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HERTZBERG-NEW METHOD INC

THE ROYAL SOCIETY OF EDINBURGH

PROCEEDINGS

Volume 9

NOVEMBER 1875—JULY 1878

PROCEEDINGS

OF

THE ROYAL SOCIETY

OF

EDINBURGH.

VOL. IX.

NOVEMBER 1875 TO JULY 1878.

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Errata to be corrected in Table at top of page 607 *opposite.*

Column 1,	second row of figures,	*for*	"286·75,"	*read*	"288."
,,	ninth ,,	,,	"573·50,"	,,	"576."
Column 3,	second ,,	,,	"+0·60,"	,,	"−0·65."
,,	ninth ,,	,,	"+1·20,"	,,	"−1·30."

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. IX. 1875–76. No. 93.

Ninety-Third Session.

Monday, 22d November 1875.

Sir ROBERT CHRISTISON, Bart., Hon. Vice-President, in the Chair.

The following Council were elected :—

President.
Sir WILLIAM THOMSON, Knt., LL.D.

Honorary Vice-Presidents.
His Grace the DUKE of ARGYLL.
Sir ROBERT CHRISTISON, Bart., M.D.

Vice-Presidents.
David Milne Home, LL.D.
Professor Kelland.
Rev. W. Lindsay Alexander, D.D.
David Stevenson, Esq., C.E.
The Hon. Lord Neaves.
The Right Rev. Bishop Cotterill.

General Secretary—Dr John Hutton Balfour.

Secretaries to Ordinary Meetings.
Professor Tait.
Professor Turner.

Treasurer—David Smith, Esq.

Curator of Library and Museum—Dr Maclagan.

Councillors.
Dr Arthur Mitchell.
George Forbes, Esq.
Principal Sir Alex. Grant, Bart.
Professor Geikie.
Dr Andrew Fleming, H.M.I.S.
Dr Charles Morehead.
Alexander Buchan, A.M.
Robert Wyld, Esq.
Dr Ramsay H. Traquair.
Dr Thomas Harvey.
Dr John G. M'Kendrick.
Dr J. Matthews Duncan.

Monday, 6th December 1875.

DAVID MILNE HOME, Esq., of Wedderburn, LL.D., Senior Vice-President, in the Chair.

The Chairman delivered the following opening Address:—

Gentlemen, Fellows of the Royal Society of Edinburgh,—In compliance with a request of the Council, I have the honour to come before you this evening to give an address, on this the first night of our Winter Session, in pursuance of the custom prevalent in this and most other Societies.

I need not say how much I regret, for your sakes as well as my own, that this duty is not to be discharged by our eminent President.

The *first* point which I will submit to your notice, is the nature and amount of the work we as a Society are doing, and our means of doing it.

The *second* and concluding part of my Address will have reference to the present aspect and prospects of science generally in the country.

With regard to the work we are carrying on, it may be sufficient to refer to the proceedings of our last winter's session. Our Secretary tells me that it was the longest session he remembers—it having been prolonged beyond mid-summer.

You are aware that our Society was intended by its founders to embrace *literature* as well as *science ;* and that in regard to science, we encourage investigations in any of nature's various fields. The following abstract, under different heads, of the papers read during last session, indicates the range and variety of the Society's operations :—

In Applied Mathematics or Physics, we had 11 papers read; in Pure Mathematics, 9; Notes from Professor Tait's Physical Laboratory were read at five meetings; of Geological papers, 4 were read ; of Chemical papers, 3; of Physiological papers, 3; of Anatomical papers, 3; of Meteorological papers, 2; of Literary papers, 2; separate Biographical Memoirs of eleven deceased Members were read. Many interesting experiments were shown at our meetings; and in particular, our President, at one of our meetings, exhibited and explained his wonderful tide-calculating machine, by means of which there can be obtained in a few seconds, results which hitherto have required minute and laborious calculations.

The three Prizes which the Society has at its disposal, were awarded as follows :—

The *Keith* Prize was awarded to Professor Tait, for his paper on a "First Approximation to a Thermo-Electric Diagram."

The *Makdougall Brisbane* Prize was awarded to Professor Lister, for his paper "On the Germ Theory of Putrefaction and Fermentation."

The *Neill* Prize was awarded to Mr Charles William Peach, for his "Contributions to Scotch Geology and Zoology."

Gentlemen, an important part of our work as a Society is to publish in a volume of Transactions the most deserving of the papers read at our meetings. A copy of these Transactions is, as you know, obtained *gratis* by every member. Copies also, to the extent of considerably above a hundred, go to foreign libraries, foreign universities, and foreign societies. Many of these papers are necessarily not of so popular a character as to pay, by the sale of them, the cost of printing. But these papers, though not interesting to the general community, may be of the highest importance for the advancement of science. Fortunately our Society is sufficiently wealthy to be able to defray the expense, not only of printing, but of a large gratuitous circulation. I believe that it is a knowledge of this fact which obtains for our Society so large a membership, and so satisfactory a revenue.

With regard to our membership, we have now 358 Ordinary Fellows. I observe from the address which I had the honour of giving five years ago, that the number then was 326, so that there has been in the interval an increase of 32 members.

The number of members whom we have lost by death is, I am sorry to say, larger than usual, being altogether 14. The following are the names alphabetically arranged :—

1. *Foreign Honorary Fellow.*—Le Comte de Remusat.

2. *British Honorary Fellows.*—Sir Charles Lyell, Bart. of Kinnordy; Sir William Edmund Logan, LL.D.; Sir Charles Wheatstone, D.C.L.

3. *Ordinary Fellows.*—Rev. Dr D. Aitken; John Auld, Esq.; Professor Hughes Bennett, Edinburgh University; Rev. Professor Crawford, Edinburgh University; Colonel Seton Guthrie, Thurso; Sir William Jardine of Applegarth, Bart.; Professor William Macdonald, St Andrews' University; the Hon. Lord Mackenzie; Edward Meldrum, Esq., Dechmont; the Venerable Archdeacon Sinclair.

I propose to give an obituary notice of several in this list, with regard to whom I have succeeded in obtaining information, chiefly through the good offices of our Secretary, Professor Balfour.

Charles, Comte de Remusat, a distinguished French politician, philosopher, and man of letters, was born at Paris on the 14th of March 1797. His father held various public offices under the first Empire. His mother was an intimate friend of the Empress Josephine. The young Remusat, after a brilliant course at the Lycée Napoleon, betook himself at first to the study of law, but he soon turned to literature, and wrote as a journalist in newspapers and reviews from 1818 till 1830. In company with Guizot, Cousin, and Jouffroy, he was on the staff of the "Globe," a periodical founded by Dubois in 1824, which struggled against the growing absolutism of the Restoration. He continued afterwards, in concert with Guizot, to support *doctrinaire* constitutionalism, and in philosophy he was on the whole of the school of Cousin. His name appears in the list of journalists who protested against the ordinances which brought about the Revolution of July. In 1830, he was chosen deputy by Toulouse, and soon followed the leadership of Thiers in the Chamber. In 1838, he was for a short time Under-Secretary of State in the ministry of Count Mole, and in 1840 he was Minister of the Interior, under Thiers. After the Revolution of 1848, he continued a member of the Constituent Assembly, and supported the party of order. During the whole period of the second Empire, he withdrew from political life, and devoted himself to literary and philosophical labours, sceptical of the possibility of an Imperial government restoring liberal institutions. The Revolution of 1870 brought the Count de Remusat back to public life, as Minister of Foreign Affairs under M. Thiers, with whom he fell in May 1873, and with whom he agreed in regarding the Republic as, if not the political ideal, at least the best practical solution of the difficulties of France. He died at Paris on the 6th of June 1875.

The Count de Remusat was a copious, solid, and eloquent writer. Besides his large contributions to the periodical press, especially the "Revue des Deux Mondes," he was the author of many valuable works. One of his earliest essays was connected with his legal studies, and appeared in 1820 ("Sur la procedure en Matière Criminelle"), followed by other tracts on the responsibility of ministers of State, the liberty of the press, and the law of elections. His most brilliant and productive period as a writer was after 1840. Among his other works are the following:—

1. Essai sur la nature de Pouvoir,	1840.
2. Essais de Philosophie,	1842.
3. Abelard,	1845.
4. Melanges Philosophiques,	1847.
5. St Anselm,	1852.
6. Bacon—Sa Vie, son Temps,	1858.
7. La Philosophie Religieuse,	1864.
8. David Hartley,	1874.
9. Philosophie Anglaise—Bacon jusqu'a Locke,	1875.

As may be inferred from the subjects of his studies, M. de Remusat was deeply interested in England. Probably no eminent Frenchman of his time understood English institutions and national character so well. The practical philosophers and statesmen of this country, and their readiness to accept the teaching of experience and to recognise the tendencies of the age, in a spirit of wise compromise, were all in harmony with his temper; which always inclined to moderation, and was averse to fanaticism, whether political or speculative, religious or anti-religious. In philosophy, he belonged to the school opposed to Materialism.

In M. de Remusat we have lost one of the most eminent of the

French politicians and thoughtful men of letters of the nineteenth century, and the philosophy of mind has lost one of its ablest expositors, though he may not have ranked among its discoverers and leaders.

Charles Lyell was born at Kinnordy, in Forfarshire, on 14th November 1797, and died in London 22d February 1875. He was on our list of British Honorary Fellows. His early education was obtained at Midhurst, in Sussex. He went thereafter to Oxford, and there obtained his A.M. degree in 1821. Whilst at Oxford he had the advantage of attending Dr Buckland's lectures, then Professor of Geology. On leaving the university, he studied for the English bar; but finding this line of life not likely to be congenial, and having the means of living without the aid of a profession, he betook himself to geology. The seed sown by Dr Buckland had been dropped into soil fitted to its germination and rapid development.

Probably Lyell's first paper was an account of a "Recent Formation of Freshwater Limestone in Forfarshire," his native county. This was very soon followed by many other papers, written at places visited by him in Hampshire and Dorsetshire. These were read before the Geological Society of London, of which he had become member. In the year 1824 he had shown such knowledge of geology, that he was elected one of the Honorary Secretaries of the Society.

In 1827 he contributed to the "Quarterly" a review of Mr Poulett Scrope's "Geology of Central France."

Shortly afterwards, he published his "Principles of Geology," the work in which he first showed his distrust of old geological maxims, and started his own original conceptions. Most geologists before his day had attempted to explain many things by assuming that the natural agencies of bygone times had been much more powerful than now. On the other hand, Lyell maintained that the natural agencies now on our planet were capable of producing all the effects observed, if only sufficient time was allowed for their operation.

These new views attracted great attention. The demand for the book in which they were explained was so great, that it went through five editions in a very short time—each edition containing a large amount of new matter. The work, by these numerous additions, became so changed in character, that he reconstructed it, and brought out a new work called "Elements of Geology," and greatly altered his "Principles" as regards arrangements. In the latter, he presented explanations of the various forces at work in the earth and in the universe likely to affect the earth. In the former, he described the observed effects. Subsequently he brought out the "Student's Manual of Geology," in which he brought together most of the facts mentioned in the two previous works.

No geologist before Lyell's time had devoted himself so exclusively and so laboriously to the science. He not only kept himself acquainted with the discoveries made by others, but he travelled over large portions of the earth's surface, with the view of verifying alleged facts, and making di coveries himself.

He went to Norway, Sweden, Belgium, Switzerland, Germany, Spain, Catalonia, and the Danish islands of Seeland and Monen. He was twice in America. On the first occasion, in 1841, he went, in compliance with an invitation, to deliver a course of lectures at Boston. He then remained in the New World a whole year, his explorations extending from Canada through the States to the mouths of the Mississippi. On returning to England, he published his "Travels in North America," in which, whilst geological information chiefly is given, some useful views occur on other subjects also. In 1845 he paid a second visit to America, and examined more particularly the Southern States and the coasts bordering the Gulf of Mexico. On his return to England, he published his "Second Visit to the United States,"—a companion to his former work.

The most recent of Lyell's important works was his "Antiquity of Man," which went through four editions, the first having come out in 1863, the last in 1873. But beside these elaborate works, he published numerous memoirs, most of which had been read at meetings of the Geological Society and British Association for the Advancement of Science.

In 1836, and also in 1850, he was President of the London Geological Society. The Royal Society's Copley Medal was awarded to him in 1858, and the Geological Society's Wollaston Medal was awarded to him in 1866.

In the year 1864 he presided at the Bath Meeting of the British Association for the Advancement of Science. He was Patron of the Scotch Geological Society. In the year 1848 he was knighted, and in the year 1864 he received a baronetcy in recognition of his services to science.

Sir Charles Lyell was married in 1832 to Mary Elizabeth, eldest daughter of the late Leonard Horner, himself a distinguished geologist. Lady Lyell was a devoted wife, and sympathised with her husband in his pursuits, accompanying him in all his travels, and assisting him in literary work.

During the last five or six years, Sir Charles Lyell lost his eyesight to such an extent that he could neither read nor recognise his friends. The last time that I was in his house, Harley Street, London, Lady Lyell had to lead him, and make known to him the presence of several friends who came in.

Lady Lyell's death in the year 1874 was a severe shock to her husband. After that event, Sir Charles's health rapidly failed. His death was caused by a severe fall on the staircase of his house, he having, owing to his blindness, missed the uppermost step.

Probably few men were ever so devoted to any special object, as Sir Charles Lyell was to geology, through his whole life. He was inspired by a genuine love of truth, and for its sake did not hesitate to retract opinions when he found he was mistaken. In the three first editions of his "Antiquity of Man" he had expressed his concurrence in the opinion of some Scotch geologists, that the land near the Firth of Forth had risen 25 feet since the Roman occupation. In the last edition of the work, he revoked that concurrence. In the account given by him of the Glen Roy terraces, he published his belief, that they were due to fresh water lakes. In a letter which I received from him shortly before his death, adverting to some facts recently discovered, he allows, that perhaps after all, Darwin's theory of the terrace having been made by the sea, might prove to be correct. Sir Charles Lyell in this respect showed an example to all men of science, in caring more for the interests of truth, than for mere consistency.

William Edmond Logan, another Honorary Fellow of the Society, was born at Montreal, Canada, in the year 1798, and died on 22d June 1875.

His father was originally a landed proprietor in Stirlingshire, and emigrated to Canada. He sent his son from Canada, when very young, to Scotland, to be educated in the High School, and afterwards in the University of Edinburgh.

When young Logan was in Edinburgh, geological investigations and speculations were exciting much interest, in consequence of the discussion between the Huttonians and Wernerians. Mr Logan then acquired a taste for geology; and having occasion to go to South Wales, he began to study the rocks in the coal-fields there, at this time, beginning to be more extensively worked. Having procured an Ordnance Survey map on a large scale, he was at the trouble to trace out and lay down upon it the outcrop of all the coal seams worked through extensive tracts of country. Seeing where the outcrops ceased to be continuous, he ascertained the amount and direction of the dykes and slips by which the strata had been dislocated. He descended into the mines, and studied for himself the structure of the coal, and examined particularly the fossils found in the coal. He was then struck by the fact, that every coal seam lay upon a bed of blue-coloured clay, in which apparently the plants had grown, now found petrified in coal. In several instances he discovered that some of the fossil trees which had their trunks in the coal-bed had their roots still stretching into the underlying bed of clay.

About this time Sir Henry de la Beche, who was directing the Geological Survey of England and Wales, happened to come into South Wales. Having heard of Mr Logan, he became acquainted with him; and having seen the work he had been carrying on, he at once put him on the staff of the survey.

Mr Logan having permanently adopted geology as a profession, became a Member of the Geological Society of London. Frequently joining in the discussions there, he made the acquaintance of Sir Charles Lyell, Sir Roderick Murchison, and other leading geologists.

Having obtained leave of absence to visit his father in Canada, he went there in 1841, and spent much of his time in exploring the great coal-fields of Nova Scotia and Pennsylvania.

In the spring of 1842 he returned to England, and in the Geological Society gave an interesting account of his survey in these American coal-fields. He had been particularly anxious to obtain a confirmation of his discovery, that coal seams everywhere rested on fire-clay; and he was able to afford these proofs from what he had seen in Nova Scotia.

He had made another discovery in these coal-fields. He had discovered the footprints of a reptile; and he brought to London with him the sandstone slab which contained these prints. This slab was submitted to Professor Owen, who expressed a clear opinion, that the impressions had been made by an animal which had four claws on the two fore feet, and three claws on the hind feet. The interest attaching to this discovery was, that no reptile had been discovered in rocks so old, it being at the bottom of the Carboniferous formation;—whereas, previously, no reptiles had been found below the Permian rocks.

I mention this discovery of Logan's, because I see that my friend Principal Dawson of Montreal, in his "Book on Acadian Geology," mentions that discoveries of similar reptiles, made in the year 1844 in Sweden and the United States, had been asserted to be prior to others of the same kind.

Logan's reputation as a geologist was now established. It led to his being entrusted with the charge of the Canadian Geological Survey, on the recommendation of Sir Roderick Murchison and Sir Henry de la Beche. The Canadian Legislature had wisely resolved to have the mineral riches of the country ascertained by competent surveyers. For nearly thirty years Sir William conducted the Canadian Survey, and drew most important conclusions regarding the whole series of rocks in that part of the world—conclusions universally accepted by geologists as correct.

At the Paris International Exhibition of 1855, he showed a large collection of specimens, besides magnificent maps and diagrams, which attained much attention, and received great commendation. It was on this occasion, that the British Government, in recognition of his eminence as a geologist, and of his services in Canada, bestowed on him the honour of knighthood.

Sir William did not publish anything beyond the official reports of his survey. He was not ambitious of fame, either as an author or otherwise. He stuck closely to the work he had undertaken, and continued at it till the year 1869, when failing health led him to resign.

He, however, continued to take an interest in geological pursuits, and gave, from his private funds, a donation of L.5000, for the endowment of a chair of geology in the M'Gill College, Montreal.

Charles Wheatstone was born at Gloucester in 1802, and died in Paris 19th October 1875. He was on the list of our British Honorary Fellows.

The rudiments of education were obtained by him at a private school. Whether he afterwards went to a university, I have not discovered.

His youth and early manhood were devoted to the construction of musical instruments, and to experiments with the view of discovering more exactly the laws of sound. He paid special attention to the instruments depending on vibrating springs. The present improved Concertina is due to his invention.

His first scientific memoir was in the year 1823, when he published in the "Philosophical Annals" an account of some "New Experiments on Sound." It excited considerable attention among physicists, and was translated into several foreign periodicals. In 1827, in the "Quarterly Journal of Science," he published farther "Experiments on Audition," accompanied by a description of the Kaleidophone, an instrument to illustrate both acoustical and optical phenomena.

During the next eleven years, he continued to produce papers and to invent instruments for illustrating the properties of sound.

In 1838 he seems to have entered on a different subject of investigation altogether, viz., light. He had discovered relations between waves of sound and waves of light. He communicated to the Royal Society of London, and also to the British Association, an account of some hitherto unobserved phenomena of binocular vision, illustrating them by means of the instrument which he invented, called the "*Stereoscope*." To Wheatstone is due the discovery, that the conception of solidity is due entirely to the mental union of two dissimilar perspectives.

In 1852 he invented an instrument called the "*Pseudoscope*," which still farther illustrated the mental action in certain optical phenomena. An article in the "Edinburgh Review" of October 1858, describes thus the effect of the Pseudoscope:—"When an observer looks with it at the interior of a cup or basin, he not unfrequently sees it at first in the real form, but by prolonging his gaze, he will perceive the conversion within a few minutes; and it is curious, that while this seems to take place quite suddenly with some individuals, as if the basin were flexible and were suddenly turned inside out, it occurs more gradually with others, the concavity slowly giving place to flatness, and the flatness gradually rising into convexity."*

Wheatstone was exceedingly interested in this discovery of the interference of mental action with optical phenomena, and invented several instruments with the view of ascertaining the principles on which it depended. The subject led him to study the subject of nervous organisation; but it is believed, that he effected no special discoveries in that field.

In 1834 the science of *Electricity* began to occupy Wheatstone's attention. He endeavoured to ascertain the velocity of the electrical current. He invented many most ingenious machines with that view. He seems to have made only an approximation to the truth, viz., that the current travelled through a mile of wire in less than the 360th part of a minute.

It now occurred to him that electricity might be employed in conveying intelligence along great distances by moving a magnet. By this time an idea of the same kind had occurred in Germany. Mr Cooke, when there, had become informed of the investigations by Schilling, and having come to London, made these known to Wheatstone. A proposal for a partnership between the two, was suggested, and was carried out. Messrs Cooke and Wheatstone

* A curious circumstance, analogous to the phenomena here described, was, without the help of any instrument, observed by me and other friends lately, in watching the revolutions of a cup anemometer on the top of Alnwick Castle. On looking at the instrument, it was seen revolving in a direction consisting with the truth; but on continuing to look at it, in about half a minute the anemometer suddenly appeared to change the direction of its rotation, and to continue so to rotate. We remained for some time looking at the instrument to repeat the experiment. The same result on every occasion followed, and to every one of the party, eight or nine in number.

soon thereafter were employed to establish electric telegraphs on most of the great English railways.

In 1837 the five-needle telegraph was invented; in 1840, the alphabet dial telegraph; in 1841, the type-printing telegraph; and the automatic telegraph between 1858 and 1867. By this last machine it was found possible to transmit words at the rate of from 100 to 160 words per minute.

In 1840 Wheatstone conceived the idea of a submarine telegraph cable, and pointed out both the difficulties and the means of obviating them.

His last work was to contrive a new recording instrument for submarine cables, formed by a globule of mercury moving to and fro in a capillary tube containing acid, or by a drop of acid in a tube containing mercury, and which was found to be 58 times more sensitive than any recorder previously employed. He had gone to Paris to exhibit this invention to his colleagues of the Academy of Science, when he was attacked by the fatal illness—bronchitis—which terminated in his death.

This brief notice of Wheatstone's discoveries in the science of sound, optics, and electricity gives but a poor idea of the immense amount of brain work which he went through in the long life accorded to him. The papers which he contributed to Societies both in Great Britain and on the Continent are very numerous. They were always characterised by great lucidity of style and by copious and telling illustrations, which made them both attractive and instructive.

Wheatstone was elected a Fellow of the London Royal Society in 1836, a Chevalier of the Legion of Honour in 1855, a Foreign Member of the French Institute in 1873. In 1868 the Government of Lord Derby conferred on him a knighthood.

In private life Sir Charles Wheatstone had the reputation of being reticent and unsociable. The fact probably was, that his mind was constantly absorbed with the problems which were constantly presented to it. He was so nervous and bashful, that though always ready and pleased to describe his discoveries to any single individual, he entirely broke down when he attempted to address an audience. Hence, his Professorship of Natural Philosophy in King's College, London, was little better than a title; for he never had a class.

There was no physicist of his time so universally respected. His remains were brought from Paris for interment in the family burial place at Kensal Green. The procession was followed by a vast number of carriages, including many of the nobility; and even the shops in the streets along which the funeral cortege passed were shut, whenever it was known whose it was.

David Aitken, D.D., who had been seven years an Ordinary Fellow of this Society, was born about the beginning of this century. He died on the 27th March last, in his own house in Charlotte Square, Edinburgh. He was educated at the High School and University of Edinburgh, and became a licentiate of the Church of Scotland.

I believe that he had been tutor in the family of the Earl of Minto, by whom, or through whose influence, he was in the year 1829, presented to the parish of Minto. There he remained minister for thirty-seven years; and on resigning his charge, purchased a house in Edinburgh, where he lived till his death.

Being fond of travel, he visited Norway, Italy, Egypt, and Syria. As he suffered extremely during the winter season from delicacy of chest, he often spent the winter abroad. Possessing an independent fortune, he was able to obtain the services of an assistant during his absence.

He was a person of literary tastes, was well acquainted with the German language, and was a friend and correspondent of the German philosopher Hegel. In the year 1827 he wrote an article in the "Edinburgh Review" on German literature. He also drew up the Statistical Accounts of Minto Parish, embodying an excellent account of its geology, botany, and zoology.

His knowledge of Church history was so considerable that he was offered the Chair of Church History in the University of Edinburgh. On his declining it, the late Dr Welsh was appointed. His sermons were in composition marked by great elegance and clearness; but owing to delicacy of chest, his voice was weak, and his manner in the pulpit had not the earnestness necessary to create interest.

He was exceedingly fond of natural history, and took great interest in his garden, which was always kept with scrupulous neatness.

John Hughes Bennett was born in London 31st August 1812; and died at Norwich 25th Sept. 1875. He had joined our Society in 1842.

He was educated at the Grammar School, Exeter. It is stated, however, that he was indebted for the early part of his education to his mother, a lady of brilliant intellectual attainments. Being a great admirer of Shakespeare, she caused her son to read aloud to her many of his plays, and as he did so, taught him the art of emphasis and rhetorical action. Probably to this tuition of his mother, Dr Bennett was indebted for the elegance of his composition, and for the impressiveness of his delivery when he lectured or spoke in public.

He commenced the study of medicine at Maidstone, in the year 1829, under the guidance of a practitioner there. It was there that he acquired the art of dispensing, and even obtained a certain amount of medical practice. He assisted also in *post-mortem* examinations.

To acquire better medical instruction and training, he removed to Edinburgh in the year 1833,—unacquainted with any one in that city or in Scotland. By his talents and assiduity he soon attracted the notice of his professors, and obtained the esteem of numerous fellow-students. His attention was devoted chiefly to anatomy, physiology, and pathology. Having joined the Royal Medical Society, and shown his abilities and knowledge at its meetings, he ultimately became President of the Society. Whilst still a student, in the year 1836, he published two papers which obtained for him considerable credit.

In the year 1837, he received the degree of M.D. with the highest honours, obtaining at the same time a gold medal for his thesis.

After obtaining all the knowledge which Edinburgh could supply, Dr Bennett repaired to Paris, where he studied for two years. Being able to speak and write the French language fluently, he wrote in the French medical journals, and ultimately became President of the Parisian Medical Society.

He also went to Germany, spending some time in the principal University cities, and endeavouring to acquire knowledge beyond what he had already obtained. One of his acquisitions on the Continent was ability to use the microscope in practical medicine. Nor was his pen idle, for whilst abroad, he contributed no less than seventeen articles to Tweedie's "Library of Medicine."

In 1841 he returned to Edinburgh, and commenced a course of lectures on histology. He there took the opportunity of showing to what an extent the microscope might and should be used. It was at this time that Dr Bennett published a treatise on the use of cod-liver oil as a therapeutic agent in certain forms of gout, rheumatism, and scrofula,—dedicating the treatise to Sir Robert Christison. In Germany he had seen the good effects of using this medicine in these cases.

From 1842 to 1848 he continued to give lectures on various medical subjects. In the last named year he was appointed to the Chair of Institutes of Medicine, vacant by the transference of Dr Allen Thomson to Glasgow.

For several years Dr Bennett was proprietor and editor of the "Edinburgh Monthly Journal of Medical Science," in which, besides editorial articles and reviews, he inserted multitudes of separate memoirs.

In the "British Medical Journal," where a detailed account of Bennett's life and labours is given, and from which I have culled the foregoing notices, I see a list of no less than 105 memoirs on various anatomical and pathological subjects.

In July 1848 Dr Bennett was unanimously elected to the Chair of Institutes of Medicine.

Whilst teaching in the University and in the Infirmary, Professor Bennett found time for literary work, and published his highly appreciated "Clinical Lectures on the Principles and Practice of Medicine." This book passed through five editions in this country, and six in the United States, besides being translated into French, Russian, and Hindoo.

The following additional works flowed from his ready pen. Their titles were, "Pulmonary Consumption," "Cancerous and Cancroid Growths," Introduction to Clinical Medicine," "Outlines of Physiology," "Text-Book of Physiology—General, Special, and Practical."

Professor Bennett had conferred upon him numerous honours and distinctions. He was President for two years of the Medico-Chirurgical Society of Edinburgh; Hon. Secretary and *emeritus* President of the Royal Medical Society of Edinburgh; and Fellow of numerous medical societies on the Continent. He had sent to him, about a year before his death, a special licence from the French Government entitling him to practise medicine in France. This honour was probably suggested by the fact of his having, two or three years before his death, resided in the south of France for the benefit of his health.

The enormous amount of work, both mental and physical, which Professor Bennett undertook, probably shortened his life. About 1865 his first illness appeared in the form of a throat affection. Having recovered by a sojourn in the south of France, and returned to Edinburgh, he was again prostrated in 1869. After an interval he recovered, but in the winter of 1871–2 he was obliged to return to Mentone. During the following summer, he resumed work in Edinburgh, and gave some clinical lectures. The winters of 1872–3 and of 1873–4 again forced him into a warmer climate, but each time with less benefit. In the year 1874, he resigned the Chair of the Institutes of Medicine. Last winter he spent in Nice. His last illness was owing to disease of the bladder. In August last he returned to Norwich, the place of his birth, where an operation was performed, and a stone was extracted. The debility caused by this operation, combined with previous exhaustion of constitution, brought on death.

Undoubtedly, Professor Bennett was in the medical profession a person of great eminence. He introduced many very important changes in medical practice, and made known many new principles. His devotion to study and investigation probably led to his having the character of being somewhat unsociable and austere. But those who had the privilege of intimacy with him, know that he was truthful, honest, honourable, and earnest in every relation of life.

The Rev. Dr Thomas Jackson Crawford joined our Society in

1871. He was born in 1812, and died 11th October 1875, at Genoa, at which place, when he died, he was sojourning for the benefit of his health.

His father was Professor of Moral Philosophy in the United College of St Andrews.

His son Thomas received the earlier part of his education at the Edinburgh High School. To St Andrews he went back for his more advanced studies. Intending to be a clergyman of the Scotch Church, he took his degree in 1831, and in 1834 was licensed as a preacher of the gospel by the Presbytery of St Andrews. Whilst at college he attracted the special notice of the professors by the superiority of his talents, his assiduity to learn, and the excellence of the essays which he produced. The patronage of the parish of Cults being in the gift of the Principal and Masters of the United College, he was presented to that parish.

When the Royal Commission on Church Patronage in Scotland sat, it inquired into the way in which the University of St Andrews exercised its ecclesiastical rights.

On that occasion the Rev. Dr George Cook, one of the Professors of St Andrews, explained to the Commissioners the circumstances attending Mr Crawford's presentation; adding, that though his own son was then desirous of obtaining it, and though there was a party in Cults parish wishing his appointment, he did not hesitate to prefer young Crawford to his own son.

Whilst minister of Cults, he wrote a Statistical Account of the parish, which, besides other information, contains several interesting anecdotes regarding the youthful career of Sir David Wilkie, the painter, whose father had been minister at Cults.

From Cults, Mr Crawford was translated to Glamis, and six years later he was promoted to Edinburgh, to be minister of St Andrew's Church, jointly with the late Rev. Dr Thomas Clark.

About this time he received from his *alma mater* University, the degree of D.D. He also, shortly thereafter, was made Convener of the General Assembly's Committee on Psalmody, an appointment for which he was well fitted, on account of his knowledge of and fondness for music.

Having preached a sermon in 1847 on Jewish Missions, which was afterwards published, that circumstance led to his being selected to take the oversight of the General Assembly's Scheme for the Conversion of the Jews.

In 1853 he entered the arena of controversy by publishing first a pamphlet, entitled "Presbyterianism Defended against the Exclusive Claims of Prelacy," and thereafter another pamphlet, entitled "Presbyterianism or Prelacy; which is the more conformable to the pattern of the Apostolic Churches." His views on these subjects were reiterated by Dr Crawford in the Address which he delivered from the Chair of the General Assembly, as Moderator, in the year 1867. This public advocacy of Presbyterianism, to the prejudice of Prelacy, drew forth some letters from Bishop Wordsworth, which were published in the "Scotsman" newspaper.

"The Fatherhood of God" was Dr Crawford's first important contribution to purely doctrinal subjects. Dr Candlish, some of whose views were controverted, replied to this publication.

At this time Dr Crawford was Professor of Divinity in Edinburgh University, having succeeded the Rev. Principal Lee in the year 1859.

He published also a volume on the "Atonement," in the year 1871.

In the year 1874 he was appointed the Baird Lecturer. His lectures, first delivered in Glasgow, were afterwards, by special request, re-delivered in Edinburgh, and were published in a volume under the title of "Mysteries of Christianity."

The immense amount of study which these lectures entailed, I have heard, weakened Dr Crawford's health, and prepared his constitution for the illness to which he ultimately succumbed.

In the winter following the publication of these lectures, he was obliged to reside in the milder climate of the south of England. He suffered from great delicacy of lungs. But he returned to Edinburgh last spring, whilst the sharp east winds were still prevailing, and moreover betook himself again to College work, against the advice of his medical friends.

During the summer of 1875 he went to Germany, sojourned a while in Switzerland, and then went to Italy. There he so far recovered his strength, that he could walk considerable distances, and even up steep hills, without suffering inconvenience. But the weather in the north of Italy is often dangerous to persons with weak lungs, especially when the wind is from the north. After a short illness of ten days, caused by inflammation of the lungs, he died.

Dr Crawford, besides being a man of great eminence, and most highly respected in his own profession, was a person of varied attainments. Besides having a knowledge of music, he often took his part at amateur vocal concerts, with others—some of whom are probably now present among us to-night—and who, I am sure will bear me out when I say, and I say it from a long personal acquaintance with him, that Dr Crawford was a person of most amiable disposition, and most conciliatory in all the relations of life. Though he entered into controversy he ever avoided personal aspersions; and those with whom he fought, were always ready to admit the fairness with which he wielded his weapons.

I learn from Dr Crawford's son, what I had not been aware of, that Dr Crawford kept up to the last, his knowledge of mathematics; and that frequently, when he was in want of recreation, nothing pleased him more than taking a problem and working it out.

Sir William Jardine, Bart. of Applegarth, in the county of Dumfries was born in February 1800, and died 21st November 1874. He had been fifty years a member of this Society.

He was the son of the sixth baronet, by a daughter of Thomas Maule, the representative of the Earls of Panmure.

Born in Edinburgh, he was educated partly at home, partly at York. With a view to the medical profession, he attended the medical classes in Edinburgh. But he did not carry out these professional views. Having succeeded his father when he was scarcely twenty-one years of age, he took up his residence at his family dwelling-place, Jardine Hall. By this time he had evinced a strong taste for scientific pursuits, especially natural history in all its branches.

He was a good botanist, a good geologist, and a good ornithologist. He was also a keen sportsman, both with the gun and the rod. Very many specimens in the large and valuable museum which he formed at Jardine Hall, were collected by himself.

In the year 1825 he commenced, in conjunction with the late Mr Selby of Twizell, in Northumberland, the publication of the

"Illustrations of Ornithology." In 1833 he undertook a still more important work, "The Naturalists' Library," forty volumes of which appeared in the course of the next ten years—a work for which he obtained contributions from the best scientific naturalists in the kingdom;—but of this work, no less than fourteen volumes are made up of contributions by Sir William exclusively. He also published a new edition of Alexander Wilson's "American Ornithology;" started and carried on for some time a magazine of zoology and botany; and was also for some years a joint editor of the "Edinburgh Philosophical Journal."

Here is a list of other works which flowed from his pen:—New edition of "White's Selborne," "British Salmonidæ," "Ichthyology of Annandale."

A still more important work by Sir William Jardine was entitled "Contributions to Ornithology," in three volumes, extending from the year 1848 to 1852. This work contains descriptions and coloured figures of many species of birds previously unknown. Another publication was "Memoirs of the late Hugh Edwin Strickland," in the year 1858. Mr Strickland had married a daughter of Sir William. He was a good geologist. He unfortunately was killed in a railway tunnel, the rocks of which he was examining when a train came on him unexpectedly.

Jardine's frequent visits to Northumberland, to co-operate with his friend Mr Selby of Twizell, brought him into acquaintance with Dr Johnston of Berwick-on-Tweed, who was well versed in botany and marine zoology. Dr Johnston having about this time founded the Berwickshire Naturalists' Club, Sir William Jardine joined it in September 1832, and in that year contributed papers of some value on the "Parr" and the "Silver White," small fish of the salmon species, which frequented the Tweed and many other rivers. At that time, the true nature of these fish was not known, though it has since been well ascertained that the parr are the young of the true salmon in their first year's growth.

Sir William Jardine was President of the Berwickshire Naturalists' Club in the year 1836, and frequently attended its meetings in subsequent years.

In the year 1860 he was one of the Royal Commissioners appointed to investigate the Salmon Fisheries of England and

Wales. The evidence collected by these Commissioners is of great value. Legislation followed on their report.

Shortly before his death, Sir William occupied himself in preparing a catalogue of the various objects of interest in his museum. This catalogue was in proof, and awaiting revisal when he died. The list of birds contains no less than 6000 species, and probably not less than 12,000 specimens. Sir William was most obliging in lending specimens to friends. I remember on one occasion obtaining from him on loan the skull of a fossil bear found in this country, on the occasion of a popular lecture which I was giving in Berwickshire.

Sir William Jardine was during the last ten years of his life constantly resident at Jardine Hall, enjoying the sports of country life, discharging the duties of a proprietor, and taking his share of county and parochial business.

William Macdonald was born in the year 1798, and died on 1st January 1875. He was the oldest member of our Society, in the class of Ordinary Fellows, having joined the Society in the year 1820. There is, however, one older member, my venerable friend Sir Richard Griffiths, who is an Extraordinary Fellow of the Society. He was ninety-one years of age last September, and is still in excellent health, residing near Kelso. I believe that Sir Richard would have been here to-night, had the weather been less stormy.

Dr Macdonald at an early age inherited a good estate in Argyllshire. He applied himself to the execution of extensive works in that county, for the improvement of his property, and of the district where it was situated. Unfortunately he involved himself in financial difficulties, and was obliged to sell his estate.

He then studied medicine, passed with honours, but never practised.

In 1820 he joined a number of Societies. He was the oldest member not only of our Society, but also of the Royal College of Physicians and of the Linnean Society.

