseous molecule may be. The author concluded by stating, that Wollaston's argument left the question of the finite or infinite divisibility of matter exactly where it found it.

2. Biographical Notice of the late Professor Henderson. By Professor Kelland.

In undertaking the task which has been assigned me, of laying before the Society a brief history of the life and labours of one of their most valuable members, I am influenced as much by my regard for the deceased, as by my duty to the Society. I feel that, in a place where I am a comparative stranger, I have lost a friend —a loss the greatness of which can only be appreciated by those who have experienced, as I have done, the integrity of his character, and the warmth of his heart. Mr Henderson was a man whose every action was the dictate of a right conscience. With society, his intercourse was marked by an utter want of selfishness—a rare characteristic; with his friends, it was stamped with true and unostentatious kind-heartedness. He was ready and happy, at all times, to lend them aid, or afford them sympathy in every difficulty, scientific or social. That his eulogy has fallen to the lot of one so little qualified to do it justice, I sincerely regret; but I lament it the less when I reflect, that in other quarters it has found the able advocacy of my friend Mr Galloway, and that both he and myself have had the invaluable assistance of Mr William Ivory.

Thomas Henderson, Professor of Practical Astronomy in our University, and Astronomer-Royal for Scotland, was born in Dundee on the 28th of December 1798. His father died early in life, leaving five children, of whom he was the youngest, to the care of his widow. His eldest brother having been bred to the law, and seeing prospects of success before him, destined his brother Thomas for the same profession. Accordingly, having received an excellent preliminary education at the grammar school of Dundee, he was sent, at the age of thirteen, to the academy of that town, where he remained two years under the able tuition of the present Professor Duncan of St Andrews. Here he acquired the rudiments of mathematics and natural philosophy, in which he made such progress as to merit being styled by the Professor one of the best scholars he ever had under his care. Even at this time his predilection for astronomy had developed itself; but it cannot be supposed that his acquirements sufficed to enable him to do more than manifest a partiality towards that science. In 1813 he was placed with Mr Small, writer, afterwards town-clerk of Dundee. He now bestowed considerable attention to the decyphering of the manuscripts, and particularly the ancient records of the burgh. He also laboured hard in the acquirement of an accurate knowledge of history and chronology, for which his remarkably retentive memory well qualified him. At the age of twenty-one he repaired to Edinburgh, to pursue his legal studies, and entered the office of Messrs J. and W. Murray, W.S. While in their employment, an application was made to him to undertake the arrangement and classification of the Records of the Burgh of Dundee—a work for which he was amply qualified, and which he satisfactorily performed. His abilities and business habits recommended him to the good offices of Mr Gibson-Craig, who became his patron, and ever remained his steady friend. At his recommendation, he acted as clerk to the late Lord Eldin, both prior and subsequent to his elevation to the Bench. After his Lordship's resignation, Mr Henderson accepted the office of secretary to the Earl of Lauderdale, in which capacity he visited London, where he made the acquaintance of the principal astronomers of the metropolis, from whom he received great kindness. In particular, Sir James

South gave him access to his observatory, and thus enabled him to familiarize himself with the use of instruments.

But it is to Professor Wallace, ever his steady friend, under whose care the Observatory of the Edinburgh Astronomical Institution was then placed, that he owed his rapid progress in astronomy, if not his ultimate adherence to the science. Finding in Mr Henderson a zeal for the study, and an ambition to distinguish himself in it, Mr Wallace, with his accustomed disinterestedness, unhesitatingly placed the Observatory at his command, and thus afforded him the means of acquiring that practical skill for which he was celebrated. To this circumstance I attribute much of Mr Henderson's success in astronomy. However true it be, that talent will develope itself in spite of obstacles, it is no less certain, that, in sciences like this, which owe so much to the external aid of expensive instruments, the fortuitous circumstance of an access to the requisite machinery, is a strong stimulus to exertion, without which few would undergo the drudgery of acquiring a mastery of the practical details of the science. With an observatory at his disposal, Mr Henderson saw the road to eminence in practical astronomy open before him, and he hesitated not to labour zealously to fit himself for the walk to which his inclinations prompted him.

As might be supposed, he had not long entered on a systematic course of reading, ere improvements suggested themselves to his acute mind. The first which he made public, relates to the computation of an observed occultation of a fixed star by the moon. This he transmitted to Dr Thomas Young, then secretary to the Board of Longitude in 1824. It was published by him in the *Nautical Almanac* for 1827 and the four following years; and Mr Henderson received the thanks of the Board for his communication. This paper, and many others of his, were likewise inserted in the *Quarterly Journal of Science.* It is probable that the subject which actually brought Mr Henderson into notice with astronomers, was his detection of an error in the *data* furnished to Mr Herschel for the determination of the difference of longitude of London and Paris. His paper on this subject was published in the *Philosophical Transactions* for 1827, and the Royal Society voted him a copy of the Greenwich Observations, in return for his labour. This communication had also the effect of procuring for him the friendship of Mr Herschel, whose estimate of its importance is expressed in a testimonial which he gave Mr Henderson in 1829, when candidate for the chair of Practical Astronomy, in the following terms :—" I . . . assure you . . . how highly I appreciate your astronomical acquirements, especially your habits of accurate and scrutinizing calculation. I have, on a former occasion, experienced the value of this investigating spirit and laborious industry, in your detection and correction of an error overlooked by myself in the statement sent me from the Royal Observatory, relative to the operations for determining the difference of longitude of Greenwich and Paris in 1825—a correction which had the effect of raising a result, liable to much doubt from the discordance of the individual day's observations, to the rank of a standard scientific *datum ;* and thus conferring on a national operation all the importance it ought to possess."

Thus flattering was Mr Henderson's first connection with the Royal Society ; nor was his reception by the Astronomical Society less so. In 1828, he prepared an ephemeris for 1829, of the occultations of *Aldebaran* by the moon, for ten different observatories in Europe. In return for this and other valuable communications, the Society presented him with a copy of their Transactions, handsomely bound.

Mr Henderson's reputation as an astronomer was now fully established, and it was his own wish and the desire of his friends, that he should be placed in a situation more congenial to his favourite pursuit. Two such situations presently opened; to neither of which, however, was he immediately appointed. The Town-Council of the city of Edinburgh had granted to Mr Short, in 1776, a lease of a portion of ground on the Calton Hill, on the condition that an Observatory should be erected on it ; but it was not until about forty years afterwards that any instruments adapted to astronomical purposes were placed there, and even then the want of funds prevented it taking its place as an operative establishment. Some years prior to the time of which we speak, a number of gentlemen formed themselves into a society, under the designation of the Edinburgh Astronomical Institution, and by their exertions procured the erection of the present building. Having exhausted their funds, they applied to Government for a grant, which they succeeding in obtaining. From the want of endowment, however, the business of the observatory was somewhat irregularly conducted. In 1828, Dr Robert Blair, Professor of Practical Astronomy in the University of Edinburgh, died. The office had hitherto been a sinecure, and it occurred to many interested in the science, that it might be made useful by the appointment of a person qualified to perform the duties of a practical observer ; and that, consequently, this vacancy was a favourable opportunity for uniting the professorship with the observatory. As might have been expected, great exertions were made to place Mr Henderson in the situation, but, for the present, ineffectually, from the circumstance that the Government had resolved to postpone any appointment, until it had been maturely considered on what footing the professorship could be placed, with the greatest prospect of success to the science of astronomy. Another opening occurred within a few months of this, occasioned by the death of Dr T. Young. Shortly before his decease, he delivered to Professor Rigaud of Oxford, a memorandum, recommending Mr Henderson as his successor in the superintendence of the Nautical Almanac. The appointment did not take place, but there exists perhaps no higher testimony to Mr Henderson's merit than this recommendation, when it is remembered that it arose out of his scientific reputation, altogether unaffected by private friendship, and that Dr Young ranks among the very highest of the philosophers of the present century.

Although disappointed in the instances we have mentioned, a situation shortly fell in Mr Henderson's way, which appeared likely to establish him in a suitable manner. On the death of Mr Fallows, the astronomer at the Cape of Good Hope, his qualifications were so well known to the parties with whom the appointment lay, that the office was offered to him without any solicitation on his part, or application on that of his friends. Mr Henderson accepted the appointment, and sailed for the Cape in January 1832. Immediately on his arrival there, he entered on his duties with ardour; and so indefatigable were his exertions that he amassed a most valuable series of observations, and found time, besides, to prepare and transmit to the Royal and Astronomical Societies, various papers connected with the science. The principal results of his labours at the Cape were, the determination of the latitude and longitude of his station—of the positions of stars near the South Pole, for fixing the polar positions of his instruments—of the amount of refraction near the horizon—and of the moon's horizontal parallax; together with observations on the planet *Mars*, for the purpose of computing his parallax, and that of the sun—of Encke's and Biela's comets—of occultations of fixed stars by the moon—of a transit of *Mercury*—and of between 5000 and 6000 declinations. Prior to his appointment to the Cape Observatory, Mr Henderson had had slight symptoms of a disease of the heart, and he soon found that the labours and anxieties incident to his position, together with the serious disadvantages attendant on the building in which he was compelled to reside, rendered it impossible that he should retain the situation. Accordingly, in a letter, dated May 27. 1833, he tendered his resignation to the Lords of the Admiralty, adding that, on his return to England, he would immediately proceed to the task of calculating and reducing the various observations he had made, and of extracting from them those useful results they were intended to afford.

Amongst the other annoyances to which Mr Henderson was subject at the Cape, may be mentioned that, which had been the source of much vexation to his predecessor, the state of the mural circle. Mr Fallows had found remarkable anomalies amongst the readings of the several microscopes, in different positions of the instrument, during a revolution upon its axis; whence he had been led to infer that it had suffered a change of figure since leaving the maker's hands. Mr Henderson's first employment was the rigid investigation of these anomalies, the results of which are printed in the eighth volume of the *Memoirs of the Astronomical Society*, p. 141. He came to the conclusion, that the anomalies proceeded partly from an oval form which the instrument had acquired, and partly from variations in the position of the centre of the instrument while revolving, relative to the microscopes, owing probably to the pivots not being exactly circular; whilst, in addition, the whole instrument frequently changed its position upon the pier, from the Y support of the front pivot not being perfectly steady. He agreed, moreover, with Mr Fallows in concluding, that the mean of the readings of six microscopes is little, if at all, affected by these causes; so that, on the whole, it appeared that the degree of accuracy to be obtained from the instrument was not inferior to that given by the best instruments of similar construction hitherto made. The conclusion of the matter is this:—on the circle being brought to this country and examined by Mr Simms, it was found that the large steel collar carried by the conical axis was quite loose;—"a child's hand could turn it."

On Mr Henderson's return to Edinburgh he set about reducing his own observations; a task voluntarily imposed on himself, and one which he sacrificed his own interests to fulfil. No long period elapsed, however, before a situation opened, in every way suited to his taste. An agreement was entered into between the Commissioners of the Treasury and the members of the Astronomical Institution of this place, whereby the latter gave their observatory and instruments to the use of the Professor of Practical Astronomy in the University, and the former agreed to supply a salary to the Professor. Mr Henderson was selected as the proper person to occupy this situation; and he entered on the duties of the office in October 1834. The value of the observations which he made during the ten years he held this appointment is too well known to need comment. But Mr Henderson did not confine himself to the routine of Observatory duties, important as they are. No sooner had he got the Institution into working order, than he again vigorously attacked his Cape observations, and laid the results before the world. He commenced by communicating to the Astronomical Society a valuable catalogue of the mean declinations of 172 principal fixed stars for January 1. 1837. This was followed by a memoir on the refraction of stars near the horizon, in which he concludes, that no difference of refraction north and south of the zenith appears as far as to 88° of zenith distance. Another very important communication of Mr Henderson's was the determination of the equatorial horizontal parallax of the moon. This is best effected by the comparison of results north and south of the equator. It is well known, that to obtain it by this method, was one of the chief objects of La Caille's voyage to the Cape in the middle of the last century. Ever alive to the interests of the science, Mr Henderson determined to avail himself of his own position when at the Cape, to repeat the observations. The result to which he arrives is extremely satisfactory, differing as it does but slightly from La Caille's.

These and similar labours place Mr Henderson high in the estimation of astronomers. But something more is requisite to give a man interest in the eyes of the world at large. In the field of science, many a patient cultivator who has conferred a real boon on mankind has been altogether forgotten. The successful opening of some unexplored district, or the discovery of some popularly interesting fact, confers, and properly confers, a wide-spread fame. The development of scientific knowledge, as of every thing relating to the preparation of the races of mankind for their future destinies, is regulated by an All-wise hand, which, whilst it dispenses sufficient to satisfy each generation as it passes, kindly holds back an inexhaustible store to supply the intellectual cravings of races yet to follow. The natural sciences have not yet (as a philosopher unwisely asserted a century ago they had) nearly attained their ultimate perfection; and doubtless are not destined soon to do so. Hence, whoever is privileged to make a discovery, however trifling, is worthy of respect, as having contributed towards the fulfilment of vast designs, slowly but steadily progressing towards their accomplishment. Should the investigation of the parallax of *α Centauri*, which Mr Henderson gave to the world, turn out correct, of which there is, at present, little doubt, then shall we claim for him the distinction which I have marked with honour—the distinction of having extended astronomical measures beyond the limits of our system.* Nor will it diminish his fame that a similar determination was attempted before, or that an equally successful one was nearly contemporaneous with his own. In few cases has an individual made a successful essay, without having been preceded by others, not destined to reach the goal, or accompanied by some one, to share the honour. There is reason to fear, that, in the present instance, whatever honour is due will fall to the lot of another. It is to be regretted, that (whether the conclusion shall ultimately be verified or not) no scientific award was made him, in consideration for the skill displayed ; whilst his contemporary received for his labours a medal from this country. Let us hope that his reward will be the association of his name with the discovery.

There is another point wherein, as Mr Henderson's advocate, I

* On this subject I beg to refer to my friend Mr Main's admirable memoir in the *Transactions of the Astronomical Society*, v. . It may be interesting to add the following remarks from a private communication of his to myself:—"At the time when I was requested to draw up a memoir on the subject of annual parallax, the amount of evidence of its sensible existence for any star whatever, which had even arisen from the discussion of investigations previous to Bessel's, was exceedingly small ; and I believe that, at that time, any new attempt was likely to be received with the smile of incredulity, which repeated failures will always, in the long-run, tend to produce. No sensation was caused in England by the announcement of Bessel's investigation ; and I remember that, to some astronomers, his confidence in the certainty of his result seemed far from warranted. At present there exists very little scepticism with respect to our knowledge of the parallax of 61 Cygni. But it was deduced by a method with which English astronomers were, I may assert, in general, unfamiliar, perhaps through the want of a good heliometer. . . . . . It was an evidence of very creditable faith, therefore, which induced Mr Henderson, in default of other means, to attack this star rigorously by meridian observations in both elements, and this notwithstanding his accurate knowledge (for no man living excelled him in his knowledge of every thing that had been done in every department of modern practical astronomy, from its commencement) of that total failure of every attempt that had been made under the ablest astronomers of this country, in the northern hemisphere. And so fully was he impressed with the conviction of his ultimate success, that he left it as a legacy to his successor, who has sent over a very complete and beautiful series of observations, which, after their discussion by Mr Henderson himself, not long before his decease, has, in the minds of many, pretty nearly decided affirmatively the question of the existence of a considerable parallax."

would assert his claims to reward. During a long series of years, he devoted much of his leisure time to the reduction of the Cape Observations, which having been made in a public observatory, it was the duty of the public to present in a proper shape to the world. Mr Henderson performed this duty with no other remuneration than the satisfaction derived from giving a perfect form to his own results. We lament, however, that his infant daughter will reap none of the fruits of that excessive midnight toil which hastened her father's progress to the grave, more especially as she is an orphan indeed—deprived of both her parents. To the memory of her mother, who died shortly after her birth, it is fit I should pay a tribute. She was the daughter of Mr Adie, the celebrated optician of this city. In his selection of this lady as his partner, no less than in the other acts of his life, Mr Henderson manifested the soundness of his judgment. She was in every way suitable for him. A member of a talented family especially devoted to scientific pursuits; herself gifted with a mind of great capacity, which a liberal education had cultivated and refined; of an amiable disposition and a cheerful temperament, she was well fitted to sympathise with the depressions of a spirit weighed down with fatigue, or to brighten those passages of life, which, without her aid, would have appeared gloomy. Add to this, that her attainments were considerable; so much so as to render her not only capable of appreciating and admiring her husband's ardour and enthusiasm in his favourite studies, but even of occasionally assisting him in the prosecution of them. Under these circumstances, it need scarcely be said that their union was a happy one. Her death, at a time when their fondest wishes seemed realized in the birth of a daughter, was a shock from which he never recovered. His manner, which had always been deficient in buoyancy, became from that moment solemn. In anticipations of the future, he rarely indulged; in a melancholy retrospect of the past, too often. This, added to his late habits, preyed rapidly on his constitution, and hastened his death. He expired on the 23d of November 1844, of a disease of the heart.

To draw his character—scientific or social—is an easy and a pleasing task. As an observer, he was ingenious and accurate—in testimony of which it is sufficient to say, that his observations carry the entire confidence of every astronomer in Europe. On this head, I cannot do better than allow one of their number (Mr Main) to speak for me. He says, "The praise of being the first discoverer of our distance from a fixed star, even should it be ultimately established, though a brilliant addition to his fame, is not precisely that which will cause his name to be remembered with gratitude by every one who understands what ought to be the routine duty prescribed to himself, and practised by the astronomer. His business in general—his every-day work—is not speculative, but practical ; not conversant, except, by the way, with the almost despaired-of problems of the science, but with the establishment of the data which belong to his epoch. He is to fix, with indisputable accuracy, the places of the most remarkable of the stars ; he is to bring his contribution to the perfecting of the lunar and planetary tables ; he is to assist in the measurement of our own planet, as the basis of all our ulterior comparisons ; and he must do this by submitting to a routine, whose irksomeness and labour no one can appreciate but himself. Professor Henderson did all this in a way which lays just claim to the gratitude of succeeding astronomers ; his speculations on our connection with the sidereal system were but the recreations of a mind that never swerved from the amount of toil imposed by the less dignified, but more useful occupations of the astronomer. His observations, followed up as they have been by Mr Maclear, will be the basis of all the astronomy that is peculiar to the southern hemisphere ; and it is to his results, that the astronomers of the next age will look for the facts of their science."

I apprehend, however, that no slight foundation of Mr Henderson's future fame, will be found to rest on the admirable use which he made of his own observations. Having acquired a thorough knowledge of all that had been done, and all that was desirable in astronomy, he was ever on the alert to seize any opening which the circumstances under which these were made, might seem to offer. Witness his memoirs on refraction, and on the parallax of the moon, which were suggested by the position of the observatory in which he was at that time placed. The arguments, too, from which his conclusions were drawn, are marked by singular perspicuity and acuteness. The standard which he adopted in mathematical reasoning, was the works of Euler. Not many days before his death, I found him reading some book of travels, and on expressing my pleasure at finding him so employed, he remarked, " I should very much prefer a volume of Euler, but I cannot get at it." In astronomy he looked upon Bessel as his model, almost as his master. It was my good fortune to participate in his entertainment of that distinguished astronomer, as we had previously united in the reception of Encke. I would fain efface from my memory the pleasure we experienced on both those occasions. In the case of Bessel, his was the delight of a son who had found a father. He hung on his words and watched his looks with a mingled feeling of affection and pride. That he profited by studying the writings of these great men, his own researches sufficiently testify. His path lay not in the complex analytical investigations of the French school; but what he professed, he was profoundly acquainted with. His natural modesty appears nowhere to greater advantage than in his sternly disclaiming all pretensions to knowledge with which he was only partially acquainted. In social life he was kind and affectionate; ever ready to assist his friends, without regard to his personal comfort. His naturally reserved manner unfitted him to occupy that position in general society which his extensive information and accurate memory amply qualified him for. He felt, too, what his position, as Her Majesty's Astronomer for Scotland, demanded, and having no facility of adapting himself to the peculiarities of others, he was content to limit his circle to a few chosen friends. Amongst these he never forgot his early patrons, towards whom he manifested to the last the same deep feelings of gratitude. By them, and by all who knew him intimately, he was much beloved, and as much respected. They will agree with me in saying that we have lost a valuable friend,—a man liberal and high-minded,—conscientious to a degree,—ready with heart and hand to assist, when assistance was called for,—with a judgment so sound, and experience so improved, as to render him a safe adviser, and an invaluable coadjutor. May the reputation he has left, and the kind remembrances which his friends cherish of him, stimulate us to follow his example, and imitate his virtues.

3. On the Chemical Relations of Creosote. By William Gregory, M.D., Professor of Chemistry.

The author stated, that, being struck with the singular resemblance between the properties of creosote and those of carbolic acid, as described in all chemical works, he had tried the action of a mixture of chlorate of potash and hydrochloric acid on creosote, and had thus obtained a very large proportion of chloranile, the compound yielded by carbolic acid, when treated in the same way. He had also obtained, by the action of nitric acid on creosote, evidence of the production of nitropicric acid, which is also obtained from carbolic acid.

He drew the conclusion, that if these two compounds be not identical, they are, at least, very closely connected, and in all probability, contain the same radical. It is possible that creosote may be a definite compound of carbolic acid with some allied body. At all events, it is very remarkable, that these two compounds, described as different, should agree in density, taste, smell, antiseptic property, power of combining with bases, power of dissolving resins, indigo, &c., and finally in composition; although probably perfectly pure creosote has not yet been analysed.

The author mentioned these results very briefly, having discovered, just before the meeting, that he had been anticipated in his experiments on creosote, by M. Laurent, who had obtained the same results, and drawn very nearly the same conclusions, in a very recent paper, and who was therefore entitled to priority in the matter.

4. On the Thermometric Correction of Magnetic Instruments. By J. A. Broun, Esq.

Mr Broun points out the defects of the usual methods of ascertaining the corrections for temperature, applicable to magnetic instruments generally, and the vertical force magnetometer in particular. The usual methods depend upon the knowledge of the time of vibration, or upon the statical deflection produced by a neighbouring magnet, under differing circumstances of temperature. Mr Broun shews that both these methods are liable to great objection; and he has succeeded by arranging the ordinary hourly observations of the instruments in groups, in obtaining consistent results for the temperature correction by a process of elimination.

5. On the Constitution of Bebeerine. By Douglas Maclagan, M.D., F.R.S.E., and Thomas G. Tilley, Esq., Birmingham.

Bebeerine is a vegetable alkaloid, discovered by Dr Rodie of Demerara, in the bark of the Bebeeru tree, *Nectandra Rodiei*, Schomburgk. The properties of the alkaloid and its application in the form of sulphate, in the treatment of disease, were formerly described by Dr Maclagan in a paper read before the Society, and published in their Transactions, vol. xv., part 3.

As bebeerine does not crystallize, and is coloured, its purity could only be ascertained by analysis. The authors describe a new method of purification, in which oxide of lead is employed to separate tannin, &c.

The mean results of the analysis performed, were as follows :—

Carbon, ...	71·92
Hydrogen, ...	6·49
Nitrogen, ...	4·75
Oxygen, ...	16·84
	100·00

The mean atomic weight, as deduced from the analysis of the double salt of hydrochlorate of bebeerine, with bichloride of platinum, is 3756·77 (oxygen = 100), and, making this the groundwork of the calculation, the authors were led to the formula $C_{35}\ H_{40}\ N_2\ O_6$ for bebeerine, which gives the atomic weight 3681·38.

It is remarkable that this formula is the same as that generally admitted for morphia, and not, as might be expected from the action of bebeerine, allied to those of quinine and cinchonine. The mode of arrangement of the atoms is, no doubt, different in morphine and in bebeerine, notwithstanding the apparent identity of proportions. In fact, the difference of physical properties proves a difference in the grouping of the atoms.

The authors were not able to obtain sipeerine, the substance which accompanies bebeerine, in sufficient quantity for analysis. It appears to be also an alkaloid.

The following Candidate was duly elected a Fellow of the Society :—

Professor Lewis Gordon of Glasgow.

The following Donations were presented :—

Journal of the Statistical Society of London. Vol. VIII.—Part 1. —*By the Society.*

The Electrical Magazine. Conducted by Mr Charles V. Walker. —Vol. 1. No. 8.—*By the Editor.*

Memoir of Francis Baily, Esq., D.C.L. Oxford and Dublin.—By Sir John F. W. Herschel, Bart.—*By the Author.*

Bulletin de la Société de Geographie. (Deuxieme Serie.) Tomes xvi., xvii., xviii.—*By the Society.*

Comptes Rendus Hebdomadaires des Séances de l'Academie des Sciences. Tome xix. Nos. 17–27. Tome xx. No. 1–11.—*By the Academy.*

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. II. 1845–6. No. 27.

SIXTY-THIRD SESSION.

First Ordinary Meeting, 1st December 1845.

Sir T. M. BRISBANE, Bart., President, in the Chair.

The following Communications were read:—

1. On the Sums of the Digits of Numbers. By the Right Rev. Bishop Terrot.

It was shown, that, if the ultimate sums of the digits of the terms of any arithmetic series, whose difference is prime to the local value of the notation employed, minus 1, be taken, such sums will range, without any recurrence, through all the digits of the notation, and then recur in the same order as before. It was then shewn, that in any integer series formed upon a given law, the sums of the digits of the terms will have a fixed period of recurrence. This was proved in polygonal and figurate numbers, in the series of squares, cubes, &c., in the successive powers of a given root; in the series whose general term is $m.\overline{m+1} \ldots \overline{m+r-1}$; and in that whose general term is $x^m + a x^{m-1} \ldots . + l$.

It was shewn, that whenever the *variable* or number of the term occurs as a multiplier, the sums recur after $\overline{n-1}$ terms, with certain definite relations between the intermediate terms. When the variable occurs as an index, then the recurrence was shewn to take place at shorter intervals; in the decimal notation, according as the root was of the form $3\,p$, or $3\,p \pm 1$. From the determination of the different periods of recurrence in the last case, it was shewn that every sixth power is of the form $9\,n$, or $9\,n + 1$; and that every seventh power is of the form $9n$, or $9n +$ the root employed.

2. Notes on the Topography and Geology of the Cuchullin Hills in Skye; and on the Traces of Ancient Glaciers which they present. Part 1.—By Professor Forbes.

The following Donations were reported as having been received since the close of last Session:—

Vestiges of the Natural History of Creation.—*By the Author.*

Report of the Fourteenth Meeting of the British Association for the Advancement of Science, held at York, in September 1844.—*By the Association.*

De l'Influence Curative du Climat de Pau et des Eaux Minerales des Pyrenées. Par M. A. Taylor, M.D.—*By the Author.*

A Catalogue of the Library of the Athenæum.—*By the Athenæum.*

The Transactions of the Royal Irish Academy. Vol. XX.—*By the Society.*

Journal of the Statistical Society of London. Vol. VIII., Parts 2, 3.—*By the Society.*

Memoirs and Proceedings of the Chemical Society. Parts 13, 15.—*By the Society.*

Outlines of Chemistry, for the Use of Students. Part 2. By William Gregory, M.D., Professor of Chemistry in the University of Edinburgh, &c.—*By the Author.*

Geschiedenis der Ioden in Nederland. Door M. H. J. Koenen.

Over Het Onmatig Gebruik van Sterken Drank en de Middelen om Helzelve te Keer te Gaan. Door A. W. F. Herckenroth.

Het Gebruik en Misbruik der Geestrijke Dranken. Door H. M. Duparc.

De Uitoefening de Geregtelijke Geneeskunde in Nederland. Door J. C. Van Den Broecke.

Uitkomsten der Meteorologische Waarnemingen, gedaan te Utrecht, in de Jaren 1839–43.—*By the Directors of the Provincial Society of Arts and Sciences at Utrecht.*

Natuurkundige Verhandelingen van de Hollandsche Maatschappij der Wetenschappen te Haarlem.—*By the Society.*

The American Journal of Science and Arts, conducted by Professor Silliman and Benjamin Silliman junior, for April, July, and October.—*By the Editors.*

A Physiological Essay on the Thymus Gland. By John Simon, F.R.S.—*By the Author.*

On the Comparative Anatomy of the Thyroid Gland. By John Simon, Esq.—*By the Author.*

Astronomical Observations made at the Royal Observatory, Greenwich, in the year 1843, under the direction of George Biddell Airy, Esq., Astronomer-Royal.—*By the Royal Society.*

Reduction of the Observation of Planets, made at the Royal Observatory, Greenwich, from 1750 to 1830; computed by order of the Lords Commissioners of the Treasury, under the superintendence of George Biddell Airy, Esq., Astronomer-Royal.—*By the Royal Society.*

Philosophical Transactions of the Royal Society of London for the year 1845. Part 1.

Proceedings of the Royal Society, 1844. No. 60.—*By the Royal Society.*

Transactions of the Geological Society of London (Second Series). Vol. VII., Parts 1, 2.

Proceedings of the Geological Society of London. Nos. 99, 100, and 101.—*By the Geological Society.*

Annuaire Magnetique et Météorologique du Corps des Ingenieurs des Mines de Russie. Par A. T. Kupffer. 1842. Nos. 1, 2.—*By the Author.*

Etudes sur la Mortalité dans les Bagnes et dans les Maisons Centrales de Force et de Corrections de France depuis 1822 jusqu' a 1837. Par M. Raoul Chassinat, M.D.—*By the Author.*

Resultats des Observations Magnétiques faites à Gèneve dans les Annèes 1842 et 1843. Par E. Plantamour, Professeur d'Astronomie à l'Academie de Gèneve.—*By the Author.*

Astronomische Nachrichten herausgegeben von H. C. Schumacher, Nos. 536, 537, 538.—*By the Author.*

The Electrical Magazine, conducted by Mr Charles V. Walker. Vol. II., No. 9.—*By the Author.*

Journal of the Asiatic Society of Bengal, edited by the Secretary. Nos. 146 to 154.—*By the Society.*

Carte Géologique du Globe. Par M. A. Boué.—*By the Geological Society of France.*

The Twelfth Annual Report of the Royal Polytechnic Society, 1844.—*By the Society.*

Address to the Ethnological Society of London, delivered at the Anniversary Meeting. By Richard King, M. D.—*By the Author.*

Scheikundige Onderzoekingen, gedaan in het Laboratorium der Utrechtsche Hoogeschool. 3d Deel, St. 1, 2.—*By the Editors.*

Tijdschrift voor Natuurlijke Gescheidenis en Physiologie. Uitgegeven door J. Van der Hoeven, M.D., Prof. te Leiden, en W. H. de Vriese, M.D., Prof. te Amsterdam. Deel 12, Stuks 1, 2.—*By the Editors.*

On the Vision of Objects on and in the Eye. By William Mackenzie, M.D.—*By the Author.*

Journal of the Bombay Branch Royal Asiatic Society. April and October 1843.—*By the Society.*

Bulletin de la Société de Geographie (2^{de} Serie), Tomes XVI., XVII., XVIII., XIX., and XX.; and Tomes I., II. (3^{me} Serie.)—*By the Society.*

Memoires de la Société de Physique et d'Histoire Naturelle de Geneve. Tome X., Part 2.—*By the Society.*

Memorie della Reale Accademia delle Scienze di Torino. Vol. XXXIX.—*By the Society.*

Handbuch der Mineralogie. Von J. F. L. Hausmann. Zweite Theil. —*By the Author.*

Transactions of the Zoological Society of London. Vol. II., Parts 2, 3, 4, 5; and Vol. III., Parts 1, 2, 3.—*By the Society.*

Proceedings of the Zoological Society of London.—*By the Society.*

Abhandlungen der Königlichen Gesellschaft der Wissenschaften zu Gottingen. Band 2.—*By the Society.*

Nieuve Verhandelingen der Eerste Klasse van het Koninklijk Nederlandsche Instituut van Wetenschappen, Letterkunde en Schoone Kunsten te Amsterdam. Deel II.—*By the Institute.*

Annales des Sciences Physiques et Naturelles d'Agriculture et d'Industrie, Publiées par une Société Royale d'Agriculture, &c., de Lyon. Tome VII.—*By the Society.*

Pilote Français; comprenant les Cotes Septentrionales de France depuis les Roches de Porsal jusqu' au Phare des Heaux de Brehat. 6^{me} Partie.—*Par le Ministre de la Marine.*

Pilote Francais; Instructions Nautiques (Partie des Cotes Septentrionales de France comprise entre la Pointe de Barfleur et Dunkerque et entre les Casquets et la Pointe de Barfleur Environs de Cherbourg), Redigées par M. Givry. 2 Parts, 4to.—*Par le Ministre de la Marine.*

Proceedings of the Geological Society of London. No. 103.—*By the Society.*

Proceedings of the American Philosophical Society. Nos. 30 and 31.

A Public Discourse in commemoration of Peter S. Du Ponceau, LL.D., late President of the American Philosophical Society. By Robley Dunglison, M.D., one of the Secretaries.—*By the American Philosophical Society.*

Archæologia, or Miscellaneous Tracts relating to Antiquity, published by the Society of Antiquaries of London. Vols. I., II., XI., XII., XIV., XVI., XVII., XVIII., XIX., XX., XXI., XXII., XXIII., XXIV., XXV., XXVI., XXVII., XXVIII., XXIX., XXX., and Index.—*By the Antiquarian Society of London.*

Liber Quotidianus Contrarotularis Garderobæ anno Regni Regis Edwardi Primi, vigesimo octavo, A.D. 1299 and 1300.

A Catalogue of Ordinances and Regulations for the Government of the Royal Household, made in divers Reigns, from King Edward III. to King William and Queen Mary; also Receipts in Ancient Cookery.

Magni Rotuli Scaccarii Normanniæ sub Regibus Angliæ. Opera Thomæ Stapleton. 2 vols. 8vo.

Cædmon's Metrical Paraphrase of Parts of the Holy Scriptures, in Anglo-Saxon; with an English Translation. By Benjamin Thorpe.—*By the Antiquarian Society of London.*

Urologie-Traité des Augusties ou Retrecissements de l'Urethre leur traitement rationnel. Par le Dr Leroy-D'Etoilles.—*By the Author.*

Sullo Studio Comparativo delle Lingue Osservazioni Generali di B. Biondelli.—*By the Author.*

Etudes Philologiques et Historiques. Par le Dr Halbertsma. 2 Parts. *By the Author.*

Instructions Pratiques sur l'Observations et la mesure des Propriétés optiques appelées Rotatoires, avec l'exposé succinct de leur Application à la Chimie Medicale, Scientifique et Industrielle. Par M. Biot.—*By the Author.*

Maps of the Ordnance Survey of the County of Cork.—*By the Lord-Lieutenant of Ireland.*

Comptes Rendus Hebdomadaires des Séances de l'Academie des

Sciences. Tome XX., Nos. 12–26 ; and Tome XXI., Nos. 1–16.—*By the Academy.*

Proceedings of the Philosophical Society of Glasgow. Nos. 1–11. —*By the Society.*

Monday, 15*th December* 1845.

Very Rev. Principal LEE, V.P., in the Chair.

The following Communications were read :—

1. Notes on the Topography and Geology of the Cuchullin Hills in Skye ; and the Traces of Ancient Glaciers which they present—(concluded). By Professor Forbes.

The first part of this paper refers to the relations of the hypersthene rock of the Cuchullin Hills to the adjoining formations.

The author undertook the observations here detailed without the slightest view to publication. He made only brief notes on the spot, collected but few specimens, and drew the first sketch of a map which accompanies the paper, for his own amusement and information. Finding, however, that the published writings of geologists contained little or no notice of some facts which he had clearly established, he undertook this paper as a groundwork for future and more systematic researches.

The details of the geology of the Cuchullins which exist are almost all to be sought for in the Description of the Hebrides by Dr MacCulloch. The present writer has strong reason for believing that Dr M. acquired his chief information from following the coast in a vessel, and by an excursion to Coruisk, leaving out the more plain and instructive phenomena which the eastern and northern sides of the range present. Having walked completely round the Cuchullin group, and ascended two of its principal summits, the author thinks that he has established the exact boundary of the hypersthene rock on the eastern and northern sides, stretching from Ben Blaven, which is included within the hypersthene line, by the eastern base of Scuir-na-Gillean, under the northern slope of Bruch-na-Fray, and so round to Loch Brittle, where it becomes less well defined, the hypersthene rock graduating into the common trap of that country. But along the boundary first described the hypersthene may be seen overlying the claystone of the Red Hills, and forming a sort of huge bed dipping at a moderate angle with the horizon from NW. to SE., and probably thinning out in the latter direction. In confirmation of this view, it is stated that the junction of the hypersthene and claystone occurs at a height of 2050 feet on the northern slope of the Cuchullins, all below being common trap; but the junction descends to the very bottom of Glen Sligachan near Loch-na-Nain, which is little elevated above the level of the sea. The hypersthene appears to have a decidedly slaty structure parallel to this bed, which, with its singularly intractable nature, and its resistance to weathering, occasions the jagged forms for which it is so celebrated. The author believes that it has a triple cleavage, one parallel to the bed and two perpendicular to it; and that the numerous claystone veins which traverse the mass of hypersthene up almost to the very summit, have, likewise, a threefold direction parallel to these cleavages. The author thinks that the posteriority of the hypersthene to the neighbouring rocks is probably deducible from the facts which he mentions.

The second part of the paper refers to what he considers unequivocal traces of glacier-action in many directions around and amongst the Cuchullin Hills. These are principally—

(1.) Furrowed and striated rocks and *roches moutonnées*, undistinguishable in kind from those in the Alps, and, if possible, rendered more remarkable from the unparalleled hardness and toughness of the rock operated on, and the palpable incongruity which these forms present to those natural to the hypersthene rock. These furrows are to be seen parallel to the direction of the different ravines of the Cuchullin Hills, and that at all points of the compass, thus radiating from this little mountain group as a centre, and clearly localizing the cause—the wonderful mechanical cause—which has produced such a phenomenon; which, when the nature of the rock is taken into account, must be considered as one of the most wonderful instances of abrasion of which the world presents an example.

(2.) Coupled with this is the transportation of angular blocks of immense mass, transported over chasms, and lodged high and dry in fantastic positions on ledges of rock, and on the summit of the ice-worn domes of smoothed hypersthene. These phenomena, so well seen at Coruisk, and so graphically described, though unexplained or misunderstood, by Dr MacCulloch, are in all respects analogous to the *blocs perchés* of the Alps. Of banded and terminal moraines examples are pointed out in radiating directions, east, north, and west from the Cuchullin group, completing the evidence for the origination of the requisite force within the Cuchullin group itself. Other localities appear to exhibit traces of a great and general wave which has acted parallel to itself over a vast extent of country; but here no such explanation can apply, and in steep ravines, scarcely half a mile long, we cannot imagine the generation of vast and repeated waves, or floating icebergs or pent-up lakes, as any of the machinery which has elsewhere been used to account for facts less stubborn than those presented by the adamantine surfaces of the Cuchullin hills.

2. On the recent Eruption of Hecla, and the Volcanic Shower in Orkney. By Dr Traill.

Dr Traill read an account of dust falling from the atmosphere on the 2d and 3d of September last, in the islands of Orkney.

This dust was observed by a gentleman in the island of Rousay, falling from the air in the morning of the 2d. It was collected by another at Skaill, on the western shores of Pomona, on the morning of the 3d; and by two other gentlemen in Kirkwall on the same day. It appears also to have fallen in several other parts of Orkney, probably over all the islands; and was observed also to reach the northern coasts of Caithness, within an area of which the radius cannot be less than 30 or 40 miles.

It covered, to the depth of from $\frac{1}{12}$ to $\frac{1}{6}$ inch, linen laid out to dry, glass frames in gardens, and the leaves of plants of every kind, with a fine brownish-grey dust, almost impalpable to the touch, but meagre and crowding between the teeth. It does not effervesce with acids, and consisted chiefly of silex, alumina, oxide of iron, with a trace of lime.

This dust bears much resemblance in composition and appearance to that which covered the decks and rigging of vessels in the West Indian seas, when the eruption of the Soufrière took place in St Vincent, in 1812. Those who collected the dust in Orkney, state the probability that it proceeded from some eruption of Hecla, as the ashes of that volcano *once* before fell in Orkney; and the wind for several days before the 2d of September had blown strongly from the NW.

The truth is, that such an occurrence has at least three times before happened in the Orkney and Zetland Isles, when there has been an eruption in Iceland. In 1755, during a violent eruption of the Köttlugiær Jökul, showers of ashes fell in the Zetland isles. In 1766, during a great eruption of Hecla, showers of ashes reached the Orkneys, and were long remembered there under the name of the *black snow*. Again, in 1783, a similar dust was observed to descend in Orkney, which was coincident with a terrible eruption of the Shaptär Jökul; the most calamitous which ever happened in Iceland.

Thus, the volcanic ashes of that island have thrice before reached our northern islands; and recent intelligence brought by Danish fishing vessels from Iceland announces, that, in the end of August, Hecla, after being quiescent since 1766, has emitted a violent eruption from its flanks; and there can be little doubt that the dust now exhibited is derived from that eruption.

The distance between Hecla and the Orkney Islands is about 550 miles. Volney and other writers assure us that the ashes of Etna are often carried to the plains of Egypt, or to double that distance; and the eruption of the Tomboro, as described by Raffles, in the eastern archipelago, appears to have exerted a no less astonishing projectile force.

The following Donations were announced:—

Journal of the Asiatic Society of Bengal, 1844, No. 145.—*By the Society.*

The Transactions of the Linnean Society of London, Vol. XIX., Part 3, 4.

Proceedings of the Linnean Society of London. Nos. 19 to 26.—*By the Society.*

Archives du Museum d'Histoire Naturelle, publiées par les Professeurs-Administrateurs de cet Etablissement. Tome IV., Liv[res] 1, 2.—*By the Editors.*

The Second Annual Report of the Agricultural and Horticultural Society of Auckland.—*By John Johnston, M.D., President.*

Waarnemingen en Proeven over de onlangs Geheerscht Hebbende Ziekte der Aardappelen. Door G. Vrolik, M.D.—*By the Author.*

Œuvres de La Place, 4 Tomes.—*Par Le Ministre de l'Instruction publique de France.*

Monday, 5th January 1846.

Sir T. MAKDOUGALL BRISBANE, Bart., President, in the Chair.

The following Communications were read:—

1. On a new variety of Gamboge from the Wynaad. By Dr Christison.
2. Results of the Makerstoun Observations, No. 1. On the Relation of the Variations of the Earth's Magnetism to the Solar and Lunar Periods. By J. A. Broun, Esq. Communicated by Sir T. M. Brisbane, Bart.

The observations from which the following results are deduced, were obtained during the years 1844 and 1845, by means of the bifilar magnetometer, described in the Makerstoun Observations for 1841 and 1842 ; they are corrected for temperature by a method previously described.*

From the Observations for the year 1844, the diurnal variation of the horizontal force is found to consist of two maxima and two minima ; *the* minimum occurs at $10^h\ 20^m$ A.M. (Makerstoun mean solar time is used throughout), *the* maximum at $5^h\ 30^m$ P.M. ; a minimum occurs at $2^h\ 20^m$ A.M., and a maximum at $5^h\ 30^m$, or 40^m A.M.: two inflexions occur in the mean curve between 3^h and 4^h P.M., and between 8^h and 9^h P.M. The periods of maxima and minima vary throughout the year, and the morning maximum is greater than the afternoon maximum in winter.

The diurnal range is greatest in July and least in January ; the mean of the ranges for these two months, the range for the month of March and for the month of October, are each nearly the mean diurnal range for the year.

The author shewed to the Physical Section of the British Association in June 1845, that the Makerstoun Observations for 1842 indicated a well-marked annual period of horizontal force, consisting of maxima near the solstices, and minima near the equinoxes ; in correcting the Toronto Observations for 1842, by the method already noticed, he had arrived at the same result. He has now verified this period from the Makerstoun Observations for 1843, 1844, and 1845.

* Transactions of the Royal Society of Edinburgh, Vol. XVI. Part. 1.

The mean horizontal force for each month is nearly the same as the mean for midnight and for 1^h P.M.

The secular variation is positive, the increase being considerably less for 1845, than for any of the previous years.

When the annual variation is eliminated from the means for each month, the author finds that the secular variation is scarcely sensible about $6^h\ 40^m$ A.M., an hour *after* the morning maximum, in the first six months of the year 1844, and about $4^h\ 40^m$ P.M., an hour before the afternoon maximum, in the last six months of the same year.

A lunar period for the horizontal force has been deduced, which (like the annual period, and the sun's declination) shews maxima when the moon has its greatest N. and S. declination, and minima between these periods. Each of the years 1844 and 1845 give nearly the same result.

The author has also deduced a period depending on the moon's phase or synodical revolution, consisting of a maximum of horizontal intensity, about two or three days after the new moon, and a minimum about two or three days after the full moon: this period is shewn with the same distinctness and regularity in each of seven months of 1844.

The Observations for 1844 and 1845 have each been investigated for the determination of a period connected with the moon's hour angle; the author finds this period to consist of two maxima and two minima. From the Observations for 1844, *the* minimum occurs about 5 hours before the moon's meridian passage, *the* maximum about 1½ hours after the passage of the inferior meridian; a maximum occurs before 4^h after the meridian passage, and a minimum about 8 hours after the meridian passage. These periods have been verified by the Observations for 1845, excepting that, for 1845, the secondary maximum occurs before 3 hours after the meridian passage: the range for each year is nearly the same, being about a tenth part of the mean diurnal range for the year.

3. Experiments and Investigations as to the influence exerted over some Minerals by Animal and Vegetable matter, under certain conditions. By Mrs Margaret Henrietta Marshall. Communicated by Sir T. M. Brisbane, Bart.

It occurred to the author, while examining the fossils of various strata, that possibly the animal matter of the animals originally included in the rock might have played some part in its consolidation, and thus, after their destruction as living animals, they might have continued to serve a useful purpose.

After many unsuccessful experiments, made with common lime, and raw animal or vegetable matter, she had recourse to sulphate of lime, and found that plaster of Paris, when mixed with water, and with not more than $\frac{1}{128}$th part of the weight of the whole mass of mixed animal and vegetable matter, it did not set instantly, but continued for some hours quite plastic, developed, in setting, more heat than usual, and finally acquired a remarkable degree of hardness, and the power of resisting both water and fire to a very great degree. A specimen, was exhibited, made in this way.

In other specimens, three parts of sand had been mixed with one of gypsum, and $\frac{1}{64}$th of the whole of iron had been added, with the same amount as before of organic matter. One specimen had been exposed for two years to running water, and was but little affected.

Time and exposure have a most remarkable effect in increasing the hardness and compactness of these mixtures.

Specimens were shewn of Arran granite, broken down and re-consolidated by means of $\frac{1}{10}$th of gypsum, and the usual proportion of organic matter. One of these has even taken a certain amount of polish. The author suggests, that possibly granite may, in some cases, have been brought into its present state, not by fusion, but by the combined agency of fire and water. She expects a favourable result from an attempt to consolidate granite without gypsum, which is now in progress.

Marine sands may be consolidated by the same means as those above employed, but more slowly. Several specimens were exhibited, one with an alga imbedded and well preserved. Liquor of mussels and sea-weeds were found to yield sufficient animal and vegetable matter to effect consolidation.

In several specimens, star fish and other mollusca were imbedded, and generally continued distinctly visible. They also discharged the colour of iron from the parts surrounding them.

The animal and vegetable matter used by the author was glue and starch, in her first experiments with gypsum; but in those made with marine sands, and those in which remains were imbedded, she employed the animal matter contained in the liquor of mussels, and the vegetable matter of fuci. The materials were boiled together, and then left for a longer or shorter time till consolidation took place.

As gypsum, which was used in most of these experiments, does not occur in the strata supposed to have been possibly consolidated by the action of animal and vegetable matter, the author, to ascertain whether gypsum might be dispensed with, employed pure magnesia, and with success. Specimens of consolidated magnesia were shewn. From some other experiments, the author is inclined to believe that animal matter, fossilized in the centre of a mass containing lime, attracts to itself the lime from the general mass.

Further experiments must determine how far the observations of the author are capable of being applied to explain any part of the phenomena of the consolidation of strata, or of the fossilizing of organic remains.

The following Gentlemen were duly elected Ordinary Fellows of the Society:—

Dr Taylor, of Pau.
Dr Pagan, F.R.C.S., Edinburgh.

The following Donations to the Library were announced:

Recueil des Actes de la Séance Publique de l'Académie Impériale des Sciences de Saint Petersbourg, tenue le 29 Decembre 1844.

Memoires de l'Académie Impériale des Sciences de Saint Petersbourg (Sciences Politiques, &c.) Tome V., Livres 5 and 6.

Memoires de l'Académie Impériale des Sciences de Saint Petersbourg (Sciences Mathematiques, &c.) Tome IV., Livres 6.

Memoires de l'Académie Impériale des Sciences de Saint Petersbourg (présentés par divers Savans.) Tome IV., Livres 6.—*By the Academy.*

Catalogue of the Edinburgh Subscription Library, 1794-1846.—*By the Office-Bearers of the Library.*

Journal of the Statistical Society of London. Vol. VIII., Part 4.—*By the Society.*

Proceedings of the Geological Society of London. Vol. IV., No. 104. —*By the Society.*

The Journal of Agriculture, and the Transactions of the Highland and Agricultural Society of Scotland. January 1846.—*By the Society.*

Journal of the Asiatic Society of Bengal, Nos. 146 and 147.—*By the Society.*

The Derivation of many Classical proper Names from the Gaelic Language, or the Celtic of Scotland. By Thomas Stratton, M.D.—*By the Author.*

Monday, 19*th January* 1846.

Right Rev. BISHOP TERROT, V.P., in the Chair.

The following Communications were read:—

1. On the Action of Soluble Lead Salts on Natural Waters. By Professor Connell.

In a former communication to the Society, the author noticed that spring, well, and river waters, even after being boiled, usually yield, with acetate of lead, a precipitate readily soluble, in whole or great part, in acetic acid; and as the solution appeared not to be attended with effervescence, it was conceived to be due to organic matter. The author has since found that effervescence is more common than was at first supposed, and in that case the precipitate is due to the presence of carbonate of lime in the water. It was ascertained by boiling a solution of carbonate of lime in water containing excess of carbonic acid, that the trace of carbonate of lime retained in solution after ebullition, was too slight to explain the reaction of the spring waters, on the idea that it had been originally taken up by them in this way. It was farther found that distilled water, when left in contact for several days in a close vessel with impalpably pounded marble, took up a very little more; but what the author conceives to be the more probable source of the carbonate of lime in the spring waters is double decomposition between a soluble lime-salt and a carbonated alkali. The reaction may be imitated by adding to an ounce of distilled water one drop each of carbonate of potash, muriate of lime, and sulphate of magnesia, when acetate of lead will be found to yield a cloud like spring waters, soluble with effervescence in acetic acid. When the precipitate in the spring water is truly dissolved without effervescence by acetic acid, it is due to organic matter, provided no chloride is present in sufficient quantity to produce the reaction.

2. Claudia and Pudens; an attempt to shew that the Claudia mentioned in St Paul's Second Epistle to Timothy, was a British Princess. By Archdeacon Williams.

The following Donations to the Library were announced:—

Third Bulletin of the Proceedings of the National Institute for the Promotion of Science at Washington. February 1842 to February 1845.—*By the Institute.*

Tenth Letter on Glaciers.—*By Professor J. D. Forbes.*

Notes on the Topography and Geology of the Cuchullin Hills in Skye, and on the Traces of Ancient Glaciers which they present. By Professor J. D. Forbes.—*By the Author.*

Journal of the Asiatic Society of Bengal. No. 158.—*By the Society.*

The Electrical Magazine. Conducted by Mr Charles V. Walker. October 1845.—*By the Editor.*

The Journal of the Royal Geographical Society of London. Vol. XV., Part 2.—*By the Society.*

Bulletin de la Société de Geographie. Tome III., of 3d Series.—*By the Society.*

Abhandlungen der Königlichen Akademie der Wissenschaften zu Berlin. 1843.

Bericht uber die zur Bekanntmachung geeigneten Verhandlungen der Konigl.-Preussische Akademie der Wissenschaften zu Berlin. Juli 1844 bis June 1845.—*By the Academy.*

Novorum Actorum Academiæ Caesareæ Leopoldino-Carolinæ Naturæ Curiosorum voluminis decimi noni Supplementum et volumen vigesimum.—*By the Academy.*

Scheikundige Onderzoekingen gedaan in het Laboratorium der Utrechtsche Hoogeschool. Deel III., Stuk. 3.—*By the Editors.*

Monday, 2d February 1846.

Sir T. M. BRISBANE, Bart., President, in the Chair.

The following Communications were read :—

1. On the Decomposition and Dispersion of Light within Solid Bodies. By Sir David Brewster, K.H.

After noticing various cases of solid and liquid bodies, which disperse light of a colour quite different from the transmitted tint, and Sir John Herschel's explanation of the phenomenon in the solution of acid sulphate of quinine, which, although itself colourless, disperses light of a bright blue, the author considers—

1. The internal dispersion of fluor-spar. By employing a more intense light than Sir J. Herschel did—namely, a condensed beam of the sun's light—he conceives that he has proved that the dispersion is not, as Sir J. Herschel supposes, superficial or epipolic, but, on the contrary, belongs, in some specimens at least, to every part of the crystal. The phenomenon is only seen in the green fluor of Alston, and in some pink and bluish-yellow varieties from Derbyshire. Some specimens are formed of strata alternately dispersing and non-dispersing. The author has not found the same appearances in any other mineral; but he has observed similar phenomena of dispersion in several kinds of glass, and even in some colourless glasses, which disperse a fine green tint.

2. On the internal dispersion of the acid solution of sulphate of quinine. Here, also, by using a more intense light than Sir J. Herschel, the author establishes that the peculiar dispersion occurs, not only in a stratum 1-50th of an inch thick at the surface of the liquid, but at all parts of it ; and he concludes —*First*, That a beam of light, epipolised by the action of a solid or a liquid, is capable of further dispersion, provided the thickness of the medium has not been sufficient to disperse all the dispersible rays. *Secondly*, When such a medium is thus rendered incapable of dispersing more light, it is not, as Sir J. Herschel supposes, because the light has lost a property which it previously possessed, but because it has been deprived of all the dispersible rays it contained.

The rays thus dispersed are few in number, and, by their mixture, yield blue light, but they extend over a great range of refrangibility ; while other rays, equally refrangible, are either less dispersible, or not dispersible. But this appears less surprising when we advert to the phenomena of absorption.

The difference between absorption and dispersion is, that, in the former case, the absorbed light is extinguished; while, in the latter, the dispersed light is visible. Hence, if, in powerfully absorbing bodies, the absorbed light could be rendered visible, we should have the phenomena of epipolised light.

3. On the polarisation of dispersed light. Sir J. Herschel had failed to detect the polarisation of epipolised light; but the author, by using a condensed sunbeam, discovered that the dispersed beam was polarised, partly in the plane of reflexion, and partly in a peculiar manner; which the author calls *quaquaversus* polarisation, like that effected by a congeries of small doubly-refracting crystals, having their axes in all directions. He afterwards discovered instances in which each of these kinds of polarisation was found alone. These experiments have led to more extended investigations, to be subsequently communicated. The remainder of the section contains a minute description of the beautiful phenomena of the polarisation of dispersed light. The author thinks it probable that the study of these phenomena may throw much light on the internal structure of the substances exhibiting them.

4. On the causes of the decomposition and internal dispersion of light. The author ascribes the phenomena (which do not, in fluor-spar, belong to the species, but only to certain varieties) to irregular crystallisation, and the successive deposition of strata of different refractive and dispersive power. He does not, at present, offer any theory of the cause in liquids.

2. A few Remarks suggested by Professor Forbes's Description of the effects of Glacial Action among the Cuchullin Hills, and Mr Maclaren's views of the facts observed by him at the Gareloch. By Sir G. S. Mackenzie, Bart.

The object of this paper was to shew, that if it be assumed that glaciers had actually existed among the Cuchullin Hills, or in other parts of this country, one of two conditions must necessarily have been present—either a climate much colder than the existing climate of Scotland, or a higher position of the land where the supposed marks of glaciers are seen. With respect to the first condition, the permanency of the relative positions of the heavenly bodies, and of the inclination of the earth's axis, and the adaptation to existing climates of animal and vegetable life, were appealed to as rendering it improbable. The other condition was maintained as probable, on the ground that the changes which have undoubtedly taken place in the positions of the rocky masses forming the crust of the earth, prove that parts of these masses had been depressed, while others had been elevated; so that the probability was, that the Cuchullin Hills had stood at a higher elevation than they do at present, reaching above the line of perpetual congelation.

The crust of the earth having been broken up, was formerly propounded by the author of this paper as having been the cause of such a disturbance of land and water, as would fully account for all the phenomena now presented by the surface of the globe. It was stated, that, while no proof could be brought to shew a change of climate, we had evidence in abundance of intense internal heat, which has been known to have produced force sufficient to elevate the land. Nothing seems more likely, when the bursting of the crust gave vent to the expansive matter, than the sinking of much of the crust into the vacuities produced by its escape, and the tilting up of the rest; thus giving to the surface its present rugged and uneven aspect. During such a convulsion, masses bearing glaciers may have sunk with them below the line of perpetual congelation.

The observations on Mr M'Laren's theory, which ascribed two different co-existing phenomena (striated marks and travelled boulders) to two causes—the one glaciers, and the other icebergs—went to shew, that, if a glacier had existed in the Gareloch, it would be necessary to determine whether the boulders were deposited, and the glacier was formed before or after the rising of the sea, and after its subsidence; the sea having been supposed to have risen 2000 feet, to admit of icebergs bearing boulders floating into the Gareloch. This elevation of the level of the sea had not been accounted for in any way; nor had it been considered that the supposition involved its having risen all over the globe; and no space had been provided to receive it when it retired. It was shewn that, in such a condition of things, Scotland must have been a cluster of small islands, the summit of the present mountains, which certainly would have had a climate colder than the country generally has now. But it was also shewn, that were the sea elevated, as supposed, 2000 feet higher than at present, the regions where icebergs exist now would have been a solid mass of immoveable ice, extending far above and beyond the land from which icebergs, in our day, are occasionally detached. It was supposed that the phenomena described by Mr M'Laren could be well accounted for by the effects of the crust of the earth having burst. The author did not enter on this subject.

3. Remarks on certain grooved surfaces of Rock on Arthur's Seat. By the Rev. Dr Fleming.

Dr Fleming read a paper, entitled " Remarks on certain grooved surfaces of Rock in Arthur's Seat." He referred, in the first instance, to the surface of the bed of porphyry, known as the Bog Crag, and contiguous to the Hunter's Bog; and shewed, from the directions of the markings being neither parallel to the valley, nor the neighbouring cliff, that they could not be the result of glacial, diluvial, or iceberg action, but were probably caused by the sliding of the contiguous beds at their upheaval; and this view he considered strengthened by the state of the detritus of the cliff of the Well Crag. He then took notice of a grooved surface of basalt, contiguous to a singular trough to the east of St Anthony's Well, but which might have been produced either by glacial or diluvial action.

The following Gentlemen were duly elected Ordinary Fellows of the Society.

The Rev. Dr James Robertson, Professor of Church History.
Mr J. A. Adie, Civil-Engineer.

The following Donations to the Library were announced.

Annuaire de l'Observatoire Royal de Bruxelles, par E. Quetelet, Directeur de cet Etablissement, pour 1845.

Annuaire de l'Academie Royale des Sciences et Belles Lettres de Bruxelles, pour 1845.

Bulletins des Séances de l'Academie Royale des Sciences et Belles Lettres de Bruxelles, 1844, Nos. 9, 10, 11, 12, and 1845, Nos. 1, 2, 3, 4, 5, 6.

Annales de l'Observatoire Royal de Bruxelles, publiées aux frais de l'Etat, par le Directeur A. Quetelet. Tome IV.

Nouveaux Memoires de l'Academie Royale des Sciences et Belles Lettres de Bruxelles. Tomes XVII. and XVIII.

Memoires Couronnés et Memoires des Savants Etrangers publiés par l'Academie Royale des Sciences et Belles Lettres de Bruxelles. Tomes XVII. and XVIII.—*By the Academy.*

Memoire de Simon Stevin, par A. Quetelet.—*By the Author.*

The Geology of Russia in Europe and the Ural Mountains. By Roderick Impey Murchison, Edouard de Verneuil, and Count Alexander von Keyserling. 2 vols. 4to.—*By the Authors.*

The American Journal of Arts and Science, conducted by Professor Silliman, for January 1846.—*By the Editor.*

Monday, 16*th February* 1846.

Dr CHRISTISON, Vice-President, in the Chair.

The following Communications were read :—

1. Biographical Notice of the late Sir John Robison, K.H., Sec. R. S. Ed. By Professor Forbes.

"A laudable usage was formerly more prevalent in this Society than it is at present, of recording the merits and services of persons who have conferred honour upon it by the one, or rendered themselves its benefactors by the other. Some very elaborate biographies have thus been communicated ; but, in the old "History of the Society" (to which our published "Proceedings" are in some degree parallel), numerous short notices are to be found of a kind imposing less labour on the biographer, and admitting of more extensive application than the larger memoirs just alluded to. In the hope that such notices may be more frequently contributed, I beg now to offer, within the short compass in which the very limited materials now at my disposal enable me to condense my task, a brief notice of the Life of the late Sir John Robison, whose most energetic efforts were, for many years, constantly directed to the promotion of the welfare of this Society ; a circumstance which seems to call for more specific notice than was comprised in the thanks and acknowledgment voted by the Society during his lifetime,* and which naturally devolves upon me the duty of drawing it up, not only as his successor in office, but as having enjoyed his friendship, and having been, on several occasions, indebted to him, in no common degree, for personal kindness.

"Sir John Robison was born on the 11th June 1778. His father was the late eminent Dr Robison, Professor of Natural Philosophy in our own University, and Secretary of this Society, who died in 1804 ; and his mother, formerly Miss Wright, still survives him, at an advanced, but robust old age.

Transactions of the Royal Society of Edinburgh, vol. XV., p. 680.

"Mr Robison was educated at the High School of Edinburgh, and afterwards attended some classes at College, including, of course, his father's, who, perceiving in him a decided mechanical turn, removed him from thence, after three years' study, and placed him under the charge of his old and valued friend, Mr Houston of Johnston, near Paisley, who was at that time erecting extensive manufactories upon his estate for the spinning of cotton, with the advantage of the then recently invented machinery of Arkwright. Here Mr Robison appears to have acquired the taste for constructing and superintending machinery, which remained with him during life. Two or three years afterwards, Mr Robison was removed to Manchester, where he remained in some degree under the protection of the late Mr Watt, whom, it is probable, that he visited at Soho, and made there the acquaintance of the younger Mr Watt, and the other members of that celebrated establishment. His father's great intimacy with the illustrious inventor (as he must in strictness be termed) of the steam-engine, and the great mutual services which these eminent men had rendered to one another (by far the greatest, however, in the ordinary acceptation of the term, being conferred by Dr Robison), of course, introduced Mr Robison with the utmost advantage to an establishment, then not only the first, but the only one of its kind, and must naturally have strengthened his own bent towards mechanics, and introduced him to more enlarged and correct notions of engineering than he was likely to attain in the direction even of the greatest cotton factory. I mention these circumstances now, because, though little information has reached me of his early connection with the Soho establishment, there can be no doubt that it exerted the most important influence on his future life, and the friendships which he then formed were amongst the most valuable, and, to the honour of all parties, the most lasting which he enjoyed, as we shall presently see.

"The truth of what I have now stated, as to the probable enlargement of his views and knowledge by his intercourse with Mr Watt, will appear from the speedy change in his professional views which followed upon his visit to Manchester; for, having remained in business there only a year or two, he obtained, through the influence of his father's friend, General Hay Macdowall, in 1802, a mercantile situation in Madras,* which occasioned his being sent up the country to Hyderabad, to establish a branch of the business for the purpose of collecting and transmitting the muslins and other manu-

* It would appear from a letter addressed to him by Mr Watt junior, which is still preserved, that his first destination was to Ceylon.

factures of the natives. His activity and mechanical tastes did not, however, probably find sufficient scope in this merely mercantile position, which offered nothing resembling the superintendence of the machinery to which he had been accustomed at Johnston and Manchester; and he managed, under favour of circumstances which are unknown to me, so to improve his relations with the natives, and with the native prince the Nizam, in whose territories he resided, that he contracted for the establishment and maintenance of the artillery corps in his Highness's service, including the furnishing of guns and ammunition. Of this concern Mr Robison was not only the upholder, but also the commanding officer; the subordinate officers were appointed by him, and every expense defrayed. In security for the annual charge thus incurred, the taxes of a particular province or district were assigned over to him. Every branch of artillery-engineering must therefore have become familiar to him, and probably constituted one chief attraction of his situation. I understand that he obtained from England the very best implements of manufacture, but we may easily conceive the scope given to his remarkable ingenuity in conducting so extensive an undertaking in so remote a place. He always spoke with admiration of the intelligence and mechanical talents of the natives.

"His contracts were fulfilled, as I understand, with satisfaction to his employer, and profit to himself. His mechanical skill, and his acquaintance with the advanced state of Art in Britain, were duly appreciated by the Nizam, who desired him to construct and lay out a house and grounds on the English model, and to procure for him many European conveniences. The best understanding existed between them, until having realized a competence and desiring to return once more to society and his home, he left India in 1815, being then thirty-seven years of age. His father having been long dead, he settled in the west of Scotland, and married, the following year, Miss Grahame of Whitehill, who died in 1824. He, subsequently, married Miss Benson, who also predeceased him. Two daughters by his first marriage survive him. Mr Robison's first residence was at the Grove, near Hamilton; but after some years he came to reside in Edinburgh, of which the first account I have was, that he was elected Fellow of the Royal Society of Edinburgh, 22d January 1816, and took part, in 1821, in the formation of the Society of Arts. And here the third and best known part of his life commences, —that in which we are more particularly interested.

"I shall first speak of his connection with the Royal Society. The division of the Physical and Literary Classes of the Society was then more strictly preserved than it is at present. Mr Robison was elected Secretary of the Physical Class in 1823; and upon the retirement of Dr Brewster from the chief secretaryship in 1828, Mr Robison succeeded him, and continued to hold that office till 1840, when he resigned, contrary to the wishes of all those who had been most nearly connected with him in the discharge of the duties of his office. It may be safely affirmed, that, under the favouring circumstances of affluence and leisure, seconded by congenial tastes and singular habits of order, he conducted the affairs of the Society with more watchful and anxious superintendence than is often found in such cases; and during the course of years already mentioned, the care and direction of our affairs might be said to be his chief business; for, unencumbered by any professional demands on his attention, he passed successively from object to object of the many schemes, inventions, and projects, which successively occupied his attention, but he always attached himself to the concerns of our Society as his chief and main occupation. Having been, even before my admission as a Fellow, in frequent communication with Mr Robison, and almost ever since, until his retirement, having acted as his co-operator and junior Secretary, with the valuable co-operation of my senior, Dr Christison, I have had innumerable opportunities (and I know that Dr Christison will unite in the same statement) of observing the unbounded pains which he took, publicly and privately, of fulfilling what he considered to be his duty, and of maintaining, at any sacrifice of trouble, the rights of the Society, and those of its members. In particular, we owe to him, in conjunction with our worthy President, Sir Thomas Brisbane, the endowment which we receive from Government, and which places this Society upon a surer footing of independence and respectability than any other circumstance connected with its affairs which has occurred for many years.

"Sir John Robison, as we must now call him (for he received the honour of the Guelphic Order from King William IV. in 1837, and of Knighthood from the present Queen in 1838), rarely contributed to our scientific proceedings, and the circumstance is worthy of remark: his only printed paper being one on Whitelaw's escapement, and the time-keeper set up in our Hall.* This can only be attributed to one cause, to a motive, in fact, highly honourable to him. Sir John Robison never made any pretension to be considered

* Edin. Trans., vol. xi., p. 345. This paper was read 7th Feb. 1831.

as a fully accomplished man of science, although, undoubtedly, many persons have been ranked as such upon very inferior claims. It was part of his character. Like Dr Wollaston, he had the most precise and methodical line of demarcation between what he wished to be considered competent to decide, and what he declined offering any opinion upon. It was a caution which might be well deemed excessive, as his real acquirements were undoubtedly greater than even his intimate friends were fully aware of. His own opinions also were, for the same reason, often sheltered under the impersonal form. In almost no instance that I recollect of did he suffer his name to be put as a responsible member of a Committee of the Council to decide on the merits of purely scientific communications. Nor could this have arisen from any unwillingness to such occupation or responsibility; for in another Society, of whose general business he considered himself a competent judge, no one was more active in such capacities. His backwardness in this particular can only, therefore, be ascribed to a very strict appreciation of his own acquired knowledge. His youth and middle life were spent, as we have seen, in circumstances little favourable to the acquirements of abstract science, or of literary dexterity. When leisure permitted, he did not become a regular student, which could hardly be expected at his time of life; and he seldom made a profound or consecutive study of any of the scientific topics which continually presented themselves to his acute remark during his multifarious occupations. Thus, when he associated with scientific men, after so many years spent remote from European society, he was generous enough rather to throw useful hints in the way of others, and to aid their prosecution, than to hoard the many happy ideas which he struck out, or present views essentially philosophical in a crude or merely practical form. I conjecture, too, that, clearly as he at all times expressed himself, whether in conversation or in writing, he had a sort of dislike to formal literary compositions, which contrasted with his father's extraordinary fluency in this respect, and which hindered him from attempting them. This may be inferred from the paucity of his acknowledged compositions during so active a life, which consist only, so far as I know, of the paper on Whitelaw's Timekeeper already mentioned, an article on Turning, in the Edinburgh Encyclopædia; a description both in English and French (at least the latter), of two Plates published by Mr John Milne, of a large Pumping Steam-Engine; an account of the failure of a Suspension Bridge at Paris, and another paper in the Edinburgh Journal of Science, and two or three of the numerous communications presently to be mentioned as contributed to the Society of Arts.

"In all other respects than as a contributor, his services to the Royal Society were numerous and incessant. By his example and assistance, its affairs were conducted in a more methodical and exact manner than had previously been usual; he kept more exact minutes than had before been done; he contributed to our Museum and its re-arrangement; he charged himself almost exclusively with the editorial labour of our Transactions; and, by his extensive acquaintance with the scientific men of Paris, where he was himself favourably known, he also extended the connections and character of this Society.

"But whilst thus intimately connected with the Royal Society, he took a great deal of concern in another, the Society of Arts, which he had contributed to establish, of which he acted as Secretary from 1822 to 1824, and of which he was twice Vice-President, and finally President for the year 1841–2. By far his most numerous public communications were to that Society, and the complete list of them which the present Secretary, Mr Tod, has, with great courtesy and labour, extracted for me from the minute books, includes about 60 contributions of one kind or other, spread over a period of 17 years. It would be wrong to judge of these communications as formal papers; the nature of the proceedings of the Society admitted of merely verbal and occasional notices not necessarily of an original character. During his frequent visits to Paris he seldom failed to pick up some improvement in the ornamental or useful arts, which he thought worthy of being known to practical men at home, and farther publicity he did not desire. Such were many of his communications; others of very various degrees of importance were properly original, comprising the results of his own experiments on mechanical subjects. And though many of these appear trivial by the titles, few persons capable of judging of or appreciating the ingenuity of his improvements, or the beauty of the workmanship by which he rarely failed to illustrate them, considered them really to be so. To myself, who was for a long period a constant visitor of his workroom, these memoranda recall the rapid succession of most ingenious plans and contrivances with which he was almost continually occupied, and of which only a very small part ever underwent even a partial publication. Amongst the more prominent of his communications to the Society of Arts may be reckoned his improvements in the difficult art of cutting accurate metal screws, for which the Keith

Medal of that Society was awarded to him, after a sufficient interval had elapsed to test the utility of the invention, which is declared by the Committee who examined it, "from the numerous testimonials by practical men in favour of the method described, and detailing the superiority over others in use, to deserve the highest mark of the Society's approbation." An honorary medal was soon after awarded to Sir John for a notice of experiments on the Forth and Clyde Canal, on the resistance to vessels moving with different velocities,—a subject of great interest and importance, then newly brought into special notice, from the ascertained possibility of tracking vessels advantageously at a high speed. Amongst other subjects of general interest included in his communications were the application of hot air to warming houses; of gas to the purpose of illumination, and also to the purpose of heating. These two last subjects, which admit of a great deal of practical sagacity in their study, were for many years almost constantly under his notice, and many of his friends recollect his success in organizing a kitchen in which gas was the chief combustible.

"It may, no doubt, be matter of regret that Sir John Robison did not apply more of his attention to practical problems, the solution of which would be of extensive value. The truth is, that the occupations of his life in Britain were only pastimes, and he never attached to them a very serious importance. I may mention one instance which came to my knowledge, of a practical suggestion, I believe of his own, which is undoubtedly of much utility. He remarked to me, a few years before his death, that the construction of astronomical instruments appeared to be much misunderstood by opticians, who are generally by no means skilled in the engineering of structures, or in mechanics on a large scale. Most astronomical instruments, whether large or small, have been, from the earliest times, constructed of a *multitude of detached fragments*, screwed and clamped together, very elegantly, indeed, but with a complete sacrifice of the firmness to be gained by solid castings. For large instruments, especially, Sir John observed that a Manchester machine-maker would turn out a far better article than the best optician in London has the power of making, wanting the necessary implements. I was much struck by the remark, and not long after I heard exactly similar views expressed by Mr Airy, the Astronomer-Royal, who has detailed them in an able paper in the *Notices of the Astronomical Society*, 13th May 1842, where he describes a zenith sector, constructed upon engineering principles, identical with those of Sir J. Robison's, the principal pieces having been both cast and worked by Messrs Maudsley.*

"To sum up here what may be said on Sir John's mechanical talents, it may be affirmed, that his intimate acquaintance with Technology or the useful arts, was equalled by very few persons living. Many persons who have been delighted by the exquisite specimens of handiwork produced by his own lathe, may have supposed that his sagacity consisted in what may be called neat-handedness. But it was so far from being limited to this, that he may be said to have possessed a minute technical acquaintance with a host of different trades; and such combined knowledge is an uncommon and valuable acquisition in itself. Any one who had occasion to ask advice or information on any point of the numberless operative professions into which modern ingenuity and modern luxury have divided the comparatively few practical trades known a century ago, were rarely disappointed in receiving from Sir John Robison a clear and categorical answer as to the processes and materials in use, and not unfrequently a suggestion for their extension or improvement. I can safely say that for many years our late Secretary was to me an unfailing referee upon such questions; he formed a valuable link between the men of theory and the men of practice of our time. Amongst my own acquaintance, Mr Babbage and Professor Willis are the only persons who could be brought into comparison with him in this respect.

"This extensive technical knowledge had several antecedents. First, may be ranked a larger share of theory than he ever took credit for, or than any one would readily have supposed that he possessed; this I have only learned by casual circumstances: Secondly, a continual intercourse with well-informed persons in every department of art,—not only with such men as the present Mr Watt, who has kindly communicated to me his own testimony to the value set upon Sir John Robison's talents and acquirements, not only by himself, but by his illustrious father, and by Messrs Boulton and Murdoch his coadjutors in the Soho establishment, or as Mr Peter Ewart, formerly of Manchester, then of Woolwich, or as the late Mr Barton of the Mint, or Mr Oldham of Dublin, who were his particular friends,—but with the wholesale and retail manufacturers and engine-makers of Birmingham, and more particularly of Manchester, where

* I cannot affirm positively that Sir J. Robison's opinions were original to himself, but I understood them to be so; and I know that his conversation with me long preceded the date of Mr Airy's paper.

he retained the connection of a constant friendly intercourse, which probably in many instances dated with his residence in that town at his outset in life. He was also intimately acquainted with the workshops of numberless ingenious tradesmen, not only in the strictly manufacturing towns of England, but in London, Edinburgh, Glasgow, and Paris, by whom he was known and valued. But, thirdly, all this would have been unavailing, had he not practically executed his own designs, and learnt, by experience, more or less of the difficulties of almost every trade which came under his notice. The varieties of the constructions which he executed shew great diversity of acquirement. From boring a cannon to drilling a needle's eye, nothing was strange to him ; the first, we have seen, was amongst his earlier serious occupations, the last was a problem which occupied him not long before his death. Masonry, Carpentry, and manufactures in metals, were almost equally familiar to him ; his house in Randolph Crescent was built entirely from his own plans, and nothing, from the cellar to the roof, in construction or in furniture, but what bore testimony to his minute and elaborate invention and superintendence. A door handle, a chair castor, a pair of tongs, and a candlestick, he deemed equally worthy of attention with the more important particulars of warming and ventilation, security from fire within, and from wind without. Some panes of glass having blown in by a storm, he was led to some curious observations (communicated to the Society of Arts) on the proper mode of placing window-glass in the sashes. Many particulars of his minute and ubiquitous experience may, I think, be found in different parts of the useful writings of the ingenious Mr Loudon, with whom he was in frequent correspondence. The poet Cowper says, "How various his employments whom the world deems idle." And those who knew Sir John Robison well, and who know at the same time the immense expenditure of time required for attaining mechanical dexterity in any department, or, when attained, the labour necessary to produce even the smallest or simplest object when of exquisite execution, wondered how he was enabled to perform so much. His correspondence alone would have been more than an ample occupation to most persons. The secret lay, as in all such cases, in habits of perfect order, of never doing two things at once, and of devoting his whole thoughts and skill to what immediately occupied his attention ; but to this we must add habits of great personal activity, which throughout his life rendered him incapable of sleeping for more than a short period, and a powerful constitution, which enabled him for long periods almost to dispense with exercise.

"There were other public bodies in which he took an interest besides those which have been mentioned; but they may be briefly passed over. For a number of years he was an active member of the Highland Society, and commonly Chairman of their Committee on Agricultural Implements and Machinery. He assisted in the formation of the British Association for the Advancement of Science at the meeting at York in 1831; and he acted most energetically in the capacity of Local Secretary to that body upon occasion of its visit to Edinburgh in 1834, when he also entertained M. Arago at his house. Nor did he think the regulation of the affairs of the City Police below his notice; and, like the distinguished scientific foreigner and friend just mentioned (whom I have heard dilate as warmly upon the achievement of paving a *boulevard* as upon the discovery of a comet), he sat for some years amongst the Police Commissioners, and suggested some improvements, generally felt to be such, though the originator is not so commonly known. He would undoubtedly have introduced many others; but his coadjutors probably thought more of saving the public money, *he* of the public convenience.*

"Before closing this brief notice I have pleasure in adverting to a trait of our late Secretary's character, which did him honour. I mean his sincere desire to render himself useful to those with whom, as officially representing the Society, or from congeniality of pursuit, he was brought in contact. It may be, that all whom he thus favoured were not equally deserving, but that, as a general rule, he endeavoured to attract notice to merit cannot be doubted. The slightest recommendation, or none besides ingenuity, were sufficient to claim an interest in him, and during many years his house and his table were hospitably open to those who asked his advice or required his encouragement. I have a pleasure in recording my personal obligations to him at a time when I had scarcely a single scientific friend; when, and for long after, I received from him such substantial proofs of kindness, as it is impossible that I should ever forget.

* The method of naming the streets in a conspicuous and durable manner was a favourite topic with him, and it is a duty of the police certainly too much neglected, more especially in London. The substitution of our present cast-iron lamp-post, with the name of the street cast on each, replacing the expensive, ugly, and ineffective construction of the old ones, was, I believe, due to Sir John.

The considerable number of foreigners who annually visit Edinburgh, and to whom his leisure was always dedicated, found at his house almost a home; and as he spoke the French language with fluency and correctness, his society was invariably sought by them, whatever might be their department of study, their country, or political opinions. The exiled Royal Family of France, and their numerous friends, had, during their stay in Edinburgh, reason to be gratified by his attentions, and they shewed that they were so. Down to the period of his death he remained in correspondence with some of those whose acquaintance he then made; and their letters shewed that his zeal for their interests was not restricted to the period of their stay in this country.*

"Sir John Robison was, like his distinguished father, of a tall and commanding figure. No likeness has, I believe, been preserved of him, except a daguerreotype and some calotypes, done shortly before his death. One of these is an admirable portrait. In general he enjoyed excellent health, and was scarcely ever absent from the meetings of any society which he usually attended. A considerable deafness, which affected him for the last two or three years, was, however, exceedingly irksome to him, and interrupted this regularity. Although subject for many years (at least since 1831) to occasional palpitation of the heart, he kept disease at a distance by habitual abstemiousness; and his constitution did not appear to suffer any shock until the 6th February 1843, when he abruptly quitted his usual place at the table of the Royal Society Club, owing to some indisposition, which did not, however, immediately assume a threatening form. The activity of his mind did not desert him; for, on the 27th February, he gave a notice of Messrs Naysmith's beautiful invention of the Steam Forge-hammer at the Society of Arts, which was also the principal subject of his last personal communication with myself. His death, which took place after only a few days' illness, and which was occasioned by the rupture of some important bloodvessel, occurred on the 7th March 1843, being then in his 65th year."

* A letter to him, from one of the exiled French nobility, which arrived a few days after his decease, was filled with commissions involving no common trouble and responsibility, which he was requested to execute.

2. On a method of rendering Magnetical Instruments Self-Registering. By J. A. Broun, Esq. Communicated by Sir T. M. Brisbane, Bart.

A sheet of paper being stretched tightly across a flat frame, if fine iron filings be dusted uniformly over it, and a fine pointed sewing needle, magnetised to saturation, held vertically, be moved below it, the point being kept at a short distance from the paper, the motion of the needle-point will be indicated by a fine line of filings which, attracted by the magnet, separate themselves from the homogeneous dust around. The fineness of this line will depend on the sharpness of the needle point, and the smallness of the particles of iron; the distance of the point from the paper will depend on the strength of the magnet, and the smoothness of the surface on which the particles lie. In some experiments already made, lines have been obtained as fine as could be produced by the sharpest pointed pencil, and this with a portion only of a small sewing needle. It will require a more extensive series of experiments to determine the best size for the magnet, and the best kind of paper for the filings. The particles of iron having a certain weight, are not easily shaken from their position if moderate care be taken.

If a fine needle be placed vertically at one extremity of a declination magnet, and a sheet of paper dusted with iron filings be moved by clock-work, in the direction of the magnetic meridian, the oscillations of the magnet towards the E. or W. will be indicated in the iron filings by the corresponding motions of the vertical needle.

One scale division of the declinometer used in the Makerstoun Observatory is equivalent to about 40″, and a tenth of a division or 4″ can be estimated: with the addition of a light and rigid rod to the declination magnet, by means of which the vertical needle could be placed at a distance of about three feet from the suspension thread, a change of declination to the amount of 6″ would be equivalent to a motion of ·001 of an inch at the needle, a quantity which may be estimated with the aid of a scale and vernier.

Mr Brown proposes to convert a vertical into a horizontal motion, by employing the influence of a magnet moved vertically upon another suspended horizontally. In this way he intends to measure the movements of the vertical force magnetometer, and of the common barometer, by means of a floating magnet.

3. Verbal Notice respecting the Thyroid, Thymus, and Supra-renal Bodies. By John Goodsir, Esq.

In a verbal notice of researches into the nature of the thyroid, thymus, and suprarenal bodies, Mr Goodsir stated that these three anomalous organs, situated respectively in the neck, chest, and abdomen, are in the embryo connected together in the form of two continuous masses, one on each side of the spine, from the base of the cranium to the origin of the omphalo-mesenteric arteries. The first or abdominal portion of each of these masses, grouped around the origins of the omphalo-mesenteric arteries, become the supra-renal capsules. The middle or thoracic portions, situated in the lateral parts of the thorax, where the lungs, at this period in the middle line, are afterwards to be, pass forwards to give place to the latter, and unite across the front of the pericardium, and become the thymus gland.

The anterior or cervical portion of this united mass separates from the posterior or thymus, groups itself around the anastomosing branches (inferior and superior thyroid arteries) of the first and second branchial arteries (the carotids and subclavians), and remains permanently as the thyroid.

The two lateral masses, out of which these three bodies are thus originally formed, Mr Goodsir has found to be a portion of the membrana intermedia of the area vasculosa of the egg. Such being the morphology, the detection of the function of these three bodies would require very delicate chemical inquiry. In general terms, however, it may be stated to be the same as that of the blastoderma.

The following Candidates were duly elected Fellows of the Society:—

Dr Buist, Bombay,
W. Murray, Esq., of Henderland.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. II. 1845–6. No. 28.

Sixty-Third Session.

Monday, 16*th February* 1846.

The following Donations of Books to the Library were announced :—

Journal of the Asiatic Society of Bengal.—No. 161.—*By the Society.*

Comptes Rendus Hebdomadaires des Seances de l'Academie des Sciences. Tome XXI. Nos. 17-25, and Tome XXII. Nos. 1, 2. —*By the Academy.*

The Electrical Magazine, conducted by Mr Charles V. Walker. January 1846.—*By the Editor.*

Proceedings of the Royal Society of London. No. 61.—Philosophical Transactions of the Royal Society of London, 1845. Part 2. —*By the Royal Society.*

Catalogue of Stars of the British Association for the Advancement of Science; containing the mean Right Ascensions and North Polar Distances of Eight Thousand three hundred and seventy-seven, reduced to January 1. 1850, with a Preface explanatory of their construction and application. By the late Francis Baily, D.C.L., President of the Royal Astronomical Society of London.—*By the British Association.*

Magnetical and Meteorological Observations made at the Royal Observatory, Greenwich, in the year 1843; under the direction of George Biddell Airy, Esq., M.A., Astronomer-Royal—*By the Royal Society.*

Kongl. Vetenskaps Akademiens Handlingar för Ar 1843.

Arsberättelse om Zoologiens Framsteg under Aren 1840-42. Första Delen af C. J. Sundeval.

Arsberattelse om Zoologiens Framsteg under Aren 1843-44. Andra Delen af C. H. Bohemian.

Arsberattelser om Botaniska Arbeten och Upptackter af J. E. Wikstrom.

Arsberattelse om Framstegen i Kemi och Mineralogi af Jac. Berzelius.—*By the Royal Academy of Sciences of Stockholm.*

Leçons de Géologie Pratique par L. Elie de Beaumont. Tome I.—*By the Author.*

Monday, 2d March 1846.

SIR THOMAS M. BRISBANE, Bart., President, in the Chair.

The following Communications were read:—

1. On the recent Scottish Madrepores, with Remarks on the Climatic Character of the Extinct Races. By the Rev. Dr Fleming.

The author, in this communication, referred, in the first instance, to the three species of Lamelliferous Polyparia, described in his "British Animals," Edin., 1828, exhibiting specimens of the *Caryophyllea cyathus*, and *Turbinolia borealis* of that work, together with a characteristic drawing, by the late Mrs Hibbert, of the *Pocillopora interstincta*, there alluded to as a native of the Zetland seas. He then exhibited a specimen, six pounds in weight, of the *Madrepora prolifera* of Müller, which was found last summer by fishermen, their lines having become entangled with it, in the sea between the islands of Rum and Egg. This species was known to Pontoppidan, as a native of the Norwegian seas, and is now ascertained to be a native of the Hebrides.

The author next exhibited specimens of the *Turbinolia sepulta* of the crag, together with a new and recent species from the Cape of Good Hope. In conclusion, the author observed, that while, from an acquaintance with the habits of a few *individuals*, we could safely speculate respecting the geographical and physical distribution of a species, we cannot, from our acquaintance with the history of one *species* of a genus, predicate with any confidence respecting the character of other species of the same genus. Thus, there are species of Madrepores natives of tropical seas, and there are species natives of the North seas. After illustrating his views by a reference to the species of the genera *Bos* and *Elephas*, the author closed his observations by stating, that the evidence, proving the climate, during the deposition of the mountain limestone, to have been warmer than at present, as derived from its contained organic remains, was defective, since the organisms compared did not belong to *individuals* of the same species, but to *species* of similar genera.

2. On the principle of Vital Affinity, as illustrated by recent Observations in Organic Chemistry. Part I. By Dr Alison.

The objects of this paper were, *first*, to vindicate the use of the term affinity, and assert the principle which that term is intended to express, viz., that in living bodies ordinary chemical affinities undergo a certain change or modification, either by the addition of affinities peculiar to the living state, or the suspension of some of those which act elsewhere; and, *secondly*, to attempt, from a review of facts recently ascertained, an exposition of the laws, according to which these modifications of ordinary chemical affinities take place, and a discrimination of those changes in living bodies, which may be ascribed to them.

In proof of the first of these points, the author referred particularly to the facts known as to the formation of starch, or its allied compounds, from carbonic acid and water by an action of certain parts of living vegetables under the influence of light, whereby the carbonic acid is decomposed and oxygen evolved; maintaining that this change, essential to the condition of all organized bodies, is so distinctly at variance with the ordinary chemical relations of carbon and oxygen, and even with those which shew themselves in other parts of vegetables in the living state, and in all parts in the dead state,—that we are equally bound to regard it as a strictly vital phenomenon, as the contraction of a muscle on a stimulus; and that we cannot rightly apprehend either phenomenon unless we regard them as dependent on certain laws of vital action or of vitality.

On the second point, he observed, that the physiologist is concerned only with those formations and resolutions of organic compounds which take place in the interior of living bodies, and that, premising that the first introduction of every species of organized being into the world must have been by a miraculous interposition of Divine Power, beyond the limits of scientific inquiry, the objects of investigation in this department of physiology appear to be more definite, and the strictly vital affinities which now operate, from the commencement of the life of vegetables to the death and decomposition of animals, to be fewer and simpler than had generally been supposed.

I. The first kind of action which may be ascribed to vital affinity, he described as the mere selection and retention, by certain portions of a solid, of certain substances, whether elementary or compound, already existing in a fluid that is brought in contact with it, or what is called by some a chemical filtration. This power is exemplified in living vegetables, particularly in the appropriation by them of some of the earthy and saline matters which are brought to their roots, and the rejection of others; it is more strikingly seen in the development of the lower classes of animals, especially those of the radiata and mollusca, which have horny or earthy integuments; and it is certainly the chief power concerned in all those functions of animals, to which we give the names of absorption, secretion, and even nutrition.

In regard to this simplest form of vital affinity, the following points seem ascertained:—

1. That it is usually, if not always, performed in a perfect organized being, by an attractive agency of living or growing cells which seem always to perform the double office of extracting from the nourishing fluid the material of their own growth and reproduction, and extracting also the fluid or solid matter which they are to contain, or with which they are to be incrusted.

The matters thus consolidated from a fluid in which they previously existed, by a simple process of attraction and increased aggregation, not precipitated by any chemical separation of their component parts, assume the forms peculiar to each organized body to which they are thus added, but retain that peculiarity which in inorganic matter exists only in fluids,—that the smallest portion of them contains all the chemical ingredients which belong to the mass, and thus any crystalline arrangement is prevented.

2. That no difference, of form or of composition, can be detected in the different cells of an organized structure, to explain the difference of the matters which they thus extract; and that, in the first development of organized beings, the difference of selecting power exercised at different points of the germinal membrane, appears to be determined by no other condition than their *position*,—just as different portions of nervous matter, differing only in anatomical position, exert perfectly different vital powers, or, in the state of disease (*e. g.*, of inflammation), peculiar attractions and repulsions ap-

pear to exist, for a time, simply at particular spots of the vascular system.

The attractions by which living cells thus appropriate to themselves portions of contiguous fluids, are obviously analogous to those by which even inorganic porous substances attract different fluids with different degrees of force, and thereby produce the phenomena of endosmose and exosmose, but are broadly distinguished from them by the peculiarity of the changes thus effected, by their infinite variety, even in different parts of the same structure, and by their uniformly temporary existence.

II. The actual transformations, or new arrangements of the chemical elements which take place in living bodies, and are peculiar to them, are illustrated by the examples of the formation of starch from water and carbonic acid, oxygen escaping; and of the formation of fat from starch, carbonic acid and water escaping.

It appears to be in the *cells* of organized structures that those transformations are likewise effected; and as the action of cells in simply extracting portions of the nourishing fluid, is analogous to the physical principle of endosmose, so their action in these metamorphosis may be illustrated, but by no means explained, by comparing them to those chemical actions to which the term catalysis is applied.

Two general observations may be made on both these modifications of the power of vital affinity,—*first*, that they obviously *transferred* from the portions of matter already endowed with them, to those which, in the growth of living beings, are added to, or substituted for, those portions of matter; just as muscular fibres already existing, communicate to all the matter which is added to them by the process of nutrition, the same contractile properties which they themselves possess; *secondly*, that every portion of matter to which any such vital properties are imparted, appears to enjoy them only for a short time; losing them so rapidly that a vital process of absorption and excretion is necessary, throughout the whole existence at least of animals, to eliminate from their bodies materials which have lost these properties and reverted to the condition of dead matter.

After stating these general principles regarding vital affinities, the author made some more special remarks on the most fundamental of all the changes in organized beings which may be referred to their action, viz., the formation of starch and its allied compounds from carbonic acid and water under the influence of light, and consequent purification of the atmosphere; and he insisted chiefly on the following points:—

1. That this change is probably gradual; the carbonic acid being taken into the juices of the plant and slowly decomposed there, more or less completely, according to circumstances, whence result not only starch or its allied compounds, but likewise different organic acids and various oils.

2. That the formation of sugar in plants is probably to be regarded rather as a simply chemical action than as a result of vital affinities; or that it is a first product of the decomposition of starch by the agency of water and oxygen.

3. That, on the other hand, the formation of lignin, containing more carbon and less oxygen, from starch or from cellulose, and from the carbonic acid and water brought into the cells, appears to be the result of a strictly vital affinity, strongest at the period of greatest vigour of the plant.

4. That in this, as in other of the metamorphoses which take place in living beings, and which he proposes farther to examine, the carbon, thus originally fixed on the earth's surface from the atmosphere, appears to be the chief material employed by nature for the formation of all organized structures, and to be invested, for that purpose, with peculiar and transient vital affinities, while oxygen hardly appears to exert any chemical powers in living bodies, different from those which it manifests elsewhere; but is taken into the interior of all living bodies, only that it may support the excretions which are continually going on in them, and resolving organized into inorganic matter; and thus, that it gradually resumes its power over the carbon which had been temporarily separated from it for the formation of the animated part of creation.

The following Donations to the Society's Library were announced:—

The Journal of Agriculture, and the Transactions of the Highland and Agricultural Society of Scotland, for March 1846.—*By the Society.*

Journal of the Asiatic Society of Bengal. No. 160, for 1845.—*By the Society.*

Life and Correspondence of David Hume. From the Papers bequeathed by his Nephew to the Royal Society of Edinburgh, and other original sources. By John Hill Burton, Esq., Advocate. 2 vols. 8vo.—*By the Author.*

Natural History of New York. 10 vols., 4to. Geological Map of New York, published by Legislative authority in 1842.—*By the Governor and Secretary of State of New York.*

Monday, 16th March 1846.

The RIGHT REV. BISHOP TERROT, Vice-President, in the Chair.

The following Communications we read :—

1. On the Personal Nomenclature of the Romans, with an especial reference to the Nomen of Caius Verres. By the Rev. J. W. Donaldson, Author of the New Cratylus. Communicated by Bishop Terrot.

2. On the appearance of the Great Comet of 1843, at the Cape of Good Hope, with illustrative Drawings. By Professor C. P. Smyth. Communicated by the Secretary.

This comet attracted much attention from its excessive brightness at and near its perihelion passage, as well as from the length and form of its tail. The drawings were intended to represent these particulars, and the changes which occurred during the time that the comet remained visible from the Cape Observatory.

It was seen for a short time, but not generally, before the perihelion passage, which took place on the 27th February 1843; and it ceased to be visible by the naked eye towards the end of March, though it could be discerned with the telescope till the 19th April.

After passing the perihelion, this object was seen on the 3d March, about a quarter of an hour after sunset. The head then glistened like a star of the second magnitude, and had a well-defined planetary disc, about 20″ in diameter, having an envelope of rays, the brightest of which came out like wings on both sides. The tail, which was 40° long, was in form *bifid ;* its two sides were very narrow, bright and straight, the space between the sides almost as dark as the neighbouring sky. There were also two faint streamers on either side of the tail.

On the 4th March, one of the streamers had disappeared, and the space between the central streamers had become less dark. The planetary disc was also less defined.

On the 9th March, the planetary disc had disappeared, in place of which there was a mere coma, having a thickening towards the middle. Both streamers of the tail had disappeared, and the dark axial space was filled with a faint luminosity. The tail, however, had become longer.

The tail, which was at this time preceding the head, was curved slightly towards the aphelion, and was also in advance of a line joining the sun with the comet's head.

With a small telescope of low power, indications of a nucleus often appeared, but were as often dispelled by the employment of more powerful instruments. When the tail appeared to the naked eye upwards of 40° long, nothing like a solid or stellar nucleus could be discovered. There was only a mass of vapour, which, though condensed in certain parts, was still permeable to the rays of the minutest stars.

3. On the Existence of Fluorine in the Bones from Arthur's Seat. By Dr G. Wilson.

4. On the Composition of the Bones from Arthur's Seat. By Dr Christison.

The author found that the bones of animals lately disinterred in the course of the new drive, contained $\frac{1}{8}$ of the quantity of gelatine common in recent bones.

The following Gentlemen were duly elected Ordinary Fellows of the Society:—

George Turnbull, Esq., W.S.
George J. Gordon, Esq.

The following Donations to the Society's Library were announced:—

The London University Kalendar, 1846.—*By the University.*

Journal of the Asiatic Society of Bengal, No. 159.—*By the Society.*

The Electrical Magazine. Conducted by Mr Charles V. Walker. January 1846.—*By the Editor.*

Twenty-fifth Report of the Council of the Leeds Philosophical and Literary Society for Session 1844-45.—*By the Society.*

Biographical Notice of the late Sir John Robison, K.H., Sec. R.S. Ed. By Professor Forbes.—*By the Author.*

Il Cimento; Giornale di Fisica, Chimica e Storia Naturale. 1844 and 1845, January to Aug.—*By Professor Forbes.*

Fig. 1. Two Foci. Ratios 1 : 2.

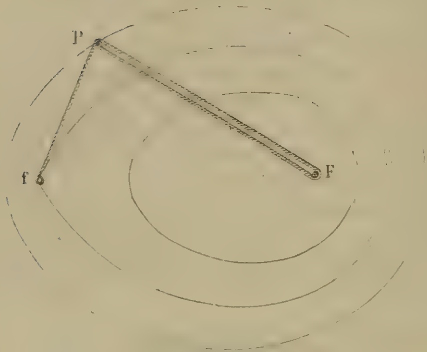
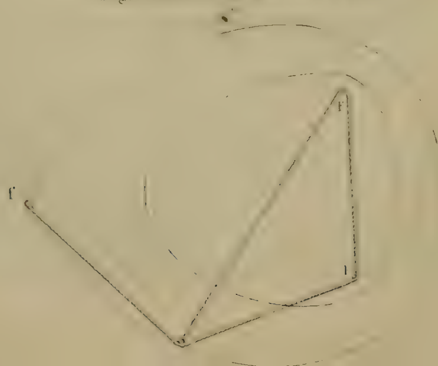


Fig. 2. Two Foci. Ratios 2 : 3.

Fig. 3. Three Foci. Ratios of Equality.



Nieuwe Verhandelingen der Eerste Klasse von het Koninklijk-Nederlansche Instituut van Wetenschappen, Letterkunde en Schoone Kunsten te Amsterdam. Deel XII., Stuk 1.—*By the Institut.*

Monday, 6th April 1846.

SIR THOMAS M. BRISBANE, Bart., President, in the Chair.

The following Communications were read :—

1. On the Description of Oval Curves, and those having a plurality of Foci. By Mr Clerk Maxwell junior; with remarks by Professor Forbes. Communicated by Professor Forbes.

Mr Clerk Maxwell ingeniously suggests the extension of the common theory of the foci of the conic sections to curves of a higher degree of complication in the following manner :—

(1.) As in the ellipse and hyperbola, any point in the curve has the *sum* or *difference* of two lines drawn from two points or *foci* = a constant quantity, so the author infers, that curves to a certain degree analogous, may be described and determined by the condition that the simple distance from one focus *plus* a multiple distance from the other, may be = a constant quantity ; or more generally, m times the one distance + n times the other = constant.

(2.) The author devised a simple mechanical means, by the wrapping of a thread round pins, for producing these curves. See Figs. 1 & 2 (Plate II.) He then thought of extending the principle to other curves, whose property should be, that the sum of the simple or multiple distances of any point of the curve from three or more points or foci, should be = a constant quantity ; and this, too, he has effected mechanically, by a very simple arrangement of a string of given length passing round three or more fixed pins, and constraining a tracing point, P. See Fig. 3. Farther, the author regards curves of the first kind as constituting a particular class of curves of the second kind, two or more foci coinciding in one, a focus in which two strings meet being considered a double focus ; when three strings meet a treble focus, &c.

Professor Forbes observed that the equation to curves of the first class are easily found, having the form

$$\sqrt{x^2+y^2}=a+b\sqrt{(x-c)^2+y^2},$$

which is that of the curve known under the name of the First Oval of Descartes.* Mr Maxwell had already observed that when one of the foci was at an infinite distance, (or the thread moved parallel to itself, and was confined in respect of length by the edge of a board,) a curve resembling an ellipse was traced; from which property Professor Forbes was led first to infer the identity of the oval with the Cartesian oval, which is well known to have this property. But the simplest analogy of all is that derived from the method of description, r and r' being the radients to any point of the curve from the two foci;

$$m\,r + n\,r' = \text{constant},$$

which in fact at once expresses on the undulatory theory of light the optical character of the surface in question, namely, that light diverging from one focus F without the medium, shall be correctly convergent at another point f within it; and in this case the ratio $\frac{n}{m}$ expresses the index of refraction of the medium.†

If we denote by *the power of either focus* the number of strings leading to it by Mr Maxwell's construction, and if one of the foci be removed to an infinite distance, if the powers of the two foci be *equal* the curve is a parabola; if the power of the nearer focus be *greater* than the other, the curve is an ellipse; if the power of the infinitely distant focus be the greater, the curve is a hyperbola. The first case evidently corresponds to the case of the reflection of parallel rays to a focus, the velocity being unchanged after reflection; the second, to the refraction of parallel rays to a focus in a dense medium (in which light moves slower); the third case to refraction into a rarer medium.

The ovals of Descartes were described in his Geometry, where he has also given a mechanical method of describing one of them,‡ but only in a particular case, and the method is less simple than Mr Maxwell's. The *demonstration* of the optical properties was given by Newton in the *Principia*, Book I., prop. 97, by the law of the sines; and by Huyghens in 1690, on the Theory of Undulations in his *Traité de la Lumière*. It probably has not been suspected that so easy and elegant a method exists of describing these curves by the use of a thread and pins whenever the powers of the foci are com-

* Herschel on Light, Art. 232; Lloyd on Light and Vision, Chap. vii.

† This was perfectly well shewn by Huyghens in his Traité de la Lumière, p. 111. (1690.)

‡ Edit. 1683. Geometria, Lib. II., p. 54.

mensurable. For instance, the curve, Fig. 2, drawn with powers 3 and 2 respectively, give the proper form for a refracting surface of glass, whose index of refraction is 1·50, in order that rays diverging from f may be refracted to F.

As to the higher classes of curves with three or more focal points, we cannot at present invest them with equally clear and curious physical properties, but the method of drawing a curve by so simple a contrivance, which shall satisfy the condition

$$m\,r + n\,r' + p\,r'' + \text{\&c.} = \text{constant},$$

is in itself not a little interesting; and if we regard, with Mr Maxwell, the ovals above described, as the limiting case of the others by the coalescence of two or more foci, we have a farther generalization of the same kind as that so highly commended by Montucla,* by which Descartes elucidated the conic sections as particular cases of his oval curves.

2. On the Influence of Contractions of Muscles on the Circulation of the Blood. By Dr Wardrop.

In this paper, Dr Wardrope states that he has endeavoured to shew, by a series of observations and experiments, that the muscles, besides being the active organs of motion, perform, by their contractions, an important office in the circulation of the arterial as well as venous blood; an office which has not hitherto been described by physiologists, but which appears to be capable of explaining several interesting phenomena in the living body, of which no satisfactory account has yet been given.

3. On the Solubility of Fluoride of Calcium in Water, and the relation of this property to the occurrence of that Substance in Minerals, and in recent and Fossil Plants and Animals. By Dr G. Wilson.

After a preliminary reference to the existence of fluorine in recent and fossil bones, Dr Wilson stated that he had made a series of experiments with a view to discover what solvent carried fluoride of calcium into the tissues of plants and animals. His first trials were made with carbonic acid, which was passed in a current through water containing pure fluor-spar in fine powder suspended in it. The fluor was by this treatment dissolved, yielding a solution which precipitated oxalate of ammonia, and when evaporated left a residue

* Histoire des Mathematiques. *First Edit.* II., 102.

which, on being heated with sulphuric acid, gave off hydrofluoric acid.

The author was, in consequence, inclined to suppose that carbonic acid conferred upon water the power of dissolving fluoride of calcium. But on observing that long after the whole of that gas had been expelled by warming the liquid, the latter remained untroubled, he became satisfied that water alone can dissolve fluoride of calcium, contrary to the universal statement of writers on chemistry.

On prosecuting the inquiry, he found that water at 212° dissolved more of the fluor than water at 60°, but he has not yet ascertained the proportion taken up by that liquid at either temperature.

The aqueous solution of fluoride of calcium was found to give, with salts of baryta, a precipitate which required a large addition of hydrochloric or nitric acid to redissolve it. The author pointed out the difficulty which must in consequence occur, in distinguishing between dissolved fluoride and sulphates, and suggested that fluorides may have been mistaken for sulphates in the analysis of mineral water.

He referred also to the objection which must now lie against the present method of determining the quantity of fluorine present in bodies, consisting, as it does, in converting that element into fluoride of calcium, which, in the course of the necessary analytical operations, is washed freely, and must be sensibly diminished in quantity; a fact which has of necessity been hitherto overlooked. Dr Wilson stated that he was not yet able to suggest an unexceptionable quantitative process; but that the fluoride of barium, being much less soluble than the fluoride of calcium, might, in the meanwhile, be substituted for it in the estimation of fluorine.

The author proceeded to state, that in consequence of the observations he had made as to the solubility of fluoride of calcium on water, he had been led to look for that body in natural waters, and had found it in one of the wells of Edinburgh, namely, in that supplying the brewery of Mr Campbell in the Cowgate, behind Minto House. At the same time, he stated that preceding observers had already found it in other waters. He believed, however, that he was the first to detect it in sea-water, where, by using the bittern or mother-liquor of the salt-pans in which water from the Frith of Forth is evaporated, he had found it present in most notable quantity. The author referred to the presence of fluorine in sea-water, as adding another link to the chain of observed analogies between that body and chlorine, iodine, and bromine.

Dr Wilson further stated, that he had confirmed the observations of Will, as to the presence of fluorine in plants, and Berzelius' discovery that fluorine exists in the secretion from the kidneys; and had, in addition, detected fluorine in the blood and milk, in neither of which has it been hitherto suspected to occur. The paper was concluded by some observations on the presence of fluorine in fossils, and its relations to animal life.

The following Gentleman was elected an Ordinary Fellow:—

WILLIAM BALFOUR, Esq.

The following Donations to the Library were announced:

Proceedings of the American Philosophical Society. Vol. IV., Nos. 32 and 33.

Transactions of the American Philosophical Society held at Philadelphia, for promoting Useful Knowledge. (New Series), Vol. IX., Part 2.—*By the Society.*

Flora Batava. Nos. 139 and 140.—*By the King of the Netherlands.*

Journal of the Statistical Society of London. Vol. IX., Part 1. March 1846.—*By the Society.*

The American Journal of Arts and Science, conducted by Professor Silliman, B. Silliman junior, and James D. Dana, for March 1846.—*By the Editors.*

The Quarterly Journal of the Geological Society. Vol. I., and Vol. II., Part 1.—*By the Society.*

Proceedings of the Royal Astronomical Society. Vol. VI., Nos. 9 to 17, and Vol. VII., Nos. 1, 2, 3.

Memoirs of the Royal Astronomical Society. Vol. XV. *By the Society.*

Meteorological Observations for 1842 and 1843, made at the Bombay Government Observatory. By George Buist, LL.D., in the charge of the Observatory.

Magnetic Observations made at the Bombay Government Observatory from May 1842 to Dec. 1843. By George Buist, LL.D.

Tracings of the Wind-Gauge for 1842 and 1843, made at the Bombay Government Observatory from May 1842 to Dec. 1843. By George Buist, LL.D.—*By George Buist, LL.D.*

Journal of the Asiatic Society of Bengal. No. 162.—*By the Society.*

Memoirs and Proceedings of the Chemical Society. Part 16.—*By the Society.*

Maps of the Geological Survey of the United Kingdom of Great Britain.—*By Sir H. T. De la Beche, Director-General of the Geological Survey.*

Monday 20*th April* 1846.

The Right Rev. BISHOP TERROT, Vice-President, in the Chair.

The following communications were read :—

1. On the Constitution and Properties of Picoline, a new organic base from Coal-Tar. By Dr T. Anderson.

The author, after alluding to the investigations of the oily bases in coal tar, by Hoffmann, who had failed in obtaining Runge's pyrrol, stated, that in searching for that substance among the more volatile products of the distillation of coal-tar, he had been enabled to confirm its existence in small quantity, as well as that of the new base to which he has given the name of picoline.

Picoline is obtained in the pure state by several successive distillations of the mixed bases contained in coal-tar, after the perfect separation of pyrrol and empyreumatic oils, by processes described at length in the paper, and finally by collecting the product which distilled at 272° Fahrenheit. The analysis of the base purified by these processes, gave the following mean result, viz. :—

	Mean.	Calculated.
Carbon,	77·17	77·29
Hydrogen,	7·69	7·43
Nitrogen,	15·14	15·28
	100·00	100·00

This corresponds with the formula $C_{12}\ H_7\ N$, which is that of aniline; and the author further found by the analysis of the platinum salt of picoline, that its atomic weight is identical with that of aniline.

The identity in constitution, however, of these substances is accompanied by an entire difference in properties. Picoline having a specific gravity of 0·955 boiling at 272°, and being soluble in water in all proportions; it is incapable of giving the violet colour with chloride of lime, and the yellow colour to fir wood moistened with hydrochloric acid, which are produced by aniline, and it gives, with chloride of gold, a highly characteristic precipitate, soluble in hot water, and deposited, on cooling, in delicate yellow needles. The author observed that these properties approximated in some respects to those of Unverdorben's odorin, and stated that he had separated from the oleum cornu cervi, a mixture of several oily bases, one of which was soluble in water, but which did not agree perfectly either with the characters of picoline, or those which Unverdorben has attributed to odorin.

The author then details the properties and constitution of the compounds of picoline, which differ in many respects from those of aniline. It gives with sulphuric acid an acid compound which deliquesces rapidly in the air, and which has the formula $C_{12}\ H_7\ N\ +\ 2\ (H\ O,\ S\ O_3)$. Its platinum salt is identical in constitution with that of aniline, but its mercury compound is $C_{12}\ H_7\ N\ +\ Hg\ Cl_2$, while that of aniline is $2\ (C_{12}\ H_7\ N)\ +\ 3\ Hg\ Cl_2$.

The author then treats of the products of the decomposition of picoline, a branch of the subject on which, owing to the small quantity of the substance at his disposal, he was enabled only to make a few observations, which, however, tend to shew that the action of reagents on it is remarkably different from that which they produce on aniline. The action of nitric acid was found to be extremely slow and partial, long continued ebullition producing only a very slight evolution of nitrous acid, without any of the blue colour which aniline gives, and apparently without the formation of carbazotic acid. Bromine gives an oily product heavier than water, and different, of course, from the solid bromaniloid of Fritsche.

The author concludes by remarking, that the present is the first perfectly established case of isomerism among organic bases, those previously recorded being devoid of absolutely conclusive evidence, and by pointing out the interest which attaches to the isomerism of two substances such as aniline and picoline, which are members of one of the most interesting and extensive groups of organic substances, the indigo, salicyl, and benzoil series.

2. Notice of Polished and Striated Rocks recently discovered on Arthur Seat, and in some other places near Edinburgh. By David Milne, Esq.

Mr Milne stated, that, in the gully situated between Arthur Seat and Sampson's Ribs, a great extent of rock had been recently exposed (by the removal of clay and other superficial deposits) which was found to be smoothed as well as furrowed or scratched.

The gully is about 30 feet wide, at the lowest level to which it has been hollowed out, and at one part, both of its sides are composed of these smoothed furrowed rocks ; but, in general, it is only on one side, viz., that next to Arthur Seat, that rock exists. There, the appearances of smoothing and rutting extend for about 80 yards.

The gully runs about NW. and SE. by compass. The highest point in it is near the north end. At both ends it is open and sinks to a level with the adjoining level country. The gully is about 200 feet above the level of Duddingston Loch, and 400 feet above the sea. Arthur Seat forms on the east side of it a precipitous cliff of about 250 feet.

The walls of the gully consist (so far as yet exposed, in the formation of the Victoria road), for about 5 feet upwards, of vertical rock.

This rock towards the north end of the gully is a compact porphyry; towards the south end, of friable porphyry. At the north end the polishing has been greatest.

The scratches are in general nearly horizontal; a few slope upwards to the south ; these are at the north end of the gully, where it is narrowest.

The longest scratches are about 6 feet long, from $\frac{1}{3}$ to $\frac{1}{2}$ inch deep and an inch wide.

There are, especially towards the south end of the gully, many spots of a few inches square, where there has been neither polishing nor scratching. These all face towards the south.

The deposit immediately above those rocks, and which has completely filled up the gully, is a brown tenacious clay, full of boulders of all sizes. The boulders consist of traps (some of them of rock not existing in the neighbourhood) and sedimentary rocks. Whilst there are sandstone fragments, which are very similar to those on Salisbury Crag, there are limestones, supposed not to exist nearer than Fife.

This boulder clay is not so tenacious as the blackish-blue boulder clay generally prevalent in the Lothians. It, however, resembles in all respects a deposit of the same kind, existing at the foot of Sampson's Ribs, which is about 160 feet below the level of the gully.

Above the boulder clay in the gully there is a mass of debris, derived apparently from the crumbling of the rocks above on the face of Arthur Seat. Three species of marine shells have been found in this mass ; but, as human bones and Roman remains have also been discovered in it, the probability is, that these shells have been brought by human hands.

In the cuttings for the North British Railway, between Arthur Seat and Musselburgh, the upper sides of the large boulders are generally found smoothed and scratched. The scratches seem to be from NW. to WNW. by compass. On some of the boulders there are indications of more recent scratches running W. ½ S. by compass.

The boulders in the railway cuttings between Haddington and Dunbar exhibit scratches running from NW. to WNW.

The opinion formed by the author on these data was,—

(1.) That the agent which had polished and scratched the rocks on Arthur Seat, was the same as that which had polished and scratched the boulders.

(2.) That it had acted from the north-westward over a large and low district of country.

(3.) That the polishing and scratching had been effected by the gravel and angular blocks existing in the boulder clay and diluvial gravel.

(4.) That there had been rushes of water along the country, which bore along the mud, sand, gravel, and boulders now spread over the country, and which, in passing over the rocks and large boulders, smoothed and rutted them.

(5.) That, at this period and subsequently, water must have stood, in a comparatively tranquil state, above the level of Sampson's Ribs, to account for the beds of sand existing on the south side of Arthur Seat, and at a level of 200 feet above Duddingston Loch.

(6.) That the outline or configuration of the district, thus submerged, could not have been materially different from what it now presents.

3. Results of the Makerstoun Observations, No. II. On the Relation of the Variations of the Vertical Component of the Earth's Magnetic Intensity to the Solar and Lunar Periods. By J. Allan Broun, Esq. Communicated by General Sir T. M. Brisbane, Bart.

The following results are deduced from the observations of the balance or vertical force magnetometer described in the Introduction to the Makerstoun Observations. The observations were corrected for temperature by a method previously described.

The diurnal variation of the vertical component consisted, in the year 1844, of three maxima and three minima, occurring as follows :—

	H. M.	
The principal minimum at	14 10	Makerstoun mean time.
A secondary maximum at	20 50	....
A secondary minimum at	22 50	....
The principal maximum at	5 30	....
A third minimum at	12 10	....
A third maximum at	13 10	....

The third maximum and minimum are but faintly marked. The form of the diurnal curve, and the periods and number of maxima and minima, vary throughout the year; neglecting the inferior maximum and minimum, the diurnal curve is single in winter and double in summer, the principal minimum occurs at noon in summer.

The diurnal range is greatest at the equinoxes, and least at the solstices, the range at the former being nearly two and a half times that at the latter.

Each of the mean values of the vertical component at 21^h and 0^h, is nearly equal to the mean for the year. The secular variation is negative.

The investigation for the relation of the variations of the vertical component to the moon's hour angle, gives for the mean of the two years 1844 and 1845—

The principal minimum about 5 hours before the moon's passage of the inferior meridian.
The principal maximum about 3 hours after ....
A secondary minimum about 4 hours before ....superior...........
A secondary maximum about 1 hour after...

The results for each year differ little from the mean of both; the secondary maximum and minimum are not so well shewn in 1844 as in 1845. The observations of single months, when free from magnetic disturbances, have been found to give the same periods.

Investigations were made for a period connected with the moon's phase or synodical revolution; each of the years 1844 and 1845 indicates maxima of the vertical component near the quadratures, and minima near the syzygies.

4. Two Verbal Notices. (1.) On the Geology of Arthur Seat. (2.) On the Dentition of the Walrus. By the Rev. Dr Fleming.

(1.) Dr Fleming read the following passage from Townson's "Tracts and Observations in Natural History," Edin. 1799, when treating of the "Rocks in the Vicinity of Edinburgh:"—"The first of the chains or ranges of rocks that I lately described, as lying at the back of Salisbury Crags, extends from Arthur Seat to St Anthony's chapel. It is composed of basalt and sandstone, neither of which are like the whin and sandstone of Salisbury Crags. The basalt is the same as that near Duddingston Loch. The stratified matter forms a bed two or three yards thick near St Anthony's chapel. Some of it is very hard, and strikes fire with steel, but effervesces with acids, and has an argillaceous smell, but the greatest part is soft and friable, and seems to be merely the finer debris and powder of the breccia, perhaps a kind of trass or terrass. *It contains a great many vegetable impressions, with the charred matter still existing.* They appear to be the same which are so frequently found in the strata that accompany coal. The large irregular basaltic columns at St Anthony's chapel rest upon this." P. 214. Dr Fleming stated that he was induced to bring into notice this seemingly neglected observation of Townson, as likely to interest those members of the Society who attach themselves to the study of the geology of the district.

(2.) Mr Lyell, in the first volume of his "Travels in North America," London 1845, p. 258, has the following remarks in reference to the skull of a Walrus, from the tertiary beds of Gayhead, in the island of Martha's Vineyard, Mussachusetts,—"I purchased from a fisherman, residing near the promontory, a fossil skull, which he told me had fallen out of this conglomerate upon the beach below. It retained but a small portion of the original animal matter, was slightly rolled, and Mr Owen recognised it as the cranium of a Walrus or Morse, nearly allied to the existing species (*Trichecus Rosmarus*, Linn.) On comparison it was observed to differ from it, in having *six* (a misprint for *three*, as is evident from the figure given of the organism in Plate V.) molar teeth, instead of four on each side of the upper jaw. There are eleven specimens of recent species in the College of Surgeons, in all of which there are no more than four grinders on each side. The tusk, also, of the Gayhead fossil has a rounder form than that of the recent Morse (see Plate V.)" Dr Fleming stated that, on perusing the above passage, he was induced to examine the examples of the recent skulls of the Walrus in his possession. This examination led him to observe a degree of irregularity in the dentition of this animal, differing from the statement in the passage, as depending on the appearances of the eleven specimens in the Museum of the College of Surgeons of London. He placed on the table four examples.

1. In the first specimen there were three grinders on each side,

as in the fossil example, and an alveolus on each side, behind, nearly filled up.

2. In the second example there were four grinders on the one side and five in the other, the last being small.

3. In the third example there were four grinders on each side, and two shallow sockets on each side behind, out of which the teeth had recently fallen, and the trace of a seventh obliterated alveolus on each side.

4. In the fourth example there were five grinders on each side and one nearly obliterated alveolus on each side.

The incisors appeared to be equally irregular in their number and development. Thus, in the 1*st*, there was one incisor on the one side and only an obliterated socket in the other. In the 2*d*, one incisor on one side. In the 3*d*, two incisors on one side and one in the other. In the 4*th*, one on each side.

Dr Fleming concluded his communication by stating that the few examples which were before the Society, while they indicated very plainly an irregularity in the dentition of the Walrus, not perceived in the eleven London examples, forbade our attaching any importance to the Gayhead organism, as referable to an extraordinary variety, and still less to a new species of the genus Trichecus.

The following Gentleman was elected an Ordinary Fellow :—

Dr L. Schmitz, Rector of the High School.

The following donations to the Society's Library were announced :—

Metaphysical Analysis, revealing, in the Process of the Formation of Thought, a new Doctrine of Metaphysics. By J. W. Tombs. —*By the Author.*

Novorum Actorum Academiæ Cæsareæ Leopoldino-Carolinæ Naturæ Curiosorum, voluminis vicesimi primi pars prior.—*By the Academy.*

Comptes Rendus Hebdomadaires des Seances de l'Academie des Sciences. Tomes XXII., Nos. 2 to 12—*By the Academy.*

Journal of the Asiatic Society of Bengal. No. 163.—*By the Society.*

A Work on the Science of Mathematics, embracing Conic Sections, Perspective, &c. By Nuwab Shums-ool-oomiah of Hyderabad; the Illustrations lithographed by the Author.—*Dr Burt.*

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. II. 1846–7. No. 29.

Sixty-Third Session.

First Ordinary Meeting, 7th December 1846.

Sir THOMAS MAKDOUGALL BRISBANE, Bart., President, in the Chair.

The following Communications were read:—

1. On the Mean Height of the Barometer in different Latitudes. By Professor Hansteen of Christiania. Communicated in a letter to the Secretary.

Professor Hansteen first shews, that the remarkable difference of 1·62 French lines, by which the barometer reduced to the level of the sea, is higher at Paris than at Christiania, cannot be ascribed solely to the effect of the difference of gravity of the mercury in the barometer at the two places. The author then gives an empirical formula to represent the mean height of the barometer in any latitude which he compares with observations. This formula is the following—

$$\psi = 336{\cdot}810 + 1{\cdot}304 \cos. 2\varphi - {\cdot}748 \cos. 4\varphi - 0{\cdot}914 \cos. 6\varphi + 0{\cdot}543 \cos. 8\varphi.$$

Where ψ is the oscillation in French lines, and φ the latitude.

It gives a minimum for $\varphi = 0$, and $\varphi = 68^\circ\ 24'$.
And maximum for $\varphi = 36^\circ\ 13'$, and $\varphi = 90$.

The following table gives φ for every fifth degree of latitude ·—

φ	ψ	φ	ψ
0	336·995	50	337·246
5	7·012	55	6·240
10	7·096	60	5·345
15	7·291	65	4·801
20	7·623	70	4·715
25	8·057	75	5·037
30	8·478	80	5·561
35	8·714	85	6·034
40	8·612	90	6·216
45	8·101		

2. On the Extent to which Fluoride of Calcium is Soluble in Water at 60° F. By Dr George Wilson.

Dr Wilson stated, that the object of his communication was to supplement a paper read to the Royal Society, April 6, 1846, on the solubility of fluoride of calcium in water. He was not then able to state the extent of its solubility in that liquid, but he has since ascertained the weight of the salt in question, which water at 60° can dissolve. The solutions referred to in the following statements were prepared from native well-crystallized fluor-spar. This was first digested in warm dilute aqua regia, to remove any trace of metallic oxides, soluble lime-salts, or other foreign matters, and afterwards carefully washed till it exhibited no acid reaction. Distilled water was then boiled on the purified fluor till it became saturated; the solution filtered whilst warm, and left at rest for some days in stoppered bottles, at a temperature of about 60°, till it deposited the excess of salt soluble above that temperature. It was then filtered a second time. A certain volume of this solution, measured at 60°, was evaporated to dryness on the vapour bath, in a counterpoised platina basin.

Twenty-four pints of the solution were thus made use of. In six experiments, 1 imperial pint of the solution (16 fluid ounces, or 7000 grs.) was taken at each trial. In four trials, 3 pints were evaporated at each experiment. In one, 6 pints were employed.

The following are the results :—

		Per Pint.
Exp. 1.	Evaporated 1 pint of solution.	Residue, 0·27 grs.
Exp. 2.	,, ,, ,,	,, 0·28 ,,
Exp. 3.	,, ,, ,,	,, 0·28 ,,
Exp. 4.	,, ,, ,,	,, 0·24 ,,
Exp. 5.	,, ,, ,,	,, 0·27 ,,
Exp. 6.	,, ,, ,,	,, 0·25 ,,

Average, 0·265 per pint.

			Per Pint.
Exp. 7.	Evaporated 3 pints of solution.	Residue, 0·79	= 0·263 grs.
Exp. 8.	„ „ „	„ 0·78	= 0·260 „
Exp. 9.	„ „ „	„ 0·78	= 0·260 „
Exp. 10.	„ „ „	„ 0·77	= 0·257 „

Average, 0·260 per pint.

			Per Pint.
Exp. 11.	Evaporated 6 pints of solution.	Residue, 1·62	= 0·270 grs.

Average, 0·270 per pint.

Twenty-four pints of distilled water thus dissolved 6·330 grains of fluor-spar, so that the mean amount dissolved by one pint will be 0·2637 grains. One grain, therefore, of fluor will require 26·545 grains of water at 60° to dissolve it, or water, at that temperature, will take up $\frac{1}{26545}$th of its weight of that salt.

The solubility here indicated must be considered great for a salt hitherto reputed quite insoluble. It is still more soluble in water at a high temperature, as the deposit left by warm solutions on cooling shews.

These facts will now be connected with the appearance of fluoride of calcium in plants and minerals, as well as in mineral veins and elsewhere; and may, perhaps, prove sufficient to explain these hitherto somewhat perplexing phenomena.

3. New Observations on the Glaciers of Savoy. Part I. By Professor J. D. Forbes.

This paper describes the remarkable changes which the glacier of La Brenva, on the south side of Mont Blanc, has undergone between the years 1842 and 1846.

It has increased to such an extent, that it has risen against the opposing wall of rock on the side of the valley nearly 200 feet vertically; it has covered a large additional surface of ground, and appears to be approaching the moraines of 1818. The author describes the measures which he took for comparing its size at any future period, and he cites some observations made by the Vicar of Courmayeur (M. Guicharda), which plainly establish its continued motion during winter.

Professor Forbes then describes the remarkable development of the *veined structure* which has occurred since his last observations opposite to the promontory of rock on which the chapel is built, which was formerly partially ruined by the glacier. He is of opinion, that the position in which the veined structure has been developed is conclusive as to its cause, being the *forced separation* of the central parts of the glacial mass from the lateral portion, which has become checked by the rocky promontory alluded to, in such a way as to assist the motion of the sides of the glacier, in a way which was not the case in 1842. He confirms this view by actual observations of the velocity of different parts of the glacier, which shew that the lateral ice is completely embayed, and must undergo such a wrench from the central part as had been supposed, to which the veined structure is ascribed.

The following Gentleman was duly elected an Ordinary Fellow:—

C. P. Smyth, Esq., Professor of Practical Astronomy in the University of Edinburgh.

The following Donations of Books to the Library were announced:

The Electrical Magazine, conducted by Mr Charles V. Walker, for April, July, and October 1846.—*By the Editor.*

Annuaire de l'Observatoire Royal de Bruxelles, par le Directeur A. Quetelet, pour l'Année 1846.

Lettres au Duc Regnant de Saxe-Cobourg et Gotha, sur la Theorie des Probabilités, appliquée aux Sciences Morales et Politiques, par A. Quetelet.—*By the Author.*

Annuaire de l'Academie Royale des Sciences des Lettres et des Beaux Arts de Belgique pour 1846.

Bulletins de l'Academie Royale des Sciences et Belle Lettres de Bruxelles. Tome XII. Ptie. 2.—*By the Academy.*

Proceedings of the Royal Society. Nos. 62, 63, 64, and 65.

Philosophical Transactions of the Royal Society of London for the year 1846. Parts 1, 2, 3.—*By the Royal Society.*

The Thirteenth Annual Report of the Royal Polytechnic Society.—*By the Society.*

Structure and Classification of Zoophytes. By James D. Dana, A.M.—*By the Author.*

Nachrichten von der Georg-Augusts Universitäts und der Konigl. Gesellschaft der Wissenschaften zu Göttingen. Von Juli bis December 1845.—*By the Royal Society of Sciences of Göttingen.*

Handbuch der Mineralogie von J. F. L. Hausmann, Zweiter Theil. Dritte Abtheilung.—*By the Author.*

United States Exploring Expedition—Zoophytes. By James D. Dana.—*By the Author.*

Flora Batava. Nos. 141, 142, and 143.—*By the King of the Netherlands.*

Report of the Fifteenth Meeting of the British Association for the Advancement of Science, held at Cambridge in 1845.—*By the British Association.*

Journal of the Asiatic Society of Bengal. Nos. 164, 167.—*By the Society.*

Journal of the Statistical Society of London. Vol. IX., Parts 2, 3. —*By the Society.*

Journal of the Royal Asiatic Society. Vol. X., Part 1.—*By the Society.*

Bulletin de la Société de Geographie. (3me. Serie), Tomes IV. et V. —*By the Society.*

Archæologia; or, Miscellaneous Tracts relating to Antiquity; published by the Society of Antiquaries of London. Vol. XXXI. —*By the Society.*

Memoirs of the American Academy of Arts and Science. (New Series), Vol. II.—*By the Academy.*

Bulletin des Séances de la Societe Vaudoise des Sciences Naturelles. Tome I.—*By the Society.*

Monthly Prize Essays. Vol. I., No. 1.—*By the Director.*

The Journal of Agriculture, and the Transactions of the Highland and Agricultural Society of Scotland, for July and October.—*By the Society.*

Proceedings of the Zoological Society of London, Jan. 14 to Dec. 9, 1845.—*By the Society.*

Memoirs of the Geological Survey of Great Britain, and of the Museum of Economic Geology in London. Vol. I.—*By the Lords Commissioners of Her Majesty's Treasury.*

Memoires de la Société Royale des Antiquaires du Nord, 1844.

Bulletin de la Société Royale des Antiquaires du Nord, 1843.—*By the Society.*

Abhandlungen Herausgegeben von der Fürstlich Jablonowskischen Gesellschaft.

The American Journal of Science and Arts, conducted by Professors B. Silliman and B. Silliman Jun., and James D. Dana, for July and Sept.—*By the Editors.*

Memoires de la Société Physique et d'Histoire Naturelle de Genève. Tome XI., Ptie. 1.—*By the Society.*

The Journal of the Royal Geographical Society of London. Vol. XVI., Part 1.—*By the Society.*

The Quarterly Journal of the Geological Society. Nos. 7 and 8. Edited by the Vice Secretary.—*By the Society.*

Astronomische Nachrichten. Nos. 539 to 559.—*By Professor Schumacher.*

Report to the Principal Secretary of State for the Home Department on the Royal Observatory of Edinburgh.—*By Professor Smyth.*

Scheikundige Onderzoekingen gedaan in het Laboratorium der Utrechtsche Hoogeschool. Deel III., Stuk. 5.—*By the Editors.*

Memoirs and Proceedings of the Chemical Society. Part. 19.—*By the Society.*

Annuaire Magnétique et Météorologique du Corps des Ingenieurs des Mines de Russie. Par A. F. Kupffer, 1843. Nos. 1 and 2.—*By the Author.*

Report of the Astronomer-Royal, Greenwich, to the Board of Visitors, 1846.—*By the Author.*

Biographical Sketch of the late Robert Graham, M.D., Professor of Botany in the University of Edinburgh. By Charles Ransford, M.D.—*By the Author.*

Correspondence of the late James Watt on his Discovery of the Theory of the Composition of Water. Edited by J. P. Muirhead, Esq.—*By the Editor.*

Astronomical Observations made at the Royal Observatory, Greenwich, in the year 1844, under the direction of George Biddell Airy, Esq. M.A., Astronomer-Royal.—*By the Royal Society.*

Bericht uber die zur Bekanntmachung geeigneten Verhandlungen der Königl. Preuss. Akademie der Wissenschaften zu Berlin. Juli 1845 bis Juni 1846.

Abhandlungen der Koniglichen Akademie der Wissenschaften zu Berlin, 1844.—*By the Academy.*

On the superficial Detritus of Sweden, and on the probable causes which have affected the surface of the Rocks in the Central and Southern portions of that Kingdom. By Sir Roderick Impey Murchison.—*By the Author.*

Address to the British Association for the Advancement of Science, September 10, 1846. By Sir Roderick Impey Murchison, President.—*By the Author.*

Det Kongelige Danske Videnskabernes Selskabs Naturvidenskabalige og Mathematiske Afhandlinger. Deel XI.

Do. Do. Do.

Historiske og Philosophiske Afhandlinger. Deel VII.—*By the Royal Society of Sciences of Copenhagen.*

Mnemonic Dictionary of Languages, arranged in Mnemonic Tables. By A. Jazwinski, Doctor in Philosophy.—*By the Author.*

Monday, 21*st December* 1846.

Sir GEORGE S. MACKENZIE, Bart., V.P., in the Chair.

The following Communications were read:—

1. New Observations on the Glaciers of Savoy, (concluded). By Professor Forbes.

The Second Part of this paper contained an account of the determination of the motions of several glaciers which had not been before observed, such as the glacier of Talefre, Nant Blanc, Miage, and Brenva, and the results of a comparison of the measures made in 1846 with those of former years, at different points of the Mer de Glace; shewing the unusual speed of motion during last summer. There is detailed also the history of the loss of a knapsack in the ice of the glacier of Talefre in 1836, and the recovery of its fragments in 1846, shewing a motion of 4300 feet in ten years, on a declivity of about 15° at a mean. Some remarks were then made on the process of the conversion of the *névé* into glacier, which the author ascribes entirely to intense pressure and friction, aided by the molecular influences which time develops. The first stage of *glacification* is the formation of the *blue bands*, nearly vertical, which are due to the friction arising from the differential motion of the particles. The paper concludes by some remarks on the apparent ejection of stones and other foreign bodies from glaciers.

2. On General Differentiation. Part III. By the Rev. Professor Kelland.

The object of this memoir is to effect the solution of differential equations, and equations of differences, in which the index of differentiation is a simple fraction. The process employed is the calculus of operations in which symbols of operation are treated in the same way as symbols of quantity, provided the former are subject to the same laws as the latter.

3. Dr T. Anderson communicated the following Extracts from a Letter of Baron Berzelius:—

"In the investigation of the vegetable alkalies which I have made for the new edition of my Hand-Book, I consider myself to have established, in a satisfactory manner, that the alkaline constituent of these substances is ammonia, coupled with a variety of different compounds, such as carbo-hydrogens, organic oxides, chlorides, oxychlorides, sulphurets, amides, and even saline compounds. The ammonia in these substances can be converted into amide, ammonium, or oxide of ammonium, under different circumstances. Although, however, I conceive we are now on the right road with the basic constituent of the alkaloids, we are still very much in the dark with regard to the substances which are coupled with the ammonia. The investigation of these will be a much more difficult problem, if, indeed, we ever succeed in solving it.

"I had, during the month of October, a visit of Fritsche of St Petersburg, accompanied by young Struve, the son of the astronomer, in companionship with whom, Fritsche has discovered and examined a new acid compound of osmium, part of the investigations of which were performed in my laboratory. This acid is of great theoretical interest; it is a fulminating acid, whose silver salt explodes at 66° Cent. as violently as the fulminate of silver. As far as the difficult analyses have gone, its constitution is—

$$Os\,O_4 + Os\,N.$$

It is therefore osmic acid, coupled with the nituret of osmium, and constitutes, as nearly as may well be, a proof of the view which I some time since suggested, with regard to the constitution of the common fulminic acid, viz., that it might consist of a non-explosive acid, coupled with a metallic nituret.

"Schonbein's gun-cotton is now interesting all our chemists. I have found that lignin is the substance which produces this compound, and that it may be obtained from the lignin of any vegetable. It is not xyloidine, which is a compound peculiar to starch alone. I have prepared it, for example, from the *sphagnum palustre*, which had been previously treated with potash; and, best of all, from decayed wood (also after treatment with potash), which will probably form the cheapest raw material for its preparation, especially in a country so rich in forests and half-rotten saw-dust as ours is. Reindeer moss and Iceland moss afford only xyloidine when treated with nitric acid.

"I consider the gun-cotton to be a sort of nitrate of lignin, and the products of its explosion contain much cyanogen; so that its constitution must be such, that the nitric acid may exactly suffice to form carbonic acid and cyanogen, with the carbon $= 2\ NO_5 + C_9$, together with HO, in a hitherto undetermined quantity."

The following Donations to the Library were announced:—

Journal of the Statistical Society of London. Vol. IX., Part 4.—*By the Society.*

Elements of Chemistry. By the late Edward Turner, M.D. Edited by Baron Liebig and William Gregory, M.D. Part 1.—*By Messrs Taylor and Walton.*

Magnetical and Meterological Observations made at Washington, under orders of the Hon. Secretary of the Navy, dated Aug. 13, 1838.

Astronomical Observations made at the Naval Observatory, Washington, under orders of the Hon. Secretary of the Navy, dated Aug. 13, 1838.—*By Lieutenant Gillis.*

Irish Ordnance Survey Maps—County Kerry.—*By the Lord Lieutenant of Ireland.*

Memoires de l'Academie Imperiale des Sciences de St Petersbourg. (Sciences Mathematiques, &c.) Tome IV., Liv^{re} 2.

Memoires de l'Academie Imperiale des Sciences de St Petersbourg. (Sciences Naturelles.) Tome V., Liv^{res} 3, 4.

Memoires de l'Academie Imperiale des Sciences de St Petersbourg. (Memoires Presentés par divers Savants.) Tome V. et Tome VI., Liv^{re} 1.—*By the Imperial Academy.*

Monday, 4th January 1847.

Sir THOMAS M. BRISBANE, Bart., President, in the Chair.

The following Communications were read :—

1. On the presumed long-continued Presence of Arsenic in the Human Stomach. By Dr Gregory.

2. Notes on the Superficial Strata of the Neighbourhood of Edinburgh. By Dr Fleming. (Commenced.)

The following Gentlemen were duly elected Ordinary Fellows :—

George Makgill, Esq. of Kembach.

David Gray, Esq., M.A., Professor of Natural Philosophy, Marischal College, Aberdeen.

The following Donations to the Library were announced :—

Journal of Agriculture and Transactions of the Highland and Agricultural Society, for January.—*By the Society.*

Inquiry into some points of the Sanatory State of Edinburgh. By James Stark, M.D.

Report of the Mortality of Edinburgh and Leith for the Six Months, June to November 1846. By James Stark, M.D.—*By the Author.*

Proceedings of the Linnean Society of London. Nos. 27, 28, and 29.

Transactions of the Linnean Society of London. Vol. XX., Part 1.—*By the Society.*

Twenty-sixth Report of the Council of the Leeds Philosophical and Literary Society at the Close of the Session 1845-46.—*By the Society.*

Report of the Proceedings in the Cambridge Observatory Relative to the New Planet. By Professor J. Challis.—*By the Vice-Chancellor of the University.*

Observations on the General and Medical Management of Indian Jails; and on the Treatment of some of the Principal Diseases which Infest them. By James Hutchinson, Esq., Surgeon on the Bengal Establishment, &c.—*By the Author.*

Monday, 18th January 1847.

Sir GEORGE S. MACKENZIE, Bart., V.P., in the Chair.

The following Communications were read :—

1. An Attempt to Elucidate and Apply Mr Warren's Doctrine respecting the Square Root of Negative Quantities. By the Right Rev. Bishop Terrot.

The author shewed, in the first place, that the successive radii of a circle forming equal angles with one another, are properly symbolized by the successive expressions, a, $a \cdot 1^{\frac{\vartheta}{2\pi}}$, $a \cdot 1^{\frac{2\vartheta}{2\pi}}$ &c., where a is the arithmetical length of the radius, and ϑ the angle which the several radii make with one another. He proceeded to shew how this theory of symbols, first advanced by Mr Warren, is applicable to the problems of plane trigonometry. This was shewn in several elementary propositions, in a new demonstration of Cotes' properties of the circle, and in the demonstration of a property closely connected with these, namely, that if A be the extremity of a diameter, and P, P_1, P_2, P_3, &c., be the extremities of a series of equal arcs measured in the same direction from it, then $A P_1 \times A P_2 \ldots \times A P_{n-1} = n R^{n-1}$. The author finally shewed that though this symbolism is applicable to the most elementary propositions of geometry, it is applicable under forms which are unsuitable to the purposes of elementary instruction.

2. Notes on the Superficial Strata of the Neighbourhood of Edinburgh, concluded. By Dr Fleming.

The author began by adverting to the position of Edinburgh as favourable to the study of geology, from the proximity of the transition rocks, the old red sandstone, and the coal measures ; and then stated, that the deposits belonging to the modern epoch were equally accessible and interesting.

In order to illustrate the character of the superficial strata, the author considered, in the first instance, the condition of the *surfaces of the rocks*, on which these loose deposits rest. He had found many examples, as at Craigleith, Granton, and Sampson's Ribs, where, on the removal of the boulder-clay, the surfaces of the rocks were *dressed* and scratched in a direction nearly from W. to E. ; while in other cases, as on Arthur's Seat, the Calton Hill, and Torduff to the westward of Bonally, near Colinton, the surfaces were equally dressed and scratched although the superficial strata were absent.

In the neighbourhood of Edinburgh, the oldest member of the superficial strata is the BOULDER-CLAY, which rests on the dressed surfaces. The *boulders* are chiefly of trap, rounded, and, in many instances, scratched, and the accompanying *fragments* are portions of the strata in the immediate neighbourhood. The author was inclined to the belief, that the boulders had acquired their form previous to their enclosure in the clay, through which they are irregularly distributed, scarcely ever in *contact*; and in which, even supposing the mass in motion, they must, in a great measure, be in relative rest.

The author considered it probable, that the *dressing* had taken place previous to the formation of the boulder-clay, and at the period when the boulders themselves acquired their form; and that it was produced, when the rocks were under water, by the attrition of stones or ice. He here adverted to the action of icebergs, and considered their influence as having been exerted in *fresh water*, according to a notion which he had long entertained, that, during the formation of the older members of the superficial strata, the land under consideration was covered by a lake of fresh water. He then adverted to the glacial theory of Agassiz, as well fitted, if otherwise probable, to account for the dressings; because, if a sheet of ice covered the country for a series of years, it must have experienced innumerable shiftings, and produced corresponding abrasion of the rocks, *by its expansions and contractions as a solid*, with every change of temperature, independent of any dilatation by freezing.

The author here took notice of the structural character of the trap rocks of the district, as exhibiting, in the *crumpled surfaces* of their strata, and the *slickensides* with which they abound, appearances which may readily be confounded with *dressings*.

The difficulties attending any explanation, yet proposed, of the mode of formation of the boulder-clay, the author considered as very great, and frankly stated that hitherto he had not been able to arrive at any satisfactory conclusion.

The boulder-clay becomes more sandy towards the upper portion of the mass, and passes, by means of alternating beds, into the next newest formation, the boulder gravel and sand. The gravel forms the lowest portion of this mass, and frequently contains numerous boulders of trap towards its lowest portion. The sand towards the upper portion is horizontally stratified with numerous examples of disturbance, while, towards the boulder-clay, it is very imperfectly assorted by water. Throughout the upper portion of the mass there are beds of silt or brick-clay.

The author then proceeded to the consideration of the great deposits of brick-clay and stratified sand at Portobello and Granton. At the former place the clay is finely stratified, graduating by alternations with fine sand, into the incumbent sand. At Granton the brick-clay is very imperfectly stratified, and is abruptly covered with a mass of gravel distinctly assorted by water.

On comparing the relative position of the boulder-clay, gravel, sands, and silt, and their transitions, the author considers them as the production of a single series of changes, and in the later stages as having a soil with plants and animals at no great distance.

The subject of *Erratics* was next brought under the notice of the Society; and the author remarked that those which occur on the surface, at the north end of Salisbury Crags, had probably been exposed by the washing away of the boulder-clay. There were no angular blocks resting on the surface to be seen in the immediate neighbourhood, such as abound in the district around Aberdeen, and which are different from the contents of the inferior clay.

The Marine Diluvium next occupied the attention of the author. He stated that this deposit occurred on the sea-shore from Portobello to Granton, and had been regarded as an ancient sea-beach, now exposed in a raised position by an elevation of the land. The author, after illustrating the character of a modern sea-beach, stated that this bank of shells did not exhibit any of the characters of a beach, but gave unequivocal proofs of its being *storm-raised*, in the mixture of littoral and deep water shells, in overturned boulders with limpets, barnacles, &c., in their original position, in the direction of flat stones in the bed, and in the appearances of stratification. This view the author stated had been forced upon him by an examination of the prolongation of the same bed at Borrowstonness, and which he had published in Dr Thomson's Annals of Philosophy, for August 1814.

The author concluded his remarks by stating, that the sea-shells found a few feet below the surface of the *carse clay* at Clackmannan, Stirling, &c., were in like manner derived from a violent inundation of the sea, and could not be considered as an old sea-beach—an opinion which had been adopted by several of the authors of illustrations of the geology of the district, on very insufficient grounds.

Monday, 1st February 1847.

Sir THOMAS M. BRISBANE, Bart., President in the Chair.

The following Communications were read :—

1. Speculation respecting the origin of Trap-Tuff, the Cause of Earthquakes, and of Partial Changes of the Bed of the Ocean. Part I. By Sir G. S. Mackenzie, Bart.
2. On the Principle of Vital Affinity. Part II. By Dr Alison.

The following Gentlemen were duly elected Ordinary Fellows :—

W. Thomson, Esq., Professor of Natural Philosophy in the University of Glasgow.
J. H. Burton, Esq., Advocate.

The following Donations to the Library were announced :—

On the Volcanoes of the Moon. By James D. Dana.—*By the Author.*

Eleventh, Twelfth, and Thirteenth Letters on Glaciers. By Professor J. D. Forbes.—*By the Author.*

Illustrations of the Viscous Theory of Glacier Motion. From the London Philosophical Transactions. By James D. Forbes, F.R.S.S.L. and E.—*By the Author.*

Zur Vergleichenden Physiologie der Wirbellosen Thiere. Von Dr Carl Schmidt.—*By the Author.*

Entwurf einer Allgemeinen Untersuchungs Methode der Säfte und Excrete des Thiereschen Organismus. Von Carl Schmidt.—*By the Author.*

Astronomical Observations made at the Royal Observatory, Edinburgh. By the late Thomas Henderson, F.R.S.S.L. and E. Reduced and Edited by his Successor, Charles Piazzi Smyth. Vol. VI., for the year 1840.—*By the Astronomical Observatory.*

On the Laws of the Tides on the Coasts of Ireland. By G. B. Airy, F.R.S.—*By the Author.*

An Explanation of the Observed Irregularities in the Motion of

Uranus on the Hypothesis of Disturbances caused by a more Distant Planet. By J. C. Adams, Esq. M.A. *By the Author.*

Scheikundige Onderzoekingen gedaan in het Laboratorium der Utrechtsche Hoogeschool. Deel 4de., Stuk 1ste.—*By the Editors.*

Guide to the Geology of Scotland. By James Nicol, Esq.—*By the Author.*

Account of Iceland, Greenland, and the Faroe Islands. By the same.—*By the Author.*

Specimen of Metamorphic Limestone, dislocated by the vicinity of Trap-Rocks, near North Berwick.—*By Sir G. S. Mackenzie, Bart.*

Monday, 15*th February* 1847.