Dr Macdonald frequently read papers to us on various subjects. He held peculiar views on some points of anatomy, which were entirely at variance with those generally held; but he never would concede that he was in error.

He was very partial to natural history, and wrote upon "The Structure of Fishes," "The Unity of Organisation, as exhibited in the Skeleton of Animals," and "On the Vertebral Homologies, as applicable to Zoology."

In the year 1849 he accepted an appointment to that somewhat anomalous professorship of "Civil and Natural History" in St Andrews, but I am not sure whether he ever had any students.

He had formed a large and interesting collection of specimens in natural history and anatomy.

Principal Shairp informs me that, a few years before his death, Dr Macdonald made over this collection to the University Museum.

Donald Mackenzie became a Fellow of this Society in 1870. He was born 19th June 1818, and died 17th May 1875, at Norwood, near London, where he had gone on account of ill health.

Though born in Edinburgh, his father was from Sutherlandshire, and a Captain in the Royal North British Fusiliers. His mother was Robina Jamieson, one of the seventeen children of John Jamieson, D.D., who wrote the well-known Dictionary of the Scottish language.

Donald was the eldest of seven children, all of whom he survived, though he, too, died at the comparatively early age of fifty-seven.

At first he studied for medicine, and received the degree of M.D. from the University of Edinburgh in 1838. He was also a Fellow of the Royal College of Surgeons.

But he abandoned that profession, and came to the Scottish bar, influenced, it is believed, by the expectation that as his uncle, Robert Jamieson, advocate, had a large amount of practice in the Courts, he would be able to give him a lift. Robert Jamieson I remember well in the Parliament House, being the most conspicuous figure there for height and breadth, and a lawyer of great acuteness. His sister, Donald's mother, lived to the age of eighty-four.

Donald, to whom this notice refers, did not inherit the Jamieson constitution. He was narrow-chested and slim, but walked with elastic step.

Having come to the bar in 1842, he soon got into considerable practice, and was popular among his brethren in the Parliament House. He was appointed Advocate-Depute in 1854, an office which he held till 1858, when he lost it on a change of Government. He was reinstated in 1858, and was appointed Sheriff of Fife in 1861. In the discharge of this office, he is said to have given great satisfaction, both to the practitioners in the Sheriff-Court and to the resident gentry.

Mr Mackenzie was raised to the Bench in 1870, and was not only most conscientious in his attention to the judicial duties, but was successful in pronouncing judgments which were seldom reversed. It is related that on two occasions, when they were reversed in the Inner House, they were, by an appeal to the House of Lords, adhered to.

Lord Mackenzie was exceedingly fond of all country sports. A serious illness was contracted, about two years before his death, in consequence of his continuing to fish in wet clothes, till he got a severe chill. In November 1874 he became so ill, that he was obliged to ask leave of absence for the winter. During the subsequent Christmas holidays, he attempted to return to his work in the Bill Chamber, but he was obliged to give it up, and confine himself to bed. Disease of the heart, aggravated by rheumatism, had set in. He continued more or less an invalid for a whole year before his death, seldom discharging any judicial work.

Lord Mackenzie was universally respected for his close attention to duty, his sound knowledge and judgment as a lawyer, his freedom from guile, and his conciliatory disposition toward all with whom he was brought in contact. His life was shortened by a determination to perform any duty incumbent on him, though probably conscious that he was thereby weakening his constitution.

John Sinclair was born 20th August 1797, and died 22d May 1875. He was the third son of the Right Hon. Sir John Sinclair, Bart. of Ulbster, in the county of Caithness, author of that valuable repertory, the Statistical Account of Scotland. His mother was Diana, daughter of Alexander, the first Lord Macdonald.

His education commenced in Edinburgh University; but he went afterwards to Pembroke College, Oxford.

In the last book which he published, entitled "Old Times and Distant Places," he mentions that, when at Edinburgh University, he was the chief means of forming what was called the "Rhetorical Society," among the members of which were the present Earl of Wemyss, the late Adam Anderson (afterwards Lord Anderson), and David Robertson, who, whilst on his death-bed, was created Lord Marjoribanks.

When he went to Oxford, he proposed a similar society; but "the Dons" (he says) "frowned upon him, and prevented it." The project was renewed some years after. The "Oxford Union Club" was then formed, embracing among its members the present Archbishop of Canterbury, Mr Gladstone, Mr Lowe, and others who afterwards became men of distinction.

Having gone through the necessary forms for taking orders in the Episcopal Church, he was ordained by the Bishop of Lincoln in 1820. He was shortly thereafter appointed to St Paul's Episcopal Chapel, Carrubbers Close, where he remained till he became assistant to the Rev. Mr Alison, the officiating clergyman of the then new and handsome chapel of St Paul's, in York Place.

It was in the year 1820 that Mr Sinclair joined our Society. I see from his little book, that he took a considerable interest in our proceedings, as he mentions our Dinner Club, of which he was a member, and specifies several duties which he undertook as a member of Council.

Thus he was selected by the Council to endeavour to induce Dr Williams, rector of the English Academy, to shorten the length of a paper he was to read on Greek particles, a subject on which he had read several long papers before, much to the *ennui* of the majority of members. Dr Williams, it seems, was not a person who could be easily diverted from his purpose; Mr Sinclair undertook to try his hand upon the inflexible Welshman. He explains, in an amusing way, how he succeeded.

Another more important work with which Mr Sinclair was entrusted by our Council, was the arrangement of the unpublished MSS. of Hume, the historian. These MSS. had been left as a legacy to the Society by the late Baron Hume, the historian's nephew. In this duty he was conjoined with the late Lord Meadowbank and Dr Abercrombie; but the chief part of the work fell on Mr Sinclair.

He mentions that it was in the year 1828 that he became acquainted with Dr Thomas Chalmers, when the latter resigned his professorship of Moral Philosophy at St Andrews, to become Professor of Divinity in Edinburgh University. Having a great admiration for the doctor's character and writings, he attended his first course of lectures, and describes the intense interest with which he and the other students listened to the professor's expositions. The salary of the professors being then very small—only £200—the idea of offering a testimonial to Dr Chalmers, at the end of his first course, occurred to Mr Sinclair. Accordingly a sum of £200 was raised from the voluntary students, and presented to the new professor.

In the year 1839 Mr Sinclair went to London, apparently to consult Mr Wardrop, the celebrated occulist, about his eyes. He had to submit to a painful operation and to severe discipline, which confined him to a room in London for some weeks.

Whilst he was there, a vacancy occurred in the office of Secretary to the National Society—a great Society, established, among other things, for the encouragement and support of schools connected with the Church of England. Mr Sinclair was asked to fill the vacant office. At first he refused, as it would oblige him to leave Edinburgh altogether, and he could not be certain of being so well received in London as he had been in his own country. But finding that the two London Archbishops and other persons of influence were anxious that he should accept, he consented. He was at the same time appointed to be examining chaplain to the Bishop of London.

Immediately after entering on this new office, he found himself involved in a great public controversy, which called for the utmost exertion, with great tact on his part. The controversy had reference to the schools of the National Society receiving aid from Government. After the administration of the Education Grant was transferred from the Lords of the Treasury to the Privy Council Committee, a system of inspection, to see that the schools were properly conducted as regards teaching, was resolved on. The

Church of England did not object to inspectors; but inasmuch as the religious instruction in the schools was to be reported on by Government inspectors, the Church desired to have some security as to the qualifications of the inspectors to judge of that instruction.

The National Society, at the suggestion of Mr Sinclair, resolved to intimate to Government that they would recommend the managers of all Church of England schools to refuse the Government grants, unless some arrangement satisfactory to the Church was made on that point. The Government having at first refused to make any concession, notice was sent to the Privy Council from the managers of about 200 schools, that they would not in future receive the grant. Mr Sinclair, in support of the National Society's views, appealed to the Universities of Oxford and Cambridge, and secured their help. He preached on the subject; he induced several of the leading London newspapers to advocate the views of the Church; he obtained the assistance of Lord Ashley and other influential public men. At length the Privy Council Committee yielded,—agreeing that no inspector should be allowed to examine any Church of England School whose name had not first been submitted to the Archbishops of Canterbury and York for their sanction. This privilege was extended also to the schools in Scotland connected with the Church.

The grants to the Church of England Schools were paid to the Treasurer of the National Society, and were by him distributed to the schools. When the above arrangement had been completed, the office of treasurer was held by a Mr Watson, who was so averse to the proceedings of the Privy Council, that to avoid touching the unclean thing, he refused to receive the Government grant, or grant a receipt for it, and sent in his resignation. Mr Sinclair on this occasion was appointed to be treasurer, so that he was installed into the two most important and laborious offices of the Society.

In the year 1843 Mr Sinclair, in addition to these duties, was called on to undertake important pastoral work. In that year he was appointed Vicar of Kensington, and in the following year Archdeacon of Middlesex.

The population of that new part of London had immensely outgrown the means of public worship, so he set himself to work on behalf of Church Extension. He remained Vicar and Archdeacon for the last thirty years of his life. When he came into the district there were three parishes; before the close of his career, he had been the means of forming in it twenty-three parishes.

Whenever Mr Sinclair found it necessary to carry any important measure in later years, he seems to have acted on a hint given to him by the late Dr Chalmers, on the last occasion, as he says, that he saw this great and good man. This was in the year 1843. He had been telling the Doctor of what he was doing for the support and extension of the Church of England National Schools, and in particular, how he had received promises of support from hundreds of influential people, including members of the Cabinet and of both Houses of Parliament. Dr Chalmers, he says, "heard me patiently for some time, and then replied, 'Mr Sinclair, I perceive you are an enthusiast; your National Society must, under God, depend upon the *nation* for support, and not on Cabinets or Parliaments.'"

After this conversation, very little is said by Mr Sinclair in his autobiography about applications by him to influential individuals; whilst a good deal is said about the public meetings which he resorted to when he wanted to raise money, or to influence public opinion. He never spoke from the platform himself; for after leaving the University, he lost the fluency of speech, which he says, he had acquired there; but he had great tact in arranging meetings and providing speakers who were likely to be listened to.

Several amusing stories of this kind are told in his little book. One may be mentioned. Mr Thackeray had recently come to reside in Kensington, and Mr Sinclair thought his name would be a powerful attraction. Mr Sinclair called upon him. Thackeray was unwell, and in his bedroom. Mr Sinclair having sent up his card, Thackeray came down stairs, when Mr Sinclair explained his object. Thackeray at once declined, saying he had never in his life made a speech in public, and that he only wrote for the public; and besides he was too ill to leave the house. Mr Sinclair said that he would not insist on a speech, but that it was very difficult to get up a meeting in Kensington, and that if Mr Thackeray would only allow his name to be printed in the handbills, he would not insist on his saying much, and would have the speaking done by others. Mr Thackeray was amused, and said, "Well, if I am alive, I will come to your meeting." The handbills were accordingly issued with Thackeray's name in them. A great crowd assembled. Mr Thackeray appeared on the platform. He found when there he could not avoid saying something. His words were few but telling, and they were received with enthusiasm. Mr Sinclair adds, that this was the only time that the rhetorical powers of the great novelist were proved at a public meeting.

It was not merely in London that Mr Sinclair was of use. During general periods of great distress in the manufacturing and mining districts of Wales and Lancashire, the bishops of these dioceses obtained his services to enable them to raise funds and devise measures of relief; his services in these respects being thought of, on account of his well-known business habits, and also his sympathy with the working classes.

In the year 1853 he was sent out to the United States as one of a deputation from the Church of England to the General Convention of the Protestant Episcopal Church in New York. When there, he made acquaintance with Mr Washington, the nephew of the great man who had founded the American Republic, and with whom his father, Sir John Sinclair, had corresponded.

I have had sent to me a long list of pamphlets, books, and sermons, published by the Archdeacon. The largest work is one in two volumes, published in 1837, on the Life and Times of his father.

From what I have said, it will be perceived that Mr Sinclair, by the energy with which he threw himself into every work he undertook, justified Dr Chalmers' opinion, that he was an enthusiast. But his enthusiasm was—which is rarely the case—tempered with great good sense and sound judgment. His untiring industry, his practical usefulness, and his benevolence of character, showed that he was no unworthy son of a most excellent and patriotic Scotchman.

Having concluded all that it has occurred to me to state regarding ourselves—I mean regarding the work we are doing, and our means of doing it—I proceed to submit to you a few remarks regarding the present state of science generally in our own country.

It appears to me that a great educational movement, amounting almost to a revolution, is at present taking place in our land, and especially in that branch of public education which relates to science. I will not say that old institutions are being subverted; but undoubtedly new institutions are rising up very different from the old, and the old are undergoing considerable changes. I believe that the seed from which all these changes have sprung, and are springing, was planted by one man—the late Prince Consort. I know of no other person of weight and influence who so constantly took every opportunity of urging on the people of this country the introduction into our universities and schools of scientific instruction.

This opinion is shared by others more entitled than me to speak on this subject; to whom I will now shortly refer.

Three weeks ago, at Oxford, His Royal Highness Prince Leopold agreed to perform the duty of distributing prizes to students of the School of Science and Art established in that town. The prince was introduced on that occasion by the Duke of Marlborough, lord-lieutenant of the county, and who, some years ago, was President of the Government Department on Education. His Grace, on introducing the prince, said that "it was not surprising that His Royal Highness should take a warm interest in every thing that belonged to Science and Art, when they remembered that he trod in the steps of the illustrious prince to whom the development of Science and Art in this country was mainly if not wholly attributable."

Prince Leopold responded to this sentiment. "I do not forget," said His Royal Highness, "that there is devolved upon me, as well as upon other members of my family, a sacred trust, to foster, in such manner as we are able, the general study throughout the kingdom of Science and Art. From the passage I am about to read," he continued to say, "you will perceive that only a few years ago, and even in our university, Science and Art studies received little, if any, support. I will quote from an address by my revered father on the occasion of his laying the first stone of the Birmingham and Midland Institute, almost exactly twenty-four years ago." The passage quoted by Prince Leopold, was a remarkable one. Its first sentence was as follows:—"The study of the laws by which the Almighty governs the universe is our bounden duty." Prince

Albert, in the passage so read by his son on this occasion, went on to show that, besides being our duty as human beings, it was for our interest as citizens to attend on these studies. "I advise you," said the prince, "to follow, in undivided attention, the sciences of mechanics, physics, and chemistry, and the fine arts of painting, sculpture, and architecture. You will thus confer upon your country an inestimable boon, and in a short time have the satisfaction of witnessing the beneficial results upon our national powers of production. Other parts of the country will emulate your example, and I live in hope that all these institutions will some day find a central point of union, and thus complete the national organisation."

Weighty as these words of Prince Albert were, and coming from an authority so much respected, I am not sure that they would have been universally listened to, had it not been for the great international exhibition of works of industry held in London in the year 1851—itself a measure due to the sagacity of that excellent prince. There the people of this country first saw, with their own eyes, what were the fruits of the superior schools for scientific instruction existing in Germany, Austria, Switzerland, and France.

Shortly afterwards, royal commissions were issued to ascertain to what extent any of the sciences specified by the Prince Consort were taught in our schools.

The result of these inquiries was sufficiently remarkable.

In the year 1864 the Public Schools' Commission, after special inquiry, reported that from all the first class schools in England, the teaching of science was practically excluded.

This official exposure had some effect; for in the year 1868 another Government commission, the Endowed Schools' Commission, reported, that a majority of the endowed schools in England had intimated their willingness and their intention to introduce science teaching.

To how very small an extent this promise was fulfilled, may be judged of by the revelations of the Oxford and Cambridge school examinations made throughout England during the last three or four years. Even in this very year of 1875 what has been ascertained? Out of 461 candidates for certificates of good scholarship from 40 first-class English schools, there were only 28 scholars in chemistry, 21 scholars in mechanics, 15 in physical geography, and 6 in botany; whilst for Greek there were 433 scholars, for Latin 438, and for elementary mathematics 458.

The small amount of scientific instruction given in the English endowed schools was ascertained, still more precisely, by the Royal Commission, which has only recently framed its report. I mean the commission over which the Duke of Devonshire presided. From that report it appears that a circular was sent out by this commission two years ago to about 250 endowed schools, requesting them to fill up a schedule showing what amount of scientific instruction, if any, was given in them.

Only 128 answers were received. That fact alone was significant. But when these answers came to be examined, it was found that out of the 128 answers, only 87 gave any definite information. Of these 87, 65 confessed to giving no science teaching whatever; of the remaining 22, the utmost time allotted to any kind of scientific instruction, was four hours per week, in eighteen of the schools.

But though it was right to ascertain the truth in this matter through the authentic inquiries of royal commissions, the people of this country, knowing well enough, from their own experience and observation how the matter stood, would not wait for these official inquiries. They took the matter into their own hands, and set to work at once to supply what was required. I do not know any stronger proofs of public patriotism in our country, than what this educational movement affords. In all the great centres of industry, arrangements were made for having institutions established in which not only science in its various branches should be taught, but the arts and literature also.

At Manchester, John Owens bequeathed about L.100,000 for the endowment of a new college. No part of that money, however, being allowed to be expended on buildings, his fellow-citizens supplied what was needed. A sum of L.250,000 was raised; and in the year 1870 the foundation stone of a magnificent edifice was laid for a Science and Art College, the Duke of Devonshire presiding.

In the year 1871, a physical science college was established in Newcastle, for which L.35,000 was raised; and as it was to be affiliated with Durham University, that university agreed to give L.1000 a year out of its revenue for the institution.

In the year 1873, Josiah Mason, who had made a large fortune as a manufacturer at Birmingham and Kidderminster, gave the princely sum of L.250,000 for the erection and endowment of a College of Practical Science in Birmingham.

In January 1874, an association was formed for the promotion of scientific industry in Lancaster, at which the Earl of Derby presided—an association formed chiefly at the instance of Lancaster manufacturers and artizans, who, having visited the Vienna International Exhibition held in the autumn of 1873, had seen there the rapid and alarming progress of Continental nations in many of the arts.

In the same year, the Yorkshire College of Science was begun in Leeds, of which college Lord Frederick Cavendish is president, there being L.100,000 subscribed for it.

In the course of last summer, steps were taken to establish in Bristol a College of Science, to be affiliated to Oxford University, for which L.26,000 has been already subscribed. Nottingham, Sheffield, and other towns, not so wealthy as to found colleges, are, however, stirring for the establishment of schools and societies for the teaching of classes.

In Scotland, Dundee is stirring, wishing to have a college which is to be affiliated with St Andrews University, and for which it is proposed to raise as much as L.200,000.

Nor are our old, time-honoured national universities, in the midst of this great educational movement, asleep. Asleep or indifferent they could scarcely remain, for very obvious reasons. Both at Cambridge and at Oxford, science lectures and fellowships have been at length introduced; and the Chancellor of Cambridge, the noble Duke of Devonshire, has, from his own funds, presented that university with a splendid chemical and physical laboratory, having a most complete apparatus, at a cost of L.10,000.

Our own University of Edinburgh has during the last five years had three new chairs created and endowed for engineering, geology, and political economy; and farther measures of extension, on a large scale, are being adopted, for which above L.85,000 have been already subscribed.

Even the *farmers*, who are not generally proverbial for moving out of old paths, or even for moving in them, except at a slow pace, are showing signs of progress. The Royal English Agricultural Society last year set apart L.500 to be given yearly in scholarships to encourage instruction in the sciences bearing on agriculture.

The Highland and Agricultural Society has this year set apart L.250 for a similar purpose.

But I must here offer a word of apology for the managers of the English endowed schools, which, in their programmes of studies, made no provision whatever for science. I remember when my son was at one of these schools, that I went to the head master and ventured to hint the disappointment I felt at the want of such provision. His answer was—" We are obliged to suit our teaching to university requirements. Only certain subjects are taught at Oxford and Cambridge; and we endeavour to prepare our scholars in the subjects taught in these universities."

I thought the excuse satisfactory; but now that the old universities have introduced science teaching, and now that new colleges are being established all over the land with the same view, and bursaries are given by societies, corporations, and associations, in almost every large town, these secondary endowed schools will have no longer an excuse for not giving science instruction; they will be even under a necessity to give it for their own sakes.

Here again, however, I have to observe that the country refused to wait this slow progress of school amelioration. The Government, with the entire approval of Parliament, by means of a special department at Kensington, encouraged the establishment throughout England and Scotland of schools and classes for the teaching of science and art. This encouragement was and is now given by prizes to scholars, remuneration to teachers, and loans of apparatus to the schools. The result has been marvellous. The scheme has been in operation for only nine years. At first it was little known and not well understood; but now these schools are extending rapidly; for whilst in the year 1869, that is three years after the scheme was started, there were in Great Britain only 523 schools with 24,865 scholars, there were in 1874 (since which date I have seen no reports), 1336 schools and 53,050 scholars.

It is also proper to mention that the national elementary schools which are recognised by the Education Act for England and for Scotland, are encouraged to include various sciences in their programme of lessons; there being capitation grants of money to the managers of these schools for scholars who, at the annual inspections, pass satisfactory examinations in various branches of science.

I have thus at some length explained what has been done during the last ten, and more particularly during the last five years, for increasing the means of scientific instruction in our universities, colleges, and even in elementary schools, because of the important bearing of these measures in promoting such objects as this Society aims at. When vast multitudes of our population become conversant with science, who knew nothing of science before, who can doubt that investigation will be stimulated, and that discoveries and inventions will be made with a speed hitherto unprecedented?

But there is another measure of even greater importance to science, which is about to be taken in this country. Our schools, colleges, and universities are institutions for teaching truths, and explaining facts already known. It is now proposed to establish colleges of research, as they have heen called, for aiding in the discovery of truths, facts, and principles not yet known.

In the year 1868, the British Association for the advancement of science appointed a committee of some of its most eminent members to report on the two following questions:—

"Does there exist in the United Kingdom of Great Britain and Ireland, sufficient provision for the vigorous prosecution of physical research?"

"If not, what farther provision is needed, and what measures should be taken to secure it"

In the following year that committee gave in a report, answering these questions thus:—

"The provision now existing in the United Kingdom is far from sufficient for the vigorous prosecution of physical research.

"Whilst greatly increased facilities for extending physical research are required, your committee do not consider it expedient to define how these facilities should be provided." The committee added, that "as the whole question of the relation of the State to Science is at present in a very unsatisfactory position, they urge that a Royal Commission alone is competent to deal with the subject."

That report having been approved of by the Association, an influential deputation waited on Her Majesty's Government, to suggest the appointment of a Commission; and accordingly in May 1870 such a Commission was appointed.

This Commission has been most diligent in its investigation and discussion of the several points remitted to it. They have examined several hundred witnesses, and have issued no less than eight reports.

Besides ascertaining the condition of our universities, colleges, and endowed first-class schools, as regards their teaching power, and suggesting in many cases that assistance should be given to them by the State, the Commissioners took up the other important question, to which the British Association specially had called attention—viz. this, whether the State ought not to aid researches for discovering new scientific facts and truths.

As the report of the Commissioners on this question is of great interest alike to men of science and to scientific bodies in this country, I quote a few sentences to show the opinion of these Royal Commissioners, and the advice they give to Her Majesty's Government:—

"The great advances in physical science which have been made in this country, and within this century, by such men as Dalton, Davy, and Faraday, *without aid from the State;* the existence of numerous learned societies; and the devotion of some few rich individuals to the current work of science, at first sight appear to reduce the limits within which State aid to research is required in this country.

"But whilst we have reason to be proud of the contributions of some great Englishmen to our knowledge of the laws of nature, it must be admitted that at the present day scientific investigation is carried on abroad to an extent, and with a completeness of organisation to which this country can offer no parallel. The work done in this country by private individuals, although of great value, is small when compared with that which is needed in the interests of science; and the efforts of the learned societies, not excepting the Royal Society, are directed merely to the discussion and publication of the scientific facts brought under their notice. These societies do not consider it any part of their corporate functions to undertake or conduct research. . . . . .

"But whatever may be the disposition of individuals to conduct researches at their own cost, the advancement of modern science requires investigations and observations extending over areas so large, and periods so long, that the means and lives of nations are alone commensurate with them.

"Hence the progress of scientific research must in a great measure depend upon the aid of Governments. As a nation, we ought to take our share of the current scientific work of the world. Much of this work has always been voluntarily undertaken by individuals, and it is not desirable that Government should supersede such efforts; but it is bound to assume that large portion of the national duty, which individuals do not attempt to perform, or cannot satisfactorily accomplish."

The sentences which I have now read are the preamble and the basis of the conclusions to which the Commissioners unanimously came. These conclusions are as follows:—

1. "The assistance given by the State in this country for the promotion of scientific research is *inadequate;* and it does not appear that the concession or refusal of assistance takes place upon sufficiently well-defined principles."

2. "More complete means are urgently required for scientific investigations, in connection with certain Government departments. Physical as well as other laboratories and apparatus for such investigations ought to be provided."

3. "Important classes of phenomena relating to physical meteorology, and to terrestrial and astronomical physics, require observations of such a character, that they *cannot be advantageously carried on otherwise than under the direction of the Government.*"

4. "Whilst national collections of natural history are accessible to *private investigators*, it is desirable that they should be made still more useful for purposes of research than they are at present. We would now express the opinion that corresponding *aid* ought to be afforded to persons engaged in important physical and chemical investigations; and that, whenever practicable, *such persons* should be allowed *access*, under proper limitations, *to such laboratories as may be established or aided by the State.*"

5. "It has been the practice to restrict grants of money made to private investigators for purposes of research, to the expenditure actually incurred by them. We think that such grants might be considerably increased. We are also of opinion, that the restriction to which we have referred, however desirable as a general rule, should not be maintained in all cases, but that, under certain circumstances and with proper safeguards, investigators should be *remunerated* for their time and labour."

6. "The grant of L.1000 administered by the Royal Society, has contributed greatly to the promotion of research, and the amount of this grant may with advantage be considerably increased."

"In the case of researches which involve, and are of sufficient importance to deserve, exceptional expenditure, direct grants, in addition to the annual grant made to the Royal Society, should be made in aid of the investigations."

7. "The proper allocation of funds for research; the establishment and extension of laboratories and observatories; and generally, the advancement of science, and the promotion of scientific instruction as an essential part of public education, would be most effectually dealt with, by a *Ministry of Science and Education*, and we consider *the creation of such a ministry of primary importance*."

8. "The various departments of the Government have from time to time referred scientific questions to the Council of the Royal Society for its advice. We believe that the work of a Minister of Science, even if aided by a well-organised scientific staff, and also the work of the other departments, would be materially assisted, if they were able to obtain, in all cases of exceptional importance or difficulty, the advice of a *Council* representing the scientific knowledge of the nation."

9. "This Council should represent the chief scientific bodies in the United Kingdom. With this view, its composition need not differ very greatly from that of the present Government Grant Committee of the Royal Society. It might consist of men of science selected by the Council of the Royal Society, together with representatives of other important scientific societies, and a certain number of persons nominated by the Govenment."

Such, gentlemen, are the conclusions and recommendations of these Royal Commissioners on a subject deeply interesting not only to all scientific bodies, and men of science in this country, but to the nation at large. The Commissioners are men eminently qualified by social position, by enlightened knowledge, and by a thorough investigation of the subject, to pronounce an opinion, and I feel very confident that when their report comes before Parliament, their conclusions will be accepted, the organisation recommended by them agreed to, and the necessary supplies ungrudgingly voted.

I have, before concluding, only one other point to mention. No great measure, whether political or educational, can be adopted in this country by the Government, or even by Parliament, which has not obtained previously the general assent of the community. Now it is a gratifying circumstance, that during the last few months, many distinguished men, good judges of public opinion, and who also themselves influence public opinion, have recently taken occasion to advert to the question of scientific instruction. I have already mentioned the names of His Royal Highness Prince Leopold and his Grace the Duke of Marlborough. It so happens that the same page of the "Times" newspaper, of the 12th Nov., which reports what they said, gives speeches in the same direction by Sir Alexander Cockburn, Lord Chief-Justice of England, and by Mr Gladstone, the ex-Premier. Going back a few weeks, I find speeches by the Duke of Devonshire, the Marquis of Hartington, the Earl of Derby, the Marquis of Ripon, Lord Winmanleigh, Lord Frederick Cavendish, Sir Stafford Northcote, the Right Hon. Lyon Playfair, and Mr Bell, M.P. for Hartlepool, one of our most extensive and intelligent iron-masters.

These names I mention to show that the great landowners of the country, and also many distinguished statesmen, are responding heartily to the appeal made to them by our manufacturers and merchants, who feel that their own interests, and the continued prosperity of the country in trade and commerce, require institutions which will give to their sons, and also to the working classes, a more technical education than they have hitherto received. With such combined action, who can doubt that an immense impetus will be given both to scientific teaching and to scientific research? Wonderful indeed have been the discoveries during the last half century, even with the scanty appliances which men of science have hitherto had at their command. These discoveries, the Lord Chief-Justice Cockburn said, "perfectly overwhelmed him with astonishment," and as the Royal Commissioners said, may justly invoke national pride, that so many of them should be due to the unaided efforts of individuals. What, then, may we look forward to in the next half century, with the additional appliances which these Commissioners recommend?

But, perhaps, here a word of caution, even from so humble an adviser as myself, may be allowed. Lord Chief-Justice Cockburn, on the occasion to which I have referred, says—"No one bows with a more profound and reverent worship at the shrine of science than I do. No one values more than I do classical attainments. Nevertheless, allow me to say, that I know of no study more valuable to an Englishman than the study of *English*. Nothing is more valuable than the power of English composition, English oratory, and English elocution; and greatly as I value classical knowledge, and the knowledge of foreign languages, I still say, that the English language and English composition are of the first importance to Englishmen." These remarks he followed up by announcing his wish to give a prize of twenty guineas annually for a piece of English composition.

Much to the same purpose, our distinguished colleague Mr Lyon Playfair, when assisting the other day to inaugurate the Science College at Leeds, expressed a hope that the institution would not be confined to science, but would embrace letters and the arts.

These views suggest one danger to be avoided by those who are anxious to establish colleges and schools for scientific teaching. The country, willing as it undoubtedly is to supply deficiencies in this respect, will certainly not agree that a knowledge of science shall be all that a well-educated Englishman or Scotchman ought to possess.

But there is another danger, and one more serious. Mr Gladstone, when distributing the prizes of the science and art classes at Greenwich, three weeks ago, made these impressive remarks:—"Whatever I may think of the pursuits of industry and science, and of the triumphs and glories of art, I do not mention any one of these things as the great specific for alleviating the sorrows of human life, and meeting the evils which deface the world. I believe firmly in science and art, for their own purposes. I believe in their reality, their efficacy, and their value; I believe they are efficacious and valuable for the purposes for which they are ordained, but not for purposes for which they were not ordained. If I am asked what is the remedy for the deeper sorrows of the human heart—what a man should chiefly look to in his progress through life, with which to sustain him under trials nd affliction—I must point to something very different, to something which in a well-known hymn is called 'the old, old story.' It is this 'old, old story, told in a good old book, with the teaching to be found there, which is the greatest and best gift ever given to mankind, a gift carrying with it and imposing upon all alike, the most solemn trusts and responsibility, because arousing the fullest recollections of the past and the brightest hopes of the future. I venture upon this observation for myself, lest, in speaking of the immense value which is to be attached to the subjects with which we are dealing to-night, it should be supposed I was setting them up as having some exclusive right to allegiance upon your minds and hearts, or, at any rate, a right paramount to every other."

I much fear that this warning of the ex-Premier is needed. I fear it may be said, not merely of men of science, but of others also, that they often allow their hearts and minds to be so occupied—so engrossed with pursuits and studies, as to leave no room for other things which should find a place there also.

Men of science have sometimes been charged, not merely with allowing their minds to be too much engrossed in this way, but with conceit and arrogance, engendered by the consciousness of possessing wisdom above the great bulk of their countrymen. The *true* man of science, is fairly amenable to no such charge. So far from possessing that "pride, and arrogance, and froward mouth," which is condemned in the good old book referred to by Mr Gladstone, he *is*, and at all events *should be*, the reverse of all this; for whatever amount of knowledge he acquires, whatever the discoveries he achieves, no one sees so clearly the immensity of what still remains to be discovered. Even in our own planet, how little do we yet know of the composition of the earth's interior, how little of its deep oceans, how little of ne great atmosphere which surrounds us! And even if we knew and understood all and every part of our own habitation, what is that, when we think what a tiny atom that habitation is in the great system of the universe, seen and unseen!

The true man of science, knowing all this, is humble-minded, not arrogant or supercilious; diffident, not presumptuous; forbearing, not intolerant.

If these are the qualities which men of science possess and show, whilst prosecuting their studies and researches, they will secure favour for themselves and for their noble pursuits. They will be accepted and respected as the expounders of the grand and beautiful *laws* by which God governs the universe—*laws*, a knowledge and a right application of which will assuredly conduce, alike to the prosperity of nations and to the happiness of the human race.

The following statement in regard to the number of the present Fellows of the Society was laid on the table by the Secretary:—

1. Honorary Fellows—

Royal Personage—

His Royal Highness the Prince of Wales, . . 1

British Subjects—

John Couch Adams, Esq., Cambridge; Sir George Biddell Airy, Greenwich; Thomas Andrews, M.D., Belfast (Queen's College); Thomas Carlyle, Esq., London; Arthur Cayley, Esq., Cambridge; Charles Darwin, Esq., Down, Broomley, Kent; John Anthony Froude, Esq., London; James Prescott Joule, LL.D., Cliffpoint, Higher Broughton, Manchester; William Lassell, Esq., Liverpool; Rev. Dr Humphrey Lloyd, Dublin; William Hallowes Miller, LL.D., Cambridge; Richard Owen, Esq., London; Lieut.-General Edward Sabine, R.A., London; George Gabriel Stokes, Esq., Cambridge; James Joseph Sylvester, LL.D., London; William Henry Fox Talbot, Esq., Lacock Abbey, Wiltshire; Alfred Tennyson, Esq., Freshwater, Isle of Wight, . . 17

Carry forward, . 18

Brought forward, . 18

Foreign—

Claude Bernard, Paris; Adolphe Théodore Brongniart, Paris: Robert Wilhelm Bunsen, Heidelberg; Michael Eugène Chevreul, Paris; James D. Dana, LL.D., Newhaven, Connecticut; Heinrich Wilhelm Dove; Jean Baptiste Dumas, Paris; Charles Dupin, Paris; Christian Gottfried Ehrenberg, Berlin; Elias Fries, Upsala; Herman Helmholtz, Berlin; August Kekule, Bonn; Gustav Robert Kirchhoff, Heidelberg; Herman Kolbe, Leipzig; Albert Kölliker, Wurzburg; Ernst Edward Kummer, Berlin; Johann von Lamont, Munich; Richard Lepsius, Berlin; Rudolph Leuckart, Leipzig; Urbain Jean Joseph Leverrier, Paris; Joseph Lionville, Paris; Henry Milne-Edwards, Paris; Theodore Mommsen, Berlin; John Lothrop Motley, United States; Louis Pasteur, Paris; Professor Benjamin Peirce, United States Survey; Adolphe Pictet, Geneva; Henry Victor Regnault, Paris; Angelo Secchi, Rome; Karl Theodor von Siebold, Munich; Bernard Studer, Berne; Otto Torell, Lund; Rudolph Virchow, Berlin; Wilhelm Eduard Weber, Gottingen; Friedrich Wohler, Gottingen, . . 35

Total Honorary Fellows at March 1875, —— 53

The following Foreign Honorary Fellows were elected in March 1875—

Dove, Kekule, Kolbe, Kummer, Lionville, Motley.