Dr CHRISTISON, Vice-President, in the Chair.

The following Communications were read:—

1. A Speculation, connecting the origin of Trap-Tuff, the cause of Earthquakes, and of Partial Changes of the Bed of the Ocean. Part II. By Sir G. S. Mackenzie, Bart.

The author, having observed in Iceland some remarkable intermixtures of lava with volcanic tuff; and, more recently, on the coast of East Lothian, a somewhat similar intermixture of masses of greenstone in trap-tuff, conceived the idea that cavities existed underneath the crust of the earth, or deeply seated within it, containing water exposed to heat, and the agitation of which, occasioned by the production of steam, had reduced the materials of trap-tuff to the state in which we find them, which had been afterwards erupted at the bottom of the sea, or forced between the beds of superincumbent rocks. Some time afterwards, on reading the researches of Mr Hopkins in Physical Geology, he was gratified to find that profound philosopher advocating the existence of vast cavities containing lava, and connected with volcanic vents at a depth such as would admit of steam-power throwing the lava to the surface. It did not now appear difficult to imagine similar cavities containing heated water, and the materials of trap-tuff; and he therefore extended the hypothesis of Mr Hopkins so as to account for its origin, and for masses of trap occurring in it, the form of which seemed to indicate that they had been forced into the tuff at the time of its eruption, from some neighbouring cavity containing melted matter.

The author next adverted to the phenomena of earthquakes; and referring to the Great Geyser, the eruptions of which, at the time when he saw it, were preceded by sounds resembling the discharges of artillery, and trembling of the ground, he inferred that the noises and shaking of the ground during earthquakes, were caused in the same manner in great cavities, such, most probably, as those which contained the materials for trap or volcanic tuff. The cause of the noises the author conceived tc be the occasional production of masses of steam, reaching the upper and colder portions of the cavity, or coming in contact with water and being suddenly condensed, so as to produce a vacuum with extreme rapidity, the collapse, on its being instantly filled up, causing sound and concussion. To explain this part of the subject, he referred to a paper on sound, which he had read to the Society some years before, and part of which will appear in the number of Jameson's Journal for April 1847.

The author next proceeded to extend the hypothesis of Mr Hopkins farther; and endeavoured to shew, that the operations going on within the cavities would necessarily go on extending their dimensions, and weakening their roofs. This weakening would be greatly increased by the eruption of the contained matter, and the enormous masses of igneous rocks existing in volcanic districts and elsewhere, proves that proportional spaces must have been left empty underneath. Vast tracts of land by being thus undermined may have sunk and disappeared under the ocean. Several geologists have supposed it probable, from various facts, that Europe and America were once united, the Northern Atlantic having been occupied by land. This being admitted, the author referred to the probability of the sinking of the land having been caused (perhaps in large portions at different periods) by the extension of such cavities as those which Mr Hopkins has supposed to exist, and to their roof giving way. The British Islands, the Faroe Island, and Iceland, may be regarded as remnants of the lost continent; and the latter island exhibits the strongest possible indications of preparation for the catastrophe of its disappearance. The precipitous shores of these remnants prove that violent fracture had taken place; and that this island had been more elevated at a former period, is rendered probable by the exceedingly shattered condition of their rocks. Admitting this, and the extension of the land over the Atlantic, the climate of Britain must formerly have been severe enough to produce glaciers, which have left those marks which have been ascribed to them.

The author took occasion to shew, that the effect of the sudden sinking of a great mass of land, and the access of the sea to the emptied space, would be to raise, by vast currents rushing into it, an enormous mass of water to a great height, forming a huge initiatory wave, consisting of greatly agitated water, loaded with the debris of the former land, as well as terrestrial and aquatic animals, many of them dragged from warmer latitudes, by the violent currents rushing from all sides towards the empty space. This huge mass of water, as soon as its weight balanced and overcame the momentum of the current that had lifted it, would produce a wave which, being propagated till it reached our land, would break over it, and form powerful currents guided by the inequalities of the land, and leaving behind them the detritus which at present covers the rock surface.

The author also noticed, in consequence of some discussion that had taken place on the appearances presented by the diluvial masses, a fact he had several times observed, shewing that the appearance of stratification might not, in every case, depend on deposition from water comparatively at rest. On artificial heaps of heterogeneous matter which had been left undisturbed for many years, being cut down, he had noticed, and pointed out to others, that the materials had arranged themselves in lines, in the manner of flint in chalk, and strata of finer materials. Hence he supposed it probable that, in many cases, the diluvial masses had, in like manner, arranged themselves, and assumed a stratified appearance. He was led also to the inference that, instead of having been quietly deposited, many of the great masses of sandstone, now exhibiting distinct layers, had been produced at once by ancient debacles.

2. On Vital Affinity. Parts II. and III. By Dr Alison.

In continuation of his former paper, Dr Alison proceeded to review the chief facts known in regard to the formation of the *oils* and the *albuminous* compounds in organized bodies, with the view of illustrating the modification given by vitality to chemical affinities. The formation of fat or oil in vegetables appears to be effected simply by the separation of oxygen from some of the varieties of starch; but in animals, where no oxygen is evolved, and where, nevertheless, oily compounds may certainly be formed from starch (as shewn by Liebig, Chevreul, and Milne-Edwards), this appears to be effected by an affinity of the greater part of the carbon and hydrogen of the starch, for a small part of the oxygen, to form fat, while a smaller part of the carbon and hydrogen, with the greater part of the oxygen of the starch, aided by additional oxygen absorbed from the air, passes off as carbonic acid and water. In both cases, the essential characters of that affinity, which appears to be peculiar to the state of life, lies apparently in the attraction of carbon for hydrogen, with a much smaller proportion of oxygen than exists in the compounds of these elements existing in the inorganic world ; and as no such compound is formed from starch, under the same circumstances in other respects, without the presence of living cells, he regards the formation of oil in living bodies as an effect of vital affinity ; although admitting that in the course of the decomposition of animal compounds, by various chemical agents, oily matters may be formed by simple chemical action, as in the well-known example of the formation of adipocere from fibrin.

The fact that the formation of fat in the animal body is so notoriously diminished or restrained by exercise, increasing the supply of oxygen, and promoting, therefore, the excretion of carbonic acid and water, he thinks very important, as indicating, along with other facts, the principle that vital affinities do not supersede the usual chemical relations of the elements that are liable to them, but are merely added to these, and allow of a division of those elements between compounds formed by vital and by simply chemical powers.

He next referred to the important question, now warmly disputed among chemists, whether albumen can be formed in the animal body, or only passes into it, directly or indirectly, from vegetables, where it is believed to be formed, by a vital action, from the elements of starch, and those of ammonia (in whatever way this last may be supplied), the elements of water and a little oxygen passing off at the same time in the usual exhalations of the plant. He pointed out that, by the addition of a full supply of oxygen, it is quite possible that the elements of starch with ammonia in the animal body may divide themselves into two portions—the one containing the greater part of the carbon falling by vital affinity into the proportions of the albumen, while the other, absorbing the oxygen, passes off as carbonic acid and water, the constant excretions of animals ; and he conjectured that the elements of ammonia, requisite for this action, may be supplied in animals by the air which is continually taken into the stomach, in the water and in the saliva which are habitually swallowed, and which will there be under conditions very similar to those in which air and water are known to form ammonia. He stated likewise that as it is certain that fat is formed in the animal body from starch, and that gelatin is formed in it, probably from albumen, it is certain that the term assimilation, in the physiology of animals, cannot be restricted to the mere selection and appropriation of compounds already formed in vegetables (as Dumas supposes), but must include, also, certain processes of transformation (as maintained by Dr Prout). And admitting that the question, as to the actual formation of albumen in animals, can only be finally decided by ascertaining whether the whole quantity of azote thrown off from an animal in a given time is greater than is introduced into it in the form of albuminous ingesta, he urged various reasons for inclining to the opinion, that a certain quantity of albumen is formed, chiefly from the anylaceous ingesta, in the animal body.

He admitted, however, that a comparison of the proportion of the elements in the azotised aliments, and in the excretions, shews that the formation of albumen in the animal frame must be to a very small extent only ; and stated that the general distinction of the azotised, or the chief nutritious portion of the aliments, and the non-azotised or chiefly calorific portion of them, and the doctrine of these last protecting the albuminous part of the blood, and the textures from the agency of the oxygen taken into the blood, and that of the wasting of the textures, and death by anœmia in weakening diseases, being due to the action of the oxygen, appear to him to be quite consonant with clinical observations in various diseases, and to be an important addition to pathology ; besides giving us more precise ideas as to the nature of the function of digestion, and the law of Prout, of the necessity of mixture of aliments for the support of animal life. This doctrine is the strongest illustration of what was formerly stated as to the use of the oxygen taken into the animal body, viz.,—not to take any direct part in the formation of compounds by strictly vital affinities, but to exert a simply chemical action on all organic compounds capable of yielding to simply chemical affinities, and so to support the excretions.

The formation of gelatin in the living animal body, he considered as certainly owing to the separation of carbon and hydrogen, by help of the oxygen of the air ; but pointed out the possibility of this taking place, not merely, as Liebig states, from the action of the oxygen on the elements of albumen, but likewise from its action on the elements of starch with ammonia, provided that a certain quantity of oil or fat is formed at the same time.

He then proceeded to consider the general theory of excretion in the animal body, beginning this subject with the striking fact, noticed by both Prout and Liebig, that although oxygen from the air must be a main agent in forming all the excretions (because oxygen exists in them all in a larger proportion than in those aliments and those textures of the body from which they must be respectively derived), yet the oxygen taken in at the lungs does not appear to enter into the combinations by which the excretions are formed (particularly, does not form carbonic acid), as long as it is passing along the arteries, but "changes its mode of action" when it reaches the capillaries, where it must meet with the matter absorbed from the textures. He stated it as the general, and apparently first, opinion of physiologists, that the excretions are furnished partly by redundant ingesta, and partly by "effete" matter in the system itself; but the important agency of oxygen in maintaining them had not been so generally recognised, and the term "effete" has in general no very definite idea attached to it. But, combining together all that is known as to the continued interstitial absorption in animal bodies, the continual introduction of oxygen into them, and as to the nature and quantity of the excretions, as compared with the ingesta, he stated it as the most general expression of these facts, that throughout all the parts of any living animal where nutrition and absorption are going on, *i. e.*, at the extremities of the capillary vessels, in the more perfect animals, carbon, nitrogen, hydrogen, and oxygen, are continually forming two sets of compounds,—certain portions of these elements, recently introduced in the form of aliments, either separating, in the form of the organic compounds, from the other constituents of the blood, or uniting to form those compounds,—and in either case attaching to themselves particles of earthy and saline and inflammable bodies, taking the form of cells or fibres, and building up the organised frame; while other portions of the same, which have been for some time in the body, rejecting these adventitious matters, and uniting with oxygen from the air, are continually falling into the proportions by which the compounds destined to excretion are formed, which are poisonous to the system if retained, which tend always to the crystalline form, and, in fact, are steps in the process of the gradual restoration of these elements to the inorganic compounds, carbonic acid, water, and ammonia, out of which the agency of vegetable life had originally formed them.

This general fact he considered as the clearest proof of that principle of the modification of chemical affinities by the contact of living structures, to which the term Vital Affinity is applied; and, at the same time, as an indication of this distinctive peculiarity of the vital affinities, that, like all other actions strictly called vital, they are of *transient duration only;* and that the life of every individual animal is maintained only by the successive life and death of all the atoms of organised matter of which it is composed; every portion, as it dies, being removed from its place by interstitial absorption, becoming liable to the influence of the oxygen (as it would be, if separated from the body, and undergoing decomposition), and serving for the maintenance of the different excretions.

This opinion he stated as having been adopted by several physiologists of late years, in regard to those portions of animal textures which are engaged in active vital operations, particularly muscular and nervous parts; but its importance as a general physiological principle, connecting together the necessity of continued nutrition for the maintenance of animal life (even during the decline of the body), the intention of interstitial absorption, the use of respiration, and the necessity and nature of the excretions, had not, as he thought, been pointed out in any physiological work. He acquiesced, however, in the statement of some recent authors, that this partial death of portions of the animal frame appears to be greatly accelerated by local increase of any of the strictly vital actions; and under this law, he thought we may include not only the fact of the loss of power in a muscular or nervous part from over-exertion (leading to increased interstitial absorption, and thereby, in a healthy constitution, and where the exertion is not excessive, to subsequent deposition and hypertrophy), but likewise, the increased interstitial absorption, the ulcerative absorption, the partial sloughing, or general gangrene of a part that has been inflamed,—all these being results of the death of certain particles of matter concerned in the inflammation, and varying in degree as this death is more partial and gradual, or more general and sudden.

On the whole, therefore, he maintained, that not only the general principle may be held to be ascertained of peculiar vital affinities actuating the portions of matter by which the organic principles are formed, and organised structures developed, but that the facts already known on the subject, justify several important inferences in regard to the peculiarities of these affinities, both as to their nature and duration; and that we can point out, with at least much probability, the share which these affinities take in all the changes to which the elements composing organised bodies are subjected, from the time of the first separation of carbon from oxygen, which is essential to vegetable life, till the restoration of all the elements thus employed, by the aid of oxygen taken into the body, to the condition of inorganic compounds, in the excretions that are continually thrown off from living animals.

The following Donations to the Library were announced :—

Astronomische Nachrichten. Edited by Professor Schumacher. Nos. 560 to 586.—*By the Editor.*

Astronomische Beobachtungen auf der Koniglichen Universitäts Sternwarte in Konigsberg, von F. W. Bessel fur 1835; und von A. L. Busch fur 1836.—*By the Authors.*

Observations on the Mortality of the Scottish Widows' Fund and Life Assurance Society, from 1815 to 1845. By James Beg-bie, M.D., F.R.S.E.—*By the Author.*

Niewe Verhandelingen der Eerste Klasse van het Koninklijk-Nederlandsche Instituut van Wetenschappen, Letterkunde en Schoone Kunsten te Amsterdam. Deel 12, Stuk 3.—*By the Institute.*

The American Journal of Science and Arts. Conducted by B. Silliman junior, and James D. Dana. For January 1847.—*By the Editors.*

Astronomical Observations made during the year 1845 at the National Observatory, Washington, under the direction of M. F. Mauray, A.M., Lieut. U.S. Navy, Superintendent.—*By the U.S. Naval Observatory.*

Ricerche Fisico-Chimico-Fisiologiche sulla Luce del Prof. Abate Francesco Zantedeschi.—*By the Author.*

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. II. 1846-7. No. 30.

Sixty-Third Session.

Seventh Ordinary Meeting, 1st March 1847.

Sir THOMAS MAKDOUGALL BRISBANE, Bart., President, in the Chair.

The following Communications were read :—

1. On the co-existence of Ovigerous Capsules and Spermatozoa in the same individuals of the Hydra viridis. By Dr Allen Thomson.

In this communication, the author described some observations made by him in the autumn of 1845, by which he had ascertained that in the Common Green Polype, as had previously been observed in the *Hydra fusca*, generation takes place at the approach of winter by the development of impregnated ova.

Professor Thomson ascertained, that, at this season, while a number of the polypes bore the spermatic capsules near the base of the arms, a few animals were to seen, on which, besides these capsules, an ovum was developed from the exterior of the middle of the body.

The act of fecundation was observed : the sub-division of the yolk, while the ovum still remained in its capsule, attached to the parent, was distinguished, as well as the subsequent separation of the ovum ; but the author had not an opportunity of tracing the formation of a young polype from the ovum.

The author was not inclined to regard the process of gemmation, which is the more frequent mode of multiplication in these animals, as explicable on the supposition that the buds are developed from ova that have been previously impregnated, and are retained in the substance of the parent's body.

2. On the Parallel Roads of Lochaber; with Remarks on the change of relative Levels of Sea and Land in Scotland, and on the Detrital Deposits in that Country. Part I. By David Milne, Esq.

The author, after referring to the views of former observers, stated, that though he had proceeded to Glen Roy under a strong impression that Mr Darwin's marine theory afforded a solution of the question, he had felt himself constrained, after an examination of the valleys, to abandon that theory, and that he had satisfied himself that the shelves had been formed by lakes of fresh water. He referred to the proofs still existing of the mode in which these lakes had been discharged, and he described particularly the unequivocal traces of an old river course running from the head of Glen Glaster to Loch Laggan, with a delta at the level of shelf 4 on Loch Laggan side, formed by this ancient but now extinct river.

The author considered that the blockage of the waters in the valleys had been formed of boulder-clay and other ancient detrital matter. There was proof that this detritus had been deposited in the district before the waters had begun to be depressed, as the shelves were, in some places, *indented* on the boulder-clay; and this detrital matter must, when deposited in the valleys, have been in sufficient quantity to have dammed back the waters, as there were still abundant traces of it on the sides of the hills at a higher level than that of any of the shelves.

The author differed from Dr MacCulloch and Sir Thomas Dick Lauder, in supposing that it was necessary to assume the occurrence of an earthquake or any convulsion of nature to account for the breaking down of barriers, by which the lakes were blocked up. He considered that it was an error to assume that the first depression of the lake in Glen Roy was 82 feet, and shewed that there must have been at least three intermediate depressions; and that after the first depression, the water must have flowed out by Glen Glaster into Glen

Spean. He adduced several considerations to shew that the blockage at the west end of Glen Roy could have been nothing else than detritus capable of being worn down and removed by the gradual operation of a stream, and in this manner be accounted for the ultimate removal of all the blockage which had previously dammed back the water in the several valleys marked by shelves.

He next adverted to the theory of Agassiz, that the waters had been blocked up by the moraine of a glacier descending from Ben Nevis;—a theory which, at the best, could not explain the formation of a lake in Glen Gluoy. The author shewed, however, that it would not explain the shelves in Glen Roy; as the two uppermost shelves, stopped short by two miles of the place where the Ben Nevis moraine is said to have been formed, and the lowest shelf went beyond this place by three or four miles.

The following Gentleman was duly elected an Ordinary Fellow:—

James Nicol, Esq.

The following Donations to the Library were announced:—

Medico-Chirurgical Transactions published by the Royal Medical and Chirurgical Society of London. Vol. XXIX.—*By the Society.*

Memoirs of the Wernerian Natural History Society, for the years 1837–8. Vol. VIII., Part 1.—*By the Society.*

The Journal of Agriculture, and the Transactions of the Highland and Agricultural Society of Scotland.—*By the Society.*

Five Geological Memoirs, viz.—

Geological Features of the Mines of Taurus.

On the Gogofan Mine, Cærmarthenshire.

On the Mining Establishment of France.

On Coal and Lignite, and on Iron and Steel manufactured in France.

On the Mining Academies of Saxony and Hungary. By Warington W. Smyth, Esq.—*By the Author.*

Monday, 15*th March* 1847.

Right Rev. BISHOP TERROT, V.P., in the Chair.

The following Communications were read :—

1. On the Course of Observation to be pursued in future at the Royal Observatory of Edinburgh. By Professor C. Piazzi Smyth.

Being at present chiefly taken up with the computation of his predecessor's observations, the author takes this opportunity of examining into what is going on in other Observatories; and then endeavours, from the results of such a survey, to select some unpursued branch of Astronomy, as the proper subject to which the Edinburgh Observatory should be devoted; and he dwells much on the great caution to be used, in the present multiplication of astronomical observatories, that several of them be not working against each other on the same subject; and urges the extreme importance of first selecting an appropriate object of research, and then following it up with a constancy enduring through ages, after the manner of the Greenwich Observatory, which has consequently produced results of such inestimable service to the promotion of the science.

He then proceeds to describe the several observatories, and the objects pursued at Greenwich, the Cape of Good Hope, Cambridge, Oxford, Armagh, Durham, and Liverpool; and concludes that they are all doing their work so well, that it would be quite useless for any other establishment to follow in the same paths.

But while the movements of the planets round the sun are so well attended to at Greenwich, he remarks that the question of the motion of the sun itself amongst the stars, is equally important, is peculiarly the business of our age to investigate, is only possible to be solved by public observatories with good meridian instruments, and, being unpursued elsewhere, may well be adopted as the peculiar mission of the Edinburgh Observatory.

The proposed method of solving this problem is then described, and consists generally in procuring accurate places of a great number of stars, for three distinct epochs, during a period of thirty years; constants and fundamental points being taken, for specified reasons, from Greenwich.

The Edinburgh Observatory is not, however, to be wholly engrossed in this long and distant inquiry, but is also to do something immediately useful for amateur astronomers, by fixing the absolute places of all stars which have been used by those gentlemen in differential measurements; the want of some establishment to which private observers might send for the determination of their stars of reference, having long been a matter of regret.

In conclusion, the author alludes to the various foreign observatories, whose subjects of observation will, he thinks, by no means interfere with that selected for Edinburgh; and he congratulates himself that having made his election entirely on independent physical considerations, he has at last arrived at the same point to which his predecessor seemed to be drawing nearer and nearer every year.

2. Observations of Terrestrial Temperature made at Trevandrum Observatory, from May 1842 to December 1845. By John Caldecott, Esq. Communicated by Professor Forbes.

This paper is a continuation of the Register of Observations already recorded at page 29 of this volume of the "Proceedings." The following table contains the mean corrected results of the observations at 3, 6, and 12 French feet, and of the temperature of the air for 1843, 1844, and 1845 taken together:—

	No. 1. 12 feet Thermometer.	No. 2. 6 feet Thermometer.	No. 3. 3 feet Thermometer.	Air Temperature.
January	85·528	85·618	84·954	78·930
February	85·784	86·625	86·838	80·386
March	86·373	88·110	88·789	82·730
April	86·916	88·527*	89·614	83·370
May	....	88·224†	88·413	81·603
June	86·878†	86·883	85·012	79·023
July	86·537	85·114	83·250	78·450
August	85·894	84·736	83·566	78·990
September	85·633	85·133	84·575	79·973
October	85·680	85·632	84·722	79·076
November	85·651	85·271	84·622	79·750
December	85·607	85·303	84·228	78·030
Means	86·043	86·264	85·715	80·025

* Mean of Two Years only. † Result of 1843 only.

3. On the Temperature of Wells and Springs at Trevandrum in India (Lat. 8° 31′, Long. $5^h\ 8^m$.) By Major-General Cullen, Madras Artillery. Communicated in a Letter to Professor Forbes.

The tables accompanying this letter include daily observations of three wells and one spring for the greater part of 1842 and 1843. The following is an abstract:—

Abstract of the Depth and Temperature of Wells at Trevandrum for the years 1842–43.

Dates.		Garden Well. Depth from surface of ground 40 feet.		Kitchen Well. Depth from surface of ground 48 feet.		Village Well. Depth from surface of ground 36 feet.		Spring.	Rain.
		Mean depth of Water.	Mean Temp.	Mean depth of Water.	Mean Temp.	Mean depth of Water.	Mean Temp.	Mean Temp.	Total.
		Feet.	Degrees.	Feet.	Degrees.	Feet.	Degrees.	Degrees.	Inches.
1841	Dec. .	$11\frac{1}{3}$	83	15	81	12	82		0·1
1842	Jan. .	9	83	12	$81\frac{1}{2}$	9	$82\frac{1}{2}$		$3\frac{3}{4}$
...	Feb. .	7	83	9	81	7	82		$0\frac{1}{3}$
...	March	6	84	$2\frac{1}{3}$	$82\frac{3}{4}$	$2\frac{1}{3}$	$82\frac{3}{4}$		$0\frac{3}{4}$
...	April	$5\frac{1}{2}$	$84\frac{3}{4}$	$1\frac{1}{5}$	$83\frac{1}{8}$	$0\frac{1}{2}$	$83\frac{1}{4}$		$3\frac{1}{2}$
...	May .	$6\frac{3}{4}$	84	2	$82\frac{3}{4}$	2	$82\frac{3}{4}$		$13\frac{3}{4}$
...	June .	$13\frac{3}{4}$	$83\frac{1}{4}$	12	$82\frac{1}{4}$	$11\frac{1}{2}$	$82\frac{3}{4}$		$9\frac{1}{2}$
...	July .	$8\frac{3}{4}$	$83\frac{1}{2}$	$9\frac{3}{4}$	$82\frac{3}{4}$	$7\frac{3}{4}$	83	$86\frac{1}{2}$	$4\frac{1}{4}$
...	Aug. .	8	$83\frac{1}{4}$	$7\frac{1}{2}$	83	6	83	86	$3\frac{3}{4}$
...	Sept. .	$7\frac{1}{2}$	$83\frac{3}{4}$	4	$82\frac{3}{4}$	3	83	86	$6\frac{1}{3}$
...	Oct. .	$8\frac{1}{4}$	84	6	83	$3\frac{3}{4}$	83	86	5
...	Nov. .	$11\frac{1}{3}$	$82\frac{3}{4}$	$9\frac{3}{4}$	83	7	82	86	$8\frac{3}{4}$
...	Dec. .	12	$83\frac{3}{4}$	$12\frac{1}{2}$	$82\frac{3}{4}$	8	$82\frac{3}{4}$	86	$0\frac{1}{3}$
	Mean	9	$83\frac{1}{2}$	7	$82\frac{1}{2}$	7	$82\frac{3}{4}$	86	$59\frac{1}{2}$
1843	Jan. .	$6\frac{1}{2}$	84	$7\frac{1}{2}$	$82\frac{1}{2}$	$4\frac{1}{2}$	$82\frac{1}{2}$	$85\frac{2}{8}$	$0\frac{3}{4}$
...	Feb. .	$4\frac{1}{3}$	$84\frac{3}{4}$	$2\frac{1}{2}$	$82\frac{1}{2}$	$0\frac{3}{4}$	82	$85\frac{2}{8}$	0
...	March	6	85	$1\frac{1}{2}$	$82\frac{3}{4}$	$0\frac{1}{2}$	82	$84\frac{4}{8}$	$2\frac{1}{2}$
...	April	7	$84\frac{3}{4}$	1	$82\frac{3}{4}$	$0\frac{1}{2}$	$82\frac{1}{2}$	85	$8\frac{3}{4}$
...	May .	11	$84\frac{1}{4}$	5	83	$3\frac{1}{4}$	83	86	17
...	June .	$21\frac{1}{2}$	$83\frac{1}{2}$	$20\frac{1}{2}$	83	$16\frac{1}{4}$	83	$86\frac{1}{4}$	$16\frac{1}{2}$
...	July .	$24\frac{1}{4}$	83	$26\frac{1}{2}$	$82\frac{3}{4}$	20	83	$85\frac{6}{8}$	13
...	Aug. .	$15\frac{1}{4}$	$83\frac{3}{4}$	$19\frac{1}{2}$	82	14	$83\frac{3}{4}$	86	$2\frac{1}{2}$
...	Sept. .	$10\frac{1}{2}$	84	$11\frac{1}{2}$	82	$7\frac{1}{4}$	83	86	3
...	Oct. .	9	84	8	$82\frac{3}{4}$	4	83	86	$7\frac{1}{2}$
...	Nov. .	9	84	5	83	$2\frac{3}{4}$	$82\frac{3}{4}$	86	2
...	Dec. .	$14\frac{1}{4}$	83	$13\frac{1}{4}$	82	$8\frac{1}{3}$	$82\frac{3}{4}$	86	$11\frac{1}{2}$
	Mean	$11\frac{1}{2}$	84	10	$82\frac{1}{2}$	$7\frac{1}{6}$	$82\frac{3}{4}$	$85\frac{3}{4}$	85

The following remarks in Major-General Cullen's letter illustrate some particulars of the observation.

"In a dry season the spring sometimes ceases altogether, but it is very singular, that on its reappearance after a few days' rain, its temperature continues the same nearly as before.

"I had supposed that the temperature of such a spring, as that shewn by Mr Caldecott's ground-thermometers, would have been nearly the mean annual temperature of the atmosphere at the place of observation, but I perceived that his mean annual temperature for Trevandrum is only about 80°; but Colonel Sabine has already pointed out some peculiarities in the meteorology of Trevandrum which may have a wider application than has hitherto been suspected. The early sea breezes which affect the barometer, &c., may not extend their effect below the surface of the ground.

"Even the temperature of the wells is remarkably uniform, though lower than that of the spring. In none does the monthly temperature differ above 1° from the annual. The kitchen and village well are upon the top of a swell, while the garden-well is on a considerably lower level, down a slope half-way down to the spring, and the average depth of water of the garden-well is also greater than the other two. May these differences account for the higher temperature of the garden-well? The depth from the surface of the ground of all the wells was nearly alike, 40, 48, and 36 feet."

4. Chemical Notices. By Professor Gregory.

(1.) *On a Fatty Substance derived from Animal Matter.*

This substance, derived from a pig buried for fifteen years on the side of a hill, was found to consist of free fatty acids, with a mere trace of animal matter, and no appreciable trace of phosphate of lime. The bone earth appears to have been dissolved by the water percolating through the soil.

(2.) *On a Black Powder which appeared on the Surface of Loch Dochart, on the morning of* 23*d November* 1846.

This powder was found to have the composition of humus in a very advanced state of decay, and was probably derived from peat. It contained 77 per cent. of carbon; and left, when burned, hardly a trace of ashes.

(3.) *On the Preparation of Hippuric Acid.*

The author, after describing this acid, and the interest attached to it, gave an improved, easy, and productive method of preparing it, and exhibited specimens of the acid so prepared.

The process consists in boiling the urine of the horse with lime, and then rapidly boiling down the filtered solution, after which the addition of hydrochloric acid causes a deposit of impure hippuric acid. This is easily purified by a repetition of the process of boiling with lime, &c.

The following Donations to the Library were announced:—

The American Journal of Science and Arts. By Professors Silliman and Dana. Second Series, No. 6, 8vo.—*By the Editors.*

The Quarterly Journal of the Geological Society. No. 9, 8vo.—*By the Society.*

The Journal of the Royal Asiatic Society. No. 17, Part II., 8vo. —*By the Society.*

Resultate des Magnetischen Observatoriums in München während 1843–4–5. Von Dr J. Lamont. 4to.—*By the Author.*

Travaux de la Commission pour fixer les Mésures et les Poids de l'Empire de Russie, redigés par A. Th. Kupffer. 2 vols 4to, 1 vol. folio.—*By the Author.*

Memoires de la Société Géologique de France. Deuxième Serie. Tome II., première Partie.—*By the Society.*

Annuaire Magnétique et Météorologique du Corps des Ingénieurs des Mines de Russie, par A. T. Kupffer. Année 1843, Nos. 1 and 2, 4to.—*By the Russian Government.*

Carte Climatologique de Varsovie, par Albert Jastrzebowski. One Sheet.—*By the Author.*

Monday, 5th April 1847.

Sir THOMAS MAKDOUGALL BRISBANE, Bart., President, in the Chair.

The following Communications were read:—

1. Remarks on the Hypothesis of Progressive Development in the Organic Creation. By Sir G. S. Mackenzie, Bart.

This paper the author considered as nothing else than an abstract of the thoughts he entertained on the subject. His object was to shew there was no analogy in nature, rendering a prospective law of progressive development from lower to higher types probable; and he brought forward various examples to prove that, in all cases in which variation took place among domestic animals and vegetable productions, the varieties, though they might be regarded as improved or new, were not permanent, but required selection and care to preserve them; so that new forms and qualities appearing, could not confirm the progressive hypothesis.

Referring to what may have been the original condition of things on their being created, that condition, in respect to many, may not have altered, either in improvement or deterioration. It is apparent, however, that special constitutions must have been given originally to those animals and vegetables, more particularly intended for the use of man, which admitted of natural or artificial, but not permanent, variation. Man himself possesses the constitution giving a tendency to vary, so that every individual may be deemed a variety; but no natural progress towards a higher type is apparent. In this case, as well as in portions of inferior creation, though we can compare one being with another, and perceive improvement or deterioration, we have no standard to appeal to for the purpose of examining the extent of variation, one way or the other, from the period of creation downwards to our time. Man, in his best condition, can only aspire to see his race possessed of what he esteems desirable in moral, intellectual, and physical qualities; and may, to no inconsiderable extent, succeed in his endeavour to advance, if he make an effort. Unless, however, it be a sustained one, he will fall back in the scale of humanity, instead of advancing, as daily experience proves. Whoever will look carefully at what is called the progress of civilization, may be convinced that the honoured word is applied too exclusively to the progress of wealth, power, and luxury, rather than to the promotion of the qualities that properly distinguish humanity, and which are found wanting wherever an effort has not been made to elevate the human character by education and moral training. The very idea of education indicates a tendency to deterioration, and the necessity of an effort to counteract it. On the whole, it does not appear that any natural analogy can be found to support the idea of progressive development from lower to higher types. We see no new races appearing, and we find only varieties. The stronger, because cultivated (not newly developed), intellect and energy of Europe are subduing or extirpating the inferior and weaker races in other parts of the world; but the substitution of a better race in this manner is not progressive development.

2. On the Parallel Roads of Lochaber; with Remarks on the change of relative Levels of Sea and Land in Scotland, and on the Detrital Deposits in that Country. Part II. By David Milne, Esq.

The author proceeded to shew that the lake theory of the Lochaber shelves was not inconsistent with any established geological truths, but was on the contrary supported by them.

He alluded to the occurrence in the Lochaber district, as well as in other parts of Scotland, in valleys far from the sea, not only of lakes at high level, but of beach lines on hill-sides precisely analogous to those of Glen Roy, and shewing depressions of water to nearly the same extent. One of the localities referred to is a valley near Inverournan, where three parallel roads are to be seen shewing a depression first, of about 197 feet, secondly, of 94 feet, and lastly, of 184 feet. The blockage required for this ancient lake, and of which a small remnant still exists, was nearly as extensive as that require for Glen Roy.

Other localities were pointed out where parallel roads on hill-sides, similar to those of Lochaber, were to be seen.

In corroboration of the existence of lakes at high levels which no longer exist, reference was made to the existence of River Haughs at considerable heights above the present course of the rivers.

The author proceeded next to shew, that when the rivers ran in these higher channels, the sea stood at a higher level than at present. He, in proof of the former submergence of the land beneath the waters of the ocean, and its gradual emergence from it, referred, 1*st*, to the occurrence of marine remains at considerable heights above high-water mark; 2*d*, to the existence of extensive sand-banks which could have been formed only at the bottom of a deep sea; and, 3*d*, to lines of ancient sea-cliffs and terraces along the coast.

The author alluded next to the nature of the deposits formed, when the land was covered by the sea, and endeavoured to shew, that during this period the boulder-clay had been transported by some great oceanic movement whereby the valleys were filled with detritus. He shewed that the detritus had come from the westward, and, therefore, that valleys situate, like those of Lochaber, on the east side of lofty mountains, would be especially liable to be blocked up by detritus.

Reference was made to a number of boulders resting on beds of sand and fine gravel in the counties of Nairn and Moray, which appeared to have been floated by ice, at a later period, and as the land was emerging from the sea.

An account was given of a number of phenomena, which apparently were due to the emergence of the land from beneath the waters of the ocean. In particular, an account was given of beds of gravel lying over beds of sand and mud, contrary to the law of specific gravity; of long ridges of gravel and sand generally parallel to the lines of coast, or the direction of valleys, and of old sea-cliffs and sea-terraces at various heights above the sea.

In reference to this last point, the author observed, that there were reasons why geologists should not reject as unworthy of notice, the possibility, that the land may, in some cases, not have been the moving body, but that owing to elevations and depressions in the bed of the ocean, the waters may have advanced or receded, and thus formed the lines of ancient sea-cliffs.

Verbal Communication on Fossils of the Lias Formation, from South Africa. By Dr Fleming.

The following Donations to the Library were announced:—

Journal of the Asiatic Society of Bengal, 1846. No. 170.—*By the Society.*

Journal of the Royal Geographical Society of London. Vol. XVI., Part 2.—*By the Society.*

Memoirs of the Literary and Philosophical Society of Manchester. Vol. VII., Part 2.—*By the Society.*

Proceedings of the Royal Irish Academy. Nos. 48 to 53. 8vo.

Transactions of the Royal Irish Academy. Vol. XXI., Part 1.—*By the Academy.*

Monographie Generale de la Famille des Plantaginées, par F. M. Barneoud. 4to.

Monographie des Crucifères du Chili, par F. M. Barneoud. 8vo.

Memoire sur le Developpement de l'Ovule, de l'Embryon et des Corolles Anomales, dans les Renonculacées et les Violariées, par F. M. Barneoud. 8vo.

Memoire sur le Developpement de l'Ovule et de l'Embryon dans le Schizopetalon Walkeri, par F. M. Barneoud. 8vo.—*By the Author.*

Kongl. Vetenskaps-Akademiens Handlingar, for Ar. 1844. 8vo.

Arsberättelse om Framstegen i Kemi och Mineralogi af Jac. Berzelius. 8vo.

Ofversigt af Kongl. Vetenskap-Akademiens Forhandlingar, 1845. 8vo.—*By the Academy.*

Memoiries de l'Academe Royale des Sciences de l'Institut de France. Tome XIX. 4to.

Memoires presentés par divers savants à l'Academie Royale des Sciences de l'Institut de France. Tome IX.—*By the Institute.*

Observations Météorologiques faites à Nijne-Taquilsk. Année 1845. 8vo.

Voyage dans la Russie Meridionale et la Crimée, par la Hongr Valachie et la Moldavie, executé en 1837, sous la direct M. Anatole de Demidoff. Planches; Liv. 12, fol.—*By the Author.*

Proposed Bridge across the River Clyde for the Glasgow, Paisley, Kilmarnock, and Ayr Railway.

Monday, 19th April 1847.

Dr CHRISTISON, V.P., in the Chair.

The following Communications were read:—

1. On Certain Products of Decomposition of the Fixed Oils in Contact with Sulphur. By Dr T. Anderson.

The investigations contained in this paper were undertaken with the view of ascertaining the nature of the action of sulphur in the free state on organic substances. The author endeavoured, in the first instance, to examine the action of that agent upon some of the simpler organic compounds, but without obtaining any definite results; and finally confined his experiments entirely to the fixed oils.

The distillation of oil of almonds with sulphur, which is attended by the violent evolution of sulphuretted hydrogen, afforded a peculiar nauseous oil, and a crystalline product deposited on cooling from the latter portions of the oil. In order to ascertain the source of these products, comparative experiments were made with pure stearic and oleic acids. It was thus found, that stearic acid, when distilled with sulphur, gave products identical with those obtained by its simple distillation, but that oleic acid gave an oily fluid and crystals similar to those produced from the crude oil.

The crystalline matter obtained from oleic acid, was an acid, and possessed all the properties of margaric acid; but being formed in very small quantity, the author, in obtaining it for analysis, made use of almond oil, expressed at a low temperature, which, by a comparative experiment, was found to yield no margaric acid when distilled alone. The analysis of this acid gave the following results, which correspond with those of margaric acid:—

	Experiment.		Calculated.
C_{34} . .	75·27	75·40	75·55
H_{34} . .	12·51	12·66	12·59
O_4 . .	12·22	11·94	11·86
	100·00	100·00	100·00

The silver-salt gave 28·53 and 28·70 per cent. of silver, the calculated results being 28·65, and the analysis of its ether was likewise found to correspond to margaric ether.

The oil which distilled along with this substance, and which possessed a most disgusting odour, was rectified; the product collected in separate portions, and analysed; but without affording concordant results. It was found, however, to contain a substance capable of giving precipitates with corrosive sublimate and bichloride of platinum.

The precipitate obtained by corrosive sublimate was purified by washing with ether and solution in boiling alcohol, from which it is deposited on cooling. It then forms a white pearly powder, which, under the microscope, presents the appearance of tabular crystals. It is insoluble in water and ether, sparingly soluble in alcohol, and rather more so in coal-naphtha. It gave to analysis the following results, corresponding to the formula $C_{16} H_{16} S_5 Hg_4 Cl_2$.

	Experiment.	Calculated.
C_{16}	14·61	14·46
H_{16}	2·72	2·42
S_5	12·48	12·13
Hg_4	60·01	60·32
Cl_2	10·67	10·67
	100·49	100·00

The author, from the similarity of the properties of this substance to those of the mercury compound of allyl, considered it to possess an analogous constitution, and to be derived from a substance having the formula $C_3 H_8 S_2$, existing in the oil. According to which view its rational formula may be represented by

$$(C_3 H_8 S_2 + Hg_2 Cl_2) + (C_3 H_8 S_2 + Hg_2 S.)$$

When treated with sulphuretted hydrogen it became black, and an oil was separated, having a peculiar odour, and giving precipitates with corrosive sublimate and chloride of platinum. This the author considers to be the compound $C_3 H_8 S_2$; but he was unable to obtain enough for analysis.

The precipitate with bichloride of platinum is yellow, insoluble in water, and sparingly soluble in alcohol and ether. By hydrosulphuret of ammonia it is converted into a brown powder.

The oil from which these substances were separated likewise contained sulphur; but the author had not yet proceeded with its investigation.

2. On the structural relation of Oil and Albumen in the Animal Economy. By Dr J. H. Bennett.

Nitrogenised principles of food are subservient to the formation of albumen, whilst the non-nitrogenised are mostly converted into fat or oil. The fact, that a union of these is essential to nutrition, is explained, according to the chemist, by supposing that albumen constitutes the basis of the tissues, and that oil furnishes the elements of respiration and animal heat. This theory, however, does not explain the origin and maintenance of all growth, which is so essential to the vital functions. The author considered that the necessity of oil and albumen was accounted for by their being both necessary to the formation of the tissues, and he endeavoured to shew that there is no elementary cell into which these principles do not enter as constituent parts.

Dr Ascherson of Berlin shewed, in 1838, that oil could not come in contact with fluid albumen, without the formation of a membrane, and that, on producing an emulsion by rubbing them together, cells were formed composed of an albuminous membrane inclosing oil, which were identical with those found in milk. That the milk globules were not loose particles of oil, the author considered to be proved by the following facts :—1*st*, They float in a fluid, roll freely over each other, and do not unite. 2*dly*, They possess the property of endosmosis and exosmosis. 3*dly*, An excess of ether dissolves them, leaving behind a molecular mass. 4*thly*, Acetic acid dissolves the albuminous envelope, leaving the oil unaffected, when the globules are easily made to unite. 5*thly*, Mechanical means are necessary to extract the butter from milk in the dairy ; the act of churning lacerates the minute envelopes. The author obtained the same results with the globules formed artificially by the union of oil and albumen. On mixing oil with other glutinous substances, however, such as gelatine, gum, and syrup, he could not obtain the haptogen membrane of Ascherson, and he found that the mechanical globules so formed, readily united together when at rest. That a delicate albuminous membrane possesses the property of rolling up and uniting its edges so as to form shut sacs, the author has demonstrated, by lacerating nerve tubes, which may then be seen under the microscope to form globules with double lines. From all these facts it was concluded that the globules of milk, as well as those formed mechanically by the union of oil and albumen, were structures composed of an envelope and contents, and that they were endowed from the moment of their formation with the physical property of endosmosis and exosmosis.

The author then alluded to the elementary molecules, granules, and nuclei found in the blastema of all organised formations, which, he agreed with Ascherson, might be demonstrated to possess a like composition and structure to the globules formerly described. He quoted some recent experiments of Professor Matteucci, which proved that an oily emulsion would pass through a membrane by endosmosis, if the fluid on the other side was slightly alkaline. He noticed also the fact ascertained by Donné, that after the removal of the globules from milk, the remaining fluid contained fat in solution. He thought there would be no difficulty now in recognising that the action of the stomach and intestines was directed to the formation of an emulsion of oil and albumen, which, on passing through the intestinal walls, constituted the basis of chyle, and that the property of endosmosis and exosmosis must be in continual operation in elementary molecules, nuclei, and cells.

The structures found in milk, or produced mechanically by the nion of oil and albumen, are not vital structures, but when formed in the animal body under certain conditions, they become so. The physical relations pointed out are only necessary preliminary steps for the addition of that unknown force called vitality, which directs the ultimate forms these structures assume. They are a *sine qua non*, without which vitality cannot be called into existence. The author thought that these facts, without being capable of explaining the mystery which envelopes the assimilation of organic into organised matter, will constitute another link to the chain of physical actions introductory to its accomplishment. This chain he considered might be composed as follows:—1*st*, Introduction into the stomach and alimentary canal of organic matter. 2*d*, Transformation of this by the *chemical* process of digestion into albuminous and oily compounds. 3*d*, The *physical* imbibition of these, and their union to form elementary granules and cells in the villi and lacteals; and, lastly, the *vital* transformation of these into blood. We observe the same order of changes when exudation takes place from the blood; viz., 1*st*, Exudation of liquor sanguinis, containing oil and albumen in solution. 2*d*, The *mechanical* union of these to form elementary granules and nuclei; and, 3*d*, The vital transformation of these into various tissues.

The author then proceeded to point out various conditions of the animal economy in which the healthy relations of the oily and albuminous principles were more or less deranged.

Sometimes we have general or local collections of fatty matter as in obesity; fatty degeneration of the liver, kidney, and muscles; fatty tumours and the compound granular corpuscles so common in inflammatory softenings, which he considered evidences of local fatty collections. In the same manner we have excessive leanness, and alterations known as cicatrices, indurations, strictures, and fibrous tumours, which are local accumulations of the albuminous compounds. To this class also belong tubercular deposits. The excess of one of these in a tissue leads to atrophy of the other; thus in fatty liver we have excess of the cellular and diminution of the fibrous element, whilst the contrary is the case in cirrhosis. The emunctory organs of these two principles are exposed to like alterations from excess of fat or albumen, and those principles themselves give rise to crystallized products, viz., cholesterine and uric acid, causing obstructive diseases.

Inflammatory diseases in healthy persons give rise to an exudation containing corpuscles, with nuclei and cell walls composed of oil and albumen in certain proportions. These diminish in organisable power as the exudation abounds in albumen or becomes tubercular, and assume an excessive growth and power of reproduction, as it abounds in the oily element, as in soft cancer. Tubercle is generally found in organs destitute of fat, such as the lungs and fibrous membranes, whilst it is rare in fatty organs, such as the brain and liver. On the other hand, cancer is most common in fatty organs, such as the mamma and liver, and is exceedingly rare in the lungs and fibrous tissues. Again tubercle is common in the young, in whom assimilation rarely produces an excess of fat; whereas cancer is most frequent in advanced life, when obesity and fatty accumulations are especially apt to occur. The importance of these facts in regulating the diet of animals, and in a system of therapeutics, must be evident.

3. Experiments on the Ordinary Refraction of Light, by Iceland Spar. By W. Swan, Esq. Communicated by Professor Kelland.

The following Gentlemen were duly elected Ordinary Fellows:—

W. Macdonald Macdonald, Esq., of St Martins.
Robert Handyside, Esq., Advocate.
Alexander Christie, Esq., Surgeon, H. E. I. C. Service.

The following Donations to the Library were announced:—

On the Silurian Rocks of Parts of Sweden. By Sir R. I. Murchison, F.R.S.—*By the Author.*

A Brief Review of the Classification of the Sedimentary Rocks of Cornwall. By Sir R. I. Murchison.—*By the Author.*

Address delivered at the opening of the New Hall of the Royal College of Physicians, Nov. 27, 1846. By William Beilby, M.D., President.—*By the Royal College of Physicians.*

Journal of the Royal Asiatic Society. Vol. X., Part 2.—*By the Society.*

Comptes Rendus Hebdomadaires des Séances de l'Academie des Sciences. Tome XXII., No. 12, to tome XXIV., No. 10.—*By the Academy.*

On the Polarization of the Atmosphere (from Johnston's Physical Atlas). With a Plate, one leaf folio.—*By the Author.*

Drawing Illustrative of a Geological Section on the Caledonian Railway, 2 miles from Edinburgh. By Sir G. S. Mackenzie, Bart.

Monday, May 3, 1847.

VERY REV. PRINCIPAL LEE, V.P., in the Chair.

The following Communications were read:—

1. On the Boulder Formation and Superficial Deposits of Nova Scotia. By J. W. Dawson, Esq. Communicated by Dr Gregory.

In this paper the author, describes 1*st*, the geological position of the gypsum deposits. The gypsum beds of Nova Scotia all belong to the carboniferous system. No gypsum occurs in older formations, and in the overlying new red, only slender veins and small nodules are found. The gypsum characterises the lower members of the carboniferous series, underlying the productive coal measures. The associated rocks are red sandstones and clays, with very few fossil plants, and thick beds of limestone abounding in marine shells. In the only well ascertained instance of gypsum occurring above the coal measures, it is accompanied by limestone with some of the same shells. The gypsum forms regular conformable beds sometimes 100 feet thick. They are much fractured and difficult to trace from the wooded state of the country; but one thick bed was traced by the author for ten miles.

The beds or gypsum often rest on, or are overlaid by, limestone, and these rocks sometimes pass into one another, as at Ogden's Point and Wallace Harbour. In other cases the gypsum is embedded in marly sands and clays, or in reddish grey and purple sandstone; but limestone is never far distant in ascending or descending order.

2*d*, Its characters. The gypsum is remarkably pure and usually white. This purity, according to the author, indicates its chemical origin, as distinguished from the detrital character of the associated beds. It is also highly crystalline, being often large grained and distinct; but usually lamellar; the plates of selenite having occasionally a stellar arrangement. The beds rarely exhibit a fibrous structure, which is chiefly seen in the narrow veins; but they often shew a laminated structure, parallel to the beds.

3*d*, The foreign matters contained in the gypsum. These are, 1, grains of quartzose sand; 2, coaly or bituminous matter. Near the mouth of the Shubenacadie is a bed of black gypsum, included in red sandstone, without a trace of bitumen; 3, crystals and fragments of carbonate of lime, and small grains of magnesian limestone; 4, red oxide of iron, especially in the veins; 5, anhydrite.

The anhydrite is usually associated with the common variety, sometimes forming thick beds, and large rounded masses. It is white or grey, and crystalline, lamellar, or granular. It contains occasionally bituminous matter. It does not occur in veins, but is traversed by veins of gypsum.

The gypsum beds are entirely destitute of fossils.

Many salt springs rise from the lower carboniferous and gypsiferous rocks, but rock-salt has not been discovered.

The lower carboniferous limestones are usually filled with shells and corals; but there are some limestone beds which are granular and crystalline without fossils. These beds agree with those of gypsum in their structure, and in containing bituminous matter.

4*th*, The origin of the gypsum. The author concludes by stating his opinion, that the gypsum beds cannot have been formed *in situ*; but that they may have originated from the action of sulphuric acid, conveyed by the rivers into estuaries of small extent, the waters of which, from the abundance of marine fossils, must have been rich in carbonate of lime. The acid may, he thinks, have been derived from the oxidation of iron pyrites, which is very abundant in the older formations. That free acid may have been present, he considers probable, from the blanched appearance of certain sands and clays adjoining the beds, while the neighbouring beds are strongly coloured by iron. Moreover, as might be expected, on the hypothesis of the decomposition of pyrites, oxide of iron is very abundant, both colouring the rocks very strongly, and in the form of large beds of brown hematite. The oxidation of the pyrites may have been promoted by internal heat, of the action of which on the older rocks there is abundant proof. There is also good evidence that the beds of gypsum were deposited in trough-shaped hollows of small extent; and if we suppose the supply of acid to be intermittent, this would account for the alternation of beds of gypsum, and of shell limestone.

The author considers the regular bedding of the anhydrite and its association with unaltered rocks, to preclude the idea of its being gypsum altered by heat *in situ*; and thinks it may have been first altered by heat, and subsequently deposited at the bottom of the sea, although its great purity is not favourable to this hypothesis.

There was exhibited, in illustration of this paper, a large collection of specimens, presented by the author to the Society.

2. On the mode of occurrence of Gypsum in Nova Scotia, and on its probable origin. By the Same.

In this paper, the author, after a general description of the geological character of the country, describes the superficial deposits which he divides into—

1. *The unstratified drift or boulder formation.* This, the lower of the two superficial deposits, is characterised by the circumstance, that most of the materials have been derived from the rocks on which they now rest, or those in the vicinity. The fragments are angular, and altogether devoid of any regular arrangement. This unstratified drift, however, does contain boulders from distant localities, which may generally be traced. The appearances indicate that the materials have been transported from the northward, and also, to a less extent, from the southward, and, indeed, in various directions.

Polished and scratched surfaces have been observed only in a few localities ; but do not indicate a uniform direction.

2. *Stratified sand and gravel.* This deposit generally rests on the former. Sections were exhibited shewing this in two localities. The pebbles of this gravel are comparatively small. It often forms mounds of singularly regular form, resembling works of art.

After minutely describing various localities, the author proceeds to say that the facts indicate more than one cause of change. 1*st*, He is disposed to consider the contour of the surface on which these deposits rest as the result of powerful submarine currents, occurring probably during the gradual rise of the land. 2*d*, He considers the unassociated state of the unstratified drift, and the small amount of attrition it has undergone as proofs of subaerial disintegration, possibly effected by the frost and thaws of an extreme climate. 3*d*, The great confusion of the fragments, and the presence of foreign boulders indicates, he thinks, a subsequent period of submergence, and the agency of icebergs, transporting these boulders in various directions. Lastly, the mounds and ridges of stratified gravel may have been formed during this period of submergence, or during the gradual rise of the land.

The agencies which have produced these deposits have played a very important part in preparing the surface of the country for agricultural operations ; and this is possibly one of the principal uses which these deposits have been intended to serve.

3. On certain Anomalous Deviations of the Transit Instrument at the Royal Observatory. By Professor C. P. Smyth.

Professor Henderson had found a connection between the changes in the level of the transit axis and the readings of a thermometer which he conceived to shew the temperature of the surface of the rock outside the Observatory; and was accustomed latterly to determine the level error of the axis merely from the indications of the thermometer.

The author has found from Professor Henderson's own observations, a temperature effect on the azimuthal position of the axis four times as strong as in the case of the level.

Indeed the extent was so great as almost to vitiate the observations; for the amount of error varied regularly to such an extent from day to day, as to preclude the possibility of employing the only unexceptionable method of determining the azimuth of the instrument: viz. three consecutive observations of a star above and below the pole.

In order to try to ascertain how heat produced this effect, or what were the parts acted on, six different thermometers were discussed. Five (Professor Forbes's) had their bulbs buried in the rocks 50 feet outside the Observatory, at the depths of 24, 12, 6, 3, and 0· French feet respectively; the last having its bulb merely covered with sand; a sixth thermometer had been observed under the floor of the Observatory, and its indications came between the 0· and the 3 feet instruments.

The deeper the thermometers, the greater was found their difference from the azimuthal errors of the transit; the test consisting in the prominence and similarity of the daily variations, and the degree of retardation of the grand annual wane of heat.

Of the former it was difficult to get a numerical estimate, but of the latter a result was obtained shewing the ratio of the quantity of heat in the first half of the year, to that in the last:—

In the case of the	3 feet Thermometer, the ratio was		1 : 1·941
...	...	Thermometer under floor, .	1 : 1·639
...	...	0· feet, ...	1 : 1·351
...	...	Instrumental errors of Transit,	1 : 1·158

plainly shewing that the parts acted on by temperature were more quickly affected by it than a thin coat of sand, and so could neither be the rocky foundations of the piers, or the massive stone piers themselves; but must be, considering too the effect of the walls and roof covering the instrument, something very small, and most probably metallic.

The field of inquiry being thus curtailed, the brass supports of the axis on the top of the stone piers were examined, and the cause was then supposed to be discovered in a bad principle in the mode in which the adjustment for level was contrived.

The *experimentum crucis* which the author proposes, is to do away with the adjustable bearings, and have plain simple blocks of brass in their stead, trusting to calculations and not to screws for correcting the instrument. But, as the adoption of this proof will take up a long period, he, in the meanwhile, shews, that this cause which he thinks to have discovered is a *vera causa*, is *sufficient*, so far as the quantity is concerned, and is agreeable as to the direction in which it would act: and as a farther test that the disturbing cause is something near the instrument, and not the rocks outside, the readings of the thermometer attached to the barometer, and the outer thermometer read off during the mural circle observations were discussed, and gave the following highly confirmatory results:—

Barometer-thermometer, first half of year: second half: : 1 : 1·220
Outer .... −1 : 1 : 114

4. Results of Makerstoun Observations No. III. On the Solar and Lunar periods of the Magnetic Declination. By J. A. Broun, Esq. Communicated by Sir T. M. Brisbane, Bart.

The absolute westerly declination at Makerstoun, for the mean epoch, June 1844 = 25° 17′·12.

The annual motion of the north end of the needle towards the east = 5′·67.

The annual period of magnetic declination consists of a double oscillation, having nearly the following epochs of maxima and minima.*

A max. Jan. 30. The min. Ap. 30. The max. Sep. 10. A min. Dec. 10.

The author examines Cassini's observations (1783–7). Although they confirm this law to some extent, it is not conceived that they can be trusted for such a determination. The author also verifies his result by grouping a large mass of modern observations. The observations at Washington and Toronto, with other facts, prove that the oscillation is inverted, when the secular motion of the needle has an opposite sign, and Colonel Beaufoy's observations (1817–20) seem to prove, that when the secular motion is zero, the annual period is

* By maximum is always meant an extreme *westerly* position of the north end of the needle, and by minimum an extreme easterly (or rather northerly) position.

a combination of the oscillations for a positive and negative secular motion.

The mean range of the annual period at Makerstoun is 0′·96.

The variation with reference to the *moon's age*, consists of

A principal maximum 2 days after full moon.
A principal minimum 2 days after new moon.

with two secondary maxima and two secondary minima, which are exhibited with considerable regularity in each of the results for 4 years. The range of the mean = 0′·83.

The variations with reference to the *moon's declination* and distance from the earth are less distinct for evident reasons. The former seems to consist of

The maximum about the time of the moon's greatest N. declination.
Minima .... when the moon is on the equator.
A secondary maximum ...... of the moon's greatest S′ declination.

Diurnal periods. 1. *The Sun's hour angles.* In the mean of two years observations (1844–5), the north end of the needle moves nearly 8′ towards the west from 6^h 10 A.M. till 45^m past noon, and as much to the east from the latter epoch till 11^h P.M.; from 11^h P.M. till 2^h 35 A.M., the north end of the needle moves 0′·7 towards the west, returning to the east again about as much by 6^h A.M. The epoch of the greatest westerly declination seems to be connected with its annual period, or with the value of the *mean* westerly declination, and the epoch of the principal mininum, with the sign of the secular change. The author considers that the diurnal oscillation is double at all seasons of the year.

2. *The Moon's hour angles.* The following are the result.

Moon in opposition north of the equator; mean of 13 lunations; oscillation single; range = 0′·8.

Maximum 1^h before inferior transit. Minimum 4^h or 5^h before superior transit.

Moon in opposition south of the equator; mean of 12 lunations; oscillation double; range = 0′·6.

Maximum $2\frac{1}{2}^h$, and minimum 6^h after superior transit. Secondary maximum at inferior transit. Minimum 5^h after it.

Moon in opposition northward, south of equator; mean of 25 lunations; oscillation double; range = 0′·6.

Secondary maximum $4\frac{1}{2}^h$, and minimum 8^h after superior transit. Maximum at superior transit, minimum 6^h after it.

Single lunations verify these epochs very nearly.

3. Notice of two Ores of Copper, one of them a new Mineral. By Professor Connell.

The first of the two ores here described, found in Cornwall, is a new combination of chloride of copper, sulphate of copper, and water. It occurs in beautiful small deep blue acicular crystals, of high lustre, grouped in bundles. The quantity was too small for a quantitative analysis.

The other is essentially a double carbonate of zinc and copper. It is from Matlock. It is pale-green, with a laminated structure and pearly lustre. In the qualitative examination of it, the author observed indications of one, or even of more than one, metallic oxide, which he could not identify satisfactorily with any known substance. This oxide was found to adhere to the copper, when that metal was precipitated by sulphuretted hydrogen. When the sulphuret was dissolved in aqua regia, and precipitated at a boiling heat by potash, the new oxide remained dissolved in the alkali, and the solution yielded on evaporation a small quantity of a soluble salt of a beautiful orange-yellow colour. The solution of this salt, when acidulated, gave, with sulphuretted hydrogen, a red-brown precipitate, which, when dry, was insoluble in muriatic acid, but soluble in aqua regia. On comparison with other known oxides yielding yellow compounds with bases, it appeared to differ from all; but the author had so minute a quantity to operate on, that he cannot pronounce decidedly till he has made further investigation.

The analysis of 3·16 grains of the mineral gave for 100 parts,

Carbonic acid and water, . .	27·5
Oxide of copper, . . .	32·5
Oxide of zinc, ...	42·7
Magnesia, ...	Trace
Lime, ...	Trace
	102·7

This might give the formula, $\left(\left.\frac{Cu}{Zn}\right\} O\right)^2 \; C\,O_2 + H\,O$, that is, 1 atom of dicarbonate of copper aud zinc, combined with 1 atom of water, which gives 27·9 per cent. of carbonic acid and water together ; but the smallness of the quantity analysed, prevented the determination of the relative proportions of carbonic acid and water.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. II. 1847–8. No. 31.

Monday, May 3, 1847.

The following Donations of Books to the Library were announced:—

Memoirs and Proceedings of the Chemical Society. Part 20. 8vo. —*By the Society.*

The American Journal of Science and Arts. Conducted by Professors Silliman and J. D. Dana. For March 1847. 8vo.—*By the Editors.*

On three several Hurricanes of the Atlantic, and their relations to the Northers of Mexico and Central America, with notices of other storms. By W. C. Redfield. 8vo.—*By the Author.*

Proceedings of the American Academy of Arts and Sciences, May 26, 1846. 8vo.—*By the Academy.*

The Fourteenth Annual Report of the Royal Cornwall Polytechnic Society, 1846. 8vo.—*By the Society.*

Magnetical and Meteorological Observations made at the Royal Observatory, Greenwich, in the year 1844. Under the direction of George Biddell Airy, Esq., M.A., Astronomer-Royal. 4to. —*By the Royal Society.*

Sixty-Fifth Session.

First Ordinary Meeting, 6th December 1847.

Sir THOMAS MAKDOUGAL BRISBANE, Bart.,
President, in the Chair.

The following Communications were read:—

1. Biographical Memoir of the late Dr Hope. By Dr Traill.

2. Note on the Constitution of the Phosphates of the Organic Alkalies. By Dr Thomas Anderson.

The author had been led to investigate the phosphates of the organic alkalies, with the view of determining the accuracy of an analysis of the phosphate of strychnia by Regnault, which gave results incompatible with the known constitution of the inorganic phosphates. He alluded to the investigation of the phosphates of aniline by Nicholson, and proceeded to the statement of his own observations.

Phosphate of Strychnia, with one equivalent of Strychnia, was obtained in long truncated needles, by digesting strychnia in tribasic phosphoric acid. It dissolved readily in water, and was acid to test-paper. By analysis it gave results corresponding to the formula

$$(C_{44}\,H_{23}\,N_2\,O_4\,HO)\;2\,HO\;PO_5$$

The crystallized salt was found to contain 4 equivalents of water of crystalization.

Phosphate of Strychnia, with two equivalents of Strychnia. By long-continued digestion of strychnia with the foregoing water in solution, an additional atom of the alkaloid is dissolved, and the solution on cooling deposits rectangular tables of a salt which is neutral to test-paper. It is less soluble in water than the acid phosphate, and its constitution was found to be represented by the formula

$$2\,(C_{44}\,H_{23}\,N_2\,O_4\,HO)\;HO\;PO_5$$

Phosphate of Brucia, with two equivalents of Brucia, is obtained by the solution of Brucia in phosphoric acid, and crystallizes from the concentrated solution in short prisms. The crystals are neutral to test-paper, and contain a large quantity of water of crystallization, which they lose by efflorescence. The formula of the salt is

$$2\,(C_{44}\,H_{23}\,N_2\,O_7\,HO)\;HO\;PO_5$$

A double phosphate of Brucia and soda was also formed, but could not be obtained perfectly pure.

Phosphate of Quinine, with three equivalents of Quinine. By digesting quinine with phosphoric acid, a solution of this salt is obtained, which becomes a solid mass of silky needles on cooling. They are extremely soluble in hot water, and are quite neutral to test-paper. They gave, by analysis, a result corresponding with

$$3\,(C_{20}\,H_{12}\,NO_2\,HO)\,PO_5$$

These results the author considered sufficient to establish the fact, that the phosphate of the organic alkalies agree in their constitution with the inorganic salts of that acid ; and he concluded his paper by observing, that the relations of these bases to phosphoric acid might be made use of as a means of classifying them. Thus quinine, which replaces three equivalents of water in phosphoric acid, might be compared to oxide of lead and the oxides of the heavy metals. Brucia might represent the inorganic alkalies. While strychnia, which, under ordinary circumstances, replaces only one equivalent of water, belongs to a class which has no analogue among the series of inorganic bases.

The following Donations to the Library were announced:

Annals of the Lyceum of Natural History of New York. Vol. IV., No. 67. 8vo.—*By the Society.*

Address delivered at the Anniversary Meeting of the Geological Society of London, on the 19th February 1847. By Leonard Horner, V.P.R.S., President of the Society. 8vo.—*By the Author.*

Proceedings of the American Philosophical Society. Vol. IV., No. 34. 8vo.

Transactions of the American Philosophical Society, held at Philadelphia, for Promoting Useful Knowledge. Vol. IX., Part 3. —*By the Society.*

A Treatise on Atmospheric Phenomena. By Edward Joseph Lowe, Esq., 8vo.—*By the Author.*

Annales de l'Observatoire Royal de Bruxelles. Tom. V.—*By M. A. Quetelet.*

Mémoires Couronnés et Mémoires des Savants Etrangers, par l'Académie Royale de Bruxelles. Tom. IX., 1845 et 1846 ; Tom. XX., 1846, 2 Parties ; Tom. XXI., 1846. 4to.—*By the Academy.*

Nouveaux Mémoires de l'Académie Royale des Sciences et Belles Lettres de Bruxelles. Tom. XIX. 4to.—*By the Academy.*

Mémoires de l'Académie Royale des Sciences, des Lettres, et des Beaux Arts de Belgique. Tom. XX. 8vo.—*By the Academy.*

Bulletin de l'Académie Royale des Sciences, des Lettres, et des Beaux Arts de Belgique. Tom. XIII. (complète) et Tom. XIV., Nos. 1, 2, 3, 4, 5, 6. 8vo.—*By the Academy.*

Bulletin de la Commission Centrale de Satistique de Belgique. Ext. de Tom. III., sur les Anciens Recensements de la Population Belge, par M. Quetelet; et la même livraison, De l'Influence du Libre Arbitre de l'Homme sur les Faits Sociaux. Par M. A. Quetelet. 8vo.—*By the Author.*

Bulletin de la Société Impériale des Naturalistes de Moscou. Nos. 3, 4 (1846); No. 1 (1847). 8vo.

Séance Extraordinaire de la même Société, du 22 Février 1847. 8vo. —*By the Society.*

Berichte Über die Mittheilungen von Freunden der Naturwissenschaften in Wien. Von W. Haidinger. Band I., Nos. 1—6, und Subscriptions Liste. 8vo.—*By the Author.*

Bulletin der Königl. Akademie der Wissenschaften zu Berlin. Nos. 1–77. 8vo.—*By the Academy.*

Gelehrte Anzeigen, von der Akademie der Wissenschaften zu Berlin. Bde. 16–23. 8vo.—*By the Academy.*

Abhandlungen der Mathematisch-Physikalischen Classe der Königl. Bayerischen Akademie der Wissenschaften. 4 Band. 3 Abtheilung. 4to.—*By the Academy.*

Über das Studium der Griechischen und Römischen Alterthümer. Von Ernest von Lasaulx.—*By the Author.*

Über die Ordealien bei den Germanen in ihrem Zusammenhange mit der Religion. Von Georg Phillips. 8vo.—*By the Author.*

Die Überbleibsel der Altægyptischen Menschenrace. Von Dr Franz Pruner. 8vo.—*By the Author.*

Almanach der Akademie der Wissenschaften zu Göttingen, für 1847. 8vo.—*By the Academy.*

Annuaire de l'Académie Royale de Belgique, 1846 & 1847. 8vo.—*By the Academy.*

Annuaire de l'Observatoire Royale de Bruxelles. Par M. Quetelet. 1847.—*By the Author.*

Journal of Agriculture, and Transactions of the Highland and Agricultural Society of Scotland. No. 17 (July) and No. 18 (October) 1847. 8vo.—*By the Society.*

Journal of the Statistical Society of London. Vol. X., Part 3. 8vo. —*By the Society.*

Quarterly Journal of the Geological Society. No. 2 (August). 8vo. —*By the Society.*

Monthly Journal of Medical Science. No. 81 (September). 8vo. —*By the Editor.*

Thirteenth Annual Report of the Royal Cornwall Polytechnic Society. 8vo.—*By the Society.*

Journal of the Royal Asiatic Society. Vol. X., Parts 2 and 3. 8vo. —*By the Society.*

Journal of the Asiatic Society of Bengal. Edited by the Secretary. Nos. 171, 172, 173. 1846. 8vo.—*By the Editor.*

Do. do. Edited by the Secretaries. New Series. Nos. 174, 175, 176, and 181; with Supplementary Number published January 1847. 8vo.—*By the Editors.*

American Journal of Science and Arts. Conducted by Professors Silliman and Dana. Second Series. Nos. 5, 8, 9, 10, 11. 8vo.—*By the Editors.*

Philosophical Transactions of the Royal Society of London. Part 1, 1847. 4to.—*By the Society.*

Kongelike Dánske Videnskabernes Selskabs Naturvidenskabelige og Mathematiske Afhandlinger. Tolvte Deel. 4to.—*By the Royal Society of Sciences of Copenhagen.*

Flora Batava. No. 147. 4to.—*By the King of the Netherlands.*

Handbuch der Mineralogie. Von J. F. L. Hausmann. 2 Theil. 8vo.—*By the Author.*

Astronomische Beobachtungen der Königl. Universitäts-Sternwarte in Königsberg. Von F. W. Bessel. 19 and 21 Abtheilung. Fol.—*By the Author.*

Annales de la Société Royale d'Agriculture de Lyon. Tom. VIII. 1845.—*By the Society.*

Recherches sur les Mouvements de la Planête Herschel. Par M. Le Verrier. 1846.—*By the Author.*

Ankuendigung und Probe einer Neuen Kritischen Ausgabe, und Neuen Uebersetzung der Syrischen Chronik, des Gregor Bar-Hebraeus. Von G. H. Bernstein. 1847. 8vo.—*By the Author.*

Sur la Publication des Monuments de la Géographie. 8vo.—*By the Author.*

Satistique Générale. Rapport au Ministre de l'Intérieur sur les Travaux de la Commission Centrale, et des Commissions Provinciales de Satistique.—*By M. A. Quetelet.*

Rapport sur les Travaux de l'Académie Royale des Sciences et Belles Lettres de Bruxelles, pendant l'année 1842–43. Par A. Quetelet. 8vo.—*By the Author.*

Rapport sur les Travaux et les Titres Scientifiques de M. Duponchel, lu à la Société des Enfants du Nord. 2 copies. 8vo.—*By the Society.*

Bulletin des Séances de la Société Vaudoise des Sciences Naturelles. Nos. 14 & 15. 8vo.—*By the Society.*

Censura Commentationum Soc. Reg. Danicæ Scientiarum, anno 1846, ad præmium reportandum oblatarum.—*By the Society.*

Öfversigt över det Kongelike Dánske Videnskabernes Selskabs Forhandlinger, og dets Medlemmers Arbeider i Aaret, 1846, af H. C. Örsted. 8vo.—*By the Author.*

On the Nucleus of the Animal and Vegetable "Cell." By Martin Barry, M.D. 8vo.—*By the Author.*

Memoirs and Proceedings of the Chemical Society. Part 21. 8vo. —*By the Society.*

On the Origin of Continents. By James D. Dana. 8vo.

Origin of the Grand Outline Features of the Earth. By James D. Dana. 8vo.—*By the Author.*

Greenwich Astronomical Observations. 1845. 4to.—*By the Royal Society.*

Medico-Chirurgical Transactions. Vol. XIII. 8vo.—*By the Editor.*

Everest's Measurement of the Meridional Arc of India. With Plates. 4to.—*By the Directors of the East India Company.*

Results of Astronomical Observations at the Cape of Good Hope. By Sir J. F. W. Herschel, Baronet. 8vo.—*By the Duke of Northumberland.*

Researches for a Remedy against Communism. By Baron Dersenyis. 8vo.—*By the Author.*

Turner's Chemistry. 8th Edition. By Liebig and Gregory. 8vo. —*By the Editors.*

Description and Conquest of Ceylon. By Henry Marshall. 8vo. —*By the Author.*

Elements of General and Pathological Anatomy. Second Edition. By David Craigie, M.D. 8vo.—*By the Author.*

Observations on the Famine of 1846–7 in the Highlands of Scotland and in Ireland. By W. P. Alison, M.D. 8vo.—*By the Author.*

The Acts of the Parliaments of Scotland. Vol. I. (1124–1423.) Fol.

Acta Dominorum Concilii. Oct. 5, 1478 ad Nov. 15, 1495. Fol.

Acta Dominorum Auditorum. Oct. 9, 1466 ad Dec. 16, 1494. Fol.—*By the Lords Commissioners of the Treasury.*

Proceedings of the Royal Society. No. 67, 1846, and No. 68, 1847. 8vo.—*By the Society.*

Memorie della Reale Academia delle Scienze di Torino. Ser Seconda. Tom. III., IV., V., VI. 4to.—*By the Academy.*

Memoirs and Proceedings of the Chemical Society. Part 22. 4to. —*By the Society.*

Journal of the Statistical Society of London. Vol. X., Part 4. 8vo. —*By the Society.*

Observations on the Temple of Serapis. By Ch. Babbage. 8vo.—*By the Author.*

Bulletin de la Société de Géographie. 3ième Série, Tom. VII. 8vo. —*By the Society.*

Etudes d'Astronomie Stellaire. Par M. Struve. 1847. 8vo.—*By the Author.*

Die Cephalopoden des Salzkammergutes, aus der Sammlung Seiner Durchlaucht Fürsten Von Metternich. Von F. R. Von Hauer. 4to.—*By Prince Metternich.*

Journal of the Asiatic Society of Bengal. Nos. 178 and 179, for May and June 1847; and Index to Vol. XV. 8vo.—*By the Society.*

Proceedings of the Zoological Society of London. Nos. 155, 177. 8vo.

Reports of Council and Auditors of the Zoological Society of London, for April 1847. 8vo.

A List of the Fellows, &c., of the Zoological Society of London, for April 1847.

Transactions of the Zoological Society of London. Vol. III., Part 4.—*By the Society.*

A large Collection of the Admiralty Charts of Great Britain.—*By the Lords Commissioners of the Admiralty.*

Monday, 20th December 1847.

Right Rev. BISHOP TERROT, V.P., in the Chair.

The following communication was read:

Examination of some Theories of German Writers, and of Mr Grote, on the Authorship of the Iliad and Odyssey. By Professor Dunbar.

In the first part of the paper the question was examined, whether the art of writing was known and practised at the time in which Homer is supposed to have lived? It was found that there was no evidence, either in the Iliad or Odyssey, that it was practised at that time; but that these poems must have been transmitted orally by the Bards for a period of nearly three centuries. It was then considered whether poems of such a length as the Iliad and Odyssey could have been composed and committed to memory by one man; and it was shewn, from several examples, that there was no impossibility in the matter. Mr Grote's theory, "that no such poet of the name of Homer ever existed," was then examined, and shewn, from the testimony of several of the most eminent Greek authors, to be fallacious. It was stated that lays, containing the history of the ancestors of powerful chiefs, were composed by the Bards attached to their families, and that Homer, in all probability, availed himself of them in working up the Iliad and Odyssey. The mode in which these poems were circulated through Greece by the Nomads, was next pointed out, by their reciting them on public occasions in every part of Greece. It was shewn that they were not committed to writing till a little before the age of Solon and Pisistratus. Wolfe and Lachmann's theories were then examined, and shewn to be altogether fallacious. The opinion of Mr Grote that the Iliad was first an Achilleis, and that the books, including the second and the subsequent ones to the eleventh, were the compositions of a later or later poets, was examined, and it was shewn, by a reference to several incidents in these books, that they must have been composed by the same author, and formed a necessary part of the story of the Iliad. It was stated, contrary to Mr Grote's opinion, that the Iliad possessed more unity than the Odyssey, and that internal evidence proved that it was in all probability composed by the author of the Iliad, and not by a piecing together of the lays of later poets. The opinions of some German critics, that the Odyssey was of a later date than the Iliad, was then examined, and shewn to be well founded. It seemed likely to have been composed in Homer's old age, and bore the same resemblance to the Iliad in point of execution as the Paradise Regained of Milton to that of the Paradise Lost of the same poet. The paper concluded with a quotation from Mr Grote, in which he seemed to have departed from his original opinions.

The following Gentlemen were duly elected Ordinary Fellows :

John Wilson, Esq., F.G.S.
Moses Steven, Esq.

The following Donations to the Library were announced :—

Emploi de l'Airain à défaut du Fer chez la plupart des peuples des cinq parties du monde, &c. Notice intéressant les Peintres d'Histoire et les Archéologiques, Extraite du livre intitulé, Déconvertes dans la Troade. Par A. F. Mauduit. 3 copies. 8vo.—*By the Author.*

Défense de feu Le Chevalier, et du feu Comte de Choiseul Gouffier contre M. P. B. Webb. Par M. Mauduit. 4 copies.—*By the Author.*

Appendices du livre Découvertes dans la Troade, publié en 1840 par M. Mauduit; Défense de Le Chevalier et du Comte Choiseul Gouffier, &c. 4to.—*By the Author.*

Verhandlungen der Schweizerischen Naturforschenden Gesellschaft bei ihrer Versammlung zu Chur. 1844. 4to.—*By the Society.*

Actes de la Société Helvétique des Sciences Naturelles. Trentième Session. 4to.—*By the Society.*

Journal of the Asiatic Society of Bengal; Edited by the Secretaries. September, No. 182. 8vo.—*By the Society.*

Scheikundige Onderzœckingen, gedaan in het Laboratorium der Utrechtsche Hooge-school. 4de Deel, 4de Stuk. 4to.—*By the University.*

Mittheilungen der Naturforschenden Gesellschaft in Bern, aus dem Jahre, 1844–46. Nos. 13–38, 57–86.—*By the Society.*

Bulletin de la Société des Sciences Naturelles de Neuchatel. 1844–45–46. Tom. 1. 8vo.—*By the Society.*

Mémoires de la Société des Sciences Naturelles de Neuchatel. Tom. I., II., III. 4to.—*By the Society.*

Abhandlungen der Königl. Akademie der Wissenschaften zu Berlin, aus dem Jahre 1845. 4to.—*By the Academy.*

Bericht über die zur Bekanntmachung geeigneten Verhandlungen der Königl., Preuss. Akademie der Wissenschaften zu Berlin. Juli—December 1846, und Januar—Juni 1847. 4to.—*By the Academy.*

Bemerkungen über Gyps und Karstenit, von J. F. L. Hausmann. 4to.—*By the Author.*

Nachrichten von der Georg-Augusts-Universität und der Königl. Gesellschaft der Wissenschaften zu Göttingen. 1846.—*By the Society.*

Tradescant der Aeltere 1618 in Russland. Von Dr J. Hamel. 4to.—*By the Author.*

Annuaire Magnétique et Météorologique du Corps des Ingénieurs des Mines de Russie, ou Recueil d'Observations Magnétiques et Météorologiques, par A. T. Kupffer. Année 1844. Nos. 1 & 2.—*By the Editor.*

An Engraving of the late Principal Robertson.—*By John Russell, Esq.*

Monday, 3d January 1848.

RIGHT REV. BISHOP TERROT, V.P., in the Chair.

The following communication was read :—

1. On Algebraical Symbolism. By Bishop Terrot.

The author commenced by proving the propriety of the adoption of the minus sign for quantities of an affection opposite to those affected by the plus sign, and detecting the limits within which this use of the absolute minus was reasonable and effective.

He observed that this was merely a particular case of the notation which symbolizes the inclination of lines by the factor $1^{\frac{\delta}{2\pi}}$, namely, the case where $\delta = 180^\circ$, and proceeded to shew within what limits the algebraical rules for the treatment of exponential quantities are applicable to the symbols of inclined lines.

After referring to the use of this notation in all the problems of plane Trigonometry, especially those which treat of the sines and cosines of multiple arcs, he gave some examples of its applicability to some elementary propositions in the first and fourth books of Euclid.

The following Donations to the Library were announced :—

The American Journal of Science and Arts, conducted by Professors Silliman and Dana. Vol. IV., No. 12. November, 1847. 8vo.—*By the Editors.*

The Journal of Agriculture. N. S., No. 19. January, 1848. 8vo. —*By the Publishers.*

On certain Laws of Cohesive Attraction. By James D. Dana. 8vo.

A General Review of the Geological Effects of the Earth's Cooling from a State of Igneous Fusion. By James D. Dana. 8vo.

Conspectus Crustaceorum. Auctore Jacobo D. Dana. 8vo.—*By the Author.*

Leeds Philosophical and Literary Society. Annual Report. 1846-7. 8vo.—*By the Society.*

The Mathematical Analysis of Logic. By George Boole. 8vo.—*By the Author.*

Königl. Vetenskaps Handlingar, för Ar 1845. Hft. 1 & 2. Stockholm, 1847. 8vo.—*By the Academy.*

Öfversigt af Königl. Vetenskaps-Akademiens Förhandlingar. 1846. Nos. 7–10. 1847. Nos. 1–6. Stockholm. 8vo.—*By the Academy.*

Årsberättelse om Zoologiens Framsteg under Åren 1843–44. Af C. J. Sunderall. Stockholm, 1846. 8vo.—*By the Editor.*

Tal Hället vid Praes. Nedläggande uti Königl. Vetenskaps-Akademien, den 7 April 1841. Af N. G. Sefström. Stockholm, 1846. 8vo.—*By the Editor.*

Berättelse om Framstegen i Fysik, ären 1843 and 1844, afgiven till Königl. Vetenskaps-Akademien, af A. F. Svanbergoch, och P. A. Siljeström. Stockholm, 1847. 8vo.—*By the Editors.*

Memoirs of the Royal Astronomical Society. Vol. XVI. 4to.

Proceedings of the Royal Astronomical Society. Vol. VII., Nos. 1–17. 8vo.—*By the Society.*

Monthly Journal of Medical Science, No 85. January, 1848. 8vo. —*By the Editors.*

Monday, 17th January 1848.

Dr CHRISTISON, Vice-President, in the Chair.

The following Communications were read :—

1. Account of a Geological Examination of the Volcanoes of the Vivarais. By Professor Forbes.

The author having, on former occasions, stated some results of his travels in Auvergne and the Cantal, gives a more detailed description of the volcanoes of the Vivarais, which have been less frequently and less accurately described.

He first gives an account of the journey from Le Puy across the chain of the Cevennes which culminate at the volcanic summit of the Mezeuc, by the course of the Loire, to Montpezat in the department of the Ardêche.

The best descriptions of the Vivarais are those of Foujas de St Fond and Mr Scrope. The plates illustrating the work of the latter leave almost nothing to desire. These authors have described more or less fully the following volcanic orifices—Coupe de Jaujac, Souillolsor Neyvac, Mouleynes or Thuez ; Montpezat and Aysac. Other writers have described the cone of Bauzou, and the (so-called) crater of Elevation of Pal, which are generally supposed not to have given birth to any lava stream. The present author has given a more minute and detailed account of each of these volcanoes, and of the great beds of basaltic lava to which they have respectively given birth. He discusses the relative age, the remarkably columnar structure, and the surprising erosion by water of these (comparatively modern) lava flows, which he illustrates by an exact map of the formations, based upon Cassini's, and by very numerous levels barometrically determined. He has also been able to add to the list of known volcanoes in this district, two craters which he believes never to have been described, occurring in remarkable positions, and giving rise to extensive lava streams, one in the valley of Budzet, the other in that of la Bastide. The former he believes to be unparalleled amongst ancient or modern lavas for the length and slenderness of its stream, shewing a surprising liquidity, which he illustrated by some experiments on the powers of melted iron solidifying in narrow channels.

A series of specimens illustrating the paper had formerly been presented to the Society.

2. Geological Notices. By Dr Fleming.

(1.) *Additional example of Diluvial Scratches on the Rocks in the neighbourhood of Edinburgh.*

The author stated that, recently, an opportunity had presented itself of observing, at a newly-opened sandstone quarry, *dressed and scratched surfaces*, at an elevation above the level of the sea greater than any examples of the same kind of diluvial action as yet recorded, as occurring in the neighbourhood. The locality is eastward of the east Cairn Hill, in the Pentland Hills, at a place termed "Thomson's Walls," and its elevation, according to Knox's map of Mid-Lothian, is 1400 feet.

Dr Fleming then stated, that, last autumn, in addition to the example of a dressed and scratched surface 130 yards westward of Granton Pier, *on a level with the beach*, he had observed a similar occurrence at the east side of the harbour of North Berwick, near the "Auld Kirk," on the surface of a rock of amygdaloid; and added, that he had found similar scratches, at the sea-level, on the south side of Montrose Basin.

The author next adverted to an example of dressed vertical surfaces, with *horizontal* scratches, on the northern base of North Berwick-Law. He likewise referred to the horizontal scratches on a vertical face of rock recently exposed at the Hadderwick Lime Quarries, north from Montrose.

Dr Fleming next called the attention of the Society to the Blackford Hill example of a dressed and scratched surface, and intimated that the scratches had a dip to the eastward, reaching, in some cases, to 50°. He stated it as probable, that the phenomena, instead of having resulted from diluvial action, had been produced by the abrading operations of the Braid Burn.

Verbal Notice.

(2.) *On the Fluor-Spar of Aden.*

Dr Fleming exhibited to the Society several beautiful examples of quartz and calcedony, in the form of cakes or circumscribed stalagmites, from the fort of Aden in the Red Sea, which had been sent to him by Dr Buist of Bombay. They were remarkable as having a few minute crystals of fluoride of calcium, the matter of which aggregated during the evaporation of the water, which had furnished, in greater quantity, the siliceous materials forming the support.

The following Gentleman was duly elected an Ordinary Fellow :—

James Tod, Esq., W.S.

The following Donations to the Library were announced :—

Ordnance Survey. Account of the Measurement of the Lough Foyle Base in Ireland. By Capt. William Yolland. 4to.—*By the Honourable Board of Ordnance.*

Natural History of New York. Botany. By John Torrey. Vol. I., Part 2. Vol. II., Part 2. 4to.

Do Do. Agriculture. By E. Emons. Part 5. 4to. —*By the State of New York.*

Flora Batava. Parts 148 and 149. 4to.—*By the King of the Netherlands.*

Journal of the Asiatic Society of Bengal, No. 183. October, 1847. 8vo.—*By the Secretaries.*

A Collection of Fossil Plants from the Newcastle Coalfield.—*By Sir G. S. Mackenzie, Bart.*

Monday, 7th February 1848.

Sir THOMAS MAKDOUGAL BRISBANE, Bart., President, in the Chair.

The following Communication was read :—

1. On the Preparation of Kreatine, and on the amount of it in the flesh of different Animals. By Dr Gregory.

After some remarks on the present state of animal chemistry, the author commenced by giving a brief account of the recent discoveries of Liebig in regard to the constituents of the "juice of flesh," or the liquid contained in the substance of the muscles, which is distinguished from the blood by the large proportion of free acid it contains. This remarkable animal fluid has been found, by Liebig, to contain phosphoric and lactic acids in large quantity, inosinic acid in small proportion, and some other acids not yet studied; also, potash in large quantity with a little soda, a considerable proportion of magnesia, and a little lime, chloride of potassium, with a little chloride of sodium, and, besides some compounds of animal origin not yet investigated, the new base Kreatinine, and the very remarkable substance, Kreatine, first discovered by Chevreul, but in vain sought for by Berzelius and other chemists.

He then described the process, essentially that of Liebig, by which kreatine is extracted from the flesh of quadrupeds, birds, and fishes, in all of which hitherto tried, it has been found, although in small and variable quantity. A table was exhibited, shewing the percentage obtained from different kinds of flesh and fish, and the result was, that this interesting substance may be most easily and cheaply prepared from fish, especially cod, herring, salmon, and mackerel, all of which yielded much more than beef or horse-flesh, and nearly as much as fowl, which was the most productive. The maximum proportion of kreatine was 3·2 per 1000 parts of flesh. The average about 1·5 per 1000.

The author stated that he had found inosinic acid only in the flesh of fowl and turkey; and he is informed, by Baron Liebig, that it is quite possible that this acid may also have been confined to the flesh of fowls in his experiments, as it was often absent, although he cannot now ascertain the cases in which it was present.

He concluded by stating, that as kreatine is found in the urine, along with kreatinine, it appears to be, in part at least, a substance intended for excretion. Its crystalline character renders this probable; and, at all events, if it has any function to perform in the body, that function is not yet known. It must be regarded, in the mean time, as one of the numerous series of less complex products derived from the decomposition, in the body, of the effete tissues; and although we cannot yet produce it artificially, yet, from the rapid progress recently made in the study of the products of decomposition of the albuminous substances, we may hope soon, not only to do this, but also to discover, from these products, the true formulæ of the albuminous compounds.

2. Notices of a Flood at Frastanz, in the Vorarlberg, in the Autumn of 1846. By William Brown, Esq.

The author noticed the general effects of running water, in dissolving, rubbing down, and transporting to a lower level, the solid parts of the earth's surface; and referred to the gradual change which it is producing on the relative level of sea and land. He then described an occurrence which he had witnessed in the Vorarlberg, during the autumn of 1846.

After a hot and dry summer, a succession of heavy rains for nearly a fortnight, swelled all the streams flowing into the river Ill, flooded the lower grounds, and inflicted a great deal of injury on the fields, roads, and bridges. At Frastanz, a small stream brought down from the mountains an enormous quantity of gravel, which continued for at least three weeks after the rains had ceased. When first seen by him, on the 6th of September, the volume of water in the stream was not very great, nor was its velocity unusual; but immediately beneath the surface of the water, which was quite transparent, innumerable stones were seen to be in motion. These stones were generally of the size of an egg. The quantity of gravel brought down was so great, that the bed of the stream was elevated to the height of 25 feet in one part. The village of Frastanz was considered to be in danger, from this curious torrent of stones rolled along by the water; and several hundreds of men were employed to bank it in by large trees laid lengthwise, and supported by strong posts driven into the ground. In the course of the following year, a wooden canal was formed in the lowest part of the stream, by which about a third of the mass of gravel has been washed down. This has raised the level of the Ill, into which the Frastanz stream flows, for two or three miles. The quantity of loose stones in the upper part of the ravine is still so great as to threaten a renewal of the catastrophe at any time when an unusual flow of water shall set it in motion.

3. Contributions to the Phenomena of the Zodiacal Light. By Professor C. Piazzi Smyth.

The purport of this paper was to place on record certain observations made during the years 1843–4–5, in the southern hemisphere, at those times of the year when the Zodiacal Light cannot be seen in the Northern hemisphere; to test, by means of these new data,—which, besides the novelty of the geographical position, had the further one of being determined by instrumental measurement,—what laws of the phenomena may be considered to have been satisfactorily made out, and what required further elucidation; and to recommend these latter to the attention of observers situated in more favourable parts of the world than those commanded by European Observatories generally.

After discussing the history of the subject, and mentioning the results arrived at by different observers, the author mentions the manner in which his attention was first particularly directed to the subject, describes the particular course of observation which he then commenced, and which consisted principally in observing the right ascension and declination of the apex of the light, by means of a small equatorial instrument of particular construction, which gave results not affected with more than 2° of probable error. Combining his own observations with those of former investigators, the author concludes, that the hypothesis proposed by Cassini, and subsequently maintained by La Place, Schubert, Poisson, Biot, and Humboldt, viz., that the Zodiacal light is in the form of a ring encircling the sun, is decidedly untenable, but that it is rather, as first suggested by Mairan and since affirmed by Olbers and Sir John Herschel, in the form of a lenticular mass. Mairan's idea, too, of the body being excentrically disposed about the sun, being endued with a rotation, and occasionally crossing the earth's orbit, seems to be confirmed. But the exact quantity of such excentricity, the period of rotation, the position of the plane of the body, the question of any actual periodical increase in the size and brightness of the Zodiacal light, and the physical nature of that light, whether entirely reflected, or whether, as rendered probable by some observations, partly direct, are matters, for the satisfactory determination of which more data are required. For the assistance of those who may be inclined to prosecute the inquiry, the author adds descriptions, both verbal and pictorial, of what the Zodiacal light is like, what observers may expect to see; and mentions the times of the year at which, in different latitudes, the phenomenon may be best seen, together with a number of other attendant circumstances which are necessary to be complied with, in order to procure undeniable observations.

The following Donations to the Library were announced:—

Observations made at the Magnetical and Meteorological Observatory at St Helena. Vol. I. 1840–43. 4to.—*By H. M. Government.*

Fusinieri (A.) sulle Ipotesi del Signor Melloni circa il Calore Raggiante. 4to.—*By the Author.*

Astronomical Observations made at the Royal Observatory, Edinburgh. By the late Thomas Henderson. Reduced and Edited by C. P. Smyth. Vol. VII. for 1841. 4to.—*By the Royal Observatory.*

Fellenberg (L. R. de), Fragmens de Recherches comparées sur la Nature constitutive de différentes sortes de Fibrine du Cheval dans l'état Normal et Pathologique. 8vo.

—— Analyse de l'Eau Minérale de Weissenburg. 8vo.

——Ueber die bei der Consolidation des Faserstoffes stattfindenden Veränderungen der elementar-analytischen Bestandtheile desselben. 8vo.—*By the Author.*

Bulletin des Séances de la Société Vaudoise des Sciences Naturelles. No 13. 8vo.—*By the Society.*

Journal of the Royal Geographical Society of London. Vol. XVII., Part 2. 1847. 8vo.—*By the Society.*

The London University Calendar for 1848. 12mo.—*By the University.*

Acta Academiæ Cæsareæ Leopoldino-Carolinæ Naturæ Curiosorum. Vol. XXI., Pars 2. 4to.—*By the Academy.*

Monday, 21*st February* 1848.

Right Rev. BISHOP TERROT, V.P., in the Chair.

The following Communications were read:—

1. Practical Illustration of the Adjustments of the Equatorial Instrument. By Professor C. Piazzi Smyth.

The object of this paper was partly to introduce to the notice of travellers and residents in tropical countries, a small equatorial instrument, specially contrived for observing the Zodiacal light; and partly to bring forward before amateur astronomers, prominently, the advantage of equatorial mountings in general; as well as to exemplify the best and easiest methods of adjusting and rectifying the instruments, and placing the telescopes in every respect in the most favourable circumstances for yielding good results. After describing how, in the history of astronomy, equatorial or parallactic stands were twice taken up and abandoned again, from the erroneous estimate formed of the purposes to which they were adapted, the author mentioned the impracticable nature of the altitude and azimuth mountings which followed; and dated the present era of the perfection and the rational employment of equatorials to have commenced in 1820, when Sir J. Herschel, in conjunction with Sir J. South, erected one of these instruments, to give, by its A. R. and Declination circles, absolute places roughly; and, by means of a micrometer applied to the focus of the telescope, small differences very exactly: the old error having been, to attempt to determine absolute places with the utmost precision.

After particularising the various merits and imperfections of the two grand divisions of equatorials, viz., the English and the German, and mentioning a new construction in progress for the Edinburgh Observatory, combining with the single-pier and short polar axis of the German form, the advantage which the English possesses, of large circles, and a position for the telescope *between* the two bearing ends of the polar axis, together with an exceeding degree of firmness and stiffness,—the author proceeded to describe the six errors of adjustment to which all equatorials are subject, and to show, by means of a model placed within a representation of the celestial sphere, contrived for the purpose, how all the rectifications might be made by means of observations of stars.

The application of clock motion to equatorials, for the purpose of keeping a celestial object stationary in the field, was next entered into; and the plan explained by which the hitherto ungovernable fits of "knocking" of the revolving balls in the later form of English clocks, has been remedied in the case of the Edinburgh Equatorial;—viz., by having *three* pendulum-balls 120° *apart*, attached to the vertical spindle of the governor, instead of only *two* at 180°.

As a proper micrometer to be used for very faint objects, in place of the ring micrometer,—which the author unhesitatingly condemned, as never having furnished accurate results either in A. R. or Decl., but more especially in the latter, in any person's hands, though so strongly recommended, and extensively used, on account of the beauty and truth of one of the theoretical principles involved in it,—he recommended a bar-micrometer, wherein right ascensions were measured by transits across three parallel bars, marking both the immersions and emersions, so as to get rid of error of focus and irradiation of light; and declinations were measured by a bar at right angles to the former, the objects being bisected alternately with either edge of the bar. Observers already provided with position-micrometers might easily have some of these bars inserted, which in one position might be used for A. R., and in another for Decl. Such a micrometer has been found to require no illumination on the darkest nights, even in telescopes of small aperture, and to produce results, with the amorphous masses of faint comets, almost equal in accuracy to those obtained from stars observed with fine wires in an illuminated field.

The paper concluded with a short special description of the Zod. Light-equatorial, which, for economy, lightness, and general effectiveness, seemed well fitted for scientific travellers.

2. On the Vertebral Column, and some Characters that have been overlooked in the Descriptions both of the Anatomist and Zoologist. By Dr Macdonald.

After noticing that the vertebral skeleton has usually been compared to a column, of which the basis (in man) is formed by the sacrum and coccyx, the shaft or columnar part being the bodies of the true vertebræ, as they are usually styled, and surmounted by the splendid composite capital the cranium, the author proposed restricting the observations to the columnar portion, usually divided into 7 cervical, 12 dorsal, and 5 lumbar vertebræ. This division was denounced, and beginning at the summit, he shewed that the upper or cervical region consisted only of 6 vertebræ, as the 7th, in its normal position in the mammal class, had a rib partly articulated to its body, and therefore acquired the character of a dorsal vertebra.

Restricting the cervical to six, the arrangement of the atlas and axis indicates the tendency to a combination into pairs in the course of the vertebral axis. The body of the atlas is almost entirely replaced by the intrusion of the odontoid process of the axis; and thus, by their combined form and articulations, the head resting on them is provided with an equatorial and azimuth motion, as the astronomers say. The pairing of the 1st and 2d osteologically, is further strengthened neurologically, which is also applicable to the next pair of the 3d and 4th. These two pairs are more properly to be considered as the acostal cephalic portion, as the cervical plexus is principally distributed to the upper region of the body, as far as the motosensory part of the system is concerned, although it also contributes to the thoracic and abdominal portions of the nutrient or splanchnic system. The third pair, formed by the 5th and 6th cervical vertebræ, are the acostal constituents of the humero-brachial regions, and with the 7th, 8th, 9th, and 10th, are neurologically connected together in supporting the nerves, forming the brachial plexus as they emerge from the spinal canal. This arrangement completes the cervical region in all the mammals except the *Bradipus tridactylitis;* which is also illustrative of the coupling or pair principle here proposed, as the additional vertebræ only form another pair. The idea of the different classes of vertebræ composing the cervical region, as proposed by De Blainville and Knox, was examined, and shewn to be incomplete, as it considered the 7th vertebra to be a class by itself. As De Blainville's view coincided with that now submitted, in separating the 7th from the upper vertebræ, it was *pro tanto* adduced in evidence; but the most striking corroboration was found in the examination of the skeletons of many of the mammals, several of which were exhibited and demonstrated, where it was shewn that in many, if not all, mammals, the normal position of the head of the rib was opposite the intervertebral space, and that as 12 ribs require 12 spaces, there must be 13 costal vertebræ. This additional thoracic or costal vertebra is provided by the 7, which is only (in man) deprived of its costal connection possibly by the traction of the subclavian artery, which is the remains of one of the primitive reptilöid branchial arches, even here the first rib is occasionally in its normal situation, and when it is found that, in all cases, there are in man eight and generally nine of the ribs in their normal situation, and, also, that an undue share is given to the 8th vertebra, as the whole of the 1st, and part of the 2d, rib is connected to it, we are surely authorised to consider this as the normal position in all mammals. The importance attached to this osteological discovery is, that it corrects an error in the universally assumed character of mammals, which Daubenton and Cuvier first applied, with the sole exception above noticed of the Sloth, which, however, still remains the exception to the number, while it corroborates the principle of coupling or pairing the cervical vertebræ, which is of considerable use in unravelling the cranial vertebræ, and which De Blainville speaks of as still unintelligible " to those who have been unable to elevate themselves to this kind of questions" (the signification of the skeleton transcendentally considered) " *partly* on account of the nature of their minds, and *partly* from the want of proper and sufficient subjects of contemplation."

In this view of the cervical vertebræ, there was no examination of what are known as floating or cervical ribs, first pointed out and described by Vicq d'Azyz in the Memoirs of the Academy of Paris for the year 1774, and which lately, Professor T. Bell of King's College, London, has described in the case of the Bradypus. This class of ribs ought to be regarded as quite different from the thoracic ribs; and there was a beautiful example exhibited on the table, which the kindness of Professor Goodsir enabled the author to shew to the Society, and which forms part of a series collected and described by Dr Knox, in the London Medical Gazette, some years ago. In various classes there are similar ribs, quite unconnected with, and differing from, the thoracic ribs, which are rather homotypes of the styloid process of the temporal bone, and possibly of the lower floating or the 10th, 11th, and 12th, or abdominal ribs. (?) The consideration of these, in the next part of the communication, with the exposition of the cranial vertebræ, will form a subject for a farther communication.

Having demonstrated that the 7th rib is attached to the 7th vertebra in the Mammal class, as in the Monkeys; the Carnivora, as far as examined; the Elephant, Hog, and Horse, among the Pachydermata; the Deer, Elk, Giraffe, Camel, Ox, Sheep, among the Ruminants; and the Dugong, Porpese, and Whale, among the Cetacea,—the only exception being the Seal and Walrus, in the specimens of the Barclay Museum of the Royal College of Surgeons of Edinburgh; and also having assigned a sufficient cause for the abnormal situation of the 1st rib in Man on the 8th vertebra, instead of between the 7th and 8th,—the enumeration of the cervical region will be 6 instead of 7, as hitherto described by all systematic naturalists, who depend on organic structure for the characters of their classification.

The following Donations to the Library were announced :—

An Attempt to discover some of the Laws which govern Animal Torpidity and Hibernation. By Peter A. Browne, LL.D. 8vo.—*By the Author.*

Cambridge and Dublin Mathematical Journal. Nos. 13 and 14. 8vo. —*By the Editor.*

Journal of the Asiatic Society of Bengal. No. 184. 8vo.—*By the Society.*

Journal of the Indian Archipelago and Eastern Seas. Nos. 1, 2, 3. 8vo.—*By the Editor.*

Guyot (A.) Note sur la distribution de Roches dans le Bassin Erratique du Rhone. 8vo.

—— Note sur le Bassin Erratique du Rhin. 8vo.

—— Note sur la Topographie des Alpes Pennines, &c. 8vo.—*By the Author.*

Monday, 6th March 1848.

The Very Rev. PRINCIPAL LEE, V.P., in the Chair.

The Chairman, after a brief account of the Keith Foundation, presented to General Sir Thomas M. Brisbane, Bart., G.C.B., President of the Society, the Keith Prize Medal, awarded to him by the Council, for the Makerstoun Observations on Magnetic Phenomena, made at his expense, and published in the Transactions of the Society.

The Chairman also announced that the Council had also awarded, independently of the Keith Prize, a Silver Medal to J. A. Broun, Esq., in token of their sense of his merits, in conducting and superintending the Makerstoun Observations.

The following Communication was then read :—

On the Theory of the Parallel Roads of Lochaber. By James Thomson, Esq. jun., Glasgow. Communicated by Professor Forbes.

The author, after briefly stating the views of Mr Milne, and the remarks of Sir G. S. Mackenzie, gave his reasons for agreeing with the former, that the terraces were the beaches of lakes, formed by barriers across the valleys; and, with the latter, in holding that these barriers could not have been formed of earthy detritus. He then proceeded to shew that the theory of Agassiz, according to which the barriers were formed of glaciers, was the most probable yet advanced, and while it required some modification in the details to render it consistent with recently observed facts, was strongly supported by the researches of Professor Forbes, both in regard to the former existence of glaciers in our latitudes, as demonstrated in the case of the Cuchullin Hills, and in regard to the laws of the motion of glaciers, as developed in Professor Forbes's papers on the Glaciers of the Alps.

He pointed out that all the difficulties of the theory of earthy barriers were connected with the notion of their being composed of earthy detritus, and that both Sir T. D. Lauder and Mr Milne admitted the great difficulty of accounting for their disappearance.

He then explained the modifications which were required to render the theory of Agassiz capable of explaining all the facts hitherto observed.

The highest shelf in Glen Roy stops short just above the opening into Glen Glaster, and this would have been the result had the barrier which formed that shelf blocked up Glen Roy above the latter glen, and thus forced the water to be discharged by the water-shed at the head of the valley of the Spey. It must also have blocked up Glen Collarig nearly to the Gap. To form the middle shelf, this barrier had only to retire a little, so as to open up Glen Glaster, when the water would discharge itself by the ancient river-course leading from the water-shed in Glen Glaster, first pointed out by Mr Milne.

The lowest shelf would be formed when the glacier retired to near the mouth of Glen Spean.

The blockage of Glen Gluoy seems to have been unconnected with that of the other glens. The author ascribes it to a glacier occupying the site of Loch Lochy, and fed from the high mountain to the north. He explains the occurrence of a lower shelf in Glen Gluoy, which stops short of the mouth of the glen, by a reference to analogous phenomena observed by Professor Forbes in the Lac de Combal.

The shelf in a glen near Kilfinnan observed by Mr Darwin, is accounted for by the glacier supposed to have occupied the site of Loch Lochy.

Mr Milne objects, to the notion of a glacier descending from Ben-Nevis, and crossing Glen Spean to block up Glen Roy, that the inequalities of the intervening ground are so great as to render the existence of a glacier in this direction highly improbable, more especially as the ice had a comparatively easy outlet northward towards Fort-William.

The author, however, endeavours to shew, that, if we assume a climate intermediate between that which produces the glaciers of the Alps, and that which forms the glaciers of the arctic and antarctic regions, there is no real difficulty in imagining the existence of a great expanse of ice descending from Ben-Nevis, at a level considerably higher than that of the intervening hills, as well as of the highest shelf in Glen Roy.

That such a climate may very probably have existed, the author considers as proved by the researches of Professor Forbes among the Cuchullin Hills, the elevation of which is much less than that of Ben-Nevis.

There is, in the phenomena of the great erratic blocks of the Alps, proof of the former prodigious horizontal extension of glaciers, although, in the existing climate of the Alps, the glaciers no longer exhibit the same horizontal development. The author also referred to the indications of glaciers found in many parts of Great Britain, some of them in the Lochaber district, to the occurrence of organic remains of an arctic character, and to the marks of the supposed action of icebergs, as supporting the view of the existence of a glacial climate at some remote period.

The diluvial theory of Sir G. S. Mackenzie was briefly examined, and certain objections urged against it.

The author also alluded to the objection urged by Mr Lyell to the glacial theory, on the score of the changes of relative level on sea and land; and denied that there was any evidence of such changes having occurred since the termination of the supposed glacial period.

The paper was illustrated by a large map of the district of Lochaber, enlarged from that of Sir G. S. Mackenzie.

The following Gentlemen were duly admitted Ordinary Fellows:—

Dr James Allan, Deputy Inspector of Hospitals.
John Hall Maxwell, Esq. of Dargavel.
Thomas Stevenson, Esq., Civil Engineer.

The following Donations to the Library were announced:

Lalande's Catalogue of Stars. 8vo.
Lacaille's Catalogue of Stars. 8vo.—*By the British Association.*
The Journal of Agriculture, and Transactions of the Highland and Agricultural Society of Scotland. No. 20, March 1848. 8vo. —*By the Society.*

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. II. 1847–8. No. 32.

Sixty-Sixth Session.

Monday, 20th March 1848.

Dr CHRISTISON, V.P., in the Chair.

The following Communications were read:—

1. On an Instrument for measuring the extensibility of Elastic Solids. By Professor Forbes.

This instrument is almost a faithful reproduction of S'Gravesande's apparatus described in his "Physices Elementa Mathematica," 1742 (but not in the previous editions). It is described or alluded to by few modern writers, except Biot in his "Traité de Physique." It consists of a strong wooden table or frame, with a vice at each end, between which a wire or lamina may be stretched with a determinate tension by means of a weight attached by a cord, passing over a pulley in the manner of the musical apparatus, called a Monochord. After the tension is adjusted both vices are screwed fast, the space included between them being exactly 50 inches. If now, any deviation of the middle point of the wire included by the vices be made (similar to the action of sounding a harp-string), the force required to pull it a certain distance aside will depend, 1*st*, on the length of the wire; 2*d*, on its tension; 3*d*, on its extensibility, or the modulus of elasticity.

S'Gravesande employed his apparatus to verify Hooke's law, that the extension is as the extending force within the limits of perfect elasticity. But it does not seem to have occurred to him, nor (singularly enough) to later experimenters, to deduce from the forces required to produce given deviations, the specific extensibility, or what Dr Young calls the *Modulus of Elasticity* of the body.

It is essential that the deviation from the rectilinear position of the wire should be ascertained with great nicety, and S'Gravesande's contrivance effects this in a very neat and satisfactory way. A fine steel chain attaches by a hook to the middle point of the extended wire, the other end being secured to the circumference of a nicely-centred wheel. Another chain attached similarly to the wire of the same wheel, has a scale attached to it, a weight placed in which causes the wheel to revolve, and by means of the first chain and hook, pulls the wire out of the straight line. A long index fixed to the same axis with the wheel, points out the deviation on a much magnified scale, referred to a divided semicircle of brass. Thus a weight being placed in the scale, the corresponding deviation is instantly shewn.

Let P be the weight in the scale, D the deviation of the wire, s = *half* the length of the wire between the vices; it was proved in this communication that,

$$P = 2\,T\,\frac{D}{s} + M\left(\frac{D}{s}\right)^3$$

where M is the *modulus of elasticity*, measured in grains, which is easily reduced to the equivalent length of a similar wire or lamina, according to Dr Young's definition. This is on the supposition that Hooke's Law of Elasticity (the extension is as the extending force) is correct; and that law is verified if the term $M\left(\frac{D}{s}\right)^3$ be found for the same wire practically to vary as the cube of the deviation. The value of M, the modulus, is also at once given by a single observed deviation, the tension being known.

A small correction in the value of P is to be made, for the weight of the wire deflecting it from a straight line. This small correction had not escaped the notice of S'Gravesande when he verified Hooke's law.

Example.—A steel pianoforte wire, tension 50,000 grains = T; s = 25 inches.

Values of D.	Values of P.	$2\,T\,\frac{D}{s}$	$M\left(\frac{D}{s}\right)^3$
·25 inch	1100 grains	1000	100
·50 ...	2720 ...	2000	720
·75 ...	5400 ...	3000	2400

The numbers in the last column should vary as the cubes of those in the first, or be as 1, 8, 27. If we deduct 10 grains from each of them for the action of the weight of the wire depressing itself, we shall have these numbers—

90	710	2390

dividing respectively by 1, 8, and 27,

90	89	88·5

Hence the mean result for $M\left(\frac{D}{s}\right)^3$ for $D = \cdot 25$, or $\frac{D}{s} = \frac{1}{100}$, is nearly 89 grains, consequently,

$$\frac{M}{(100)^3} = 89$$

and $M = 89{,}000{,}000$ grains.

But a foot in length of the wire in question weighs 11 grains. The equivalent modulus of elasticity is therefore very nearly 8,000,000 feet of the wire in question, which agrees closely with the received numbers for steel-wire.

2. On the Anthracite of the Calton Hill. By Dr Fleming.

The occurrence of anthracite in this locality has been noticed and recorded by several observers, who seem to have overlooked some of its more remarkable peculiarities. At present, this mineral may be detected in a series of anastomosing vertical veins of calcareous spar, including, at the same time, angular portions of the claystone porphyry in which they are situate, on the upper north walk on the north side of the Observatory. The anthracite occurs, 1. In rounded, drop-like pieces, on which the calcareous spar has been moulded, not unlike the colophonite of Norway. 2. In masses with plane surfaces, apparently produced by the laminæ of the surrounding spar. 3. In angular masses, with sharp edges, and a conchoidal fractured surface. The drop-like pieces must have been solid previous to the solidification of the spar, while the flattened masses have yielded to the pressure of its crystallization. The angular masses, however, which are the most numerous, occur crowded together, of different sizes, while the neighbouring fragments unequivocally indicate, by their corresponding shape, that they had been broken when in a hard state, and separated in the very spot they now occupy, being suspended in the calcareous parts which afterwards became spar. The author exhibited analogous examples of fractures and separations in the Beryl and Tourmaline.

The author next adverted to the opinion, that the Calton was a mass of trap which had burst through the coal formation; and that the anthracite was the altered bitumen, which the rising trap had enveloped in its passage. But, in opposition to these views, although countenanced by Jameson, Cunningham, and Milne, he stated that the Calton, with the exception of its two small *trap dykes*, consisted of a series of sedimentary matters or strata, which had been assorted by water; and that, in position, these strata occupied a place far below the mountain limestone, as is demonstrated by their dip and connection with the group extending under the coal measures from the Castle Hill to Joppa, which he had no hesitation in referring to the old red sandstone.

3. On some Phenomena of Capillary Attraction observed with Chloroform, Bisulphuret of Carbon, and other Liquids. By Dr George Wilson.

The object of this communication was to bring before the Society some phenomena which are exhibited when certain liquids, denser than water, are exposed, while contained in glass or porcelain vessels, to the alternate action of acids and alkalies. The liquids with which the phenomena described were noticed, were chloroform, bisulphuret of carbon, Dutch liquid ($C^4\ H^4\ Cl^2$), bromine, oil of cloves, of sassafras, of cinnamon, and of bitter almonds. The majority of the experiments were made with chloroform, and the phenomena presented in common by all the liquids mentioned were described in full, as presented by chloroform.

When that fluid is dropped into water, contained in a glass or glazed porcelain vessel, it falls to the bottom as a brilliant, highly mobile globule. If potass, soda, or ammonia, be now added, the globule at once collapses, sinks as if pressed down by the alkali, and flattens out on the bottom of the containing vessel. On slightly supersaturating the alkali with an acid, the flattened chloroform recovers, with great rapidity, its globular shape, as if suddenly relieved from pressure.

When the acid in its turn is supersaturated with alkali, the flattening again occurs; and by alternating the addition of these re-agents, the same globule may be successively flattened and rounded for any number of times.

If the experiments referred to be made with quantities of chloroform sufficiently large to touch the walls of the containing vessel on every side, and to form a stratum of some depth, the effect of adding alkaline water is to give the chloroform a surface apparently horizontal, and the addition of acids makes its upper boundary highly convex.

Change in configuration, however, is not the only alteration which the globule of chloroform undergoes. Several of its physical properties are strikingly modified by its contact with aqueous solutions of acids and alkalies. When these are alternately made to act on chloroform at the bottom of a flat white porcelain vessel, which admits of the resulting phenomena being distinctly observed, the reagents in question change the sensible characters of the denser liquids in the following way. Under acidulated water the globules are brilliant, very mobile, and obedient to the solicitation of gravity. Detached globules, when they meet, readily run together, and scarcely one is to be seen without a bubble of air attached to its upper surface, and adhering tenaciously. Under alkaline water, on the other hand, the globules of chloroform spread out into flat discs, with rounded edges; or elongated into irregularly ovoidal or flattened cylindrical forms, which vary their shapes on the slightest impulse. These flattened globules are, moreover, much less mobile than the rounded ones under acid. They move sluggishly, cling to the vessel, and leave a tail behind them when urged to move rapidly. Their brilliancy is sensibly diminished, and no air-bells adhere to their upper surfaces.

Chloroform admits of being coloured by digestion on litmus, alkanet root, &c. &c. Globules of the coloured liquid flatten out greatly more under alkalies than the colourless chloroform does, so that blue or red globules spread over an irregular area five or six times greater than they occupy under acids. Their apparent viscidity, loss of mobility and of brilliancy, are also more marked than when colouring matter is absent.

Phenomena similar to those noticed with the colourless chloroform were observed with the several liquids previously mentioned. The author, in conclusion, declined to enter on the theory of the phenomena described, further than to ascribe an important share in their production to the action of lighter and heavier liquids on each other.

The following Gentlemen were duly elected Ordinary Fellows :—

Henry Davidson, Esq.
Rev. J. Hannah, M.A.

The following Donation to the Library was announced :—

First Report on the Coals suited to the Steam Navy. By Sir Henry De la Beche and Dr Lyon Playfair. Fol.—*By Sir Henry De la Beche.*

Monday, 3*d April* 1848.

Sir THOMAS MAKDOUGALL BRISBANE, Bart., President, in the Chair.

The following Communications were read :—

1. Notice of the Orbit of the Binary Star α Centauri, as recently determined by Captain W. S. Jacob, Bombay Engineers. By Professor C. P. Smyth.

The object of this paper is to point out the extremely important and interesting characteristics of the orbit of the two stars of α Centauri round their centre of gravity, with the object of procuring as many observations as possible from southern observers during the periastral passage in 1851·5.

Professor Henderson and Sir J. Herschel, the only two observers who had specially applied themselves to the subject previously to Captain Jacob, were not able to make any thing of it, with such materials as had been accumulated up to their time. They merely ventured to predict, that a very close affinity of the two stars might be expected about the year 1867 ; and both seemed to consider that the period of revolution was something very great,—that the star was increasing in distance at the time that it was observed by Lacaille in 1751, and by Maskelyne in 1761 ; and that it had occupied the intervening time in reaching its maximum distance, without any sensible change of angle of position.

Sir J. Herschel, in concluding his review of the subject, said, " that no subject more worthy of continued and diligent attention could possibly be urged on the attention of southern astronomers ;" and this has since been most eminently borne out by Captain Jacob's observations, which, beginning about the time that Sir John left off (1838), have been continued up to the present year. All the conclusions, indeed, ventured on by former authorities have turned out erroneous; but the close approximation to the truth now obtained exhibits far more interesting features than were ever expected. At the epochs of Lacaille and Maskelyne, the distance was on the decrease, instead of the increase; and the stars were seen in almost exactly the same relative position by the latter observer in 1761, as by Sir J. Herschel in 1838; and in the interval they had, instead of merely gaining the aphastre without sensible change of angle of position, really made a whole revolution, and altered that angle by 360°. The period is about 77 years; the mass three-fourths of the solar; the greatest distance 22″·5, the least distance 0″·5; and the periastre takes place in 1851·5, when, on account of the excessive excentricity of the orbit, the change of angle of position will be actually 2° 40′ per day.

That will, therefore, be a most crucial period for testing the theory by observations of the facts; and affords the very best and strongest instance for it of all the double stars yet discovered; in addition to which, the accurate determination of the parallax of the star by Professor Henderson gives it a crowning importance.

2. On the Colouring Matter of the *Morinda citrifolia*. By Dr Thomas Anderson.

The substance examined by the author was imported into Glasgow from Bombay, under the name of sooranjee, as a substitute for madder, but had been found useless by the dyers. No information could be obtained regarding its botanical origin in this country, but the importers having written to their correspondents received a quantity of seeds, labelled, Seeds of the Sooranjee plant, *Morinda citrifolia*. These seeds did not germinate, but agreed in their characters with those of *M. citrifolia*; and, for reasons stated at length in the paper, are considered by Dr Balfour to belong to that plant.

The colouring matter to which the author applies the name of morindine, was extracted from the bark of the root by boiling alcohol, and purified by successive crystallizations, and finally by solution in boiling spirit, acidulated with hydrochloric acid, which was found necessary for the separation of the last traces of ash. It then was in the form of minute acicular crystals of a fine yellow colour.

and satiny lustre, sparingly soluble in water, and in cold alcohol, but much more so in boiling dilute spirit, from which it is deposited on cooling, and insoluble in ether. It dissolves in alkalies with a fine red, and in sulphuric acid with a violet colour, and is decomposed by heat with the production of a crystalline sublimate. The analysis gave the following results :—

Carbon, .	55·46	55·40	55·39
Hydrogen, .	5·19	5·03	
Oxygen, .	39·35	39·57	
	100·00	100·00	

From which the author deduces the formula $C_{28}\ H_{15}\ O_{15}$, which differs from that of sublimed madder purple, the probable formula of which is $C_{28}\ H_{16}\ O_{16}$, by a single equivalent of water only. It approaches very closely in many chemical characters to the madder colouring matters, but differs from them in its relations as a dye. For the author had found that it was incapable of producing colours with alum and iron mordants, but with turkey-red mordant it produces a dark red.

By the sublimation of morindine, the author obtained another substance, to which he gives the name of morindone, in the form of fine red needles of considerable length, and which is insoluble in water, both hot and cold, but soluble in alcohol and ether. It dissolves also in alkalies with a magnificent purple colour, and in strong sulphuric acid, with the same colour. The author had been able to make only an imperfect analysis of this substance, the results of which approximated to the formula $C_{28}\ H_{10}\ O_{10}$, and should this be confirmed, would differ only from madder red by a single equivalent of water, and be a polymeric of gentianine.

The author concluded by remarking that morindine formed the type of a new class of colouring matters, fixing only on turkey-red mordant, which would, in all probability, throw some light on the obscure subject of the theory of turkey-red dyeing.

3. An Attempt to improve the present Methods of determining the Strength and Direction of the Wind at Sea. By Professor C. P. Smyth.

The laxity of the present methods having been brought, by his friend Captain Cockburn, R.N., before the author, with a desire to be furnished with some sort of anemometer, and with some easy means of eliminating the effect of the motion of the vessel, he began to consider what would be the *most appropriate* form of anemometer to be used at sea; for several kinds had been already tried, but had failed, as he thought, from not being constructed on a suitable principle. The species which the author considered the best, was that which should imitate, as nearly as possible, *mutatis mutandis*, the log-line by means of which the ship's way through the water is determined; for that instrument seems to have preserved its situation and supremacy over all others on board-ship amongst all nations, and from the time of Columbus to the present, mainly on account of the appropriateness of the principle involved. After describing several means by which the principle might be imitated to different degrees, the preference was given to Mr Edgeworth's anemometer, in which a horizontal wheel, armed with hemispheres on the end of each spoke, revolves in the *same direction*, from *whatever quarter* the wind may blow; and the centre of each cup moves at one-third the velocity of the current, by reason of the greater force of the wind on the concave than on the convex side. A series of experiments was entered on to determine the best shape and size to give the machine in practice, and the result at length arrived at was exhibited on the table, in the form of an anemometer with four horizontally revolving arms, on the ends of which were hemispheres, each four inches in diameter, with a radial distance of six inches. An endless screw on the vertical axis of this revolving part gave motion to a train of wheels which served to count the number of revolutions made in a given time. A weight of 1½ grains in the centre of one of the cups, was found sufficient to overcome the resistance to motion.

Some experiments were described which seemed to shew that the instrument could be fully depended on, and that the *strength* or *velocity* of the wind at sea might now be always entered in the log-book, as being of so many knots per hour, instead of in the usual unmeaning manner; and as the vanes actually used in ships give the *direction* of the wind with sufficient accuracy, all the elements necessary for eliminating the effect of the motion of the ships, or for deducing the true wind from the apparent one, may be assumed as being attainable, but the description of the practical method proposed for adoption was deferred for the next Meeting.

The author next described the problem of determining the true wind from the observed motion of the vessel and the apparent wind, as being merely a special instance of the general theorem of the parallelogram of forces, the course of the ship being one side; the apparent wind, the diagonal; and the true wind, another side; so that the case might always be reduced to the calculation, according to the usual rules for plane triangles, of the third side and one of the angles of a triangle, from the two other sides of their included angle, excepting in the simpler conditions of going exactly with or against the wind.

But as the calculations in this way, though easy enough, threatened to be uselessly and overpoweringly burdensome, considering the enormous number of cases which would have to be computed in any voyage, the small degree of accuracy required, and the untoward nature, for trigonometrical calculations, of the data as observed for the ordinary purposes of navigation,—the author contrived a simple set of scales, in which, *entering* with the directions and velocities of the ship and apparent wind, as usually observed, the direction and velocity of the true wind are given at once by inspection; and might be inserted, with very little trouble to naval officers, in two appropriate columns introduced into the log-books, as kept at present.

The following Donations to the Library were announced:—

First Report on the Coals suited to the Steam Navy. By Sir Henry De la Beche and Dr Lyon Playfair. 8vo.—*By Sir Henry De la Beche.*

The American Journal of Science and Arts. Conducted by Professors Silliman and Dana. Second Series. January 1848, No. 13. 8vo.—*By the Editor.*

Bouet-Villaumez (Le Comte E.) Description Nautique de l'Afrique Occidentale. 8vo.

Petit-Thouars (Abel du). Voyage auteur du Monde sur la Frégate la Vénus. Tom. VI., VII., VIII., IX., X. (Physique, par U. de Tessan. Tom. I., II., III., IV., V.) 8vo.

—— Atlas Hydrographique du même. Imp. Fol.

Collection des Cartes des Côtes de France. Imp. Fol. (In sheets.)

Beyat (P.) Traité de Géodésie a l'usage des Marins. 8vo.

—— Exposé des Opérations Géodésiques. 1839. 4to.

Beyat. Le même. 1844. 4to.

Jehenne (M.) Renseignements Nautiques sur Nossi-Bé, Nossi-Mitsiou, Bavatoubé, &c. 8vo.

Urville (J. Dumont d') Voyage au Pole sud, et dans l'Océanie, sur les Corvettes L'Astrolabe et la Zelée. 8vo.

Daussy (M.) Nouvelle Méthode pour calculer la Marche des Chronomètres. 8vo.

—— Table des Positions Géographiques des principaux lieux du Globe. 8vo.

Brossay (M. Chiron du). Instructions Nautiques sur l'Attérrage et la Navigation de la Platte. 8vo.

Maucroix (M. D'Estremont de). Note sur le Banc de Feroë. 8vo.

Moutravel (L. Tardy de). Instructions pour naviguer sur la Côte Septentrionale du Brésil et dans le fleuve des Amazones. 8vo.

Condé (M. de Maussion). Notice sur le Golfe de Honduras. 8vo.

Bourdieu (L. du). Notes sur quelques Ports de l'Ile de Haïti. 8vo.

Keller (F. A. E.) Des Ouragans, Tornados, Typhons, et Tempêtes. 8vo.

Périer (M. du). Notes sur l'Attérrissage du Rio de la Plata. 8vo.

Kerhallet (Charles P. de). Instruction pour remonter la Côte du Brésil. 8vo.

Jehenne (M.) Renseignements Nautiques sur l'Ile Mayotte. 8vo.

Lartigue (M.) Exposition du Système des Vents. 8vo.

Pagel (Louis). La Latitude par les Hauteurs hors de Méridien. 8vo.

Presented by the French Government, Marine Department.

Spittal (Robert), M.D., Introductory Discourse on Pathology and the Practice of Medicine. 12mo.—*By the Author.*

Journal of the Statistical Society of London. Vol. II. Part 1. March 1848. 8vo.—*By the Society.*

Monday, 17th April 1848.

Dr CHRISTISON, V.P., in the Chair.

The following Communications were read:—

1. On the Action of the Dry Gases on Organic Colouring Matters, and its relation to the Theory of Bleaching. By Dr George Wilson.

This communication is divided into six sections. In the first, the author states that the object of his paper is to supply a defect in the theory of chlorine-bleaching, by endeavouring to explain why the removal of water from that gas arrests its bleaching action. In the preliminary discussion, he refers at some length to Davy's theory, that moist chlorine does not bleach directly, but only in a secondary way, by combining with the hydrogen of the associated water, and liberating the oxygen, which is the true bleacher. After pointing out the untenable assumptions and self-destructive arguments on which this theory is built, the author proceeds in the second section, which discusses the influence of sunlight on the bleaching action of dry chlorine, to shew, that Davy's proposition that dry chlorine does not bleach dry organic colours is true, provided direct sunlight be excluded, but does not apply to the gas when exposed to the actinic influence of the sunbeam. In a comparative trial, one specimen of dry litmus paper was found to resist the decolorizing action of dry chlorine for more than eight months when kept in darkness; whilst the colour of another portion of the same paper totally disappeared after six weeks' exposure to sunshine. Another comparative experiment was not so successful as regarded rapidity of actinic bleaching; but both sets of trials led the author to infer, that darkness as well as dryness is essential to the negative action of chlorine on colours.

The third section is occupied with the record of experiments, instituted with a view to determine whether the presence of water is as essential to the bleaching action of oxygen, sulphurous acid, and sulphuretted hydrogen, as it is to that of chlorine. The general result of the trials made was, that the gases mentioned may be retained, when dry, for months over dry litmus, without decolorizing it; and that they are, therefore, at least as much dependent as chlorine on water for their power of bleaching.

The fourth section investigates, in like manner, the extent to which the acid gases and ammonia have their power to change organic colouring matters, influenced by the absence of water from the gas and the colour. The author finds that the modifying action of carbonic, sulphurous, and hydrosulphuric acids on colours, is totally arrested by the abstraction of water; and that that of hydrochloric acid and ammonia is long delayed. He infers, from the results detailed in sections third and fourth, that there is nothing exceptional or anomalous in the non-bleaching action of dry chlorine, and that it is only a particular case of a general law, applying to all gases which affect colours, and teaching that elastic fluids, when anhydrous, lose in whole, or in part, the power to destroy or change the tints of organic bodies, which they possess when associated with water.

Section fifth reviews the methods employed for drying gases, and the tests of gaseous dryness. It enters at length into the question, how far it is possible to confer absolute dryness on an elastic fluid, and suggests some modifications of the processes at present in use, which the author thinks will prove serviceable. Non-action on colouring matter is likewise pointed out as a negative test of dryness of some value, in relation to the gases which act on colours.

Section sixth is devoted to the question, Does water accelerate the action of gases on colours, in virtue simply of its conferring mediate liquidity on the gas? The author thinks not, and refers to the slow action which he has observed of liquid anhydrous bromine and sulphurous acid on blue litmus, as contrasted with their rapid production of destruction or modification of colour when dissolved in water, as shewing that the liquefaction of the gas is not the only cause of its rapid action when moist. He contends that this is only to be fully accounted for by taking into consideration the power of water to liquefy *both* the colouring matter and the gas, and thus to bring them into a closeness of physical contact in the highest degree favourable to energetic chemical action. The author was led, in connection with this view, to infer that dry gases would act on dry colours in other liquids besides water, provided only these could dissolve both the gas and the colouring matter. He finds, however, that this cannot be laid down as a general proposition, at least so far as chlorine is concerned, the only gas on which he has had opportunity to make researches in reference to this point. The volatile oils of the type of spirit of turpentine (C 5, H 4) and chloroform, which contain no oxygen, and sulphuret of carbon, which contains neither of the elements of water, dissolve the colouring principle of alkanet root, and also chlorine, but the gas does not destroy the colour. Solutions of dry litmus, on the other hand, in chloroform and sulphuret of carbon, are instantly bleached by dry chlorine.

The author's final conclusion is, that the function of water in bleaching, speaking generally, is to dissolve the colour and the gas, and so to bring them within the sphere of chemical affinity; and that water is more efficacious in accelerating bleaching than other liquids, simply because it excels most of them in solvent power.

2. On the Products of the Destructive Distillation of Animal Substances, Part I. By Dr Thomas Anderson.

In this communication the author details the general properties of bone-oil, the substance employed in his experiments, and those of certain of the volatile bases contained in it.

The oil was first rectified, and the product collected in two separate portions, each of which was separately agitated with dilute sulphuric acid for the separation of the bases. The acid solution so obtained was then boiled down to a small bulk for the purpose of separating any non-basic oil which might have been dissolved, and distilled with potash, soda, or slaked lime. The bases passed over in solution in water, from which they were separated by means of solid caustic potass; similar processes were performed with both portions of the bone-oil, but in the present paper the author confines himself to the pure volatile portion only.

The oil separated by this process from the more volatile portion was found to be a mixture of at least four or five different bases, which were separated from each other by fractionated distillation.

The most volatile of these, which boils at about 175° Fahr., was present in extremely minute quantity only. For it the author proposes the name of petinine (from πετεινος, volatile.) It is a transparent colourless fluid, highly soluble in water, alcohol, and ether. The smell is pungent, and resembles that of ammonia, but is accompanied by the odour of decaying apples. It gives, with chloride of gold, a pale-yellow precipitate, and with bichloride of platinum and corrosive sublimate, beautiful crystallisable salts, the former resembling iodide of lead, the latter in pearly plates. The analysis of petinine gave results corresponding with the formula $C_8\ H_{10}\ N$, which was confirmed by the constitution of the platinine salt, the formula of which was found to be $C_8\ H_{10}\ N, H\ Cl, Pt.\ Cl_2$. The author details, as far as the minute quantity at his disposal would allow, the properties of the salts of this base.

He then takes up the consideration of that portion of the mixed basis, which distilled between 270° and 280°, which, after successive rectifications, was found to give the formula $C_{12}\ H_7\ N$, and to agree in all its properties with the base formerly obtained by the author from coal-tar, and described under the name of picoline. Aniline was also found present in that portion of the mixed tars which distilled above 355°, and was distinguished by its reaction with chloride of lime.

The author also mentions the existence of several other bases, the constitution and properties of which will be described in the second part of his researches.

3. Note respecting the Refractive and Dispersive Power of Chloroform. By Professor Forbes.

From an experiment made in very cloudy weather, and therefore rather unfavourable light, I determined the following indices of refraction for pure chloroform, prepared by Dr George Wilson, of sp. gr. 1·4966.

The measure of the refracting angle of the prism was 39° 41′. References were made to the principal lines of the spectrum, as below: the temperature of the fluid was probably 54°.

Extreme red, ...	$\mu = 1{\cdot}4475$
B (in the red), ...	1·4488
D (in the orange-yellow), . . .	1·451
b (in the green), ...	1·456
F (in the blue), ...	1·457
H (in the violet, being the least refrangible of the two groups so designated), . .	1·463
Extreme violet, ...	1·4675

Hence the refractive index is by no means remarkably great, being nearly that of wax, spermaceti, and several of the essential oils.

The dispersive power, or $\frac{\delta\mu}{\mu-1}$ is equal to ·045, which again agrees nearly with that of the essential oils. The high specific gravity of the body appears to have no marked influence in increasing its action on light.

The following Gentlemen were duly elected Ordinary Fellows:—

Dr Patrick Newbigging.

W. Swan, Esq.

The following Donations to the Library were announced :—

Greenwich Magnetical and Meteorological Observations, 1845. 4to. —*By the Observatory.*

Annales des Sciences Physiques et Naturelles, d'Agriculture et d'Industrie, publiées par la Société Royale d'Agriculture, &c., de Lyon. Tom. IX., 1846. 8vo.—*By the Society.*

Abhandlungen der Königl. Gesellschaft der Wissenschaften zu Göttingen. 3 Bde. 1845-47. 4to.—*By the Society.*

Philosophical Transactions of the Royal Society of London, for 1847. Part II. 4to.

List of Fellows of the Royal Society. 4to.

Proceedings of the Royal Society. 1847, No. 69. 8vo.—*By the Society.*

Proceedings of the American Academy of Arts and Sciences. January 27, 1847—January 4, 1848. 8vo.—*By the Society.*

Handbuch der Mineralogie, von J. F. L. Hausmann. Theil 2. 8vo. —*By the Author.*

Thoughts on the Principles of Taxation, with reference to a Property Tax, and its exceptions. By Charles Babbage, Esq. 8vo.—*By the Author.*

Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. Tom. XXIV., No. 11, to Tom. XXVI., No. 11. 4to.—*By the Academy.*

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. II. 1848–9. No. 33.

SIXTY-SEVENTH SESSION.

Monday, 4th December 1848.

Sir T. MAKDOUGALL BRISBANE, Bart., in the Chair.

The following Communications were read:—

1. Geological Notes on the Valleys of the Rhine and Rhone. By Robert Chambers, F.R.S.E.

The principal part of this paper was devoted to a description of alluvial terraces, as seen along the banks of the Rhine at Bonn, Mainz, and Basle, and in the valley of the Arve, the well-known tributary of the Rhone. These terraces, which rise high above the reach of the present river, yet slope in the same direction. The intermediate hollow or trough in which the river runs, has evidently been cut out of what was at first an entire sloping sheet of detrital matter, filling the valley from side to side up to a certain height. At the lower part of the Arve valley, the detrital matter has been discharged across the valley of the Rhone, so as to form a barrier for the Lake of Geneva.

At Vevay, on this lake, there is a short side valley, containing tiers of sloping terraces, which have been called ancient moraines, but are set down in this paper as deltas or river alluvia, marking the stages of the subsidence of a recipient body of water. The chief of these terraces are respectively about 165 and 442 feet above the present level of the lake.

To illustrate the formation of these terraces and deltas, reference was made to the recently-drained Lake of Lungern, in Unterwalden The inpour of mountain-streamlets into such lakes is over a sloping sheet of detritus, extending from the base of the mountains to the edge of the lake, and passing on in the same direction, below the lake, till, owing to the check of the water, it comes to an abrupt termination, and stoops suddenly down. In the case of the Lake of Lungern, on the water being withdrawn, the stream, no longer received into a body of still water, has begun to cut down its delta, as the Niagara River cuts through the strata on which it runs, until a deep channel is formed all the way back to the mountains, the removed matter being, of course, carried forward into the receded waters of the lake. This the author of the paper regarded as an explanation of the formation of alluvia in valleys, and their subsequent intersection. The detritus was received into a body of water occupying the valley ; on that being withdrawn, the river was allowed to cut down and form a channel for itself in the alluvial sheet. A number of reasons were adduced for believing that, in general cases, this recipient body of water was the sea.

A description was given of an ancient lake-bed in the vale of Chamouni, the barrier of which was the right side moraine of the anciently more extended Glacier des Bois or Mer de Glace. Some notices were appended regarding smoothings of fixed surfaces and zones of erratic blocks in Switzerland, and an endeavour was made to establish the probability of icebergs borne on the sea having been concerned in producing some of those phenomena. It was particularly remarked that the zone of blocks at Monthey was just about the same height above the present level of the sea (1670 feet) as the uppermost of the terraces at Vevay ; while M. Saussure had found traces of running water on Mont Salève, at the height of the celebrated erratics above Neufchâtel.

These facts were not presented in opposition to the doctrine of a former greater extent for the glaciers, which was, on the contrary, admitted, though not to the extent demanded by some previous observers. Different agencies, or the same agencies differently applied, might, the author thought, often lead to nearly similar effects.

2. On the Classification of Colours. By Professor J. D. Forbes.

As the reading of this paper was only commenced at this meeting, the abstract is deferred until the conclusion.

The following Donations to the Library were announced:—

Ancient Sea-Margins. By Robert Chambers, Esq. 8vo.—*By the Author.*

Account of the Skerryvore Lighthouse; with Notes on the Illumination of Lighthouses. By Alan Stevenson, LL.B. 4to.—*By the Author.*

Reduction of Greenwich Lunar Observations, under the superintendence of G. B. Airy, Esq. 1750–1830. Vols. I. & II. 4to. —*By the Observatory.*

Modern Languages; their Historical Development, and claims, as a branch of Academical Study. By Sigismund Wallace, Dr Ph. 12mo.—*By the Author.*

Mémoires de la Société de Physique, et d'Histoire Naturelle de Genève. Tom. XI., 2me Partie. 4to.—*By the Society.*

Scheikundige Onderzoekingen, gedaan in het Laboratorium der Utrechtsche Hoogeschool. 4de Deel, 5de Stuk. 8vo.—*By the University.*

Transactions of the American Philosophical Society. N. S., Vol. X., Part 1. 4to.

Proceedings of the American Philosophical Society. Vol. IV., Nos. 36–39. 8vo.—*By the Society.*

American Journal of Science and Arts. Conducted by Professors Silliman and Dana. 2d Ser., No. 14. 8vo.—*By the Editors.*

Annals of the Lyceum of Natural History of New York. Vol. IV., Nos. 8 and 9. 8vo.—*By the Lyceum.*

Journal of the Asiatic Society of Bengal. Edited by the Secretaries. Nos. 185, 186, 187; (N. S., Nos. 12, 13, 14.) 8vo.—*By the Editors.*

Journal of the Indian Archipelago, and Eastern Asia. Vol. I., Nos. 4–6. Vol. II., Nos. 1 and 2. 8vo.—*By the Editor.*

Quarterly Journal of the Chemical Society of London. Edited by Edmund Ronalds, Ph. D. No. 182. 8vo.

Memoirs and Proceedings of the Chemical Society. Part 23. 8vo. —*By the Society.*

Journal of the Statistical Society of London. Vol. XI., Part 2. May 1848. 8vo.—*By the Society.*

Quarterly Journal of the Geological Society. No. 14. 8vo.—*By the Society.*

Journal of the Royal Geographical Society of London. Vol. XVIII., Part 1. 1848. 8vo.—*By the Society.*

Fifteenth Annual Report of the Royal Cornwall Polytechnic Society. 1847. 8vo.—*By the Society.*

Journal of Agriculture, and Transactions of the Highland and Agricultural Society of Scotland. No. 21, N. S. July 1848. 8vo. —*By the Society.*

Address delivered at the Anniversary Meeting of the Geological Society of London, 18th February 1848. By Sir H. T. De La Bêche. 8vo.—*By the Society.*

Memoirs of the Literary and Philosophical Society of Manchester. N. S., Vol. VIII. 8vo.—*By the Society.*

Histoire des Révolutions de la Philosophie en France. Par le Duc De Caraman. Paris, 1845–8. 3 tom. 8vo.—*By the Author.*

Flora Batava. Parts 144, 145, 146. 4to.—*By the King of Holland.*

Transactions of the Royal Irish Academy. Vol. XXI., Part 2. 4to.—*By the Academy.*

Report of the British Association for the Advancement of Science, for 1847. 8vo.—*By the Association.*

Astronomical Observations made at the Royal Observatory, Greenwich, in the year 1846, under the superintendence of G. B. Airy, Esq. 4to.—*By the Observatory.*

Journal of the Asiatic Society of Bengal. Edited by the Secretaries. N. S., Nos. 15 and 16. 8vo.—*By the Editors.*

Journal of the Royal Asiatic Society of Great Britain and Ireland, 1848. No. 18. 8vo.—*By the Society.*

Geology of the Silurian Rocks in the Valley of the Tweed. By James Nicol. 8vo.—*By the Author.*

Annals of the Lyceum of Natural History of New York. Vol. IV., Nos. 10, 11. 8vo.—*By the Lyceum.*

Journal of the Statistical Society of London. Vol. XI., Part 3. 8vo.—*By the Society.*

Flora Batava. Parts 152 and 153. 4to.—*By the King of Holland.*

Philosophical Transactions, Royal Society of London, 1848. Part 1. 4to.—*By the Society.*

Annales de l'Observatoire Royale de Bruxelles. Tom. VI. 4to.—*By the Observatory.*

Mémoires Couronnés et Mémoires des Savants Etrangers, publiés par l'Académie Royale des Sciences, &c., de Belgique. Tom. XXII. 1848. 4to.

Mémoires de l'Académie Royale des Sciences, &c., de Belgique. Tom. XXI. and XXII. 1848. 4to.

Bulletins de l'Académie Royale des Sciences, &c., de Belgique. Tom. XIV., Part 2; Tom. XV., Part 1. 8vo.

Annuaire de l'Académie Royale des Sciences, &c., de Belgique. 1848. 12mo.—*By the Academy.*

Annuaire de l'Observatoire Royale de Bruxelles. Par A. Quételet. 1848. 12mo.

Catalogue des Livres de la Bibliothèque de l'Observatoire Royale de Bruxelles. 1847. 8vo.—*By the Observatory.*

Bulletins des Séances de la Société Vaudoise des Sciences Naturelles, Nos. 16 and 17. 8vo.—*By the Society.*

Proceedings of the Royal Society, 1847, No. 70. 8vo.—*By the Society.*

Address of the Marquis of Northampton to the Royal Society, June 9, 1848. 8vo.—*By the Society.*

Observations des Phénomènes Périodiques. (Acad. R. de Belgique, Extrait du Tom. XXI., des Mémoires.) 4to.

Du Système Social et des lois qui le régissent, par Ad. Quételet. Paris, 1848. 8vo.

Rapport à le Ministre de l'Interieur, sur l'état et les travaux de l'Observatoire Royale de Bruxelles. 1847. 8vo.—*By M. Quételet.*

Bulletin de la Société Impériale des Naturalistes de Moscou, 1847, No. 2. 8vo.—*By the Society.*

Journal of Agriculture, and Transactions of the Highland and Agricultural Society of Scotland. N. S., No. 22. October 1848. 8vo.—*By the Society.*

Athenæum (London) Rules and Regulations, List of Members, and Donations to the Library, 1846; with Supplement for 1847. 12mo.—*By the Athenæum.*

Proceedings of the American Philosophical Society. Vol. V., No. 40. January—April 1848. 8vo.—*By the Society.*

Journal of the Asiatic Society of Bengal. Edited by the Secretaries. No. 191, June 1848. 8vo.—*By the Society.*

Proceedings of the American Academy of Arts and Sciences. Vol. I. May 1846 to May 1848. 8vo.—*By the Society.*

American Journal of Science and Arts. Conducted by Professors Silliman and Dana. Vol. VI. No. 17. 8vo.—*By the Editors.*

Nieuwe Verhandelingen der Eerste Klasse van het Koninkl. Nederländsche Instituut te Amsterdam. 13e Deel. 4to.