The following are the Honorary Fellows deceased during the year—

Foreign—M. Comte de Remusat, ... 1

British—Sir Charles Lyell, Bart., Sir W. E. Logan, Sir Charles Wheatstone, ... 3

—— 4

49

2. Non-resident Fellow under the Old Laws—

Sir Richard Griffiths, ... 1

Total Honorary and Non-resident Fellows, 6th Dec. 1875, 50

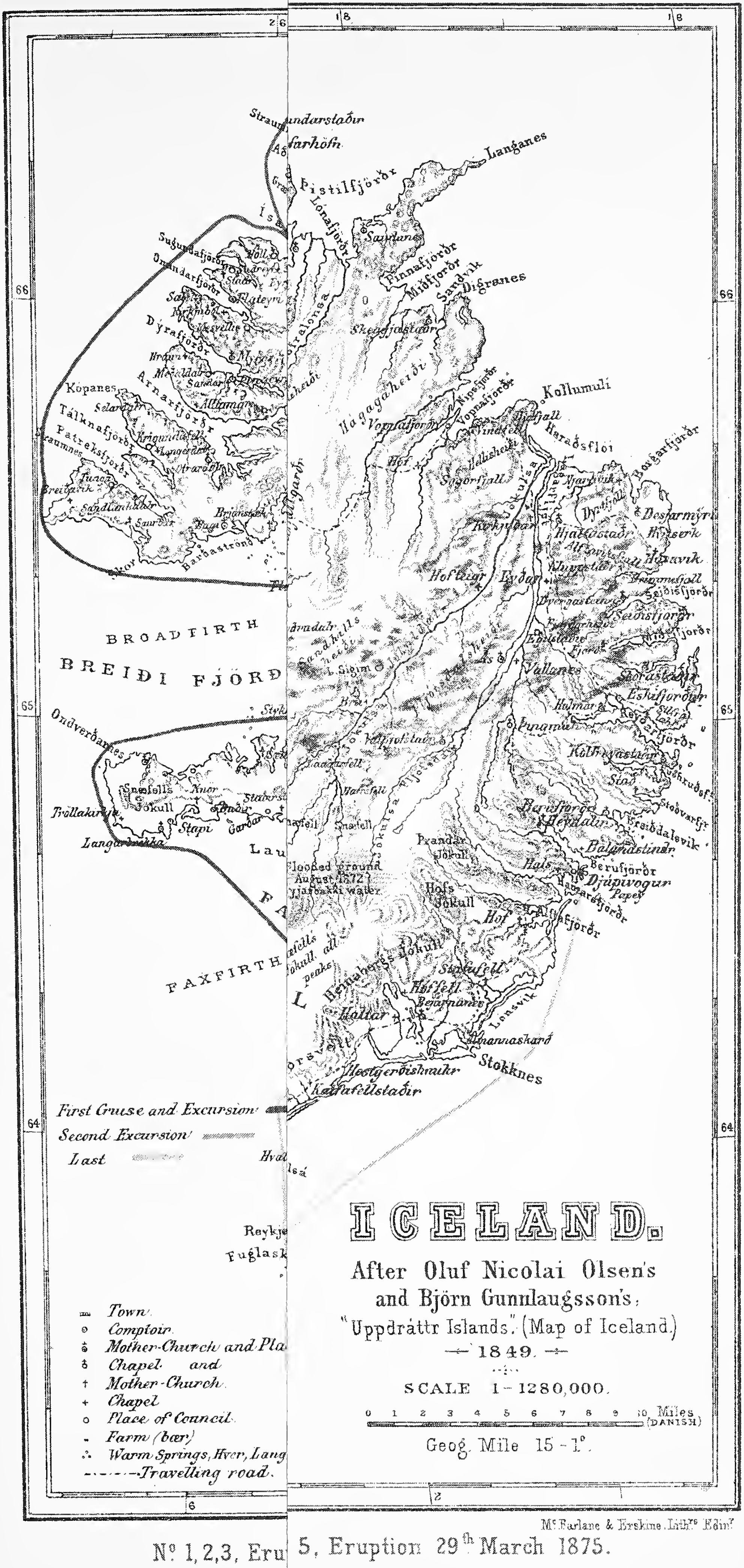
3. Ordinary Fellows—

Ordinary Fellows at November 1874, ...		345
New Fellows, 1874-75.—John Aitken, Esq.; The Hon. James Bain; Dr Ludwick Bernstein; James Bryce, LL.D.; John Christie, Esq.; Robert Clark, Esq.; Dr T. S. Clouston; Dr William Craig; Daniel G. E. Eliot, Esq.; Thomas Fairley, Esq.; Robert Gray, Esq.; Sir John Hawkshaw; William Jack, Esq.; Archibald Kirkwood, LL.D.; John Ramsay L'Amy, Esq.; C. H. Millar, Esq.; John Milroy, Esq.; E. W. Prevost, Esq.; Ralph Richardson, Esq.; Michael Scott, Esq.; James Syme, Esq.; James Thomson, LL.D.; Charles Wilson Vincent, Esq.; Professor Daniel Wilson, ...	24	
B. Baden Powell, formerly elected, but not admitted till 1874; Dr Alexander Wood (re-admitted), .	2	
Total New Fellows, . .		26
		371
Deduct Deceased.—Rev. Dr Aitken; John Auld, Esq.; Dr J. Hughes Bennet; Rev. Dr Crawford; Col. Seton Guthrie; Sir William Jardine, Bart.; Professor Macdonald; Hon. Lord Mackenzie; E. Meldrum, Esq.; Ven. Archdeacon Sinclair, ...	10	
Resigned.—Rev. Thomas M. Lindsay; John L. Douglas Stewart, Esq., ...	2	
Cancelled.—Charles Lawson, Esq., ...	1	
		13
Total number of Ordinary Fellows at November 1874, .		358
Add Honorary and Non-Resident Fellows, . . .		50
Total Ordinary and Honorary Fellows at commencement of Session 1875 (6th December), ...		408

The following Communication was read:—

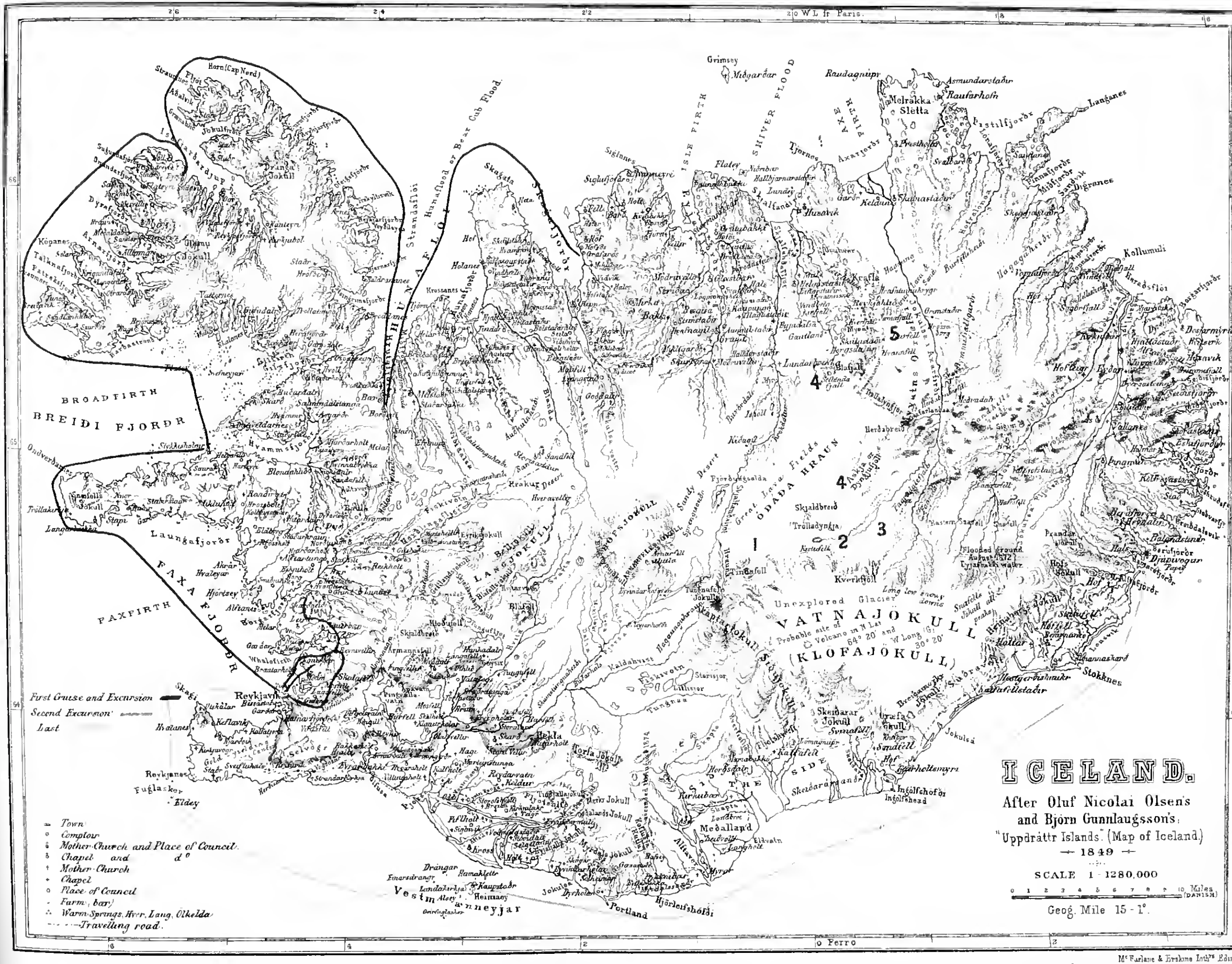
The Volcanic Eruptions of Iceland in 1874 and 1875. By Captain Burton. (With two Maps of Iceland).

Shortly after reading "Volcanic Eruptions in Iceland" (the "Scotsman," May 21), and "An Appeal for Iceland" (the "Times" July 1), I made a trip to Arctis, partly with a view of inspecting and inquiring into the last outbreaks. Perhaps your energetic Society may not be unwilling to have an unprejudiced account of what was seen and heard.



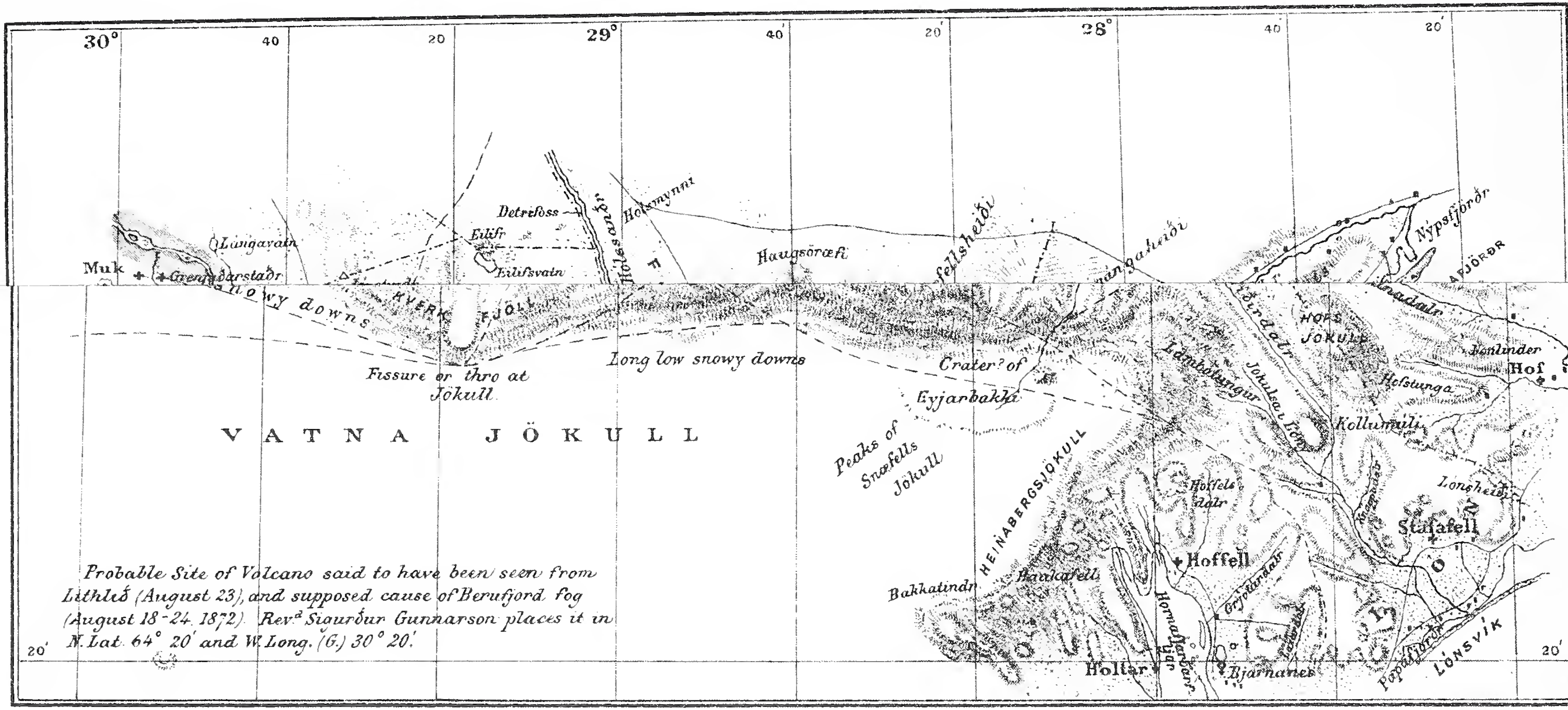
McFarlane & Erskine, Lithrs Edinr

Nº 1,2,3, Eru 5, Eruption 29th March 1875.



Nº 1, 2, 3, Eruptions with intermissions during the years 1867, 1869, 1870-72. Nº 4, Eruption about Christmas 1874. Nº 5, Eruption 29th March 1875.
The brown shading mark the Ashes thrown S. E. to East.

William P Nimmo, London & Edinburgh.



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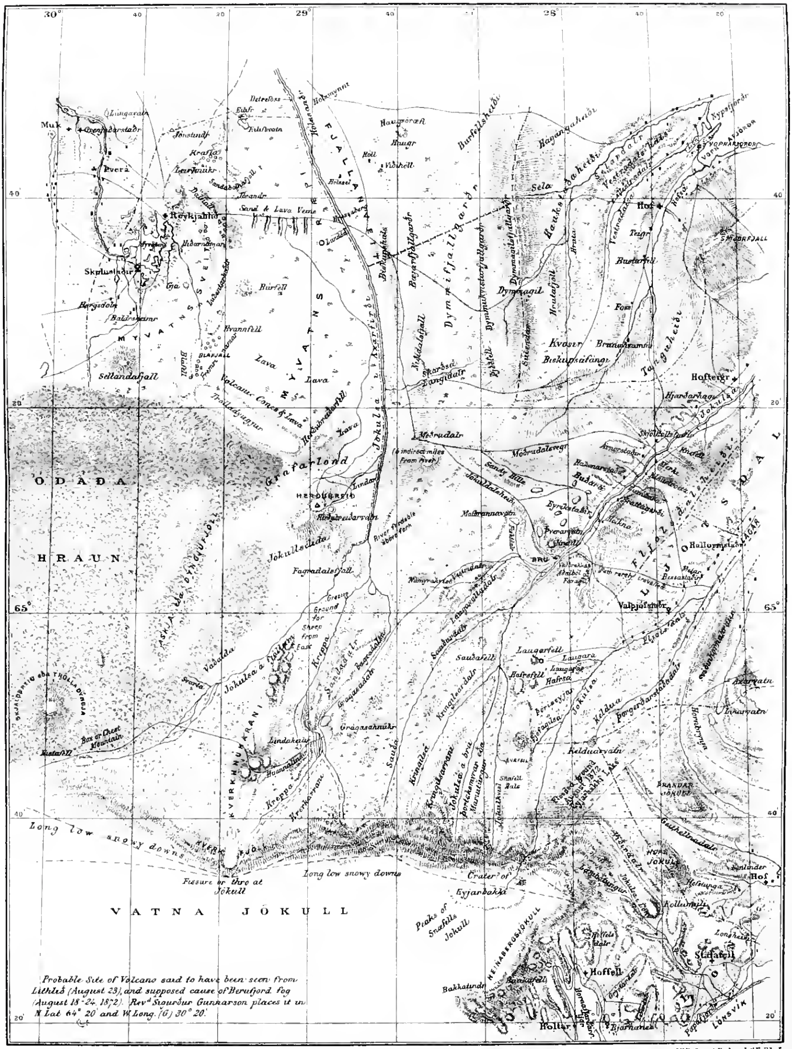
William P. Nimmo, London & Edinburgh.



William P Nimmo, London & Edinburgh

The accompanying maps, prepared for my forthcoming volume, ("Ultima Thule"), mark the four paroxysmal eruptions which took place upon the same area during 1867, 1869, 1870 (to 1872, at which last date all activity had subsided), and about Christmas 1874. Number five, the latest phenomenon, broke out on March 29, 1875.

These movements may or may not be connected with the five days' eruption of Skaptár Jökull (January 9, 1873), recorded by all the journals of Europe; but they certainly occupy the heart and the southern outskirts of the Odáða Hraun, the great lava field subtending the north of the Vatna Jökull, and extending to the N.N.E., almost as far as the Lake Region—Mý-vatn and its oasis. The name is variously translated "Terrible wilderness" (Henderson), and "Desert of the Evil Deed" (Baring Gould), whilst the area is differently calculated at 1160 to 1500 square miles; in fact, one half of the Vatna Jökull. Viewed from the nearest heights—Bláfjall, for instance—it is a grim and ghastly picture, a region of ruin and desolation, a fitting *mise en scène* for the Last Man: my companions remarked that such a spectacle would soon give them the horrors. I see no difficulty beyond a certain expense in crossing and exploring this waste: at the same time, I doubt that the feat would yield any results; and exploration purely for exploring appears to me like "climbing for climb."

This "great and terrible wilderness," so small and mean in comparison with the Sahrás of Africa, Asia, and South America, and yet so grisly in its brown-black desolation, is supposed by Baring Gould to be the gift of the Trölladýngjur and of Herðubreið, while Mr W. L. Watts would derive it solely from Skjaldbreið. I find it to be the produce of a multitude of craters which opened in and south of it, before the days when Öræfi and other lofty peaks, attracting rain and snow, built up the mighty *névé*, which monopolises the south-eastern corner of Iceland. The peculiarity of the latest outbreaks (1867–1875) is the distance, not to say isolation, of the vents from any large body of water, suggesting the unpleasant fact that we must modify received opinion. For instance, the eruption of Christmas 1874 upon the south-eastern flank of the distorted horseshoe Askja (oval-shaped wooden casket) or Dyngjufjöll (Cubilia or "Chamber Hills"), are at least thirty-eight direct geographical miles from Mý-vatn (midge-water), and forty-five from the nearest seashore. Nos. 1, 2, and 3 vents are more distant from the lake, and but little nearer the coast. The non-maritime Andine volcanoes are popularly supposed to be connected by fissures and strata-faults with the Pacific. Here, however, the foci are separated from the Atlantic eastward by the valley of the broad and deep Jökulsá í Axarfírði, the longest, if not the largest, river in Iceland. To the west they are guarded by the Skjálfandi Fljót, and south by the huge Vatnajökull, whilst palagonite is not a rock which maintains permanent fissures like porphyry. I can only suggest that the eternal snows of the mighty *névé* take the place of lake and sea water.

Our approach to Iceland was heralded by volcanic phenomena. On Friday, July 8, as my shipmates were recovering from the sufferings which began in Pentland Firth, we found the milky blue sea patched and streaked with what many supposed to be rye—the cargo of some wrecked vessel—but which proved to be pumice, the largest piece hardly equalling a bean. On the return voyage (August 9) we passed through a similar discharge, and we heard of dense and choking ash-showers. Landing (July 10) at Húsavík, the old export harbour of the great Northern Brimstone Mines, we found burned stone thrown up in tons on the beach north-east and south-west of the factory. A few of the bits were equal to a man's fist: some were slightly vitreous, and others had a fibrous texture like asbestos; they much resembled those brought from the Askja by Mr W. L. Watts. Lastly, when we approached the focus of eruption we picked up common specimens of an intermediate size, where certainly none existed in 1872. Our maximum distance then was 70 or 80 miles, and the line of our direction was from south-west and west to north-east and east. As will presently appear, a single morning (March 19) is supposed to have discharged 3840 tons in four hours. All authorities are agreed that the ashes fell in Norway within twenty-four hours—a rapid but not an unusual rate of progress. In the Hekla eruption of 1693 the scoriæ were also carried by the winds in one day to the Fœroe Islands; the same was the case with the Skaptár outbreak of 1783; and in 1845 the goodwives of Shetland, when bleaching their linen, were surprised to find it covered with pepper from Hekla. But instances of this nature need not be multiplied.

On July 17, while collecting specimens of brimstone from the great Mý-vatn mines, a select company of the expedition rode over the Námaskarð ("Fountain-scaur"), the gap in the *Montes Phlegræi*, east of Mý-vatn, and thence we took the highroad, or rather the bridle-path, leading eastwards over the Mý-vatnsörœfi (Desert of the Midge-water) to the greatest of the three Jökulsárs. After some three hours of sharp canter, which covered more than half-way, we sighted to the south of the road and north-east of Búrfell, a low black-blue mound with white patches. It was about a mile long; and a solitary puff, escaping every quarter of an hour, told us that it was burning low. Nothing could be meaner than this outbreak, which I will call the Mý-vatn eruption: it looked by the side of older formations as if Vulcan had struck work, and the underground furnace of Iceland were being "drawn." Shortly after our departure, however, Mr W. L. Watts here observed a huge Gjá, and an active eruption, which he briefly noticed in the "Times" newspaper, and which I hope he will presently describe at greater length, accompanying his description with a ground-plan and elevation.

This No. 5 is connected by a band of old lava with No. 6, a mound to the north of the road. It was first seen (Feb. 18) from the Grimstðair farm, erupting to the west of the Sveinagjá, in what is called the Austurfjöll or Mý-vatnsfjöll. The great smoky fire (jarðeld) springing from 14 or 16 mound craters (gosborg) lying on a meridian, formed, say the natives, a molten river 300 to 400 fathoms broad and one *vegarleid* (= 3 English miles) long, throwing up lava, pumice (vikur) and stones, often the size of a man, which fell down upon the crater lips. Some of the hot material melted the snow. The lava soon set, but the ground was too hot for walking, and the stone flood glowed white beneath 2 to 4 feet of the upper black stratum which had cooled in a few minutes. The plain around was split with hideous Gjár (fissures); the frequent hornitos, blisters, and hillocks on the run probably the effects of steam, were hollow, with a capacity of 2 to 4 hogsheads, and the smoke (vapour?) hung upon the horizon like a cloud. On March 10 the eruption lasted all night, and the most violent effort was on April 13. By that time the area of the foci (eldgígar) was about 40 square fathoms; and where the ground before was level, rose a lava hill 30 to 60 feet high. The greatest flow was to the north, and southwards a fire stream (eldgos), one mile long and 500 fathoms broad, was covered with high and rough blisters; and was overhung with whitey-blue fumes. The view was confined by the fine dust to 300 feet: during daylight it wore the semblance of a mirage (Landjóldu or Tíðbrá), and at night it became a pillar of fire.

Our disappointment was tempered by meeting a party of three Icelanders driving two ponies, whose imitation Icelandic coffers bespoke the English owner. The head guide introduced himself as Páll Pálsson; I recognised him as the godfather of "Mount Paul" in the heart of the Vatnajökull. Led by Mr W. L. Watts, he and five other islanders set out from the Núpstaðir farm on June 24, and after twelve nights and days (July 7) in the snow, the adventurous band issued from the great *névé* between Kistufell and Kverkfjöll, and on July 10—a fortnight and more—they reached Grímstaðir, whither their horses had been sent round *viâ* the Eastern path. This is, indeed, a unique feat of travel which I hope will not be its own reward. Paul, who was physically as well as morally the best type of an Icelander, accompanied us to our head-quarters at Reykjahlíð and gave me a detailed acconnt of the southern outbreaks. He saw south-east of the Kistufell and north of the Vatnajökull, but not in the snow, two small smokes, remnants of Nos. 1 and 2, distant some six hours' ride, and three others appeared in the Dýngjufjöll. This account was confirmed by Dr P. E. Julius Halldórsson, government physician of the Thíngeyjar Sýsla. Both agreed that No. 4 was still active, and they placed the site on the south-eastern bend of the Askja or southern Dýngjufjöll the curious horseshoe of the map which they would break up into detached hills, and would moreover open to the N.E. instead of the N.N.E. The lava on April 10 was about three (Danish) miles long by a maximum of half a mile broad. At night tne farm rooms were completely illuminated by the fire-blush (Eldroði), and when this ceased violent earthquakes came on. Showers of ashes fell north of Mý-vatn (Thíngeyjar Sýsla), and more copiously in the Jökulsdal (Múla Sýsla), covering the ground with a stratum a quarter of an ell thick.* The darkness during the dust showers prevented the Jökulsdælingar (people of Jökulsdal) reading by day, and many of them left their farms and drove their cattle to grass on the Vopnafjörðr. This outbreak is supposed to have come not solely from the Dýngjufjöll, which, since April 5, emitted only heavy smoke, but from several other places in the northern, the north-eastern, and the north-western faces of the Vatnajökull. In addition to this movement, which may be called the "Dýngjufjöll eruption," and which is frequently referred to in the local and in our home papers, Dr Julius places another vent, hitherto unnumbered, about seven miles to the S.S.W. of Herðubreið, the "broad-shouldered" and perpendicular-sided mountain of palagonite, which I had attempted to ascend in 1872. I am happy to say that Mr W. L. Watts also noted the projecting buttress from the south-west, which, descried too late, appeared to me the only place for successful climbing. Here was the outbreak of May 29, 1875, and hence, according to my informant, the greater part of the ashes and pumice had been carried to the north-east. On the other hand, Mr W. L. Watts saw no crater south-west of Herðubreið, and would derive the pumice and ashes from Askja. The Medico placed a suplementary crater in the old lava-field on a meridian between Herðubreiðarfell and the Reykjahlíð-Jökulsa road. Thus we have five several vents:—A and B, north and south of the road (March 29); C, continuing the line southwards; D (May 29), near Herðubreið; and E, the Askja or southern Dýngjufjöll

On July 29 the expedition received, at Húsavík, a visit from Mr W. L. Watts, who was fresh from the conquest of the Vatnajökull, and he gave us the first intelligent account of the movement. He had found fresh ashes, but no pumice, on the snows of the Vatnajökull, about the middle or in N. lat. 64° 25′. Kistufell was quiet; smoke or vapour issued intermittently from Kverkfjöll, which I saw in 1872 vomiting a glacier, and about Skjaldbreið rose a large mound of old lava, but no new signs of action appeared. He walked over layers of pumice, extending a score of miles, from the Svartá

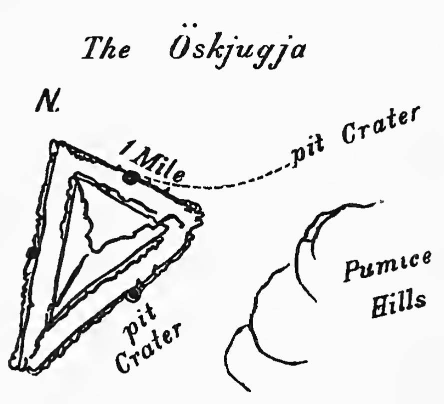
* The Danish measures are:—

12 inches = 1 foot (= 12·356 English).
24 thumblungar or 2 feet = 1 alen (ell).
24,000 feet = 1 mile (= 4½ English statute miles in round numbers).

to Herðubreið, the deepest drifts measuring some eight feet, being north-east of Vaðalda to five miles south of the "Broad-shouldered."

The explorer's chief work was about the Askja or southern Dýngjufjöll—I must warn my readers not to confound these "Chamber hills" with the "Trölladýngja (sing.), perhaps better known as Skjaldbreið, the "Broad shield. As Mr Watts intends to publish his discoveries, I must not abuse the liberality with which he gave his information. He would break up the fanciful horseshoe of the great map, which in these parts is a mere field-sketch, into a heart-shaped series of hills, mounds, and cones, here connected, there separated, by "Gils" and broad passes: on the western side the Odáða lava has penetrated into the *enceinte*, and a latitudinal bar of heights traverses the southern quarter. By walking over the eastern hills Mr Watts came in sight of the centre of eruption; the aneroid stood at 25·05 (= 5000 feet in round numbers, whilst the northern plain is about 1000 feet lower. Various angles to Herðubreið (80° to 40° for mag. var., west = 40), and the Skjaldbreið (170° to (04°=130°) placed the Gjá-site inside the horseshoe in N. lat. 64 45′ and about W. long. (f.) 17

The centre of the eruption, which we will call the Oskjugjá, was a mere fissure, an acute-angled triangle with the apex to S.S.W.; stepping the base gave a little more than a mile, and the circumference would be about five. The three arms were deep and perpendicular crevasses opened in the hills by the eruption, and the lips readily fell in. The heights around it, especially those to the east, were strewn with thick strata of pumice, ejected wet, and decomposing under atmospheric action; they also showed that surface-streams of water had lately flowed over the new matter.



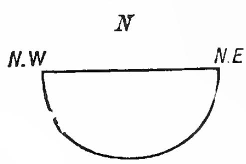
The most curious part of this Gjá is that it contains a triangle within a triangle, both similar in all their accidents; moreover, there is a series of smaller fissures extending from the centre to the apex, and to the two other angles, north-western and north-eastern. At the eastern arm, a deep pit about a quarter of a mile in circumference discharges volumes of steam and fatty fetid loam, which falls in granules. At the base is another pit-crater, opening in high and broken ground; it is also covered with pumice and offensive loam, which the depths continue to vomit. The explorer distinctly saw a pit of hot water spouted from the western side of the inner triangle: he could trace, through the volumes of steam, its shaft or column, and he heard the rain fall upon the rocks, bringing down with it an avalanche of stone. The Askja is not a Jökull, and only a few streaks of snow lay upon the flanks. This, therefore, may be considered a crucial instance of water erupted from a fire vent.

Meanwhile there are no reports of any outbreak at Herðubreið, nor in the Trölladýngur, the inadequate features from which Baring Gould would derive the Odáða Hraun. I have taken the liberty in my map of counter-marching the "Troll's Bowers," from a meridian to a diagonal, beginning south of Bláfjall and abutting almost upon Herðubreið. In local history we read dreadful accounts of Trölladýngjur's seven eruptions; of A.D. 1150 (the earliest); of 1180; of 1340, when the "Broad-shouldered" is said to have vomited for the first time; of 1359; of 1475; of 1510 (when the second outbreak of Herðubreið is reported); and, finally, of 1862, when there was an eruption of ashes, concerning which we have few and uncertain details. Thus the ratio of outbreaks from Trölladýngjur was 7 to 26 Heklas, 13 Katlas, 11 submarines, and 5 Öræfas.

The local papers, especially the "Norðanfari" of Akreyri (February 19, March 3, April 9 and 17, and May 13 and 19; and the "Isafold," of March 27), give ample accounts of the late movements. The Mý-vatn eruptions have been visited by many parties of farmers, but only one has yet reached the Öskjugjá (Askja or Dýngjufjöll Gjá). The *relations* are chiefly from the pen of Jón Sigurðsson, of Gautland near Mý-vatn, Knight of the Dannebrog, and Althingismaður (M.P.), whom some Englishmen have lately confounded with another "White John," the celebrated agitator who lives at Copenhagen. Much of the matter has been translated and published by our home press, but there are interesting details which have not been noticed. Generally—allow me to remark—the accounts, though utterly unscientific, bear an aspect of sobriety and truthfulness wholly wanting in the older Icelandic descriptions recorded by Mackenzie and Henderson, and they show that the spirit of enterprise has not wholly died out of Iceland. It must, however, be borne in mind that both features are mere crevasses opened by the tension of gases, and that due allowance must be made for "hills and hillocks," for "cliffs and precipices."

The "Isafold," a new paper published twice or thrice a month by Hra Björn Jönsson, and printed by Einar Thórðarson of Reykjavik, gives (No. 2, of March 27) a letter from Mý-vatn, which well describes the outbreak nearest the lake. It owes its chief interest to the fact that it is the only one which corroborates the testimony of Mr Watts, in mentioning torrents of hot water that cannot be melted snow. At 11 A.M., on February 16, an expedition ascended and crossed in half-an-hour the eastern flank of the volcano, which in that direction sends out a long spit. After mounting a low hill with a steep cliff to the south, the explorers reached a narrow crevasse lying on a parallel of latitude, and forming a "vinkil," apex, or angle to the south. Here they found a deep flat recess about half a (Danish) mile in diameter, surrounded by heights with perpendicular scaurs to the east and south; west and north-west the land was lower and flatter. Snow covered the whole country. Hard by to the south-east and on plain ground rose the crater which vomited the densest smoke, but there was no new lava except upon the lips. The stone-rain, and hot ground burning their shoes, prevented them approaching it nearer than 70 fathoms, but they computed the diameter at 40 to 50 fathoms, and the cone sides were so steep that the breadth above and below was about the same. The crater jetted in paroxysms. The thick smoke made the ejected matter appear like torn fragments of coat lining—evidently melted stone or burnt mud, most of which fell back into, or on the edges of the bowl. The smaller rapilli were thrown to a minimum height of 100 fathoms. No fire appeared in the crater. Some 80 to 90 fathoms to the west was a cliff probably formed by the eruption; it measured a tenfold area (8000 to 9000 square fathoms). The rocky edge, except to the north-west where it was lowest, stood some 6 fathoms high. Below and south of the cliff rose a second and a somewhat smaller crater. It jetted steadily, but not so high as the other: discharging a lava-rain to the south-west; *and a rivulet of almost transparent water flowed to the north-west, where it formed a little basin under the rocks.*

The expedition did not attempt the cliff because they had no ropes, and both rocks and snow were cracked and crevassed. A little further west rose the third crater, which vomited only smoke. Its "Vinkil" or apex was a horseshoe, with the two heels to the north and the toe facing south. It is perhaps a little higher than the level of the Mý-vatnssveit (the adjoining midge-lake country). If much more lava flow, it must be filled up, and then the fiery torrent will run over and along the cliff to the sandy waste on the left



bank of the Jökulsá. East of the new mountain and the recess was an old Hraun or lava stream, which seemed to have discharged eastwards; the bed showed no signs of craters nor volcanic vents, but the snow will prevent till next summer any examination of the Steintegundir (minerals).

Ash showers have been blown to the north-east of the Austurfjöll and, falling on the grass, which was bare of snow, they will probably injure the pastures. It is reported that stone-rain extended to Kelduhver east of the Reykjarheiði. About New Year's day, an earthquake opened great crevasses where formerly the ground was smooth. These movements were numerous near the volcano. The expedition built a snow-house under the ravine-cliff, but the falling stones compelled them to abandon it.

The "Norðanfari" of February 19, relates that during the winter of 1874–5, a strong earthquake, proceeding from the southern or outer Dýngjufjöll (hin fremri), shook the farms of Viðidal, Grímstaðir, and Möðrudal á Fjöllum, in the latter levelling some buildings. From No. 13 of March 3, we learn that four men of Mý-vatn (Mý-vatningar) set out on Feb. 15, directly southwards, and after walking 24 hours, hearing frightful subterranean thunders (dunur) like cataracts from a mountain, and smelling sulphur fumes (eldlykt), they reached the Askja, which has been incorrectly laid down on the map. The jets of stone and lava, thrown many feet high, prevented them approaching nearer than 60 to 70 fathoms. The vents consisted of one large focus and of many small parasites, a single one discharging lava. In early February smoke appeared every day, and presently came a slight earthquake. In April 9th, Jakob Hálfdánarson, the farmer of Grímstaðir on the Mý-vatn, reports his visit to the Mývatnsfjöll, where, on March 10th, the eruption lasted all night; and next day the smoke, hanging for a full eiktarlengd (three hours) upon the horizon, was dispersed by a storm. He walked northwards over the lava hill (Hraunmalarkamb), and saw the molten stone in the crevasses as if a fire had been built with wood and charcoal. East of the Kamb (coombe), he inspected two big crevasses erupting large stones, which fell back into them. The lava had flowed for two days, and small fragments lay 300 fathoms distant from the fire stream: 160 fathoms to the west smaller bits were found; but the greatest quantity was heaped up within ten fathoms of the vent (eldsupptök). An anonymous account of the same eruption, supposed to be by old Pètër Jónsson of Reykjahlíð, father-in-law to Jakob Hálfdánarson, is given in the "Norðanfari" of April 17, 1875. He reports that sundry Laxárdalers rode some six hours from Reykjahlíð to explore the new volcano, *viâ* Hvannfell, where they heard loud thunderings. A storm raging at the time in the north-west made them mistake the cause: these rumblings became fiercer as they approached the focus. The earthfire springing out of three places in a meridianal line, formed high lava hills npon the level ground. The greatest altitudes were to the north: 50 to 80 fathoms west of the northernmost, upon a tract which had sunk more than three mannahæðir (stature of man), lay a great "gil" or crevasse. About the three foci, which owned from 20 to 30 parasites, the lava had run mostly to the south-east, but now he saw it flowing from the southernmost to the S.S.W. The northern was an elongated rise, and from its crater, about 300 fathoms in length, hot lava jetted 200 to 300 feet aloft, and fell in small cold drops upon a scanty area No fire appeared during the day, only white mist (gufa), growing whiter as it rose in the air; it was so thick that it towered many fathoms high, and the direction was perpendicular, although a strong wind was blowing. In the darkness of the night conflagrations became visible. No ashes fell at Mý-vatn, though they were thickly spread by a strong north wind, and were strewed together wîth pumice over the eastern regions, especially at Jökulsdal, Fljótsdal, and Seyðisfjörð. In the first-mentioned place candles were lit for five, in the second for three, and in the third for four hours. The layer of pumice and ashes measured some 4½ inches deep in the Jökulsdal, and 1½ in the Seyðisfjörð. This was the sixth explosion since the outbreak, and about every tenth or twelfth day the violence increased. The line extended through the Odáðahraun to a little north of Reykjahlið-Grímstaðir road.

About Easter-day a thick smoke was seen at Möðrudal á Fjöllum; it rose south of Herðubreið, and many erroneously thought that it came from Möðruðalsland. Others supposed it to rise from the Dýngjufjöll, but it was certainly from Vatnajökull, or from the Tungu (Doab or Mesopotamia), formed by the westermost forks of the Jökulsá. The discharge of pumice (*Danicé* "Pimpsteen") was so abundant that for days the ferry boats could not cross the stream.

The "Norðanfari" of May 13 contains an unsigned article, bringing up the account of our Mý-vatn eruption to May 5. Loud thunderings with thick smoke were noticed on the last "Tuesday in the winter," that is, on April 13; the summer beginning with "Sumarmál," April 17. On the "first summer day" (April 22) four men took horse to visit the volcano. From Kollóttafjall they saw a fiery crevasse, made like a mountain "fjargyá," or sheep fissure where the animals take refuge during bad weather; and on the borders of the Sveinagjá, where a fine grassy plain formerly extended, they found a high hill of lava pierced with three craters lying on a meridian. These vents roared loudly, and threw up rocks, which returned to the earth after 45 seconds. The smaller rapilli rising like smoke disappeared in the air, and presently fell like snow. From the largest focus, which lay south of the road, a fiery flood ran westward: it had been reported three (Danish) miles long, but it proved to be about 1000 fathoms, with a breadth of 300 to 400. The people of Mý-vatnssveit have lost a little grass, chiefly to the north of the road, and their ponies may suffer during the winter. Some convulsion has taken place in the Dýngjufjöll, whence, for a long time, more smoke issued than during the winter. There was a great eruption close to the Odáðahraun on March 18 and 19, and the concussion of the air drove the farm peeple from their beds. On March 23 fire was reported to have proceeded from forty places lying close to the Hólsfjöll road, but it lay west, not east, of the Jökulsá.