Tijdschrift voor de Wis-en Natuurkundige Wetenschappen, uitgegeven door de Eerste Klasse van het K. Nederländsche Instituut van Wetenschappen. Eerste Deel. 1, 2, 3, Aflevering. 1847–8. 8vo.—*By the Society.*

Journal of the Indian Archipelago and Eastern Asia. Supplement to No. VI. of Vol. I.; Vol. II., Nos. 3, 4, 5, 6, 7, 8. 8vo. —*By the Editors.*

The Ethnological Journal. No. 5. October 1848. 8vo.—*By the Editor.*

Reprint of the Report of the Trustees of the Massachusetts General Hospital, with a History of the Ether Discovery, and Dr Morton's Memoir to the French Academy. Edited by R. H. Dana, jun. 8vo.—*By the Editor.*

Magnetical and Meteorological Observations made at the Royal Observatory, Greenwich, in the year 1846, under the direction of G. B. Airy, Esq. 4to.—*By the Observatory.*

Bulletin des Séances de la Société Vaudoise des Sciences Naturelles. No. 18. 8vo.—*By the Society.*

Proceedings of the Literary and Philosophical Society of Liverpool, during the 36th Session. No. 4. 8vo.—*By the Society.*

Proceedings of the Royal Astronomical Society. Vol. VIII. Supplement, No. 9. 8vo.—*By the Society.*

On the Manufacture of Artificial Stone with a Silica Base. By Frederick Ransome. 8vo.—*By the Author.*

Twenty-Eighth Report of the Leeds Philosophical and Literary Society. 1847–8. 8vo.—*By the Society.*

The Ethnological Journal. No. 6. 8vo.—*By the Editor.*

Astronomical Observations made at the Observatory of Cambridge, by the Reverend James Challis. Vol. XV. 1843. 4to.—*By the Observatory.*

Annuaire Magnétique et Météorologique du Corps des Ingénieurs des Mines, ou Recueil d'Observations Magnétiques et Météorologiques, faites dans l'entendue de l'Empire de Russie. Par A. T. Kupffer. Année 1845. Nos. 1 and 2. 1848. 4to.

Résumés des Observations Météorologiques faites dans l'entendue de l'Empire de Russie. Par A. T. Kupffer. 1er Cahier. 4to.—*By M. Kupffer.*

An Introduction to the Birds of Australia. By John Gould, F.R.S. 8vo.—*By Professor Forbes.*

Journal of the Asiatic Society of Bengal. Edited by the Secretaries. No. 161. May 1848. 8vo.—*By the Society.*

Journal of the Asiatic Society of Bengal. Edited by the Secretaries. No. 193. July 1848. 8vo.—*By the Society.*

Journal of the Statistical Society of London. Vol. XI., Part 4. 8vo.—*By the Society.*

Monday, December 18, 1848.

The Very Rev. Principal LEE, D.D., in the Chair.

The following communications were read :—

1. Description of a Mud-slide at Malta. By A. Milward, Esq. Communicated by the Secretary.

A large quantity of mud, dredged up from the harbour of Valetta, having been deposited on a piece of slightly-inclined ground, and having been subsequently moistened to an excessive extent by rain, and by the overflow of a neighbouring tank, the upper parts began to flow down over the lower and the original boundary, in six distinct streams ; this separation being apparently caused by the difference of resistance and level of the under stratum.

These streams exhibited, to a remarkable degree, all the phenomena of glaciers, and thus tended greatly to confirm the theory which looks upon them not as a solid, or a collection of solid masses, but as a viscous fluid. Curved bands of dark and light mud were seen crossing the streams similarly with Professor Forbes's "dirt-bands" on the glaciers ; and the crevasses had their counterparts in the cracks in the mud.

2. An Attempt to explain the "Dirt-Bands of Glaciers." By A. Milward, Esq.

A second mud-slide having occurred at Malta, with more marked features than the first, Mr Milward was enabled to ascertain that the curved bands of dark and light mud are not only accompanied by a difference of elevation in those parts, but that these wrinkles are the *cause* of that difference of colour; the more watery parts draining from the ridge, leave that rough, and form a smooth alluvium in the hollow.

Applying these facts to the glaciers, Mr Milward supposes that the dirt-bands, *i.e.* the bands of alternate compact and porous ice, must be accompanied by a similar difference of elevation, which, though destroyed by the action of the sun on the lower parts, may yet be visible near the origin of the glaciers in the early spring; and he further supposes that the bands may be formed, in the first instance, by the irregular descent of the nevé in winter and summer, so that they become in fact the annual rings of the glacier, shewing its age and rate of increase.

3. On the rate of Progression of the Himalayan Glaciers. By Lieutenant R. Strachey, Bengal Engineers.

Mr Strachey's letter contained some interesting observations which he has been making on the motions of the glaciers in the Himalayan mountains; and his measures, conducted on the plan of Professor Forbes in the Alps, are the earliest that have been taken in Asia. The Pinduree glacier, on which the observations were made during cold weather, was found to move 3 feet 1 inch in 5 days at the centre, and at the sides about 1 foot 5 inches.

Lieutenant Strachey's former Researches on the Glaciers of the Kumaon Himalaya (published in one of the Indian Journals), have satisfactorily proved the existence of glaciers in lat. 30° 20′, which present in detail all the phenomena of those of Europe.

4. Observations on the preceding Communications, and especially on the cause of the Annual Rings of Glaciers. By Professor Forbes.

Professor Forbes stated that Mr Milward's shrewd suspicion of the bands of ice of different consistence being accompanied also by wrinkles or elevations, had been discovered by himself some years before, at the very place and time pointed out as most likely; and he shewed that, while there is a tendency in a tenacious viscous fluid to produce wrinkles, under pressure capable of effecting detrusion, even where the supply of the fluid is uniform; that this quality is greatly increased when the supply of the fluid is by fits, as it is in fact at the head of this glacier, where the quasi-hydrostatic pressure from behind, combined with the frontal resistance, produces a thickening, or convex lip or wrinkle.

He likewise mentioned the analogous instance of the production of equidistant wrinkles in the sides of railway banks, from mere pressure above; and more particularly in turnings of coarse malleable iron, where, though the detruding force is constantly equal, still detrusion takes place at intervals, forming in the shaving so many wrinkles, by which frontal resistance too, it is thickened, and consequently shortened, similarly with the mechanism of the glacier.

The Astronomer-Royal, Mr Airy, then favoured the meeting with a discourse on the telescopes of Lord Rosse and Mr Lassel, specially pointing out those parts in the progress of construction, in which these two eminent mechanists and opticians differed, and sometimes widely, from each other; though they have both arrived equally, at last, at the same goal of all but perfection.

The following Donations to the Library were announced:—

Die Fortschritte der Physik im Jahre 1846. Dargestellt von der Physikalischen Gesellschaft zu Berlin. 2 Jahrgang. 8vo.—*By the Society.*

Medico-Chirurgical Transactions. Vol. XXXI. 1848. 8vo.—*By the Society.*

Troisième, Cinquième, Sixième, et Septième Mémoires sur l'Induction. Par M. Elie Wartmann. 8vo.—*By the Author.*

The Ethnological Journal. No. 7. Dec. 1, 1848. 8vo.—*By the Editor.*

Tuesday, January 2, 1849.

Dr CHRISTISON in the Chair.

The following Communications were read:—

1. An Account of Carnot's Theory of the Motive Power of Heat,* with Numerical Results deduced from Regnault's Experiments on Steam.† By Professor William Thomson, of Glasgow.

The questions to be resolved by a complete theory of the motive power of heat, are the following:—

I. What is the precise nature of the thermal agency by means of which *mechanical effect* is to be produced, without effects of any other kind?

II. How may the amount of this thermal agency necessary for performing a given quantity of work be estimated?

I. On the nature of Thermal Agency, considered as a Moti\ Power.

The whole theory rests on a principle generally admitted as an axiom, which Carnot expresses in the following terms:‡—

"In our demonstration, we tacitly assume that after a body has experienced a certain number of transformations, if it be brought identically to its primitive physical state as to density, temperature, and molecular consiitution, it must contain the same quantity of heat as that which it initially possessed; or, in other words, we suppose that the quantities of heat lost by the body under one set of operations, are precisely compensated by those which are absorbed in the others. This fact has never been doubted; it has at first been ad-

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* Published in 1824, in a work entitled, "Réflexions sur la Puissance Motrice du Feu," by Mons. S. Carnot. An account of Carnot's theory is also published in the *Journal de l'Ecole Polytechnique*, vol. xiv., 1834, in a paper by Mons. Clapeyron.

† An account of the first part of a series of researches undertaken by Mons. Regnault, by order of the late French Government, for ascertaining the various physical data of importance in the theory of the steam-engine, has been recently published in the *Mémoires de l'Institut*, of which it constitutes the twenty-first volume (1847). The second part of the researches has not yet been published.

‡ The passage quoted in the text is translated from a note to p. 37, in Carnot's Treatise.

mitted without reflection, and afterwards verified in many cases by calorimetrical experiments. To deny it would be to overturn the whole theory of heat, of which it is a fundamental principle. It must be admitted, however, that the chief foundations on which the theory of heat rests would require a most attentive examination. Several experimental facts appear nearly inexplicable in the actual state of this theory."

Since the time when Carnot thus expressed himself, the necessity of a most careful examination of the entire experimental basis of the theory of heat has become more and more urgent. Especially all those assumptions depending on the idea that heat is a *substance* invariable in quantity, not convertible into any other element, and incapable of being *generated* by any physical agency; in fact, the acknowledged principles of latent heat, would require to be tested by a most searching investigation before they ought to be admitted, as they usually are, by almost every one who has worked on the subject, whether in combining the results of experimental researches or in reasoning *à priori*.

The extremely important discoveries recently made by Mr Joule, of Manchester, that heat is evolved in every part of a closed electric conductor, moving in the neighbourhood of a magnet;* and that heat

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* I cannot omit this opportunity of correcting an expression which I made use of in a note published in the Philosophical Magazine (vol. xxxiii., p. 315), in alluding to the *generation* of heat by such operations, which I inadvertently asserted to have been proved by "*known experiments*, adduced by Mr Joule." It is true that the *evolution* of heat in a fixed conductor, through which a galvanic current is sent from any source whatever, has long been known to the scientific world; but it was pointed out by Mr Joule that we cannot infer, from any previously published experimental researches, the actual *generation* of heat when the current originates in electro-magnetic induction, since the question occurs, *Is the heat which is evolved in one part of the closed conductor merely transferred from those parts which are subject to the inducing influence?* Mr Joule, after a most careful experimental investigation, with reference to this question, finds that it must be answered in the negative. (See a paper "On the Calorific Effects of Magneto-Electricity, and on the Mechanical Value of Heat; by J. P. Joule, Esq.;" read before the British Association at Cork, in 1843, and subsequently communicated by the author to the Philosophical Magazine, vol. xxiii., pp. 263, 347, 435.)

Before we can finally conclude that heat is absolutely generated in such operations, it would be necessary to prove that the inducing magnet does not become lower in temperature, and thus give compensation for the heat evolved in the is *generated* by the friction of fluids in motion seem to overturn the opinion commonly held that heat cannot be *generated*, but only produced from a source where it has previously existed, either in a sensible or in a latent condition. In the present state of science, however, no operation is known by which heat can be absorbed into a body, without either elevating its temperature or becoming latent, and producing some alteration in its physical condition; and the fundamental axiom adopted by Carnot may be considered as still the most probable basis for an investigation of the motive power of heat; although this, and with it every other branch of the theory of heat, may ultimately require to be reconstructed on another foundation, when our experimental data are more complete. On this understanding the author of the present paper refers to Carnot's fundamental principle, as if its truth were thoroughly established.

If we consider any case in which mechanical effect is obtained from a thermal origin, by means of the alternate expansions and contractions of any substance whatever, and follow a perfectly rigorous process of reasoning indicated by Carnot, we arrive at the following conclusion, by which the first proposed question is answered:—

The thermal agency by which mechanical effect may be obtained, is the transference of heat from one body to another at a lower temperature.

II. On the measurement of Thermal Agency, considered with reference to its equivalent of mechanical effect.

The criterion of what may be called *a perfect thermo-dynamic engine* is thus stated:—

A perfect thermo-dynamic engine is such, that, whatever amount of mechanical effect it can derive from a given thermal agency, if an equal amount be spent in working it backwaxds, an equal reverse thermal effect will be produced.

Any two perfect engines, however different in their constructions,

conductor. I am not aware that any examination, with reference to the truth of this conjecture, has been instituted; but in the case when the inducing body is a pure electro-magnet (without any iron) the experiments actually performed by Mr Joule render the conclusion probable, that the heat evolved in the wire of the electro-magnet is not affected by the inductive action otherwise than through the reflected influence, which diminishes the strength of its own current.

or in the physical media employed, must derive the same equivalent of mechanical effect from a given thermal agency. Carnot describes a steam-engine and an air-engine, each of which satisfies the criterion laid down above (the construction being however in each case, practically impossible); and he shews how, with certain physical data, with reference to steam in one case, and with reference to air or any gas in the other, the equivalent of mechanical effect, derivable from a given thermal agency, may be calculated. Thus, if M denote the amount of mechanical effect due to the *descent* of H units of heat (or caloric) from a body A at the temperature S, through the medium of a perfect engine of any kind, to a body B at the temperature T, we find, by Carnot's method of reasoning,

$$\mathrm{M} = \mathrm{H}\int_{\mathrm{T}}^{\mathrm{S}} (1-\sigma)\,\frac{\frac{dp}{dt}}{k}\,dt = \mathrm{E}\,p_0 v_0 \int_0^{\mathrm{H}} \int_{\mathrm{T}}^{\mathrm{S}} \frac{1}{v}\frac{dv}{dq}\,dt\,dq$$

In the first expression, deduced by the theory of the steam-engine, p denotes the pressure, σ the density, and k the latent heat of a unit of volume of saturated vapour from any liquid, at the temperature t. In the second, deduced by the theory of the air-engine, E denotes the coefficient of expansion (·00366, if the centigrade scale of the air-thermometer be adopted) of a gas; p_0 the pressure of a given mass of gas when reduced to the freezing point of temperature, and to the volume v_0; p the pressure of the same gaseous mass when occupying the volume v, at the temperature t; q the quantity of heat which must be added to the same mass to raise its temperature from 0 to t, when its volume is at the same time changed from v_0 to v; and $d\,q$ the heat absorbed by the gas when, with its temperature kept at t, its volume is augmented from v to $v + d\,v$.

Hence the mechanical effect to be obtained by the *letting down* of a unit of heat from a body A, to a body B at a lower temperature t, if the interval between their temperatures be an extremely small quantity τ, will be, according to the first expression:

$$(1-\sigma)\,\frac{\frac{dp}{dt}}{k}\,\tau,$$

and, according to the latter,

$$\frac{\mathrm{E}\,p_0 v_0}{\mathrm{H}}\int_0^{\mathrm{H}} \frac{1}{v}\,\frac{dv}{dq}\,dq\,.\,\tau$$

If H be taken infinitely small, the latter expression becomes

$$\mathrm{E}\, p_0 v_0 \frac{1}{v} \frac{dv}{dq} \cdot \tau.$$

Hence, if $\mu\tau$ denote the mechanical effect due to the descent of one unit of heat from A at the temperature $t+\tau$ to B at the temperature t, we have

$$\mu = (1-\sigma)\frac{\frac{dp}{dt}}{k} = \mathrm{E}\, p_0 v_0 \frac{1}{v}\frac{dv}{dq}.$$

The value of μ (" Carnot's coefficient"), which is independent of the nature of the liquid or gas employed, may be determined for an assigned temperature, by means of observations upon any gas, or any liquid and its vapour. The most complete series of experiments from which the values of μ at different temperatures may be deduced, are those by means of which Regnault has determined the latent heat of a given weight, and the pressure, of saturated steam, at all temperatures between 0° and 230°. Besides these data, however, the density of saturated vapour must be given, in order that k, the latent heat of a unit of volume, may be calculated from Regnault's determination of the latent heat of a given weight. Between the limits of 0° and 100°, it is probable, from various experiments which have been made, that the density of vapour follows very closely the simple laws which are so accurately verified by the ordinary gases ;* and thus it may be calculated from Regnault's table, giving the pressure at any temperature within those limits. Nothing as yet is known with accuracy as to the density of saturated steam between 100° and 230°, and we must be contented at present to estimate it by calculation from Regnault's table of pressures ; although, when accurate experimental researches on the subject shall have been made, considerable deviations from the laws of Boyle and Dalton may be found.

Such are the experimental data on which the calculation of the mean values of μ, for the successive degrees of the air-thermometer from 0° to 230°, at present laid before the Royal Society, is founded.

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* This is well established by experiment, within the ordinary atmospheric limits, in Regnault's *Etudes Météorologiques*, in the *Annales de Chimie*.

The unit of length adopted is the English foot; the unit of weight, the pound; the unit of work, a "foot-pound;" and the unit of heat, that quantity which, when added to a pound of water at 0°, will produce an elevation of 1° in temperature. In making the calculation, the factor σ, in the expression for μ, which for all temperatures between 0° and 100° is less than $\frac{1}{1700}$, is neglected. The mean value of $\frac{dp}{dt}$ for any degree of the scale is found to a sufficiently high degree of approximation by merely taking the difference, the pressures given by Regnault at the temperatures immediately above and below it; and, to complete the calculation on the same system, the denominator of the fraction is taken as the mean value of k for that degree. The amount of mechanical effect due to the descent of a unit of heat through the nth degree of the scale, will be simply the nth value of μ in the table thus calculated.

The following abstract of the table, exhibits the sum of the first twenty values of μ, of the second twenty, of the third twenty, and so on; as well as the first value, the twenty-first, the forty-first, &c.

Mean values of μ for Cent. degrees of the Air-thermometer.		Sums of values of μ for intervals of 20°.	
No. on the scale.	Ft. lbs.		Ft. lbs.
1	5·12	From 1 to 20	99·8
20	4·85	... 21 ... 40	94·2
40	4·57	... 41 ... 60	88·8
60	4·31	... 61 ... 80	83·9
80	4·09	... 81 ... 100	79·7
100	3·90	... 101 ... 120	76·2
120	3·73	... 121 ... 140	73·3
140	3·60	... 141 ... 160	70·7
160	3·48	... 161 ... 180	68·5
180	3·37	... 181 ... 200	66·7
200	3·30	... 201 ... 220	65·2
220	3·23		
230	3·19		

As an example of the usefulness of these tables, let it be required to find the amount of mechanical effect produced by a steam-engine working with perfect economy, for each unit of heat which, after entering the water of the boiler, is *let down* through the engine to the condenser, and there evolved. The "thermal agency" here is a unit of heat let down from a body at the temperature of the water in the boiler to another at the temperature of condensation, and the "mechanical effect," therefore, cannot be determined, unless those temperatures be given. Let us suppose then, in a particular engine, that the water of the boiler is at 120°, and the condenser at 40°, during the working of the engine. The required mechanical effect, calculated by adding the "sums" in the preceding table for all the intervals from 40° to 120° is found to be 328·6 foot-pounds.

2. Theoretical Considerations on the Effect of Pressure in lowering the Freezing-Point of Water. By James Thomson, Esq., jun., Glasgow. Communicated by Professor W. Thomson.

At the commencement of this paper the two following propositions are laid down:

I. That water at the freezing-point may be converted into ice by a process solely mechanical, and yet without the final expenditure of any mechanical work.

II. That the freezing-point of water must become lower as the pressure to which the water is subjected is increased.

The first of these is given as being interesting in itself, and as having been the original means of suggesting the second to the author. It may be deduced directly by the application to the freezing of water of the principle developed by Carnot, that no work is given out when heat passes from one body to another without a fall of temperature; or rather by the application of the converse of this, which, of course, equally holds good,—namely, that no work requires to be expended to make heat pass from one body to another at the same temperature. The first being established, the reasonableness of the second will readily be admitted; because the ordinary supposition of the freezing-point being constant, would involve the absurdity of a perpetual motion (or, more strictly, a perpetual source of mechanical work) being possible. For if a quantity of water were enclosed in a vessel with a moveable piston and frozen without the expenditure of work, the motion of the piston consequent on the expansion being resisted by pressure, mechanical work would be given out; and there would be no expenditure of any thing whatever to serve as an equivalent for this mechanical work given out, because the water, after having been frozen, might be again melted, and so reduced to its original state, without the expenditure of work, according to the principles of Carnot already referred to. By the continued repetition, with the same mass of water, of the processes thus indicated, an unlimited quantity of work might be developed out of nothing, which is impossible. It, therefore, appears that if the water be made to perform work while freezing, either *work* or some equivalent agency must have been expended in freezing it. Now, the only way of accounting for this expenditure is by the assumption of the second proposition.

The fact of the lowering of the freezing-point by pressure being demonstrated by the method of which an outline has just been given, it becomes desirable, in the next place, to find what is the freezing-point of water for any given pressure. The most obvious way to determine this would be by direct experiment with freezing water. This experiment has, however, not as yet been made; and it would be difficult to make it with the precision which would be desirable, since the variation to be appreciated is extremely small; so small, indeed, as to afford sufficient reason for its existence never having been observed by any experimenter. The exact amount of the variation may, however, be deduced in a different way from experimental data, of which we are already in possession. These data are (1.), The known expansion of water in freezing; and (2.), The quantity of work given out by a unit of heat in descending through a degree near the freezing-point, which has been deduced from the experiments of Regnault on steam, and has been already laid before the Royal Society, in a paper by Professor William Thomson. The desired result is expressed in the formula,

$$t = {\cdot}0072\ \mathrm{P},$$

in which P is the pressure above the first atmosphere, expressed in atmospheres as units, to which the water is subjected, and t the lowering of the freezing-point, expressed in degrees centigrade, produced by the addition of that pressure. This formula may be applied for any pressure from nothing up to many atmospheres.

The following Donations to the Library were announced:—

The Journal of Agriculture, and Transactions of the Highland and Agricultural Society of Scotland. No. 23, N. S. 8vo.—*By the Society.*

Description de l'Observatoire Astronomique central de Pulkova. Par F. G. W. Struve. 2 tom. fol.

Beobachtungen des Halleyschen Cometen bei seinen erscheinen in Jahre 1835, auf der Dorpater Sternwarte angestellt von F. G. W. Struve. fol.

Stellarum Duplicium et Multiplicium Mensuræ Micrometricæ per Magnum Fraunhoferi Tubum, annis a 1824 ad 1827, &c. Auctore F. G. W. Struve. fol.

Additamentum in F. G. W. Struve Mensuras Micrometricas Stellarum Duplicium, &c. 4to.

Catalogus Novus Stellarum Duplicium et Multiplicium. Auctore F. G. W. Struve. fol.

Table des Positions Géographiques principales de la Russie. Rédigée par M. W. Struve. 4to.

Nouveaux Catalogues d'Etoiles Doubles. fol.

Catalogus Stellarum ex Zonis Regiomontanis. Auctore M. Weisse. 4to.

Expédition Chronométrique exécutée en 1843, entre Pulkova et Altona. fol.

———— Le même, exécutée en 1844, entre Altona et Greenwich. fol.

Notice sur l'Instrument des Passages de Repsold, &c. Par M. Struve. 4to.

Über die Flächeninhalt der 37 Westlichen Gouvernements und Provinzen des Europ. Russlands. Von F. G. W. Struve. 4to.

Resultäte der in dem Jahre 1816 und 1819, Ausgefürthen Astronomischen Trigonometrischen vermessung Leilands. Von W. Struve. 4to.

Astronomische Artsbestimmungen, in der Europaischen Turkei, &c. Von F. G. W. Struve. 4to.

Sur le Coefficient constant dans l'Observation des Etoiles fixés, &c. Par M. W. Struve. 4to.

Etudes d'Astronomie Stellaire. Par F. G. W. Struve. 1847, 8vo.

Resultate aus Beobachtungen des Polarsterns am erstelschen vertikalkreise der Pulkowaer Sternwärte. Von Dr C. A. F. Peters. 4to.

Von der Kleinen Arlenkungen der Lothlinie und des Niveans welche durch die Anziehungen der Sonne, &c. Von Dr C. A. F. Peters. 4to.

Über Prof. Mädlers Untersuchungen über die Eigenen Bevegungen der Fixsterne. Von Dr C. A. F. Peters. 4to.

Catalogus Librorum Speculæ Pulcovensis. 8vo.

By the Observatory of Pulkowa.

Mémoires de l'Académie Imp. des Sciences de St Pétersbourg. VIme Série. Tom. VI., Liv. 5 et 6 ; Tom. VII., Liv. 4, 5, et 6 ; et Tom. VII. ; (Tom. V., Liv. 1 et 2 ;) Tom. VIII., Liv. 1 et 2. 4to.

Recueil des Actes de la Séance publique de l'Académie Imp. des Sciences de St Pétersbourg, tenue le 29 Decembre 1845, et le 11 Janvier 1847. 4to.—*By the Academy.*

Acta Societatis Scientiarum Fennicæ. Tom. I. Tom. II., part. 1 and 2. 4to.

Notiser ar Sällskapets pro Fauna et Flora Fennica Förhandlingar. 1 Häftet. 4to.—*By the Society.*

Journal of the Asiatic Society of Bengal. No. 192. (Supp. No. for June 1848.) No. 194, Aug. 1848. 8vo.—*By the Society.*

Abhandlungen der K. Akademie der Wissenschaften zu Berlin. 1846. 4to.

Monatsbericht der K. Akademie der Wissenschaften zu Berlin. 1847, Juli—December; 1848, Januar—June. 8vo.—*By the Academy.*

Monday, January 15, 1849.

The Right Reverend Bishop TERROT, Vice-President, in the Chair.

The following Communications were read :—

1. On the Early History of the Air-Pump in England. By Dr George Wilson.

The early history of the English air-pump has been latterly allowed to fall into great confusion, so that the steps by which the instrument was improved, the periods at which those improvements were made, and the parties by whom they were effected, are all more or less confounded with each other, or mis-stated.

It is in connection with the double-barrelled air-pump, that the accepted history of the instrument is chiefly erroneous, but the mistakes made in reference to the more complex engine, have ultimately involved in confusion even the authentic records of the steps by which the earlier single-barrelled air-pump was improved, so that the account of its successive alterations must commence with its earliest and simplest construction.

The history of the English air-pump may be divided into four stages, three of which belong to the seventeeth century, and the fourth to the eighteenth. They are as follow, the dates, as given in the original authorities, being according to the Old Style:—

1659. The construction of a Pneumatical Engine consisting of a single-barrelled pump with a solid piston, moved by a rack and pinion, and a globular glass receiver directly communicating with the cylinder, which had an aperture closed and opened by a plug moved by the hand, and playing the part of a valve.

1667. The separation of the glass receiver from the cylinder, and introduction of the air-pump plate, on which bell-jars could be placed and used as receivers. The pump, still single-barrelled, and wrought by a rack and pinion, but with an aperture in the piston instead of in the cylinder, furnished with a moveable stopper.

1676. The introduction of a double-barrelled air-pump, with self-acting valves in the cylinders and pistons, and with piston-rods suspended at opposite ends of a cord passing over a pulley.

1704. The combination of the rack and pinion of the first and second air-pumps, with the two barrels, twin pistons, and self-acting valves of the third. The following are the more important details concerning those instruments.

Sometime before 1658, Boyle having heard, as he informs us (*Birch's Boyle*, Ed. 1772, vol. i., p. 6), of Guericke's air-pump and pneumatic experiments, had an exhausting engine of some kind constructed for him by Gratorix, a London instrument-maker of the time. No drawing or description of Gratorix's air-pump is extant, but it was so ineffective a machine that it was set aside as useless almost as soon as finished. Boyle had then recourse to Robert

Hooke, who constructed for him the air-pump which he employed in his first series of pneumatic researches. It appears to have been commenced in 1658, and completed in 1659, according to the separate testimony of Boyle (New Experiments, &c., touching the Spring and Weight of the Air, written in 1659, published in 1660, *Birch's Boyle*, vol. i., p. 7), and *Hooke* (*Waller's Life of Hooke*, p. iii.). The first English air-pump, which may be dated from 1659, had a single brass barrel about 14 inches in length, and 3 in internal diameter. It stood upon a strong wooden tripod, with its mouth turned downwards. The piston or sucker was solid. The shank or piston-rod had teeth cut on it, so as to form a rack, and was moved by a toothed wheel or pinion working into the rack, and turned by a handle, as in the air-pumps of the present day. A hole was bored in the side of the upper end of the cylinder, provided with a ground brass plug or stopper, which could be drawn out or pushed in by the hand. This was the only valve in the engine. The object of the inversion of the cylinder, was to allow the globular or pear-shaped glass receiver, from which it emptied the air, to be placed in a vertical position above the pump.

The receiver had a large opening at the top for inserting objects into it. The opening could be narrowed by a tight-fitting broad brass ring, in the centre of which was an aperture provided with a brass stopper to close it. The receiver terminated below in a narrow neck cemented into a brass stop-cock, which was ground to fit an opening in the upper end of the cylinder, near to the valve in it.

In using the pump to exhaust, the piston was first made to ascend or driven home, whilst the valve in the cylinder was open, and the stop-cock of the receiver shut. The valve was then closed by its stopper or plug, the stop-cock opened, and the piston drawn down. The stop-cock was then closed a second time, the valve opened, and the rarefied air which had entered the cylinder from the receiver, expelled from the former by the second ascent of the piston, and so on *ad infinitum*.

By reversing the order in which the valve and stop-cock were closed and opened, the pump could be made to condense instead of rarefying the air of the receiver. The valve for that purpose was opened, whilst the stop-cock was shut, and the piston drawn down so as to allow the cylinder to be filled with atmospheric air. The valve was then shut, the stop-cock opened, and the piston as it ascended condensed the air into the receiver.

The most important points to be noticed about the earliest English air-pump, are, that it was provided with one barrel and a manual valve, and that, unlike any later air-pump, the cylinder and receiver were directly connected. The designation by which Boyle preferred to distinguish his machine, was "Pneumatical Engine," and he called it, in contradistinction to his later air-pumps, the "Great Pneumatical Engine." It was presented to the Royal Society immediately after its incorporation in 1662, and Boyle desisted from pneumatic researches for some six or seven years.

In 1667 he constructed his second pneumatical engine, as appears from a letter dated, 24th March of that year, and published at Oxford in 1669, with the title, "A Continuation of New Experiments, &c., &c., touching the Spring and Weight of the Air, in a letter to Lord Dungarvan." Several persons supplied him with suggestions in the way of improvements, of whom, however, he mentions the name only of Hooke.

The second pneumatical engine did not resemble the first in appearance, but, like it, had a single brass barrel. This stood with its mouth upwards, in a large wooden box or trough, filled with water, which rose above the mouth of the cylinder, so that the latter was entirely under water. The object of this arrangement was to keep the leather of the sucker or piston always wet, and therefore "turgid and plump," so as to move air-tight in the barrel. The latter had no valve in it. The piston which was moved by a rack and pinion had an aperture passing vertically through it, which was closed and opened alternately, by thrusting in and pulling out a long stick managed by the hand of the operator. But the great improvement and peculiarity in the engine was, that the receiver was not directly attached to the pump. A tube, provided with a stop-cock, passed from the upper part of the side of the cylinder, in a horizontal direction along a wooden board covered with a thick iron plate, and was then bent up so as barely to project through the iron. The receiver was no longer a globe or pear-shaped vessel, but a bell-shaped hollow glass jar, which was turned with its mouth downwards, like an inverted drinking-glass, and, to use Boyle's homely but expressive phrase, "whelmed on upon the plate, well covered with cement." This arrangement of an air-pump plate, and detached bell-jar receiver, has been retained in all later air-pumps. In using the pump, the piston, with the aperture in it open, was forced to the bottom of the cylinder. The stick was then thrust into the hole in the piston, and the latter drawn up. It ascended lifting the water with it, and leaving a vacuum below. When the piston had risen above the mouth of the tube communicating with the receiver, the stop-cock was opened, and the air of the receiver allowed to expand into the cylinder. The stop-cock was then shut, the stick pulled out of the aperture in the piston, and the latter forced to the bottom of the cylinder. The air bubbled up through the aperture, and when it had escaped, the stick was inserted into the hole in the piston, and the manipulations proceeded as before. If the stop-cock were opened, as it was liable to be, at the wrong stroke, the receiver, instead of being emptied of air, was filled with water.

Six or seven years again elapsed, without any further improvement being effected on the English air-pump. In 1676, the celebrated and ingenious Frenchman, Denis Papin, came to England, bringing with him a novel pneumatic engine, and became Boyle's assistant. An engraving and description of Papin's air-pump are given in Boyle's tract entitled "A Continuation of New Experiments, &c., touching the Spring and Weight of the Air, and their effects. *Second Part.*"—(*Birch's Boyle*, 2d Ed., vol. iv., p. 505.) The great peculiarity of Papin's air-pump, as contrasted with former air-pumps, was, that it had two barrels, but it had other distinctive arrangements, which makes it singular that it should have been overlooked by later writers on Pneumatics.

It had two pumps standing side by side, the mouths of the barrels being turned upwards. Each of the piston-rods terminated in a stirrup attached to its upper end, and the stirrups were connected by a rope or cord, which passed over a vertical grooved wheel or large pulley. To work the machine, the exerciser of the pumps, as he is called in the original account, put his feet into the stirrups, and holding on, as it should seem, by his hands to the upper part of the framework of the air-pump, or leaning against it (for the description is not precise on this particular), moved his feet alternately up and down as a handloom weaver does, or a culprit on the treadmill. The pistons or suckers had valves (probably of bladder) opening upwards like that of an ordinary water-pump, and similar valves were placed at the bottom of the cylinders, which were filled with water to a certain height, that the pistons might move air-tight in them. From the cylinders, tubes passed to a common canal, terminating in the air-pump plate, on which receivers to be exhausted were laid, as in Boyle's second engine.

It is not a little singular that Papin's machine should have been overlooked by most later writers. It is not referred to in any recent English work of authority, although its curious stirrup arrangement, which has been employed in no English air-pump, might have been expected to direct attention towards it. Papin is mentioned incidentally by Nairne as an improver of the air-pump.—(*Phil. Trans.*, 1777, p. 635.) Dr Hutton, in his Mathematical Dictionary (vol. i., p. 55, 1796), mentions Papin's two barrels and twin pistons, but not the stirrup arrangement. In Shaw's abridged Boyle, the whole machine is described and figured, but Papin's name is not mentioned.

Recent writers on Pneumatics having overlooked Papin's machine, whilst they universally acknowledge the importance of two barrels with the pistons counterbalancing each other, have attributed this great improvement to Boyle, to Hooke, or to Hauksbee.

Boyle's imputed claim to the honour of having first constructed a double-barrelled air-pump, may be summarily dismissed, as he himself disavows the honour, refers to Papin's air-pump as new to him, and ascribes its invention to Papin.—(*Birch's Boyle*, vol. iv., p. 506.) Mr Weld, however, puts Boyle's claim on another, and at first sight apparently satisfactory, basis. The Royal Society, according to the former, who is its assistant-secretary, possesses Boyle's original air-pump, which has two barrels, and otherwise much resembles an air-pump of the present day.—(*History of Royal Society*, vol. i., p. 96.)

If, however, the instrument shewn to visitors to the Royal Society's apartments, be the earliest English air-pump, then Boyle was not only the first to employ a double-barrelled pneumatic pump, but his earliest pneumatical engine had two barrels. The instrument, however, which, as Boyle informs us ("Continuation of New Experiments, &c., on the Spring and Weight of the Air," Oxford, 1669, Preface), he gave to the Royal Society, in 1662, was his "Great Pneumatical Engine," which he described and figured in 1659. It had a single barrel, and was quite unique in its construction and appearance. The first double-barrelled air-pump to which Boyle refers is Papin's, with which he did not become acquainted till some seventeen years after he presented his earliest air-pump to the Royal

Society. Mr Weld, therefore, is certainly mistaken in conceiving that the old double-barrelled pump in the Society's possession is Boyle's original air-pump. It is probably not an instrument of Boyle's century.

Dr Thomas Young also supposes the first English air-pump to have had two barrels, and ascribes their introduction to Hooke.—(*Natural Philosophy*, Kelland's edition, p. 278.) The latter, however, states distinctly, that the instrument he made for Boyle had one barrel (*Waller's Life of Hooke*, p. iii.), and his drawing of it, which is engraved in the vignette frontispiece on the title-page of the several volumes of Birch's Boyle, represents the great pneumatical engine as possessing but one cylinder. Professor Robison, in his treatise on Pneumatics, ascribes the double pump in one place to Hooke (*Enc. Brit.*, 7th Ed., p. 80), and in another to Hauksbee (p. 93). Professor Robison does not refer to any writing of Hooke's as containing a claim, on his part, to the invention in question; and it is impossible to suppose that Hooke could have constructed a double air-pump before Papin did, without Boyle being aware of the circumstance. At all events, till it is shewn that Hooke himself claimed the double air-pump as his invention, it is unnecessary to discuss his supposed merits as its inventor.

Hauksbee appears to have been the first Englishman who constructed an air-pump with two barrels. He described it in his Treatise entitled "Physico-Mechanical Experiments on various subjects, byFrancis Hauksbee, F.R.S., 1709." It was constructed in, or about, 1704, so that it cannot come into competition with a double air-pump of Hooke's invention (if he ever devised one), seeing that he died in 1702. Still less can it supplant Papin's instrument, which was brought to England in 1676, and must have been known to Hauksbee.

Hauksbee's air-pump was a combination of the rack and pinion of Hooke's pneumatical engine which he constructed for Boyle, and the two barrels, twin pistons, and self-acting valves of Papin's pump.

From all, it appears, that no English claimant, at least, can dispute priority, so far as the double pump is concerned, with Papin. Winkler, who was Professor of Natural Philosophy at Leipsic in the middle of last century, in his Sketch of the History of the Air-Pump, refers to Hauksbee's as the first constructed with two barrels.—(*Elements of Natural Philosophy*, 1757, English Translation,

p. 119.) M. Libes (*Histoire Philosophique des Progrès de la Physique*, Paris, 1810–1812), mentions Papin and Hauksbee as the only claimants of the double pump (t. iii., p. 56); and adds, that Cotes, the mathematician of Cambridge, who was contemporary with Hauksbee, regarded Papin as the author of the invention. If there are no claimants known even to Continental historians of science, but Hauksbee and Papin, the latter, whose instrument was constructed more than twenty years before Hauksbee's, is entitled to the whole honour due to the inventor of the double air-pump.

2. On the Classification of Colours. Part II. By Professor J. D. Forbes. (See p. 190.)

The object of this paper is chiefly one of nomenclature. Every one has felt the difficulty of describing with the precision the innumerable hues which occur in nature and in art; and which it is equally desirable for the optical philosopher, the artist, and the manufacturer, to be able to refer to in a clear and definite manner. But such a nomenclature or classification must proceed upon some admission as to the manner of compounding complex hues out of simple ones; and, therefore, the author first treats of the (so-called) Primary Colours. He admits it as highly probable, that all known colours may be formed out of Red, Yellow, and Blue; although, when we attempt to compound pigments, we have a very notable loss of light, and also an unavoidable impurity, which is most visible in the compound tints. The author, in passing, endeavours to explain clearly why the union of pigments never can produce a perfect white, although the coloured light of the spectrum does so; for, by adding blue light to yellow light, we not only change the colour, but we increase the illumination; whereas, by adding a blue to a yellow pigment, whilst we change the colour, we at the same time reduce the luminousness of the surface, the blue particles being far less reflective than the yellow ones. Inferring from Newton's empirical rule, the quantities of red, yellow, and blue light, which should combine to make white light; and adopting Lambert's results as to the reflective powers of the brightest pigments, the author concludes, that the mean illumination of a disk put in rapid revolution, and containing coloured sectors, will be 4·57 times *less* than if it reflected the whole incident light, or it will reflect only about *half* the light which white paper does under the same illumination, therefore it will appear relatively *grey* under any given external illumination.

The author then states, that the triangular arrangement of colours first proposed by Mayer, and farther carried out by Lambert, appears to afford the clearest and truest mode of displaying at a glance the modification of colour due to the varying proportion of the three primary elements. In this triangle, perfect red, yellow, and blue, occupy the three corners; and these colours graduate into one another, according to the simple law of the distance of any point in the triangle from the three corners. The sides of the triangle are occupied by binary colours or compounds, by two and two; the interior is occupied by triple compounds; and the centre of gravity of the triangle ought to be a neutral grey.

Hence it will appear, that any hue not purposely diluted with black or white, as composed of a compound of a binary colour with neutral grey. Hence a convenient nomenclature suggests itself as follows: the first column containing the binary colours.

RED.	Greyish Red.	Grey Red.	Red Grey.	Reddish Grey.	Grey.
Orangish Red,	*	*			
Red Orange,	*	*	*		
Reddish Orange,	*	*			
ORANGE.	Greyish Orange.	Grey Orange.	Orange Grey.	Orangish Grey.	Grey.
Yellowish Orange,	*	*			
Yellow Orange,	*	*	*		
Orangish Yellow,	*	*			
YELLOW.	Greyish Yellow.	Grey Yellow.	Yellow Grey.	Yellowish Grey.	Grey,
&c.	&c.	&c.	&c.	&c.	&c.

These colours are supposed to be of the standard or maximum attainable intensity.

They may be diluted with white on the one hand, forming *tints;* or with black, forming *shades.*

Mayer's triangle may be repeated with these modifications; but as the colour tends to extinction, either in the direction of perfect blackness or perfect whiteness, the number of compartments in the triangles may be diminished as the dilution of the colours increases. Thus, the whole may be formed into a double pyramid of colour, converging to white above and to black below.

The author has been much indebted to Mr D. R. Hay, the ingenious author of the "Nomenlature of Colours," and other works, not only for specimens of coloured papers formed by the actual mixture of the three primary colours, but also for many valuable suggestions, of which, in the course of this paper, he has freely availed himself.

It is the author's wish to be able to obtain a series of coloured enamels complete, according to Mayer's and Lambert's classification. Some he has already obtained from the Vatican Collection (of which he gives a short description), and he hopes to render it more complete.

3. Verbal Notice of Siliceous Stalactites on Arthur's Seat. By Dr Fleming.

Dr Fleming began by stating, that a paper of his, "On the Neptunian formation of Siliceous Stalactites," was read before the Society, March 7, 1825, and published in "Brewster's Journal of Science," for April of the same year, p. 307. To this paper Dr Hibbert has referred, in his description of the "Limestone of Burdiehouse," *Edin. Phil. Trans.*, vol. xiii., p. 280, but has misrepresented, in an unaccountable manner, the facts which had been stated. Dr Fleming, expressly said, in describing a limestone containing the remains of dicotyledonous plants, and consisting of flinty and calcareous layers, that it "*dips under* the great bed of limestone belonging to the coal formation which extends north towards Linlithgow," which "encloses the remains of those marine animals which are common in the limestones of the coal formation." Dr Hibbert, on the other hand, confounds the two beds, or rather represents the bed with the vegetable remains as having been viewed as identical with the bed of limestone with marine remains; for he adds, "Dr Fleming's remark, that this limestone encloses the remains of those marine animals, which are common in the limestones of the coal formation, I consider as a mistake." The limestone, however, with vegetable remains, had been described as differing in structure, and occupying a lower position, than the limestone with marine remains.

Dr Fleming then stated that, in the paper referred to, he had described siliceous stalactites as occurring in the trap-rocks of the north side of Fife (a prolongation of the Ochils), and for some time looked for similar concretions, in vain, in the corresponding rocks of Arthur's Seat. Lately, however, he had detected them, hanging from the under surface of a bed of porphyry interstratified with laminated clay, at the Bog-Crag on the east side of the Hunter's Bog. The aqueous origin of the stalactites would not now be disputed, nor, in the present state of chemistry, would their occurrence excite surprise.

The following Donations to the Library were announced:

The Quarterly Journal of the Chemical Society. No. 4. 8vo.—*By the Society.*

Railway Economy: An Exposition of the advantages of Locomotion by Locomotive Carriages, instead of the present expensive system of Steam Tugs. By Lewis Gordon, C. E. 8vo.—*By the Author.*

Transactions of the Royal Scottish Society of Arts. Vol. III., Parts 2 and 3. 8vo.—*By the Society.*

Monday, 5th February 1849.

Sir T. MAKDOUGALL BRISBANE, Bart., President, in the Chair.

The following communications were read:—

1. On some peculiar Impressions on the Surface of certain Strata of Greywacké Schist, at Goldielands, in Roxburghshire. By James Elliot. Communicated by David Milne, Esq.

After some prefatory remarks on the general character of the greywacké formations in the south of Scotland, on their entire destitution of organic remains, or even decided impressions, and on the general prevalence of marks, produced apparently by shallow water, a singular series of schistose strata is described, of little more than two feet in thickness altogether, presenting everywhere peculiar features. First, there are two opposed surfaces, the one sprinkled over with thin, short, raised streaks, and the other with small cylindrical grains, all lying perfectly parallel to each other, and consisting of a hard substance, differing from the material and colour of the greywacké rocks. Next, there are a few seams of fine schist, and then a surface, covered with minute, sharply-defined indentations, having every one a lip turned up on one side, and sometimes clinging to the lip, a small speck of the same hard brown substance, which appears on the two surfaces first mentioned. The lips are invariably on the same side of the indentations, giving the surface the appearance of a farrier's rasp; and the uniform direction in which the lips are thrown out from the indentations, is exactly parallel to the streaks and grains first described. At right angles to that direction are narrow undulating ridges, such as would be produced by a cutting wind on a tenacious surface. The marks are preserved by a very thin coating of fine earth, and the opposite surface is not a counter-impression, but has peculiarities of its own. After the intervention of a few other seams, there follow repetitions of those already described, but somewhat varied. In conclusion, an explanation of all the appearances is attempted. The author suggests that they have been caused by showers of sand, driven by a strong wind upon the surface of the rocks before they had become hardened. The sand, he supposes, has been derived from volcanoes in activity at the period, and the existence of which is inferred by the igneous character of many of the neighbouring hills.

2. On the Causes of Local Peculiarities of Temperature in different parts of Great Britain. By James Elliot. Communicated by David Milne, Esq.

Many remarkable diversities of temperature are observed in this island, which have not yet been satisfactorily accounted for, either by difference of latitude or of elevation, by shelter or exposure, or by the influence of currents in the ocean. It is attempted to shew that other causes usually assigned have no validity,—that the proximity of high, and consequently cold mountains, has no effect in cooling the low ground near them, even when their summits are covered with perpetual snow, and that a difference in the clearness of the sky, or in the radiating power of the surface of the ground, produces no effect on the average temperature. The great cause, then, of the diversities in question is to be found, the writer considers, in the latent heat of vapour,—in the caloric disengaged by its condensation, or absorbed in its formation. He shews the great addition which may be made to the temperature of the atmosphere by a heavy fall of snow to the windward, and, on the other hand, the great loss of temperature by evaporation. The differences, in the amount of rain, he attributes almost entirely to the general slope of the surface over which the wind passes, in connexion with the height of the ground over which it has previously passed, and the differences of evaporation to the material of the soil and its covering, and to its state of drainage, natural or artificial. Some experiments are then detailed, shewing the amount of moisture capable of being retained by various coverings of soil, moss, &c., and the extent to which some of these promote evaporation. The writer concludes by shewing the great improvement which may be made in the general climate of this island, and particularly in that of its mountainous districts, by complete draining.

3. Verbal Notices. By Dr Fleming.

1. *On the Shell referred to by Ure in his "History of Rutherglen and Kilbride," as "a species of Patella."*—Dr Fleming called the attention of the Society to the extraordinary merits of Mr Ure, who died in 1798, leaving a memorial, in the work above referred to, and which was published at Glasgow in 1793, of an acquaintance with organic remains unequalled on the part of any contemporary author of the United Kingdom. This work, however, is very seldom referred to by modern palæontologists, although eminently useful in illustrating the progress of discovery. It was likewise stated, that an additional degree of interest must be felt by the members of the Society, in consequence of a collection of organic remains, chiefly marine, and from the carboniferous limestone, which belonged to Mr Ure, having been presented by Mr Stark, and which now occupies a place in the cases up stairs.

Dr Fleming stated, that the description of the Patella, referred to at p. 305, and delineated in Tab. xv., figs. 9, 10, is so obscure, doubtless in consequence of the imperfect specimen then in Ure's possession, that it was not until he had succeeded in procuring examples in nearly the same condition, along with others more characteristic, that he could refer the organism to its type, or, rather to the genus DISCINA of Lamarck. This genus was unaccountably confounded with *Orbicula* of Lamarck, by Mr G. B. Sowerby, in a paper in the thirteenth vol. of Linn. Trans., 465, and the errors there introduced have been propagated in the "Silurian System" of Murchison; the "Geology of Yorkshire," by Phillips; and the "Geological Report on Londonderry," &c., by Portlock. The species indicated for the first time by Ure, is probably referable to the *Orbicula rugata* of the Silurian System, p. 610, T. v., f. 11, although much doubt must rest on the determination. The shell consists of several somewhat easily separable layers. The external one, cuticle-like, exhibits regular concentric grooves, constituting the character of the *O. rugata*, while in the different aspects of the inferior layers, may be contemplated the *O. nitida* of Phillips, and the *O. striata* of Sowerby. At the period when Ure wrote he seems to have been in possession of only imperfect examples, or the upper valve of this BRACHIOPOD, but in his collection now on the table there are good specimens of the lower valve, as well as characteristically-marked upper ones.

2. *On the "Fossil Echini" of Ure.*—Dr Fleming stated, that having found plates of Ure's Echinus in company with fragments of spines, with proximal extremities, similar to the figure, Tab. xvi., f. 8, and the distal denticulated extremities conjoined, he had characterised the species in his "British Animals," denominating it CIDARIS *Urii*, thereby commemorating the labours of the discoverer. Captain Portlock, in the work already reported, has described and figured portions of, apparently, the same species, as CIDARIS *Benburbensis*, manufacturing new species besides, without being aware of the differences in the form of the spines, as well as in the sculpture of the plates, from different parts of the crust; truths illustrated by the existing British CIDARIS *papillata*, giving evidence to the palæontologist of the expediency of combining a knowledge of recent with extinct forms. Dr Fleming concluded this notice with stating that he found, during last autumn, the remains of this carboniferous limestone organism in the lowest bed of the old red sandstone series, on the Berwickshire coast, in which he detected marine remains, beginning at the fundamental *conglomerate*, where it rests on the "Transition Rocks" at the Siccart Point, and proceeding westward to Dunbar.

4. Notice by Professor Piazzi Smyth of Locke's Electric Observing Clock.

This instrument, which has been invented in America, consists of an electro-magnetic machine, which, being placed in connection with an ordinary astronomical clock, does not interfere with the regularity of its going, while it marks the instant of each vibration of the pendulum on a revolving cylinder, whose circumference moves at the rate of one inch per second. Two wires being then taken to an observer at any distance, if he, when he observes a star crossing the meridian-wire of his telescope, makes contact with the wires, that moment is immediately marked on the same moving cylinder where the even seconds are registered. The fraction of a second may be then obtained with as much accuracy as the space of an inch may be subdivided by ordinary mechanical means: say to the hundredth of a second. This method is further available in many cases where the present mode of noting transits is not, and admits of a great multiplication of observations during the short space of time that the star occupies in crossing the field of view.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. II. 1849. No. 34.

Sixty-Seventh Session.

Monday, 5th February 1849.

Sir T. MAKDOUGALL BRISBANE, Bart., President, in the Chair.

The following Donations to the Library were announced:—

Abhandlungen der Mathematisch-Physikalischen Classe der K. Bayerischen Akademie der Wissenschaften. Bde. I., II, III., & IV., Abtheil. 1 & 2. 4to.

Abhandlungen der Philosophisch-Philologischen Classe der K. Bayerischen Akademie der Wissenschaften. Bde. I., II., III., & IV., Abtheil. 1 & 2. Bd. V., Abtheil. 1. 4to.

Die Chemie in ihrem verhältnisse zur Physiologie und Pathologie. Von D. Max. Pettenkofer. 4to.

Denkrede auf Joseph Gerhard Zaccarini. Von Carl F. P. v. Martins. 4to.

Rede bei eröffnung der Sizung der K. Bayerischen Akademie der Wissenschaften am 28 Marz 1848. Von Carl F. P. v. Martins. 4to.—*By the Academy.*

Bulletin de la Société de Géographie. Tom. IX., 3me Série. 8vo. —*By the Society.*

Journal of the London Geographical Society. Vol. XVIII., Part 2. 8vo.—*By the Society.*

Transactions of the China Branch of the Royal Asiatic Society. 1847. 8vo.—*By the Society.*

The American Journal of Science and Arts. 2d Series. Vol. VI., No. 18. 8vo.—*By the Editors.*

The Quarterly Journal of the Geological Society. No. 16. 8vo. —*By the Society.*

Memoirs of the American Academy of Arts and Sciences. N.S. Vol. III. 4to.—*By the Academy.*

The Ethnological Journal. No. 8. 8vo.—*By the Editor.*

Remarks on the Improvement of Tidal Rivers, illustrated by reference to works executed on the Tay and other rivers. By David Stevenson, F.R.S.E. 8vo.—*By the Author.*

Annales des Sciences Physiques et Naturelles, d'Agriculture et d'Industrie, publiées par la Société Royale d'Agriculture, &c., de Lyon. Tom. X. 8vo.—*By the Society.*

Annales de la Société Linnéenne de Lyon. Années 1845–6. 8vo. —*By the Society.*

Proceedings of the Royal Astronomical Society. Vol. IX., No. 2. 8vo.—*By the Society.*

Monday, February 19, 1849.

Mr CADELL in the Chair.

The following Communications were read :—

1. Abstract of a Communication on Rolling Curves.

This paper commenced with an outline of the nature and history of the problem of rolling curves, and it was shewn that the subject had been discussed previously, by several geometers, amongst whom were De la Hire and Nicolè in the *Mémoires de l'Academie*, Euler, Professor Willis, in his *Principles of Mechanism*, and the Rev. H. Holditch in the *Cambridge Philosophical Transactions*.

None of these authors, however, except the two last, had made any application of their methods ; and the principal object of the present communication was to find how far the general equations could be simplified in particular cases, and to apply the results to practice.

Several problems were then worked out, of which some were applicable to the generation of curves, and some to wheelwork ; while others were interesting as shewing the relations which exist between different curves ; and, finally, a collection of examples was added, as an illustration of the fertility of the methods employed.

2. On the Extraction of Mannite from the Dandelion. By Messrs Smith; with an Analysis of the Mannite, by Dr Stenhouse. Communicated by Dr George Wilson.

Messrs Smith stated that they had extracted from the dandelion, a large amount of a crystalline sweet substance, having all the physical characters of mannite. It was analysed by Dr Stenhouse, and found to contain carbon, hydrogen, and oxygen, in the proportions which characterise the accepted formula for mannite; viz., $C_6 H_7 O_6$, so that it certainly was the substance it was supposed to be.

Messrs Widnmann and Frickhinger, it was stated, had anticipated Messrs Smith in the separation of mannite from the dandelion juice, and were led to believe that the mannite did not pre-exist ready formed in the dandelion; but was formed in the juice as the result of a peculiar fermentation which it underwent. This result was confirmed by the Messrs Smith, who experimented with large parcels of the plant, and found that even from quantities of the fresh root, so large as 40 lb., no mannite could be extracted, if the expressed juice were prevented from fermenting; whilst, if fermentation were permitted, the same weight of roots yielded a large quantity of mannite, which appears to be derived from the sugar, inulin, &c., of the dandelion, which were converted into mannite, gum, and lactic acid.

The Messrs Smith stated, in conclusion, that they had not been able to confirm the statement of Polex, that the dandelion contains a bitter crystallizable substance, such as he had described under the name of Taraxacine.

3. On some new Voltaic arrangements with Chlorous and Chromic Acids, with an account of a Battery, yielding electricity of great intensity, in which the negative, as well as the positive element is Zinc. By Dr Thomas Wright. Communicated by Dr G. Wilson.

The author, after referring to the principle on which the intense batteries of Daniel and Grove are constructed, and to the disadvantages connected with the use of the porous cells in those arrangements, stated that he had some time ago instituted a series of experiments, with a view to the construction of a voltaic circle of high electro-motive force, capable of being excited by a single solution, similarly to the battery of Mr Smee. Having employed a great variety of solutions, he was led to consider mixtures of chromic or chlorous acid with dilute sulphuric acid, best adapted to the purpose he had proposed.

The chromic acid battery was arranged by twisting round one end of a cylinder of coke a copper conducting wire, soaking the part in boiling wax, and afterwards covering it with varnish, to protect the wire from the acid: the coke was then surrounded by a cylinder of amalgamated zinc, and firmly fixed in its place by wedges of varnished cork. To form the exciting liquid, a measure of strong sulphuric acid was added to an equal measure of a hot saturated solution of bichromate of potash: the mixture was then diluted with four measures of water, and set aside to cool. The coke and zinc cylinders placed in a tumbler of the solution possessed a high degree of electro-motive force, a single alternation being capable of decomposing acidulated water with platinum electrodes. The author stated, that the arrangement was not constant, its action gradually declining after immersion. But he considered that a small pair was well adapted for the excitation of electro-magnetic apparatus, from its possessing about three times the intensity of Smee's arrangement. In an experiment made by him with Dr Wilson, a series of four pairs, roughly put together in half-pint tumblers, decomposed acidulated water, at the rate of two cubic inches of mixed gases per minute with a cold, and four with a hot, charge of the chromic acid solution. No gas was evolved from the amalgamated zinc surface in either case. One of the advantages of the battery was, that a series of cylinders, however extensive, might, as in Wollaston's arrangement, be immersed and removed from the solution at once, and the energetic effects of first immersion obtained at pleasure. Platina, or boxwood charcoal might be used in place of coke: a small series of thirteen pairs (charcoal and amalgamated zinc), each exposing a surface of about a quarter of an inch square, afforded a shock equal to a Cruickshank's battery of fifty pairs of four-inch plates, a perceptible shock being even felt from four pairs.

The solution of chlorous or hypochloric acid ($Cl\ O_4$) was prepared, by pouring a drachm of powdered chlorate of potash into a wine glass containing an ounce of concentrated sulphuric acid, and in ten minutes afterwards plunging the mixture into seven ounces of water. A pair of plates (amalgamated zinc and thin sheet-brass) excited by the last solution gave a powerful current, until the chlorous acid was exhausted. The author had, however, more than once failed to obtain any current from such an arrangement. A mixture of chlorate of potash and concentrated sulphuric acid formed a good charge for the negative side of Grove's double cell.

The author then proceeded to describe the platinized zinc battery. He was led to its discovery by observing that zinc (not amalgamated) has a tendency to assume an inactive state in some solutions containing a large quantity of sulphuric acid. Two or three arrangements were described, in which the negative plate consisted of zinc in various fluids, viz., dilute sulphuric acid, hypochloric acid and sulphuric acid, solution of sulphate of copper and sulphuric acid, &c. The most powerful battery of this class was formed by a negative plate of zinc included in a porous cell, containing a mixture of one measure of nitric acid with four of sulphuric acid, and associated with a double plate of zinc in a solution of potash or common salt. Such a circle was found to have an intensity equal to the battery of Professor Daniel, but by simply brushing the negative plate with a very dilute hot or acid solution of chloride of platinum, *the electro-motive force of the battery was doubled*, a single cell being then capable of decomposing water with platinum electrodes. A platinized zinc battery of three cells was placed on the table, the arrangement of the porous and outer cell being that of Professor Grove. The positive plate of each outer cell was folded over into the porous cell, and formed the negative plate of the pair next in series; there was therefore no necessity for binding screws, solderings, or mercury cups in the whole arrangement. The platinized zinc of each cell exposed a surface of 2·5 in. by 3·7 in. The series of three cells gave a cubic inch of mixed gases in 37 seconds, but the author considered that the battery would give two cubic inches of gas per minute, when the charge of the arrangement had become warm by the passage of the current. After use, the zinc plates were well rinsed in clean water and allowed to dry, and it was considered advisable to give them a slight brushing with the solution of platinum before their being again used. The platinized zinc battery was a *constant* arrangement.

The following Donations to the Library were announced:—

American Journal of Science and Arts. Conducted by Professors Silliman and Dana. Second series. Vol. VII., No. 19. 8vo.—*By the Editors.*

Proceedings of the Academy of Natural Sciences of Philadelphia. Vol. IV., Nos. 3, 4, 5. 8vo.—*By the Academy.*

Journal of the Statistical Society of London. Vol. XII., Part I. 8vo. *By the Society.*

Quarterly Journal of the Geological Society. No. 17. 8vo.—*By the Society.*

The Ethnological Journal. No. 9. 8vo.—*By the Editor.*

Journal of the Indian Archipelago and Eastern Asia. Vol. II., Nos. 9, 10, 11, 12. 8vo.—*By the Editor.*

A Monograph of the British Naked-eyed Medusæ, with figures of all the species. By Edward Forbes, F.R.S. 4to.—*By the Author.*

Monday, March 5, 1849.

The Very Rev. Principal LEE, V.P., in the Chair.

The following communication was read:—

Biographical Notice of Dr Chalmers. By the Very Rev. E. B. Ramsay.

The writer of this paper commenced by stating his purpose of viewing Dr Chalmers as a public character only, and to avoid all questions which belonged to the peculiar relations in which he stood to his own religious communion; and after a brief outline of the various circumstances of his life, and the dates belonging to each, the paper proposed to consider Dr Chalmers,—

I. As an Author.

II. As a Political Economist.

III. As a Speaker.

I. Under the first head were noted the peculiarities of Dr Chalmers's mode of treating a subject, and the distinguishing points of his style, the abundance of his *imaginative* faculty, and the effects which were produced by it upon his writings.

II. Under the head of political economy, the principal object was to exhibit Dr Chalmers as a true Christian philanthropist. The circumstances connected with his management of the poor in St John's parish, Glasgow, were detailed; and the influence which that experiment had upon his views of pauperism and poor-laws, generally. Some objections to his Glasgow scheme were considered, and some misapprehensions of it were explained. Under this head were detailed his views on Ecclesiastical and Academical Endowments, and his testimony to the value of the English Church and Universities.

III. In considering Dr Chalmers as a public speaker, some specimens of his sermons were read, with remarks upon his eloquence in the pulpit. The case of his speech on the Catholic Emancipation Bill, was cited as an instance of great effect produced by eloquence at a public meeting. His striking reply to an accusation of inconsistency brought against him in the General Assembly, and a specimen of his mode of lecturing were given. Some observations were then made on his love of natural scenery, and his admiration of the beauties of the material world. His private character and social intercourse were referred to—his powers of conversation, and his perfect freedom from affectation, bigotry, or intolerance; of these qualities examples were cited, as also of his turn for humour, and his keen sense of the ridiculous. The author of the paper then concluded by stating his conviction, that although there might be differences of judgment regarding particular portions of Dr Chalmers's public acts and opinions, yet that no one who had known him personally, and who had been well acquainted with his real character—that no one who had studied and appreciated the spirit and tendencies of his writings, would hesitate to admit that he was a good and a great man—that he was a sincere friend to the poor,—and that his great aim in life was to promote the glory of God, and the wellbeing of his fellowmen—that he was a Scotchman of whom Scotland might be proud —and that his labours and his writings have gained a name, and established a reputation, which will not pass away.

The following Donations to the Library were announced:—

Catalogue of the Library of the Literary and Philosophical Society of Newcastle-upon-Tyne. 8vo.—*By the Society.*

Catalogue of the Library of the Royal College of Physicians of Edinburgh. 8vo.—*By the Royal College of Physicians.*

Journal of Agriculture, and Transactions of the Highland and Agricultural Society of Scotland. No. 24; (N. S.) 8vo.—*By the Society.*

Magnetische und Meteorologische Beobachtungen zu Prag. 1841, 1842, 1843, 1844, 1845, 1846, 1847, 1848. 4to.

Magnetische und Geographische Ortsbestimmungen Böhmen in dem Jahren 1843–5. Von Karl Kreil. 4to.

Magnetische und Geographische Ortsbestimmungen in Österreichischen Kaiserstaate. Erster Jahrgang. 1846. 4to.—*By the Observatory of Prague.*

Monday, March 19, 1849.

The Right Rev. Bishop TERROT, V. P., in the Chair.

The following Communications were read:—

1. An Attempt to compare the Exact and Popular Estimates of Probability. By Bishop Terrot.

The author began by defining probability as being that state of mind in which we are inclined to believe a proposition, without being absolutely convinced that it is true. Objectively, every proposition is either true or false; subjectively, it may be certainly true, probable, or impossible.

The measure of probability he shewed to be the same as the measure of the cause producing it; that is, the ratio of the reasons inclining us to believe a proposition, to the whole number of reasons bearing upon it, whether for or against; all of which reasons are founded, either upon necessary inference from experience, or from testimony.

How inference from experience may lead to a definite expectation of an event different from that which has been experienced, was shewn in the case of an urn, containing two balls, white or black, and from which a white has been drawn. There is then a probability of $\frac{1}{3}$ that the remaining ball will be black; and if a witness asserts that at a second drawing black was drawn, his credit must be very low indeed, if his assertion does not raise this probability from $\frac{1}{3}$ to above $\frac{1}{2}$, that is to say, render it more likely that the event in the second drawing was different from, though not contradictory, to that observed in the first drawing, than that it was not different.

The author then examined the probabilites arising from combined testimony, as expressed by the formula,

$$\frac{1}{1+\left(\frac{1-v}{v}\right)^n \cdot \frac{1-p}{p}} = \pi$$

where p is the antecedent probability, v the average veracity of the witness, and n the number of witnesses. The cause of an antecedent probability was stated to be a natural tendency to believe in the continuity of series, and to be, in fact, a modification of the inference from experience.

It was then shewn, that this expression is applicable to the probable correctness of the verdicts of juries, assuming v to represent the probability of correctness in each juryman. Taking the number of the jury at three, it appeared that the probability of getting a correct unanimous verdict, was to the probability of getting a correct verdict by a majority as 3 to 4. But, on the other hand, it appeared that assuming p to be $\frac{9}{10}$, the probability of error in the unanimous verdict is $\frac{1}{6562}$, while the probability of error when two divide against one is $\frac{1}{82}$; that is to say, about eighty times as great as in the former case. Hence it was inferred that verdicts, by a bare majority, are admissible in civil cases, where it is sufficient that the verdict be probably correct; but not in criminal cases, where it is desirable that the probability of correctness should be carried as near to certainty as possible.

Lastly, Whereas there are, in every criminal charge, a certain number of points, all of which the jury desire to know, and which, if all proved, whether for or against the accused, would give a definite probability of his guilt or innocence, but of which one or more may remain untouched by the evidence, it was shewn that the jury would not deal fairly with the case if they threw the non-established point out of view, or if they gave the benefit of the doubt, as it is called, to the prisoner. Supposing $\frac{a}{a+b}$ to be the probability of guilt, independently of the non-established point, which must be considered as equally likely to be really for as against the accused, we must see that the consideration of this adds $\frac{1}{2}$ to a, and the same to b. So that $\frac{a}{a+b}$ becomes $\frac{a+\frac{1}{2}}{a+b+1}$; and these, reduced to a common denominator D, are $\frac{2a^2+2ab+2a}{D}$ and $\frac{2a^2+2ab+a+b}{D}$. So that the consideration of the omitted point increases or diminishes the probability of guilt, according as $a+b$ is greater or less than $2\,a$, or a greater or less than b: That is to say, the preponderance of the greater probability is diminished by the consideration of each unproved note. If, as was supposed to be generally the case in criminal trials, the probability from the evidence preponderates against the accused, then he does get a benefit from the due consideration of each unproved note, though very far from the benefit he would have gained by proving it in his favour.

2. On the Gradual Production of Luminous Impressions on the Eye, and other phenomena of Vision. By William Swan, F.R.S.E.

The object of this communication was to ascertain the relation between the apparent brightness of a light, and the time during which it acts on the eye. In order to examine the intensity of luminous impressions of short duration, the author made use of discs, having sectors of known angles cut out of their circumferences, which were made to revolve at known velocities between the eye and a luminous object. In this manner, the object is seen at each revolution of the disc for a short interval of time, of which the duration is easily ascertained. An instrument termed a selaometer (from σελας, *brightness*), to indicate its use as a measure of the intensity of luminous impressions, was devised for the purpose of comparing the brightness of the flashes caused by the revolution of the disc, with a light of known intensity. This instrument consists of two screens, placed so as to face each other, having each a circular aperture of the same diameter, to which is fitted a piece of obscured glass. A disc, having a sector of a known angle, revolves in front of one of these screens, so that the aperture in it is visible at each revolution of the disc throughout the sector. The apertures are illuminated by gas flames behind them, which admit of having their distances from the screens varied, so as to increase or diminish the illumination of the apertures. A rectangular prism of glass is placed half way between the apertures, with its faces inclined at angles of 45° to the line joining their centres; so that they are seen in apparent contact by reflexion from the faces of the prism, and their relative brightness can thus be compared with great nicety. The light behind the revolving disc is kept at a constant distance from the screen during an experiment; and, before causing the disc to revolve, the apertures are made equally bright by varying the distance of the other light from its screen. When the disc is put in motion, the apparent brightness of the aperture behind it is instantly diminished; and the equality of the apparent brightness of the apertures in the screens is restored, by increasing the distance of the light from the other screen. The ratio of the brightness of the impression produced by the light during the revolution of the disc, to the brightness of its impression, when seen by uninterrupted vision, is that of the squares of the distances of the other light from the aperture in its screen.

The following are the principal results obtained by means of this apparatus:—

(1.) When the eye receives, from a light of constant intensity, a succession of flashes of equal duration, which succeed each other so rapidly as to produce a uniform impression, this impression will also have a constant intensity, provided the number of flashes in a given time varies inversely with the duration of each flash.

(2.) The brightness of the impression produced by flashes of light of a given intensity, which succeed each other so rapidly as to produce a uniform impression on the eye, is proportional to the number of flashes in a given time.

(3.) When light of a given intensity acts on the eye for a short space of time, the brightness of the luminous impression on the retina is exactly proportional to the time during which the light continues to act. This law has been proved to be true for impressions lasting from $\frac{1}{18432}$ to $\frac{1}{24}$ of a second. The intensity of the impression produced by light which acts on the eye for $\frac{1}{100}$ of a second, is almost exactly $\frac{1}{10}$ of the brightness of the light when seen by uninterrupted vision; and it is also ascertained that light requires about the tenth part of a second to produce its full effect on the eye.

(4.) It is found that lights of different intensity act on the eye with equal rapidity, so that even the light of the sun produces an impression with no greater rapidity than that of a common gas flame.

(5.) Rays of different refrangibility act on the eye with equal rapidity.

(6.) Since Professor Wheatstone's experiments have proved that the light of the electric spark of high tension continues for less than the millionth part of a second, and it has been shewn that the brightness of the impression, produced by light on the eye, increases in the exact arithmetical proportion of the time during which it continues to act on the retina, it follows that the apparent brightness of the electric spark is only $\frac{1}{100000}$ of what it would become if the duration of the spark could be prolonged to $\frac{1}{10}$th of a second. From the great apparent brilliancy of the nearly instantaneous electric spark of high tension, when compared with the sensibly continuous light of Voltaic electricity, it is inferred that the brightness of electrical light increases with the tension of the electricity.

3. Note on the Refractive and Dispersive Powers of the Humours of the Eye, determined by Experiment. By John Adie, Esq.

The author's object in undertaking these experiments, was to discover if the achromatism of the eye could be accounted for by the differences in the dispersive ratios of the fluids forming that organ.

The indices for several of the fixed lines were determined in the aqueous humour; with the crystalline no satisfactory result could be obtained.

In subjecting the vitreous humour to experiment, only the strongest of the fixed lines could be seen, and that with great difficulty; one remarkable feature, however, was observed, viz., that on dividing the mass of humours two spectrums were formed, the one placed over the other, having a greater deviation, and, consequently, refractive power. Thus proving, that that humour is not of equal density throughout, as has heretofore been supposed.

The following Donations to the Library were announced:—

The London University Calendar for 1849. 8vo.—*By the University.*

The Ethnological Journal, No. 10. 8vo.—*By the Editor.*

Suite of the Collection of Hydrographic Charts, with Sailing Directions, &c.—*By the Lords Commissioners of the Admiralty.*

Monday, 2*d April* 1849.

General Sir T. MAKDOUGALL BRISBANE, Bart., President, in the Chair.

The following Communication was read :—

1. On Grooved and Striated Rocks in the Middle Region of Scotland. By Charles Maclaren, Esq.

In this paper an account was given of grooved, striated, and abraded rocks in various parts of Scotland, from Glen Spean on the north, to the Pentland and Lammermoor Hills on the south. After indicating the direction in which the groovings pointed, it was shewn,—that the appearance of these grooved and striated rocks is irreconcilable with the hypothesis which ascribes the phenomena to a supposed great Atlantic wave or transient flood, of which one part swept across the low lands of Scotland, while another part was turned back by the mountains,—that in the district between the Clyde and the Spean, where the largest and best marked groovings were observed, there is satisfactory evidence to prove, that they were produced by bodies of vast depth occupying the valleys, moving from the mountain group as a common centre, toward the coast and the Lowlands in all directions, and exerting an immense force of pressure vertically and laterally,—that this quaquaversal motion, as well as the form, position, and size of the groovings, are conclusive against the idea that they were caused by currents of water loaded with stones and gravel, since no collected mass of water exists, or could exist, of the requisite magnitude and elevation, to send out streams in all directions capable of acting powerfully at the height of a thousand feet or more above the bottoms of the valleys,—that the effects mentioned, therefore, can only be accounted for by the agency of glaciers, as exemplified in the Swiss Alps, where glacier ice is found covering immense areas, filling the valleys, and grooving and abrading their sides to the height of one or two thousand feet,—finally, that the striæ, groovings, and abrasion seen in the great central valley of Scotland, and on the Pentland Hills, are probably due to icebergs or rafts of ice, to which also the transportation of many travelled boulders may be ascribed.

The following Donations to the Library were announced :—

Annuaire Météorologique de la France pour 1849. Par MM. J. Haeghens, Ch. Martins, et A. Bérigny. 8vo.—*By the Authors.*

Bulletin des Séances de la Société Vaudoise, No. 19. 8vo.—*By the Society.*

Memorie della R. Accademia delle Scienze di Torino. Serie 2da. Tom. VII,, VIII., IX. 4to.—*By the Acadmy.*

Öfversigt af Kongl. Vetenskaps-Akademiens Förhandlingar. 1847, No. 10; 1848, Nos. 1, 2, 3, 4, 5, 6, 7, 8, 9. 8vo.

Kongl. Vetenskaps-Akademiens Handlingar för År 1846. 8vo.—med Plaucher. 4to.

Årsberättelse om Zoologiens Framsteg under Åren 1833–44. Tredje Delen, af S. Lovén. 8vo.

Årsberättelse om Zoologieus Framsteg under Åren 1845 och 1846, af C. H. Boheman. 8vo.

Årsberättelse om Framstegen i Kemi och Mineralogi, af Jac. Berzelius. 8vo.—*By the R. Academy of Stockholm.*

Œuvres de Laplace. Tom. V., VI., VII. 4to.—*By the French Government.*

Aanteekeningen, &c. van het Provinciaal Utrechtsch Genootschap van Kunsten en Wetenschappen. 1847 & 1848. 8vo.

Verslag, &c. van het Provinciaal Utrechtsch Genootschap van Kunsten en Wetenschappen. 1847 & 1848, 12°.—*By the Society.*

Report on the Epidemic Cholera at Madras in 1824. By William Scot. 8vo.—*By the Author.*

Monday, April 16, 1849.

Dr CHRISTISON, V.P., in the Chair.

The following communications were read :—

1. On a Simple Form of Rain-Gauge. By the Rev. Dr Fleming.

The author began by stating, that during a calm, when the rain-drop was under the influence of the centripetal force only, the form and position of the rain-gauge were unimportant. The case, however, was widely different when the drop was likewise influenced by wind, for then any object raised above the surface, such as a rain-gauge projecting three or four feet from the ground, occasioned deflections and eddies, whereby the regular fall of the rain into the collector was prevented. The Author then recommended, that in all cases the gauge should be placed on the ground, with its mouth on a level with a regularly trimmed grass plot, so as to prevent eddies and evaporation. The form which he considered unexceptionable (an example executed by Mr James Bryson, 66 Princes Street, being exhibited) was that of a copper cylinder, with a funnel-form partition placed about an inch and a half below the mouth, having an aperture for the index of a float, which rises as the rain passes into the receiving portion of the lower part of the cylinder, at the bottom of which is a stopcock for letting off, at times, the accumulated water. A second cylinder of copper, closed at bottom, is provided, to be inserted into the ground for the reception of the gauge, the latter having a shoulder or flange to prevent the entrance of earth. By this arrangement the collector and receiver are equal in area, so that errors of workmanship are avoided. The state of the gauge is known by simple inspection of the index of the float, and extreme facility of emptying and adjusting the instrument secured.

2. On a Method of Cooling the Atmosphere of Rooms in a Tropical Climate. By Professor C. Piazzi Smyth.

After stating the case distinctly, and dwelling emphatically on its importance, as shewn by individual instances in private life, and by the statistics of the world at large, the author proceeded to describe the various methods adopted at present in India, and shewed their incapacity to meet the end proposed, as they merely agitated the air already in a room, or perniciously overloaded it with moisture.

To take the most difficult case that could occur, he chose that of a country where the mean temperature of day and night, and summer and winter, is never below 80°, and where there could, consequently, be no coolness in springs or rivers, or in the night air; where also the atmosphere being saturated with moisture, no cold could be produced by evaporation; and under such circumstances proposed that a method should be found of lowering the temperature of the air in a room; doing, in fact, there, the reverse of what is effected in a cold room by lighting a fire.

The principle of the plan which he brought forward was dependent on the property of air to increase in temperature on compression, and to diminish on expansion; the air was to be compressed by a forcing pump into a close vessel, then cooled or rather deprived merely of its acquired heat of compression, and then being allowed to escape into the room desired to be cooled, would issue at a temperature as much below that of the atmosphere as it had risen above on compression.

That this was a *vera causa* there was no doubt; the *sufficiency* and the *practicability* were the only matters of doubt. These the author attempted to solve, by shewing the quantity of increase of heat due to a certain amount of compression; and by devising the most convenient form of the necessary apparatus, and concluded that a one-horse power should supply a room with 30 cubic feet of air per minute, cooled 20° below the surrounding atmosphere. The various sources of mechanical power likely to be met with in warm countries, were then described; and particularly a new and simple, and at the same time, a remarkably compact and effective form of windmill; as the wind is everywhere so cheap and abundant, and in the tropics so certain a species of moving power. Methods also of ventilating the cooled room, *i. e.*, of keeping it constantly supplied with cooled fresh air, and removing the vitiated, were explained, as well as a natural principle for meeting the residual difficulty that might be expected to arise in some cases, viz., the too great moisture of the cooled air.

3. Notice of a Shooting-Star. By Professor C. Piazzi Smyth.

The object of this notice was merely to call attention to the importance of observing the phenomena of shooting-stars more carefully and rigidly, and of applying to them more correctly than has generally been the case hitherto, the measurement of time and of space, and to exemplify what may be done in this way by the calculation of a recent instance. This instance, the rare one of an *ascending* shooting-star, was furnished by Captain W. S. Jacob, Bombay Engineers; and he having given the place where the body first appeared, that where it disappeared, and the time, the author of the paper, who had great faith in his friend's exactitude, considered the opportunity favourable for trying what results would be given by the application of Sir J. Lubbock's theory.

Some dissatisfaction has been felt about theories of shooting-stars, inasmuch as no one of them will explain *all* the observed phenomena. But though this is undoubtedly a necessary characteristic of a true theory, still great allowances are necessary here where so many different classes of cosmical and atmospherical objects may be confounded even by practised observers; and where the greater number of observers are utterly unpractised, and their senses wholly uneducated for scientific observation. Allowing that some electrical and magnetical effects have been mistaken for shooting-stars, but excluding the baseless electrical, chemical, and lunar hypotheses, a great proportion are undoubtedly of a cosmical nature, and belong properly to astronomy; and these may be divided into two classes of small bodies. 1*st*, Those which are circulating round the sun as a primary; and 2*dly*, Those which are revolving round the earth as such. The first we may occasionally see when passing near them in their orbits, but are not likely to come within sight of the same again, unless, indeed, they approach so near the earth as to gravitate towards it instead of the sun, and so become satellites or shooting-stars of the second class.

Sir J. Lubbock's theory is, that the shooting-stars shine by reflected light, and are extinguished by entering the earth's shadow; and he has given formulæ on this supposition for computing the distance of the body from the spectator by noting the place in the sky where, and the time when, the extinction occurs.

These formulæ have been rendered more convenient for computation by Mr Archibald Smith, *Phil. Mag.*, March 1849; and, computed according to them, Captain Jacob's observation gives, for the distance of the body from the observer, 1721 miles; and that entry into the earth's shadow was the true cause of the disappearance, is borne out by the fact that the direction of motion was *towards* the axis of the earth's shadow. And, on account of the extremely small distance of the body, its change of place during flight would sufficiently account for its gradually appearing in the lower part of the sky when coming out of conjunction, increasing in brilliancy during its flight (reaching, at its maximum, the brightness of Venus), and then slowly vanishing as it entered first the penumbra and then the umbra of the earth's shadow, in a slanting direction; and lastly, the body can hardly fail of being a satellite, as its distance is so much less than that of a shooting-star, which M. Petit of Toulouse has pretty well identified as revolving about the earth in $3^h\ 20^m$, or at about 3000 miles from the surface.

4. A few unpublished particulars concerning the late Dr Black. By Dr George Wilson.

The object of this communication was to lay before the Society a few characteristic incidents concerning Dr Black, gathered from Mrs Elizabeth Wordsworth, who was a servant in his household during the five last years of his life.

The facts recorded do not admit of abridgment, but they completely confirmed the accounts contained in the published biographies of Black, concerning his valetudinarian and methodical habits, whilst they gave no countenance to the statement which had been credited in some quarters, that the great chemist was an avaricious or penurious man. Some interesting particulars were adduced illustrative of the amiability and gentleness which characterised Dr Black; and the author concluded by noticing that an error had been committed as to the date of the philosopher's death, which was not the 26th of November 1799, as stated by Robison, but the 6th December of that year, a fact which Mr Muirhead first pointed out (*Watt's Correspondence*, p. xxii.), but which is confirmed by the newspapers of the period. (*Vide Edinburgh Mercury* of 14th December 1799.)

Dr Christison then exhibited some interesting Specimens of Alum-Slate, illustrative of the Manufacture of Alum, and described both the natural and artificial processes.

The following Donations to the Library were announced:—

Journal of the Asiatic Society of Bengal. Edited by the Secretaries. Nos. 196 and 197. 8vo.—*By the Editors.*

Proceedings of the Royal Astronomical Society. Vol. IX., No. 5. 8vo.—*By the Society.*

Monday, April 30, 1849.

Bishop TERROT, V.P., in the Chair.

The following communications were read:—

1. On a New Voltaic Battery of Intense Power. By Dr Wright. Communicated by Dr George Wilson.

The author placed on the table a battery of four pairs, each consisting of a rod of coke $2\frac{1}{2}$ inches long by $1\frac{1}{2}$ in diameter, surrounded by a cylinder of amalgamated zinc, $2\frac{1}{2}$ inches high by $2\frac{1}{4}$ inches in diameter; the different pairs were firmly attached to the same bar of wood, and could be immersed in jars of stoneware at once. The arrangement was charged with a mixture of nitric acid one part, sulphuric acid four parts, water eight parts. The author stated that he considered the power of the battery, which was twice that of Grove's, was due to the heat generated on the surface of the zinc by the local action of the nitric acid.

2. On a New Species of Manna, from New South Wales. By Thomas Anderson, M.D.

About thirty years ago a species of manna, obtained from the Eucalyptus Mannifera, was brought from New South Wales, and was examined by Dr Thomas Thomson, and afterwards by Professor Johnston, both of whom ascertained it to contain a new species of sugar, different from the mannite which exists in ordinary manna. The author had, through the kindness of Mr Sheriff Cay, an opportunity of examining a very different species of manna, remarkable both from its chemical constitution, and from its possessing a definitely organised structure. This substance was discovered by Mr Robert Cay in 1844, in the interior of Australia Felix, to the north and north-west of Melbourne, where it occurs at certain seasons on the leaves of the Mallee plant, Eucalyptus Dumosa, and is known to the natives by the name of Lerp.

It consists of numerous small conical cups of the average diameter of a sixth of an inch, more or less distinctly striated, and covered on the outside with hairs of considerable length. The cup resembles some of the smaller species of patella, and its mouth is perfectly smooth and round. Several of the cups are frequently attached to one another by the edges, and always so that their mouths form a plane, by which it would appear they have been attached to the leaves. The hairs, when examined under the microscope, were found to consist of uniform tubes, with a granular structure, and indistinct traces of transverse striæ; they are coloured uniformly blue by iodine. The cups are made up of a confused mass of closely-compacted cells resembling starch globules, and coloured blue by iodine.

The taste of Lerp is distinctly saccharine, but this is confined entirely to the hair, the cup having merely a mucilaginous taste. The chemical examination shewed it to consist of an uncrystallisable sugar similar in its character to that found in fruits, of starch, gum, inulin, and cellulose, the absolute identity of the latter two of which was determined by ultimate analysis. There were also found minute traces of resinous matter and nitrogen, and 1·13 per cent of ash. The following is the result of its quantitative analysis :—

Water, ...	15·04
Sugar with a little resinous matter, .	49·06
Gum, ...	5·77
Starch, ...	4·29
Inulin, ...	13·80
Cellulosa, ...	12·04
	100·00
Ash, ...	1·13

The author, in concluding his paper, remarked that all the species of manna before observed consisted of soluble substances, and were considered to be produced by the puncture of an insect, which caused the exudation of their constituents in the fluid form, and that they gradually dried up upon the surface of the leaf, but that the existence in Lerp of the insoluble cellulose and starch, and the sparingly-soluble inulin, seemed scarcely compatible with such an explanation of its origin.

3. Account of a peculiar Structure found in the Vagmarus Islandicus. By Dr John Reid. Communicated by Professor Goodsir.

4. Notes to a Paper on the Motive Power of Heat. By Professor William Thomson.

(1.) *On the Values of μ derived from Observations on the Vapours of various Liquids.*

An important test of the truth of the axiom on which Carnot's Theory is founded, will be afforded by comparing the values of μ deduced from observations on various liquids. I am informed by Mons. Regnault, that, by the end of this year, data as complete as those which we at present possess for water, will be supplied for five or six different liquids, from certain investigations with which he is now occupied. Carnot gives values of μ for the temperatures of the boiling of sulphuric ether, alcohol, water, and essence of turpentine, derived from various observations upon those liquids. The comparison of these with the values of μ, deduced from Regnault's continuous series of observations on water, are exhibited in the following table:—

Names of the Liquids.	Boiling-points.	Carnot's deduced values of μ.	Values of μ deduced from Regnault's Experiments on Water.	Differences.
		Ft. lbs.	Ft. lbs.	
Sulphuric Ether,	35·5	4·48	4·51	·03
Alcohol, . .	78·8	3·96	4·03	·07
Water, . .	100	3·66	3·84	·18
Essence of Turpentine, .	156·8	3·53	3·45	– ·08

The coincidences of the results obtained by such very different experiments are very striking. The differences certainly lie within the limits of the errors of observation; for it happens that the difference of the two results deduced by the different experimenters, from water at the boiling-point, is greater than any of the other differences. It is very remarkable that the feature of the gradual decrease of μ with the temperature should be so clearly brought out by observations performed on different liquids, at different temperatures.

(2.) *On the Heat developed by the Compression of Air.*

Carnot demonstrates the following proposition :—

Equal volumes of all elastic fluids, when compressed to equal smaller volumes, disengage equal quantities of heat.

This very remarkable proposition, given as a theorem by Carnot, was enunciated as a probable experimental law by Dulong ; and it therefore affords a very powerful confirmation of Carnot's fundamental principle.

Mr Joule of Manchester has made some important experiments on this subject. The view which he takes of a thermal "equivalent" for motive power is at variance with Carnot's theory, but his experimental results agree with its indications in a very satisfactory manner. In endeavouring to effect a comparison, I found that the following propositions are a consequence of Carnot's Theory.

1. *In compressing a gas of which the temperature is kept invariable, the amount of work spent is exactly proportional to the quantity of heat developed.*

2. *The amount of work necessary to produce a unit of heat in this manner is the same, whatever be the gas operated on, but depends upon the temperature, being determined by the expression*

$$\frac{\mu(1+\mathrm{E}\,t)}{\mathrm{E}}.$$

(3.) *On the Specific Heats of Gases.*

Carnot proves, as a theorem, that *the excess of the specific heat* under a constant pressure above the specific heat at a constant volume is the same for all gases at the same temperature and pressure.*

This result agrees well with the experimental results obtained by Dulong.

Carnot's theory affords the following determinate expression for the difference alluded to in the enunciation :

$$\frac{\mathrm{E}^2\,p}{\mu(1+\mathrm{E}\,t)^2}$$

---

* *i.e.* The "capacity for heat" of a unit of volume.

(4.) *Comparison of the Relative Advantages of the Steam-Engine and Air-Engine.*

In the steam-engine, with the expansive principle pushed to the utmost, as Carnot points out, the *effective range of temperature*, or the *fall* utilised, is from the temperature of the boiler to that of the condenser. The superior limit of temperature is restricted by the circumstance, that the pressure of saturated steam is enormously great for high temperatures; so that in practice, the temperature in the boiler is not in any ordinary engines so high as 150° per cent., but is in general very much below this limit. Carnot points out, that in this respect, the air-engine has a vast advantage over the steam-engine; as there is no limit to the temperature in the hot part, except such as the preservation of the materials requires; and, therefore, in it an enormously greater portion of the whole fall, from the temperature of the coals to that of the atmosphere, may be made use of. In other respects, we have no reason *a priori* for giving a preference to one kind of engine above the other. We cannot, however, feel confident that any air-engine has yet been constructed, which is capable of economising the fall actually used, as well as is done by steam-engines, with their comparatively limited range of temperature, or even that the duty for fuel consumed has in any actual air-engine exceeded or even come up to the duty performed by the best steam-engines.

(5.) *On the Economy of Actual Steam-Engines.*

The following table affords a synoptic view of the performances and theoretical duties, in various actual cases.*

When heat is transmitted from a body at 140°,† through an engine, to a body at 30°, the work due to each unit of heat is 439 foot-pounds. This is the "theoretical duty" referred to in the last column in the table.

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* I am indebted to the kindness of Professor Gordon, of Glasgow, for the experimental data.

† Pressure $3\frac{1}{2}$ atmospheres; 37 lb. on the square inch of the safety-valve.

TABLE A.—*Various Engines in which the Boiler is at* 140°, *and the Condenser at* 30°.

CASES.	Work produced for each lb. of coal consumed.	Work produced for each lb. of water evaporated.	Work produced for each unit of heat transmitted.	Per-centage of theoretical duty.
	Ft.-lbs.	Ft.-lbs.	Ft.-lbs.	
(1.) Fowey Consols Experiment, reported in 1845,	1,488,000	175,000	283	$64\frac{1}{2}$
(2.) Taylor's Engine at the United Mines, working in 1840,	1,167,000	137,300	222	$50\frac{1}{2}$
(3.) French Engines, according to contract,	* * * *	98,427	159	36
(4.) English Engines according to contract,	565,700	66,550	108	$24\frac{1}{2}$
(5.) Average actual performance of Cornish Engines,	631,000	74,240	120	$27\frac{1}{3}$
(6.) Common Engines, consuming 12 lb. of coal per hour, per horse-power.	165,000	19,410	31·4	$7\frac{1}{8}$
(7.) Improved Engines, with expansion cylinders; using an equivalent to 4 lb. of best coal per horse-power, per hour.	495,000	58,240	94·3	$21\frac{1}{2}$

5. Note regarding an Experiment suggested by Professor Robison. By Professor J. D. Forbes.

In his memoir of Dr Chalmers, lately read to this Society, Mr Ramsay has referred to an experiment which Dr Chalmers was anxious to have performed on the tide-wave in the Bay of Fundy. The object was to determine the earth's density by the attraction of the tide-wave on a plummet or spirit-level, on the same principle as

Maskelyne's experiment on Schiehallion, but with the superior advantages arising from the perfect homogeneity of the attracting mass, and from the circumstance that all the observations might be made at a single station. The experiment might, in short, appear to unite the advantages both of Maskelyne's and Cavendish's methods of determining the earth's density.

The suggestion was Dr Robison's, and Dr Chalmers had it from him. It is contained in the *Elements of Mechanical Philosophy*, Edit. 1804, page 339, and is given in the following words:—"Perhaps a very sensible effect might be observed at Annapolis-Royal in Nova Scotia, from the vast addition of matter brought on the coast twice every day by the tides. The water rises there above 100 feet at spring tide. If a leaden pipe a few hundred feet long were laid on the level beach, at right angles with the coast, and a glass pipe set upright at each end, and the whole filled with water, the water will rise at the outer end, and sink at the end next the land as the tide rises. Such an alternate change of level would give the most satisfactory evidence. Perhaps the effect might be sensible on a very long plummet, or even a nice spirit-level."

It is needless to observe that the methods proposed by Dr Robison are not the best which might be suggested; but that, in consequence of the extreme simplicity of the observation, considered as a purely astronomical one, a deviation of the direction of gravity of only a very few seconds could be ascertained within small limits of error.*

I thought it worth while to make the calculation approximately for an assumed height of the tide-wave. Had the result been at all encouraging I should have taken pains to ascertain, on good authority, the exact rise of the tide, and the circumstances of the locality whence the rise is greatest.

I have calculated the horizontal attraction of a semicylinder of water 100 feet thick, and of about two, four, and eight miles radius upon a point at the extremity of the axis of such a semicylinder; because these conditions can easily be reduced to calculation, and because

* The micrometric observation of a plumb-line, as in a zenith sector, would be sufficient; or, as Professor Smyth has suggested to me, the view of the wires of a transit instrument, with a collimating eye-piece, as reflected in a mercury trough,—an observation, the accuracy of which may, he states, be brought within $\frac{1}{20}$ of a second.

they represent very approximately the circumstances of an attracted point placed at high water-mark on a vertical sea-wall facing a basin or estuary. The radius of the attracting mass of water being represented (more accurately) by 10,000, 20,000, and 40,000 feet, I find the influence of a tide-wave 100 feet thick upon a plumb-line to produce a deviation of only 0·″44 (forty-four hundredths of a second), 0·″50, and 0·″53; the effect increasing extremely slowly with the radius, as might be expected. If the tide rose only fifty feet, the first effect would be reduced to 0·″246.

Even the greatest of these calculated deviations affords no ground for hoping that the method of Robison could be applied with any success to determine the earth's density.

It is rather singular that this ingenious suggestion is not once alluded to, so far as I am aware, by any writer on the figure and density of the earth; yet surely it was as worthy of notice as Dr Hutton's proposal to measure the attraction of an Egyptian pyramid.—(*Phil. Trans.* 1821.)

The following Donations to the Library were announced:—

American Journal of Science and Arts. Conducted by Professors Silliman and Dana. 2d Ser. Vol. VII., No. 20. 8vo.—*By the Editors.*

Ethnological Journal. No. 11. 8vo.—*By the Editor.*

Passages in the History of Geology. By Andrew C. Ramsay, F.G.S. 8vo. (2 copies.)—*By the Author.*

On the Nature of Limbs. By Richard Owen, F.R.S. 8vo.—*By the Author.*

Proceedings of the Philosophical Society of Glasgow. Vol. II. 1844–8. 8vo.—*By the Society.*

The Philosophy of Trade; or Outlines of a Theory of Profits and Prices. By Patrick James Stirling. 8vo.—*By the Author.*

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. II. 1849–50. No. 35.

Sixty-Eighth Session.

Monday, 3d December 1849.

Hon. Lord MURRAY, V.P., in the Chair.

The following Communication was read :—

1. Personal Observations on Terraces, and other proofs of Changes in the relative Level of Sea and Land in Scandinavia. By Robert Chambers, Esq., F.R.S.E., &c.

In this paper were given descriptions of alluvial formations of a terassiform character in the valley of the Lir river, near Drammen, in Norway, and of similar objects in valleys near the foot of the Miösen lake. The author then described a remarkable terrace which runs for fully fourteen miles at one elevation along the upper part of the valley of the Logan, in the Dovre field. It is composed on the left side of the valley of water-laid sand, and is believed to be about 2150 feet above the level of the sea. On the Dovre field, several hundred feet higher, are morasses containing the remains of much greater trees than are now growing in that district, the highest vegetation of which is a dwarf birch; and Mr Chambers remarks, that when the terrace was on the sea-level this district would enjoy a temperature fit for the production of such large timber. Mr Chambers next described some remarkable terraces in the valleys near Trondhiem, and particularly the great terrace of erosion which overlooks that city at an elevation of 522 feet above the sea.

The remainder of the paper was chiefly devoted to an account of a remarkable couple of terraces, which are traceable along the coasts of Nordlands and Finmark, apparently at one level (57 and 143 feet above the sea), excepting in the sounds near Hammerfest; where, throughout a space of twenty-five miles, they are upon an inclination. This portion of the phenomena fell under the attention of M. Bravais, of the French Scientific Expedition of the North, by whom they were measured barometrically and described. The present observer took measurements of these inclined terraces by the level and staff, in eighteen or twenty places, and thus confirmed the views of his predecessor. By Mr Chambers's observations, the following new points are ascertained: 1. The terraces, as being inclined, form an exceptive case, in contrast with those of a neighbouring district of coast of much larger extent (at least 180 geographical miles). 2. The disturbed district has moved on an axis of rest near Nœverfiord, where the two terraces are about the normal height. 3. A line, 14° west of north, (being nearly the line of the magnetic meridian), being drawn across the disturbed district, the inclination is shewn to be equable throughout equal spaces of that line, which is thus proved to be the meridian of the movement. 4. The northern extremity of the dip of the upper line at Hammerfest is 58 feet below, and the southern extremity, abreast of the Alten terraces, is 96 feet above the axis of rest.

The author then described visits which he paid in September 1849 to two of the places in the Gulf of Bothnia where marks have been made in order to detect the rate of movement of the land; at the rock near Löfsgrund he found the sea about six inches below the mark made sixteen years before by Sir Charles Lyell; while, on the cliffs of Grasöe, where Flumen made a mark in 1820, the water was exactly eleven inches lower.

His Grace the Duke of Argyll

was duly elected an Ordinary Fellow.

The following Donations to the Library were announced:—

Address delivered at the Anniversary Meeting of the Geological Society of London, 16th February 1849. By Sir H. de la Bêche. 8vo.—*By the Author.*

Proceedings of the American Philosophical Society. Vol. V., No. 41. 8vo.—*By the Society.*

Journal of the Statistical Society of London. Vol. XII., Pt. 2. 8vo. —*By the Society.*

Scheikundige Onderzoekingen, gedaan in het Laboratorium der Utrechtsche Hoogeschool. 5de Deel, 1ste, 3de, & 4de Stuk. 8vo.—*By the University.*

The American Journal of Science and Arts. Vol. VII., No. 21. 8vo. Edited by Professors Silliman and Dana.—*By the Editors.*

Journal of the Asiatic Society of Bengal. Edited by the Secretaries. New Series, No. 25. 8vo.—*By the Editors.*

Quarterly Journal of the Geological Society of London. No. 16. 8vo.—*By the Society.*

Bulletin de la Société Géologique de France. Tom. XIV., & Tom. I. & II. 2de Série. 8vo.—*By the Society.*

Sixteenth Annual Report of the Royal Cornwall Polytechnic Society, 1848. 8vo.—*By the Society.*

The Journal of Agriculture and Transactions of the Highland and Agricultural Society of Scotland. No. 25, N.S., July 1849. 8vo.—*By the Publishers.*

The Journal of the Royal Geographical Society of London. Vol. XIX., Part 1, 1849. 8vo.—*By the Society.*

Verhandelingen der Eerste Klasse van het K. Nederlandsche Instituut van Wetenschappen, Letterkunde, en Schoone Kunsten te Amsterdam. 3de Reeks, 1sten Deels, 2de Stuk. 4to.

Tijdschrift voor Wis-en Natuurkundige Wetenschappen, uitgegeven door de Eerste Klasse van het K. Nederlandsche Instituut van Wetenschappen, Letterkunde en Schoone Kunsten. 2de Deel, 3e & 4e Afleverings. 8vo.—*By the Institute.*

Report of the Eighteenth Meeting of the British Association for the Advancement of Science, held at Swansea, in August 1848. 8vo.—*By the Association.*

Neue Denkschriften der Allgemeine Schweizerischen Gesellschaft für die gesamten Naturwissenschaften. Bde. 8 & 9. 4to.

Verhandlungen der Schweizerischen Naturforschenden Gesellschaft bei ihrer Versammlung zu Winterthur 1846 & 1847. 8vo.

Mittheilungen der Naturforschenden Gesellschaft in Bern. Nos. 87–134. 8vo.

Die Wichtigsten Momente aus der Geschichte der drei ersten Jahrzende der Schweizerischen Naturforschenden Gesellschaft. 1848. 8vo.—*By the Society.*

Antiquités Celtiques et Antidiluviennes. Mémoire sur l'Industrie primitive et les arts à leur origine. Par M. Boucher de Perthes. 8vo.—*By the Author.*

Meteorologische Beobachtungen angestellt auf Veranstaltung der Naturforschenden Gesellschaft in Zürich. 1837–46. 4to.

Denkschrift zur Feier des hundertjahrigen Stiftung festes der Naturforschenden Gesellschaft in Zürich am 30 November 1846. 4to.—*By the Society.*

Mittheilungen der Naturforschenden Gesellschaft in Zürich. Heft I., (No. 1–13). 8vo.—*By the Society.*

Proceedings of the American Philosophical Society. Vol. V., January, March, 1849. No. 42. 8vo.—*By the Society.*

The Progress of the development of the Law of Storms, and of the Variable Winds, with the practical application of the subject to Navigation. By Lieut.-Colonel William Reid. 8vo.—*By the Author.*

On the Geological Structure of the Alps, Apennines, and Carpathians, more especially to prove a transition from Secondary to Tertiary Rocks, and the development of Eocene Deposits in Southern Europe. By Sir Roderick Impey Murchison. 8vo.—*By the Author.*

Account of the effect of a Storm on Sea-Walls or Bulwarks on the coast near Edinburgh, as illustrating the principle of the construction of Sea-Defences. By W. M. Rankine. 8vo.

An Equation between the Temperature and the maximum elasticity of Steam and other vapours. By W. M. Rankine. 8vo.—*By the Author.*

The American Journal of Science and Arts. Conducted by Professors Silliman and Dana. 2d Series, No. 22, July 1849. 8vo.—*By the Editors.*

Journal of the Asiatic Society of Bengal. Edited by the Secretaries. No. 200, February 1849. 8vo, and N.S., No. 28, April 1849, and No. 203.—*By the Editors.*

Journal of the Statistical Society of London. Vol. XII., Parts 3 and 4. 8vo.—*By the Society.*

Journal of the Geological Society of Dublin. Vol. IV., Part 1. 8vo.—*By the Society.*

Catalogue of the Calcutta Public Library. 8vo.—*By the Council.*

Flora Batava. 159 Aflevering. 4to.—*By the King of Holland.*

A Letter addressed to the Earl of Rosse, President-Elect of the Royal Society. By Marshall Hall, M.D. 8vo.

On the Neck as a Medical Region, and on Trachelismus ; on Hidden Seizures ; on Paroxysmal Apoplexy, Paralysis, Mania, Syncope, &c. By Marshall Hall, M.D. 8vo.—*By the Author.*

Astronomical Observations made at the Radcliffe Observatory. By Manuel J. Johnson. 1842, 1843, 1844, 1845, 1846, 1847. Vol. III.—VIII. 8vo.—*By the Radcliffe Trustees.*

Quarterly Journal of the Geological Society. No. 18, 1849. 8vo. —*By the Society.*

Journal of the Indian Archipelago and Eastern Asia. Vol. III. Nos. 1, 2, 3, 4. 8vo.—*By the Editor.*

The American Journal of Science and Arts. Conducted by Professors Silliman and Dana. Second Series. No. 23. 8vo.—*By the Editors.*

Memoirs of the Ganglia and Nerves of the Uterus. By Robert Lee, M.D. 4to.

On the Ganglia and Nerves of the Heart. By Robert Lee, M.D. 4to.—*By the Author.*

Athenæum. Rules and Regulations, List of Members, &c. 1847. 12mo.

——— Annual Report—General Abstract of Accounts. 1848.—*By the Athenæum.*

Description of a Machine for Polishing Specula, with Directions for its use. By W. Lassell, Esq. 4to.—*By the Author.*

Monday, 17*th December* 1849.

Right Rev. Bishop TERROT, V.P., in the Chair.

The following Communications were read :—

1. Note respecting the Dimensions and Refracting Power of the Eye.* By Professor J. D. Forbes.

" Whilst lecturing lately on the subject of Vision, I consulted some recent authorities on the dimensions and curvatures of the refracting apparatus of the eye; and having calculated from them the con-

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vergence of rays within the eye, it may save trouble to others to put them on record.

"The measures of the eye given in almost every English work on the subject, are those given by Young on his own authority, or that of Petit. In the fifth volume of Dove's Repertorium, I find a series of measures collected by Treviranus from his own and preceding observations, which I have converted below from French lines into decimals of an English inch. In these the curvatures are supposed spherical. In the same work of Dove, I find a series of measures by Dr Krause of Hanover, on eight recent human eyes, which seem to have been made with uncommon care, and in which the deviation of the surfaces from sphericity is noticed. I have preferred these last for the purpose of calculation, because *all* the measures are taken from the same eye, which is not the case with the numbers collected by Treviranus. I have consulted the original paper of Krause in Poggendorff's Annalen, vols. xxxi. and xxxix., where it appears, (1.) That the cornea is thicker at the sides than in the centre; (2.) The anterior curve of the cornea is nearly spherical, the posterior parabolic; (3.) The anterior surface of the lens is elliptical, the lesser diameter being in the axis of vision, the posterior surface is parabolic; (4.) The figure of the retina, or the posterior surface of the vitreous humour, is an ellipsoid.

"The following are those given by two eminent German authorities, Treviranus and Krause, when reduced to English inches:—

	Mean of several Authors by Treviranus.	Mean of eight measures by Krause.
	Inches.	Inches.
Thickness of cornea (central part), .	0·032	0·040
Distance of first surface of lens from back surface of cornea, . . .	0·104	0·107
Pupil behind cornea, . . .	0·096	0·083
Thickness of lens, . . .	0·181	0·181
Axis of vitreous humour, . .	0·548	0·567
Axis of the eye from interior of the cornea to the retina, ...	0.833	0·855
Radius of first surface of cornea. .	0·301	0·348
Radius of first surface of lens, . .	0·280	0·369
——— of second do., . . .	0·196	0·201
Curvature of retina near the axis, .	0·534	0·523

These numbers agree tolerably well, only that the radius of curvature of the first surface of the lens is disproportionately great in the last column. This arises from the circumstance, that it is derived by calculation, for the curvature of an ellipse at the lesser axis, the two axes of which are alone given by Krause. Now, it is evident, that if we regard the lens *as a whole*, or even any considerable breadth of it, its mean radius of curvature will be sensibly smaller. In fact, Krause finds that it may be tolerably represented by a circular curvature, having a radius of ·329 inches. It occurred to me, however, that by taking the *greatest* density of the lens, as given by Brewster, and the curvature of the middle part, both anterior and posterior, as given by Krause, I ought to arrive at a close approximation to the course of the axial pencil.

"I have adopted for the refractive indices of the parts of the eye, those given by Sir D. Brewster in his original paper in the Edinburgh Philosophical Journal, vol. i., page 44, with the exception of that of the densest part of the lens, which is almost certainly misprinted. They are as follow :—

Aqueous humour, . . .	1·3366 $=\mu_1$
Crystalline, outer coats, . .	1·3767
——— middle coats, . .	1·3786
——— central coats, . .	1·3990* $=\mu_2$
——— the whole, . .	1·3839
Vitreous humour, . . .	1·3394 $=\mu_3$

"Calculating from the preceding data, with Sir D. Brewster's indices of refraction, the author finds the positions of the foci, towards which the rays converge, after refraction at the successive surfaces, to be the following (reckoning from the *interior* surface of the cornea, the thickness of which has been neglected)—

	For rays falling parallel on the cornea. Inches.	For rays diverging from a point 10 inches distant. Inches.
After first refraction at the aqueous humour, . . .	= 1·382	1·541
After second refraction at first surface of the lens, . .	= 1·260	1·377
After third refraction into vitreous humour, . . .	= 1·060	1·135

* In the Edinburgh Phil. Journ., we find 1·3999. But I take this to be a misprint, as in Sir D. Brewster's own subsequent writings, we always find 1·3990.

"Now the measure of the axis of the eye we have seen to be only ·833 inch, according to Treviranus, and ·855 according to Krause; consequently, rays of mean refrangibility (to which Brewster's measures refer) converge to a point no less than ·227 inch behind the retina, when the rays fall parallel on the cornea, and ·302 when the object viewed is at 10 inches' distance. The axis of the eye, as even measured by Dr Young, though somewhat greater than we have reckoned it above, (Dr Young makes it ·91), does not come up to the requisite dimensions; and Dr Young, with his usual acuteness, ascribes the difference to the gradually varying density of the strata or coats of the lens,* the dense small nucleus evidently acting as a lens of comparatively short focus; and this explanation is probably the correct one, to which we may add, that the configuration of the coats of equal density, which, near the surface of the lens, are very elliptical, become, near its centre, gradually nearly spherical. On this account, it is all but impossible to predict the exact course of the rays through a structure of so much complication.

"Dr Young had considered the case with his usual attention and penetration. He investigates the focus of a spherical lens, or lens with surfaces which are segments of spheres, and whose density is variable, and the result may be recalled here as one which, perhaps, has not been sufficiently remarked. "On the whole," he says, "it is probable that the refractive power of the human crystalline in its living state is to that of water nearly as 18 to 17 [gives index refr. = 1·415]; that the water imbibed after death from the humour of the capsule reduces it to the ratio of 21 to 20 [1·403], but that, on account of the unequable density of the lens, its effect on the eye is equivalent to a refraction of 14 to 13 [1·439] for its whole size."†

"On the whole, these calculations, as well as the considerations into which I entered in a former paper, read to the Society in 1844,‡ on the mechanism of the focal adjustment, have left on my mind the conviction that the optical and mechanical structure of the organ of sight is even less understood than it is commonly believed to be. Simple as are its general arrangements, and comparable, in some respects, to those of artificial combinations, we perceive surfaces figured in a complex manner, and structures of varying refractive

* Nat. Phil., vol. ii., p. 580.

† Nat. Phil., vol. ii., p. 82.

‡ Transactions Royal Society of Edinburgh, vol. xvi., p. 1.

density combined in a very complicated manner. Krause's measures of the curvature of the surfaces of the lens confirm the inadmissibility of the all but universal opinion of the variation of density of the crystalline being intended to correct the aberration of spherical surfaces, when, in reality, no such surfaces exist. We are quite unable to trace the exact course by which the rays of light are focalised on the retina, since it depends on the internal constitution of the lens that they do not meet very far behind it; and it still remains at least doubtful how the adjustment to distinct vision of objects at different distances is effected.

"Finally, the question of achromatism of the eye has its own difficulties. It is not now contended that the eye has the power of converging equally rays of different refrangibilities; but it is not unreasonable to suppose that the chromatic aberration is at least partially corrected. One result of the calculations into which I have entered (which were first in part undertaken at my request, by Mr James Clerk Maxwell, and since entirely repeated and extended by myself), is a clear exhibition of the physical conditions of perfect achromatism in the eye. The form is simpler than I have elsewhere seen, and may at once satisfy any reasonable person of the possibility that the eye might be rendered achromatic, at least for objects at a certain distance; to prove which, so much has been written, and at so great length. The result may be stated in two lines. If we calculate the effect upon the *final* focal distance of the whole refracting system of the eye (q''), of a variation in the refractive index of each of its three humours (denoted by μ_2, μ_3, μ_4). We find this equation when the incident rays are parallel, or reach the eye from a very distant object:—

$$\delta q'' = 1{\cdot}579\ \delta\mu_2 + 1{\cdot}150\ \delta\mu_3 - 2{\cdot}788\ \delta\mu_4.$$

Let the coefficients $\delta\mu_2$, $\delta\mu_3$, $\delta\mu_4$ denote the dispersion or differences of the indices of refraction for extreme rays, corresponding to the three media, then it is evident, from the negative sign of the third term on the right hand, that they may be so chosen as to annihilate the second side of the equation, or make the variation of focal distances *nothing*, for the differently refrangible rays.

"If the rays proceed from a point 10 inches distant from the eye, the equation for the variation of the focus will be

$$\delta q'' = 1{\cdot}873\ \delta\mu_2 + 1{\cdot}402\ \delta\mu_3 - 3{\cdot}298\ \delta\mu_4$$

and the condition which makes this equal to zero, or the focus independent of small variation of the refrangibility of the ray may be satisfied, at the same time that the former equation is satisfied also; consequently, *with three media, as in the eye, we may have perfect achromatism for any two distances;* which would also be sensibly perfect for the intervening ones. Of course by perfect achromatism, we here mean a union of the extreme red and violet rays; the *irrationality* of dispersion does not concern this question."

2. On the Intensity of Heat reflected from Glass. By Professor J. D. Forbes.

The author, after referring to a communication made to this Society, on the 18th March 1839, on this subject, and noticed in the "Proceedings" of that date, stated, that being about to recommence his observations on radiant heat, so long and unavoidably interrupted, he had carefully examined the unpublished observations on which the previous notice was founded, with a view to ascertain what might be the numerical discrepancy which they present from Fresnel's Theoretical Law. The variation in the results of experiment for each of the angles was very considerable, arising from a multitude of causes as yet imperfectly estimated, but which appear to have been encountered by other observers, who, since that time, have undertaken the same research. Under the circumstances, the mean of the whole observations made between November 1838 and March 1839, have been taken for each angle of incidence; and the results being projected in the usual manner, the angles of incidence forming the line of abscissæ, and the intensities the ordinates, an interpolating curve was drawn through the whole. The numbers thus obtained (which are presented as only a rude first approximation), are shewn in the following table, and compared with Fresnel's Formula, calculated for an index of refraction of 1·50.

Incidence.		Proportion of Heat Reflected.		Fresnel's Numbers, when $\mu = 1{\cdot}50$.
0°	......	·038	......	·040
10	......	·040	......	·040
20	......	·044	......	·040
30	......	·051	......	·042

Incidence.		Proportion of Heat Reflected.		Fresnel's Numbers, when $\mu = 1{\cdot}50$.
40°	......	·060	......	·046
50	......	·076	......	·058
60	......	·105	......	·089
70	......	·185	......	·171
80	......	·433	......	·388
85	......	·68	......	·613
90	......	1·00	......	1·000

The results of experiment are generally in excess. This may be due to the impossibility of obtaining rays of heat quite parallel from terrestrial sources. To avoid this and other difficulties, experiments have recently been made in Germany by M. Knoblauch, and in Paris by MM. Provostaye and Depains. The results of the last named observers are very conclusive in favour of the accuracy of Fresnel's law. Their memoir had not reached the author of this paper until his calculations were almost completed.

3. On the solution of certain Differential Equations. By Professor Kelland.

Until recently, general solutions of several classes of equations, such as that which occurs in the theory of the figure of the earth, could not be arrived at. An ingenious transformation lately rendered it a matter of comparative ease to arrive at a solution of these equations in those forms in which they are presented in the solution of physical problems; but still much remains to be done. The object of the present paper is to supply some portion of the deficiency in this respect, by the introduction of a new transformation, and the adoption of the function Γ. The solutions thus obtained are perfectly general, and are arrived at with the greatest facility.

The Most Noble the Marquis of Tweeddale was duly elected an Ordinary Fellow.

The following Donations to the Library were announced:—

The Astronomical Journal. Vol. I., No. 1. 4to.—*By the Editor.*

Athenæum—Annual Report—General Abstract of Accounts from 1st January to 31st December 1848. 8vo.—*By the Athenæum.*

Twenty-Ninth Report of the Council of the Leeds Philosophical and Literary Society. 1848–49. 8vo.—*By the Society.*

Smithsonian Contributions to Knowledge. Vol. I. Published by the Smithsonian Institution. 4to.

Report, &c. of Smithsonian Institution. 1849. 8vo.—*By the Institution.*

Fauna Antiqua Sivalensis, being the Fossil Zoology of the Sewalik Hills, in the North of India. By Hugh Falconer, M.D., and Proby T. Cautley, F.G.S. Parts I. and IX. Fol.

Do. do. Letter-Press. Part I. 8vo.—*By the Author.*

United States Exploring Expedition during the years 1838, 1839, 1840, 1841, and 1842, under the command of Charles Wilkes, U.S.N. Atlas. Zoophytes. By James D. Dana, A.M. Imp. Fol.—*By the Author.*

Astronomical Observations made at the Royal Observatory, Greenwich, in the year 1847, under the direction of George B. Airy, Esq. 4to.—*By the Observatory.*

Philosophical Transactions of the Royal Society of London for the year 1849. Parts I. and II. 4to.

List of Fellows, &c. of the Royal Society, 30th November 1848. 4to.

Proceedings of the Royal Society. 1848. Nos. 71 and 72. 8vo. *By the Society.*

Monday, 7th January 1850.

Sir THOMAS M. BRISBANE, President, in the Chair.

The following Communications were read :—

1. On the Muscular Substance of the Tongue. By Mr Zaglus. Communicated by Professor Goodsir.

Professor Goodsir communicated an abstract of a paper by Mr Zaglus on the muscular structure of the tongue. The author of the paper had found the muscular substance of the tongue to consist of a cortical layer, which surrounds the organ on all sides, except its posterior attachment, and in the middle line of its inferior surface.

The cortex consists of a complicated network of fibres, derived from the hyoglossi, styloglossi, lingualis, chondroglossi, and a pair of new muscles, named by the author Notoglossi. The minute details of the arrangement of these muscles have now been ascertained by the author, and their actions in producing the peculiar volubility of the organ.

The cavity of the cortex is occupied by a medulla of transverse and perpendicular muscles, some of which are limited to the cavity itself; others pass into it from without.

The transverse system consists of transversales proprii, with the palatoglossi and glossopharyngei, the perpendicular of external or proper, perpendicular muscles, and internal or geneoglossi.

The transverse and perpendicular muscles are arranged in the medulla, in transversely parallel laminæ, which consist alternately of perpendicular and transverse systems, which pass through the muscles of the cortex to the mucous membrane.

It was also stated, that the human tongue and the ruminant form two types, the latter presenting root, body, and tip, the former wanting the tip. These two types, also, differ in the former possessing, and the latter wanting, a mesial fibro-cartilaginous septum.

2. On the Volcanic Formations of the Alban Hills, near Rome. By Professor J. D. Forbes.

The author thus sums up the general results of his memoir:—

"In the first place, it appears that the Alban volcano (for it is essentially one) has acted throughout a great period of time; for not only has it evidently repeatedly changed its form and materials of eruption, but it is surrounded by knolls of basaltic formations which seem to indicate very ancient and very repeated ejections, without taking the regular form of craters. Such are probably Monte Algido, Civita Lavinia, Monte Giove (Corioli), the Capuccini of Albano, Rocca Priore, Colonna, and perhaps even Capo di Bove, and several open craters, such as one a little below Albano, the Lago Cornufelle near Frascati, the Lake of Gabii, and one near Colonna, which, on the authority of Ponzi, appear to have ejected peperino. The horse-shoe form of the old crater of the Alban Mount, which, whether formed by the elevation process or not, appears to be composed of beds of basalt, lapilli, tuff, or peperino, and here and there of the lava called *Sperone*, gave way, like that of Somma, on the western or seaward side, and I cannot but think it in no small degree probable, that the vast lava beds which lie under Nemi and Genzano, and which dip at a small angle under Monte Cavo, are part of the dislocated walls of the ancient crater displaced by the convulsion which rent it on the western side, and which was accompanied by a prodigious fluid discharge of peperino, which then formed the strata of La Riccia and Albano, and which, overwhelming the broken-down wall of the ancient crater, formed at the same time the Monte Gentile, and the peperino beds above Nemi. This is confirmed by the prodigious lava blocks imbedded in these rocks, which bespeak the violence of the convulsion during which they were formed. Ages later, the present summit of Monte Cavo and the crater of the Campo d'Annibale were formed, and the latter gave out its currents of *tefrine* or grey basalt, and raised the crater of La Tartaruga and others in the valley of La Molara, and in the central crater; at the same time ejecting great volumes of pulverulent lapilli. It may have been coeval with these perfectly regular and comparatively modern eruptions, or it may have preceded them, that, after a period so long that the surface of the ancient eruptions of peperino were covered with vegetation and timber, the tremendous outbursts which forced open the craters of Albano and Nemi took place, the former producing some slight ejections of peperino or boiling mud, near Castel Gandolfo ; and at the same time a separate orifice, opening at the foot of Monte Cavo, may have discharged into the valley of Marino the remarkable variety of peperino described in this paper, and containing vegetable stems. A long, perhaps even a final, repose succeeded this paroxysm. Even from the very dawn of Italian history these scenes of previous turmoil and desolation appear to have enjoyed profound tranquillity, and to have been immemorially covered with impenetrable groves sacred to the sports of Diana.

" It will be seen, then, that we admit tufas or peperinos of three very different periods, one of which is coeval with, or even anterior to, the formation of the exterior cone, another largely developed, which accompanied the great breach in it towards the sea; and a third, which probably produced some local streams, such as that of Marino, which has evidently flowed since the ground took its present configuration, and was covered with plants. Of lavas, likewise, we must admit at least three periods; 1*st*, the compact basalts of the outer circuit, which, if Von Buch's theory be correct, have flowed under a less inclination than they at present have; 2*dly*, The well-marked leucitic, or partridge-eyed lavas, which form the interior circuit; and, 3*dly*, the compact basaltic lava which flows past Rocca di Papa towards Grotta Ferrata, which is possibly coeval with the dikes occurring at Capo di Bove and elsewhere. This leaves the origin of the *lava sperone* still uncertain. It is undoubtedly one of the more recent products, for it not only overlies the whole of the old basaltic series at Tusculum and Nemi, but the leucitic lavas of the newer cone at Rocca di Papa. The easiest solution would be to consider it as a scoriform basalt; but even to this there are difficulties, not only mineralogical, but from position. For how can we connect the mantle-shaped covering of Monte Cavo up to its highest point, with the basalt, which nowhere attains a height (so far as I know) within several hundred feet of it? It is still more difficult to conceive any continuity between the sperone of the central cone and that of Tusculum, which is separated from it by the great valley of La Molara.

The following gentleman was duly elected an Ordinary Fellow:—

W. J. M. Rankine, Esq., C.E.

The following Donations to the Library were announced:—

The Phenomena Diosemeia of Aratus, translated into English verse, with Notes. By John Lamb, D.D. 8vo.—*By the Author.*

Abstract of Exposition on the Strength of Materials. Read before the Royal Scottish Society of Arts at the request of the Council. By George Buchanan, F.R.S.E. 8vo.—*By the Author.*

Sopra alcuni punti della Teoria del Moto dei Liquidi. Memoria del Prof. P. Tardy. 4to.—*By the Author.*

Annalen der K. Sternwarte bei München, herausg. von Dr J. Lamont. Bde. 1 & 2. 8vo.—*By the Observatory.*

Journal of the Asiatic Society of Bengal. Edited by the Secretaries. Nos. 204 and 205. 8vo.—*By the Editors.*

Journal of Agriculture and Transactions of the Highland and Agricultural Society of Scotland. No. 27, N. S., January. 8vo. —*By the Society.*

Mémoires de l'Académie R. des Sciences, &c. de Belgique. Tom. XXVIII. 4to.

Annuaire de l'Académie R. des Sciences, &c., de Belgique. Tom. XV., 2me partie. Tom. 16me, 1re partie. 8vo.—*By the Academy.*

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. II. 1850. No. 36.

Monday, January 21, 1850.

Dr CHRISTISON, V.P., in the Chair.

The following Communications were read:—

1. On the Gamboge Tree of Siam. By Dr Christison.

Although Gamboge has been known in European commerce for nearly two centuries and a half, and its applications in the arts have been extended in recent times, the tree which produces it is still unknown to botanists.

The late Dr Graham, in 1836, was the first to describe accurately a species of Garcinia, which inhabits Ceylon, and which is well known there to produce a sort of Gamboge, not, however, known in the commerce of Europe. Resting on a peculiarity in the structure of the anthers, which are circumscissile, or open transversely by the separation of a lid on the summit, he constituted a new genus for this plant, and called it *Hebradendron cambogioides*. At the same period the Author examined the properties of this Gamboge, and found that it possesses the purgative action of the commercial drug in full intensity, and that the two kinds agree closely also, though not absolutely, in chemical constitution.

At an earlier period Dr Roxburgh described, in his "Flora Indica," another species of Garcinia, under the name of *Garcinia pictoria*, which inhabits the hills of Western Mysore, and which also was thought to produce a sort of Gamboge of inferior quality. In 1847 specimens of the tree and its exudation were obtained near Nuggur on the ghauts of Mysore by Dr Hugh Cleghorn of the East India Company's service; and the author, on examining the Gamboge, found it all but identical with that of Ceylon in physiological action, in properties as a pigment, and in chemical constitution. The same plant, with its Gamboge, was about the same time observed by the Rev. F. Mason, near Mergui in Tavoy, one of the ceded Burmese provinces.

A third species, inhabiting the province of Tavoy, and also producing a kind of Gamboge, was identified by Dr Wight in 1840 with Dr Wallich's *Garcinia elliptica*, from Sylhet, on the north-east frontier of Bengal. Its exudation was long thought to be of low quality. But, although this substance has not yet been examined chemically, it has been stated by Mr Mason to be, in his opinion, quite undistinguishable as a pigment from Siam Gamboge.

It is a matter of doubt whether Graham's character is sufficiently diagnostic to be a good generic distinction. But it was shewn by Dr Wight in 1840, that a well characterised section at least of the genus Garcinia consists of species which have "sessile anthers, flattened above, circumscissile, and one-celled;" and that all these species, and no others, appear to exude a gum-resin differing probably very little from commercial Gamboge.

Still the tree which produces Siam Gamboge, the finest and only commercial kind, continues unknown. A strong presumption however arose, that the last species was the Siam tree, as it grows in the same latitude with the Gamboge district of Siam, and not above 200 miles farther west. But if the information recently communicated to the author be correct, the Siam tree is a fourth distinct species of the same section. In December last he received from Mr Robert Little, surgeon at Singapore, specimens taken from two trees which were cultivated there by Dr Almeida, a resident of the colony, and which were obtained by him "direct from Siam" as the Gamboge tree of that country. These specimens are not such as to allow of a complete description; yet they are sufficient to shew that the plant presents the characters of Wight's Gamboge-bearing section of the genus Garcinia; but that it is not any of the species hitherto so fully described as to admit of comparison with it. The fruit is round, not grooved, crowned by a four-lobed knotty stigma, and surrounded by numerous sessile or subsessile aborted anthers, and by a persistent calyx of four ventricose fleshy sepals. The male flowers consist of a calyx of the same structure, a corolla of four ventricose fleshy petals, and a club-shaped mass of about forty subsessile anthers, closely appressed, connected only at the mere base, one-celled, flattened at the top, and opening by a circular lid along a line of lateral depressions; and there is no appearance of an aborted ovary amidst them. These are the characters of the three species presently known. These three species very closely resemble one another in general appearance and special characters. The new species presents the same close resemblance to them all; and, in particular, its foliage is undistinguishable from that of *Garcinia elliptica*, the leaves being elliptic, acuminate, and leathery, exactly as described and delineated by Wight. But it differs from them all in the male flowers and fruit being peduncled. The male flowers are fascicled, and have a slender peduncle three-tenths of an inch in length. The single young fruit attached to one of the specimens has a thick fleshy peduncle, like an elongated receptacle, half as long as the male peduncle. All the other species hitherto described have both male and female flowers sessile or subsessile. As this difference cannot arise from a mere variation in the same species, the plant must be a new one. The evidence however that it produces Gamboge, and more especially the commercial Gamboge of Siam, is not yet complete; and, until further information on this point be obtained, which the author expects to receive in the course of the year, it appears advisable not to attach to it a specific name. A question may even arise whether the male flowers and the fruit here described may not belong to two species instead of one; but this is far from probable.

2. Notice respecting a Deposit of Shells near Borrowstounness. By Charles Maclaren, Esq.

This deposit of shells is situated about a mile and a half west from Borrowstounness, where the Carse of Falkirk terminates in a strip of flat land a furlong in breadth. The shells are exposed in two openings, each about 300 feet long, made in the soil to procure limestone for Mr Wilson's iron-works. The bed can be traced in these openings along lines having an aggregate length of 1000 feet. Over all that space the shells form an unbroken stratum of very uniform depth (nearly three inches), and almost perfectly horizontal. They are covered by a bed of dark-brown sandy clay, from two to three feet thick, and rest on a deposit of the same substance, which closely resembles the mud spread over the present beach. The shells are all of one species, the cockle, or *Cardium edule*, and of various sizes down to the most minute. They are mixed with a portion of the clay which covers them, but lie so compactly, that they present to the eye the appearance of a layer of chalk nodules. Very few of them are fractured, and the two valves are generally united. The openings reach within 12 or 15 yards of the high-water line; but the number of broken shells seen on the beach shews that the bed had once extended farther northward, and that part of it has been cut away by the sea. The bed is at present about the level of high water, or a little above it, while the natural abode of the cockle, according to Mr Broderip, is from the low-water line to a depth of 13 fathoms. The continuity of the bed, its regular level, its remarkable uniformity, its composition confined to a single species, and the state of the shells, which are generally entire, and have the two valves united, shew that they are in their native locality, and prove that they could only have been brought to their present position by an upheaval of the land. This upheaval must have been to the extent at least of 18 feet, which is the difference betwixt high and low water, but very probably it was to the extent of 20, 30, or 40 feet. Inundations of the sea, caused by storms, have been called in to account for such deposits, but in my opinion very inconsiderately. That a sudden and violent movement of the sea should sweep away a bed of shells from its original locality, is intelligible enough; but that, while transporting them over some hundred feet or yards, it should preserve them unbroken, with the valves still united,—that the rushing water, instead of ploughing up the dry land it invaded, should smooth and level an area of more than an acre, then spread out the shells upon it with mathematical regularity, in an uninterrupted stratum of nearly uniform depth,—that, finally, it should cover them with a bed of clay two or three feet thick, and then withdraw;—these seem to me to be effects utterly irreconcileable with the known agency of floods. I would as soon believe that the West India hurricane, instead of levelling the planter's house, transports it *en masse*, with its walls, roof, and furniture all entire, from one end of a field to the other.

3. An Account of some Monstrosities. By the late Dr J. Reid. Communicated by Prof. Goodsir.

4. The Effect of Pressure in Lowering the Freezing-Point of Water experimentally demonstrated. By Professor W. Thomson, Glasgow.

On the 2d of January 1849, a communication, entitled "Theoretical Considerations on the Effect of Pressure in Lowering the Freezing-Point of Water, by James Thomson, Esq., of Glasgow," was laid before the Royal Society, and it has since been published in the *Transactions*, Vol. XVI., Part V. In that paper it was demonstrated that, if the fundamental axiom of Carnot's Theory of the Motive Power of Heat be admitted, it follows, as a rigorous consequence, that the temperature at which ice melts will be lowered by the application of pressure; and the extent of this effect due to a given amount of pressure was deduced by a reasoning analogous to that of Carnot from Regnault's experimental determination of the latent heat, and the pressure of saturated aqueous vapour at various temperatures differing very little from the ordinary freezing-point of water. Reducing to Fahrenheit's scale the final result of the paper, we find

$$t = n \times 0{\cdot}0135\,;$$

where t denotes the depression in the temperature of melting ice produced by the addition of n "atmospheres" (or n times the pressure due to 29·922 inches of mercury), to the ordinary pressure experienced from the atmosphere.

In this very remarkable speculation, an entirely novel physical phenomenon was *predicted* in anticipation of any direct experiments on the subject; and the actual observation of the phenomenon was pointed out as a highly interesting object for experimental research.

To test the phenomenon by experiment without applying excessively great pressure, a very sensitive thermometer would be required, since for ten atmospheres the effect expected is little more than the tenth part of a Fahrenheit degree; and the thermometer employed, if founded on the expansion of a liquid in a glass bulb and tube, must be protected from the pressure of the liquid, which, if acting on it, would produce a deformation, or at least a compression of the glas that would materially affect the indications. For a thermometer of extreme sensibility, mercury does not appear to be a convenient liquid; since, if a very fine tube be employed, there is some uncertainty in the indications on account of the irregularity of capillary action, due probably to superficial impurities, and observable even when the best mercury that can be prepared is made use of; and again, if a very large bulb be employed, the weight of the mercury causes a deformation which will produce a very marked difference in the position of the head of the column in the tube according to the manner in which the glass is supported, and may therefore affect with uncertainty the indications of the instrument. The former objection does not apply to the use of any fluid which perfectly wets the glass; and the last-mentioned source of uncertainty will be much less for any lighter liquid than mercury, of equal or greater expansibility by heat. Now the coefficient of expansion of sulphuric ether, at 0° C., being, according to *M.* I. Pierre,* ·00151, is eight or nine times that of mercury (which is ·000179, according to Regnault); and its density is about the twentieth part of the density of mercury. Hence a thermometer of much higher sensibility may be constructed with ether than with mercury, without experiencing inconvenience from the circumstances which have been alluded to. An ether thermometer was accordingly constructed by Mr Robert Mansell of Glasgow, for the experiment which I proposed to make. The bulb of this instrument is nearly cylindrical, and is about $3\frac{1}{2}$ inches long and $\frac{3}{8}$th of an inch in diameter. The tube has a cylindrical bore about $6\frac{1}{2}$ inches long: about $5\frac{1}{2}$ inches of the tube are divided into 220 equal parts. The thermometer is entirely enclosed, and hermetically sealed in a glass tube, which is just large enough to admit it freely. On comparing the indications of this instrument with those of a thermometer of Crichton's with an ivory scale, which has divisions, corresponding to degrees Fahrenheit, of about $\frac{1}{25}$th of an inch each; I found that the range of the ether thermometer is about 3° Fahrenheit; and that there are about 212 divisions on the tube corresponding to the interval of pressure from 31° to 34°, as nearly as I could discover from such an unsatisfactory standard of reference. This gives $\frac{1}{71}$ of a degree

* See Dixon on Heat, p. 72.

for the mean value of a division. From a rough calibration of the tube which was made, I am convinced that the values of the divisions at no part of the tube differ by more than $\frac{1}{30}$th of this amount, from the true mean value; and, taking into account all the sources of uncertainty, I think it probable that each of the divisions on the tube of the ether thermometer corresponds to something between $\frac{1}{68}$ and $\frac{1}{76}$ of a degree Fahrenheit.

With this thermometer in its glass envelope, and with a strong glass cylinder (Œrsted's apparatus for the compression of water), an experiment was made in the following manner:—

The compression vessel was partly filled with pieces of clean ice, and water: a glass tube about a foot long and $\frac{1}{10}$th of an inch internal diameter, closed at one end, was inserted with its open end downwards, to indicate the fluid pressure by the compression of the air which it contained: and the ether thermometer was let down and allowed to rest with the lower end of its glass envelope pressing on the bottom of the vessel. A lead ring was let down so as to keep free from ice the water in the compression cylinder round that part of the thermometer tube where readings were expected. More ice was added above, so that both above and below the clear space, which was only about two inches deep, the compression cylinder was full of pieces of ice. Water was then poured in by a tube with a stopcock fitted in the neck of the vessel, till the vessel was full up to the piston, after which the stopcock was shut.

After it was observed that the column of ether in the thermometer stood at about 67°, with reference to the divisions on the tube, a pressure of from 12 to 15 atmospheres was applied, by forcing the piston down with the screw. Immediately the column of ether descended very rapidly, and in a very few minutes it was below 61°. The pressure was then suddenly removed, and immediately the column in the thermometer began to rise rapidly. Several times pressure was again suddenly applied, and again suddenly removed, and the effects upon the thermometer were most marked.

The fact that the freezing-point of water is sensibly lowered by a few atmospheres of pressure, was thus established beyond all doubt. After that, I attempted, in a more deliberate experiment, to determine as accurately as my means of observation allowed me to do, the actual extent to which the temperature of freezing is affected by determinate applications of pressure.

In the present communication, I shall merely mention the results obtained, without entering at all upon the details of the experiment.

I found that a pressure of, as nearly as I have been able to estimate it, 8·1 atmospheres produced a depression measured by $7\frac{1}{2}$ divisions of the tube, on the column of ether in the thermometer; and again, a pressure of 16·8 atmospheres produced a thermometric depression of $16\frac{1}{2}$ divisions. Hence the observed lowering of temperature was $\frac{7\frac{1}{2}}{71}$, or ·106° F. in the former case, and $\frac{16\frac{1}{2}}{71}$, or ·232° F. in the latter.

Let us compare these results with theory. According to the conclusions arrived at by my brother in the paper referred to above, the lowering of the freezing-point of water by 8·1 atmospheres of pressure would be $8{\cdot}1 \times {\cdot}0135$, or ·109° F.; and the lowering of the freezing-point by 16·8 atmospheres would be $16{\cdot}8 \times {\cdot}0135$, or ·227° F. Hence, we have the following highly satisfactory comparison, for the two cases, between the experiment and theory.

Observed Pressures.	Observed Depressions of Temperatures.	Depressions according to Theory, on the hypothesis that the Pressures were truly observed.	Differences.
8·1 Atmospheres.	·106° F.	·109° F.	− ·003° F.
16·8 Atmospheres.	·232° F.	·227° F.	+ ·005° F.

It was, I confess, with some surprise, that, after having completed the observations under an impression that they presented great discrepancies from the theoretical expectations, I found the numbers I had noted down indicated in reality an agreement so remarkably close, that I could not but attribute it in some degree to chance, when I reflected on the very rude manner in which the quantitative parts of the experiment (especially the measurement of the pressure, and the evaluation of the division of the ether thermometer) had been conducted.

I hope, before long, to have a thermometer constructed, which shall be at least three times as sensitive as the ether thermometer I have used hitherto; and I expect with it to be able to perceive the effect of increasing or diminishing the pressure by less than an atmosphere, in lowering or elevating the freezing-point of water.

If a convenient *minimum* thermometer could be constructed, the effects of very great pressures might easily be tested by hermetically sealing the thermometer in a strong glass, or in a metal tube, and putting it into a mixture of ice and water, in a strong metal vessel, in which an enormous pressure might be produced by the forcing pump of a Bramah's press.

In conclusion, it may be remarked, that the same theory which pointed out the remarkable effect of pressure on the freezing-point of water, now established by experiment, indicates that a corresponding effect may be expected for all liquids which expand in freezing; that a reverse effect, or an elevation of the freezing-point by an increase of pressure, may be expected for all liquids which contract in freezing; and that the extent of the effect to be expected may, in every case, be deduced from Regnault's observations on vapour (provided that the freezing-point is within the temperature-limits of his observations), if the latent heat of a cubic foot of the liquid, and the alteration of its volume in freezing be known.

5. On the Extinction of Light in the Atmosphere. By W. S. Jacob, Esq., H.E.I.C. Astronomer, Madras. Communicated by Prof. C. Piazzi Smyth.

In a letter dated Madras, November 1849, Captain Jacob says, "I have been much interested in reading, lately, Professor Forbes's paper in the Philosophical Transactions, 1842, Part 2, on the Extinction of Light and Heat in the Atmosphere." As his results agree very closely with those of my experience on the Trigonometrical Survey of India, and which, though not founded on any precise measures, being still the conclusions of some years' experience, are perhaps worth noticing, particularly when they agree with the results of more exact measures.

On commencing work with heliotropes in 1837, I soon found that for long distances it was necessary to enlarge the apertures *more* than in the simple ratio of the distance (though such was Colonel Everest's practice); and before the end of the first season, I had formed a scale of apertures for corresponding distances, which afterwards needed very little alteration, but when finally corrected by subsequent years' observation, stood as follows :—

Aperture. Inches.	Maximum Distance. Miles.	Maximum Distance without Absorption.
0·5	15	15
1·0	23	30
2·0	33	60
4·0	45	120
8·0	60	240

Our heliotropes were circular glass mirrors, 8 inches in diameter; and for the smaller apertures, diaphragms were used between the heliotropes and the observer. At the distances stated the light was just visible to the naked eye in clear weather, and when seen over a *valley:* if the ray *grazed* near the surface, the light was much reduced. On one occasion I employed a heliotrope at $6\frac{1}{4}$ miles, and used an aperture of $\frac{1}{6}$ of an inch, and found it rather brighter than usual, so that probably $6\frac{1}{2}$ or 7 miles would be the normal distance for that size.

This agrees well enough with the rest of the scale, but there is no need to employ a conjectural quantity; and if the rate of absorption corresponding to the above be computed, so close an agreement will be found, as may entitle the numbers to be looked on as something better than mere estimates,—as the results, indeed, of a species of observation.

The mean of the whole shews a loss of ·0610 in passing through one mile of atmosphere; with the barometer at 27·0 inches (that being about the average height of my stations), but reduced to 30·0 inches, the quantity will be ·0671.

Hence the loss of light in passing from the zenith through a homogeneous atmosphere of 5·2 miles will be ·303, or only about one per cent. less than Professor Forbes's result. And as my air was considerably drier than his (the mean humidity being not much above ·30 instead of ·56), this will probably account for the difference; and, at any rate, the agreement is much closer than could have been expected.

I once mentioned this matter to Captain Waugh, the present Surveyor-General of India, then my fellow-assistant; but he not only had not noticed the thing, but did not even apprehend my meaning. He assented to my remark on the *loss* of light in passing through the atmosphere, but asserted that the aperture should vary as the distance, thus allowing for *no* loss! 0·1 inch per mile answered, he said, for all distances that he had tried! So it might answer for the distances most usually occurring on the Survey; for 4 inches would be proper for 40 miles, and 2 inches not much too bright at 20, and it is not often that these limits would be passed. Yet it is hardly possible to conceive that he should not have noticed the different intensity of the lights; had not his opportunities been perhaps rather unfavourable, as his work lay chiefly in plains, where, as mentioned above, the light of a grazing ray is very much reduced, and the atmospheric effect would therefore be mixed up with disturbing local causes.

I myself was much astonished at first discovering that the air had so great absorbent powers, and many ideas are suggested by the fact. We see at once how easily many of the planets may be rendered habitable to beings like ourselves. Mars, *e. g.*, may enjoy a temperature little inferior to our own, by having a *less* absorbent envelope; and Venus may be kept as cool as we are, by having one *more* so.

The following Gentlemen were duly elected Ordinary Fellows:—

Mr Alex. K. Johnston.
Dr John Scott, F.R.C.P.
Dr Sheridan Muspratt, Liverpool.

The following Donations to the Library were announced:

Annuaire Magnétique et Météorologique du Corps des Ingénieurs des Mines; ou Recueil d'Observations Météorologiques et Magnétiques faites dans l'entendue de l'empire du Russie, par A. T. Kupffer. Nos. 1 & 2, 1849. 4to.—*By the Russian Government.*

Verhandelingen der Eerste Klasse van het K. Nederlandsche Instituut van Wetenschappen, Letterkunde, en Schoone Kunsten te Amsterdam. 3de Reeks, Deel 1, Stuk 3 en 4. 4to.

Tijdschrift voor de Wis-en Natuurkundige Wetenschappen uitgegeven door de Eerste Klasse van het K. Nederlandsche Instituut van Wetenschappen te Amsterdam. 3de Deel, 1 & 2 Afleverings. 8vo.—*By the Academy.*

Jaarboek van het K. Nederlandsche Instituut van Wetenschappen, Letterkunde, en Schoone Kunsten te Amsterdam, 1847, 1848, 1849. 8vo.—*By the Academy.*

Catalogue of 2156 Stars, formed from the Observations made during Twelve Years, from 1836 to 1847, at the Royal Observatory, Greenwich. 4to.—*By the Royal Society, Lond.*

Proceedings of the Philosophical Society of Glasgow, 1848–9. Vol. III., No. 1. 8vo.—*By the Society.*

Quarterly Journal of the Chemical Society of London. No. 8. 8vo. *By the Society.*

Proceedings of the Royal Astronomical Society. Vol. X., No. 2. 8vo. —*By the Society.*

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. II. 1850. No. 37.

Monday, 4th February 1850.

The Hon. Lord MURRAY in the Chair.

The following Communications were read :—

1. Abstract of a Paper on the Hypothesis of Molecular Vortices, and its Application to the Mechanical Theory of Heat. By William John Macquorn Rankine, Civil Engineer, F.R.S.E., F.R.S.S.A., &c.

The object of this paper is to shew how the laws of the phenomena of Elasticity and Expansion, as connected with heat, may be reduced to mechanical principles by means of an hypothesis called that of Molecular Vortices.

The author ascribes the first distinct statement of an hypothesis of this kind to Sir Humphrey Davy, and refers to Mr Joule as having supported it; but he states that its consequences, to the best of his knowledge, have not hitherto been developed by means of the principles of Analytical Mechanics.

The author has endeavoured to do this, so far as the present state of experimental knowledge enables him, introducing such modifications into the hypothesis as are necessary in order to connect it with the undulatory theory of radiation. His researches were commenced in 1842, but were laid aside for nearly seven years from the want of experimental data, which, however, have at length been to a great extent supplied, so far as gaseous bodies are concerned, by the experiments of M. Regnault. The author has thus been enabled to resume his investigations, and has obtained formulæ, agreeing with experiment, and applicable to practice, for the expansion and elasticity of gases,—the elasticity of vapours in contact with their liquids,—the specific heat of gases,—tho heat produced by their compression,—the latent and total heat of evaporation,—the expansive action of vapours,—the power of the steam-engine,—and the mechanical value of heat in general.

One of the most useful in practice of those formulæ,—that for calculating the elasticity of steam and other vapours in contact with their liquids,—was published separately in the Edinburgh New Philosophical Journal for July 1849, with tables and a diagram, shewing its agreement with experiment, but without any account of the reasoning from which it is deduced.

The theory of radiant heat, like that of light, having been reduced to a branch of mechanics by means of the hypothesis of undulations, it is the object of the hypothesis of Molecular Vortices to reduce the theory of stationary heat also to a branch of mechanics, and thus to make a further step towards the fulfilment of the wish of Newton,—"UTINAM CÆTERA NATURÆ PHÆNOMENA EX PRINCIPIIS MECHANICIS DERIVARE LICERET."

The hypothesis of molecular vortices is defined to be that which assumes, *that each atom of matter consists of a nucleus or central point, enveloped by an elastic atmosphere, which is retained in its position by attractive forces, and that the elasticity due to heat arises from the centrifugal force of those atmospheres, revolving or oscillating about their nuclei or central points.* According to this hypothesis, *quantity of heat is the* vis viva *of the molecular revolutions or oscillations.*

The author, for the present, leaves indeterminate the following questions, as he has not as yet found it necessary to make any definite supposition respecting them.

First, Whether the elastic molecular atmospheres are continuous, or consist of discrete particles? This includes the question, Whether expansive elasticity is wholly the result of the mutual repulsions of particles, or is, to a certain extent, a primary quality of matter?

Secondly, Whether, at the centre of each atom, there is a real *nucleus* or extremely small central body, or a mere *centre* of condensation and force?

Thirdly, What are the figures of the orbits described by the particles of the atomic atmospheres in their revolutions or oscillations?