Extracts from the letter of my friend, Sira Sigurður Gunnarsson, the priest of Hallormstað, addressed to the "Norðanfari" of April 24, appeared in the "Times" of July 1, 1875. It is dated March 29, 1875, and headed "Fall of Pumice and Ashes in Múlasýla." The author, I may remark, has more than once visited the Vatnajökull. The following interesting details may be added to the abstract:—"During the Yule of 1873, and in early 1874, an earthquake shook the eastern regions, after which the people of the Fjöll country saw two tall pillars of thick smoke apparently proceeding from the Askja or Dýngjufjöll; and viewed from Hallormstaðarháls they rose at a considerable distance from each other. Early in the year there was no fire in the Mý-vatnsörœfi, and the earthquake became less violent towards the end of the winter."

After noticing the thunderings and the ash and pumice rain of March 29 (Easter Monday) reported in the "Times," my reverend friend continues:—"The movement appears to have taken place in the southern part of the Dýngjufjöll, westward of Herðubreið, and a short way north of the winter Gjá. The direction of the ashes was on both sides of a line to Mödrudal and Fossvellir, as far as the Unáos in Hjaltarstaðarthingá and the Vatnsdalsfjall. Another shower, travelling from west to east, and extending four (Danish) miles, fell at Brú, and a mile and a half east of Aðalbol (Rrafnkelsdal), Kleif (Fljótsdal), Skriðdal, and as far as Fáskruðsfjörð to the south-east. The amount which fell east of that line in Breiðdal and Stöðvarfjörð was trifling. If we draw one straight line from the focus of the eruption eastward between Fáskrúðs and Stöðvarfjörð, and a second from Vatnsdalsfjall near Njarðvik, also to the east, the area upon which the ashes and pumice rained would hardly be less than 100 square miles. Also assuming the average depth of the layer at 3 inches, we must assign to the discharge of March 19 a weight of 3840 tons."

"It is reported that the ash showers have ruined twenty farms in the Jökulsdal (between the Lagarfljót and the eastern Jökulsá) and in the northern Múla Sýsla, where the owners are preparing to abandon their property. The position of the Fljótsdalshèrad, where the scoriaceous rains fell thickest, are the Jökulsdal, Fell, Fljóstdal, Skogar, Skriðdal, Vellir, and Eyðathinghá. Heavy and terrible showers also desolated Norðfjörð, Reyðarfjörð, Mývafjörð, and

Loðmundarfjörð. Where the land has abundance of water, as in parts of Skriðdal, Vellir, and Eyðathinghá, the farmers hope that the ashes will disappear during the spring, and that they will be dissolved by the rains." This interesting letter concludes with an exhortation "not to abandon the holdings for good," and with excellent advice about the measures to be taken. Yet it owns that "from this fearful visitation all husbandry in the east country must come to utter ruin," and the less Icelanders are advised not to emigrate the better for the island.

The writer of "An Appeal for Iceland" ("Times," July 1, 1875), compares this mild and harmless eruption, which has not destroyed a single life, with the terrible convulsions of 1783, which killed some 14,000 human beings. He also calculates the destruction of pastures to the extent of 2500 to 3000 square miles, while popular computations make 4000 square miles the habitable area of Iceland.

According to Páll Pálsson only four farms on the west of the Jökulsá have suffered severely. These are, going from south to north, Brú, Eyrikstaðir, Hákonarstaðir, and Arnórstaðir. Herra Thórður Gudjónsson, factor at Húsavík, never even heard of the eruption till I showed him the nepawspers. Finally, the brown shadings in my chart, marking the eastern and north-eastern limits of the ash showers, and copied from an Iceland map obligingly lent to me by my friend, Mr Robert Mackay Smith, may be allowed to prove that the damage extends over a small area.

Mr Jón A. Hjaltalín, of the Advocate's Library, Edinburgh, received (June 26) trustworthy accounts of the ash and pumice rain. "It extended over several parts of Norður Múlasýsla and Suður Múlasýsla, depositing a layer about 1½ inches thick. In some places the winds have carried it off, but sundry parishes will be unable to keep their live stock at home this summer. Next hot season, however, it is expected that the pastures will be all right."

Mr W. L. Watt, who has just ridden over the ground, found the pumice and ashes beginning about the middle course of the Svartá (N. lat. 64° 50′), and extending northwards to Herðubreið (65° 10′), or a total depth of 20 to 25 miles, bounded eastward by the Jökulsâ, where the country is not, and never has been, habited by man.

The limits of this paper do not permit me to enter into all the details of the last eruption in Iceland; but the reader may be assured that the outline and the main features of the subject are correctly drawn.

The following Gentlemen were elected Fellows of the Society:—

Bruce Allan Bremner, M.D.
Rev. Francis Edward Belcombe.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. IX. 1875–76. No. 94.

NINETY-THIRD SESSION.

Monday, 20th December 1875.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. Vortex Statics. By Sir William Thomson.

(*Abstract.*)

The subject of this paper is *steady motion* of vortices.

1. Extended definition of "steady motion." The motion of any system of solid or fluid or solid and fluid matter is said to be steady when its configuration remains equal and similar, and the velocities of homologous particles equal, however the configuration may move in space, and however distant individual material particles may at one time be from the points homologous to their positions at another time.

2. Examples of steady and not steady motion:—

(1.) A rigid body symmetrical round an axis, set to rotate round any axis through its centre of gravity, and left free, performs steady motion. Not so a body having three unequal principal moments of inertia.

(2.) A rigid body of any shape, in an infinite homogeneous liquid, rotating uniformly round any, always the same, fixed line, and moving uniformly parallel to this line, is a case of steady motion.

(3.) A perforated rigid body in an infinite liquid moving in the manner of example (2.), and having cyclic irrotational motion of the liquid through its perforations, is a case of steady motion. To this case belongs the irrotational motion of liquid in the neighbourhood of any rotationally moving portion of fluid of the same shape as the solid, provided the distribution of the rotational motion is such that the shape of the portion endowed with it remains unchanged. The object of the present paper is to investigate general conditions for the fulfilment of this proviso; and to investigate, farther, the conditions of stability of distribution of vortex motion satisfying the condition of steadiness.

3. *General synthetical condition for steadiness of vortex motion.*—The change of the fluid's molecular rotation at any point fixed in space must be the same as if for the rotationally moving portion of the fluid were substituted a solid, with the amount and direction of axis of the fluid's actual molecular rotation inscribed or marked at every point of it, and the whole solid, carrying these inscriptions with it, were compelled to move in some manner answering to the description of example (2). If at any instant the distribution of molecular rotation* through the fluid, and corresponding distribution of fluid velocity, are such as to fulfil this condition, it will be fulfilled through all time.

4. *General analytical condition for steadiness of vortex motion.*—If, with (§ 24, below) vorticity and "impulse," given, the kinetic energy is a maximum or a minimum, it is obvious that the motion is not only steady, but stable. If, with same conditions, the energy is a maximum-minimum, the motion is clearly steady, but it may be either unstable or stable.

5. The simple circular Helmholtz ring is a case of stable steady motion, with energy maximum-minimum for given vorticity and given impulse. A circular vortex ring, with an inner irrotational annular core, surrounded by a rotationally moving annular shell (or endless tube), with irrotational circulation outside all, is a case of motion which is steady, if the outer and inner contours of the

* One of the Helmholtz's now well-known fundamental theorems shows that, *from the molecular rotation at every point of an infinite fluid* the velocity at every point is determinate, being expressed synthetically by the same formulæ as those for finding the "magnetic resultant force" of a pure electro-magnet. —*Thomson's Reprint of Papers on Electrostatics and Magnetism.*

section of the rotational shell are properly shaped, but certainly unstable if the shell be too thin. In this case also the energy is maximum-minimum for given vorticity and given impulse.

6. In these examples of steady motion, the "resultant impulse" (V. M.* § 8) is a simple impulsive force, without couple; the corresponding rigid body of example 3 is a circular toroid, and its motion is purely translational and parallel to the axis of the toroid.

5. We have also exceedingly interesting cases of steady motion in which the impulse is such that, if applied to a rigid body, it would be reducible, according to Poinsot's method, to an impulsive force in a determinate line, *and a couple with this line for axis.* To this category belong certain distributions of vorticity giving longitudinal vibrations, with thickenings and thinnings of the core travelling as waves in one direction or the other round a vortex ring, which will be investigated in a future communication to the Royal Society. In all such cases, the corresponding rigid body of § 2 example (2) has both rotational and translational motion.

7. To find illustrations, suppose, first, the vorticity (defined below, § 24) and the force resultant of the impulse to be (according to the conditions explained below, § 29) such that the cross section is small in comparison with the aperture. Take a ring of flexible wire (a piece of very stout lead wire with its ends soldered together answers well), bend it into an oval form, and then give it a right-handed twist round the long axis of the oval, so that the curve comes to be not in one plane (fig. 1). A properly-shaped twisted ellipse of this kind [a shape perfectly determinate when the vorticity, the force resultant of the impulse, and the rotational moment of the impulse (V. M. § 6), are all given] is the figure of the core in what we may call the first† steady mode of single and simple toroidal

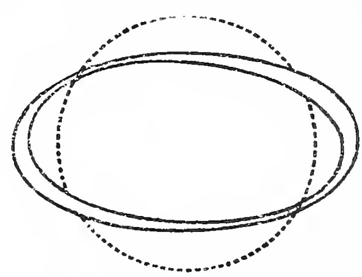


Fig. 1.

* My first series of papers on vortex motion in the "Transactions of the Royal Society of Edinburgh," will be thus referred to henceforth.

† First or gravest, and second, and third, and higher modes of steady motion to be regarded as analogous to the first, second, third, and higher fundamental modes of an elastic vibrator, or of a stretched cord, or of steady undulatory motion in an endless uniform canal, or in an endless chain of mutually repulsive links.

vortex motion with rotational moment. To illustrate the second steady mode, commence with a circular ring of flexible wire, and pull it out at three points, 120° from one another, so as to make it into as it were an equilateral triangle with rounded corners. Give now a right-handed twist, round the radius to each corner, to the plane of the curve at and near the corner; and, keeping the character of the twist thus given to the wire, bend it into a certain determinate shape proper for the data of the vortex motion. This is the shape of the vortex core in the second steady mode of single and simple toroidal vortex motion with rotational moment. The third is to be similarly arrived at, by twisting the corners of a square having rounded corners; the fourth, by twisting the corners of a regular pentagon having rounded corners; the fifth, by twisting the corners of a hexagon, and so on.

In each of the annexed diagrams of toroidal helixes a circle is introduced to guide the judgment as to the relief above and depression below the plane of the diagram which the curve represented in each case must be imagined to have. The circle may be imagined in each case to be the circular axis of a toroidal core on which the helix may be supposed to be wound.

To avoid circumlocution, I have said, "give a right-handed twist" in each case. The result in each case, as in fig. 1, illustrates a vortex motion for which the corresponding rigid body describes left-handed helixes, by all its particles, round the central axis of the motion. If now, instead of right-handed twists to the plane of the oval, or the corners of the triangle, square, pentagon, &c., we give left-handed twists, as in figs. 2, 3, 4, the result in each case will be a vortex motion for which the corresponding rigid body describes right-handed helixes. It depends, of course, on the relation between the directions of the force resultant and couple resultant of the impulse, with no ambiguity in any case, whether the twists in the forms, and in the lines of motion of the corresponding rigid body, will be right-handed or left-handed.

8. In each of these modes of motion the energy is a maximum-minimum for given force resultant and given couple resultant of impulse. The modes successively described above are successive solutions of the maximum-minimum problem of § 4; a determinate problem with the multiple solutions indicated above, but no other

solution, when the vorticity is given in a single simple ring of the liquid.

9. The problem of steady motion, for the case of a vortex line with infinitely thin core, bears a close analogy to the following purely geometrical problem :—

Find the curve whose length shall be a minimum with given

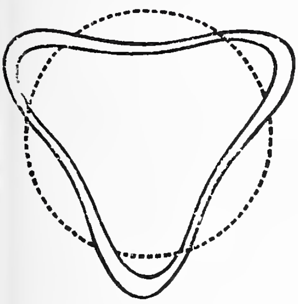


Fig. 2.

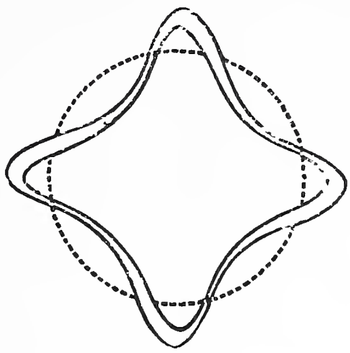


Fig. 3.

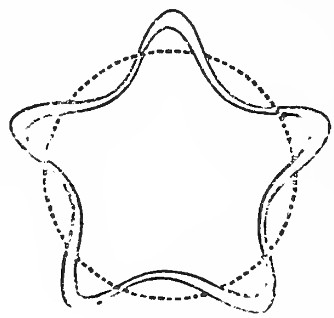


Fig. 4.

resultant projectional area, and given resultant areal moment (§ 27 below). This would be identical with the vortex problem if the energy of an infinitely thin vortex ring of given volume and given cyclic constant were a function simply of its apertural circumference. The geometrical problem clearly has multiple solutions answering precisely to the solutions of the vortex problem.

10. The very high modes of solution are clearly very nearly identical for the two problems (infinitely high modes identical), and are found thus :—

Take the solution derived in the manner explained above, from a regular polygon of N sides, when N is a very great number. It is obvious that either problem must lead to a form of curve like that of a long regular spiral spring of the ordinary kind bent round till its two ends meet, and then having its ends properly cut and joined so as to give a continuous endless helix with axis a circle (instead of the ordinary straight line-axis), and N turns of the spiral round its circular axis. This curve I call a toroidal helix, because it lies on a toroid * just as the common regular helix lies

* I call a circular toroid a simple ring generated by the revolution of any singly-circumferential closed plane curve round any axis in its plane not cutting it. A "tore," following French usage, is a ring generated by the revolution of a circle round any line in its plane not cutting it. Any simple

on a circular cylinder. Let a be the radius of the circle thus formed by the axis of the closed helix; let r denote the radius of the cross section of the ideal toroid on the surface of which the helix lies, supposed small in comparison with a; and let θ denote the inclination of the helix to the normal section of the toroid. We have

$$\tan\,\theta = \frac{2\pi a}{\mathrm{N}.2\pi r} = \frac{a}{\mathrm{N}r};$$

because $\frac{2\pi a}{\mathrm{N}}$ is as it were the step of the screw, and $2\pi r$ is the circumference of the cylindrical core on which any short part of it may be approximately supposed to be wound.

Let κ be the cyclic constant, I the given force resultant of the impulse, and μ the given rotational moment. We have (§ 28) approximately

$$\mathrm{I} = \kappa\pi a^2,\ \mu = \kappa\mathrm{N}\pi r^2 a\,.$$

Hence

$$a = \sqrt{\frac{\mathrm{I}}{\kappa\pi}},\ r = \sqrt{\frac{\mu}{\mathrm{N}\kappa^{\frac{1}{2}}\pi^{\frac{1}{2}}\mathrm{I}^{\frac{1}{2}}}},$$

$$\tan\,\theta = \sqrt{\frac{\mathrm{I}^{\frac{3}{2}}}{\mathrm{N}\mu\kappa^{\frac{1}{2}}\pi^{\frac{1}{2}}}}\,.$$

11. Suppose, now, instead of a single thread wound spirally round a toroidal core, we have two separate threads forming as it were a "two-threaded screw," and let each thread make a whole

ring, or any solid with a single hole through it, may be called a toroid; but to deserve this appellation it had better be not very unlike a tore.

The endless closed axis of a toroid is a line through its substance passing somewhat approximately through the centres of gravity of all its cross sections. An apertural circumference of a toroid is any closed line in its surface once round its aperture. An apertural section of a toroid is any section by a plane or curved surface which would cut the toroid into two separate toroids. It must cut the surface of the toroid in just two simple closed curves, one of them completely surrounding the other on the sectional surface: of course, it is the space between these curves which is the actual section of the toroidal substance, and the area of the inner one of the two is a section of the aperture.

A section by any surface cutting every apertural circumference, each once and only once, is called a cross section of the toroid. It consists essentially of a simple closed curve.

number of turns round the toroidal core. The two threads, each endless, will be two helically tortuous rings linked together, and will constitute the core of what will now be a double vortex ring. The formulæ just now obtained for a single thread would be applicable to each thread, if κ denoted the cyclic constant for the circuit round the two threads, or twice the cyclic constant for either, and N the number of turns of either alone round the toroidal core. But it is more convenient to take N for the number of turns of both threads (so that the number of turns of one thread alone is $\frac{1}{2}$N), and κ the cyclic constant for either thread alone, and thus for very high steady modes of the double vortex ring

$$I = 2\kappa\pi a^2,\ \mu = \kappa N\pi r^2 a\,,$$

$$\tan\ \theta = \sqrt{\frac{(\frac{1}{2}I)^{\frac{3}{2}}}{N\mu\kappa^{\frac{1}{2}}\pi^{\frac{1}{2}}}}\,.$$

Lower and lower steady modes will correspond to smaller and smaller values of N, but in this case, as in the case of the single vortex core, the form will be a curve of some ultratranscendent character, except for very great values of N, or for values of θ infinitely nearly equal to a right angle (this latter limitation leading to the case of infinitely small transverse vibrations).

12. The gravest steady mode of the double vortex ring corresponds to N = 2. This with the single vortex core gives the case of the twisted ellipse (§ 7). With the double core it gives a system which is most easily understood by taking two plane circular rings of stiff metal linked together. First, place them as nearly coincident as their being linked together permits (fig. 5). Then separate them a little, and incline their planes a little, as shown in the diagram. Then bend each into an unknown shape determined by the strict solution of the transcendental problem of analysis to which the hydro-kinetic investigation leads for this case.

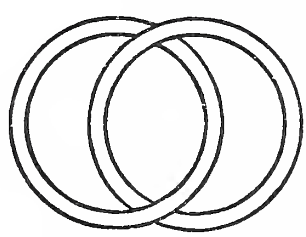


Fig. 5.

13. Go back now to the supposition of § 11, and alter it to this:—

Let each thread make one turn and a half, or any odd number of half turns, round the toroidal core: thus each thread will have an end coincident with an end of the other. Let these coincident ends be united. Thus there will be but one endless thread making an odd number N of turns round the toroidal core. The cases of N = 3 and N = 9 are represented in the annexed diagrams (fig. 9).*

Imagine now a three-threaded toroidal helix, and let N denote the whole number of turns round the toroidal core, we have

$$I = 3\kappa\pi a^2, \quad \mu = \kappa N \pi r^2 a,$$

$$\tan\theta = \sqrt{\frac{(\frac{1}{3}I)^{\frac{3}{2}}}{N\mu\kappa^{\frac{1}{2}}\pi^{\frac{1}{2}}}}.$$

Suppose now N to be divisible by 3: then the three threads form three separate endless rings linked together. The case of N = 3 is illustrated by the annexed diagram (fig. 6), which is repeated from the diagram of V. M. § 58. If N be not divisible by 3, the three threads run together into one, as illustrated for the case of N = 14 in the annexed diagram (fig. 7).

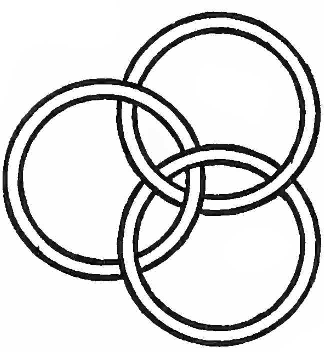


Fig. 6.

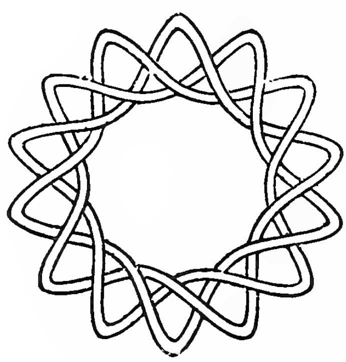


Fig. 7.

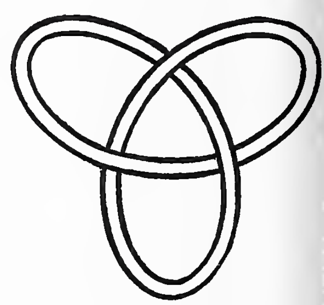


Fig. 8. "Trefoil Knot

14. The irrotational motion of the liquid round the rotational cores in all these cases is such that the fluid velocity at any point is equal to, and in the same direction as, the resultant magnetic force at the corresponding point in the neighbourhood of a closed gal-

* The first of these was given in § 58 of my paper on vortex motion. It has since become known far and wide by being seen on the back of the "Unseen Universe."

vanic circuit, or galvanic circuits, of the same shape as the core or cores. The setting forth of this analogy to people familiar, as modern naturalists are, with the distribution of magnetic force in the neighbourhood of an electric circuit, does much to promote a clear understanding of the still somewhat strange fluid motions with which we are at present occupied.

15. To understand the motion of the liquid in the rotational core itself, take a piece of Indian-rubber gas-pipe stiffened internally with wire in the usual manner, and with it construct any of the forms with which we have been occupied, for instance the symmetrical trefoil knot (fig. 8, § 13), uniting the two ends of the tube carefully by tying them firmly by an inch or two of straight cylindrical plug, then turn the tube round and round, round its sinuous axis. The rotational motion of the fluid vortex core is thus represented. But it must be remembered, that the outer form of the core has a motion perpendicular to the plane of the diagram, and a rotation round an axis through the centre of the diagram, and perpendicular to the plane in each of the cases represented by the preceding diagrams. The whole motion of the fluid, rotational and irrotational, is so related in its different parts to one another, and to the translational and rotational motion of the shape of the core, as to be everywhere slipless.

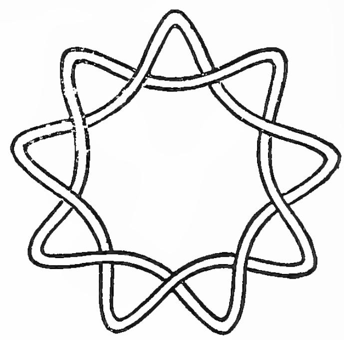


Fig. 9. "Nine-leaved Knot."

16. Look to the preceding diagrams, and, thinking of what they represent, it is easy to see that there must be a determinate particular shape for each of them which will give steady motion, and I think we may confidently judge that the motion is stable in each, provided only the core is sufficiently thin. It is more easy to judge of the cases in which there are multiple sinuosities by a synthetic view of them (§ 3) than by consideration of the maximum-minimum problem of § 8.

17. It seems probable that the two- or three- or multiple-threaded toroidal helix motions cannot be stable, or even steady, unless I, μ, and N are such as to make the shortest distances between different positions of the core or cores considerable in

comparison with the core's diameter. Consider, for example, the simplest case (§ 12, fig. 5) of two simple rings linked together.

18 Go back now to the simple circular Helmholtz ring. It is clear that there must be a shape of absolute maximum energy for given vorticity and given impulse, if we introduce the restriction that the figure is to be a figure of revolution, that is to say, symmetrical round a straight axis. If the given vorticity be given in this determinate shape the motion will be steady, and there is no other figure of revolution for which it would be steady (it being understood that the impulse has a single force resultant without couple). If the given impulse, divided by the cyclic constant, be very great in comparison with the two-thirds power of the volume of liquid in which the vorticity is given, the figure of steadiness is an exceedingly thin circular ring of large aperture and of approximately circular cross section. This is the case to which chiefly attention is directed by Helmholtz. If, on the other hand, the impulse divided by the cyclic constant be very small compared with the two-thirds power of the volume, the figure becomes like a long oval, bored through along its axis of revolution and with the ends of the bore rounded off (or trumpeted) symmetrically, so as to give a figure something like the handle of a child's skipping-rope, but symmetrical on the two sides of the plane through its middle perpendicular to its length. It is certain that, however small the impulse, with given vorticity the figure of steadiness thus indicated is possible, however long in the direction of the axis and small in diameter perpendicular to the axis and in aperture it may be. I cannot, however, say at present that it is certain that this possible steady motion is stable, for there are figures not of revolution, deviating infinitely little from it, in which, with the same vorticity, there is the same impulse and the same energy, and consideration of the general character of the motion is not reassuring on the point of stability when rigorous demonstration is wanting.

19. Hitherto I have not indeed succeeded in rigorously demonstrating the stability of the Helmholtz ring in any case. With given vorticity, imagine the ring to be thicker in one place than in another. Imagine the given vorticity, instead of being distributed in a symmetrical circular ring, to be distributed in a ring still,

with a circular axis, but thinner in one part than in the rest. It is clear that with the same vorticity, and the same impulse, the energy with such a distribution is greater that when the ring is symmetrical. But, now let the figure of the cross section of the ring, instead of being approximately circular, be made considerably oval. This will diminish the energy with the same vorticity and the same impulse. Thus, from the figure of steadiness we may pass continuously to others with same vorticity, same impulse, and same energy. Thus, we see that the figure of steadiness is, as stated above, a figure of maximum-minimnm, and not of absolute maximum, nor of absolute minimum energy. Hence, from the maximum-minimum problem we cannot derive proof of stability.

20. The known phenomena of steam rings and smoke rings show us enough of, as it were, the natural history of the subject to convince us beforehand that the steady configuration, with ordinary proportions of diameters of core to diameter of aperture, is stable, and considerations connected with what is rigorously demonstrable in repect to stability of vortex columns (to be given in a later communication to the Royal Society) may lead to a rigorous demonstration of stability for a simple Helmholtz ring if of thin enough core in proportion to diameter of aperture. But at present neither natural history nor mathematics gives us perfect assurance of stability when the cross section is considerable in proportion to the area of aperture.

21. I conclude with a brief statement of general propositions, definitions, and principles used in the preceding abstract, of which some appeared in my series of papers on vortex motion communicated to the Royal Society of Edinburgh in 1867–68 and 69, and published in the Transactions for 1869. The rest will form part of the subject of a continuation of that paper, which I hope to communicate to the Royal Society before the end of the present session.

Any portion of a liquid having vortex motion is called *vortex core*, or, for brevity, simply " core." Any finite portion of liquid which is all vortex core, and has contiguous with it over its whole boundary irrotationally moving liquid, is called *a vortex*. A vortex thus defined is essentially a ring of matter. That it must be so was first discovered and published by Helmholtz. Sometimes the word *vortex* is extended to include irrotationally moving liquid circulating round or moving in the neighbourhood of vortex core; but as different portions of liquid may successively come into the neighbourhood of the core, and pass away again, while the core always remains essentially of the same substance, it is more proper to limit the substantive term *a vortex* as in the definition I have given.

22. *Definition I.*—The circulation of a vortex is the circulation [V.M. § 60 (*a*)] in any endless circuit once round its core. Whatever varied configurations a vortex may take, whether on account of its own unsteadiness (§ 1 above), or on account of disturbances by other vortices, or by solids immersed in the liquid, or by the solid boundary of the liquid (if the liquid is not infinite), its "circulation" remains unchanged [V. M. § 59, Prop. (1)]. The circulation of a vortex is sometimes called its *cyclic constant.*

Definition II.—An axial line through a fluid moving rotationally, is a line (straight or curved) whose direction at every point coincides with the axis of molecular rotation through that point [V. M. § 59 (2)].

Every axial line in a vortex is essentially a closed curve, being of course wholly without a vortex.

23. *Definition III.*—A closed section of a vortex is any section of its core cutting normally the axial line through every point of it. Divide any closed section of a vortex into smaller areas; the axial lines through the borders of these areas form what are called vortex tubes. I shall call (after Helmholtz) a vortex filament any portion of a vortex bounded by a vortex tube (not necessarily infinitesimal). Of course, a complete vortex may be called therefore a vortex filament; but it is generally convenient to apply this term only to a part of a vortex as just now defined. The boundary of a complete vortex satisfies the definition of a vortex tube.

A complete vortex tube is essentially endless. In a vortex filament infinitely small in all diameters of cross sections "rotation" varies [V. M. § 60 (*e*)] from point to point of the length of the filament, and from time to time inversely as the area of the cross section. The product of the area of the cross section into the rotation is equal to the circulation or cyclic constant of the filament.

24. Vorticity will be used to designate in a general way the distribution of molecular rotation in the matter of a vortex. Thus, if we imagine a vortex divided into a number of infinitely thin vortex filaments, the vorticity will be completely given when the volume of each filament and its circulation, or cyclic constant, are given; but the shapes and positions of the filaments must also be given in order that, not only the vorticity, but its distribution, can be regarded as given.

25. The vortex density at any point of a vortex is the circulation of an infinitesimal filament through this point divided by the volume of the complete filament. The vortex density remains always unchanged for the same portion of fluid. By definition it is the same all along any one vortex filament.

26. Divide a vortex into infinitesimal filaments inversely as their densities so that their circulations are equal; and let the circulation of each be $\frac{1}{n}$ of unity. Take the projection of all the filaments on one plane. $\frac{1}{n}$ of the sum of the areas of these projections is (V. M. §§ 6, 62) equal to the component impulse of the vortex perpendicular to that plane. Take the projections of the filaments on three planes at right angles to one another, and find the centre of gravity of the areas of these three sets of projections. Find, according to Poinsot's method, the resultant axis, force, and couple of the three forces equal respectively to $\frac{1}{n}$ of the sums of the areas, and acting in lines through the three centres of gravity perpendicular to the three planes. This will be the resultant axis; the force resultant of the impulse, and the couple resultant of the vortex.

The last of these, that is to say, the couple is also called the rotational moment of the vortex (V. M. § 6).

27. *Definition IV.*—The moment of a plane area round any axis is the product of the area multiplied into the distance from that axis of the perpendicular to its plane through its centre of gravity.

Definition V.—The area of the projection of a closed curve on the plane for which the area of projection is a maximum will be called the area of the curve, or simply the area of the curve. The area of the projection on any plane perpendicular to the plane of the resultant area is of course zero.

Definition VI.—The resultant axis of a closed curve is a line through the centre of gravity, and perpendicular to the plane of its resultant area. The resultant areal moment of a closed curve is the moment round the resultant axis of the areas of its projections on two planes at right angles to one another, and parallel to this axis. It is understood, of course, that the areas of the projections on these two planes are not evanescent generally, except for the case of a plane curve, and that their zero values are generally the sums of equal positive and negative portions. Thus their moments are not in general zero.

Thus, according to these definitions, the resultant impulse of a vortex filament of infinitely small cross section and of unit circulation is equal to the resultant area of its curve. The resultant axis of a vortex is the same as the resultant axis of the curve, and the rotational moment is equal to the resultant areal moment of the curve.

28. Consider for a moment a vortex filament in an infinite liquid with no disturbing influence of other vortices, or of solids, immersed in the liquid. We now see from the constancy of the impulse (proved generally in V. M. § 19) that the resultant area, and the resultant areal moment of the curve formed by the filament, remain constant, however its curve may become contorted; and its resultant axis remains the same line in space. Hence, whatever motions and contortions the vortex filament may experience, if it has any motion of translation through space this motion must be on the average along the resultant axis.

29. Consider now the actual vortex made up of an infinite number of infinitely small vortex filaments. If these be of volumes inversely proportional to their vortex densities (§ 25), so that their circulations are equal, we now see from the constancy of the impulse that the sum of the resultant areas of all the vortex filaments remains constant; and so does the sum of their rotational moments: and the resultant areal axis of them all regarded as one system is a fixed line in space. Hence, as in the case of a vortex filament, the translation, if any, through space is on the average along its resultant axis. All this, of course, is on the supposition that there is no other vortex, and no solid immersed in the liquid, and no bounding surface of the liquid near enough to produce any sensible influence on the given vortex.

2. Experiments illustrating Rigidity produced by Centrifugal Force. By John Aitken, Esq.

If an endless chain is hung over a pulley and the pulley driven at a great velocity, it is well known that the motion so communicated to the chain has almost no tendency to change the form of the curve in which the chain hangs, and that the principal effect of the motion is to confer on the chain a quasi-rigidity which enables it to resist any force tending to alter its curvature.

This is only true in a general sense, and possibly may be true of some ideal form of chain; but in all chains we can experiment on there are forces in action in the moving chain which tend to cause the chain to depart from the form which it has while at rest.

I shall refer to these disturbing forces later on. As the disturbing forces in most chains are very small, we shall neglect them, and for the present suppose the centrifugal force just balances the tension at all points. The following experiments were made to illustrate the balance of these forces, to show that into whatever curves we may bend the chain when in motion, the centrifugal force has no tendency to alter these curves: that all forms are forms of stability, as far as the centrifugal force is concerned.

The first experiments were to show the effect of destroying the balance between the tension and the centrifugal force. In these experiments the links on the descending side of the loop were allowed to fall on a platform, so that part of the chain lay loose on the platform, thus destroyed the tension produced by the centrifugal force at the lower part of the chain. The chain was made to take the same velocity as the driving pulley, by being pressed into contact with it by means of an elastic wheel.

I. When the chain was pressed at the point where it leaves the pulley there was no alteration in the path of the chain, because the chain after it leaves the pulley is moving in a straight line, and as there is no deviating force, there is no centrifugal force, and therefore, removing the tension in the chain has no effect on the direction of the motion of the links.

II. When the chain was pressed at a point a little higher up the pulley, then the centrifugal force of the curved part of the chain resting on the pulley at the descending side, being unbalanced by the tension, rises from the pulley, and is shot in a direction away to one side of the pulley. Of course the curved part of the chain on the other side of the pulley has also a tendency to rise, but is kept in its place by the tension produced by putting the chain in motion after being stopped by the platform.

III. When the chain is pressed on the ascending side of the pulley, then the chain rises up off the pulley and forms itself into a somewhat irregular curve resting on the platform, and touching the pulley at only one point. When the velocity is sufficient to raise it to a certain height, the conditions become altered. The chain in rising takes up all the slack chain lying on the platform, and a tension is produced in the chain by the centrifugal force, and unless we keep increasing the speed of the chain, it can no longer keep in its elevated position, because the centrifugal force is now balanced by the tension, and as the force of gravitation is now unbalanced, it gradually flattens the curve till the chain again comes to rest on the top of the pulley and spreads itself out in an irregular curve on the platform.

IV. At the beginning of the previous experiment the centrifugal force being unbalanced by the tension, it overcomes the force of gravitation and causes the chain to rise into the air. After all the slack chain has been taken up, and a tension is produced in the chain by the centrifugal force, then the centrifugal force is balanced by the tension and is no longer capable of opposing gravitation, and the chain begins to fall; but at this point its fall may be stopped, or the chain may be made to rise again by destroying the tension at the lower part of the chain. If we cause the chain, instead of meeting the platform at an acute angle, to strike it as near as possible at right angles, then the motion of the chain where it strikes the platform is partly destroyed, and the chain again rises and may be kept balanced for a long time resting on the platform, and only touching the driving wheel at one point. The reason for this being, that if we partially stop the motion of the links by causing them to strike the platform, or if we alter the direction of their motion by causing them to strike the platform, then there will be less tension in the lower part of the chain than in the upper, as the tension in the lower part will be only that due to partially changing the direction of the motion of the links. The centrifugal force of the upper part of the chain will be therefore unbalanced, and will cause the chain to rise and keep its elevated position against the force of gravitation. If a quick upward motion is given to the platform, the chain may be thrown up in the air, and again dropped on the platform like a solid body.

The next experiments are to show that centrifugal force may produce sufficient rigidity to cause a chain to run along a platform like a wheel. A short endless chain was put over a pulley which was driven at a great velocity; the chain was then dropped on the platform, along which it ran for some distance. It is not necessary that the chains form circular loops to do this. The loop may be tall and narrow, and will, while running along, keep the longer axis of the curve in its original upright position. Nor need the chain be heavy. A watch-guard was hung over a pulley about eight inches diameter; it then formed a loop about eight inches broad by about two feet high. When thrown off the pulley it glided along the platform for some distance. The chains were also dropped on an inclined polished surface, on which they remained standing in rapid motion for some time.

All these experiments only illustrate the balance of the centrifugal force, and the tension when the motion is all in one plane. The next experiment is to illustrate this balance when the motion takes place in different planes. This is easily illustrated by means of a circular disc of paper, or any other flexible material. If we bend the disc while it is rotating, we find that the bent part does not rotate with the disc, and that the disc only slowly regains its original flat form. If we load the outside of the disc with a row of flattened pellets of shot, we increase the resistance or rigidity of the disc while in motion, and if the weight is such that it just balances the elasticity of the paper, then the bend will remain in the same place for a very long time while the disc is rotating rapidly. The disc may even be bent till the circumference touches the centre, and while the bend keeps its place the chain of shot is passing through many planes, and the tension at the different points just balances the centrifugal force.

Before proceeding to experiment with the horizontal chain, I must refer to the disturbing forces in action causing the chain to change its form while in motion. When looking at these endless chains in motion, the most marked effect of this motion is to cause a curious reverse curve just after the chain has turned at the lowest point of its path and has begun to ascend. This reverse curve was supposed to be produced by friction from the great tension produced by the centrifugal force; but that it is not really so, is easily proved by taking two precisely similar chains and oiling one and passing the other through a flame to remove all grease. The only difference between the two chains now is that the friction in the one is greater than in the other. If we hang these two chains over two pulleys of the same diameter on the same shaft so as to drive both chains at the same velocity, we find that the oiled chain has the reverse curve well marked, while the friction in the other chain causes the loop to open out and take up a curve approaching a circle and shows no reverse curve, and when both chains are compelled to have the same curvature at bottom, the reverse curve is much the least where the friction is greatest. The reverse curve seems to be due to the change of motion which takes place in the links when moving in a path of varying curvature. For instance, when the links are descending along the flat part of the curve, their motion is almost simply one of translation, whereas when passing round the curves they have a motion of rotation as well as a motion of translation, the result of which is, that the links resist this rotation at the entrance of the curve, and thus flatten out the curve on that side, and after the rotation has been communicated to them, they tend to keep this rotation, and thus continue the curve at the lower end of the chain much farther round than if the chain was not in motion. And, for very evident reasons, the quickest part of the curve is not at the bottom, but a short distance up the ascending side; and farther, the rotation of the link at the bottom is quicker than that corresponding to the curvature. These points may all be illustrated by a chain in which the links are short and the chain as thick as possible, so that the moment of inertia of the links round an axis perpendicular to the plane of the motion of the links is as great as possible. Such a chain when properly made gives a series of large and well-marked waves all the way up the one side of the loop and down the other. The length of the links also tends to change the form of the curve. If we have two chains of the same material and same size every way, except in the length of the link, then the larger the link the more the chain tends to open out the curves and take the circular form, and the smaller the links the nearer it approaches the form it has while at rest, and the more marked the reverse curve becomes.

An elastic band in rapid motion will also tend to take up a circular form, because the strain at the quick part of the curves will tend to open them out, in the same manner as when the band was at rest. An elastic chain while in motion does not show the reverse curve like a chain, probably because the strain on the material prevents it doing so.

In the previous experiments gravitation acted on the chains, so that whatever form we might impress on them, gravitation constantly tended to change that form and bring it back nearly to the form it would have if gravitation alone acted on it. An attempt was therefore made to get quit of the disturbing effect of gravitation. Different ways were tried of effecting this, but none of them were thoroughly successful. The next experiment shows the most successful method tried, namely, suspension. The chain is hung by means of a number of fine cords to a circular disc, capable of rotating about a vertical axis placed as far above the chain as possible. The chain is driven by means of a rapidly revolving horizontal pulley running on a vertical axis, and to give sufficient friction the chain is pressed to the pulley by means of an elastic wheel. The centre of suspension is so arranged that it can be brought over the driving pulley, or removed to some distance from it, so as to be able to bring the centre of suspension over the centre of gravity of the chain, whatever shape the chain may be caused to take. The chain so suspended, when in rapid motion, retains for a considerable time whatever form we please to give it. It may be moulded into a most complicated series of curves, and though it resists any effort made to alter these curves, it has itself but little tendency to do so. If we observe the chain closely, we will however find that the disturbing forces, which I have already referred to, are acting on the chain, tending to change its curvature. For instance, if we keep the point of suspension over the centre of gravity of the chain, we will find after some time that the chain will take up a circular form. This is caused by the friction in the chain, and other causes. Again, the effect of the varying rate of rotation of the link on its own axis is also well marked, but is quite different from what we get when the chain is hung over the driving pulley. When the chain is hung over the pulley, there is a tension due to the weight of the chain. This tension gives rise to the wave form which certain chains take up when in motion. The tension due to the centrifugal force has no such effect. When, therefore, the chain is suspended and gravitation removed, there is no tension preventing the chain from continuing to curve always in the same direction; and if we use a chain specially prepared to show this effect, such as the one already referred to, the chain goes on bending further and further round till it comes against the part of the chain coming in the opposite direction and stops the motion, even though the chain at that point is also bending out of the way on account of the resistance offered by the links to rotation on their axis.

Monday, 3d January 1876.

Sir WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. On the Electrical Conductivity of Stretched Silver Wires. By J. G. MacGregor, M.A., B.Sc. Communicated by Professor Tait.

The apparatus which I used in a few experiments on silver wires was as follows:—To a beam, supported in stonework, a plate of copper was fastened, upon which a smaller plate could be tightly screwed. Between the two plates a very thick copper wire was secured, vertically. Its lower end was provided with a small plate of copper, fastened by screws. This plate served to make fast one end of the silver wire under investigation. The other end was joined in the same way to a second thick copper wire; this was provided with a horizontal round brass plate, through the centre of which it passed, and which acted as weight-carrier. A length of about 8 mm. at the end of the part of the copper wire which projected below the weight-carrier was amalgamated, and, while hanging quite free, dipped into a glass cup containing mercury, which, by means of a long screw, could be elevated or depressed by any desired amount. When measurements of resistance were made it was always placed in such a position that the amalgamated part of the copper wire was just beneath the surface of the mercury. The glass cup served also to support the weight-carrier during the adjustment of the weights, that the silver wires might be subjected to no jerks. After putting on weights the cup was lowered very slowly and steadily until the weights hung free. A copper wire (4·7 mm. thick and 30 cm. long), dipping in the mercury, joined up the silver wire as one of the arms of a Wheatstone's bridge. At the upper end of the copper wire, which was fastened to the beam, two other copper wires were fastened by binding screws. One of them went to the galvanometer; the other was the standard wire, with whose resistance that of the silver wires was compared. For all the observations on a single wire, it had, in all cases, as nearly as possible the same temperature. That it might not be affected by warm or cold currents of air it was defended by a coating of gutta percha, and made to pass through a tube of water whose temperature could readily be noted. By dipping into a mercury pool it was joined up as a second arm of the Wheatstone's bridge. A length of about 5 mm. of the end which dipped in the mercury was well amalgamated. Above that the wire was varnished by a non-conductor, so that contact began always at the same point of the wire. The other two arms of the bridge consisted of the segments of a German-silver wire,—Kirchhoff's form of the Wheatstone bridge being used exactly as described by Wiedemann in his "Galvanismus."* The galvanometer used was Wiedemann's mirror galvanometer,† the deflections of the mirror being observed by means of a telescope. The current employed was that of a Bunsen's cell of great internal resistance. The length of the wire was determined by a very delicate cathetometer, which could measure accurately to ·02 mm. The lower end of the copper wire which was fastened to the beam, was smooth and flat, and cut at right angles to its vertical axis. The edge of the small plate was correspondingly cut, so that the exact point at which the silver wire was seized and compressed by the copper plate could be seen through the telescope of the cathetometer. The clamp which seized the lower end of the silver wire was arranged in the same way. The wires, of whose resistance measurements were made, were of pure silver, and were carefully drawn by M. E. Stöhrer, philosophical instrument maker of Leipzig. They were always raised to a red heat before being subjected to tension, care being taken that fusion did not occur in any part. In order to determine the effect of tension on the conductivity of the wires, it was necessary to know the relation of their diameter before to their diameter after being stretched. This was estimated by a careful measurement of lengths and specific gravities. For the latter purpose a chemical balance was employed, which could weigh accurately to ·0001 grm. As the wires had to be rolled up to prevent their touching the sides of the vessel containing the distilled water in which they were weighed, the measured specific gravity was pro-

* "Galvanismus," vol. i. pp. 251–255, 2d German ed., 1872.

† "Galvanismus," vol. ii. pt. 1, pp. 227–230, 2d Ger. ed., 1873.

bably not exactly that of the wire of measured resistance. The error, however, must have been very slight.

The course of procedure was as follows:—The wire was heated red hot in an alcohol flame. After cooling, its specific gravity was determined. It was then fastened by the copper plates to the thick copper wires, and a slight weight was attached just sufficient to straighten the wire, that its length might be accurately ascertained (it was straightened as much as possible by being drawn between the fingers before being fastened in the apparatus). When straight enough for the determination of its length, its resistance was also measured by the method of double observation, as described in the "Galvanismus" (see above).* After the determination of resistance, weights were carefully piled upon the carrier, which during the operation was held fast from below. They were then allowed to stretch the wire gradually until it hung quite free, and its elongation had ceased. Then the length, the resistance, and finally the length a second time, were determined, the second measurement of length being made in order to be certain that there had been no further elongation. The weights were now carefully taken off, the carrier being again supported from below, and the specific gravity of the wire was measured as before, the compressed ends, however, having been cut off. A large number of experiments were rendered useless by the fact that too great weights were attached. Either the wires broke, or they were found on inspection to have too variable a diameter to be regarded as uniform. In the result given below such small weights were used that almost no difference of diameter throughout the whole length of the wire could be noticed by means of a magnifying instrument. Thus the wire could be treated as uniform, and the specific gravity method assumed to give its diameter.

The results of the examination of three wires are given in the following table:—

* Instead of the formula given in "Galvanismus," the following was used:—

$$s^3 + s^2(3d_{12} - d_1 - d_2) + s(d_1 d_{12} + d_2 d_{12} - 3d_1 d_2) - d_1 d_2 d_{12} = 0.$$

The length of the German-silver wire as found by this formula was 1108·795 mm. As measured by the cathetometer its length was 1108·8 mm.

	Length (mm.)	Weight (grms.)	d* (mm.)	Resistance (standard copper wire =unity).	Length (length before stretching=unity).	Resistance (resistance before stretching =unity).
Wire I.	777·62 809·56	2246 5075	198·8 239·8	1·4369 1·5519	1 1·0411	1 1·0800
Wire II.	830·82 890·04	1246 6746	234·9 311·2	1·5376 1·7803	1 1·0713	1 1·1579
Wire III.	660·86 729·14	1246 7911	114·7 216·9	1·2308 1·4864	1 1·1033	1 1·2105

These results agree, as might be expected, with those which Mousson† has published on steel, iron, and copper wires, in the fact that the resistance increases very much faster than the length. This must be the case unless there be a diminution of resistance, due to tension, sufficient to neutralise the increase of resistance due to decrease of the cross section of the wire. It is interesting to ask, then—Does the decrease in the diameter of the wire account for that part of the increase of its resistance which is not due to the increase of its length? The following table answers this question. The column headed "calculated resistance" contains the resistance as it ought to have been if its increase had been due only to change of dimensions:—

	Specific Gravity		Observed resistance after stretching.	Calculated resistance.
	Before stretching.	After stretching.		
Wire I.	10·4784	10·5330	1·080	1·092
Wire II.	10·4967	10·5646	1·157	1·155
Wire III.	10·5051	10·5394	1·210	1·220

The agreement of the figures in the observed and calculated columns is very close, notwithstanding the many sources of error to which the experiments were liable, such as the change in

* See Wiedemann's "Galvanismus," vol. i. p. 255.

† "Galvanismus," vol. i. p. 310; "Neue Schweizerische Zeitschrift," vol. xiv. (1855), p. 33.

specific gravity produced by the rolling up of the wires, their extension by weight between the first determination of specific gravity and the first determination of resistance, the irregularity in their cross section produced by stretching, and the slight contraction of the wires after the removal of the weights and before the second determination of specific gravity,—all of which, however, must have been exceedingly slight. It seems to warrant the statement that if tension has any effect upon silver wires at all the effect is exceedingly small. This differs from Mousson's conclusion as to steel, iron, and copper wires. He found that the increase in their resistance produced by stretching was not fully accounted for by the change of their dimensions.

In the course of the experiments I found that by raising a silver wire, which had been stretched, to a red heat, its resistance was very slightly diminished. A wire of about the dimensions of No. III., which, after having been stretched by 6985 grms. had a resistance of 1·8135, had, after being heated red hot, a resistance of 1·8103. This is again different from what Mousson has found to be true of steel, iron, and copper wires, but it agrees with a determination made by Becquerel on silver wires.*

The following tables contain series of observations made for the purpose of finding the relation between the stretching weight and the total increase in the resistance of the silver wires used. In these determinations, the constant resistance with which the resistances of the stretched wires were compared was that of a silver wire. Both wires were surrounded by a coating of steam. The stretched wire, in order that, by its being kept at a high temperature, greater elongations might be produced by the same weights; the constant wire in order that thermo-electric effects might be eliminated. The steam coating was formed by enclosing the wires in glass tubes, and these tubes in a much larger tube, and conducting steam between them. In other respects the apparatus and mode of procedure were quite the same as before. The observations were made when the appended weights had ceased to produce any appreciable elongation, and with the steam coating half-an-hour was generally found to be a sufficient length of time for the production of the total stretching effect.

* "Ann. de Chimie et de Physique" (3), xvii. 1846, p. 253.

able I.

Weight (grms.)	d mm.	Resistance (Constant Silver Wire = unity).
500	102·0	·8315
750	99·8	·8348
1000	99·6	·8351
1250	98·8	·8363
1500	95·8	·8409
1750	91·4	·8477
2000	82·0	·8623
2250	63·0	·8925

Table II.

1285	60·6	·8963
1535	52·6	·9094
1785	42·6	·9260
2035	26·6	·9531
2285	— ·6	1·0011
2535	—42·2	1·0791

Table III.

873	4·2	·9924
1285	— 24·6	1·0453
1535	— 48·2	1·0909
2035	—121·4	1·2459

Table IV.

1873	678·6	·2407
2873	676·8	·2419
3214	670·6	·2463*
4857	663·0	·2516
5171	659·6	·2540
5921	647·2	·2629
6490	635·0	·2717
6964	623·4	·2802
7650	600·8	·2971
8176	578·8	·3141
8307	567·0	·3233
8842	532·0	·3515

* This measurement is marked in my notes as "inaccurate, owing to an error of observation."

It will be seen that the relation between appended weights and thereby increased resistances is not that of simple proportion. In this respect silver wires appear again to differ from copper wires. Some experiments made by Messrs Meik and Murray* having shown that the changes of resistance of copper wires, when stretched by weights, are directly proportional to the weights.

I am deeply indebted to Professor Wiedemann of Leipzig, in whose laboratory these experiments were performed, for the excellent apparatus which he kindly placed at my disposal, and for the advice and assistance with which he favoured me.

2. On the Defoliation of the Coniferæ. By Dr Stark.

3. On Diamagnetic Rotation. By George Forbes, Esq. M.A., F.R.A.S.

Faraday's discovery of the magnetic rotatory polarisation of light may be expressed in the following manner:—Let two electro-magnets, in the form of iron tubes, surrounded by helices of wire, be placed end to end, so that in the space between them the lines of force are very intense. Let a rod of dense glass be placed in this space, so that a ray of light may pass through the two tubes and the rod of glass. Let such a ray on entrance be plane-polarised, so that the direction of vibration is in a vertical direction. If the electro-magnet be now magnetised, the emergent ray will be polarised, so that its vibrations are inclined to the vertical at a small angle. The direction in which the line of vibration has been rotated is the same as the direction of the positive current in the helices.

The same effect might be produced without the aid of magnetism if the rod were rotated round the axis of the ray of light with great velocity. The rotation of the plane of polarisation

* Proc. Roy. Soc. Edin., Session 1869–70, p. 3.

would be the angle rotated whilst the light traverses the glass rod. This is on the assumption that the ether within the rod is likewise rotated. In the magnetic experiment it is easy to produce a rotation of 1° in a piece of glass three inches long. Light takes $\frac{1}{4,000,000,000}$ of a second to traverse this distance. Hence, to produce an effect equal to the magnetic effect, the glass rod would require to be rotated 10,000,000 times in a second. We cannot determine with great precision the plane of polarisation of a ray of light, hence we cannot measure any rotation of the plane of polarisation which might be thus produced.

In the same manner, if we suppose the molecules of glass and the accompanying ether to be rotated round the lines of magnetic force, and in the direction of the positive current producing the given magnetic field, then the phenomena observed by Faraday would be explained; and we should be able to determine the number of rotations per second induced in any specimen of glass with a given intensity of magnetic field.

So soon as the electro-magnet is demagnetised, the rotation of the molecules ceases. It seems, then, that there is a friction among the molecules tending to stop the rotation. Hence we should be led to conclude that the energy of the magnet is gradually used up by the presence of the piece of glass.

Assuming, then, that there is a friction among the molecules of glass, it follows that when the electro-magnet is magnetised the rod of glass has a tendency to turn bodily round an axis through its centre of gravity parallel to the lines of force; and if the rod of glass were free it would turn round this axis.

In the winters 1872–3 and 1873–4, I made a number of experiments to put this hypothesis to the test. The general idea of the experiments was this. A rod of glass was suspended by a fine skein of silk fibres between two poles of an electro-magnet, one pole above, the other below. A small mirror was fixed on the rod, and a lamp and scale arrangement was mounted for measuring rotations. Readings were taken when there was no current, and also in the two positions of the commutator.

The result of these experiments seems to be that there is an effect of the kind anticipated. Sometimes, it is true, a deflection was produced by a diamagnetic repulsion, owing to a want of absolute symmetry in the arrangements. This rotated the glass rod slightly, and produced a little confusion. But beyond this there was an undoubted effect, for the nature of the deflection was found to depend upon the polarity of the two poles, the effect being different according as the upper pole was a north or a south one. I was sometimes able, simply by timing the reversals of the commutator, to get up a very large rotation-swing, and by the same means I was able to stop the rotation swinging.

I delayed the publication of these experiments with the view of establishing more certain results. But much time has elapsed, and I have still been unable to find time for this. Hence, I am unwilling any longer to withhold their publication. I will now give the experiments in detail, and will conclude by collecting the general results.

Description of the Apparatus.

A rod of glass was suspended by a strand of silk fibres attached to one end. This was supported on a stone imbedded in the wall of the laboratory. The rest of the apparatus was on a separate stand. A horse-shoe electro-magnet was placed so that the poles were vertically over each other, and as nearly above and below the axis of the glass rod as possible. Thus the axis of the glass rod lay along the lines of magnetic force. The electro-magnet was connected through a commutator with a battery of Grove cells. Upon the glass rod was attached a small piece of silvered glass, by means of which the light reflected from a paraffin lamp fell upon a scale divided to millimetres. The distance of the scale from the glass rod was about seven decimetres. The apparatus was arranged so that the glass rod was suspended within a glass jar or bottle, to get rid of currents of air.

During the session 1872–73 thick flint glass tubes were employed for suspension, and an electro-magnet weighing about 6 lbs. After that a rod of Faraday's heavy glass was used, and an electro-magnet weighing about 50 lbs.

Details of Experiments.

The first experiments were made in 1873, March 28, and they were continued regularly until April 16. The rod of flint-glass was suspended in air, water, and oil. A rotation was nearly always communicated to the rod when contact was made with the electro-magnet. When the magnetism of the electro-magnet was reversed I sometimes got a rotation in the opposite direction, and sometimes a rotation in the same direction, but to a different extent. The apparatus was repeatedly dismounted and set up again with all the parts changed. The effects were generally unaltered. In noting the experiments, I used the terms "no contact," "right contact," and "left contact," as being least liable to lead to error. "No contact" means that the circuit was broken. "Right contact" means that the lower pole of the electro-magnet would, if free, point to the north. "Left contact" means that the lower pole of the magnet would, if free, point to the south. When the readings of the scale are increasing the glass is rotating in the direction of the hands of a watch with the face upwards. The glass was sometimes put into water or oil, and in this case also rotation was generally observed.

The rotation oscillations in air were so great as to necessitate reading the scale at the end of each half oscillation, and taking a mean. In the following table each reading is a mean of many observations:—

March 29.—*Flint-Glass in Air.*

No contact, . .	245	scale reading.	Means.
Right ,, . .	155		L = 146.
Left ,, . .	146		R = 155.
No ,, . .	235		N = 240.

Flint-Glass in Water.

No contact, . .	415	
Right ,, . .	268	L = 248.
Left ,, . .	248	R = 268.
No ,, . .	410	N = 412.

Flint-Glass in Oil.

Right and left contact each continued to diminish the scale reading during twenty-two minutes. The change from left to right contact diminished the readings with a jump. "No contact" again reversed the rotation.

Flint-Glass in Water.

No decided effect.

Flint-Glass in Air.

No contact, . .	370 scale reading.	Means.
Right ,, . .	523	N = 370.
Left ,, . .	478	L = 478.
No ,, . .	370	R = 523.

The poles of the electro-magnet were now put so far to the east of the glass as possible, and again as far to the west. The same kind of results were obtained.

Oil was again used, and no certain effect was observed. *March* 31.—The apparatus, as left last day, with oil in the jar, showed no effect. No effect was shown with air. But on trying a new rod of flint-glass the old effects were observed. The oil seemed to have destroyed the effect.

Flint-Glass in Air.

Right contact, steady rise from 400 to 543 in about 4 minutes.

No contact, ...	558	
Left ,, ...	567	
Right ,, ...	546	
Left ,, ...	556	R = 538.
No ,, ...	540	N = 542.
Right ,, ...	531	L = 552.
No ,, ...	529	
Left ,, ...	533	

Half-an-hour interval.

No contact, ...	522	
Right ,, ...	512	R = 509.
Left ,, ...	523	N = 515.
Right ,, ...	497	L = 521.
Left ,, ...	519	
No ,, ...	508	

By keeping right contact for decreasing rotation-swing, and left for increasing, the range of swing increased after 6 swings to 123 divisions. On reversing this action for 6 swings the range was reduced to 12 divisions.

Note.—These are the most striking experiments. It will be noted that here the "right" gives lower readings than the "left" contact. But I cannot be sure that the commutator arrangements had not been changed. Here all disturbing effects are done away with, and the right and left contacts produce deflections on opposite sides of the "No contact" reading.

Flint-Glass in Water.

No contact, . . .	541 scale reading.	Means.
Right ,, . . .	531	R = 531.
Left ,, . . .	551	N = 541.
No ,, . . .	540	L = 551.

A new rod was now prepared, which also showed a very decided difference between right and left contact, right being lowest.

Numerous experiments under various conditions were now tried, but no new effects were discovered. It is desirable, however, to record the following, which were made to be certain as to the direction of rotation under different conditions of polarity :—

No contact, ...	850
Upper pole would point to north if free, . .	840
No contact, ...	848
Lower pole, ...	854
Upper pole, ...	835
Lower pole, ...	850
No contact, ...	845

The general result of the experiments of the winter is, that *when a pole tending to point to the north is above the glass rod, and a pole tending to point to the south is below, the rod turns in the opposite direction to the hands of a watch with the face upwards.*

On placing the magnet with the poles in a horizontal plane, rotations were sometimes observed. This is in direct contradiction to the theory that led to these experiments. I can offer no explanation, but merely record the fact.

In the session 1873–74 similar experiments were made with a piece of Faraday's heavy glass. But in this case the glass was inserted in a hole in a cork, to which also the mirror was attached. On examining the cork it was found to be magnetic. This accounted for all the phenomena observed.

But in the previous session this would not account for the phenomena. There was no cork employed; and experiments made to test this by altering the position of the magnet showed that there was no magnetism in the arrangement.

It was the doubt that hung over the last winter's experiments that made me wish to delay publishing any results until I should have finally settled the matter. I have been unable to do so hitherto, and offer the original experiments in the meantime.

Note on the preceding paper.

[The first statement is that a rotation of the plane of polarised light might be produced by rotating a transparent body about the ray as an axis.

It is improbable that no such effect would be produced, but that the question is by no means a simple one may be seen by looking at Sir W. Thomson's paper on this subject (Proc. R. S. Lond., 1856).

I have also tried a great many hypotheses besides those which I have published, and have been astonished at the way in which conditions likely to produce rotation are exactly neutralised by others not seen at first.

There can be no doubt, however, that a rotation of some kind is going on in a diamagnetic medium under magnetic force, and this may be of the molecules of the glass of the ether or what not, and this probably goes on in all media whether transparent or not.

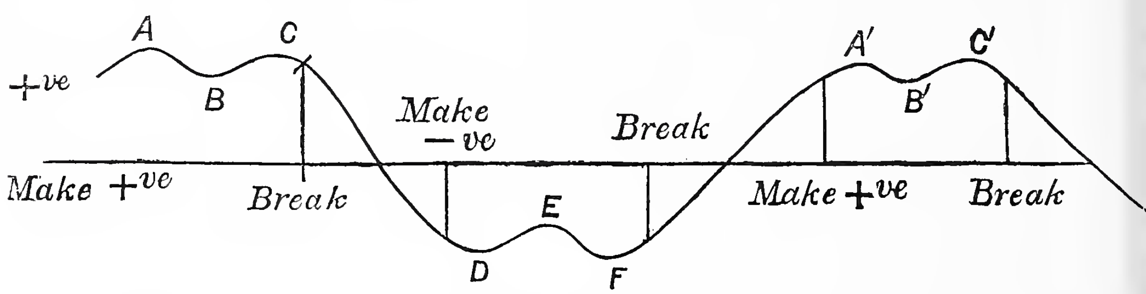
This rotation, as Prof. George Forbes says, stops as soon as the magnetic force is removed. He supposes that it is stopped by friction, and therefore, that energy is being dissipated at all times as long as magnetic force acts on a medium.

But we know that a magnet will retain its magnetism for a long time, and it has never been shown that a magnet must necessarily lose its magnetism. Hence we must admit that the molecular rotation is not accompanied with friction, but that it is set up by electro-motive force, and exerts electro-motive force when it is stopped, like a rotating body having inertia.

(α) If the friction supposed by Prof. Forbes exists, it would act as an accelerating force on the glass, so that if free it would rotate faster and faster up to a certain great velocity, and if suspended by a fibre, it would rotate till the moment of friction was balanced by the moment of tortion of the fibre.

(β) If there is no friction the only effects possible would be those due, not to the maintenance, but to the starting and stopping of the molecular rotation.

To investigate (α) experimentally we must observe the elongations of the oscillation as follows:—



Make $+^{ve}$ observe three turning points A B C, break for nearly half a complete vibration. Make $-^{ve}$ observe three turning points D E F, break again, and make $+^{ve}$, and so on. Then the result is obtained by taking

$$\frac{1}{8n} \Sigma\{A + 2 B + C - (D + 2 E + F)\}$$

when n represents the number of repetitions of the series of six observations.

To investigate (β) experimentally we must make and break when the mirror is passing the point of equilibrium.

In Prof. Forbes's experiments there is a disturbing effect due to the ordinary diamagnetic action of the electro-magnet on the tube, which, if the tube is not perfectly symmetrised about the axis of the fibre, will tend to produce rotation. This force, however, is the same whether the current be + or −, provided the position of the tube is the same. Hence, if the + and − currents are exactly equal, it may be possible to distinguish this effect from the effect sought by Prof. Forbes.

J. Clerk Maxwell.]

4. On the Linear Differential Equation of the Second Order. By Professor Tait.

(*Abstract.*)

This paper contains the substance of investigations made for the most part many years ago, but recalled to me during last summer by a question started by Sir W. Thomson, connected with Laplace's theory of the tides.

A comparison is instituted between the results of various processes employed to reduce the general linear differential equation of the second order to a non-linear equation of the first order. The relation between these equations seems to be most easily shown by the following obvious process, which I lit upon while seeking to integrate the reduced equation by finding how the arbitrary constant ought to be involved in its integral.

Let u and v be any functions of x,

$$\xi = \frac{\mathrm{A}\frac{du}{dx} + \mathrm{B}\frac{dv}{dx}}{\mathrm{A}u + \mathrm{B}v} = \frac{u' + \mathrm{C}v'}{u + \mathrm{C}v} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1),$$

where B and A, and therefore their ratio C, are arbitrary constants. The elimination of C from (1) must of course give a differential equation of the first order in ξ.

We have

$$\xi' = \frac{u'' + \mathrm{C}v''}{u + \mathrm{C}v} - \left(\frac{u' + \mathrm{C}v'}{u + \mathrm{C}v}\right)^2 .$$

Now we have, by adding and subtracting multiples of (1), &c.,

$$\xi' = \frac{u'' + \mathrm{P}u' + \mathrm{Q}u + \mathrm{C}(v'' + \mathrm{P}v' + \mathrm{Q}v)}{u + \mathrm{C}v} - \left(\frac{u' + \mathrm{C}v'}{u + \mathrm{C}v}\right)^2 - \mathrm{P}\xi - \mathrm{Q} ;$$

whence, if u and v are independent integrals of the equation

$$y'' + \mathrm{P}y' + \mathrm{Q}y = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2),$$

we have the required equation

$$\xi' + \xi^2 + \mathrm{P}\xi + \mathrm{Q} = 0$$

and the process above shows why it takes this particular form.

But (2) gives

$$y = \mathrm{A}u + \mathrm{B}v$$

as the complete integral, so we see that

$$\frac{y'}{y} = \xi .$$

Various classes of cases in which this form is integrable are given, of which the following is one:—

Let $\xi = \eta\sqrt{Q}$, then the equation becomes integrable in the form

$$\frac{\eta'}{\eta^2 + m\eta + 1} + \sqrt{Q} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3),$$

provided

$$P\sqrt{Q} + \tfrac{1}{2}\frac{Q'}{\sqrt{Q}} = mQ ,$$

$$i.e., \qquad \frac{\epsilon^{-\int P dx}}{\sqrt{Q}} = -m\int \epsilon^{-\int P dx}\, dx .$$

The next subject treated is the effect of the alteration of sign of P or Q in (2). This is illustrated by the equation

$$y'' \pm xy' \pm y = 0 ,$$

which is integrable or at least reducible to quadratures for any of the four combinations of sign.

The always integrable case where

$$P = (C - x) Q$$

is next examined.

Another portion of the investigation deals with certain infinite but convergent series, whose sums can always be expressed in terms of the integral of a linear differential equation of the second order.

Consider, for instance, the expansion

$$\epsilon^{px + \frac{1}{x}} = \left(1 + px + \ldots + \frac{p^n x^n}{\underline{|n}} + \ldots\right) \times \left(1 + \frac{1}{x} + \ldots + \frac{1}{x^n \underline{|n}} + \ldots\right) (4)$$

$$= \Sigma\, P_n x^n , \text{ suppose.}$$

Obviously we have

$$P_n = p^n P_{-n} = \frac{p^n}{\underline{|n}} + \frac{p^{n+1}}{\underline{|1}\,\underline{|n+1}} + \frac{p^{n+2}}{\underline{|2}\,\underline{|n+2}} + \quad . \quad . \quad . \quad .$$

From this at once

$$\frac{dP_n}{dp} = P_{n-1} , \text{ whence } P_n = (\int dp)^n P_0 \quad . \quad . \quad (5).$$

Also

$$\frac{d}{dp}\left(\frac{P_n}{p^n}\right)=\frac{P_{n+1}}{p^{n+1}}\,,\ \text{whence } P_n=p^n\left(\frac{d}{dp}\right)^n P_0 \quad . \quad . \quad (6).$$

Eliminating P_n between (5) and (6), we obtain

$$P_0=\left(\frac{d}{dp}\right)^n p^n\left(\frac{d}{dp}\right)^n P_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (7).$$

This equation is thus true for all positive integral values of n, and its form at once shows that it is true for negative integral values also. It is very singular that such a series of equations of all orders should have a common solution. But it depends upon the fact, which I do not recollect having seen in print, that

$$\left(\frac{d}{dx}\,x\,\frac{d}{dx}\right)^n=\left(\frac{d}{dx}\right)^n x^n\left(\frac{d}{dx}\right)^n .$$

This can be verified at once by applying it to x^m; as can also the companion formula

$$\left(x\,\frac{d}{dx}\,x\right)^n=x^n\left(\frac{d}{dx}\right)^n x^n .$$

Suppose we had, instead of (5) and (6),

$$\frac{dQ_n}{dq}=Q_{n+1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5^1),$$

$$\frac{d}{dq}\left(q^n Q_n\right)=q^{n-1}\,Q_{n-1} \quad . \quad . \quad . \quad . \quad . \quad (6^1),$$

we should find the *same equation* (7) for Q_0 as for P_0. In fact, as is easily seen,

$$Q_n=P_n .$$

Other pairs which alike give the equation

$$R_0=\left(\frac{d}{dr}\right)^n r^{-n}\left(\frac{d}{dr}\right)^n R_0 \quad . \quad . \quad . \quad . \quad . \quad (7^1)$$

are

$$\frac{dR_n}{dr}=R_{n+1}\,,\ \frac{d}{dr}\left(\frac{R_n}{r^n}\right)=\frac{R_{n-1}}{r^{n-1}}$$

and

$$\frac{dS_n}{ds}=S_{n-1}\,,\ \frac{d}{ds}\left(s^n S_n\right)=s^{n+1}\,S_{n+1} .$$

We thus get the two distinct particular integrals of each of the corresponding differential equations.

More generally,

$$\left(\frac{d}{dp}\right)^{\nu} P_n = P_{n-\nu},$$

and

$$\frac{P_n}{p^n} = \left(\frac{d}{dp}\right)^{\nu} \frac{P_{n-\nu}}{p^{n-\nu}};$$

whence

$$P_{n-\nu} = \left(\frac{d}{dp}\right)^{\nu} p^n \left(\frac{d}{dp}\right)^{\nu} \frac{P_{n-\nu}}{p^{n-\nu}}.$$

Changing $n-\nu$ to m, this becomes

$$P_m = \left(\frac{d}{dp}\right)^{-m} \left\{ \left(\frac{d}{dp}\right)^{n} p^n \left(\frac{d}{dp}\right)^{n} \right\} \left(\frac{d}{dp}\right)^{-m} \frac{P_m}{p^m},$$

which, when $m=0$, agrees with (7). Here n may have any positive integral value not less than m. When we write $n=m$ we have merely a truism. If we put $n=m+1$, we arrive at the same result as we should have obtained directly from the *first* forms of the equations (5) and (6). All these series satisfy differential equations of the form

$$x\frac{d^2y}{dx^2} - (n-1)\frac{dy}{dx} = y.$$

Corresponding properties are easily proved for the series forming the co-efficients of the various powers of x in the expansions of expressions like

$$\epsilon^{px^m + \frac{1}{x^n}}, \quad \epsilon^{px + \frac{\sqrt{-1}}{x}}, \text{ \&c., \&c.}$$

It is easily seen that what has been called P_0 above is the infinite series

$$P_0 = 1 + \frac{p}{1^2} + \frac{p^2}{1^2.2^2} + \frac{p^3}{1^2.2^2.3^2} + \ldots = f(p) \quad . \quad . \quad (8),$$

and that quite generally if

$$\Pi_m = 1 + \frac{p}{1^m} + \frac{p^2}{1^m.2^m} + \frac{p^3}{1^m.2^m.3^m} + \text{\&c.}$$

we have

$$\Pi_m = \left(\frac{d}{dp}\right)^n \left(p^n \left(\frac{d}{dp}\right)^n\right)^{m-1} \Pi_m$$

whatever positive integer be represented by n. Of this the simplest case is $\Pi_1 = \epsilon^p$, where of course

$$\left(\frac{d}{dp}\right)^n \Pi_1 = \Pi_1 \, .$$

Again, just as the solution of this equation has the property

$$\epsilon^p \; \epsilon^q = \epsilon^{p+q} ,$$

so it is easy to see that we have in (8)

$$f(p) \; f(q) = f(\overline{p+q}) ,$$

where the bracket over $p+q$ is employed to indicate that in the expansion we must *square* the numerical co-efficients of each term of a power of this binomial, *i.e.*,

$$\begin{aligned} \overline{p+q} &= p+q , \\ \overline{p+q}^2 &= p^2+q^2+2^2pq , \\ \overline{p+q}^3 &= p^3+q^3+3^2(p^2q+pq^2) , \\ \overline{p+q}^4 &= p^4+q^4+4^2(p^3q+pq^3)+6^2p^2q^2 , \\ \&c., & \qquad\qquad \&c., \end{aligned}$$

and a similar property, though of course involving higher powers of the co-efficients, holds for each of the functions Π_m above.

For the product of any two similar expansions (with different variables) is easily seen to have all its numerical co-efficients raised to any given power when those of the separate expansions are so raised.

The paper contains also an account of various attempts to solve the general equation of the second degree, of which the following may be noted.

a. Transform to

$$\frac{d^2y}{dx^2} - \mathrm{X}y = 0 ,$$

and evaluate

$$\iint \frac{1}{y}\frac{d^2y}{dx^2}\,dx^2$$

at once, just as

$$\int \frac{1}{y}\frac{dy}{dx}\,dx$$

is evaluated. The difficulty is reduced to finding the value of

$$\iint \left(\frac{dy}{dx}\right)^2 dx^2 ,$$

where a *single* operation is to be effected.

b. Transform to

$$\frac{d\xi}{dx} + \xi^2 = X ,$$

and express this by the help of an auxiliary operation in terms of a merely artificial quantity z, so that

$$\frac{d\xi}{dx} + \xi^2 = \epsilon^{x\frac{d}{dz}} Z$$

so that all equations of the kind considered can be reduced to the very simple form

$$\frac{d\xi}{dx} + \xi^2 = A\epsilon^{ax}$$

If this were integrated, the only remaining difficulty would lie in the separation of symbols from the quantities they operate upon.

5. On Two-dimensional Motion of mutually influencing Vortex-columns, and on Two-dimensional Approximately Circular Motion of a Liquid. By Sir W. Thomson.

Monday, 17th January 1876.

The Right Rev. Bishop COTTERILL, Vice-President, in the Chair.

The following Communication was read:—

On the Origin of Language—Max-Müller, Whitney. By Professor Blackie.