The author introduces into the hypothesis of molecular vortices a supposition peculiar to his own researches, for the purpose of connecting it with the undulatory hypothesis as to radiation. It is this: *That the vibration which, according to the undulatory hypothesis, constitutes radiant light and heat, is a motion of the atomic nuclei or centres, and is propagated by means of their mutual attractions and repulsions.* The absorption of light and of radiant heat, according to this supposition, is the transference of motion from the nuclei or centres to their atmospheres, and the emission of light and radiant heat, the transference of motion from the atmospheres to the nuclei or centres. The author enumerates several advantages which he conceives that this hypothesis possesses over the common supposition of a luminiferous ether pervading the spaces between ponderable particles.

The present paper refers solely to the condition of bodies in the state of gas or vapour. It is divided into two parts, the first of which treats of the Statical Relations of Heat and Elasticity, or their relations when both are invariable; and the second, of their Dynamical Relations, which take place when gaseous bodies expand or contract, and involve the principles of the mutual conversion of heat and expansive power, and those of the latent heat of expansion and evaporation.

The first section of the first part explains the general principles of the hypothesis, of which a summary has just been given.

The second section contains the mathematical investigation of the general equation between the heat and the elasticity of a gas. The total elasticity is divided into two parts, — the *superficial atomic elasticity*, being the elasticity of the atomic atmospheres at the bounding surfaces of the atoms, which is always expansive, and a function of density and heat, and an elasticity arising from the mutual forces exerted by separate atoms, which may be expansive or contractive, and in the perfectly fluid state is a function of density only.

The more substances are rarefied, that is to say, the more the forces which interfere with the operation of the elasticity of the atomic atmospheres are weakened, the more nearly do they approach to a condition called that of perfect gas, in which the total elasticity at a given temperature, is simply proportional to the density. This is therefore assumed to be the law of the elasticity of the atomic atmosphere of any given substance; so that the superficial atomic elasticity is held to be proportional to the density of the atomic atmosphere, at its bounding surface.

It is shewn, that although the form of such bounding surfaces in a perfect fluid is a rhombic dodecahedron, it may be treated without sensible error, in calculation, as if it were spherical, and the atmosphere of each atom may be conceived to be composed of concentric spherical layers, the density being uniform for each layer, but varying for different layers.

An oscillatory movement is supposed to be propagated from the nucleus or atomic centre in an inappreciably short time, to every part of the atmosphere, so that the mean velocity of movement is uniform throughout. The quantity of heat in one atom, or any other mass of matter, is expressed in terms of the force of gravity, by the weight of that mass, multiplied by the height through which it must fall at the earth's surface, in order to acquire that velocity. This oscillatory movement is conceived to be resolved into two components, one in the direction of radii passing through the atomic centre, the other performed in spherical surfaces described round that centre. The latter component alone produces centrifugal force; and it is afterwards shewn to be probable, that the ratio which the *vis viva* of this latter component bears to the whole *vis viva* of the oscillations, depends on the chemical constitution of the substance. The centrifugal force thus arising, has a tendency to increase the superficial density and elasticity of the atomic atmosphere, and must, at each layer of that atmosphere, be *in equilibrio* with the forces arising from the elastic pressure of the adjacent layers, and from the attraction towards the nucleus or centre. The condition of this equilibrium is expressed by a differential equation, which at the same time shews it to be stable. By the integration of that equation, there is obtained a general expression for the elasticity of a gas, in terms of its density and heat.

The first and largest term is simply proportional to the density of the gas, multiplied by a function, which varies as a certain fraction of the heat increased by a constant. In a perfect gas, this term constitutes the whole elasticity.

It is followed by an approximative converging series, chiefly negative, in terms of the reciprocals of the powers of the function of the heat before mentioned, representing the effect of the actions of the nuclei or centres in modifying the superficial-atomic elasticity. The numerators of the terms of this series are functions of the density, diminishing along with it, and requiring to be determined by experiment.

The last term of the expression represents the effect of the mutual action of separate atoms, and is a function of the density, to be determined by experiment.

The third section treats of Temperature and of Real Specific Heat. Bodies are defined to be *at the same temperature*, when the powers of their atoms to communicate heat are equal; and the proper *measure* of temperature is defined to be the elasticity of a perfect gas at constant volume, or its volume under constant pressure. Those quantities are, in all perfect gases, proportional to the temperature, as measured from a point 274·6 centigrade degrees, or 494·28 degrees of Fahrenheit's scale, below the temperature of melting ice. This point is called the *absolute zero*, and temperatures, as measured from it, *absolute temperatures*.

It is shewn from the equations in the preceding section, that absolute temperature, as thus defined, is simply proportional to the quantity of heat in one atom, *plus* a constant, multiplied by a constant coefficient. The constants depend on the nature of the substance, and the coefficient especially on its chemical constitution.

The reciprocal of this coefficient is, of course, the *real specific heat of one atom*, which, being divided by the atomic weight, gives the *real specific heat of unity of weight*.

The following laws, which have been to a great extent established experimentally by Dulong, are inferred from the theory—

That the specific heats of all simple atoms are either the same, or vary only in certain simple numerical ratios.

That the specific heats of atoms of similar chemical constitution are either the same, or vary only in simple numerical ratios.

The fourth section relates to the actual coefficients of elasticity and expansion of gases. The coefficient of increase of elasticity with temperature at constant volume, and the coefficient of expansion under constant pressure, are the same, and equal to each other, for every substance in the state of perfect gas, being the reciprocal of the absolute temperature of melting ice, (or ·00364166 per centigrade degree), when the volume and pressure at that temperature are respectively taken as units. The state of perfect gas, however, can be only approximated to in nature; for in all gases, especially the more dense and composite, the actions of the atomic nuclei or centres on their atmospheres, and of separate atoms upon each other, have more or less influence on the elasticity.

M. Regnault has made several elaborate series of experiments, to determine the deviations from uniform expansibility thus produced, in various gases.

The author, by applying his theory to data furnished by the experiments of M. Regnault, has obtained formulæ for the coefficients of expansion of atmospheric air, carbonic acid gas, and hydrogen, the results of which agree closely with those of observation, in every case in which a comparison is possible.

The fifth section treats of the elasticity of vapour in contact with the same substance in the liquid or solid state, or what is called the pressure of vapour at saturation.

The equilibrium of a substance filling a limited space, partly in the form of vapour, and partly in that of liquid or solid, is shewn to depend on three conditions.

The first condition of equilibrium is, that the total elasticity of the substance in the two states must be the same.

The second condition of equilibrium is, that the superficial elasticities of every two contiguous atoms must be the same at their surface of contact, and hence, that the superficial-atomic elasticity must vary continuously; so that, if, at the bounding surface between the liquid or solid and its vapour, there is an abrupt change of density, (as the reflection of light renders probable) there must there be two densities corresponding to the same superficial atomic elasticity.

The third condition of equilibrium is deduced from the mutual attractions and repulsions of the atoms of liquid or solid and those of vapour. In a gas in which the atomic centres are equidistant, the actions of the several atoms on each individual particle at an appreciable distance from the bounding surface of the gas, balance each other, and are accordingly treated as merely affecting the total elasticity by a quantity which is a function of the density; but near the bounding surface between a liquid or solid and its vapour, the action of the liquid or solid upon any atom must be greater than that of the vapour. A force is thus produced which acts on each particle in a line perpendicular to that bounding surface, and which is probably attractive towards the liquid or solid, very intense close to the bounding surface, but inappreciable at all perceptible distances from it. Such a force can be balanced only by a gradual increase of superficial-atomic elasticity in a direction towards the liquid or solid. Hence, although at perceptible distances from the liquid or solid, the density of vapour is sensibly uniform, the layers close to that surface are probably in a state of condensation by attraction, analogous to that of the earth's atmosphere under the influence of gravity.

Professor Faraday has expressed an opinion that certain well-known phenomena arise from a state of condensation of this kind, produced in gases by the superficial attraction of various solid substances.

This third condition of equilibrium is expressed by a differential equation, the integral of which, taken in conjunction with the first two conditions, would be sufficient to determine the respective densities, and the total elasticity of a liquid or solid and its vapour, when in contact with each other in a limited space at any temperature, provided we had a complete knowledge of the laws of molecular force. In the present imperfect state of that knowledge, the integral in question indicates the *form* of an approximate equation, expressing the logarithm of the elasticity of vapour at saturation, in terms of the reciprocals of the first and second powers of the absolute temperature, the coefficients of which the author has calculated empirically, for water and mercury, from the experiments of M. Regnault, and for alcohol, ether, turpentine, and petroleum, from those of Dr Ure,—three experimental data being required for each fluid, to calculate three constants. The agreement of the results of the formulæ thus obtained with those of experiment is as close as the uncertainties of observation render possible, throughout the whole range of pressures and temperatures observed. For steam, in particular, the coincidence is almost perfect. The author gives a table of the constants for the fluids enumerated, and refers to the Edinburgh New Philosophical Journal for July 1849, for the details of the comparison between calculation and experiment.

The section concludes with a speculation as to the probable effects of the atmospheres of dense vapours supposed to exist at the surfaces of solid and liquid bodies. The author conjectures that the presence of such atmospheres may be the cause which prevents solid bodies from cohering when brought together, and produces that resistance to contact which is visible not only in them but in drops of liquid. He conceives it possible that it may also be the cause of the "spheroidal state" of liquids at high temperatures, and may assist in maintaining the vesicular state, if such a state exists.

The sixth and last section of the first part relates to mixtures of gases and vapours of different kinds.

The principle stated in the second section *that the elasticity of the atomic atmosphere is proportional to its density*, is here expressed in the form, *that the elasticity of any number of portions of atomic atmosphere, compressed into a given space, is equal to the sum of the elasticities which such portions would respectively have if they occupied the same space separately.* It is shewn, that if this principle be considered true, not only of portions of atomic atmosphere of one kind of substance, but also of portions of atomic atmospheres of substances of different kinds, when mixed, it leads to the well-known laws of the elasticity and diffusion of mixed gases and vapours. He also speculates on the possibility of solid bodies, which have no perceptible vapours of their own at ordinary temperatures, acquiring the power of resisting cohesion by means of a superficial atmosphere of foreign substances.

The second part of the paper treats of the dynamical relations of the heat and the elasticity of bodies in the gaseous state.

The first section contains the general theory of the mutual conversion of heat and expansive power.

After recapitulating the mode of expressing quantities of heat in terms of gravity, the author refers to the experiments of Mr. Joule on the production of heat by electro-magnetic currents, by friction, and by the compression of air, as proving the convertibility of heat and mechanical power. He states reasons, however, for believing that the mechanical value of heat as deduced from those experiments (viz., from 760 feet to 890 feet per degree of Fahrenheit, applied to liquid water) is too large, owing to various causes of loss of power, and gives the preference to experiments in which no machinery is used, such as those on the velocity of sound, as data for such a calculation.

The laws of the production of heat by compression, and its consumption by expansion, are then deduced from the following two principles, the first of which is peculiar to the hypothesis of molecular vortices, while the second is a consequence of the law of the conservation of *vis viva*.

First, As every portion of an atomic atmosphere is urged towards the nucleus or atomic centre by a centripetal force equal to the centrifugal force arising from the oscillation which constitutes heat, it follows that, when by compression, each portion of such an atmosphere is made to *approach* the centre by a certain distance, the *vis viva* of its oscillation will be *increased* by the amount corresponding to that centrifugal force, acting through that distance; and conversely, that, when, by expansion, each portion of the atmosphere is made to *retreat from* the centre, the *vis viva* of its motion will be *diminished* by a similar amount.

Secondly, Let a portion of any substance undergo any changes of temperature, volume, and figure, and at length return to its primitive volume, figure, and temperature. Then, the absolute quantity of heat in the substance, the arrangement of the atoms, and the distribution of their atmospheres, being the same as at first, it follows that *the algebraical sum of the* vires vivæ *consumed and produced during the changes, whether in the shape of expansion and compression, or in that of heat, must be equal to zero ;* that is to say, if on the whole, a certain amount of mechanical power has appeared, and been given out from the body in the form of expansion, an equal amount must have been communicated to the body, and must have disappeared in the form of heat; and if a certain amount of mechanical power has appeared and been given out from the body in the form of heat, an equal amount must have been communicated to the body, and must have disappeared in the form of expansion.

From those principles the author deduces an algebraical expression of three terms. The first term represents the variation of heat arising from mere change of volume ; the second, the variation of heat produced by change of the distribution of the density of the atomic atmospheres dependent on change of volume ; and the third, the variation of heat due to change of the distribution of the density of the atomic atmospheres, dependent on change of temperature. In all those terms there is a common factor, bearing a constant ratio to the absolute quantity of heat in the body. In the first term, that factor is multiplied by the variation of the logarithm of the density of the body, and in the second and third by certain functions of the density and temperature depending on the law of the influence of molecular attraction and repulsion upon the superficial-atomic elasticity.

This section concludes by contrasting the author's theory with that of Carnót, which has hitherto been followed, either explicitly or virtually, in all calculations respecting the motive power of heat (except in the investigations of Mr Joule, already referred to), and of which a very clear and able account, with copious illustrations, was read before the Royal Society of Edinburgh, in January 1849, by Professor Thomson. Carnôt considers heat to be something of a peculiar kind, whether a condition or a substance, the total amount of which, in nature, is incapable of increase or diminution. It is not, therefore, according to his theory, convertible into mechanical power, but is capable, by its transmission through substances under particular circumstances, of causing mechanical power to be developed which did not before exist. According to the author's theory, on the contrary, as well as to every conceivable theory which regards heat as a modification of motion, the production of expansion by heat, and of heat by compression, consist in the transformation of mechanical power from one shape into another.

The second section relates to real and apparent specific heat, especially in the state of perfect gas. The apparent specific heat of a given substance is defined to be the sum of the real specific heat, and of that heat which is employed in producing those changes of volume and of molecular condition which accompany an elevation of one degree in the temperature of the substance. The same substance may therefore have different apparent specific heats, according to the manner in which the volume is made to vary with the temperature. The general algebraical expression for apparent specific heat is deduced from the equations of the preceding section. That expression being applied to the case of a perfect gas, or of a gas which may be treated in practice as sensibly perfect, it is shewn that the apparent specific heat of such a gas, at constant volume, is sensibly equal to the real specific heat, and that the apparent specific heat at constant pressure exceeds the specific heat at constant volume in a ratio which is sensibly constant for a given gas. Laplace's method of calculating this ratio from the velocity of sound is referred to, and applied to atmospheric air, oxygen, and hydrogen, using the correct coefficients of dilatation of those gases, as determined by M. Regnault.

The following laws, which have already been inferred from experiment by Dulong, are then deduced from the theory :

The specific heat of unity of volume, at constant volume, varies for different perfect gases inversely as the fraction by which the ratio of the two specific heats exceeds unity.

Equal volumes of all substances in the state of perfect gas, at the same pressure, and at equal and constant temperatures, being compressed by the same amount, disengage equal quantities of heat.

The data now obtained being employed to calculate the value of heat in terms of the force of gravity, it is found that the real specific heat of atmospheric air is equivalent to a fall of 238·66 feet per centigrade degree, and the apparent specific heat of liquid water at the temperature of melting ice (being what is commonly termed a thermal unit) to a fall of 1252 feet per centigrade degree, or 695·6 feet per degree of Fahrenheit.

The author next investigates the apparent specific heat of vapour at saturation. This quantity, according to his theory, is altogether different from the variation of the total heat of evaporation, with which, according to the theory of Carnôt, it is identical. It is in general *negative ;* so that if vapour at saturation is allowed to expand, being cut off from external sources of heat, a portion of it must be liquefied in order to supply the heat necessary for the expansion of the rest, in addition to the heat set free by the fall of temperature.

The third section treats of the latent and total heat of evaporation, especially for water.

It is in the first place proved, from the principle of *vis viva*, that the latent heats of evaporation and liquefaction, at a given temperature, are equal, with contrary signs.

The total heat of evaporation is defined to be the sum of the latent heat of evaporation, and of the heat required to raise the liquid to the temperature at which it is evaporated, from some arbitrary fixed temperature—(generally that of melting ice).

The law of variation of the total heat of evaporation with temperature is then deduced from the principle of the conservation of *vis viva*, which, as applied to this subject, takes the following form :—

Let a portion of fluid in the liquid state be raised from a certain temperature to a higher temperature ; let it be evaporated at the higher temperature ; let the vapour then be allowed to expand, *being maintained always at the temperature of saturation for its density, until it is restored to its original temperature, at which temperature let it be liquefied: then the excess of the heat absorbed by the fluid above the heat given out will be equal to the expansive power generated.*

From this principle it is deduced that when a vapour is sensibly in the state of perfect gas, and of very small density as compared with its liquid, the total heat of evaporation increases uniformly with the temperature, and the rate of increase is sensibly equal to the apparent specific heat of the vapour at constant pressure. This conclusion is verified by the experiments of M. Regnault upon the evaporation of water. As an additional verification of the theory, the real specific heat of steam is calculated from the total heat of evaporation, and also from the specific heat of atmospheric air; and the results of these two processes are found to agree exactly, being equal to 0·183 of the apparent specific heat of liquid water.

The fourth and last section of the second part is an investigation of the mechanical action of steam, treated as a perfect gas, and the power of the steam-engine.

The density of steam of saturation at 100° centigrade, is calculated from its chemical composition on the assumption of its being a perfect gas, and found to agree with the result of experiment, being $\frac{1}{1696}$ of the maximum density of water; and thence it is inferred that, in the absence of more precise data, steam at ordinary pressures may be treated in practice as a perfect gas, without material error.

The mechanical action of unity of weight of steam while entering a cylinder, and before it has begun to expand, is found by multiplying its pressure by its volume. The expansive action is next investigated, taking into account the liquefaction of a portion of the steam in supplying the heat required to expand the rest. The exact expression of this action is extremely complicated; but approximate formulæ of a more simple kind are given, suitable for calculating its amount with accuracy sufficient for practice, in different portions of the scale of pressures. From the sum of those two portions of power, deductions are made for the loss of power arising from clearance, and for the effect of the counter-pressure of the escaping steam. Thus is obtained the complete expression for the gross effect of unity of weight of steam, which, being multiplied by the weight of water effectively evaporated in unity of time gives the gross effect of the engine in unity of time. The result affords the means of calculating all the circumstances connected with the working of a steam-engine according to the principle of the conservation of *vis viva*, or, in other words, of the equality of power and effect, which regulates the action of all machines that move with a uniform or periodical velocity. This principle was first applied to the steam-engine by the Count de Pambour, and, accordingly, the formulæ of this paper only differ from those of his work in the expressions for the pressure and expansive action of the steam, which are results peculiar to the author's theory. As an illustration of the use of the formulæ, the maximum useful effect of a double-acting Cornish engine is computed, and compared with the result of the calculation of M. de Pambour for the same engine, shewing the latter to be too large by about one-fifteenth.

In an Appendix are given two tables; one for calculating the volume of steam from its pressure, and *vice versa*, and its mechanical action at full pressure, the other for computing the amount of its action in expansive engines.

In order to shew the limit of the possible effect from the expenditure of a given quantity of heat in evaporating water under given circumstances, the maximum gross effect of unity of weight of steam, evaporated at a higher temperature, and liquefied at a lower, is computed in two examples, and compared with the heat which disappears during the action of the steam, as calculated directly. In the first example, the water is supposed to be evaporated at the pressure of four atmospheres, and condensed at that of half an atmosphere; in the second, to be evaporated at eight atmospheres, and condensed at one atmosphere.

In both these examples, the direct calculation of the heat rendered effective, agrees with the calculation from the power developed, thus verifying the methods of computation founded on the author's theory.

The heat converted, in those examples, into engine-power amounts to only about *one-sixth part* of the heat expended in evaporating the water, the remainder being carried off by the steam and liquid water which escape from the cylinder. In practice, the proportion of heat rendered effective is still smaller, and in some unexpansive engines amounts to only *one twenty-fourth part*, or even less. It is thus shewn, that there is a waste of heat in the steam-engine, which is a necessary consequence of its nature. It can be reduced only by increasing the initial pressure of steam, and the extent of the expansive action; and to both these resources there are practical limits.

In conclusion of the present paper, the author states, that, from his equations, many additional formulæ are deducible, with respect to the specific heat of imperfect gases, to certain questions in meteorology, and to the specific heat of liquids; but from the want of sufficient experimental data, he conceives that they are not as yet capable of being usefully applied.

2. On Probable Inference. By Bishop Terrot.

The paper commenced with a suggestion, that, as the inferences of ordinary logic admitted no premises but such as were absolutely certain, and as the premises with which we have to deal in the business of life were not certain, but only probable, therefore it was highly desirable that we should have a logic, or rules for drawing inferences on the case of probable premises.

The attention of the Society was then drawn to the 15th section of the article Probabilities, in the *Encyclopædia Metropolitana*, and especially to the following passage: "It is an even chance that A is B, and the same that B is C; and therefore, 1 to 3 from these grounds only that A is C. But other considerations of themselves give an even chance that A is C. What is the resulting degree of evidence that A is C?" To which query the answer in the *Encyclopædia* is $\frac{5}{8}$.

On this passage it was observed, in the first place, that the asserted ratio of 1 to 3, or the probability $\frac{1}{4}$ in the first syllogism, was true only on the hypothesis that A can be C only through the intervention of the middle term B. But that when such is not the case, when other ways are conceivable but totally unknown, the probability is not $\frac{1}{4}$ but $\frac{1}{2}$; these two fractions representing, the one the probability of the evidence of a complete proof that A is C, the other the probability that A is C; and it was observed, that, in practical questions, it is the latter probability alone which we have an interest in determining.

It was then shewn generally, that, if the probabilities of the premises be $\frac{p}{q}$ for the first, and $\frac{p'}{q'}$ for the second; then the probabilities of the several possible combinations are,

1. A is B and B is C, with a probability of $\frac{pp'}{qq'}$ for A is C.
2. A is B and B not C, ... $\frac{pq'-pp'}{qq'}$ for A not C.
3. A not B and B is C, ... $\frac{p'q-pp'}{qq'}$
4. A not B and B not C, ... $\frac{pp'-p'q-pq'+qq'}{qq'}$

If A can be C only through the intervention of B, then the probability of the proposition A is C is $\frac{pp'}{qq'}$. But if A may be C in other unknown ways, we must add together all the probabilities arising from all the combinations, the result of which addition was shewn to be,

$$\text{Probability } for = \frac{4\,pp' + 3\,qq' - 3\,pq'}{6\,qq' - 2\,pq'}$$

$$\text{Probability } against = \frac{3\,qq' - 4\,pp' + pq'}{6\,qq' - 2\,pq'}$$

Whence it was inferred, that if $q' = 2\,p'$, or if the second premise have a probability of $\frac{1}{2}$, each of these fractions becomes $\frac{1}{2}$, or the probability that A is C becomes $\frac{1}{2}$.

It was then shewn that a weak argument, that is to say, one affording a probability of less than $\frac{1}{2}$, diminishes instead of increasing the probability arising from any previous argument or evidence; and it was proved, that even if we take $\frac{1}{4}$ for the probability arising from the first argument, the probability arising from both conjointly was not $\frac{5}{8}$ but $\frac{3}{8}$.

The general conclusions of the paper are as follows :—

1. When the premises, which, if certain, would involve the certainty of the conclusion, are not certain, but have each a known pro-

bability, the probability of the conclusion is the product of the probabilities of the premises, in those cases only where the presence of the middle term is *necessary* for the connexion of the major and minor terms. When this is not so, then the probability of the conclusion is the product of the probabilities of the premises, *plus* the sum of the probabilities arising from the other conceivable causes of connexion.

2. In a sorites of probable premises, any premise with a probability of $\frac{1}{2}$ brings the force of the argument up to that premise inclusive to a probability of $\frac{1}{2}$.

3. When various arguments of different validities have been advanced for a proposition, or when evidence has been brought in support of argument, or argument of evidence, the resulting probability is not the *sum*, but the *average* of the several probabilities ; so that a weaker argument following upon a stronger, weakens it, or rather weakens the probability produced by it.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. II. 1850. No. 38.

Monday, 4*th February* 1850 (*continued*).

The Hon. LORD MURRAY, V.P., in the Chair.

3. On the Ante-Columbian Discovery of America. By Dr Elton. Communicated by Dr Traill.

The object proposed by Dr Elton, is a summary of the knowledge we possess on the discovery of the Continent of America, by several adventurous European voyagers, anterior to the time of Columbus.

This subject, which has been for almost a century and a half well known to the students of northern history, was first made known to the rest of Europe by the publication of the *Vinlandia Antiqua* of the celebrated *Torfæus* in 1705; and most of the facts given by Dr Elton are extracted from that work. *Torfæus* proved from existing Icelandic MSS., that America was discovered, and even attempted to be colonized, by his enterprising countrymen, in the end of the tenth and beginning of the eleventh century; and the descriptions transmitted to us prove that they landed on what are now Newfoundland, Nova Scotia, Massachusetts, and Rhode Island.

The first adventurer was *Leif*, the son of *Eirik the Red*, who, in A.D. 995, when attempting to pay a visit to his father, the colonizer of Greenland, was driven by stress of weather to the coast of Newfoundland, which he named *Helluland*, or *Rocky Land*. From that he sailed south-westward, till he arrived at a country, which from being covered with wood, he denominated *Markland*; and which, from the course and length of his voyage, is believed to be a part of Nova Scotia. Pursuing his course southwards, he reached a portion of Massachusetts, not far from Cape Cod; and coasting along this, he took up his winter quarters in a fertile country, which, from his description, is easily seen to have been about Rhode Island. This region, from the discovery of a species of wild vine found there, he termed *Vinland*. In the summer, he fitted out his vessel, and sailed to join his father in Greenland.

The fame of his discovery induced his brother *Thorwald*, in A.D. 1002, to sail for *Vinland*, intending to settle there; but in one of his excursions he encountered and was slain by a people, the Icelanders, in contempt denominated *Skrelings*, evidently Esquimaux, who then appear to have possessed the shores far to the south of their present location.

The next and most remarkable voyage to Vinland, was that of *Thorfinn Karlsefne*, which took place in A.D. 1006. He carried with him his wife, and one hundred and thirty-one followers, and domestic animals, with the intention of establishing a colony at the huts built by Leif in Rhode Island. The soil and climate were suitable, and they remained in that country till 1011, when they were attacked by a vast number of *Skrelings*, whom they repulsed; but the hostility of the natives induced him to abandon his design, and he finally settled in Iceland. *Thorfinn*, however, had a son, *Snorro*, born in America, from whom some of the most distinguished families in Iceland are lineally descended.

After this period, it appears that there were many voyages to *Vinland*, and that Iceland sent colonies thither for more than a century; for it is stated in Icelandic MSS., that *Eirik*, bishop of Greenland, went to *Vinland* in A.D. 1125, to confirm the colonists in the Christian faith.

The work of *Torfæus* also gives us a singular account of Icelandic voyages to a country, either a continent, or a vast island, lying far to the west of the British Islands, and near Vinland. It seems to have been first visited by *Are Marson* in A.D. 983, who was driven there by a great storm. He named it *Huitramannaland*, or *Land of White Men*, from the complexion of the natives, who were also Christians; and *Are* himself was then converted from the worship of Odin to the religion of Christ.

The same land was visited afterwards by *Gudleif Gudlagson*, an Icelandic trader with Ireland; who, in a voyage from Dublin to Iceland, was driven by a tempest to a far western land, where he was taken prisoner by the natives, but delivered by their chief, who turned out to be an *Icelander*. He was dismissed with presents, but forbidden to return. The natives were *white*, and seemed of European extraction, with a dialect like that of *Ireland;* and the American archæologists, with considerable reason, have considered that their *whereabout* was on some part of the new world, between the Chesapeake and Florida.

These early voyages seem to us very surprising; but they do not seem at all foreign to the habits and enterprise of the bold Icelanders of those ages; who not only traded to every part of the west of Europe, but to the Mediterranean, and explored *Baffin's Bay*, as high as *Lancaster Sound*. We have now a certain proof, that they were at least as high in it as at 72° 55′; for in 1825, a memorial stone with a Runic inscription, and the date of 1131, was found on the island of *Kingiktersoak*.

Several Runic inscriptions are said to have been found in America; but the most remarkable of these is the mass of *greywacke* on the shores of the river at *Dighton*, in the township of *Berkley*, in Massachusetts, not far from the supposed site of the settlement of *Thorfinn Karlsefne*. This has been lately carefully figured and engraved in the *Antiquitates Americanæ* of the Royal Soicety of Northern Antiquaries of Copenhagen, and repeated in *Jacob Aal's* translation of the *Chronicles of Snorre Sturleson*. Dr Elton, who has examined the original, assures us, that this engraving is a faithful transcript. On this rock, antiquaries read, amid figures supposed to represent Thorfinn, his wife and child, and his companions, the letters—*ϙrfin* and *cxxxi*, the number of his companions.

Dr Elton next adverted to the voyage of the Welsh Prince, *Madoc*, son of the greatest of the princes of North Wales *Owen Gwenedd*, about the year 1170. This voyage, though doubted by many, is fully believed in by Dr Elton, and it is noticed by Hakluyt, Purchas, Broughton, &c. Dr Elton quotes the singular story given by the Rev. Morgan Jones, chaplain to the British commander of the forces of Virginia in 1669. Jones was taken prisoner by the Tusscarora Indians, who intended to torture him in their usual way, when he began to lament his cruel fate *in Welsh*, which was understood by the Indians, and he was suffered to depart in peace. These, Dr E. thinks, may have been descendants of *Madoc's followers;* and he seems inclined to ascribe to them also those very remarkable mounds, fortifications, and enclosures, which are found in such quantity in the valleys of the *Mississippi* and the *Ohio*. He is inclined also to trace to these Welsh adventurers, or at least to some early Europeans, the now almost extinct tribe, the *Mandans*—a people fairer and handsomer than the *Red men*,—that are now found 1800 miles above St Louis, on the Missouri, as described by Lewis and Clarke, and Catlin, the American travellers.

These, and several other circumstances, which might have been adduced, prove that Columbus cannot be regarded as the original discoverer of the New World.

The following Donations to the Library were announced:

Philosophical Transactions of the Royal Society of London, 1849. Part II. 4to.—*By the Society.*

Kongl. Vetenskaps. Akademiens Handlingar, för 1847 & 1848. 8vo.

Årsberättelser om Botaniske Arbeten och Uptäckter för 1843 & 1844. 8vo.

Årsberättelse om Framstegen i Kemi under År. 1847. 8vo.

Årsberättelse om Technologiens Framsteg. 1842, 1843, 1844, 1846. 8vo.

Öfversigt af Kongl. Vetenskaps. Akademiens Forhandlingar. 1848. 8vo.—*By the Academy.*

Monday, 18*th February* 1850.

The Right Rev. BISHOP TERROT, V.P., in the Chair.

The following Communications were read:—

1. On the Equilibrium of Elastic Solids. By James Clerk Maxwell, Esq. Communicated by the Secretary.

This paper commenced by pointing out the insufficiency of all theories of elastic solids, in which the equations do not contain two independent constants deduced from experiments. One of these constants is common to liquids and solids, and is called the modulus of *cubical* elasticity. The other is peculiar to solids, and is here called the modulus of *linear* elasticity. The equations of Navier, Poisson, and Lamé and Clapeyron, contain only one coefficient; and Professor G. G. Stokes of Cambridge, seems to have formed the first theory of elastic solids which recognised the independence of cubical and linear elasticity, although M. Cauchy seems to have suggested a modification of the old theories, which made the ratio of linear to cubical elasticity the same for all substances. Professor Stokes has deduced the theory of elastic solids from that of the motion of fluids, and his equations are identical with those of this paper, which are deduced from the two following assumptions.

In an element of an elastic solid, acted on by three pressures at right angles to one another, as long as the compressions do not pass the limits of perfect elasticity—

1*st*, The sum of the pressures, in three rectangular axes, is proportional to the sum of the compressions in those axes.

2*d*, The difference of the pressures in two axes at right angles to one another, is proportional to the difference of the compressions in those axes.

Or, in symbols:—

$$1.\quad \left(P_1 + P_2 + P_3\right) = 3\,\mu\left(\frac{\delta x}{x} + \frac{\delta y}{y} + \frac{\delta z}{z}\right)$$

$$2.\quad \begin{cases} \left(P_1 - P_2\right) = m\left(\dfrac{\delta x}{x} - \dfrac{\delta y}{y}\right) \\ \left(P_2 - P_3\right) = m\left(\dfrac{\delta y}{y} - \dfrac{\delta z}{z}\right) \\ \left(P_3 - P_1\right) = m\left(\dfrac{\delta z}{z} - \dfrac{\delta x}{x}\right) \end{cases}$$

μ being the modulus of *cubical*, and m that of *linear* elasticity.

These equations are found to be very convenient for the solution of problems, some of which were given in the latter part of the paper.

These particular cases were—

That of an elastic hollow cylinder, the exterior surface of which was fixed, while the inferior was turned through a small angle. The action of a transparent solid thus twisted on polarized light, was calculated, and the calculation confirmed by experiment.

The second case related to the torsion of cylindric rods, and a method was given by which m may be found. The quantity $E = \frac{9\,m\,n}{m + 6\,n}$ was found by elongating, or by bending the rod used to determine m, and μ is found by the equation,

$$\mu = \frac{E\,m}{9\,m - 6\,E}.$$

The effect of pressure on the surfaces of a hollow sphere or cylinder was calculated, and the result applied to the determination of the cubical compressibility of liquids and solids.

An expression was found for the curvature of an elastic plate exposed to pressure on one side; and the state of cylinders acted on by centrifugal force and by heat was determined.

The principle of the superposition of compressions and pressures was applied to the case of a bent beam, and a formula was given to determine E from the deflection of a beam supported at both ends and loaded at the middle.

The paper concluded with a conjecture, that as the quantity ω, (which expresses the relation of the inequality of pressure in a solid to the doubly-refracting force produced) is probably a function of m; the determination of these quantities for different substances might lead to a more complete theory of double refraction, and extend our knowledge of the laws of optics.

2. Two Letters from W. E. Logan, Esq., to Earl Cathcart.

These letters were dated in August 1846 and September 1847. Earl Cathcart intended himself to have read them to the Society, but, having been prevented by his official duties from coming to Edinburgh, had sent them, to be communicated in his name.

In the first letter, the author, who had been sent to examine the geology of Canada, describes a visit which he made, on his way to Fort-William, Lake Superior, to the silver and copper mines on the south side of the lake, in the territory of the United States.

He considers the formation in which the mines occur as being older than the new red. They consist of parallel ranges of trap and conglomerate, apparently interstratified. They are well displayed at and near Copper Harbour. They are sometimes so thick as to form mountain ranges. The conglomerate consists of trap pebbles in trap sandstone; the trap is sometimes compact, at other times amygdaloidal. The strata run in a curvilinear direction. They dip to the north, with a slope of 16° to 30°, the veins are at right angles to the strata, and run nearly north and south. The veinstones are steatite, quartz, calcspar, and zeolites. The ores are those of copper, silver, iron, and lead, the two former being productive. The two metals are chiefly native, but occur also in other forms. They are least abundant in the conglomerate, more so in the trap, most of all in the amygdaloid. They are found also in amygdules of the rock near the veins. The quantity of native copper is very great. In the Copper Falls Mine, near Eagle Harbour, on sinking a pit to 72 feet, the compact trap and amygdaloid were found to alternate six or seven times. The main vein was 18 to 20 inches thick. In the shaft, about 40 feet down, a mass of native copper was found, of which the dimensions were estimated by the author, *in situ*, to indicate a weight of about 30 tons. It had not yet been found possible to remove it. A diagram of its position was given.

From other shafts in the vicinity, much copper had been extracted, but with prodigious difficulty, from the tough metal binding the rock firmly together, and rendering blasting useless. The author saw, on the surface, besides many pieces of 25 lb., seven masses, varying from 75 to 1200 lb., and weighing in all 4000 lb., or nearly 2 tons. Native silver is found with the copper, and a mass of 3¼ lb. had been obtained. The author saw one of 1¼ lb. The author is of opinion that the very richness of this mine in native copper may render it unproductive, from the difficulty and expense of working it.

In the Cliff Mine, on Eagle River, there is the same abundance of copper, with more silver. Part of the rock was said to yield 7 per cent. of silver; but subsequently was found hardly to pay for its extraction. The author saw here a mass of silver of 3½ lb. Every vein in the trap seen by the author contained native copper. At the Eagle River Mine, silver is found in large masses, one of which weighed 7 lb. 2 oz.

On the Canada side, as far as the author had then examined it, from Fort-William to Pigeon River, indurated shale prevails, overlaid by greenstone, with patches of porphyry. Many trap-dikes are seen, forming long narrow promontories and deep harbours on the shore of the lake. A system of veins occurs at right angles to the dikes, containing barytes, in addition to the veinstones formerly mentioned. The veins vary from 6 inches to 20 feet. In one of them 14 or 15 feet thick, well seen on Spar Island, gray sulphuret of copper occurs in considerable abundance, especially in a part of the vein, nearly 5 feet thick. Native silver also occurs in small quantity. There are also veins parallel to the dikes which contain ores of copper. But the author could not form a decided opinion in 1846 of the value of these mines.

In the second letter, he gives some of the results of an examination of the eastern townships of Canada, from Lake Champlain and the Richelieu to the Chaudiere. He observed facts proving the green mountains of Vermont to be more recent than the Loraine shales, or Hudson River group. Of the upper rocks, the most interesting was a band of serpentine, 150 to 400 yards broad, which the author traced continuously for 150 miles, and which probably extends as far again. It has occasionally rich beds of magnetic iron and of chromate of iron; of the latter, a boulder was found, weighing 6 cwt. The gravel on the Chaudiere, besides these minerals, contains titaniferous iron and gold. The author expects to find platinum, as the gravel in all other respects resembles that of the Russian auriferous district. The auriferous sand is found on the tributaries of the Chaudiere. It will probably pay for extraction. 60 bushels washed by Mr Derby, yielded 18 dwt. 8 gr., or about 1s. 6d. worth per bushel. The gold has not yet been found *in situ.*

3. Notes on Practical Chemical subjects. By Alexander Kemp, Esq. Communicated by Professor Gregory.

1. *On the Purification of Sulphuric Acid.*

The author, after describing the different methods, recommended for purifying sulphuric acid from nitric acid, namely, boiling with a little sugar, and heating with sulphate of ammonia, both of which had proved troublesome and imperfect, stated, that after trying various plans, the only one which he found to answer well, was the action of sulphurous acid on the oil of vitriol, after diluting it to the sp. gr. of 1·715, or lower. He adds one volume of water to three of the oil of vitriol, passes sulphurous acid gas through the hot liquid till it is in excess, and then boils off the excess of sulphurous acid; or, still better, three volumes of oil of vitriol are added to or diluted with, one of a saturated solution of sulphurous acid in pure water, and boiled. The acid is thus so perfectly purified from nitric acid, that when used for making hydrochloric acid, it yields a product quite colourless, which was not the case with the oil of vitriol purified by any other process.

If the oil of vitriol be diluted with one-half its volume of sulphurous acid solution (or of water, previously to passing the gas through it), the sulphate of lead is also totally separated, and the clear liquid, decanted from the precipitate, and boiled down to sp. gr. 1·845, is colourless, and almost chemically pure.

2. *On the Preparation of Pure Hydrochloric Acid.*

Professor Gregory, in his process for preparing hydrochloric acid, by heating 1 equivalent of sea-salt with 2 equivalents of sulphuric acid of sp. gr. 1·650, directs the use of patent salt, to avoid the presence of iron in the product.

The author observed, that there is always a certain quantity of iron in the residue, even when patent salt is used; but that none passes over with the hydrochloric acid. He then added iron and peroxide of iron in considerable quantity to the materials. Still no iron passed over. It would appear, that when iron had been observed by Professor Gregory in minute quantity, in the hydrochloric acid made by his process, from common salt, it had either passed over at the very end of the process, when the temperature rose very high, although the author could not, in his own experiments, observe this, or, more probably, had been present in the test employed. It is probable that, even when much iron is present in the materials, the presence of the excess of sulphuric acid, and also the low temperature at which the process goes on, prevent the formation of the chloride of iron.

The author's observations enable us to prepare, from the commonest and cheapest salt, perfectly pure and colourless hydrochloric acid, and thus still further to reduce the price of this reagent, so essential to the chemist.

Professor Gregory also briefly stated some observations by Mr Kemp and himself, on the purification of chloroform, which he was to describe more fully at a subsequent meeting.

Dr Stark

Was balloted for, and duly and unanimously re-elected a Fellow of the Society.

Monday, 4th March 1850.

General Sir THOMAS MAKDOUGALL BRISBANE, Bart., President, in the Chair.

The following Communications were read:—

1. Analysis of the Anthracite of the Calton Hill, Edinburgh. By Dr A. Voelcker. Communicated by Dr George Wilson.

Dr Voelcker observed, in the introduction to his paper, that we are in possession of analyses of anthracite from different localities, from which it appears that different specimens vary much in the proportion, but very little in the nature, of their ingredients. All samples of anthracite which have been analysed, have been found to contain carbon, oxygen, hydrogen, and nitrogen, as well as more or less inorganic matter. Sulphur also has generally been found, at least when sought for; but it does not appear in many recorded analyses.

The anthracite employed in the following analyses was furnished by Dr Fleming, and first carefully dried, after being finely powdered, by exposing it for several hours to a current of dry air, at a temperature of 230° F. The carbon and hydrogen were ascertained, by burning from three to four grains of the mineral with a mixture of oxide of copper and oxide of lead, which is much less hygroscopic than the pure oxide of copper. A mixture of this oxide and chlorate of potass was also placed in the shut end of the combustion-tube, from which oxygen was evolved in the usual way towards the close of the process.

The nitrogen was determined by Will and Varentrapp's method. The sulphur was ascertained by projecting into a red-hot platina crucible, in successive small quantities, a mixture of anthracite in powder, with nitrate of potass and carbonate of soda, and afterwards maintaining the product of deflagration at a high temperature for some time. The resulting fused mass which was perfectly white, was dissolved in water, super-saturated with hydrochloric acid, and precipitated by chloride of barium.

About ten grains of the mineral were employed in the determination of the amount of ash. It was red, and contained oxide of iron.

The following are the results of the analysis:—

Carbon, . . .	91·23
Hydrogen, . .	2·91
Nitrogen, . .	0·59
Oxygen, . . .	1·26
Sulphur, . . .	2·96
Ash, . . .	1·05
	100·00

The most remarkable peculiarity of the Calton Hill anthracite, as appears from the results given above, is the large proportion of sulphur it contains, amounting to nearly 3 per cent. Sulphur has been supposed to occur in the different varieties of coal in combination with iron, as pyrites, but the trace of that metal present in the Calton Hill anthracite is so small, that the sulphur must have been combined with the organic constituents of the mineral.

Note on the Crystallisation of Carbon, and the possible derivation of the Diamond from Anthracite and Graphite. By Dr George Wilson.

The author stated that the object of his communication was, to suggest the possibility of anthracite as well as graphite being substances from which the diamond is developed. After referring to previous theories, as all assuming that carbon must have been fluid or semifluid, before it crystallised, he stated that his hypothesis contemplated the possibility of graphite, as well as amorphous carbon, and its solid combinations, such as anthracite, undergoing crystallisation into the diamond, without losing their solidity during the change. He thought anthracite more likely than most substances to yield the diamond, for the following reasons:—

Firstly, As it occurs in nature, in many localities, it is found passing by insensible gradations, on the one hand, into common coal, on the other, into graphite; so that it may be regarded as representing the transition-state from fossilised vegetable matter to pure carbon, and as tending, under the influence of certain agencies, to change ultimately into the latter.

Secondly, The chief element of anthracite is carbon, of which it frequently contains 91, and sometimes 95 per cent.

Thirdly, Its other ingredients (with the exception of the ash, which is often under one per cent.), namely, hydrogen, oxygen, nitrogen, and sulphur, form volatile compounds with each other, and with the oxygen of the air, so that by a slow process of spontaneous decomposition, and gradual oxidation or eremacausis, all the constituents of the anthracite, except the excess of carbon and the ash, may be evolved, and carbon left free.

The separation, in this way, of the non-carbonaceous elements of the anthracite would be attended with a disturbance of the molecular equilibrium of the mineral, which would necessitate a new arrangement of its particles, and might determine the induction of the crystalline condition characteristic of the diamond. During this process, the inorganic saline matter, or ash, would either be excluded by the power crystallising bodies are known to possess of expelling heterogeneous matter, or be included in the crystallising carbon. Either view would consist with observation ; for whilst some diamonds appear to be pure carbon, many leave a slight ash when burned in oxygen.

The author further observed, that whether anthracite will crystallise into graphite or diamond, will be determined chiefly by the temperature at which crystallisation occurs, and the rapidity with which it proceeds. Graphite represents the condition of most stable equilibrium, which the crystalline molecules of carbon assume, when aggregated rapidly at a high temperature. The diamond, on the other hand, has all the characters of a crystal which has formed very slowly at a lower temperature, and it will not change into graphite, unless it be suddenly exposed to an intense heat. Whenever, therefore, carbon crystallises very slowly at ordinary temperatures, it may be expected to become the diamond rather than graphite, and the latter must be considered as a substance which, when not maintained at an elevated temperature, is liable to re-arrange its particles in the condition of more stable equilibrium characteristic of the diamond. The author, at the same time, observed, that he did not seek to affirm that all diamonds had been produced from anthracite or graphite, but thought it, on the other hand, probable, that, like other crystallisable substances, carbon might be crystallised in various ways.

2. On the Proportion of Fluoride of Calcium present in the Baltic. By Professor Forchammer of Copenhagen. With some preliminary Remarks on the presence of Fluorine in different ocean waters. By Dr George Wilson.

Dr Wilson reminded the Society that he had announced to them in 1846 the occurrence of fluorine in the water of the Frith of

Forth, and mentioned, that, in the preceding summer, he had found it in deposits obtained during the evaporation of sea-water from the Frith of Clyde, and the German Ocean. Professor Forchammer had made similar observations on the Baltic, and had furnished Dr Wilson with the account of them which follows. Before reading this, he wished to add, that he had recently examined incrustations from the boiler of a steam-vessel sailing between Liverpool and Dublin, and similar deposits from the Canada Transatlantic steamer, and H.M. war-steamer Sidon, which had been three years on the Mediterranean station. The different crusts were, without preliminary treatment, except reduction to powder, heated with oil of vitriol, and were found to yield an acid vapour which etched glass. Specimens of glass, in illustration, were shewn to the Society. From these observations, Dr Wilson inferred the presence of fluorine in the Friths of Forth and Clyde, in the German Ocean, the Irish Sea, the Atlantic, and the Mediterranean. He then proceeded to read Professor Forchammer's communication, which follows. It is dated, Copenhagen, 20th December 1849.

Abstract of a Paper by Professor Forchammer, on the rarer Substances which occur in Sea-water.

Fluorine and Phosphoric Acid.

100 lb. of sea-water, as it occurs in the Sound, near Copenhagen, of which the average quantity of salts is between 2 and $2\frac{1}{2}$ per cent., was evaporated. When the solution was so concentrated that it began to deposit salt, it was, without filtering it, mixed with an excess of ammonia, and the precipitate collected and washed. The whole precipitate which contains carbonate, sulphate, and phosphate of lime, fluoride of calcium, silica, and magnesia, was redissolved in muriatic acid, which left the greater part of silica undissolved; the solution was mixed with muriate of ammonia, and a second time precipitated by an excess of ammonia. This precipitate from 100 lb. of sea-water weighed 3·104 grains, and consisted of phosphate of lime and fluoride of calcium. It was divided into two equal parts, of which the one was in a platina crucible, mixed with concentrated sulphuric acid, and allowed to act on a slip of glass, covered with wax, in which some words were scratched with a copper needle. The glass was most decidedly etched, but the words appeared more clear

and legible if breathed upon. The second half part was likewise mixed with sulphuric acid, but in a bent tube, and distilled into a small vessel which contained a weak solution of ammonia. The tube was etched, and the vessel contained precipitated silica. It was thus completely proved that sea-water contains fluoride of calcium, but the quantity in 100 lb. sea-water from the Sound at Copenhagen can hardly exceed one-half of a grain, or since the proportion of the different salts varies very little in sea-water, it will be about one grain in 100 lb. of water of the ocean, which contains between 3·5 and 4 per cent. of salts.

All the residuums from the trials to find fluorine were dissolved in muriatic acid, and thrown down by an excess of ammonia. The precipitate, washed, dried, and heated, was mixed with potassium in a glass tube [and heated], until the excess of potassium was driven off. The lower part of the tube was cut off and thrown into water, where it for hours continued to give out small bubbles, distinguished by the peculiar smell of phosphuretted hydrogen, although they did not inflame by themselves. Thus the existence of phosphoric acid was likewise proved, although I could not try the delicate test for phosphoric acid which we owe to Mr Svanberg, it not being known at the time when I made my experiments.

In all the different species of corals which I analysed, I likewise found fluorine.

In a postscript to the preceding communication, Professor Forchammer states, that the paper, of which it is an abstract, "contains experiments on many other substances, contained in minute quantities, in sea-water; for instance, manganese, ammonia, baryta, or strontia, besides iron and silica, which occur in proportionally large quantities."—G. W.

3. On an Application of the Laws of Numerical Harmonic Ratio to Forms generally, and particularly to that of the Human Figure. By D. R. Hay, Esq.

The author stated in some prefatory remarks, that a belief in the operation of the laws of numerical harmonic ratio in the constitution of beautiful forms had long existed, although those laws had not been systematised so as to render them applicable in the formative arts. In proof of this, Mr Hay quoted a correspondence upon the subject of harmonic ratio, between Sir John Harrington and Sir Isaac Newton, in which the latter expresses his belief in such laws in the following words : "I am inclined to believe some general laws of the Creator prevailed with respect to the agreeable or unpleasing affections of all our senses; at least the supposition does not derogate from the power or wisdom of God, and seems highly consonant to the simplicity of the macrocosm in general." The belief of this great philosopher, the author trusted, would form some apology to men of science for the repeated attempts he has made to establish the fact. These attempts he had hitherto made with reference to architecture, to ornamental design, and latterly to the human head and countenance ; but on the present occasion he intended to shew the operation of these laws in constituting the symmetrical beauty of the entire human figure.

He next proceeded to point out the remarkable similarity that exists in the physical constitution of the organs of hearing and seeing, and the manner in which external nature affects the sensorium through these organs ; shewing the difference between noises and musical sounds in the one case, and irregular and regular forms in the other. He explained that each musical sound was produced by a number of equal and regular impulses made upon the air, the frequency of which determining the pitch of the sound, their violence its loudness ; and the nature of the material by which the impulses were made its quality or tone. In like manner, he shewed that the effect upon the optic nerve produced by external objects is simply that of the action of light, and amenable to the same laws. Variety of form being analogous to variety of pitch ; variety of size to that of intensity or loudness, and variety of colour to that of quality or tone.

Mr Hay next explained the nature of the harmonics of sound, which result from the spontaneous division of the string of a monochord by the formation of nodes during its vibratory motion. He then shewed how the harmonics of form could be evolved from the quadrant of a circle by the following process :—

From a horizontal line MR (figure 1, of the annexed Plate), he produced two parallel vertical lines ML and RS indefinitely, and with a radius MR described, from the centre M, the quadrant OR. From O he divided the arc of the quadrant into parts of $\frac{1}{8}$, $\frac{1}{7}$, $\frac{1}{6}$, $\frac{1}{5}$, $\frac{1}{4}$, $\frac{1}{3}$, and $\frac{1}{2}$. From the centre M, and through these divisions, he produced the lines MN, MP, MQ, MT, MU, MV, and MS, until they met RS, forming the right-angled triangles MPR, MQR, MTR, MUR, MVR, and MSR. He then shewed, that as the angles at the vertex of each of these triangles, contained respectively 45°, 30°, 22° 30′, 18°, 15°, 12° 51′ 26″, 11° 15′, they related to the right angle, as the harmonics of sound, expressed by the signs c, g, $\bar{c}$, $\bar{e}$, $\bar{g}$, $\bar{b}\flat$, and $\bar{\bar{c}}$, relate to the fundamental note C, produced by the string of the monochord. These triangles he combined in the following manner upon a line AB (figure 2, of the annexed Plate), which he said might be of any given length according to the size of the figure to be formed. From B at an angle of 11° 15′ with AB he drew the line Bg indefinitely, and from A at an angle of 15° with AB the line Ar, also indefinitely, and cutting Bg in K. Through K he drew KL at right angles with AB, forming the triangles ALK and KLB. Through K he drew the line pO parallel to AB. From A at an angle of 12° 51′ 26″ with AB he drew AV, cutting pO in M, and drew MN at right angles with AB, forming the triangle AMN. From A at an angle of 18° with AB, he drew Au, cutting pO in H, and drew HI at right angles with AB, forming the triangle AHI. From A at an angle of 22° 30′ with AB, he drew At, cutting pO in F, and drew FG at right angles with AB, forming the triangle AFG. From A at an angle of 30° with AB he drew As, cutting pO in C, and drew CD at right angles with AB, forming the triangle ACD. From C at an angle of 45° with AB and CD he drew CE, forming the triangle CDE. Thus, he observed, were the triangles arising from the harmonic angles constructed upon AB in the same relative proportions to each other, that they were when formed upon the line RS, figure 1. Upon the other side of AB he constructed similar triangles forming the equilateral triangle ACC; the right-angled isosceles triangle ECC, and the acute-angled isosceles triangles AFF, AHH, AKK, AMM, and BKK. Within this diagram he shewed that the human skeleton could be formed in the most perfect proportions, determining, at the same time, the centres of all the various motions of the joints; and also that the symmetrical beauty of the external form, whether in a front or profile view, was governed by these angles; thus endeavouring to prove that an application of the laws of numerical harmonic ratio in the practice of the sculptor and painter would give these imitative arts a more scientific character than they at present possess, and, so far from retarding the efforts of genius, would rather tend to facilitate and assist them.

FIGURE 1

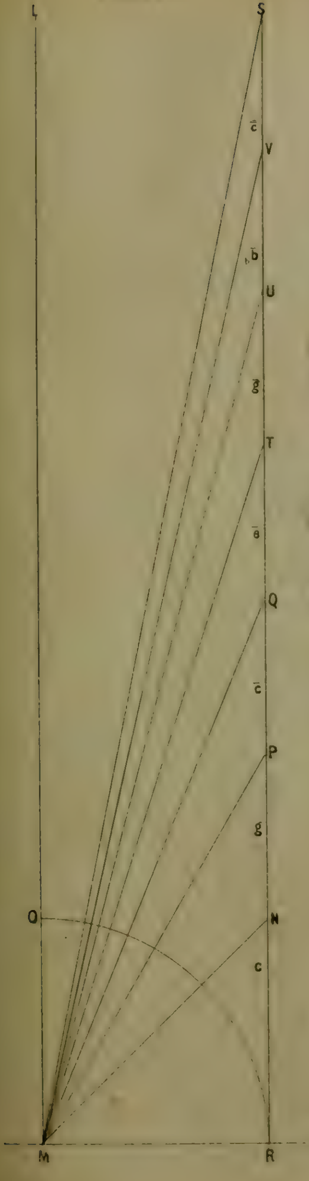
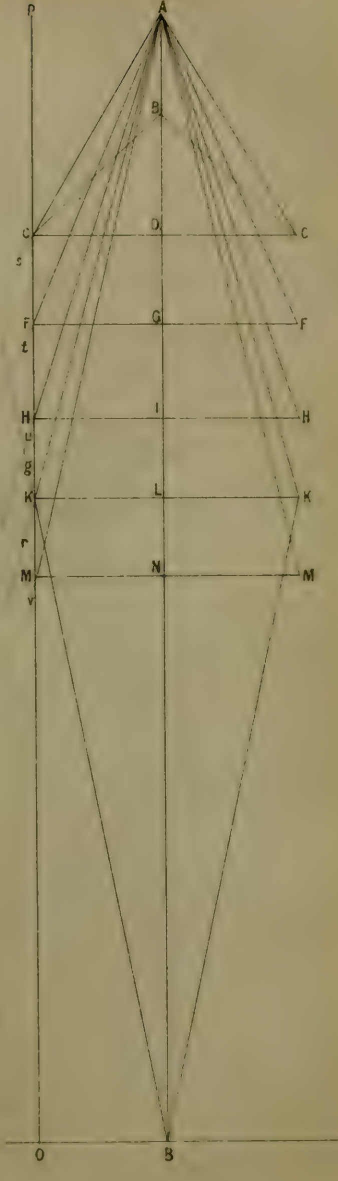


FIGURE 2



PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. II. 1850. No. 39.

Monday, 4th March 1850 (*continued*).

The following Gentlemen were duly elected Ordinary Fellows:—

Lieut. W. Driscoll Gosset, Royal Engineers.
Dr William Sellar, Pres. R.C.P.E.

The following Donations to the Library were announced at the Meeting of 18th February:—

The London University Calendar. 1850. 12mo.—*By the Publishers.*

The American Journal of Science and Arts. Conducted by Professors Silliman and Dana. Vol. IX., No. 25. 8vo.—*By the Editors.*

Mémoires de l'Académie Impériale des Sciences de St Pétersbourg. Sixième Série. Sciences Mathématiques, Physiques et Naturelles. Tome VIII^{me}, 2^{me} partie. Sciences Naturelles. Livraisons 3^{me}, 5^{me}, et 6^{me}. 4to.

Mémoires présentés à l'Académie Impériale des Sciences de St Pétersbourg, par divers Savants et lus dans ses Assemblées. Tome VI^{me}. Livraisons 2de et 3^{me}. 4to.—*By the Academy.*

Messungen zur Bestimmung des Höhenunterschiedes zurischen dem Schwarzen und Caspischen Meere, von G. Fuss, Sawitsch und Sabler. 4to.—*By the Authors.*

Rapport fait à l'Académie Impériale des Sciences de St Pétersbourg, par W. Struve. Sur une Mission Scientifique dont il fut chargé en 1847. 4to.—*By the Author.*

W. Struve sur la Delatation de la Glace d'après les expériences faites en 1845 et 1846 à l'Observatoire Central de Poulkova, par MM. Schumacher, Pohrt, et Moritz. 4to.—*By the Authors.*

Über Prof. Mädlers Untersuchungen über die Eigenen Beweyungen der Fixsterne, von C. A. F. Peters, Dr. 4to.—*By the Author.*

P. H. Fuss Nachricht über eine Sammlung Unedirter Handschriften Leonhard Eulers, und über die Begonnene gesammtausgabe seiner Ueineren schriften. 8vo.—*By the Author.*

Über die Genanig-keit der in Lalandes Catalog, publicirt von der *British Association*, enthaltenen Sternörter, von Dr Lindhagen. 8vo.—*By the Author.*

Verhandlungen der Schweizerischen Naturforschenden Gesellschaft bei ihrer Versammlung zu Slothurn. 1848. 8vo.—*By the Society.*

Mittheilungen der Naturforschenden Gesellschaft in Bern, aus dem Jahre, 1848–9. Nos. 135–161. 8vo.—*By the Society.*

The following Donations to the Library were announced at the Meeting of 4th March :—

Transactions of the Cambridge Philosophical Society. Vol. VIII. 4to.—*By the Society.*

The Astronom. Jour. Vol. I., Nos. 2, 3, & 4. 4to.—*By the Editor.*

Proceedings of the R. Ast. Soc. Vol. X., No. 3. 8vo.—*By the Society.*

Proceedings of the Linnæan Society of London. Nos. 30–40. 8vo.

Charter and Bye-Laws of the Linnæan Society. 1848. 8vo.

List of the Linnæan Society. 1849. 4to.—*By the Society.*

Journal of Agriculture and Transactions of the Highland and Agricultural Soc. of Scotland. No. 28, N. S. 8vo.—*By the Society.*

Annales des Sciences Physiques et Naturelles, d'Agriculture et d'Industrie, publiées par la Société Nationale d'Agriculture, &c., de Lyon. Tom. II. 1848. 8vo.—*By the Society.*

A Collection of Maritime Charts, with corresponding Descriptions. —*By the French Government.*

Monday, 18th March 1850.

The Right Rev. BISHOP TERROT, V.P., in the Chair.

The following Communications were read:—

1. Note regarding the American Electric-Observing Clocks. By Professor Piazzi Smyth.

The object of this communication was chiefly to exhibit a specimen of the register of the electric chronograph, wherein the second's beats of two clocks were marked side by side, one going nearly to sidereal time, and the other to solar; and the length of a second's interval on the paper was so great, and the accuracy of the punctuation such, that the minute acceleration of the one clock on the other could be registered almost from second to second.

The electric register can be applied with ease to any clock, and at any distance from the recording apparatus; and two or more clocks, or they may be simple pendulums, can be made to register their vibrations on the same slip of paper.

The author pointed out how this method might be made available for determining the density of the earth, by observations on the shores of the Bay of Fundy, during the rise and fall of the enormous tides which occur there. He likewise mentioned several purposes to which Lieutenant Maury, U. S. N., proposed to apply the electric chronograph; amongst others, to determining the height of mountains, as he thought that the accuracy capable of being attained in determining the time of vibration of a pendulum in this manner, was so extreme, that the method might be safely applied to such problems.

2. Account of a Remarkable Meteor, seen 19th December, 1849. By Professor J. D. Forbes.

" On the evening of the 19th December 1849, whilst walking near the southern part of Edinburgh, about fifteen minutes past five. Greenwich time (as I afterwards estimated), I observed a meteor, fully brighter than Venus at her average brilliancy, moving from W. towards N., parallel to the horizon, elevated 15° above it, and followed by a distinct luminous train. This angle was subsequently taken by estimation by daylight, with the aid of a theodolite; and the compass-bearing of the meteor, when first seen, ascertained in the same way, must have been 47° W. of N. When it bore 29° E. of magnetic north, it was observed to have divided into two, the one part following the other at some distance; and I soon after lost sight of it in the obscurity of the smoke of the town. When it split, its altitude was estimated at 6°. It thus described an arc of no less than 76°, in doing which it occupied, as I roughly estimated, about 15 seconds, or possibly more.

"Having sent a short notice of the appearance of the meteor to the Courant newspaper, I received from many quarters accounts of its having been seen under circumstances remarkably similar to those just described. I believe that nearly forty communications on the subject have reached me from places included between Longford, in the centre of Ireland, to near Bervie, in Kincardineshire, a distance of above 300 miles, in a direction nearly NE. and SW., whilst in a perpendicular direction, or from NW. to SE, the range of observation has been comparatively small; for I have received no information from beyond Renfrew, in the one direction, and Durham in the other; being about 140 miles distant in a straight line. The meteor was seen at Longford, in Ireland, 74 miles west of Dublin, but not in Dublin itself. It was seen at Belfast, between Carlisle and Gretna at Stewarton in Ayrshire, at Johnstone, at Paisley, Renfrew, and by many persons in Glasgow and the neighbourhood. It was also generally seen in Edinburgh, in East Lothian, near Melrose, and at Durham, as already mentioned. Further north, I have received accounts from Crail, St Andrews, Dundee, Perth, and Johnshaven to the north of Montrose.

"The greater number of these communications concur in estimating the direction of the motion of the meteor to have been from SW. to NE., although, as might be expected, they vary excessively as to its distance and magnitude; being described by some persons as only 50 or 100 yards off, and as large as the moon; by others, as a ball of 9 inches in diameter, or the size of a large egg. One person only professes to have heard a sound. The time during which it was seen was variously estimated. At Longford, by Mr Curtis, 20 sec.; at Glasgow, by Mr Stevenson, at 20 sec.; at Johnstone, by Mr Cunningham, 15 sec.; at Perth, 15 or 20 sec.; at Durham, by Mr Carrington, 30 sec.; at St Andrews, 15 seconds according to one observer, and 18 to 21 seconds according to another ; at Johnshaven, ⅞ths of a minute. The hour of the appearance of the meteor, in most of the descriptions, is stated at between 5h. 10m., and 5h. 16m.

"The arc of the horizon which it was seen to traverse depended, of course, on the point where the meteor first caught the observer's eye. At Granton, it was traced by Professor Kelland through 125° of azimuth ; at Perth, 130 ; at St Andrew's, 74° ; at Edinburgh, 76° ; at Durham, 65° ; at Glasgow, from 60° to 70°. The division of the head or nucleus into several parts, and, first of all (in most cases), into *two*, has been noticed with remarkably slight variation ; consequently, the explosion of the meteor marks a well-determined point in its path. The separation was specially noticed at Edinburgh, Granton, Glasgow, Renfrew, Melrose, Haddington, Johnshaven, Perth, Durham, St Andrews.

"In a majority of cases a luminous train was observed ; and I am confident, that the existence of this train, which has been estimated at from 2° to 3° long, cannot be questioned. Dr Adamson, however, especially remarked that no train was to be seen at St Andrews.

"On revising the whole accounts, it does not appear that any of them can be relied upon, for ascertaining the position of the meteor in space, except the observations of Mr Carrington of the Durham observatory ; of Professor Kelland, Mr Stirling, and myself, at Edinburgh ; of Dr Adamson and another observer, communicated by Professor Fischer of St Andrew's ; of a young gentleman at Perth, communicated by Thomas Miller, Esq., Rector of the Perth Academy ; and of A. D. Stevenson, Esq., and W. Gourlie, Esq., junior, at Glasgow. My inquiries were chiefly directed to the two following points : *first*, the angular elevation of the meteor in the NW. quarter of the heavens, where it is admitted by all that its path appeared almost horizontal ; *secondly*, to the bearing of the meteor at the instant of explosion.

"At Durham, Mr Carrington saw the meteor first when the bearing was true NW., the altitude (by theodolite) was then 10°, or not exceeding 11° ; when it burst, it was due N. (true), and continued to move 10° or 12° further before it disappeared. Professor Chevallier, who obligingly communicated these results, states that the meteor appeared rather to rise as it approached the north, but with a doubt. This supposition, however, appears inadmissable, from the unanimity of the other accounts.

" At Granton, near Edinburgh, Professor Kelland caught sight of the meteor a little to the N. of the moon, and several diameters below it. This corresponds, by after estimation with a theodolite, to 75° W. of magnetic N., and an altitude of 12°. Professor Kelland thinks that it rather rose afterwards. It split into two at 20° E. of magnetic N., having then an altitude of only 5°; it continued for a considerable time bright, then began to fade, as if by the effect of distance, and also to separate into several parts: it was finally lost sight of 50° E. of magnetic N. (this bearing is well ascertained), with an altitude estimated at only half a degree. The position and circumstances of these observations, made at an elevated station above the Frith of Forth, were eminently favourable.

" Mr J. Stirling, civil engineer, looking up North Hanover Street, Edinburgh, saw the meteor separate into two parts; the bearing he afterwards estimated at 25° E. of magnetic N. (the probable error not exceeding 1°), and the altitude at 8° 30′, certainly not exceeding 9°.

" I think we may conclude, that at Edinburgh the meteor attained a maximum elevation of 15° (that mentioned in the commencement of this paper), since it no doubt rose after Professor Kelland first saw it to the S. of the true W., with an altitude of only 12°. The course of the meteor was evidently such as to be nearest the spectator when in the true NW. or WNW.

" The place of the meteor when it burst stands thus :—

Kelland, N. 20° E. (mag.)	Alt. 5°.
Stirling, N. 25° E.	Alt. 8° 30′.
Forbes, N. 29° E.	Alt. 6°.

" The average is almost 25° E. of N., or about 1° W. of the true meridian, the variation being nearly 26°. The mean of the three observations of altitude would be 6° 30′; but admitting Mr Stirling's to be entitled to the greatest confidence, we may suppose it 7°, or possibly a little more.

" At St Andrews, the meteor was seen by Dr Adamson, when riding in a northerly direction, on the Largo road. Professor Fischer was so kind as to accompany him afterwards to the spot, and to reduce his observations with all the accuracy of which they were capable. It was first noticed when bearing 8½° W. of magnetic N., and disappeared at 42½° E. of N.; the altitude was conjecturally stated as between 14° and 18½°, and it appeared to move horizontally, but rather declining towards the N.

"After describing three-fourths of its course, it split into two parts, which went on close together for a little, then broke into four or five, became dull red, and rapidly disappeared; the separate pieces travelling on together until the last.

"Another intelligent observer near St Andrews, whose evidence was taken by Mr Fischer, first saw the meteor 29¾° W. of magnetic N., and estimated the point where the meteor burst at 44° E. of N.; but this last number coincides so closely with Dr Adamson's estimate of the point of final disappearance, that it is perhaps allowable to suppose, that this second observer had mixed up these two events in his description. Dr Adamson's statement, that one-fourth of the arc which he saw was described after the meteor had split, would give an azimuth at that moment of almost 30° E. of N. magnetic, or 4° E. of N. true, as Mr Fischer determined the magnetic declination to be about 25° 46′. The altitude of the meteor, as seen by this observer, appears not to have exceeded 15° (the same as at Edinburgh); which number we shall therefore adopt.

"At Perth, the passage of the meteor was seen from the North Inch, by a young gentleman of intelligence, whose observations were reduced to numbers by Mr Miller, Rector of the Perth Academy, who was so good as to accompany him to the spot, and take the angles with a theodolite. Its bearing, when first seen, was 46° S. of W. true; its angular altitude was at that time only 3° 30′. This is by far the most southern azimuth which has been observed. Its bearing, when it disappeared, was 6° W. of N., but it was then lost in a cloud. If I understand right, it had, by this time, separated into fragments. Its apparent altitude, in the middle of its course, was about 17° 30′. These observations, extending over an arc of 130°, taken along with Professor Kelland's, clearly demonstrate that the meteor appeared with a very low altitude in the SW. quarter of the heavens, and disappeared in a similar way in the NNE., attaining its greatest elevation about WNW. (true.)

"At Glasgow the meteor was very generally and well seen. Mr William Gourlie junior saw it move from SW. to NNE., over an arc of 60° or 70°, and divide into two, when it bore 40° E. of magnetic N. He estimates its greatest elevation at 30°, and that it decreased to between 15° and 17°, or even less, at the time of its separation. He adds, that he is not much accustomed to such observations. Mr A. D. Stevenson, living in South Portland Street, Glasgow, saw the meteor moving along, at a height just sufficient to clear the chimney-tops, on the west side of the street, an elevation which he afterwards estimated, as he states, with considerable accuracy at 28°. I have received farther and more minute accounts of the appearance of the meteor from Mr Stevenson, who has been most kind and intelligent in his communications; and my friend Mr James Peddie has verified the accuracy of Mr Stevenson's observations beyond the possibility of mistake. It appears that the meteor passed quite clear of a stack of chimneys on the opposite side of the street, which would give it a well-defined minimum altitude of 25° 41′; but Mr Stevenson is of opinion that it rose more than 2° higher, or to not less than 28° (perhaps even to 28° 21′); when it was highest, its bearing was 52½° W. of N. (magnetic), and it disappeared from his view when it bore 40° 27′ E. of magnetic N. *It was then decidedly single.* Now, this bearing coincides with that at which Mr Gourlie observed it to become *double;* and, consequently, the limit towards the N. of this event is severely defined.

"The following Table contains the most definite of these observations, and the azimuths are all reduced to the true meridian:—

	Greatest Altitude.	True Azimuth when first seen.	True Azimuth of disappearance.	Arc observed.	True Azimuth of first explosion.	Altitude at first explosion.
Durham,	10° 30′	N. 45° W.	N. 12° E.	57°	N.	
Edinburgh,	15°	W. 11° S.	N. 24° E.	125°	N. 1° W.	7°
St Andrews,	15°	N. 55° W.	N. 16° E.	71°	N. 4° E.	
Perth, .	17° 30′	W. 47° S.	N. 7° W. (in a cloud)	130°	?	
Glasgow,	28°			100°?	N. 14° E.	15°

Remarks on the Observations.

"1. On the whole, these observations are not consistent, and cannot (I conceive) be cleared up without additional and accurate ones, which it may now be too late to procure. The central group of stations, Edinburgh, Perth, and St Andrews, are sufficiently accordant, and indicate that the path of the meteor must have been nearly parallel to a line passing through the first and last of those places, or in a direction N. 27° E. (true); which accords well with the observations at most of the individual stations, and particularly with the *vanishing direction* in Professor Kelland's remarkable observation at Granton.

"2. The Durham observation is compatible with the above-mentioned group within the limits of error. By the combination of Durham and Edinburgh (the base line perpendicular to the assumed direction of the meteor's motion being 95 miles), I calculated that the meteor passed vertically nearly over the Island of St Kilda, with an absolute elevation of about 88 miles. But this solution seems absolutely excluded by observations at Glasgow which admit of no question, and which I have spared no pains in verifying. Had the position of the meteor been such as I have first assumed, it could not possibly have been seen over even the roofs of the houses from the station occupied by Mr Stevenson, much less over the chimney-tops. The bearing, at the moment of explosion at Glasgow, also singularly enough corroborates sufficiently well the comparatively small elevation (about 20 miles above the earth) which the combination of Edinburgh and Glasgow gives; and this bearing we have seen to have been also accurately defined by the physical obstacles bounding the observer's view; it would have given a parallax of 15°, subtended by the perpendicular on the meteor's path, referred to Glasgow and Edinburgh respectively. Now, if this calculation were anything like correct, the Perth observation is entirely wrong; and the meteor could not have risen about 6° above the horizon of Durham, instead of 10° or 11° as estimated. I am unable, in any degree, to explain these conflicting results.

"3. The observations of Professor Kelland at Granton, and those at Perth, through the great azimuths of 125° and 130°, described by the meteor with such remarkable deliberation of motion, lead, when analyzed, to the very same results which presented themselves to the mind of the spectator intuitively; namely, that the motion must have been sensibly rectilinear, equable, and parallel to the horizon at Edinburgh. Assuming that the greatest altitude at Edinburgh was 15°, and the bearing then N. 63° W. (true), we may calculate that the altitude should have been on this hypothesis, when first seen by Professor Kelland, 11° 47′,—instead of 12° as observed; at explosion, 6° 59′ (7° observed), and at its final disappearance 0° 47′ (instead of 0° 30′ observed). Again, at Perth the observed altitude, when first seen, was 3½°, and the calculated altitude 5° 3′, taking the maximum altitude at 17½°. The coincidence is, on the whole, remarkable, though it would be rash to push it to an extreme, as an error of some degrees may exist in the assumption of the direction of the meteor's course. Some later observations, received from Mr Curtis at Longford, and a consideration of the effects of perspective at Perth and Edinburgh, incline me to admit that the path might make an angle 3° or 4° greater with the meridian than I have above supposed. These conclusions are independent of the actual distance or parallax of the meteor; which, as I have said, cannot be determined without further observations, which I should be glad to receive from any quarter, but more particularly from Ireland, and from the centre and NW. of Scotland. If correct, they entitle us to infer that the meteor in question was most probably a body moving in space, in a path little curved, and not revolving round the earth."

3. Notes on the Purification and Properties of Chloroform. By William Gregory, M.D., Professor of Chemistry in the University.*

1. Chloroform has been prepared both from alcohol and from wood-spirit. The latter has been used for the sake of cheapness; but as it is a mixture of several liquids, all of which do not yield chloroform, it gives an impure product, in a proportion which varies much, but is always below that obtained from alcohol. There is

* Although I am alone responsible for the opinions contained in this paper, it is my duty to state, that all the experiments and observations mentioned in it have been made by me in concert with my able assistant, Mr Alexander Kemp, of whose ingenuity and accuracy I have had constant opportunities of judging.

therefore not only no advantage, but the contrary, in using wood-spirit, which is not, after all, much cheaper than alcohol.

2. But the chloroform from these two liquids, *when fully purified*, is quite identical in all its properties. Its smell, density, boiling point, and action on the system are, in both cases, exactly the same. That from alcohol is, no doubt, more easily purified than the other; but it also contains volatile oily impurities, which must be removed before it can be safely used. The peculiar oils which adhere to both kinds of chloroform are not identical, or, at least, not all identical; but they are of analagous constitution and properties.

3. Soubeiran and Mialhe have examined these oils. They contain chlorine, have a disagreeable smell, and, when inspired or smelt, cause distressing headache and sickness. In the case of wood-spirit, some of its own impurities distil over unchanged, and are found in the chloroform.

4. It is well known that many persons, after the use of chloroform, have suffered from headache, nausea, and even vomiting, as I have more than once seen. Headache and nausea I have myself experienced, when I have tried different specimens of chloroform, without taking so much as to produce the full effect.

5. Perfectly pure chloroform, such as is now on the table, does not, so far as I have seen or experienced, produce these disagreeable effects. It is, therefore, highly probable that when they occur, as they do with some individuals, from the use of chloroform of more than the average goodness of quality, this depends on the presence of a trace of these poisonous oils.

6. All good manufacturers of chloroform purify it by the action of oil of vitriol; which destroys the oils, while, at the same time, a part of the acid is reduced to sulphurous acid. The chloroform, to remove this, is then distilled with lime or carbonate of baryta, and is tolerably pure, if the process be well conducted.

7. But this is not quite pure, and contains a trace, more or less distinct, of the oils. I have found this to be the case with all the best chloroform made here, up to 1849; and I have several times seen headache and sickness from the use of such chloroform, which, as we all know, was the best anywhere made. I must add, however, that the quantity of oils was, although variable within certain limits, always, in the Edinburgh-made chloroform, so small, that it was fit for use, and only caused headache, &c., in a few peculiarly sensitive persons.

8. It was desirable to have a test for these impurities, as well as an easy and effectual mode of removing the last traces of them; especially as many sorts of chloroform, not made here, were far inferior in quality to that prepared in Edinburgh. One very delicate test is, that oil of vitriol, which should be quite colourless and pure (as it may be rendered by Mr Kemp's process, lately read to the Society), when agitated with the chloroform, becomes yellow or brown, from its action on the oils, which it chars and destroys. Any change of colour is easily seen by the contrast with the colourless chloroform which floats above. Pure chloroform gives no colour to the acid. It is essential that the oil of vitriol be colourless, and also of full density; for, if coloured, it is not easy to see a slight change in its colour; and if below the proper density, that is, too weak, it is not much coloured by a chloroform which will render brown the acid of proper strength.

9. Another test, still more delicate, I find to be the smell of the oils. When chloroform is poured on the hand or a handkerchief it rapidly evaporates; but the oils, being less volatile, are left behind, and their smell, previously covered by that of the chloroform, is easily recognised. Until very lately, no chloroform was sold, or, indeed, known, which would stand this test, or even the former.

10. Up to 1849, the best commercial chloroform had a specific gravity of 1·480, which was considered a guarantee of its purity. But it had been obtained, by chemists, of specific gravity 1·494 and even 1·497. I have found that chloroform of 1·480, when once more acted on by oil of vitriol, which destroys the oils and becomes brown, may be obtained, after removing the sulphurous acid, of specific gravity 1·500 at 60°. This I take to be the specific gravity of pure chloroform. Our best makers have lately, much to their credit, pushed the purification so far as to furnish chloroform even of this highest density, and also, in other respects, such as it ought to be.

11. There are still, however, many makers, in other places, whose chloroform is not so pure; and I shall now describe the method which, with Mr Kemp, I have employed for purifying, perfectly and easily, any commercial chloroform (except one remarkable specimen, of which more hereafter), a process which will enable any medical man to purify it for himself with the greatest facility.

12. The chloroform, having been tested as above, and found more

or less impure, is to be *agitated with* oil of vitriol (half its own volume will be sufficient), and *allowed to remain in contact with the acid;* of course in a clean, dry, stoppered bottle, and *with occasional agitation,* till the acid no longer becomes darker in colour. As long as the action is incomplete there will be seen, after rest at the line of contact, a darker ring. When this no longer appears, the chloroform may be drawn off, and, for greater security, once more acted on by a quarter of its volume of the acid, which should now remain colourless. It is now to be once more drawn off, and, in a dry stoppered bottle, mixed with a little powdered peroxide of manganese, with which it is gently agitated and left in contact, until the odour of sulphurous acid is entirely destroyed, and the chloroform has acquired a mild agreeable fruity smell. It has then only to be poured off into a proper phial. It will now leave no disagreeable smell when evaporated on the hand. (If the commercial chloroform, after having been *frequently well shaken,* and *left for some time in contact* with the acid, has given only a moderate tinge of colour to it, it is probable that it may be completely purified by that first process. To ascertain this, test a small portion in a tube with fresh acid, *shaking well,* and *allowing it to stand some time.* If it do not colour the acid at all, then the whole chloroform has only to be finally purified by the oxide of manganese. If the acid become coloured in the test tube, it will be as well to act on the whole chloroform a second time with fresh acid, till it stands the test. Mr Kemp has observed, in repeating this process for me, the very curious fact that, as soon as the action is complete and the oily impurities are destroyed, but not sooner, the chloroform tested with the acid in a tube exhibits a strongly convex surface downwards, where it rests on the pure acid, or, what is the same thing, the acid becomes concave at its upper surface. The smallest trace of impurity, not sufficient to affect the density of the chloroform, we have found to render the line of junction horizontal. It is probable that this may become a valuable test of the perfect purity of chloroform, but we shall not say more on this subject until we have thoroughly examined it.)

This process requires no apparatus beyond a few stoppered bottles, and a syphon, or a pipette, if we wish to draw off the whole chloroform without loss. The use of the oxide of manganese is due to Mr Kemp; and, on the large scale, the chloroform may be filtered through a cylinder full of it. In this final purification of genuine, although not quite pure chloroform, no distillation is necessary.

13. It may be considered as certain, that the use of chloroform, thus purified, will very rarely, if ever, cause the disagreeable effects above noticed.* As to more serious bad results from the use of chloroform, so often spoken of elsewhere, it is enough to state, that a large proportion of the cases must be attributed to the use of a liquid so impure, as hardly to deserve the name of chloroform at all. Such a product, I rejoice to say, our Edinburgh manufacturers have never sold; and, I may add, that, no doubt chiefly in consequence of this, our practitioners have not yet seen a fatal result from the use of chloroform. But in London, and elsewhere, chloroform has been extensively sold, so bad, that I have examined specimens which did not contain half of their bulk of chloroform; others with not one third or one fourth; and I have seen one which hardly contained any at all. But, to make up for this, they were rich in poisonous oils, and

* Dr Simpson informs me, that the purest chloroform he has used not unfrequently causes vomiting. On further inquiry I find that this occurs when it is administered after a full meal. This can easily be avoided, and must not be confounded with the headaches, nausea, and vomiting alluded to in §§ 4 and 5; which symptoms are persistent, and occurred, in my experiments, always with an empty stomach, the experiments being made an hour or two before dinner. Dr Carmichael, assistant to Dr Simpson, has mentioned to me some facts which confirm the view I have taken. At one period, for more than a week, Dr Simpson and Dr Carmichael were kept in a state of continual anxiety by the occurrence, in all the puerperal cases in which chloroform was used, of very unpleasant symptoms, particularly of frequent pulse and other febrile symptoms, lasting for some days. At last, after much annoyance from this cause, it occurred to Dr Simpson that he was using one particular specimen of chloroform, supposed to be of good quality. As soon as this idea occurred, he threw away all that remained, and returned to that which he had generally used. The unpleasant symptoms no longer appeared. (I regret much that I had not an opportunity of examining that specimen; but I may add that the maker, not an Edinburgh one, now produces chloroform of much better quality, though not yet absolutely pure.) But the striking fact is this, that Dr Simpson and Dr Carmichael state, *that during the period above alluded to, when that one kind of chloroform alone was used by them, their handkerchiefs became quite offensive from the smell left on them, which even adhered to them after washing.* There can, I think, be no doubt that here the oily impurities alluded to in §§ 4 and 5 were present in notable quantity. I suspect that a majority of the specimens mentioned in the Table would have a similar effect, more or less marked. (I have since ascertained that this chloroform, which was much above the average in quality, *had not been subjected to the action of oil of vitriol in its preparation,* which strongly confirms the view I have taken. W. G.)

often in free hydrochloric acid. Very many specimens, although better than this, are yet so impure, that no one could, with comfort or safety, use them.

14. The chloroform now, and for some time past, made here, is of first-rate quality. I have two specimens which are absolutely pure, or nearly so; and a third, which is hardly inferior, all made and sold by Edinburgh manufacturers.

15. On the other hand, I have various specimens, maker unknown, besides some from makers in other places, which are not so pure, although, in general, much purer than those which I examined nearly three years ago. But one specimen deserves a separate notice. It is labelled "*pure chloroform.*" It is yellowish, has a strong smell of the oils, and of impure wood-spirit; and, when treated with its own volume of oil of vitriol, developes much heat, colours the acid dark brown, and disappears almost entirely, any trace of chloroform it may contain being boiled off by the heat disengaged. It contains also so much free acid, that the cork is corroded. It is to be hoped that this product disgraces no longer the market. I do not know the name of its maker. Three of the specimens became milky, when mixed with the acid. One, after contact with the acid, acquired a strong smell of musk. Another lost about a third of its bulk. All but two coloured the acid decidedly at once; and all left, more or less, a disagreeable smell on the hand. One of the two which did not much colour the acid at first was that which acquired the smell of musk; the other, evaporated on the hand, left a white stain, depending partly on the matters present in the skin. This was the case also with another; yet these two coloured the acid but little at first, more strongly after a time: but both left a smell on the hand. Only one (Edinburgh made) specimen, of density 1·500, gave no colour, or only a perceptible tinge, to the acid.

16. In conclusion, I would remark, that while the use of chloroform in Edinburgh, in many thousand cases, has never yet led to a fatal result, I do not intend to maintain that the use of pure chloroform never can cause fatal effects. On the contrary, I have no doubt that, if rashly, carelessly, or ignorantly administered, so powerful an agent may, like any other powerful drug, especially in individuals of peculiar temperament, and in cases of severe, though latent internal disease, give rise to fatal results. That no such cases have here been met with is due partly to the good quality of the

chloroform used, and to the care with which it is prepared; and partly to the experience and judicious management of those whose duty it is to administer it, at the head of whom stands the introducer of chloroform, my friend and colleague, Dr Simpson.

It is much to be regretted that, in London and elsewhere, chloroform is not by any means so extensively employed as it ought to be, in consequence of the occurrence of some fatal cases, attributed (whether in all cases accurately or not, is a question) to the drug. There can be no doubt that most, if not all, of these cases have resulted from the use of very impure chloroform, such as even at a recent period was largely sold in London; and that, if pure chloroform alone had been employed, there would, by this time, have been no prejudice against its use. It is not, as I have shewn, necessary that chloroform should be very impure, in order to produce very disagreeable or even dangerous results. It is evident that even a small proportion of the oils above mentioned, if they are deleterious (and this cannot, I think, be doubted), will suffice, when applied in the form of vapour to the internal surface of the lungs, to act powerfully on the system. On the other hand, I am far from blaming those chemists who have manufactured impure chloroform for anything more than a want of due care in the preparation of an agent so energetic. And it is but fair to bear in mind that it was a new manufacture, hardly yet fully understood, and that those who made it were not probably aware, either of the existence of the impurities, or of the best mode of removing them. I have no doubt they did their best to produce a good article; and my chief object in this paper has been to put it in the power of every one to do so, and to point out strongly the bad effects of even a small amount of impurity.

While I acquit the makers of impure chloroform of any desire to adulterate it, I think it right to add that some of them must have been entirely ignorant of what was published concerning its properties. Thus some sold it of specific gravity 1·465, others of 1·347; and in the case of No. 8, which I have no doubt was under 1·000, although I had not enough to take its density accurately, the maker had evidently rejected the chloroform, and preserved the lighter liquid floating over it!—not knowing even that chloroform was a heavy liquid. It is lamentable to think that persons so ignorant are free, by our laws, to set up as makers of the most potent drugs.

I may here add, that no *rectification* at all is required from the first, if the chloroform be only washed with water till its volume no longer diminishes, and then treated, as above, with concentrated sulphuric acid.

It is possible that some of the fatal cases may have occurred from an injudicious mode of administering the vapour, or from the operator intrusting the administration to persons not qualified to recognise those signs which tell the experienced practitioner that it is time to stop. There ought always to be two well-qualified persons present, —one to watch, without intermission, the effects of the vapour, which he also administers as required; the other, of course, to operate. He who gives the chloroform must carefully attend to the state of the respiration, as has been often recommended by Dr Simpson. But these are matters beyond the proper province of this paper, and I leave them them to those who are better qualified than I am to discuss them.

I have only to add, that this paper was written and read before I heard of a recent article in "Chambers' Journal" on the subject; and that I had not the remotest knowledge of or concern in that article, which I have not yet seen, although, as I am told, the author of it agrees with some of my conclusions in regard to the employment of chloroform in London.

A tabular view of the properties of chloroform will be found on the following page.

Tabular View of the Properties of Chloroform.

Variety of Chloroform.	Specific Gravity at 60°.	Action of Concentrated Sulphuric Acid.	When evaporated on hand.	General Remarks.
No. 1	1·347	Became milky and yellow, changing to brown. After twenty-four hours, very dark brown; it also diminished in volume.	Left a strong smell.	The low density here at once proves the great impurity. In contact with the acid, it lost one-fifth or one-fourth of its volume. Very dangerous to use.
No. 2	1·465	The same as No. 1, except that it did not diminish nearly so much; after 24 hours, it had become very dark.	The same as No. 1.	Density also far too low, but less impure than No. 1. Both would be very unsafe to use.
No. 3	1·495	Scarcely affected, on mixture at first; after some time very dark, it also acquired a distinct smell of musk.	Left a very distinct smell.	This is much better than Nos. 1 or 2, but yet not pure.
No. 4	1·475	Became milky and thin yellow; after 24 hours, very dark brown.	The same as No. 1.	Resembling No. 2; probably the same maker.
No. 5	1·495	Became milky yellow, and afterwards brown, but diminished only slightly in volume; darker after 24 hours.	Distinct smell.	Of tolerable quality, but not pure.
No. 6	1·490	Became slightly yellow, but did not diminish much in volume; brown after 24 hours.	Very distinct smell.	Nearly as No. 5.
No. 7	1·495	Little colour developed at first; after frequent shaking and 24 hours contact, dark brown.	Distinct smell.	Rather better than Nos. 5 and 6. The chloroform mentioned in the note, p. 320, was not, I believe, inferior to this.
No. 8		Became dark brown, and very hot. Nearly the whole of it dissolved in the oil of vitriol used.	Left a very strong and disagreeable smell.	This certainly did not contain more than one-thirtieth of chloroform. It had not even the smell of that substance, and contained much free hydrochloric acid, as well as the poisonous oils, in large proportion. The use would be most dangerous.
No. 9	1·500	Became very pale yellow, afterwards dark brown.	Distinct smell.	The full density, and very nearly pure. Quite fit for ordinary use; although it might easily be rendered quite pure.
No. 10	1·500	Very slight change.	Just perceptible smell.	Full density. It can hardly be distinguished from the purest chloroform I have myself prepared. But even this did not exhibit the convex surface downwards, when resting on the acid.
No. 11	1·490	Became dark brown after a time, as No. 4.	Distinct smell.	Not sufficiently pure for use; but better than several others.
No. 12	1·490	As No. 9. Slightly coloured at first; after some time, and frequent shaking, dark brown.	Distinct but slight smell.	This, as well as Nos. 3, 5, 6, 7, and 11, would all have been called quite pure two years, or even one year ago. But all of these require to be purified.

The following Donations to the Library were announced:

Some Account of the last Yellow Fever Epidemic of British Guiana. By Daniel Blair, M.D. Edited by John Davy, M.D., F.R.S.L. &E. 8vo.—*By the Author.*

Das peripherische Nervensystem der Fische, Anatomisch und Physiologisch untersucht von Dr Hermann Stannius. 4to.—*By the Author.*

Neue Denkschriften der Allg. Schweizerischen Gesellschaft für die gesammten naturwissenschaften. Bd. x., mit. xiii. Tafeln. 4to. —*By the Society.*

On the Diffusion of Liquids. By Thomas Graham, Esq., F.R.S., F.C.P. 4to.—*By the Author.*

Description of the Instruments and Process used in the Photographic Self-registration of the Magnetical and Meteorological Instruments at the Royal Observatory, Greenwich. 4to.—*By the Astronomer-Royal.*

Proceedings of the Royal Astronomical Society. Vol. X., No. 4. 8vo.—*By the Society.*

Description of the Observatory at Cambridge, Massachusetts. By William Cranch Bond. 4to.

Astronomical Observations made at Cambridge Observatory, Massachusetts, 1847–8. 8vo.—*By the Observatory.*

Monday, 1*st April*, 1850.

Gen. Sir T. MAKDOUGALL BRISBANE, Bart., in the Chair.

4. On a Peruvian Musical Instrument, like the ancient Syrinx. By Dr Traill.

The author prefaced his description of the instrument, by a few general remarks on the communication, in very remote epochs, between the inhabitants of the old and new worlds, as deducible from affinities in their traditions, their cosmogenies, their religious rites and structures, their astronomical cycles, and their determination of the length of the year.

The Peruvian instrument was discovered, some years ago, in a *huaco*, or vast tumulus, that was believed to cover the remains of an Inca of Peru. It is not of unequal reeds, like the Greek syrinx, but is cut out of a piece of *potstone*, of a trapezoidal form, in which are cut eight tubular holes of unequal depths. These tubes or holes are of equal diameter, and have been carefully made with some sort of drill. The breadth of the instrument, including a short handle, is 6·2 inches; its greatest depth, 5 3 inches; and the thickness of the stone varies from 0·7 to 0·5 inch. The instrument in principle and in form is analogous to the *Pan's pipe* of antiquity, or to the *organetto* of modern Italy; but has one remarkable difference in a small ventilage on each of four of its pipes; when one is uncovered, that pipe is mute, but when covered by the fingers of the player, the full sound is produced.

A strolling Italian, who performed well on the *organetto*, was employed for several evenings to play on the Peruvian instrument; and, with the assistance of three skilful musical friends, one of whom was an adept on the violoncello, the author of the paper was enabled to ascertain the scale of the instrument. This scale extended from E on the lower line, through F sharp, G, A, D, C sharp, F to A, above the lines. By means of the ventilages, the ordinary notes of the instrument seemed to be divisable into two tetrachords,—one in the key of E minor, the other of F major—the first a perfect tetrachord; the second, nearly so.

The form of the instrument and its use have a striking similarity to the Syrinx of the Greeks, the invention of which was ascribed to the god Pan, or to Egypt; and it is worthy of notice, that the great musical system of the Greeks also consisted of tetrachords. A syrinx of unequal reeds was found by the celebrated Humboldt, in the hands of the natives, on the banks of the Orinocco. It is in use among the Arabs of the desert, and a similar instrument, composed of twelve unequal reeds, is figured by Kæmpfer among the instruments of the Japanese.

2. Some Remarks on Cometary Physics. By Professor Piazzi Smyth.

That theories of the physical appearances of comets have generally failed, appeared to the author to arise from the facts having been misunderstood or misinterpreted in general by the observers themselves.

As a particular instance of this, the wide-spread notion of comets shooting forth their tails, at, or a little before the perihelion passage, and drawing them in again afterwards, so as to be larger at that period of their orbits than at any other, was mentioned; and in place of which, the author shewed that the comets were at the perihelion, of their smallest size; the tails becoming then more visible, not from being actually produced at that time, but from being more dense, and illumined by a stronger solar light, as well as being in general seen from a smaller terrestrial distance.

The author then proceeded to collect together the facts which he thought well made out with regard to comets; to describe the corrections which the apparent, required, to give the true phenomena; and to detail the various practical methods by which better observations might be procured.

The so-called established facts mentioned above, were collected in a series of axioms, which are here appended; as they seem to be worthy of being discussed, and either disproved or assented to, by astronomers.

1*st*, A comet consists of a nucleus, and one or more gaseous envelopes.

2*d*, The nucleus, if solid and material, is infinitely small.

3*d*, The nucleus is excentrically situated in the gaseous body.

4*th*, Comets of longest period have the largest bodies.

5*th*, Those comets whose orbits have the greatest excentricity, are the most excentrically situated in their envelopes, or, vulgarly, have the longest tails.

6*th*, A comet revolves on an axis passing through the nucleus, and at right angles to the major axis of the envelope, in the same period of time that it takes to revolve about the sun; hence the tail being turned away from the sun in the normal position, is turned away from him in all other parts of the orbit also.

7*th*, This axis is not at right angles to the plane of the orbit, but variously inclined in the case of different comets, as with the planets.

8*th*, A quicker rotation round the longer axis of the body also appears to exist.

9*th*, A comet shines by reflected light, and shews a sensible phase.

10*th*, The gaseous envelope is of extreme tenuity, is elastic, and, with regard to light, is slightly reflective and imperfectly transparent; it decreases in size, but increases in density and light reflective power in approaching the perihelion, and the reverse when receding from it; and this occurs in a degree proportioned to the excentricity of the orbits of the comets.

11*th*, The axis of the tail of a comet is straight at the perihelion, but at any point between this and the aphelion, is curved; and is concave towards the latter, the radius of curvature being inversely as the excentricity of the orbit.

12*th*, The molecules composing the envelope of a comet are only held together by their mutual gravitation, each constituting almost a separate independent projectile, and describing its own parabola about the sun.

3. Abstract of Professor Kelland's Exposition of the Views of D. R. Hay, Esq., on Symmetric Proportion.

The fundamental hypothesis of the author was stated to be this:—That the eye is capable of appreciating the exact subdivision of spaces, just as the ear is capable of appreciating the exact subdivisions of intervals of time; so that the division of space into an exact number of equal parts will affect the eye agreeably in the same way that the division of the time of vibration in music, into an exact number of equal parts, agreeably affects the ear. But the question now arises, What spaces does the eye most readily divide? It was stated that the author supposes those spaces to be angles, not lines; believing that the eye is more affected by direction than by distance. The basis of his theory, accordingly, is, that bodies are agreeable to the eye, so far as symmetry is concerned, whenever the principal angles are exact submultiples of some common fundamental angle. According to this theory we should expect to find, that spaces, in which the prominent lines are horizontal and vertical lines, will be agreeable to the eye, when all the principal parallelograms fulfil the condition that the diagonals make with the sides, angles which are exact submultiples of one or of a few right angles. This application of the theory was exemplified by a sketch of the new Corn Exchange erected in the Grassmarket by David Cousin Esq., whose beautiful design was shewn to have been constructed with a special reference to the fulfilment of this condition.

The author was stated to proceed to apply his theory to the construction of the human figure, in which we should expect *à priori* to find the most perfect development of symmetric beauty. Diagrams were exhibited which represent, with remarkable accuracy, the human figure; and it was explained that not a single lineal measure is employed in their construction. The line which shall represent the height of the figure being once assumed, every other line is determined by means of angles alone. For the female figure, those angles are, one-half, one-third, one-fourth, one-fifth, one-sixth, one-seventh, and one-eighth of a right angle, and no others. It must be evident, therefore, that, admitting the supposition that the eye appreciates and approves of the equal division of the space about a point, this figure is the most perfect which can be conceived. Every line makes with every other line a good angle. The male figure was stated to be constructed upon the female figure by altering most of the angles in the proportion of 9 : 8; the proportion which the ordinary untempered flat seventh bears to the tonic.

A drawing was exhibited, which had been designed with great care from the life, by the distinguished academician John A. Houston, Esq. On this drawing the author had constructed his diagrams; and the coincidence of theory with fact was seen to be complete. Professor Kelland concluded by claiming for the author the attention of the Society. He argued, that a principle so simple and comprehensive in its character, and thus far apparently truthful in the conclusions to which it leads, merits, and should receive, the most complete and rigid examination. Whatever might be the ultimate result (and it promised to be satisfactory in the extreme), the ingenuity, energy, and zeal, shown by the author, entitle him to our warm approbation.

The following Donations to the Library were announced:

Magnetical and Meteorological Observations made at the Royal Observatory, Greenwich, 1847. 4to.—*From the Observatory.*

Journal of the Statistical Society of London. Vol. XIII., Part 1, 8vo.—*By the Society.*

Deuxième Mémoire sur le Daltonisme, ou la Dyschromatopsie, par E. Wartmann. 4to.—*By the Author.*

The Accommodation of the Eye to Distances. By William Clay Wallace, M.D. 8vo.—*By the Author.*

Transactions of the Zoological Soc. of Lond. Vol. III., Pts. 5 & 6. 4to.
Proceedings of Do. Parts 15 & 16. 8vo.
Reports of Council of Do. 1849. 8vo.—*By the Society.*

Monday 15th April.

Rev. Dr GORDON in the Chair.

The following Communications were read:—

1. On the Constitution of Codeine, and its Products of Decomposition. By Thomas Anderson, M.D.

The author commenced his paper by referring to the analysis of codeine made by different chemists. On these analyses four different formulæ had been founded; but two only, those of Regnault and of Gerhardt, required special mention, the others being now known certainly not to represent the constitution of the base. Regnault had deduced from his analysis the formula $C_{35}\ H_{20}\ NO_5$, while Gerhardt gives $C_{36}\ H_{21}\ NO_6$ as the expression of his results.

The author submitted codeine to careful analysis, and obtained the following results:—

					Calculation.
Carbon,	71·91	72·02	72·09	72·09	72·24
Hydrogen,	7·05	7·04	7·14	7·16	7·02
Nitrogen,	4·41	4·60	4·50	...	4·68
Oxygen,	16·63	16·34	16·27	...	16·06
	100·00	100·00	100·00		100·00

agreeing closely with the formula $C_{36}\ H_{21}\ NO_6$, and confirmed by the analysis of its platinum salt, which contains an equivalent of water, and gave, as the mean of seven experiments, 19·25 per cent. of platinum, while the calculated quantity is 19·19 per cent.

The author then describes in detail the properties and constitution of its salts. The hydrochlorate crystallizes in groups of short radiated needles, the formula of which is $C_{36}\ H_{21}\ NO_6\ HCl + 4\ HO$. The hydriodate is obtained in long needles, which, dried at 212°, retain two equivalents of water, and have the formula $C_{36}\ H_{21}\ NO_6\ HI + 2\ HO$. The sulphate, nitrate, phosphate, oxalate, hydrosulphocyanate, and platinochloride are also described.

The author then proceeds to the consideration of the products of decomposition of codeine.

When treated with strong sulphuric acid, codeine passes into an amorphous condition, similar to that in which quinine is obtained when treated with an excess of acid, and in which state it forms resinous compounds with acids.

With dilute nitric acid it gives a new base, nitrocodeine, the formula of which is $C_{36} H_{20} (NO_4) NO_6$, which is precipitated from its solution by ammonia, in minute silvery crystals, sparingly soluble in water, but dissolving readily in alcohol and ether; and crystallising on cooling in small yellowish needles. It dissolves readily in acids, with the formation of salts, which have a more or less yellow colour; and all crystallize except the hydrochlorate. Of these the hydrochlorate, sulphate, oxalate, and platinochloride are described.

By the action of bromine, two different bases are obtained—bromocodeine and tribromocodeine. The first of these is prepared by adding bromine water to powdered codeine until it is dissolved, and then precipitating with ammonia, when the base is thrown down as a crystalline powder, which is obtained in needles by solution in boiling water or alcohol. Its formula in the crystallized state is $C_{36} H_{20} Br NO_6 + 3 HO$. Its salts are similar, in most of their properties, to those of codeine, and all crystallize in small needles. By the further action of bromine, a yellow powder, sparingly soluble in water, is obtained, which is the hydrobromate of tribromocodeine, and from which the base is obtained by solution in hydrochloric acid, and the addition of ammonia. Tribromocodeine is a gray powder, insoluble in water and ether, but soluble in alcohol; it is an extremely feeble base, but dissolves in acids and forms salts, all of which are sparingly soluble in water and amorphous. Its formula is $C_{36} H_{18} Br_3 NO_6$.

The author found that chlorine, by acting upon codeine, gave rise to amorphous compounds, which were not obtained of definite constitution; but by the use of a mixture of chlorate of potash and hydrochloric acid he obtained chlorocodeine, $C_{36} H_{20} Cl NO_6$, similar in its general properties and constitution to bromocodeine, and resembling that substance so closely that it may be easily mistaken for it.

By the action of cyanogen another base was obtained. This substance is best prepared by passing cyanogen into codeine dissolved in the smallest possible quantity of alcohol. The gas is rapidly absorbed, and there is deposited from the solution a mass of crystals which, when dissolved in alcohol, are obtained in six-sided plates, with a fine silvery lustre. These crystals gave to analysis the following results :—

Carbon,	68·22	68·04
Hydrogen,	5·93	6·17
Nitrogen,	11·81	11·50
Oxygen,	14·04	14·27

and the author attributes to them the formula $C_{36}\,H_{21}\,NO_6\,2\,C_2\,N$, and gives to the substance the name of bicyanocodeine. It is a base; but owing to its extreme instability, no salts could be obtained. When treated with an acid it is rapidly decomposed, ammonia being formed, and, after a time, hydrocyanic acid evolved.

By treating codeine with a mixture of potash and lime, at a temperature of 250° Fahr., it undergoes slow decomposition, and a volatile base is evolved, which differs according to the circumstances of the experiment. The author found that, under certain circumstances, the base evolved had the formula $C_6\,H_9\,N$, and forms the term in the series of bases homologous with ammonia, which corresponds to metacetonic acid, and which may be called metacetamine. Under other circumstances the base evolved had the formula $C_2\,H_5\,N$, and corresponded, in all its properties, with the methylamine of Wurtz.

The following is a tabular view of the constitutions of the substances described in this paper :—

Codeine, . . .	$C_{36}\,H_{21}\,NO_6$.
... crystallised, .	$C_{36}\,H_{21}\,NO_6 + 2\,HO$.
Hydrochlorate, . .	$C_{36}\,H_{21}\,NO_6\,HCl + 4\,HO$.
Hydriodate, . .	$C_{36}\,H_{21}\,NO_6\,HI + 2\,HO$.
Sulphate, . . .	$C_{36}\,H_{21}\,NO_6\,HO\,SO_3 + 5\,HO$.
Nitrate, . . .	$C_{36}\,H_{21}\,NO_6\,HO\,NO_5$.
Phosphate, . .	$(C_{36}\,H_{21}\,NO_6\,HO)\,2\,HO\,PO_5 + 3\,HO$.
Oxalate, . . .	$C_{36}\,H_{21}\,NO_6\,HO\,C_2\,O_3 + 3\,HO$.
Hydrosulphocyanate, .	$C_{36}\,H_{21}\,NO_6\,HC_2\,NS_2 + HO$.
Platinum salt dried at 212°, . .	$C_{36}\,H_{21}\,NO_6\,HCl\,Pt\,Cl_2 + HO$.
... crystallised, .	$C_{36}\,H_{21}\,NO_6\,HCl\,Pt\,Cl_2 + 3\,HO$.

Amorphous codeine, .	$C_{36} H_{21} NO_6$.
Nitrocodeine, . .	$C_{36} H_{20} (NO_4) NO_6$.
Sulphate, . . .	$C_{36} H_{20} (NO_4) NO_6 HO SO_3$.
Platinum salt, . .	$C_{36} H_{20} (NO_4) NO_6 HCl Pt Cl_2 + 4 HO$.
Bromocodeine, . .	$C_{36} H_{20} Br NO_6$.
... hydrate, . .	$C_{36} H_{20} Br NO_6 + HO$.
... terhydrate, .	$C_{36} H_{20} Br NO_6 + 3 HO$.
Hydrobromate, . .	$C_{36} H_{20} Br NO_6 HBr + 2 HO$.
Platinum salt, . .	$C_{36} H_{20} Br NO_6 HCl Pt Cl_2$.
Tribromocodeine, .	$C_{36} H_{18} Br_3 NO_6$.
Hydrobromate, . .	$2 (C_{36} H_{18} Br_3 NO_6) 3 HBr$.
Platinum salt, . .	$C_{36} H_{18} Br_3 NO_6 HCl Pt Cl_2$.
Chlorocodeine, . .	$C_{36} H_{20} Cl NO_6$.
... terhydrate, .	$C_{36} H_{20} Cl NO_6 + 3 HO$.
Sulphate, . . .	$C_{36} H_{20} Cl NO_6 HO SO_3 + 4 HO$.
Platinum salt, . .	$C_{36} H_{20} Cl NO_6 HCl Pt Cl_2$.
Bicyanocodeine, . .	$C_{36} H_{21} NO_6 2 C_2 N$.
Metacetamine, . .	$C_6 H_9 N$.

2. On the *Physical* and Scottish *Statutory* Limits of Sea and River, as applicable to Salmon Fisheries. By Dr Fleming.

Dr Fleming directed the attention of the Society, in the first instance, to the characteristic features of *sea* and *river* proper; and then proceeded to consider the peculiarities of that *common space*, alternately sea and river, to which he restricted the term *estuary*. He then considered the nature of the space between high and low water, and pointed out the *mean level*, or mid-tide mark, as the only constant and universally applicable boundary plane. The influence of the tidal wave in reversing the current, checking the velocity, and increasing the depth of the river, was next brought under notice, and an experiment exhibited, illustrating the conservation of force, which causes the waters at the head of an estuary, and the connected river, in certain circumstances, to attain a *higher level* than the high-water mark of the neighbouring sea-shore. He then considered, successively, the tests which, on different occasions, had been proposed and employed; viz.—point of stagnation; presence of sea or river water; the growth of sea-weeds; fauces terræ; deltas and bars; and pointed out their uselessness in determining the physical limit between sea and river.

The second part of the paper was occupied with an examination of the Scottish *statutory* limit of sea and river, as applicable to the salmon fisheries; in which the author indicated *low-water mark*, *as the only limit contemplated*, and justified the sagacity of our ancient legislators, by proving that, with this limit, the object of the statutes was secured. He pointed out the inapplicability of the *physical* test which he had previously established, and of the spurious ones which had been noticed, to the settlement of the fishery question. He concluded, by expressing his regret, that the Legislature had declared certain engines, for catching fish, to be legal or illegal, according as they are used in sea or river, without defining what is *sea* or what is *river;* and his expectation that, should any bill be brought into Parliament, in connection with this subject, the present state of the law will not be permitted to remain in *culpable obscurity*.

3. On the Combined Motions of the Magnetic Needle, and on the Aurora Borealis. By J. A. Broun, Esq. Communicated by Sir T. M. Brisbane, Bart.

When a steel needle or rod is so constructed that its centre of gravity is in a finely-turned axle at right angles to its length, it will rest in any position when the axle is placed upon polished planes; when, however, we magnetize the needle, it assumes a position which is that of the direction of the magnetic force at the place: in this way we obtain the ordinary dipping-needle. The dipping-needle can obviously move only in one plane, that to which the axle is at right angles; were it possible to suspend it freely, so that it could move in every plane with every variation of the direction of the magnetic force, we should then be able, by observing the variations of its position, to determine at once the laws which a magnet in its true position obeys; this, however, we have not been able to do; even the small variations in the vertical plane, which we might expect to obtain from the ordinary dipping-needle, are nearly or altogether destroyed by the friction of the axle upon its supports; and there are many mechanical difficulties in the way of the other methods of suspension. It has been found convenient, then, to make use of the simplest methods of suspending magnets in a horizontal plane; and to endeavour to deduce, from the composition of their motions, the laws both of the variation of the force with which a truly suspended magnet is directed, and of the direction of that force itself.

The most convenient of these is that termed the declination magnet, which is suspended horizontally by a fine silken thread; the tendency of the needle to dip being obviated by placing the point of suspension north of the centre of gravity. This instrument is very convenient, especially in high latitudes, for exhibiting in a magnified form that portion of the motion of the freely suspended dipping-needle, which is at right angles to the vertical plane of the needle. Two other instruments, one termed the bifilar magnetometer, from its suspension by two threads; the other named the balance magnetometer, from its resemblance to the beam of a balance, enable us to observe the variations of the horizontal and vertical components of the force with which the freely suspended dipping-needle is directed; whether these variations be due to a change in the total value of the force, or simply to a change in its direction parallel to the vertical plane. In high magnetic latitudes, the bifilar or horizontal component magnetometer will be most affected by changes of the direction of the force in the vertical plane, and the balance or vertical component magnetometer will be most affected by variations of the intensity of force: in low latitudes the reverse is the case. In all three instruments the magnets are forced from their natural position. By means of a well-known formula, however, we can compute, from the observed variations of the two components, the variations of the total force, and of its direction in the plane of the magnetic meridian. Theoretically this operation is simple enough, but practically there are great difficulties; these difficulties are due to the effect of temperature upon the positions of the bifilar and balance magnets, which require to be eliminated, and to sources of error that I have pointed out in the Edinburgh Transactions in the determinations of the change of value of either component of force, which corresponds to a change of, say one minute in the angular positions of the magnets. I conceive that I have, by the employment of new methods, reduced the errors due to these causes to a very small amount; and it is for this reason that I claim for the results deduced from the Makerstoun Observations, a consideration which they could not otherwise have been entitled to. I refer to the part of the Transactions now in the press, for the results relative to the *separate* magnetic elements, and to the total force; I confine myself at present to those touching the *motions* of a magnet supposed freely suspended in the direction of the magnetic force.

I may state shortly the process by which the following results have been arrived at. The corrected observations for each of the three magnetometers having been discussed with reference to a particular argument; such as, the month, the moon's age, the moon's position in declination, the sun's hour angle, and the moon's hour angle; the motion of the (supposed) freely suspended needle at right angles to the plane of the magnetic meridian, was obtained with reference to the argument in multiplying the corresponding variations of declination by a constant factor (the cosine of the dip); the motion parallel to the same plane was obtained from the variations for the two components by the formula already referred to; the value of the former part of the motion for any epoch being taken as the abscissa, and that of the latter for the same epoch as the ordinate, the motion of the north end of the needle is constructed.

Annual Motions.—The difficulty of determining the law of annual variation of any of the magnetic elements has been so great, that it is doubtful whether that for the magnetic declination has ever been obtained, though the instrument upon which its determination depends is unaffected by variation of temperature. I believe that I have succeeded in the determination of the laws of all the elements, and from these the annual motion has been constructed. The annual motion deduced from the observations of the three magnetometers for the four years 1843, 1844, 1845, and 1846, is shewn in figure A; another and rather more symmetrical figure, deduced from a different combination of years, is shewn in figure B, Plate VI., Edin. Trans., Vol. xix., Part 2.

From near the vernal till the autumnal equinox the annual motion forms the half of an ellipse whose major axis, passing at the vertex through June, makes an angle of about $+11^\circ$ in figure A and of $+16^\circ$ in figure B with the projection of the magnetical meridian. At the autumnal equinox the north end of the needle again ascends till the winter solstice, after which it descends till the vernal equinox. In its descent, the north end of the needle having crossed its previously ascending path, it forms a loop which, when untwisted and continued downwards from the equinoxes, completes the ellipse; the portion formed by the loop having almost exactly the same perimeter as that regularly formed when the sun is north of the equator;

the completed portion is indicated by dotted lines in figures A and B. It does not seem improbable that in southern latitudes the figure will be inverted, and that it will be a simple ellipse near the equator.

Monthly Motions.—The motion corresponding to the moon's varying phase has not been projected, chiefly because of the irregularities still existing in the result of the four years' observations for the magnetic declination, the epoch of minimum being ill-determined; it is conceived that the figure is a simple ellipse with its major axis in the astronomical meridian, the northern extremity being at conjunction, the epoch of minimum dip, and the southern extremity at opposition, the epoch of maximum dip; this, however, is doubtful.

The motion for the moon's position in declination has been obtained in the following manner:—Having first projected the means of magnetic declination for *each* three days of the moon's position in declination, as obtained from the Tables for the years 1843–6, the day after the farthest northerly position being the abscissa, a curve was passed freely among the points; the values of the ordinates at the points of intersection by the curve were then taken as the interpolated value of magnetic declinations for the corresponding abscissæ: a similar operation was performed for the magnetic dip. In both cases very satisfactory curves, agreeing nearly with the true points, were obtained. These values are projected in figure C, Plate VI., Edin. Trans., Vol. xix., Part 2. From this figure the north end of the dipping-needle commences its ascent about two days after the moon is north of the equator, attains its highest point about two days after the moon is farthest north, and afterwards it descends till the moon is again near the equator; thus forming a figure like a portion of an ellipse with its vertex about one day after the moon is farthest north, the major axis making an angle of about $-30°$ with the magnetic meridian. It will be remarked that so far this motion is quite similar to that for the sun's position in declination, with the exception of the axis of the figure being on the opposite side of the magnetic meridian; when we trace the figure farther, the analogy still subsists;—as the moon proceeds south of the equator the north end of the needle again ascends till the moon is farthest south, thereafter descending, and, in crossing its previously ascending path, a loop is formed lying partially out of the principal figure, as in the case of the annual motion.

The correspondence of the two results gives a great weight to the accuracy of both; this will be more evident when it is remembered, that the whole motion of the dipping-needle for the moon's varying declination is included by a small circle with a diameter of little more than *one-tenth of a minute of space*, and, that no observation in the sixty thousand employed for this result has been rejected, however greatly affected by disturbance; although the graphic interpolation to remove slight irregularities may be considered as an equivalent operation.

Diurnal Motions.—The monthly mean diurnal variations for the magnetic declination and magnetic dip, from four years' observations, still present irregularities, especially from 10^h P.M. till 4^h A.M., the hourly positions for this time depending on only two years' observations. For this reason, the values from the Tables having been projected, curves were passed freely among the points, and the interpolated ordinates thus formed, were taken for the projections in Plate VII.: the interpolated quantities differ very little from the actual values, and this is especially the case for the summer months.

The diurnal motions for the four winter months, November to February, are of the same class, and they differ considerably from those for the other months (see Plate VII.): in each of these months the motion consists of a figure of two closed loops: the north end of the needle moves eastwards with little change of dip from about 1^h P.M. till 9^h or 10^h P.M., after which it turns westwards, and begins to ascend about 4^h A.M., crossing near its position at 6^h P.M.; thus forming an eastern loop, which is small compared with the western loop, excepting in December. After 6^h A.M., the north end of the needle having moved a little westwards, again descends, crossing a second time the afternoon track near 5^h P.M.; still moving westwards, it ascends about 11^h A.M. till it meets the position of 1^h P.M., thus completing the western loop. The eastern loop is not formed in March, the north end of the needle not rising sufficiently high to cross the afternoon track. The change in the figure from February to March is very great; in April and May the remains of the eastern loop are still visible, but in June and July its position is indicated by a simple inflection in the figure; in August and September the germ of the eastern loop becomes more distinct, and in October the loop is actually formed. The transition in form from autumn to winter is quite gradual, unlike that from winter to spring. In the winter months, the principal or western loop is formed by the motion from 8^h A.M. till 5^h P.M.; in the months from April to August, three-fourths of the whole diurnal motion occur between 6^h A.M. and 6^h P.M., the remaining fourth forming a slightly inflected side to each of the figures: it is this side which is gradually twisted up to form the eastern loop of the winter months.

It is evident that no proper comparison can be made of the areas of these figures on account of the involved forms in the winter months; the areas, however, of the figures from April to August, differ very little.

Perimeters of the Figures.—The twisting of the perimeters, which renders a comparison of the areas of little value, does not appear to affect the length of the motion, and this therefore seems a fair subject for examination. The following are the values of the angular motion, or length of the perimeter, for each month, as obtained approximately from Plate VII.

Jan.	Feb.	March.	April.	May.	June.
5′·60	6′·16	9′·22	12′·18	12′·04	12′·00
July.	Aug.	Sept.	Oct.	Nov.	Dec.
11′·56	11′·64	10′·48	9′·78	7′·22	5′·84

December and January shew the least perimeters, April, May, and June, the greatest, though the perimeters for the months from April to August are nearly constant.

Hourly Angular Motions.—Having obtained the approximate motion from hour to hour for each of the monthly figures, we find that, on the whole, they follow nearly the same law, that indicated in the following numbers, which are the means for each two hours of the *hourly* motions from the 12 separate months.

12^h	14^h	16^h	18^h	20^h	22^h	0^h	2^h	4^h	6^h	8^h	10^h
0′·43	0′·48	0′·46	0′·62	1′·19	1′·60	1′·34	1′·08	0′·99	0′·60	0′·57	0′·29

These numbers give the following curious result;—That the velocity of motion of the north end of a magnet freely suspended in the direction of the magnetic force is a maximum when the sun makes its superior transit of the magnetic meridian (between 10^h and 11^h A.M.), and a minimum when it makes its inferior transit of the same meridian (between 10^h and 11^h P.M.). This result is the more curious that the epoch of the minimum velocity of the diurnal motion is an epoch of maximum disturbance; and, in as far as the declination is concerned, the epoch of maximum velocity of the diurnal motion is also an epoch of minimum disturbance.

When we compare the results for the irregular disturbance, with reference to the separate elements of magnetic declination and magnetic dip (see Ed. Trans., Vol. xix., Part 2), with the velocities of motion as deduced from these figures, we find, that *when the diurnal motion is most rapid the departures from the direction of that motion are least*, and *when the diurnal motion is slowest the irregular departures from the hourly mean position are greatest.*

Thus, if we examine the mean disturbance of magnetic declination for each hour, as deduced from two years' observations, we find it a maximum during the hours from 8 P.M. till 2 A.M.; this is the period for which the motion of the needle is at once slowest and least as regards the declination; about 21^h (referring to the figure for the year, see Plate VIII., Edin. Trans., Vol. xix., Part 2), the motion is most rapid and nearly altogether in declination, the minimum disturbance in declination occurs immediately before this hour; another and nearly equal minimum occurs under the analogous circumstances about 5^h P.M.; a secondary maximum occurring about 1^h or 2^h P.M.

If we approximate to the hourly mean disturbance of the magnetic dip by means of those deduced for the two components of force, we find the *minimum* to occur about 6^h–7^h A.M., when the velocity of motion is considerable, and when almost wholly in the *direction* of dip; the disturbance increases from that time till about 2^h A.M., shewing a secondary minimum about 1^h P.M. and about 8^h P.M., at both of which times the direction of motion is chiefly that of dip: the maximum disturbance occurs from about 10^h P.M. till 3^h A.M., during which period the velocity of motion is least.

On the whole, then, the magnetic disturbance appears to be chiefly at right angles to the direction of the motion of the needle, and to be inversely as the velocity of motion.

It is scarcely possible to connect the previous facts of area, perimeter, or velocity of motion with the laws of variation of temperature. In the mean for the whole year, the temperature changes most rapidly between 8^h and 9^h A.M.; but it changes with nearly equal rapidity between 5^h and 6^h P.M. There is no corresponding fact in the previous numbers. When we compare the variations of temperature with the variations of position for the suspended magnet in the summer months, we find the difference between the two classes of facts even more marked: in summer, the temperature changes most rapidly about 7^h A.M. and 7^h P.M., the change for May, June, and July, from 6^h–8^h A.M., being $+ 3°{\cdot}80$, and from 6^h–8^h P.M., being $-3°{\cdot}54$; for the same months, the mean angular motion of the needle from 6^h–8^h A.M. $=1'{\cdot}00$, from 9^h–11^h A.M. $=2'{\cdot}12$, and from 6^h–8^h P.M. $=0'{\cdot}74$. There is a diminution in the velocity of the motion between 1^h and 2^h P.M.; there is also a slight diminution at the turning point, 6^h–7^h P.M., and between 2^h and 3^h A.M. These diminutions appear to be connected with the fact, that they occur at turning points in the figures.

It may be remarked that the line representing the astronomical meridian, and passing through the centre of gravity of the perimeters of the figures, for the months during which the sun is north of the equator, also passes through the position of greatest velocity, and nearly through that of least velocity, of the diurnal motion.

General Form and Turning Points of the Diurnal Motions.—The general forms of the diurnal motion vary between rude ellipses and circles. In the winter months, the principal portion, or loop of the figures, is elliptical, with the major axis horizontal; near the equinoxes, the figure becomes somewhat circular, and in the midsummer months it again becomes rudely elliptical, with the major axis inclined about 20° or 30° west of the magnetic meridian. In the usual investigations of the conventional element of declination, it has been remarked, that the turning from the farthest westerly position occurs near the time of maximum temperature; a coincidence which has been supposed to indicate a real connection, though there is no similar coincidence between the epoch of minimum temperature, and the eastern turning point. If, however, we examine the figures indicating the diurnal motions of a needle in its *true* position, such as those for the months of April, August, October, &c., we might find it difficult to say where is a turning point and where not; and it is difficult to see why the turning points at the extremities of the horizontal diameters of these rude circles, or at the extremities of *a* horizontal line, in the ruder ellipses, should be chosen, in preference to the turning points at the extremities of other lines drawn in the figures, as tests for a theory; unless, indeed, it be explained by the accident that a horizontal suspension of a magnetic needle, is a convenient one for observing a certain portion of the motion of a magnet, which, independently of gravity, would rest in the direction of the magnetic force.

It has been customary, however, to give theories of the cause of magnetical variations, with reference solely to the diurnal variations of the magnetic declination (and not unfrequently with a very indifferent knowledge of the facts with respect even to that element). I venture to say, that it will only be from a careful comparison of the whole facts relating to the motions of a freely suspended dipping needle, not for one place, but for different and distant portions of the earth's surface, that a satisfactory theory will be obtained. The attempt to deduce one from a consideration of the declination variations alone, can only be likened to a similar attempt with reference to planetary motions, the apparent position of the planet being studied without any relation to the direction or rate of motion of the place of observation.

Dr Lloyd, who has done so much for magnetical science, has lately brought forward a discussion of his declination observations, which he considers strongly in favour of the theory that the diurnal variations of magnetic declination are due to the sun's heating effect upon the earth, in opposition to the atmosphere. I venture also to offer my guess, founded upon a consideration of various meteorological facts, that it is in the atmosphere, and not the earth, that we shall find seated the secondary causes of magnetic variations. In the meantime, it is *facts* that are wanted.

It may be noticed, chiefly with reference to the months from March to October, that a line passing through the positions of noon and midnight, also passes through, or nearly through, the mean position, or the centre of gravity, each hour having equal weight: also a line passing through the positions, about four hours before, and four hours after noon, passes nearly through the centre of gravity of the perimeters; the former of these lines lies nearly in the direction of the minor axis, the latter nearly in that of the major axis of the rude ellipses for the midsummer months. The horizontal line passing through the centre of gravity also passes nearly through the positions of 1^h A.M. and 1^h P.M., which, therefore are the epochs of mean dip.

Angular Distances between the Hourly Positions from the Mean of all, and from the Undisturbed Days.—In order to render the following result intelligible, it must be stated that, after a careful examination of each day's observations in the years 1844 and 1845, a series was selected, in each month, of days nearly unaffected by magnetic irregularity; the diurnal variation was then obtained for these undisturbed days, and this was compared with the diurnal variation deduced from all the observations; the assumption being made that the mean for the whole 24 hours was unaffected by disturbance, the differences of the hourly values would evidently shew the effect of disturbance on the *hourly* mean position. This assumption, it was found, must be as nearly as possible true for the magnetic declination, because the monthly means of the selected days differed little or nothing from those of all the days; this, however, is not the case for the element of dip, the disturbance appeared to affect the daily or monthly mean to a small extent. Confining myself here to the result for the year (referring to the volume of the Transactions for the partial results which vary with season); the following numbers indicate the displacement of the mean hourly positions by disturbance, upon the assumption that the centre of gravity for each figure is the same:—

12^h	14^h	16^h	18^h	20^h	22^h	0^h	2^h	4^h	6^h	8^h	10^h	12^h
0·35	0′·25	0′·06	0′·15	0′·27	0′·30	0′·23	0′·31	0′·30	0′·17	0′·31	0′·39	0′·35

The diameter of the figure is little greater than 2′·0.

In the mean figure for the year (see Plate VIII. already referred to), minima occur at 4^h A.M. and about $5\frac{1}{2}^h$ P.M., the maximum occurs about 10^h P.M., and a maximum occurs between 8^h A.M. and 4^h P.M. If, making allowance for the effect of disturbance on the position of the centre of gravity with reference to dip, we suppose the centre of gravity of the dotted figure for the year, raised 0′·15 on the line of mean declination, or that of the continuous figures lowered as much, we find the maximum effect of disturbance to occur about 10^h P.M. and 10^h A.M., and the minimum effect about 4^h A.M. and 5^h P.M. This result was obtained for the magnetic declination in 1844, and is given in the volume for that year.

Motions with reference to the Moon's Hour-Angle.—These, as obtained from the means of all the lunations in the years 1844 and 1845, and as deduced from winter lunations for 1845 only, are shewn in Plate VII. The resulting figures, especially that for the winter lunations of 1845, bear some resemblance to the diurnal motion for the month of December.

Aurora Borealis.

A table of 184 auroræ seen at Makerstoun in years 1843 to 1849 is given in pages lxxv.–lxxviii. of Vol. xix., Part 2; from this table the following results have been obtained:—

A very careful outlook for auroræ was kept throughout the whole period, but especially during the first five years; an outlook warned by magnetic disturbance in circumstances unfavourable to the visibility of the meteor, and assisted by a practical acquaintance with the faintest auroral indications. In several cases, the auroral appearances were very faint; these are entered in the table as "Traces," and, in others, there was doubt whether the appearance was truly auroral; these are indicated by "Trace?" It should be noted that, with the exception of the years 1844 and 1845, auroræ were seldom looked for after midnight.

Diurnal variation of frequency of the Aurora Borealis.—The following are the numbers of times which auroræ were seen, at each hour, from 5^h P.M. till 5^h A.M., for the whole period—referring to the printed tables for the numbers for each season.

Hour,	5^h	6^h	7^h	8^h	9^h	10^h	11^h	12^h	13^h	14^h	15^h	16^h	17^h
No.,	5	19	45	57	91	75	50	37	27	15	11	3	2

The greatest number of auroræ were seen at 9^h P.M.; this result is independent of the effect of twilight, since 9^h P.M. is also the hour of maximum frequency for the winter months. This hour is nearly the hour of maximum disturbance for the magnetic declination and dip; as, however, the maximum disturbance of the total magnetic force and a maximum of the magnetic dip appear to occur about 5^h P.M., this also may be an epoch of maximum frequency or intensity, though this can only be determined in higher latitudes. It should also be remarked, that, since the epoch of maximum disturbance varies with season, so, therefore, it is probable will that of frequency of the aurora; some traces of this may be deduced from the previous table. In the winter quarter, November–January, four-fifths of the times at which auroræ were seen were for the hours *before* 10^h P.M., whereas in the spring quarter there were only three-fifths seen before 10^h P.M.

Annual Variation of frequency of the Aurora Borealis.—The first line following contains the numbers of auroræ observed in each month during the six complete years 1843–8, and the second line gives the numbers of hours at which the auroræ were seen.

Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
15	16	26	14	6	0	0	7	13	27	23	11
50	62	65	43	8	0	0	10	32	44	58	38

The greatest number of auroræ were observed in March for the first six months, and in October for the last six months of the year: none were observed in June and July. When the six months of 1849 are included, the number for February is 26, and for March, 28. The law of visible frequency of the aurora is the same as that deduced already for magnetic disturbance; namely, maxima near the equinoxes, and minima near the solstices, the minimum at the summer solstice being the principal. As, however, the shortness of night during the summer months must diminish the number of visible auroræ, it is by no means certain from these numbers that a minimum occurs at the summer solstice; the fact of the minimum at the winter solstice is involved in no such difficulty. If we could assume that the auroræ had the same diurnal law of frequency at all seasons of the year, the existence of the summer minimum could be satisfactorily determined, by comparing the numbers of times which auroræ were seen at the five hours, 10^h P.M.–2^h A.M., during which (even in the months of August and May) there is little twilight to extinguish auroræ. The numbers are as follow, for these five hours in each month of the years 1843–8 :—

Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
15	24	38	31	8	0	0	9	14	16	18	12

From these it is evident that the numbers in May and August are certainly less than for April and September; but it has been already mentioned as probable that the diurnal law of frequency varies with season, of which, indeed, a proof is to be found in the great excess of the numbers above for the spring months, compared with those for the autumn months, shewing the later epoch of the maximum frequency in the former. An examination, however, of the table for the disturbance of the magnetic declination (Table 18, Vol. xix., Part 2), will shew that, though the maximum disturbance occurs after midnight, in the months of May, June, and July; yet in August and the two following months it occurs about 10^h P.M., so that there can be no doubt of the less number for August than for September and October, if there should be a doubt in the case of May compared with April. The difference, however, even in the latter case is too great to be explained by any slight shift of the epoch of maximum frequency in the two months. Upon the whole, it appears certain that a minimum of actual as well as of visible frequency occurs in summer; a result quite in accordance with that for the amount of magnetic disturbance, which accordance is sufficiently close to permit us to complete it, by assuming that the number of auroræ is a *principal* minimum in summer.

It has been stated in the volume for 1844, p. 401, that this result was long ago obtained by Mairan; this statement, made chiefly on the authority of Kæmtz and Hansteen, is not quite accurate. It is true that Mairan's numbers give a rough indication of the law, as will be seen below; but when it is remembered that his table includes all the observations (229) of which he could find a record for upwards 1000 years, it will be evident, that the conclusion that a greater number of auroræ occurred at both equinoxes than at the winter solstice would have been hasty; this conclusion, however, is *not* made by Mairan, and, though he has combined the numbers of auroræ in a great variety of ways, he has made no combination exhibiting this fact. It did not enter into the necessities of his theory (that auroræ are the product of the solar atmosphere) to shew that a greater number of auroræ happened in the northern hemisphere at the vernal equinox than at the winter solstice; he shews, indeed, that the number for one equinox is, and, in accordance with his theory, ought to be, greater than for the other. Some other philosopher has the merit of first pointing out this fact.

The following are the numbers of auroræ by Mairan (Traité Physique et Historique de l'Aurore Boreale, par M. de Mairan, 1733, p. 199); by Kæmtz (Complete Course of Meteorology, translation by Walker, p. 458); and by Hansteen (Mem. de l'Acad. Roy. de Belgique, t. xx., p. 117).

	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Sum.
Mairan, .	21	27	22	12	1	5	7	9	34	50	26	15	229
Kæmtz, .	229	307	440	312	184	65	87	217	405	497	285	225	3253
Hansteen, .	29	31	47	34	2	0	0	17	35	33	34	23	285
J. A. Broun,	22	26	28	16	6	0	0	7	16	29	23	11	184
Sum of last three,	280	364	515	362	192	65	87	241	456	559	342	259	3722

Mairan's numbers are probably included by Kæmtz; a few of the auroræ, included in M. Hansteen's list, are identical with those in my own.

Variation of Frequency of the Aurora Borealis with the Moon's Age.—This investigation is evidently beset with considerable difficulty, since the moonlight existing nearly extinguishes the appearances of all the fainter class of auroræ, and it renders the faintest wholly invisible; the careful watch, however, which was kept for auroral appearances at Makerstoun, probably renders the table given in the Transactions better fitted for such a question than any previous series of observations.

It should be remarked, that the latitude of Makerstoun, or perhaps even a lower latitude, is better fitted for this investigation, than much higher latitudes; at least this is the case as long as only frequency of visibility can be considered. The French *Commission du Nord*, during their stay in Lapland, found auroræ existing, or probably existing, almost every night. In such places variation of frequency there is none, and variation of intensity alone remains for investigation. It is obvious, that till some better mode of measuring this intensity can be devised for these high latitudes, we are forced to perform this operation in a rude manner, by moving to lower latitudes, where the fainter auroræ become invisible, and where, therefore, frequency is a test of intensity beyond a certain limit.

Combining the numbers of auroræ observed at each day of the moon's age into six groups of 5 days (the first group, $4\frac{1}{2}$ days), we find the average number of auroræ for one day of the moon's age in each group as follows, from the $6\frac{1}{2}$ years' observations:—

Moon's age.	28^d—2^d	3^d—7^d	8^d—12^d	13^d—17^d	18^d—22^d	23^d—27^d
Number.	5·8	5·2	3·6	5·0	10·2	6·6

Did auroræ *occur* indifferently at all ages of the moon, we should expect to *see* the greatest number at conjunction, and the least number at opposition; this, however, is not the case, the greatest number was seen about two days before the end of the third quarter, and the least number about two days after the first quarter, or the visible maximum and minimum occurred at times *equidistant* from the epoch of opposition. The frequency of auroræ, therefore, is a function of the moon's age. In order to determine the actual law, we may consider the probable effect of moonlight in obliterating the auroral appearances; remarking, first, that 9^h P.M. is the epoch of maximum frequency for the aurora, and that upwards of five-sixths are seen before midnight. When the moon is about three days old, in the months from September to March, it begins to set sufficiently late, and to have sufficient light to render the earlier of the faint auroræ invisible; about the end of the first quarter, it does not set till midnight, and thus shines throughout the period of the occurrence of five-sixths of the auroræ; afterwards it increases in brightness, and the maximum effect in extinguishing faint auroræ is evidently attained at opposition, when the moon begins to rise late enough to allow the earlier auroræ to be visible; towards the end of the third quarter, when the moon does not rise till midnight, it is also evident that the number of faint auroræ rendered invisible must be very small. From the beginning of the fourth quarter, therefore, till conjunction, the numbers *seen* will obey nearly the true law of frequency; and as the visible maximum occurred before the end of the third quarter, the true maximum must have occurred even nearer to opposition. On the whole, it appears very certain, that the hypothesis of an actual maximum of frequency at opposition, and minimum at conjunction, is satisfied by the previous numbers of auroræ, seen under the conditions of the varying duration of moonlight for the hours of maximum frequency. This hypothesis is in unison with the law of magnetic disturbance, which is a maximum at opposition, and a minimum at conjunction.

Note on the Theory of the Aurora.

Although temptations to frame hypotheses have been avoided hitherto, I cannot refrain from repeating here the opinion, that the phenomena of the aurora borealis are chiefly optical.

After watching the various phases of the aurora for some years, the hypothesis of self-luminous beams and arches appeared to me unsatisfactory; and the strongest argument in its favour, that obtained from the computed height of the auroral arches, seemed of a very doubtful character. I was quite prepared, therefore, to adopt the idea, first I believe proposed by M. Morlet to the French Academy, in May 1847, that the auroral arch is an optical phenomenon of position. M. Morlet has pointed out that the arch appears generally as a segment of a circle; whereas, in these latitudes, it ought invariably to appear as the segment of an ellipse, if the hypothesis be true of a real luminous ring, with its centre on the continuation of the magnetic pole. He has also, among many other very obvious objections to that hypothesis, shewn that the summit of the arch is generally in the magnetic meridian of the place, the plane of which rarely passes through the magnetic pole, and seldom passes through the same point, for three different places. I have, however, felt even more persuaded that the aurora is, partly at least, an optical phenomenon, from a consideration of that phase of the aurora constituting the corona borealis, a persuasion that I stated in the Literary Gazette of the time, in giving an account of the beautiful corona of October 24, 1847.

Mairan, and, more lately, Dalton, have explained this phase of the aurora by a hypothesis of polar beams, long fiery rods of solar atmosphere, according to the one, of red-hot ferruginous particles, according to the other, seen in perspective, as they lie in the direction of the magnetic force. A little acquaintance with the phenomenon—the rushing and tilting of the beams against each other, one beam occasionally rising from the horizon, passing through the centre of the crown and beyond it—would shew the improbability of this hypothesis. I am persuaded, that the phenomena of the corona borealis is produced in a narrow horizontal stratum of the earth's atmosphere. Thanks to the discoveries of Dr Faraday, we do not require a ferruginous sea, in order to have polarized particles; the watery crystals that inhabit the upper regions of the atmosphere can themselves assume a polar state, determined by the passage of electric currents; and we have only to complete this fact by a hypothesis of luminous electric discharges seen refracted by these crystals, the position of visibility of the refracted rays depending on the angles of the crystals, and the deflections from the direction of the magnetic force which they suffer, by the electric currents. Such a hypothesis, which occurs at once when an optical phenomenon has to be accounted for, would explain these remarkable auroral clouds, so often seen in connection with the aurora itself; it would also serve to explain the appearance of the arch at certain altitudes, lower for lower altitudes, determined by the position of the source of light, direction of the magnetic force at the place, and the effect of the electric current in deflecting the crystals. The crystals successively deflected by electric currents would also exhibit the rushing pencils or beams.

It need scarcely be remarked, that differently formed crystals might give rise to different phases of the phenomenon; while reflection might be combined with refraction in certain cases, especially in the case of arches seen south of the anti-dip. Such a hypothesis evidently assumes a source of light, independent of these optical resultants, and the pulsations seen in many auroræ may be real luminosities.

It is hazardous, in the present ill-arranged state of auroral observation, to offer so rude a sketch of a new hypothesis, although we may suffer a considerable defeat in very good company.

Since the previous note was written, I find that M. Morlet has published a theory of the auroral arch (Ann. de Ch., t. xxvii., 3me Série). The ideas above were stated by me two years ago, to different persons.

The following Donations to the Library were announced:

Transactions of the Royal Scottish Society of Arts. Vol. III., Part 4. 8vo.—*By the Society.*

Journal of the Asiatic Society of Bengal. Edited by the Secretaries. N. S. No. 32. 8vo.—*By the Society.*

Annali di Fisica dell' Abbate Francesco Car. Zantedeschi. Fascicolo 4. 8vo.—*By the Author.*

Quarterly Journal of the Chemical Society. No. 9. 8vo.—*By the Society.*

Scheikundige Onderzoekingen gedaan in het Laboratorium der Utrechtsche Hoogeschool. 5de Deel. 6th Stuk. 8vo.—*By the University.*

Bulletin de la Société de Géographie. 3^{me} Série. Tom. 12^{me}. 1849. 8vo.—*By the Society.*

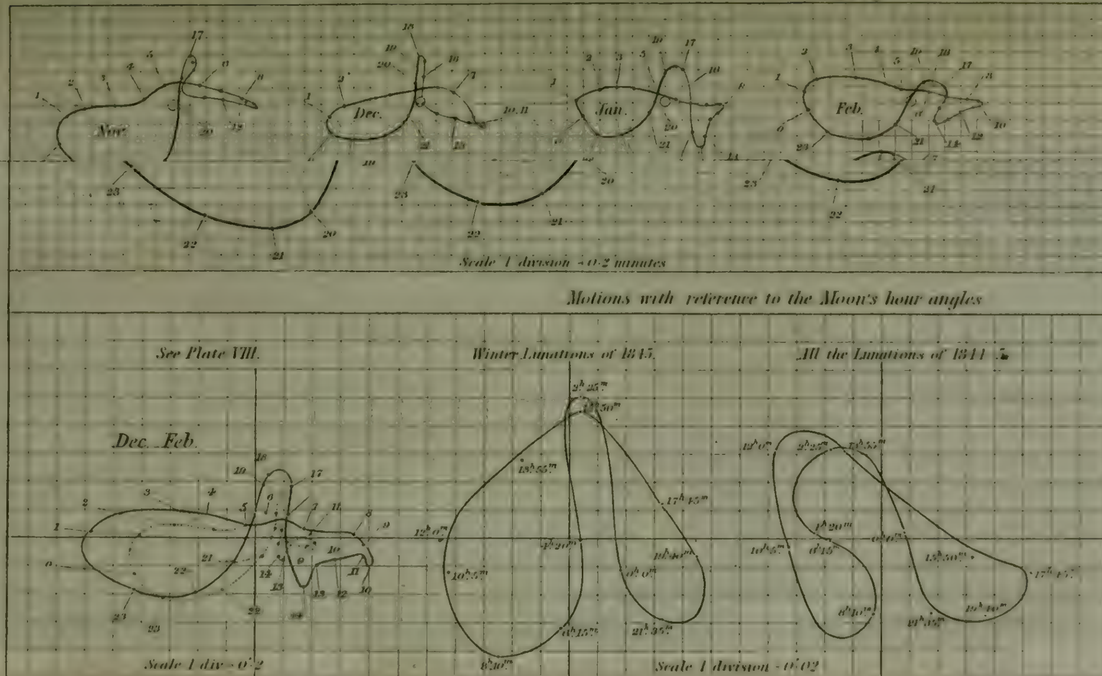
ERRATUM,

Vol. II., No. 33, page 205, line 12 from bottom,

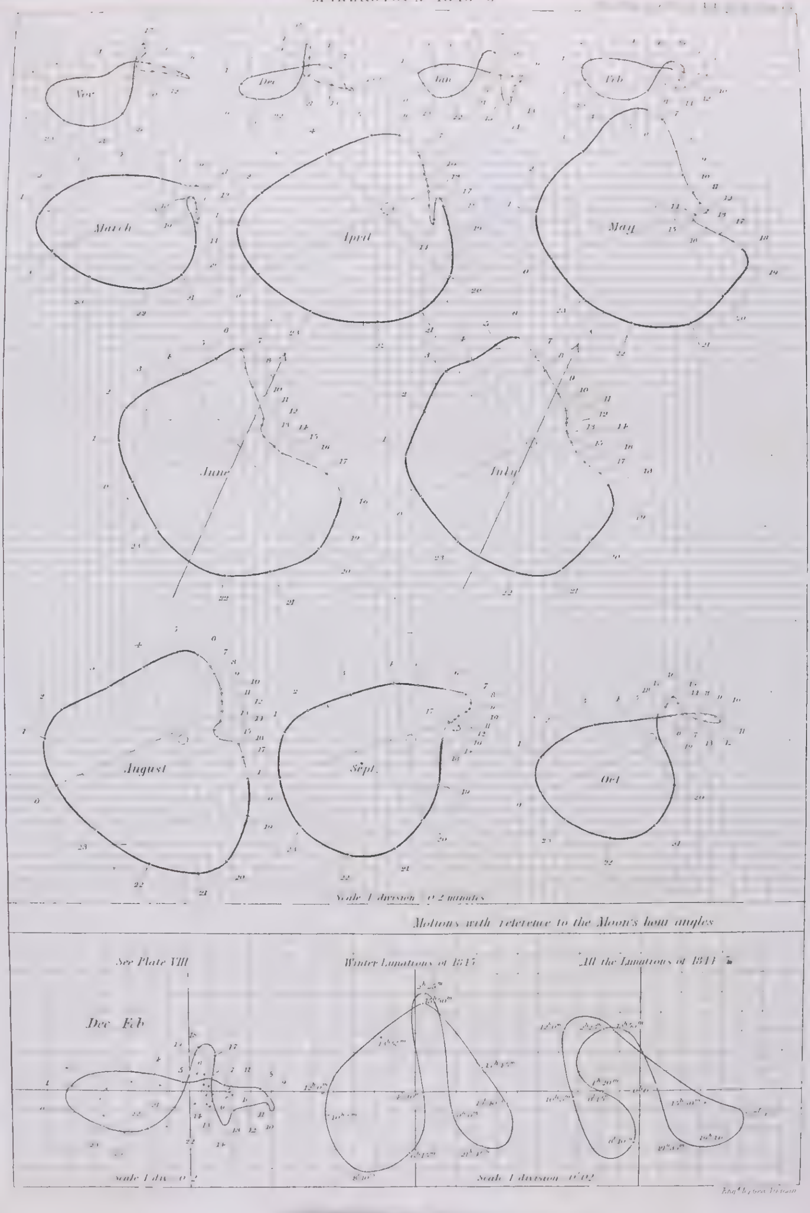
For " These data are (1.)," &c., *read* " These data are (1.), The known expansion of water in freezing; (2.), The known quantity of heat which becomes latent in the melting of ice; and (3.), The quantity of work given out," &c.

DIURNAL MOTIONS OF THE NORTH END OF A FREELY SUSPENDED DIPPING NEEDLE
MAKERSTOUN 1843 6.

Trans. Roy. Soc. Edin. Vol. XIX. Part II. Plate VII.



DIURNAL MOTIONS OF THE NORTH END OF A FREELY SUSPENDED DIPPING NEEDLE
MAKERSTOUN 1845-6



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