Professor Blackie stated that though the origin of language might be considered by some more a metaphysical than a philological question, it was yet so closely connected with philology, that whatever opinions a philologer held on this question could not fail to exercise a strong secret influence on his philological procedure. The primary elements out of which language grew were admitted by all to be three, viz., cries or interjectional exclamation, mimetic reproduction of audible sounds, technically but stupidly called onomatopœia, and gesture. But while agreeing on this threefold basis the most distinguished writers on this subject, such as Max-Müller, Wedgwood, Whitney, Bleek, Schleicher, and Steinthal, disagreed fundamentally, or at least seemed to be at daggers drawing, with regard to the course which language pursued in its further development. On the one hand, Müller, who had had a great influence in guiding public opinion in this region, stoutly asserted that out of the three primal elements, as from a root, no further growth of what we called human language, for reasonable social purposes, could take place; while, on the other hand, Wedgwood—who in this whole matter had, in his opinion, received scant recognition from the scholars of his own country—as stoutly maintained that from these three elements, as their natural root, the whole organism of the beautiful growth of language, stem, leaf, and fruit, could be satisfactorily explained. After carefully studying the arguments of the learned Oxford Professor, he was of opinion that Wedgwood was in the main right. To this conclusion he came from a course of independent investigation some years ago, and when, curiously enough, he had only used Wedgwood's dictionary for occasional consultation, without having read and pondered the discourse prefixed to the last edition of that work. The grounds of his opposition to Müller were stated to be simply these:—While in perfect agreement with him that roots significant of ideas are the ultimate facts in the analysis of languages as we now have them, and believing also that such conceptional roots are the natural and necessary expression of reason in a reasoning animal, and explicable only on the supposition of an indwelling plastic reason in man, I am at the same time unable to see why this plastic reason in the formative process of language-making should not have used the materials so amply supplied by the interjectional and mimetic elements of the simplest germs of speech. The interjectional element, I call the principle of significant vocal response; and the onomatopoetic principle, I call the mimetic, dramatic, or pictorial element in language; and I am prepared to show that, even under the many defacements and obliterations which spoken words, like old sixpences or wave-worn pebbles, suffer from the tear and wear of time, they yet show in hundreds of cases on their face the manifest superscription of their mimetic origin. For we must take note, first, that not only pigs and cuckoos, cats, curs, and crows, but all nature, is full of sounds, and that there is no absolute silence anywhere but in death; and, further, that not only an immense variety of sounds can be approximately expressed by imitation in articulated vocal breath, but that by an easy transference the impressions of the other senses can be analogically expressed in the flexible material of significant sound. It is also not only an easy but a natural and necessary process of the human mind in the formation of general concepts to use the material presented by mere sensuous impressions; and thus, while we expressly deny the perverse doctrine of the sensationists that mind can be explained from sense, or the imperial unity of thought be generated from a multiplicity of external impressions, we can see no difficulty in deriving such general concepts as *character* and *type*, for instance, from the roots χαράσσω and τύπτω, which originally were mere mimetic reproductions of an external sound, as in our word *scratch*. Language, therefore, was formed by the gradual extension of words originally expressing sensations and feelings to intellectual purposes; and there is nothing ignoble in this, for the mind uses the materials supplied by sense just as the architect uses the stones dug by the quarryman or the lime carried by the hodman. Neither can one at all see the logical justice of Max-Müller when he exposes the historical falsehood of some of Wedgwood's onomatopoetic etymologies; for the erroneous application of a principle does not in the least imply that the principle itself is erroneous; and, besides, the oldest roots to which certain very recent forms of Romanesque words may be traced back in Sanscrit can be shown in not a few cases to have been the product of that very mimetic process which Müller so persistently ignores. But while Müller seems actuated by some strange prejudice in his stout determination to make no use of the onomatopoetic element so thickly strewn in language, Professor Whitney has introduced no little confusion into this matter by talking of language as an *institution*, and reviving the doctrine of the old Greek Sophists, that language is θέσει, not φύσει. Every simile limps; but if we must have similes, it is far nearer the truth to talk of language as a growth and a living organism than to call it an institution. In some sense language is certainly a growth; in no sense is it an institution. Institutions like the Sabbath, for instance, are then creatures of positive law; but language is a direct efflux of plastic reason, and no more an institution than the song of the nightingale or a sonata of Beethoven. As to the connection between Darwinism and the origin of language, while the Darwinian philologers, with Schleicher at their head, will no doubt find a special delight in tracing the splendid roll of a Platonic period from the grumph of the primeval pig, and the mew of the pre-Adamitic kitten, those who with me look on Darwinism as a mere pleasant conceit of men besotted in the one-sided study of physical science, can, so far as philological conclusions are concerned, leave the conceit to shift for itself, being firmly convinced that whenever reason does show itself whether on the original appearance of man or at some after-stage of his development, it appears as a force altogether different from, and in some of its functions, as Professor Ferrier wisely maintained, essentially contradictory of, and antagonistic to, every kind and degree of mere sensation; and in this character brings forth language as the natural manifestation and organised body of itself.

Monday, 7th February 1876.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. Note on Certain Formulæ in the Calculus of Operations. By Professor Stokes, Hon. F.R.S.E. (In a letter to Professor Tait.)

"*January* 14*th*, 1876.

"Formulæ like those you sent me* are readily *suggested* by supposing the function operated on to be of the form $\Sigma A x^\alpha$, or say, for shortness, x^α, with the understanding that no transformations are to be made which are not equally valid for $\Sigma A x^\alpha$.

Thus

$$\left(\frac{d}{dx}\,x\,\frac{d}{dx}\right)^n x^\alpha = \alpha^2(\alpha-1)^2 \;\ldots\; (\alpha-n+1)^2 x^{\alpha-n}$$

$$= \alpha\,(\alpha-1) \;\ldots\; (\alpha-n+1)\left(\frac{d}{dx}\right)^n x^\alpha$$

$$= \left(\frac{d}{dx}\right)^n x^n \left(\frac{d}{dx}\right)^n x^\alpha\,;$$

and

$$\left(x\,\frac{d}{dx}\,x\right)^n x^\alpha = (\alpha+1)(\alpha+2) \;\ldots\; (\alpha+n)x^{\alpha+n}$$

$$= (\alpha+n)(\alpha+n-1) \;\ldots\; (\alpha+1)x^{\alpha+n}$$

$$= x^n\left(\frac{d}{dx}\right)^n x^n x^\alpha\,.$$

* See *ante*, p. 95.

The direct transformation may readily be effected by noticing, in the first instance, that any two operations of the form

$$x^{-m+1}\frac{d}{dx}x^{m}$$

are convertible. We find, in fact,

$$x^{-m+1}\frac{d}{dx}x \quad . \quad x^{-n+1}\frac{d}{dx}x^{n}=x^{2}\left(\frac{d}{dx}\right)^{2}+(m+n+1)x\frac{d}{dx}+mn\,,$$

into which m and n enter symmetrically.

Replacing the operations in the left hand member of the first formula by convertible operations, which will be separated by points, we find

$$\frac{d}{dx}x\frac{d}{dx}=x^{-1}\times x\frac{d}{dx}\;\;.\;\;x\frac{d}{dx}\,,$$

$$\frac{d}{dx}x\frac{d}{dx}x^{-1}=x^{-2}\times x^{2}\frac{d}{dx}x^{-1}\;.\;x^{2}\frac{d}{dx}x^{-1}\,,$$

and so on. Hence,

$$\left(\frac{d}{dx}x\frac{d}{dx}\right)^{n}=x^{-n}\left(x^{n}\frac{d}{dx}x^{-n+1}\right)^{2}\left(x^{n-1}\frac{d}{dx}x^{-n+2}\right)^{2}\dots\left(x\frac{d}{dx}\right)^{2},$$

$$=x^{-n}\left\{x^{n}\frac{d}{dx}x^{-n+1}\;.\;x^{n-1}\frac{d}{dx}x^{-n+2}\;\dots\;x\frac{d}{dx}\right\}^{2},$$

$$=x^{-n}\left\{x^{n}\left(\frac{d}{dx}\right)^{n}\right\}^{2}=\left(\frac{d}{dx}\right)^{n}x^{n}\left(\frac{d}{dx}\right)^{n}.$$

Again,

$$x\frac{d}{dx}x=x\times\frac{d}{dx}x\,,$$

$$x\frac{d}{dx}x^{2}=x^{2}\times x^{-1}\frac{d}{dx}x^{2}\,,$$

and so on. Hence

$$\left(x\frac{d}{dx}x\right)^{n}=x^{n}\times x^{-n+1}\frac{d}{dx}x^{n}\;.\;x^{-n+2}\frac{d}{dx}x^{n-1}\dots\frac{d}{dx}x\,,$$

$$=x^{n}\times\frac{d}{dx}x\;.\;x^{-1}\frac{d}{dx}x^{2}\dots x^{-n+1}\frac{d}{dx}x^{n}\,,$$

$$=x^{n}\left(\frac{d}{dx}\right)^{n}x^{n}.\text{''}$$

2. A Further Contribution to the Placentation of the Cetacea (*Monodon Monoceros*). By Professor Turner.

In the year 1871, I read before this Society a memoir on the Gravid Uterus of *Orca gladiator*, in which I discussed the placentation of the *Cetacea*. This memoir was published in the Transactions for that year. On the present occasion I purpose describing the placenta in a Cetacean genus in which it has not hitherto been examined.

In the month of December 1875 I received, through the intermediation of my friend Mr C. W. Peach, from Mr John Maclauchlan, the chief Librarian and Curator to the Free Library, Dundee, a cask containing the gravid uterus of a Narwhal (*Monodon Monoceros*), which had been procured by the captain of the Dundee whaling steamer "Erik." The uterus had been preserved in strong brine, and was in good condition for anatomical examination.

The uterus was two-horned, and contained a fœtus 5 feet 5 inches long in the left cornu. The gravid horn measured 7 feet 4 inches along its great curvature; the non-gravid, 4 feet. The girth of the gravid horn, at its thickest part, was 4 feet 4 inches. The length of the corpus uteri was 1 foot; that of the vagina, 1 foot 8 inches. The os was occluded by an extremely viscid mucus.

The uterine cornua were opened into by a longitudinal incision along the greater curvatures. The uterine wall was comparatively thin, and the chorion was closely adherent to its mucous lining. By an incision through the chorion, along the greater curvature of the gravid horn, the sac of the amnion was opened into and the fœtus exposed. The fœtus lay with its back in relation to the greater curvature of the cornu, its belly to the lesser curvature, its head close to the corpus uteri; whilst its caudal end was directed to the narrow end of the horn, but did not reach to within two feet of the Fallopian tube. The tail was curved forwards under the hinder part of the ventral surface of the fœtus. The pectoral flipper was directed backwards parallel to the long axis of the body. The umbilical cord was 3 feet long, spirally twisted, and bifurcating where it reached the sac of the allantois. The amnion formed an immense bag, which reached to 5 inches from the free end of the gravid horn of the chorion, but it did not extend into that part of the chorion which occupied the non-gravid horn. The amnion was closely adherent to the greater part of the chorion in the gravid cornu; but that portion of the chorion which was attached to the mucosa lining the lesser curvature of the cornu, and which lay opposite the abdominal aspect of the fœtus, was in relation to the wall of the sac of the allantois. The allantois formed a large funnel-shaped bag at the place of bifurcation of the cord. It was prolonged along the concavity of the chorion to within 2 inches of its free end in the gravid horn, and to within 9 inches of the free end of the prolongation of the chorion into the non-gravid horn. The length of the sac of the allantois was therefore much greater than that of the amnion, though its capacity was much less. The allantois was prolonged as a slender tubular urachus into the umbilical cord, which also contained two large arteries and two veins. The amnion investing the cord had numerous brownish corpuscles, resembling those I have described in *Orca gladiator*, projecting from it; and similar corpuscles were scattered over that part of the amnion which was in apposition with the wall of the sac of the allantois, and a few were seen on the amnion beyond the border of the allantois. In addition to these brown corpuscles, numerous other bodies of a dull white appearance were found. Sometimes these were slender rods, from $\frac{1}{10}$th to $\frac{4}{10}$th inch long, arranged end to end like the links of a chain, at other times they were globular, like minute shot. The rods were most numerous on the abdominal half of the cord, whilst the globules were most numerous at and near its bifurcation. The surface of the amnion adjacent to the cord had a few of these globules scattered over it. These white bodies were covered by the smooth amnion, which with a little care could be stripped off as a distinct pellucid membrane. They consisted of crowds of squamous epithelial cells, so that in structure they resembled the whitish bodies which are so abundantly developed in connection with the amnion of the cow. Between 3 and 4 inches of the abdominal end of the cord was covered with cuticle, which had the purplish-grey colour of the cuticular investment of the adjacent surface of the wall of the belly.

The two uterine cornua became continuous with each other through the corpus uteri, and were partially separated by an imperfect septum, which projected from the inferior wall. Owing to the great distension of the left cornu, this septum was pushed to the right, so that the os uteri opened directly into only the gravid horn. The chorion extended from the end of the gravid to that of the non-gravid cornu. As it passed through the corpus uteri it was somewhat constricted by the projecting septum. In the whole length of the non-gravid horn, and at the free end of the gravid horn, the chorion was raised into strong longitudinal folds, which corresponded in reverse order with a similar series of folds of the uterine mucosa radiating from the orifices of the Fallopian tube. At the os uteri the mucosa was raised into strong folds, which radiated into the gravid chorion for a considerable distance, and in some parts of their extent projected as much as 3 inches from the general plane of the mucosa, though at the os they had not more than one half that projection. The chorion in apposition with this part of the mucosa was also folded. In the gravid horn opposite the fœtus, where the expansion both of chorion and uterus was the greatest, the folds were not present. Except in a few localities, to be immediately specified, the whole of the extensive surface of the chorion was so covered with vascular villi that, to the naked eye at least, no non-villous intervals could be recognised. The chorion was adherent to the uterine mucosa, so that gentle traction was needed to draw them asunder; and, as the one was peeled off the other, the villi of the chorion were seen to be drawn out of multitudes of crypts opening on the free surface of the mucosa.

The chorion, which lay opposite the os uteri and the immediately surrounding mucous membrane, was for the most part not villous, but presented a smooth, feebly vascular appearance, which contrasted strongly with the adjacent villous chorion. This smooth spot was irregular in form, measured 6 inches by 4 inches, and from it narrow bands of smooth chorion radiated outwards for from 2 to 3 inches between the villous covered folds of the chorion. It was similar to, but much larger, than the corresponding spot in *Orca* and the Mare. Small isolated patches of villi were scattered irregularly over the surface of this smooth spot. The inner surface of the chorion at the bare patch was lined by the amnion and not by the allantois. Three inches from this large spot a bare patch, 1 inch by inch, was completely surrounded by villous chorion, The uterine mucosa opposite these smooth portions of the chorion was smooth and free from crypts, except where the isolated patches of villi were in apposition with it. Radiating for about 1 inch from the pole of the chorion in the gravid horn were narrow non-villous bands of the chorion separated by intermediate villous surfaces. These bands corresponded to folds of the mucosa free from crypts, which radiated from the orifice of the Fallopian tube, and were continuous with the longitudinal folds of mucosa in that tube. In the non-gravid horn the chorion was devoid of villi for about 5 inches from its free end, and for even a greater distance the villi were irregularly scattered so that well-defined smooth patches could be traced as far as 10 or 12 inches from the pole. On some parts of the chorion pedunculated hydatid dilatations of the villi, about the size of small peas, were irregularly scattered.

When examined microscopically, the villi were seen to be arranged in tufts, which varied in size and in the number of villi. Some tufts had not more than two or three villi, but more usually numbers were collected together, though occasionally short single villi arosefrom the chorion in the intervals between the tufts. The villi had as a rule a club-shaped form, but some divided into filiform branches. They were highly vascular, and a beautiful extra-villous layer of capillaries was distibuted, as in *Orca*, beneath the free surface of the chorion.

The free surface ofthe uterine mucosa had, as in *Orca*, a delicate reticulated appearance, and was pitted with multitudes of recesses and furrows, which again were subdivided into innumerable crypts. In the polar regions of the cornua and in the corpus uteri the mucosa was more spongy and succulent than in the greatly distended part of the gravid horn, in which the mucosa was obviously more stretched, so that the pits and furrows were almost obliterated, and the crypts opened on the general plane of the mucosa. In their general arrangement, and in the vascularity of their walls, the crypts in the Narwhal resembled so closely the corresponding structures in *Orca*, that I need not give a special description. The layer of cells which lined them was a well-defined cylindrical epithelium, many of the cells of which, however, were so swollen that the breadth almost equalled the length.

Scattered over the surface of the mucosa in the more distended part of the cornu were numerous smooth, depressed, circular or ovoid spots, the largest of which was not more than $\frac{2}{10}$th inch in diameter, though as a rule they were less than $\frac{1}{10}$th inch; so that to the naked eye they were apt to escape observation. Each spot was surrounded by a minute fold of the mucosa, sub-divided into crypts. On an average from twenty-five to thirty of these spots were found in each square inch. They resembled the smooth depressed spots described by myself* and some other anatomists in the uterine mucosa of the gravid pig. On the surface of the chorion adapted to this part of the mucosa, occasional smooth patches from $\frac{1}{10}$th to $\frac{2}{10}$th inch in diameter were seen surrounded by villous tufts which were in apposition with the smooth depressed spots on the mucosa, but they had not the definite stellate form which one sees on the chorion of the pig. The extra-villous layer of capillaries ramified beneath these non-villous spots of the chorion. In the succulent parts of the mucosa the smooth depressed spots could not be seen with the naked eye, but only after a careful search with a pocket lens were they found at the bottom of some of the trenches or pits in the membrane.

From the general resemblance between these spots and those met with in the uterine mucosa of the pig, one was naturally inclined to think that they would have a relation to the mouths of the utricular glands. I proceeded, therefore, to examine the glandular layer of the mucosa. The glands were very numerous, and branched repeatedly. Many of the branches formed short diverticula, others were much longer; sometimes they were tortuous, at others a considerable length of straight gland tube could be seen. In the deeper part of the mucosa the glands lay almost parallel to the surface; but as they approached the crypts, they were directed more obliquely, so as to be frequently divided in vertical sections. The glands were subjacent not only to the crypts, but to the smooth depressed spots, numerous examples of which I carefully examined. In one instance, I saw a tube lined by epithelium lying obliquely beneath the membrane of a spot, and opening near the middle by a distinct orifice bounded by a crescentic fold of the membrane. In

* "Journal of Anatomy and Physiology," Oct. 1875. Also Lectures on the Comparative Anatomy of the Placenta, Edinburgh, 1876.

a second instance, the end of a gland passed from under cover of the surrounding crypts, and then seemed to open by an obliquely directed mouth near the free edge of the spot. But upwards of thirty other spots examined with equal care gave me no evidence of gland mouths opening on them. Hence it would appear that these smooth surfaces on the mucosa are by no means necessarily associated with the mouths of the utricular glands, and one is disposed to conclude that the gland orifices are usually concealed amongst the crypt-like foldings of the mucosa. The very much greater number of the crypts than of the stems of the glands, negatives the idea of the crypts being merely dilatations of the mouths of the glands, so that in the Narwhal, as in *Orca*, the Pig, and Mare, the crypts are to be regarded as interglandular in position and produced by a hypertrophy and folding of the mucosa.

Through the kindness of my friend Dr Allen Thomson, I have had the opportunity of examining a portion of the gravid uterus and chorion of a Narwhal, the fœtus in which measured only $3\frac{1}{4}$ inches long. The free surface of the mucosa was gently undulating and traversed by shallow furrows, but no definite crypts could be seen. The gland-tubes were remarkably numerous, tortuous, and branching. I made a comparative measurement of their size, with that of the glands in my much more developed specimen, and found them to have only one-half the transverse diameter. The gland-stems inclined obliquely to the surface of the mucosa on which their orifices could occasionally be seen. The free surface of the chorion was not villous, but traversed by faint ridges, which without doubt fitted into the shallow furrows of the mucosa. Patches of epithelium-cells could be seen covering the surface of the chorion. It is clear therefore that in the Narwhal, as I have elsewhere described in the pig, the villi do not form on the surface of the chorion, nor the crypts on the surface of the mucosa, until the embryo has reached a stage of development in which its body, though small, has assumed a form which enables its ordinal characters to be recognised.

When I published my memoir on the placentation of *Orca*, I was under the impression that the crypts were lined by a pavement epithelium, and was not disposed to regard the crypts in the mucosa of that cetacean as secreting organs; but a re-examination of the epithelium-cells lining its crypts has convinced me, that they are not a pavement epithelium, in the sense of being squamous cells, but have an intermediate or transitional form between the columnar epithelium and the tesselated epithelium. In the Narwhal, again, the cells are cylindrical, as in so many mammals, so that I believe the Cetacea to offer no exception to the view that these cells are a secreting epithelium, and they doubtless elaborate a secretion for the nourishment of the fœtus. From the fact that the utricular glands had a much greater calibre in my specimen than in the one belonging to Dr Thomson, one may infer that even after the crypts are fully developed the glands still play a part in fœtal nutrition.

The fœtus in my specimen had an almost uniformly purplish-grey colour, but with a patch of yellowish-white on the belly near the anus. The snout was rounded; the fissure of the mouth $1\frac{1}{2}$ inch long; eye-slit $4\frac{1}{2}$ inches behind the snout, and surrounded by a faint circle; ear orifice very minute, 3 inches behind the eye-slit; blow holes above and a little anterior to eye slit. Length of flipper 7 inches, its anterior edge 12 inches behind the snout. Funis was attached to the belly about midway between the anterior and posterior end of the fœtus. A low, but distinct dorsal ridge, the rudiment of a dorsal fin, commenced a little in front of a point midway between tip of snout and end of tail, and extended backwards for between 10 and 11 inches along the middle line of back. It had a lighter greyish tint than the surrounding skin. Breadth of tail was $15\frac{1}{2}$ inches. In a profile view of the fœtus a slight depression in the contour of the top of the head was seen in the region of the blow-holes. The fœtus *in utero* differed therefore very materially in colour from such a half-grown specimen as Dr Fleming described*, in which the upper part of the body was a dusky black, the belly white, and numerous oblong spots extended horizontally along the sides; still more did it differ from old specimens, which have a whitish marbled colour. The presence of a dorsal ridge is also of interest, as the Narwhal is described as without a dorsal fin.

I availed myself of the opportunity of examining the dentition of the fœtus. On May 20th, 1872, I described to the Society the

* Memoirs Wernerian Socy., 1811.

dentition of a fœtal Narwhal $7\frac{1}{4}$ inches long, in which I found two dental papillæ developed in the gum, on each side of the upper jaw, but the early stage of development of the fœtus did not permit me to say whether the anterior or posterior denticle would have been the one to become the maxillary tusk, though I thought it probable that the more anterior would become the tusk.

In this much larger fœtus the superior maxilla was $8\frac{1}{2}$ inches long. At the anterior end of each of these bones were two well marked sockets, one opening immediately behind the other. The anterior socket contained a cylindriform rudimentary tusk. The posterior socket contained an aborted tooth $\frac{1}{2}$ inch long, and $\frac{2}{10}$ inch in its widest diameter. The hinder half of the aborted tooth was attenuated, and had several short irregular processes projecting from it; the anterior half was smooth and rounded. This tooth was inclosed in a distinct sac, formed of fibrous tissue, which, like the sac of the rudimentary tusk, was firmly united to the fibrous tissue of the gum. There can be no doubt, therefore, that I was right in my conjecture that the more anterior dental papilla becomes the tusk of the Narwhal.

3. Observations on the Zodiacal Light. By C. Michie Smith. Communicated by Professor Tait.

While engaged in cable work in the West Indies, I had, during the winter and early spring of 1875, a number of very favourable opportunities of examining the zodiacal light.

Before leaving this country I had, under the advice of Professor Tait, and with a note of recommendation from Professor Jenkin, applied to the Royal Society for the loan of a spectroscope, to make observations with during the voyage; but unfortunately I was unable to obtain one, and so had to content myself with a small pocket-spectroscope. On the outward voyage I did not notice the light at all till we got well to the south, near Cayenne, on the 8th of January; and, owing to the very bad weather we had about that time, I was not able to make any satisfactory observations till we were again somewhat farther north. The general appearance of the zodiacal light has been so often described, more or less faithfully, that I need not attempt any description of it here. I wish, however, to mention one feature with which I was much struck, and which I have never seen remarked on—namely, that, on watching the western sky from sunset onwards, it is impossible to tell when the diffused sunlight ends and the zodiacal light begins till it becomes so dark that the form of the latter can be traced to a considerable altitude, when it is seen to be longer and narrower, and inclined to the vertical at a considerable angle. I am strongly inclined to believe that near the sun it is very much wider than at some distance from it, for I have very good reason to think that what by people generally would be taken as simply the last of the sunset glow is really due to the zodiacal light. This part, of course, can only be seen in places where the twilight is very short. The best time for making observations I found to be about two hours after sunset, when all traces of twilight had certainly disappeared, and consequently all risk of confusion with it was gone.

On January 31st, in lat. 8° N., long. 56° W., the light was very bright, and I made some spectroscopic observations. At two hours after sunset the light was visible for 90° from the horizon, and so bright was it towards the horizon that I was able to get a distinct spectrum. I first opened the slit very wide, when I observed a broad strip of light, nebulous at the extremities, with a distinct reddish tinge at the one end; then, by gradually closing the slit, I obtained a narrower but tolerably pure continuous spectrum, in which I could *distinctly* see reddish, orange, and greenish-blue, and on making comparison with the spectrum of a lamp (placed at the far end of the ship so as not to dazzle my eyes), I estimated that the spectrum extended from the red to past the position occupied by the F line in the solar spectrum. A large number of observations taken on other nights, whenever the circumstances were favourable, entirely confirmed these first observations. On several nights, and especially on February 27th, when off Ponce, in the Island of Puerto Rico, I observed the spectrum from a short time after sunset till long after the last traces of twilight had disappeared, but no change was noticeable after the spectrum had become so faint that the Fraunhofer lines could not be distinguished, except in the brightness of the spectrum, as I was still able to see colour distinctly, but no traces of any bright lines.

On February 26th and 27th I took a number of sextant measurements of the zodiacal light. These are, of course, only approximate, as the light has no definite boundary-lines, but gradually fades off at the borders. The measurements were made by the help of stars, as it was quite impossible to measure the light itself. On the 26th sunset was at about six o'clock, the light was very bright for 30° from the horizon, having at the horizon a breadth of about 25°, and at an altitude of 30° a breadth of about 20°. At 9 P.M. the light could be traced quite round to the eastern horizon, a phenomenon which I observed on several other occasions. At 10 P.M. the light was scarcely, if at all, visible. That this sudden disappearance was not caused by any change in the atmospheric conditions was clearly shown by the undiminished brightness of the stars. On February 27th the breadth at the horizon was 30°, while at an altitude of 30° it was only about 20°. The centre of the band passed a little to the south of the Pleiades. I endeavoured, by means of the sextant, to measure the inclination of the band to the vertical. For this purpose I chose two bright stars near the centre of the band—one at a considerable altitude, the other close to the horizon; I then measured the angle between these and the angle between the upper one and the horizon; these angles were respectively 55° 30′ and 51°, giving the value of 31° 30′ for the inclination of the centre of the band to the vertical. I had unfortunately no access to star-charts, else I could have fixed the direction more accurately; but even these rough observations confirm the ordinary statement that the direction is slightly inclined to the ecliptic.

The spectroscope used was one of Mr Ladd's admirable small direct-vision spectroscopes, with five prisms and a single lens. Behind the slit is a small round hole, through which the light may be made to enter by opening the slit very wide, and which is very convenient for examining monochromatic light. With this instrument it is easy to see a number of the Fraunhofer lines on a tolerably clear moonlight night. And such an instrument, though in some respects inferior to a simple large prism and slit, will, I believe, be found very suitable for work such as that described above, especially when it has to be carried on on board a ship much given to rolling.

4. Note on the Volcanoes of the Hawaiian Islands. By J. W. Nichol, F.R.A.S. Communicated by Professor Tait.

The late Transit of Venus Expedition gave the writer some opportunities of visiting several islands of the Hawaiian Archipelago, some details of which may prove interesting.

They form a group of islands about 10½ hours west longitude from Greenwich, and about 20° to 22° north of the equator, and differ in size from 10 miles long by 6 or 7 broad, to 90 miles by 60, which are about the dimensions of the most easterly and largest, viz., Hawaii (the Owyhee of Cook). The general lie of the islands is from north-west to south-east, those in the east displaying the most recent traces of volcanic activity. In the older or western portion the main mountain ranges run in the same direction as the islands, rising in many places to a height of 3000 to 4000 feet, and having lateral ridges branching off at right angles, with an occasional crater of oval shape thrown up at a distance from them, and evidently of more recent origin.

The putting up of a meridian mark on one of these ridges in the island of Oahu was attended with some difficulty, the narrow space along which one had to ride, sometimes not more than a yard or two wide, with precipitous descents of 500 to 1000 feet on either side, not rendering it comfortable to any one with weak nerves.

The two most easterly islands, viz., Maui and Hawaii, although having their greatest length in the north-east and south-west directions, are composed only of mountains standing singly, and present no appearance of ranges. Maui, indeed, is nothing more than a couple of mountains joined by a very low neck of land, on the top of the eastermost of which (Mount Haleakala), at a height of 11,000 feet, is found one of the largest and most perfect extinct craters in the world, being some 10 miles in diameter, which unfortunately time would not admit of our visiting. The sides of Haleakala are not precipitous, and the general view from the sea is that of a huge hog-backed mountain. Traces of flows of lava so recent as to be quite black, and not covered with vegetation, are also seen coming down from what had been openings in its sides a short distance above the sea-level.

The island of Hawaii, or the most recent and easterly, is composed of four mountains—the Mauna Kea, 14,500; Mauna Loa, 13,800, Huallalei, 8000, and Kohala, about 5000 feet in height, with large valleys of 2000, 3000, to 6000 feet above the sea-level between. The slope of the mountains is usually gentle, and numerous small craters of 100 to 300 feet in height are found distributed on their sides, and also in the intermediate valleys. On the west side precipitous rocks face the sea, with a height of 3000 feet, which have valleys opening seawards that are almost inaccessible from the land side, owing to the precipitous character of their sides. It is on this island that the most recent displays of volcanic activity are seen, the country having been overrun in many places by lava flows, which have left large tracks quite useless for agricultural purposes. Earthquakes are common, and the summit crater of Mauna Loa, 13,800 feet up, is frequently, and that of Killauea (on a level plateau on the side of Mauna Loa, about 3000 feet above the sea) is almost always in a state of activity. The crater of Killauea is on the north-east of Hawaii, about 32 miles from the bay of Hilo, which is the most convenient starting-point for those wishing to visit the volcano. In the ride of 32 miles one has an ascent of some 3000 feet, but the ups and downs are so numerous that one can hardly detect it. During some portion of the way one passes through very dense tropical vegetation, palms, tree ferns, &c., with creepers and ferns clustering around, but for the most part the path lies over lava flows, so recent as to be almost devoid of vegetation, which render it so rugged as to compel one to walk his horse the greater part of the way. When approaching the volcano the visitor is at first struck by the sight of hundreds of steam jets rushing up in all directions, some of which are utilised as vapour baths, by putting a wooden box over them with a hole in the top large enough to admit the neck of the bather. On going some hundred yards further, an immense pit appears, at the further end of which during the day are seen large volumes of smoke, while at night a red flare is visible in the sky, with an occasional piece of white hot lava getting tossed up high enough to be seen above the edge of the inner crater. This large pit or outer crater is of oval shape, and some 3 miles long by $1\frac{1}{4}$ to $1\frac{1}{2}$ mile broad in the widest part. The sides are precipitous, and from 600 to 700 feet in height, being in many places divided into close parallel ridges, showing the height to which the lava had reached before breaking out at a lower level. There were two large sulphur beds at the side of the outer crater, about 2 miles distant from each other, and pieces of native sulphur could be picked up, each with a hole in its centre, showing that the vapour had solidified round the hole from which it had emerged. The floor of the outer crater was composed of black lava, several acres of which were covered over one night by the lava breaking out at a side of the lowermost edge of the outer crater, quite distant from the inner one.

Our first descent was made at night by means of some rustic staircases cut in the sides of the ridges, and assisted by whatever brushwood might be growing on the sides. At last our party were landed on a floor of black lava, all seamed with cracks and contorted into curious shapes, sometimes like a mass of cable ropes mingled together, at others showing large pudding-like excrescences, which are dangerous to walk upon, since they are simply large bubbles with a thin covering, to guard against which each visitor carries a thick stick wherewith to test the ground before him.

After walking for about half an hour on this black lava, and crossing innumerable cracks of from three inches to a foot in width, some of which showed a white line of fire about 6 feet beneath, the gradual ascent to the inner crater was reached. Its position in regard to the general form of the outer crater may be said to be in one of its foci, and its size, by estimation, about a $\frac{1}{2}$ mile long by $\frac{1}{4}$ broad. The increasing glare and smoke now warned us of our proximity to the more active parts, while here and there on the outer side of the inner crater were some bright red streaks, which, on our closer approach, turned out to be red hot lava flowing through holes in the outer side of the inner crater on to the general surface of the outer one.

The ascent of some 70 feet to the top of the inner crater is a gradual one, and considerable detours had to be taken to get round the parts which were being overflowed. The lava of its sides was twisted about and broken up in a most ugly manner, besides being so rotten as to break away in flakes whenever a foot was put upon it. The greatest caution was needed to test the ground before treading on it, and frequent play had to be made with our thick sticks. Tumbles were frequent, from which the writer escaped unharmed by having a thick pair of dogskin gloves on his hands. One of our party, however, a professor from Indiana, managed to fall into a crack up to his middle, and got his hands severely cut and burnt.

The view from the top, however, amply repaid the trouble. Four lakes of molten lava, the largest some 200 yards in length, and of kidney shape, and the others of smaller size, were seen in full activity. In the largest lake seven to eight fountains of white hot lava were playing up at once to a height of 30 to 40 feet, one sometimes stopping and another commencing at a different part of the side of the lake.

The lava in this lake was about 50 feet below the inner edge of the crater, and appeared to be slowly advancing toward the tunnels from which we had seen it issuing on the road up. The lakes were not at the same level, and you might see one brimful and another 60 to 70 yards off at a level of some 30 or 40 feet below it. On another of the lakes, about 50 yards wide, was a single fountain, bursting from a cavern in its side, and throwing lava half-way across its surface, while from the roof and sides of the cavern hung down lava stalactites.

After looking at this for some time, the claims of our injured friend became so strong, as to oblige us to take him back to the crater house, resolved next day to have a more deliberate inspection.

The next day proved wet, but the writer explored his way through the driving mist formed by the rain coming in contact with the heated lava, the only disagreeable incident being his getting to the leeward of a blow-hole, and having to run to get clear of the suffocating sulphur vapours. This blow-hole was about the size of a man's body, and as you went forward to it you heard a gurgling sound beneath. Smoke was coming out in considerable volumes, and on looking in, the sides were seen to enlarge beneath and be at a white heat.

Having at length got seated comfortably upon an upheaved block of lava about 20 feet above the larger lake, and 8 to 10 yards from its side, a new fountain sprung up suddenly from the side of the lake quite close at hand, which immediately forced a retreat to a more respectful distance. About the same number of fountains continued to play up as on the preceding evening, and looked red by day. Daylight, however, drowned out the redness of the lakes as seen by night, and made them appear quite black.

After watching for a considerable time, a red hot crack was seen to start suddenly from one side of the lake to the other, then other cracks in different directions, and first one-half of the lake and then the other was covered with a fresh coating of red hot lava, the former tumbling out of sight as it got shrunk and cracked in cooling.

A curiosity called Pele's * hair is found round the sides of these lakes. This is composed of fine fibres of lava cooled, broken off from the molten liquid while being spouted up in the fountains, carried away by the wind, and lodged in the cracks around.

The summit crater of Mauna Loa, some 15 miles off, and 10,000 feet above Killauea, was in activity about a month previous to our visit to the island, but limited time prevented our seeing it. Some points of curiosity may be noted before ending.

1. The lakes are not at the same level, although quite close to each other.

2. The summit crater of Mauna Loa is 10,000 feet above Killauea, and frequently in violent eruption, while Killauea is comparatively undisturbed.

3. The outer crater of Killauea appeared to act as a receptacle for the lava, which, as soon as it arrived at a sufficient height, and got the assistance of an earthquake, broke through below and covered the country, sometimes running in a broad stream for 25 miles, and leaving an indication of the level which it had reached in form of a new ridge within the lip of the outer crater.

4. The necessity of an earthquake to enable it to break through is shown by the great difference of heights of the lava even within short distances.

5. The fountains were in every case playing round the *edges* of the lakes.

5. New General Formulæ for the Transformation of Infinite Series into continued Fractions. By Thomas Muir, M.A.

* The name of the Hawaiian Fire-Goddess.

6. Laboratory Notes. By Professor Tait.

(*a*) On a Possible Influence of Magnetism on the Absorption of Light, and some correlated subjects.

Professor G. Forbes' paper, read at a late meeting of the Society, and some remarks made upon it by Professor Clerk-Maxwell, have once more recalled to me an experiment which I tried for the first time rather more than twenty years ago, in Queen's College, Belfast. I have since that time tried it again and again, whenever I succeeded in getting improved diamagnetics, a more powerful field of magnetic force, or a more powerful spectroscope. Hitherto it has led to no result, but it cannot yet be said to have been fairly tried. I mention it now because I may thus possibly be enabled to get a medium thoroughly suitable for a proper trial.

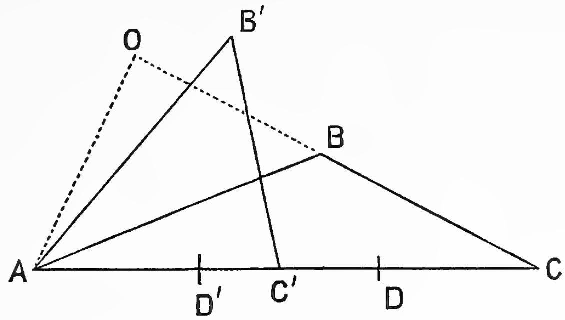
The idea is briefly this,—The explanation of Faraday's rotation of the plane of polarization of light by a transparent diamagnetic requires, as shown by Thomson, molecular rotation of the luminiferous medium. The plane polarized ray is broken up, while in the medium, into its circularly-polarized components, one of which rotates with the ether so as to have its period accelerated, the other against it in a retarded period. Now, suppose the medium to absorb one definite wave-length only, then—if the absorption is not interfered with by the magnetic action—the portion absorbed in one ray will be of a shorter, in the other of a longer, period than if there had been no magnetic force; and thus, what was originally a single dark absorption line might become a double line, the components being less dark than the single one.

Other allied forms of experiment connected with this subject were discussed.

(*b*) On a Mechanism for Integrating the General Linear Differential Equation of the Second Order.

I am anxious to explain to the Society a kinematical device for the solution of the *General Linear Differential Equation of the Second Order* before I become acquainted with the principle of the integrating machine which, I understand, was described last Thursday by our President to the Royal Society.

My arrangement consists of a combination of two equal modifications of Ammsler's Planimeter, ABC, AB′C′, the wheels of which are attached at the joints B, B′. C′ slides along AC, and the length of AC can be altered by turning either of the heads D, D′, of coaxal screws of equal pitch. Now, if we suppose D connected with the



wheel at B, and D′ with that at B′, by means of universal flexure joints (Thomson & Tait's "Natural Philosophy," § 109), it is obvious that the length of AC will depend upon its angular position, and upon the motion of C′ along AC.

Let AB = AB′ = a, BC = B′C′ = b, AC = r, AC′ = r_1, ∠ ABC = ϕ, and let θ denote the position of AC. Then, if the whole turn through an angle $d\theta$, the motion of B perpendicular to CB is the same as if it had rotated about O, where ∠ AOB is a right angle. Hence, if ρ be the radius of the wheel at B, $d\psi$ the angle through which it rotates,

$$\rho\, d\psi = -a \cos\phi\, d\theta = \frac{r^2 - a^2 - b^2}{2b} d\theta .$$

A similar expression holds, of course, for B′. Now, if α be the inclination of the threads of the screws, one right, the other left, handed,

$$dr = \rho\,(d\psi - d\psi^{\prime})\tan\alpha ,$$

$$= \frac{\tan\alpha}{2b}(r^2 - r_1^2)\, d\theta .$$

Now C′ may be made to move along any curve we choose, so that r_1 may be any assigned function of θ. Hence, by introducing the constant factor $\frac{\tan\alpha}{2b}$ for r, we may give the equation the form

$$\frac{dr}{d\theta} = r^2 - \Theta$$

to which the solution of the general linear differential equation of the second order can always be reduced.

(*c*) The Electric Conductivity of Nickel. By C. Michie Smith and J. Gordon MacGregor.

Pure nickel foil, obtained in Paris by Dr Andrews, was cut into a spiral about 20 inches long, and it was on this spiral that all the following experiments were made. During the month of November 1875 a large number of experiments were made as to its thermo-electric properties, and these were found to be almost identical with that of the specimen from observations on which the line was laid down on the "thermo-electric diagram." (*Trans. R.S.E.*, 1872-3.) This line, it will be remembered, is a peculiar one, and is very similar to that of iron, with this difference, that the peculiar changes take place at much lower temperatures in nickel than in iron. Having thus finally determined the position of the line in the thermo-electric diagram, we were anxious to discover whether, like iron, it exhibited other peculiarities about the same temperature, and for this end we made the following experiments on the electric conductivity at different temperatures. The method of observation was as follows :—

To the two ends of the nickel spiral stout copper wires were soldered, and the whole was carefully fastened together in such a way that no two coils of the spiral could touch each other. Side by side with this nickel spiral was placed a similar spiral of soft platinum wire of approximately equal resistance. This platinum was part of a wire the electric conductivity of which had been formerly carefully tested, and had been found to obey very strictly the law of being proportional to the absolute temperature. These spirals were then placed in a large pot of oil, care being taken that they hung quite free from the sides of the pot, and the ends of the thick copper wires were led to the pools of a mercury commutator, so arranged that either the nickel or platinum could be made to form one of the arms of a Wheatstone's bridge, in connection with a very delicate Thomson's dead-beat mirror galvanometer. In making the observations the oil was heated by a powerful Bunsen burner, and constantly stirred. By this means it was found perfectly practicable to keep the oil sensibly at the same temperature during the time necessary to find the resistance of the two wires by the ordinary balance method. That no errors were caused by thermo-electric effects was proved repeatedly during the experiments by completing the circuit without the galvanic cell, when no current was shown on the galvanometer. The results obtained for the nickel entirely agreed with what had been anticipated from the thermo-electric properties. For, when the conductivity is plotted in terms of the temperature, the curve shows a sudden change in direction at a temperature of about 149° C. (300° F.), indicating that there is at that point a sudden change in the rate of alteration of the conductivity with change of temperature. The curve obtained for nickel can be very well represented by two straight lines inclined to each other at an angle of about 9°, while the curve got for the platinum wire is strictly a straight line.

That no part of the effect was due to the conductivity of the oil was amply proved by the following experiment:—

Two pieces of platinum foil, each having a surface of 2·5 square inches, fastened to the ends of copper wires, were plunged in the oil when it was at a temperature of 550° F., and were kept a quarter of an inch apart; the resistance of the oil between them was then measured, and was found to exceed 9 megohms, while the resistance on causing them to touch fell to a small fraction of an ohm.

After a series of experiments had been made with the nickel, the whole spiral was heated to a white heat in the flame of a Bunsen burner, and allowed to cool in the air; another series of experiments was then made on the conductivity, but no change was observed.

The following tables contain the observations for two of the experiments, side by side with the values of the conductivity, calculated on the supposition that the curves are best represented by straight lines—the platinum being represented by a single straight line, while the nickel is represented by a broken line. The calculated and observed values, it will be seen, agree very closely with each other, except where a divergence is to be expected, namely, at the intersection of the two lines (nickel). The equations were taken from the lines obtained by plotting the conductivity in terms of the temperature. R is the resistance in thousandths of an ohm, t the temperature in degrees F.:—

January 14th, 1876.

Temperature Fahrenheit.	Nickel.				Platinum.			
	Formula up to t=272°. R.=·525 t + 131. Resistance = ohms/1000.				Formula R = ·34 t + 176. Resistance = ohms/1000.			
	Observed.	Calculated.	Difference. +	Difference. −	Observed.	Calculated.	Difference. +	Difference. −
53°	159	159	...	...	192	194	...	2
69	168	167	1	...	200	199	1	...
99	183	183	...	...	210	210	...	...
132	200	200	...	...	221	221	...	...
162	215	216	...	1	230	231	...	1
194	233	233	...	...	242	242	...	...
218	247	245	2	...	250	250	...	...
247	265	261	4	...	260	260	...	...
	Formula above t = 272°. R = ·775 t + 64.							
279	285	280	5	...	271	271	...	...
307	305	302	3	...	281	280	1	...
345	331	331	...	...	293	293	...	...
376	354	355	...	1	303	304	...	1
440	401	405	...	4	323	326	...	3

January 18th, 1876.

Temperature Fahrenheit.	Nickel.				Platinum.			
	Formula up to t = 306°. R = ·58 t + 133.				Formula R = ·326 t + 187.			
	Resistance = ohms ÷ 1000.							
	Observed.	Calculated.	Difference. +	Difference. −	Observed.	Calculated.	Difference. +	Difference. −
102°	194	192	2	...	219	220	...	1
136	212	212	...	...	230	231	...	1
169	230	231	...	1	242	242	...	...
229	266	266	...	...	262	262	...	...
301	312	308	4	...	285	285	...	...
	Formula above t = 306°. R = ·775 t + 74.							
363	356	355	1	...	305	305	...	...
407	386	389	...	3	319	320	...	1
447	420	420	...	...	333	333	...	...
502	466	463	3	...	348	351	...	3

An attempt was made to discover whether or not the conductivity curve had another peculiar point corresponding to that in the thermo-electric curve at a high temperature. For this an arrangement was used similar to that employed for the iron wire in the experiments formerly described ("Proc. R. S. E." 1874–75, pp. 629–631). But no results were obtained, owing to the breaking of the nickel ribbon when exposed to the great heat of the white hot cylinder.

The following Gentlemen were elected Fellows of the Society:—

William Skinner, Esq.
J. Ballantyne Hannay, Esq.
Peter Denny, Esq.

Monday, 21st February 1876.

Sir WILLIAM THOMSON, President, in the Chair.

The following communications were read:—

1. On the Structure of the Body-wall in the Spionidæ. By W. C. M'Intosh.

In regard to external form, *Nerine foliosa*, Sars, is generally taken as the type of the family, and therefore it may be selected for structural examination in the first instance. Anteriorly the pointed snout is completed by the intricate interlacing of the muscular fibres beneath specially thickened cuticular and hypodermic layers. As soon as the body-wall assumes a rounded form, a layer of circular and oblique muscular fibres occurs beneath the hypoderm, the majority having the latter (*i.e.*, the oblique) direction. In the centre of the area the œsophagus is suspended by strong muscular bundles (the most conspicuous of which are vertical) passing from the hypodermic basement-layer in the middle line superiorly to be attached to the œsophagal wall. A second series, as they descend to their insertion at the ventral surface, give lateral support to the tube; while a third group interlace in a complex manner, and, with the blood-vessels, fill up the space between the œsophagus and the wall of the body.

Toward the posterior part of the head is found—on the dorsal surface—a slight hypodermic prominence, which indicates the position of the central ganglia of the nervous system; the latter being quite external to all the muscular layers, and covered only by the cuticle and hypoderm. In a line with the first bristles, the layers have assumed a more definite appearance. Beneath the hypoderm is a circular muscular coat, which, however, is somewhat irregular in its arrangement; for, toward the dorsal region, the layer spreads out at each side, and the fibres mix with the oblique muscles of that part, while only a very thin layer stretches across the middle line of the dorsum. Within the former is a more or less developed longitudinal layer—best marked at the ventral aspect. A long oblique muscle extends from the lateral dorsal region on each side to the middle of the body-wall; and an important feature is the situation of the nerve-cord in close proximity to the inferior attachment of this muscle to the hypodermic basement-layer. Various muscular fasciculi, as before, attach the œsophagus to the body-wall, and the bristle-muscles and those of the lateral appendages have made their appearance.

A little behind the foregoing it is noticed that the circular muscular layer is less continuous (though strong inferiorly), and that the longitudinal has been grouped by the other fibres into certain definite bands, the most conspicuous being a double dorsal and two lateral. The former fibres, indeed, have now assumed considerable bulk, a thin circular layer only intervening between them and the hypoderm. The formation of the lateral longitudinal muscles, again, is interesting on account of their homological bearings. From the inferior bristle-tuft, and from the region on each side of it, a strong series of muscular fibres converges toward the side of the œsophagus, and then splits into two bands. The outer bundle is the more powerful, and at the infero-lateral region of the body it bends somewhat sharply outward to be attached to the wall. The fibres thus arch over a chamber on each side for the lodgment of the ventral longitudinal muscle. In ordinary transverse sections they are much stronger than the other, and, moreover, have the nerve-cords at their insertion. The second series slants downward and inward, and is chiefly composed of fibres passing from the dorsal arch by the side of the œsophagus to mingle with the circular fibres at the ventral surface. A thin layer of longitudinal fibres also occurs on the internal aspect of the ventral transverse band (a part of the circular coat).

As we proceed backward, the lateral longitudinal muscles gradually increase in breadth, while the great oblique bands nearly meet in the central line inferiorly. The dorsal longitudinal fibres are grouped in two symmetrical masses, and a strong band passes between the edges of the ventral longitudinal muscles,—the median longitudinal fibres formerly indicated lying immediately within this layer. The nerve-cords have now descended quite to the ventral surface, and have a pale intermediate area.

As soon as the body assumes a transversely-elongated form, the dorsal longitudinal muscles become much extended, and are, besides, intersected by the powerful vertical bands, which sweep from the dorsal basement-layer to the ventral surface, through the lateral longitudinal muscles (now for the most part ventral in position). The oblique muscle on each side is more horizontal, passing from the inferior bristle-bundle to the median line at the ventral surface, and going right through the vertical bands before insertion. The nerve-cords lie close together below the transverse muscle, and a small neural canal exists at the inner and upper border of each. There are still a few longitudinal fibres between the ventral attachments of the oblique muscles. The alimentary canal shows internal circular and external longitudinal fibres.

It is very soon apparent, in proceeding backward, that the vertical muscles descending from the dorsal to the ventral surface do not interdigitate with the great longitudinal muscles throughout their whole extent. They leave, as observed by the lamented M. Claparède, at the external border of each dorsal muscle a considerable mass, which bends downward, and presents in transverse section a distinctly pennate appearance. A similar arrangement occurs at the outer and inner extremities of the ventral longitudinal muscles. Finally, the nerve-cords now have a single and very large intermediate neural canal. The foregoing condition continues with little modification to the tip of the tail; though the dorsal pennate process disappears, the muscle itself being separated from its fellow, and considerably diminished in bulk, while the transverse fibres between it and the hypoderm have greatly increased.*

* The late M. Claparède, in his "Structure des Annél, Sédentaires," p. 15, &c., pl. xv., gives the structure of the hypoderm, and notices the pennate

In *Scolecolepis vulgaris*, Johnst., the body-wall is similarly constructed. Anteriorly the central ganglia of the nervous system lie outside the muscles on the dorsum, and the cords rapidly pass downward to the inferior attachments of the oblique muscles. In this region there is also a dense mass of longitudinal muscles. As soon as the oral aperture is completed posteriorly by the frilled hypoderm, the following arrangement occurs:—Within the hypoderm is an irregular circular coat, the most conspicuous part being a broad belt, which bounds the mouth at the ventral border, and stretches between the great longitudinal muscular masses on each side. Superiorly a short but distinct band also appears under the central hypodermic elevation. A fasciculated longitudinal muscle (dorsal) lies below the latter on each side, its inferior surface being attached to a chitinous sinuous band, which forms a space by its upward curve from a raphe. A somewhat triangular interval occurs, moreover, between the muscles in the median line. The form of this chitinous arch is maintained by strong transverse fibres, which curve from raphe to raphe. At the latter, on each side, there is almost a rosette of muscular fibres, the chief fasciculi being directed downward and outward in transverse section. Outside the foregoing dorsally are various oblique bands, the superior stretching from the dorsum downward and outward to the lateral hypoderm, while the lateral pass downward and inward. The chitinous arch gradually disappears as the dorsal muscle becomes fully developed. Behind the preceding region the arrangement consists, as in *N. foliosa*, of a double dorsal and two lateral longitudinal muscles, with the vertical and oblique bands, the latter passing through the former near the ventral attachment. There is also a very strong transverse ventral muscle, with a series of longitudinal fibres internally. Each nerve-cord in transverse section presents a distinct though small neural canal. The hypoderm, as well as the muscles, seems to be more largely developed than in *N. foliosa*, a feature corresponding with the increased size of the nerves.

After the dorsal muscles have expanded into a broad layer, the same interlacing with the vertical bands occurs, but the pennate

muscles and the arrangement of the nerve-cords of this form, but the foregoing observations do not interfere with his remarks.

arrangement formerly noticed does not appear in this form. The two neural canals soon increase in size, and approach each other in the middle line. With the exception of the great development of the ventral longitudinal muscles posteriorly, little further change takes place in the structure of the body-wall.

The situation of the central ganglia in *Scolecolepis cirrata*, Sars., corresponds with the preceding, and the nerve-cords follow the progress of the oblique muscles toward the ventral surface, each trunk having a small neural canal. When the body wall is completely formed (for instance about $\frac{1}{3}$ in. from the snout), the great size of the longitudinal muscles is conspicuous. The dorsal form a thick superior arch, and proceed a considerable distance down the lateral wall; while the ventral muscles constitute two great curved masses in transverse section, the inner border of each being so carried upward that a deep ventral sulcus is formed for the nerve-trunks and their hypodermic external investment. For the same reason the strong oblique muscles are rendered nearly horizontal. The rounded firm nature of the alimentary canal gives little scope for the development of vertical fibres.

The structure of the body-wall in *Spio*, and the position of the ganglia and nerve-trunks correspond with the foregoing in general features. The same may be said of the rarer *Prionospio*, which has two neural canals inferiorly.

In *Polydora ciliata*, Johnst., the body-wall anteriorly is characterised by the great development and bifid nature of the median ridge, which is flanked on each side by a prominent process of the hypoderm. In transverse section, the snout a little behind the tip presents, on each side of the dorsal process, a large rounded lobe, which projects downward to the oral aperture. Externally, the lobe is composed of a thick layer of hypoderm, having internally a series of circular fibres, which come from the transverse dorsal arch in the form of a loop on each side. The fibres pass downward within the hypoderm, curve inward ventrally, and then proceed upward over the œsophagus to the point of commencement. A well-marked series of longitudinal fibres lines the outer division of the loop, and afterwards merges into the ventral longitudinal muscle of the side. The dorsal arch of transverse fibres cuts off the hypodermic process, containing the nerve-ganglia, and in the cavity which forms therein is superiorly a small group of longitudinal fibres.

Behind the foregoing a transverse dorsal layer is found beneath the central and now solid hypodermic process, next the dorsal longitudinal muscles (the fasciculi of which, in the middle line, are directed downward and outward, while the outer are directed downward and inward); then a kind of X shaped process occurs in the centre, the legs of the X being prolonged horizontally, so that the whole resembles a figure of ∞, the two spaces containing muscular fasciculi. The lateral and oblique muscles are largely developed, the latter having the nerve-cord on each side below its ventral attachment.

The most interesting point in this form, perhaps, is the structure of the fifth body-segment, which bears the remarkable hooks characteristic of the genus, besides peculiar bristles with spear-shaped heads, and a minute fascicle of the ordinary structure. Immediately in front of the hooks, the body in transverse section shows externally a circular coat, which is thin at some parts, but greatly developed at others. Dorsally a very powerful series of fibres spread outward from the middle line on each side—some becoming continuous with the circular coat, others passing obliquely outward and downward to the superior bristle-bundle. Inferiorly a strong transverse band lies over the nerve-trunks, and forms an external investment to the ventral longitudinal muscles. The oblique muscle comes from the lower bristle-bundle, and joins the former over the nerve-trunk, after piercing the vertical bands. The superior longitudinal muscle forms a great mass on each side, and it interdigitates with the fibres of the vertical muscle. The latter is greatly developed, especially at its inner border, next the œsophagus. The same (vertical) fibres pierce the ventral longitudinal muscle in the compartment formed for it by the circular and oblique bands. The size of the ventral is less than that of the dorsal longitudinal muscles. A somewhat strong group of longitudinal fibres lies within the ventral transverse band. Finally, each fascicle of the ordinary bristles has a V shaped series of fibres extending from the base of the tuft to the lateral wall, and interdigitating with those from the transverse and other muscles of the region.

As soon as the powerful hooks of the fifth segment appear, the entire area—from the alimentary canal to the body wall—is occupied by their muscular apparatus. This consists of a dense series of fibres, which slant from the matrix of the bristles superiorly upward and outward to decussate with the fibres at the upper and outer angle of the body-wall. A still stronger series of fibres occur in the inferior division; the inner are nearly vertical, the rest incline downward and outward. It would appear, therefore, that this powerful muscular mass chiefly acts on the hooks, so as to bring their curved points against the wall of the tube or tunnel. The strong inferior fibres likewise gain additional purchase by passing through the ventral longitudinal layer to be attached to the basement-tissue of the hypoderm. In this region the nerve-cords form two almond-shaped bodies in transverse section in the ventral hypoderm, and they are separated by a distinct interval.

Posteriorly the nerve-cords still remain separate, and a large neural canal lies between them. A well-marked pennate process of the ventral longitudinal muscle occurs at its inner (median) edge.

The foregoing forms, in conclusion, were compared with the structure of *Mœa mirabilis*, Johnst., an aberrant member of the Spionidæ.

2. Note on Circular Crystals. By E. W. Dallas.

At long intervals notices of circular crystals have appeared before this Society. In 1853 Sir David Brewster read a paper on the subject, which followed one by Mr Fox Talbot in 1836, and which again had been preceded by one from Sir David Brewster about twenty years before. It is not easy to account for these long intervals, unless they may be attributed to difficulty and uncertainty in manipulation, for except in very few instances the crystals observed by Sir David Brewster are of microscopic size, and, he remarks, require the perfection of optical appliances for their observation, and naturally so when crystals of the 200th of an inch in diameter are looked upon as of respectable size.

Some time ago, being occupied with the subject, I found that by impeding crystallisation by means of gum arabic, circular crystals were formed of a greater size.* This took place with certain salts,

* Perfect crystals were exhibited up to two inches in diameter.

but not with all that were tried. Among the successful instances may be mentioned sulphate of copper, binacetate of lead, muriate of morphia, and other similar salts, which afford beautiful crystals, and are very easily manipulated. The method of proceeding is similar in all; for instance, to a solution of sulphate of copper, let gum arabic or common dextrine be slowly added until it pours oily, then tried, and more gum or more salt added until the result is satisfactory. No precise general rule can be given, as each salt will be found different in the quantity of gum required. The solution is then to be spread thinly over a plate of glass, and dried rapidly before a clear fire, or, if the plate is small, it may be dried over a gas-burner. Upon cooling, the plate, if left to itself, soon shows numerous small specks, which will gradually develope themselves into circular crystals. The process is hastened and a better result obtained by breathing on the plate, when, after a short time, they may be observed to start out very beautifully. These crystals while growing are extremely sensitive, any variation in the moisture applied for their formation resulting in the production of rings.

The growth of the crystals once begun is extremely regular. When the centre is of inappreciable size they are circular, and they proceed onwards in that form until stopped by other crystals, or until the whole vacant space is occupied by them. Should the origin have a definite shape, then that is carried on by the crystals arranging themselves always perpendicularly to the outline of that origin, while, should it be a straight line, they form beautiful fringes perpendicular to the line, and terminate at each extremity in semicircles.

The centres round which the crystals arrange themselves may be either some foreign body in the film, or be determined by some molecular arrangement of the salt at a particular point in the film. They seem to originate spontaneously, and subject to no apparent rule, for foreign particles, and even minute crystals, will not always determine centres; in fact a film may be full of little crystals of the salt, and to very few of them can the circular arrangement be traced.

The crystals present themselves under two aspects in all the salts that I have examined,—a true and an abnormal form. I designate the true form as that in which the crystallisation proceeds by the formation of spicular crystals radiating from a centre and in optical contact, while in the other many different forms may be observed, in which the component crystals are more or less of a laminate structure, often presenting most beautiful appearances. I have made this distinction, having observed that in some cases at least the true crystals are permanent, while the laminate are not so under like conditions, but change into the true form by time, or may become altogether disintegrated in a damp atmosphere into a confused mass, having few optical properties. The crystals also possess distinctive optical properties.

The production of either of these classes of crystals appears to depend on two conditions, namely, on the thickness of the film, and on the amount of moisture applied in their production,—a thin film and a moderate supply of watery vapour inducing the true form, while a thick film and an increased quantity so alter the structure that at last, although the crystallisation may proceed from a centre, the circular character is entirely lost.

Spherical crystallisation, to which the circular is to be referred, is very frequent in mineral substances. It may be seen also in the well-known experiment of the rapid crystallisation of a supersaturated solution of acetate of soda, by the introduction of a centre round which the salt forms a spherical mass, and the surface, when the action has ended, presents all the appearance of a circular crystal.

However difficult it may be to account for the origin of crystals, their growth in a circular form, when once the centre is determined in a film, is very obvious in those cases where they are produced. In the preparation of the film, not only is the superfluous solvent rapidly evaporated by heat, but a considerable part of the water of crystallisation is also driven off, and there is left on the glass plate a film of an amorphous substance, which, either by attracting moisture spontaneously from the atmosphere, or by having it added, allows the salt, whatever it may be, to resume its crystalline form. That this is the case may be seen from the fact that the crystallisation will take place, and that in a circular form, if the drying of a plate is stopped just at that point when there is sufficient water left to enable the crystals to form when the plate is cooled. In this case their formation is a repetition of the acetate of soda experiment already alluded to. A plate may sometimes also be dried and crystallised, and on being again exposed to heat the crystals will disappear, and the plate may be re-crystallised, but not so well as at first, and not from the same centres. This is the case with the binacetate of lead.

There is one remarkable form of these crystals which is of frequent occurrence, and which Sir D. Brewster seems to have observed only in mannite. The form looks very extraordinary, a properly prepared plate presenting the appearance of being covered with paraboloids. These are simply circular crystals which have been formed in the film while the general crystallisation has proceeded from one side, and is caused by the crystals overtaking one another in their onward progress in one direction. Taking this case in its simplest form by supposing equal rates of crystallisation proceeding from the edge of a plate and from a centre near the edge, their line of contact will necessarily be a parabola, for it is evident the edge is a directrix and the centre the focus of such a figure, but this will very seldom occur, since the growths are not only generally unequal, but also vary in themselves.

The question may be put whether gum arabic is the substance best suited for these experiments. I have not tried many, and with those that I have experimented upon the results were not so satisfactory. That other agents may be employed, according to the salts or other substances to be treated, is certain; for example, collodion, as may be seen in a coated photographic plate that has been allowed to dry after excitation in the nitrate of silver bath, when circular crystals of iodo-nitrate of silver are often produced. This salt, be it observed, although produced through the agency of water, being at once decomposed by that element.

Having confined my observations mainly to a very small number of salts, it would be premature to offer any general conclusion on the structure of their circular crystals or on their optical properties; besides, from the great interest Professor Tait has shown in the subject, I am in great hopes that he may be induced to make some investigation in it. I shall only mention one point that I have observed in the effect of some crystals on the black cross, something in their structure producing a more or less spiral arrangement of the arms in a horizontal direction, while on one occasion a vertical arrangement was observed, in which the arms seemed to be raised one above the other like four quadrant steps. This effect I have only seen once in a crystal of sulphate of magnesia, and have not been able to reproduce anything of the kind, but the crystallisation of that salt is so varied and irregular that many things may pass unobserved.

The only other point I shall lay before the Society is, that I have succeeded in producing a crystallisation very similar to that of frost-pictures on a window pane, and I hope to be able to make the imitation more perfect. For this purpose I have employed the sulphate of copper and magnesia,—a salt that crystallises under the rhombohedral system, the same with that of ice. This salt crystallises in the films from centres in a most remarkable manner in four different modes, viz., the true circular, the laminate, a branched or dendritic form, and another that I hardly know how to designate, unless it may be called the ostrich plume form. All these different forms may be observed on the plates, either simply or in combination, and produce most varied and singularly beautiful effects.

3. Preliminary Note on the Flame produced by putting Common Salt into a fire. By C. M. Smith, Esq. (Communicated by Professor Tait).

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. IX. 1875–76. No. 95.

NINETY-THIRD SESSION.

Monday, 6th March 1876.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read :—

1. The Annual Periods of Thunder (with Lightning), Lightning (only), Hail, and Snow, at Oxford. By Mr Buchan.

During the twenty-one years ending 1873, the maximum period of thunder with or without lightning, at Oxford, extended from about 9th April to the end of October, the middle of the period being the first week of July; the three highest days taken consecutively being those immediately preceding the summer solstice. During the five months from November to March, only thirteen cases had occurred during the period. Lightning, on the other hand, had its maximum period from May to November,—particularly during August, September, and October. The maximum period of *hail* was during the first six months of the year; whereas, during the second half of the year, very few cases occurred. The snow period extended from the middle of October to the middle of May,—most falling from December to March,—the absolutely highest month being March.

Thus, *thunder with lightning*, at Oxford, closely follows the sun, the middle of the period being only about ten days after the summer solstice; *lightning* (only) has its maximum period during that time of the year when the humidity of the air is at its maximum; *hail* is most frequent during that period of the year when the temperature is rising, or when the vertical layers of the atmosphere is in most unstable equilibrium; and *snow* during the coldest months of the year, with this striking peculiarity, that the maximum period is not in the depth of winter, but in March, in the end of winter; immediately after which the curve abruptly falls.

The intimate connection of the thunderstorm with summer rainfall, and the important bearing of the whole four curves on climatology, was referred to.

2. Note on the Origin of Thunderstorms. By Prof. Tait.

This Note does not refer so much to those great thunderstorms which extend over hundreds of miles in each direction, as to those small local storms which are often seen of from two to five or six miles only in diameter.

It refers particularly to those which are seen, in summer and autumn, to pass down the Tay valley. They almost invariably come from the westward, and I am told each is almost always accompanied by a storm of similar dimensions passing eastwards down the valley of the Forth. So far as I can ascertain, they seem both to commence almost abruptly somewhere in the district about Ben Ledi and Ben Lomond.

Seen from St Andrews, which they frequently pass at a few miles distance to the northward, they usually appear to be in a state of rotation about a vertical axis. It is not very easy to judge of the relative distances of the various clouds, so as to ascertain the *sense* of the rotation; but, in one case which I observed carefully last autumn, the rotation appeared to be in the *positive* direction,—*i.e.*, opposite to that of the hands of a watch whose face is turned upwards.

If this be generally the case, and if it should be found that the direction of rotation of the companion storm in the Forth valley is *negative*, it would seem that their common origin may be explained on the following very simple hypothesis, which has the additional recommendation of easily accounting for certain other singular phenomena.

It is known from balloon ascents that, in general, the atmosphere is arranged in horizontal strata of considerable depth or thickness, alternately moist and dry,—temperature diminishing steadily with increase of height in the moist, and remaining nearly constant throughout the dry, strata. These strata have usually horizontal velocities, differing (sometimes considerably) both in magnitude and direction. Thus near the common boundary of two such strata, fluid friction will in general tend to produce vortex motion,—the vortex columns being at first nearly horizontal, with their ends at the boundary, which is a surface of discontinuity.

A complete investigation of the possible circumstances would show *four* quite different cases:—

Vortex formed in $\left\{\begin{matrix}\text{dry}\\ \text{moist}\end{matrix}\right\}$ air, with its ends turning $\left\{\begin{matrix}\text{down}\\ \text{up}\end{matrix}\right\}$ into a stratum of $\left\{\begin{matrix}\text{moist}\\ \text{dry}\end{matrix}\right\}$ air.

The half vortex-ring thus formed tends, so far as it can, to become semicircular. It may thus extend downwards to the earth or upwards into the higher regions of the atmosphere.

If it extend downwards nearly to the earth, the lower portion will soon be destroyed by friction, and we shall have a couple of vertical vortex columns, with their ends respectively in the surface of discontinuity, and on the ground. They will of course rotate in opposite directions about the vertical, and their mutual influence will tend to cause them to progress in directions parallel to one another, the motion of each being in the same direction as that of the rotatory motion of the side which it at the moment turns to the other. This is exactly the presumed case of the little storms in the Tay and Forth valleys above referred to; the south side of the Tay column (that turned towards the Forth), moving eastward about the axis, while the axis itself moves to the east.

This theory is evidently capable of at once explaining the apparently *sudden* occurrence of such storms (of which waterspouts must be looked upon as small but quickly rotating examples), when the lower atmosphere has for hours been in a dead calm.

The disturbance has, in fact, its origin *above* the lower stratum, and works its way downwards into it.

It is also competent to explain the production of similar rotating storms in the higher regions of the atmosphere—many miles above the earth's surface—and thus to account for that by no means small number of cases of so-called "summer-lightning," which obviously cannot be explained by the occurrence of an ordinary thunderstorm at such a distance as to be below the spectator's horizon.

I have already explained to the Society that a possible source of at least a large part of the electric charge of a thunder-cloud is the contact-electricity of water-vapour and air. Thus while the precipitation of the vapour develops heat, the water particles precipitated are strongly electrified. And the aggregation into one of a number of equal little drops all charged to the same potential may increase the potential in any ratio whatever. Thus the charge on each drop in a large cloud may become so great that the electricity is driven entirely to the particles at its surface. This is supplementary to, and does not interfere with, Sir W. Thomson's explanation of the process by which the vapour is condensed.

It is possible that taking place in greatly larger spaces of air, but to a much smaller extent in each cubic foot, this sudden production, and as sudden scattering in all directions, of considerable quantities of electricity, may account for some of the main phenomena of the Aurora.

3. An Application of Professor James Thomson's Integrator to harmonic Analyses of Meteorological, Tidal, and other Phenomena, and to the Integration of Differential Equations. By Sir W. Thomson.

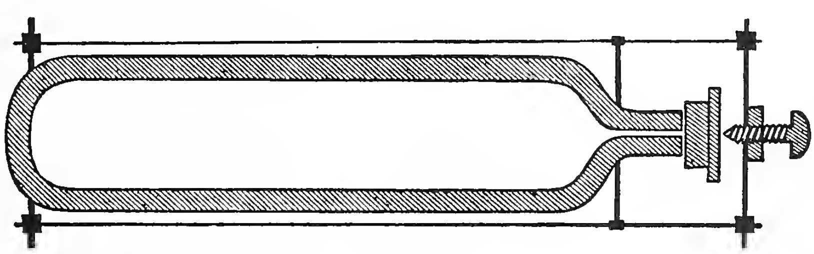
A first rough Model of Professor J. Thomson's Integrator was shown.

4. Note on the Thermo-Electric Position of Cobalt. By Professor Tait.

5. On a Glass Digester in which to Heat Substances under Pressure. By Dr E. A. Letts.

The objections to the use of sealed tubes are known to every practical chemist, and are a serious drawback to their employment. The chief of these are the time expended in the manufacture of the tubes, the amount of skill in glass-blowing required, the danger experienced in opening them, and above all, the fact that only a small quantity of material can be heated at one operation. Moreover, the same tube can seldom be used for more than three or four experiments, as each time it is sealed up its neck must be drawn out, and its length thus considerably decreased. These disadvantages were especially felt by me whilst preparing bromacetic acid, which was required in considerable quantities, and where as many as half a dozen tubes of bromine and acetic acid had to be heated before 100 grammes of the acid could be obtained. To obviate these objections I have had an apparatus constructed, which consists of a cylinder of glass, the walls of which are about half an inch thick. Its length is fifteen inches, its external diameter three inches, and its capacity about 600 cubic centimetres. At one end it is drawn out to a tube, whose aperture is only about one-sixth of an inch in diameter, though its walls are as thick as the rest of the apparatus. Originally this tube was provided with a stopcock, but at Professor Brown's suggestion, I have substituted a glass plate, which is ground flat, and accurately adapted to the top of the tube.

In order to keep the glass plate pressing on the tube the whole apparatus is placed in a frame, consisting of three brass wires arranged symmetrically around the cylinder, and attached by means of nuts, below, to a brass ring, and above, to a brass plate, through which latter a screw passes, which, when turned, presses on a brass plate placed on the glass cap.



As any experiments with such an apparatus would be attended with danger, were it necessary to be in its neighbourhood, it occurred to me that an automatic arrangement might be employed to give notice that the temperature had been reached to which it was intended to subject the digester.

For this purpose I employed a thermometer with a somewhat wide tube and large bulb. A platinum wire is sealed into the bulb, and touches the mercury, whilst a brass wire passes down the tube, and is held in position by a binding screw. The two wires are connected with an electric bell, the brass wire being so adjusted, that when a particular temperature is reached the mercury touches its end, and thus completes the circuit, and causes the bell to ring.

In order to test the digester, about 200 grammes of a mixture of two-parts bromine and three of acetic acid was placed in it, and after fixing it in its frame, the whole apparatus was immersed in an oil bath and heated to 150° C., the temperature at which reaction in this case takes place. The experiment was made in a cellar, and the bell placed in a room some distance off. The gas to heat the oil bath was led by a tube from another cellar, so that it could be regulated without going near the digester. In about an hour and a half the bell rung, and thereupon the gas was shut off; and on examining the digester next day, it was found that the reaction had taken place, and that only twelve grammes of product had been lost—a very inconsiderable quantity.

As the action of bromine on acetic acid is very sudden, and accompanied by the disengagement of a large volume of hydrobromic acid, the apparatus may be considered to have undergone a very severe test, and that its efficacy for all ordinary purposes is established.

Should the digester come into general use, it will certainly save chemists much time and labour.

The following Gentlemen were elected Fellows of the Society :—

Rev. Francis Le Grix White, M.A.
James Duncan, Esq., Benmore.
Rev. Norman Macleod.
J. S. Fleming, Esq.
James Douglas H. Dickson, M.A. Glasg., B.A. Camb.

Monday, 20*th March* 1876.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. On the Connection between Cohesion, Elasticity, Dilatation, and Temperature. By Professor George Forbes.

(*Abstract.*)

At various times there have arisen supporters of one or other of two extreme hypothesis concerning the nature of what we define as force. These are the hypothesis of "action at a distance" and of "no action at a distance."

According to the latter hypothesis, the centre of gravity of no body, however large or however small, can be moved from a position of rest, nor can its motion be altered in direction or amount, except by direct collision with another portion of matter.

Starting from this supposition as a basis of argument, and without assuming anything further as to the manner in which the different physical forces are caused by collisions, it is possible to arrive at some very general theorems; and from these theorems conclusions may be drawn as the nature and connection of some of the physical forces, which are necessarily true if the hypothesis of no action at a distance be true.

The principal result of these theorems is the following:—Let a rod be chosen of any substance whose cohesion and elasticity do not vary enormously with the temperature. Let α be its expansion, in terms of its length, when the temperature is raised 1°. Let β be the compression of the same rod, in terms of its length, when a unit weight is supported at its summit. Let c be the number of these units which, when suspended by the rod, suffice to break it by sudden rupture. Let θ be the absolute temperature at which all these experiments are made. Then the theory leads us to the conclusion that

$$\alpha = \frac{\beta c}{\theta}.$$

Only a few experiments have been made by which we can test

this law. But the following values are the most accurate, and tend to prove the truth of the law. The apparent discordance in the case of iron is in part due to the variations in the qualities of that metal in different specimens.

	$\frac{\beta c}{\theta}$	a
Gold, ...	·00001484	·00001358
Silver, ...	·00001796	·00001809
Copper, ...	·00001511	·00001481
Platinum, . . .	·00001006	·00000851
Iron, ...	·00001573	·00001220

In calculating this table, the values of c from the experiments of M. Wersheim are used; those of a from the experiments of Mr Mathiessen (except iron); those of β from the experiments quoted by Prof. Balfour Stewart in his Text-Book; and the assumed temperature is 18° C., or 283° absolute temperature.

2. Notice of the Completion of the Works designed by Sir Charles A. Hartley, F.R.S.E., for the Improvement of the Danube. By David Stevenson, Esq., V.P.R.S.E.

In 1868 I presented to the Society, on behalf of Sir Charles A. Hartley, a memoir published by the European Commission of the Danube, on the improvement of that river, and at the same time gave a notice of the works designed by Sir Charles Hartley for effecting that important object. These works have now been completed, and Sir Charles Hartley has again asked me to present to the Society a second memoir published by the Commission, which brings the history of the works constructed under their charge down to the time of their completion in 1873.

In supplement of the notice formerly communicated, which referred to a work in progress, it may not be uninteresting, now that the work is completed, to state briefly what has been effected by this most important and successful example of hydraulic engineering.

The engineering problem to be solved by the European Commission was the removal of the bar which obstructed the Sulina mouth of the Danube, which, in 1856, had a varying depth of channel never exceeding 11 feet. The design of Sir Charles Hartley—the engineer to the Commission—consisted in piers so constructed as to confine the current of the river in its passage into the Black Sea. At the date of my last notice the north pier had been extended to the length of 4640 feet, and the south pier to 3000 feet, and a maximum depth of $17\frac{1}{2}$ feet instead of 11 feet had been obtained. I, however, suggested in that notice, that as the Danube must continue to bring down an enormous mass of detritus, so in course of time the works which had proved so successful must be extended; and it appears that this has been found necessary, as the south breakwater, completed in 1871, has been extended to 3457 feet in length, and even with this additional length it is, I think, not improbable that in the course of time still farther extension may be required, for the Sulina mouth of the Danube will still discharge the same amount of water, bearing with it the same amount of alluvial matter, estimated in high floods at about 70,000 tons in twenty-four hours, the deposit of which at the extremity of the piers will still have a tendency, though in deeper water, to form a bar.

The works have, however, proved most successful, and reflect the highest credit on Sir Charles Hartley, by whom they were designed and executed, and the following is a summary of the results that have been obtained.

The total length of piers executed is 8789 feet, at a cost of L.185,352, being L.21 per lineal foot, in an average depth of 14 feet at low water. The navigable depth of the channel over the bar has been increased from 11 feet in 1856, to 20 feet in 1873. In 1853, 2490 vessels, of 339,457 aggregate tonnage, left the port; in 1869 there were 2881 vessels, with a tonnage of 676,960. Thus, while the number of vessels increased only at the rate of 16 per cent., the tonnage, due to the greater draught, had been increased at the rate of 50 per cent., a good practical proof of the value of the improvements. The number of shipwrecks at the mouth of the Danube has also been greatly diminished.

Monday, 3d April 1876.

Sir WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. Chapters on the Mineralogy of Scotland. By Professor Heddle. Chapter I.—On the Rhombohedral Carbonates. Communicated by Professor Tait.

Professor Heddle read a paper on the "Rhombohedral Carbonates occurring in Scotland," the first of a series of Chapters intended to embrace the analytical results of an investigation of all unknown or insufficiently determined Scottish species.

In this paper many analyses of the carbonates were submitted; and the pseudomorphic changes taking place in these were referred to in a special manner.

2. On Thermo-Dynamic Motivity. By Sir W. Thomson.

3. On the Vortex Theory of Gases, of the Condensation of Gases on Solids, and of the Continuity between the Gaseous and Liquid State of Matter. By Sir W. Thomson.

4. On two new Laboratory Apparatus. By William Dittmar.

The object of this communication is to submit to the notice of the Society two little inventions of mine, which, whatever may be the degree of originality which they can claim, will, I venture to hope, prove useful additions to the catalogue of chemical-laboratory appliances. The one is a new form of the precision balance, which pretends to execute *exact* weighings with a hitherto unattained degree of rapidity; the other is a contrivance for maintaining a constant pressure in a supply of gas, and thus making it possible, with comparative facility, to keep, say an air-bath, for any length of time, at a constant temperature.

The new *balance* differs from the instrument in its customary form only in two points, of which the more important is a modification of the centre of gravity "bob" arrangement, which enables one, at a moment's notice, to shift the centre of gravity of the instrument from a certain definite position, I., to a certain other (higher) position, II., matters being arranged so that in passing from I. to II., the sensibility, *i.e.* the deviation, corresponding to an overweight of, say 1 milligramme, is increased in an exactly pre-determined ratio, such as of 1 : 10, for instance. For this purpose the "bob" is made very light, so that the distance through which it has to travel in order to effect the desired change of sensibility is not too small, and, instead of to a screw as usual, is fixed by *mere friction* to a vertical triangular steel rod forming part of the needle. The other new feature in the balance is, that the rider-principle, besides being discounted in a slightly different manner from the customary one, is extended to the determination of differences of weight up to 100 (instead of 10) milligrammes.

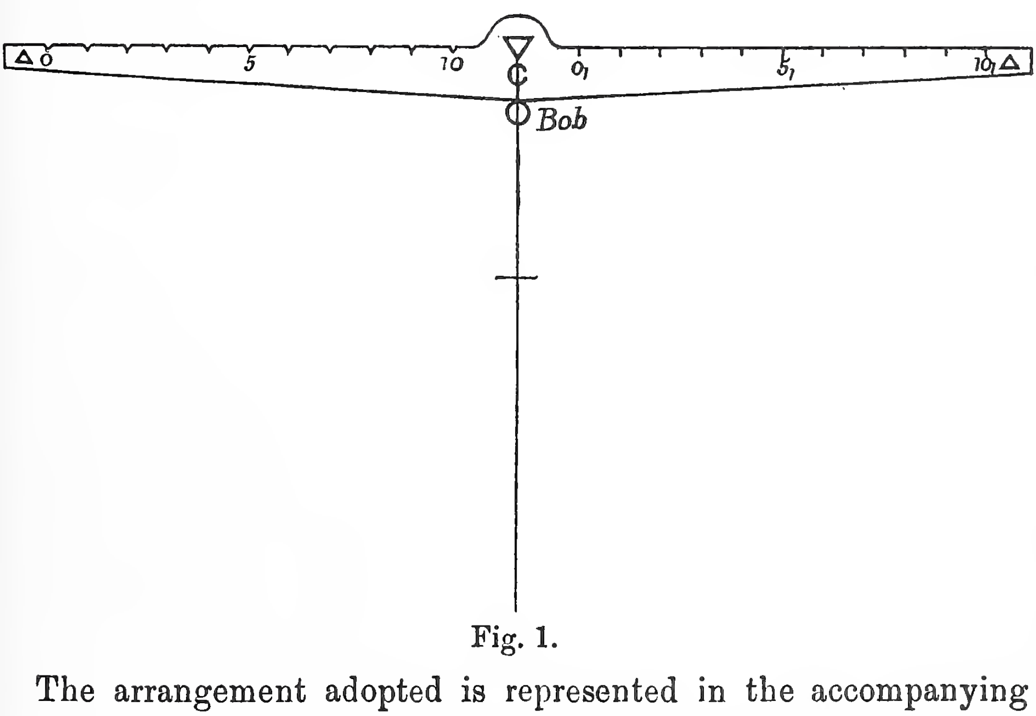


Fig. 1.

The arrangement adopted is represented in the accompanying sketch, for the interpretation of which it is only necessary to say that C (10) and (10) O are equal to C $(0)_1$ and $(O)_1$ $(10)_1$ respectively, and that both O (10) and O_1 $(10)_1$ are each divided into 10 equal parts, the former by *notches* filed into the beam, the latter by *marks;* and to add, that there are two riders, one weighing p centigrammes for the left arm, and another weighing p milligrammes for the right arm, the balance being adjusted so that, when both riders are at their zero-points, it is in equilibrium, and p being chosen so, that, supposing the large rider to be shifted to the n mark, and the small one to the m mark, this virtually amounts to the addition of $10n+m$ milligrammes to the charge in the right pan.

There is no need of my explaining how the balance is meant to be used; I will rather avail myself of this opportunity for drawing the attention of readers interested in the subject to a few inferences from the theory of the balance, which, obvious as they are, have hitherto not been sufficiently appreciated by either the authors of our physical handbooks or by practical balance-makers.

I. Given a balance in which everything is constant except the distance s of the centre of gravity of the empty instrument from the axis of rotation, and it is easily shown that (for a constant charge) the deviation α of the needle for a given over-weight Δ, and consequently the "sensibility" $a = \frac{\alpha}{\Delta}$, is the greater the less s. This, of course, is duly stated by all authors; but what is always forgotten to be pointed out are two things, viz.—1st, That the "*sensibility*" has nothing to do with the *inherent precision* of the instrument; and 2dly, That supposing the sensibility to be increased, all the other good qualities of the balance get *less;* we diminish the rate of vibration (this rate being proportional to $\sqrt{\frac{1}{a}}$); we diminish the range of differences of weight determinable by the method of vibration; we diminish the relative constancy (in opposition to variations in the charge) of the sensibility and the time of vibration. Considering now,—

II. The case of a *balance to be constructed*, the arm-length l and weight w of the empty beam also become variables, related to each other, according to some equation like $w =$ const. l, and (assuming each of the pans to bear a certain medium charge p) we have

$$\frac{t}{\sqrt{a}} = \text{const. } \sqrt{l}\ \sqrt{\text{const.} + \text{const. } l},$$

i.e., by diminishing l we can increase the sensibility without diminishing the rate of vibration (or *vice versâ*); but the other disadvantages mentioned under I. must be taken into the bargain, and, besides, the inherent precision of the balance gets less.* To pass to an example: What we gain by substituting a 7-inch for a 14-inch beam is that, for the *most convenient t*, the sensibility becomes 2 to 4 times greater; but this advantage is secured without expense in good qualities by placing before the graduated limb a lens magnifying the excursions of the needle into 2 to 4 times their natural size. This is the theory of the "*short beams*," which have lately come so much into fashion.

To come back to my own balance, I must not forget to thank Messrs Becker Sons of Rotterdam for the readiness with which they have, at their own risk, tried to realise my ideas in an actual instrument, which, by the way, is now being exhibited at South Kensington. To increase the usefulness of the instrument, I have caused Messrs Becker to add to it a glass plunger, which is adjusted so that it displaces exactly 10 grammes of water at 15°, and which consequently enables one with great rapidity to determine the specific gravities of liquids by the method of immersion.

To pass now to the new *gas-governor*, its most essential part consists of a mercury-manometer (fig. 2), of which one limb, A, is

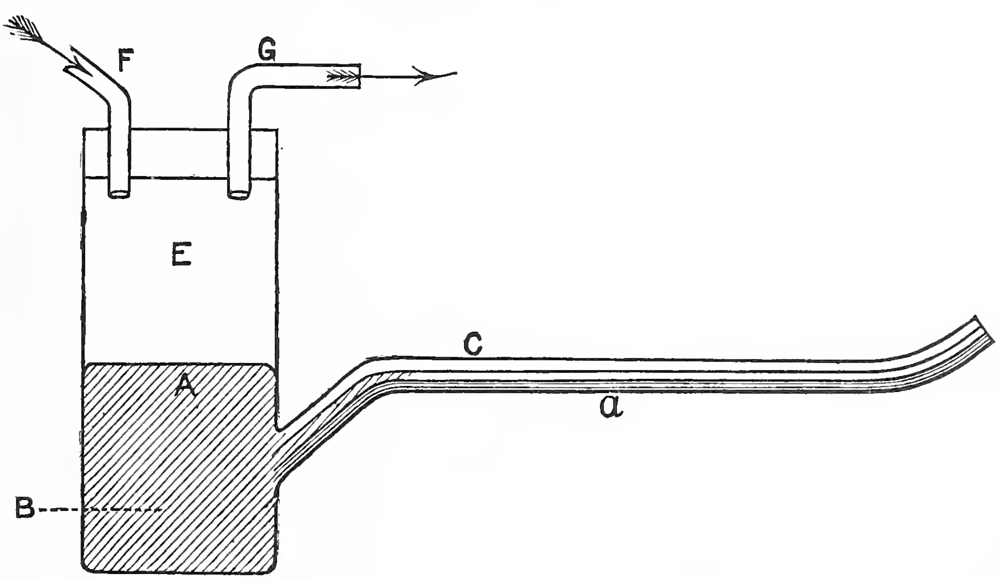


Fig. 2.

about 20 mms. wide, and stands vertical; while the other, C, is of the width of a thermometer tube, and is placed horizontally.

* For fuller explanations, see my article "Balance" in the "Encyclopædia Britannica."

The empty part of the wider limb communicates, through F, with the gas-supply, through G with the gas-lamp serving to heat the air-bath; and the quantity of mercury is adjusted so that, when the gas is at the lowest pressure which, in the course of the experiment, it is likely to assume, the mercurial index in C occupies a certain convenient position a.

The manometer is connected with a constant battery (the circuit of which includes an electro-magnetic arrangement for opening or shutting the gas-tap), in such a manner that, as soon as, through an increase of pressure in E, the index in C travels ever so little towards the right of a, the current is closed, and the gas cut off.

The following Gentlemen were elected Fellows of the Society:—

John Macmillan, M.A.
John Gibson Cazenove, D.D.

The following Gentlemen were elected Honorary Fellows of the Society:—

1. *Foreign Honorary.*

Carl Ludwig, Leipzig.
Ferdinand de Lesseps, Paris.

2. *British Honorary.*

Henry John Stephen Smith, Oxford.
Thomas Henry Huxley, London.
Thomas Romney Robinson, D.D., Armagh.

Monday, 17th April 1876.

Professor FLEEMING JENKIN in the Chair.

The following Communications were read:—

1. On an Improved Form of Galvanic Battery. By J. Cook, Esq. Communicated by Professor Tait.

I wish to direct attention to a simple improvement on battery cells, whereby porous cells are dispensed with, and the inconveniences of gravity batteries avoided.

I may say it is a year or two since I first tried the plan.

It consists in first filling the outer glass cell one-third or one-half with fine *silver* sand, then pushing a ring of glass (which I cut from a common ale pint-bottle with a hot soldering bolt) down an inch or so into the sand. The zinc element forms a ring round the glass, and the copper lies as a plate on the sand within the glass. Its superiority to the gravity batteries, and to those, such as Sir William Thomson's, where the sand forms a dividing layer between the copper with its sulphate below and the zinc with its liquid above, will be obvious. I did not find the cupric sulphate solution to diffuse into the zinc division. It so readily admits of inspection that it would be infinitely preferable to the Meidinger and other plans.

2. On the Properties of the Perigon Versor. By G. J. P. Grieve, Esq. Communicated by Professor Kelland.

3. Descriptions of some new or imperfectly understood Forms of Palæozoic Corals. By H. Alleyne Nicholson, M.D., D.Sc., F.R.S.E., Professor of Natural History in the University of St Andrews, and James Thomson, F.G.S.

(*Abstract*).

In this communication the authors gave descriptions of several new or imperfectly understood forms of Palæozoic corals. After giving a general account of the method of investigation employed by them, the genus *Heliophyllum*, Hall, was discussed at length. The external structure of this genus is very peculiar, and it was shown that the genus is not by any means as nearly related to *Cyathophyllum* as has been generally believed. The new genus *Crepidophyllum* was proposed for a group of forms possessing the extraordinary and characteristic endothecal dissepiments of *Heliophyllum*, but with the remarkable character that the central tabulate area of the corallum is shut in by a well-developed accessory wall or inner mural investment. Sometimes this secondary investment constitutes a complete circular sheath to the central tabulate area, and in this case all the primary septa become

directly connected with the outer surface of the cylindrical tube thus formed. More commonly, the secondary investment is open all down one side, and becomes directly continuous with two of the primary septa, thus constituting a horse-shoe shaped space, formed by the central tabulate area together with a wide fossula containing three short septa. It was shown that the fine coral described by Mr Billings under the name of *Diphyphyllum Archiaci* was truly a *Crepidophyllum*. It was further shown that two different forms, of very similar aspect, had been included by one of the authors under the name of *Heliophyllum sub-cæspitosum*. One of these forms, the typical one, is a *Crepidophyllum*, and will stand as *C. sub-cæspitosum*. The other is a *Heliophyllum*, and the authors described this under the name of *H. elegantulum*.

The name of *Thysanophyllum* was proposed for a genus of æstræiform corals from the Carboniferous rocks of Scotland. This genus is related to *Lonsdaleia* in the general form of the corallum, in the presence of an exterior vesicular zone of large-sized cells, and in the possession of septa, which have no connection with the outer wall. It differs from *Lonsdaleia*, however, in the fact that the columella, so conspicuous in the latter genus, is wholly wanting, and the central area of the visceral chamber is occupied by strong remote, transverse tabulæ. Two species of the genus were described, under the names of *Thysanophyllum orientale* and *T. minus*.

Finally, the genus *Lindströmia* was proposed for a group of small corals, in which the corallum is simple and conical, with an extremely deep calice. The septa are well developed, and meet in the centre of the visceral chamber, where they coalesce to a greater or less extent, and form a strong twisted pseudo-columella, which projects into the floor of the calice, and occupies a large portion of the entire visceral chamber. There are no tabulæ, but the septa are furnished with more or less strongly developed dissepiments, which, however, are remote, and do not give rise to any vesicular zone. The genus may, perhaps, be regarded as belonging to the *Aporosa*. The species *L. columnaris* was described from the Devonian rocks of North America, and it was mentioned that the authors were in possession of other forms of the genus, still undescribed, from the Carboniferous rocks of Scotland.

4. On a Stable and Flexible Arch. By Professor Fleeming Jenkin.

Monday, 1st May 1876.

Professor KELLAND, Vice-President, in the Chair.

The Council have awarded the Keith Prize for the biennial period 1873–75, to Professor Crum Brown, for his Researches on the Sense of Rotation, and on the Anatomical Relations of the Semi-circular Canals of the Internal Ear.

The following Communication was read:—

Is the Gaelic Ossian a Translation from the English? By Professor Blackie.

The recent revival by a distinguished Celtic scholar of the theory of Laing that Macpherson's Gaelic Ossian is a translation from the English, affords an opportunity of examining that question in a more strictly philological fashion than it has hitherto had the fortune to enjoy. Parts of the question were no doubt touched by Mackenzie in the Report of the Highland Society, published in 1805 by Graham in his dissertation on the authenticity of Ossian, by Dr Clerk of Kilmalie, the distinguished author of the new version of Ossian in the late splendid edition published at the expense of the Marquis of Bute; but systematically grappled with the question has never been. Having recently gone through the whole of the originals, I have made careful notes of whatever might tend to settle this question, and have come to the conclusion, in the face of the statement of Mr Campbell—whose authority, no doubt, is one of the highest on the subject, that the Gaelic is unquestionably the original. The tests by which a translator's hand seems clearly discoverable are the following five:—(1) In the English version, awkward, forced, and unidiomatic expressions frequently occur, which can be clearly traced to the influence of a Gaelic original. (2) In all poems of any antiquity handed down in manuscripts, difficulties will occur arising from obsolete words, errors in transcription, confused connection, and other causes. In such cases it is a common practice with translators to skip the difficulty, gloss over the matter with some decent commonplace, and sometime to make positive blunders, which is not difficult for a philologer to expose. All these signs of a translator's hand are frequent in Macpherson's English, and would be more so had he not indulged in such a habit of skipping generally that it is difficult to say in certain cases decidedly that the skip was made because the writer of the English wished to shirk a difficulty. (3) It is a common practice with translators, when they find a passage a little obscure, to remove the obscurity by some manifest alteration of the phrase, or even by interpolating a line, or interlarding a commentary. This also occurs in Macpherson. (4) It is not always that a translator writes under the same vivid vision, or the same fervid inspiration as the original poet; and the consequence is that he will occasionally degrade poetry into prose, and specially fail to bring out that individuality of character in his word-painting which Ruskin has so triumphantly insisted on in the case of the sister art. The instances of failure to seize the most striking features of the original, and the substitution of generic for specific epithets, are frequent in Macpherson. (5) Most translators yie —sometimes, no doubt, wisely—to the temptation of improving on their originals; and Macpherson, from what we know of him, was the last man in the world to think of resisting such a temptation. How much of the Gaelic, as we now have it—that is, his clean copy of his own originals—was subjected to this process of beautification, as we may call it, no one can now tell, but I have traced in several instances departures from the simplicity of the original Gaelic, which can be explained most naturally on the supposition that they proceed from a translator who has yielded, without any just cause, to this flattering seduction. When the results obtained by the detailed application of these tests are combined with the amount of external evidence to be found in the Highland Society's report to the effect that Macpherson actually did translate from Gaelic originals, and was seen by various parties for weeks and months employed in the work of traslation, a cumulative proof was produced that he was most anxious to see by what arguments Mr

Campbell could rebut. If that gentleman, to whom Celtic literature owes so much (and who in fact is the Wolf of the Ossianic question), or any Galician who thinks with him, shall succeed in leading a counterproof, I can only conclude that, considering the scrappy and fragmentary nature of some of the materials in Macpherson's hands, it might possibly have been the case that the translator filled up some of the gaps in his tale in English, with the intention that they might be done into Gaelic before publication by Strathmaskie, Captain Morrison, or some other of his Highland coadjutors; but that the English, as a whole, is a translation from the Gaelic, and not a translation of the best quality in many respects, may be accepted as one of the best ascertained facts in the whole range of philological investigation.

The following Gentlemen were elected Fellows of the Society:—

Professor M. Forster Heddle.
J. F. Rodger, S.S.C.
William Thomson, F.C.S., Manchester.

Monday, 15th May 1876.

Sir WILLIAM THOMSON, President, in the Chair.

The Keith Prize for the biennial period 1873–75, which has been awarded to Professor Crum Brown for his Researches on the Sense of Rotation, and on the Anatomical Relations of the Semi-circular Canals of the Internal Ear, was presented by the President.

The following Communications were read:—

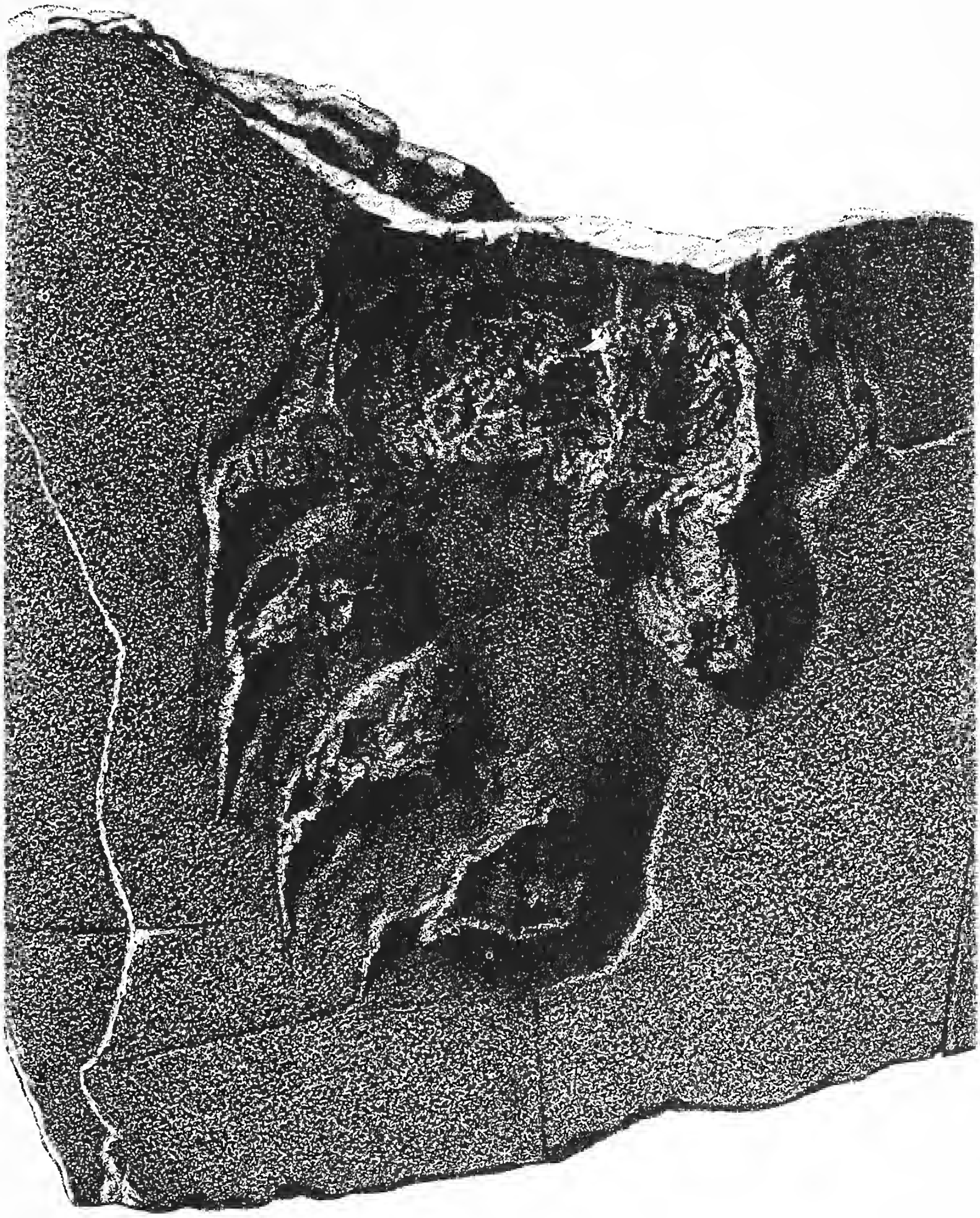
1. Notes on Dredging in Madeira, by the Rev. Robert Boog Watson, B.A., F.R.S.E., F.G.S.

The difficulties in the way of dredging at Madeira are many and considerable. This has probably prevented any of this work having been done since the publication of Mr Macandrew's list of Mollusca, presented to the British Association in 1854. The author having dredged for several years at Madeira, confirms Macandrew's generalisation of the Mediterranean character of the Mollusca—excludes 12 of Macandrew's named list as having crept in by mistake, and to the 115 remaining species identified by Macandrew as Madeiran has added 200 to 250 more, making nearly 400 in all, of which 80 or perhaps 100 are probably new. These he hopes soon to publish.

2. Note on a New Fossil Foot-Print from the Permian Sandstone of Dumfriesshire. By Patrick Dudgeon, Esq., F.R.S.E. (Plate I.)

What appears to be an entirely new foot-print has lately been found in the red sandstone of this district. I have seen many of the foot-prints from the various quarries in the neighbourhood, but have not before observed this one, nor is it like any figured in Sir William Jardine's splendid work on the "Ichnology of Annandale." The foot-prints in question were found in a bed about 20 feet from the surface, at Locharbrigg's Quarry, three miles from Dumfries. They exhibit the usual large hind and smaller fore foot; the impression of the hind foot measures ′·5 × 2′·6, the fore foot 2′·3 × 1′·9 ; the stride of the animal appears to have been about 10′. The impression of the hind foot does not interfere with that of the fore foot, as is the case with several of the foot-prints figured in Sir William Jardine's work, the interval between them being 2′: the hind foot, therefore, must have been put down in the rear of the fore foot when the animal was walking. The impression of the foot shows five toes, the *thumb* being placed far back. The most characteristic features in these foot-prints are the well-developed claws, and the oblique position of the toes, *i.e.*, they are placed to *march* one behind the other. In almost all the foot-prints I have seen, where the toes can be made out, the middle one appears the most prominent; this foot-print is markedly distinct in this respect.

As yet I have only been able to obtain one good specimen of this foot-print—a hind foot; the rest of the slab on which the casts were impressed was unfortunately used for a paving stone in a cottage in the neighbourhood. I got it lifted; but the rough



Mc Farlane & Erskine. Lithrs. Edinr.

usage it had been subjected to had greatly injured the impressions on it; they were, however, sufficiently distinct to enable me to give the above particulars.

The accompanying photograph is a good one of the hind foot in my possession, about half the size of the original. The posterior pad of the foot is not quite complete, and it, together with the pads of the toes, are somewhat broken.

I would propose for these foot-prints the provisional name of *Herpetichnus loxodactylus*, the oblique-toed Herpetichnus, with the following abbreviated character:—

Genus Herpetichnus, *Jardine* ("Ichnology of Annandale," 1853, p. 14).

Herpetichnus loxodactylus, sp. nov.

Sp. chars.—Fore foot = 2′·3 × 1′·9; hind foot = 3′·5 × 1′·6; stride about 10′; impressions free; toes 5, oblique; thumb far back; claws well developed.

Locality and horizon.—Permian Sandstone, Locharbriggs Quarry, three miles from Dumfries.

P.S.—In the discussion which followed this paper, Professor Huxley stated that so far as he could judge from the photograph exhibited, the markings closely resembled a foot-print he had described some years before in a paper read before the Geological Society of London, "On the *Stagonolepis Robertsoni* (Agassiz) of the Elgin Sandstones; and on the recently discovered Foot-marks in the Sandstone of Cummingstone" ("Quart. Jour. Geol. Soc.," 1859, xv. p. 440). The resemblance of these Cummingstone foot-marks to the *Chelichnus* of the Dumfriesshire flags was noticed by Professor Huxley in the paper referred to.

3. On the Decennial Period in the Mean Amplitude of the Diurnal Oscillation and Disturbance of the Magnetic Needle and of the Sun-spot Area. By J. A. Broun, F.R.S.

(*Abstract.*)

The author, in presenting results relating to the decennial period derived from observations made at Trevandrum during twenty-two years, has sought a redetermination of the mean duration of that

period, as shown by preceding magnetical observations connected with his own. The relation of the frequency and area of sun-spots to the amplitude of the diurnal movements of the magnetic needle gives an increased value to this investigation.

Two very different results have been obtained;—one by Dr Lamont, showing a period of 10·4 years; the other, by Dr R. Wolf, gives $11\frac{1}{9}$ years. Dr Lamont's result depends on the assumption that three periods occurred between 1787 and 1818—an assumption which is opposed to the conclusions which have been deduced from the sun-spot, auroral, and magnetic observations for that interval. Dr Wolf's result has therefore been accepted very generally by many of the most eminent scientific men in England and on the Continent.

The author determines the epochs of maximum and minimum range of the diurnal oscillations of the magnetic needle by the more exact method, in which the mean for twelve months corresponding (at its middle point) to each month of the year is obtained. Commencing with the Trevandrum observations, from the present time, proceeding backwards to the earliest series, showing a maximum, that of Cassini (Paris 1784–1788.) The maximum at this time (1787·25) is confirmed nearly by Gilpin's observations (London, 1786–1806). The latter do not show the minimnm in 1792 and maximum in 1797, which should satisfy Dr Lamont's assumption, and they are considered by him, like the observations of sun-spots at the time, as worthless for this investigation. Dr Wolf, on the other hand, finds support in both for a minimum in 1798.

It is concluded by the author, from an examination of Gilpin's observations, that a maximum really happened in 1797·7, but so little marked as to make it probable that any slight corresponding increase of sun-spots would not be noticed by the single, not very accurate, observer at the time. Evidence, however, of a slight maximum is also found in Professor Loomis's investigation for the frequency of the aurora borealis. As it is certain that another maximum occurred about 1804 to 1806, the author finds that Gilpin's observations, which agreed with Cassini's at the commencement of the series, showed in all probability the true magnetic variations afterwards.

It results from these investigations that the mean duration of the period is 10·45 years; but that it appears to undergo a variation between 8 and 12½ years in an interval of 42 years. The small maximum of 1797·7, if a true result, may be expected to repeat itself at some future time, a result which could not fail to aid in the search for the cause of these variations.

The author shows, that according to the long period of 42 years, a *maximum* should have happened in 1776; but that year Dr Wolf has concluded to be one of *minimum* sun-spot frequency. That 1776 was really a year of maximum is confirmed by the observations of Van Swinden, who, it is shown, appears to have been the first to obtain a variation due to the decennial period, and to have pointed out the appearance of a law: it is also confirmed by the observations of Cotté at Montmerency.

The ratio of the ranges of the diurnal variation in the years when it is a maximum to that in the years of minimum, is compared for different parts of the world, and found nearly the same in both hemispheres. It is also found that the law of the diurnal variation is the same in the year of maximum as in the year of minimum. The author concludes that the increase of the diurnal variation is not due to a different cause from that which produces the variation at the minimum, and that this cause acts when there are no sun-spots in the same way as, though with less intensity than, when the latter have their maximum frequency and area. The magnetic variations are therefore not due to the sun-spots; the latter appearing only when the common cause produces diurnal variations having at least two-thirds of the maximum amplitude.

The results derived from the sun-spot area are compared with those from the magnetic observations. While a general agreement is found in the decennial variations from year to year, it is evident that the attempt to calculate the amplitude of the diurnal variation from the sun-spot frequency (as has been done by Dr Wolf) must give results frequently deviating widely from the truth, as might be expected from the previous conclusion.

The decennial period of disturbance of magnetic declination at Trevandrum, deduced from hourly observations in the eleven years, 1854 to 1864, is next considered. It is found that the mean disturbance at each hour of the day shows the decennial period; but that the range of the mean value, from the minimum to the maximum year, is different for each hour, while the maximum and minimum do not happen at exactly the same time for all hours of the day. Secondary maxima and minima are also shown, which vary in their epochs gradually from midnight to noon.

No clear law appears to connect the amount of the maximum disturbance for any hour with that of the minimum for the same hour in the 11 years; the ratio of the first to the second is least for the hours near noon, and greatest for those near midnight. It is found, however, that the maximum and minimum mean disturbance in the diurnal variation for each year, as well as in the decennial variation for each hour, are connected by the following relations :—D_m being the maximum and D_o the minimum disturbance.

$$\sqrt{D_m} - \sqrt{D_o} = \text{Constant.}$$

The *monthly* mean disturbance at Trevandrum in each of the years 1854 to 1864 is compared with the monthly mean sun-spot areas deduced by Messrs De La Rue, Stewart, and Lœwy, from Carrington's and the Kew Observations, with the following result :—The monthly mean disturbance in the years 1854–56 had a considerable value, and marked variations when there were few or no sun-spots. In 1857 to 1862 there are found several maxima and minima of disturbance and sun-spots which occur simultaneously. In some cases, and especially in June 1862, there is a well-marked sun-spot area maximum without any corresponding change of magnetic disturbance. The cause of the solar disturbance did not then extend its action to the earth at that time.

The author concludes with a notice of the hypotheses proposed to explain the decennial period of magnetic variations and of sun-spot frequency, as well as of the cause of sun-spots. It is pointed out that no theory of sun-spot formation can be accepted which does not explain their non- (or very rare) appearance every 10 or 11 years, and therefore the cause of the decennial period is bound up with this explanation. A planetary action which disturbs the equilibrium of the solar gases has been proposed; no other seems to present itself, and this the author believes will be found ultimately to be in question; and though he has not himself been able to find any satisfactory evidence in its favour, yet remarkable results have been obtained by Messrs De De Rue, Stewart, and Lœwy.

4. On the Parallel Roads of Lochaber. By David Milne Home, LL.D.

(*Abstract.*)

The author referred to the papers written on the subject, beginning with that by Dr Macculloch, in the year 1817; and he explained the various theories suggested.

He intimated his adoption of the Lake theory, and expressed his adherence to the view he took in the memoir read by him in this Society in the year 1846, that the blockages of the lakes had been effected by detrital matter.

In support of this view, he pointed out that all over this district of the Highlands there were immense beds of clay, sand, and gravel up to the tops of the hills, at even 2000 feet above the present level of the sea.

These deposits he considered to be undeniable proofs of the prevalence of the sea over this part of the earth's surface to a height of 3000 feet at least.

When the sea retired, so as to expose to atmospheric action the higher parts of the country, there would be depressions in the surface of the land, where lakes would be formed. These lakes would continue at high levels, till the streams issuing from them cut through the detritus. In some cases, the process of erosion would be so gradual, that the lakes would subside without producing any conspicuous beach-lines on the mountain sides. In other cases, the removal of the blockages would be on a large scale, owing to the looseness of the detritus; and if these removals were separated from one another by a considerable interval of time, beach-lines of a permanent character would be formed on the sides of the mountains enclosing the valleys.

The author referred to the existence in this district of the Highlands, now, of several lakes at high levels, which were kept up by detrital blockages. He instanced, in particular, Loch Earba, in the Lochaber district, at a height of about 1150 feet, which was kept up by such means, and on whose banks there was evidence that the lake had once stood 30 feet higher than at present. Near Kingussie there was Loch Gwynae, at a height of 1015 feet above the sea, on whose sides there were traces of five terraces, the highest of which is 132 feet above the present surface of the lake.

He referred to the ample means of cutting through and removing detrital matter possessed by streams and rivers, mentioning particularly the enormous cliffs of detritus cut through by the Rivers Spean and Spey.

He next proceeded to discuss the theories of other geologists.

With regard to the theory that the parallel roads were formed by the *sea*, he adduced arguments to show, that this view was impossible, inasmuch as the "roads" should in that case have all stood at the same level; whereas, in the different glens, they stood at different levels. Moreover, it had been found, that old river courses existed, by which the water in Glen Gluoy flowed into the water in Glen Roy, and that the water in Glen Roy flowed through Glen Glaster into Loch Laggan,—a state of things utterly fatal to the marine theory.

With regard to the blockage of the lakes having been formed by *ice*, the following objections were stated:—

1*st*, The improbability that some of the glens were filled with water, whilst others were filled with ice, the temperature of those glens being all much the same, in consequence of nearly equal altitudes above the sea.

2*d*, The impossibility of getting a glacier to come to the exact spot, where the lakes stopped, to form barriers several miles long, so solid and permanent in structure, as to prevent the escape of the water from lakes above 300 feet deep.

The author concluded by referring to the numerous examples in the Lochaber district, of boulders perched on tops of hills, and of rocks smoothed and striated. These phenomena had been ascribed by some geologists to the action of land-ice. But, coupling with these high-perched boulders, the occurrence of kaims or eskars on the sides of the hills (above the parallel roads), and therefore formed before the Lake period, the author was inclined to ascribe these phenomena to one agent—viz., a sea loaded with ice, when the land was submerged, and to a strong current in the sea, from the north-west, which swept over the submerged land, and through such valleys as Glen Spean and Glen Roy. The lakes, he referred to the period when the land was rising out of the sea. Their beaches were formed on the marine detritus;—which also for a time dammed back the lake waters.

Monday, 5th June 1876.

D. STEVENSON, Esq., C.E., Vice-President, in the Chair.

The following Communications were read :—

1. Physical Observations in Northern Asia. By Professor G. Forbes.

2. On Parallel Motions. By the Rev. John Wilson, M.A., Bannockburn Academy. Communicated by Professor Kelland.

It has been well said that the transmission of force is an "essential condition in machinery." It is no less true that directness in transference is important; that the fewer links in the chain binding driver and follower together, the less likely is the machine to be put out of gear. There is no question here as to the comparative values of the different modes of conveying motion from a prime mover,—rolling contact, sliding contact, wrapping connectors, or linkwork. Each has its own excellencies; each its special advantages; and one is to be preferred to another only according to the nature of the work to be done.

I. *Watt's Parallel Motion.*—The general problem is the "commutation of circular with rectilinear motion." The importance of the question began to be felt soon after the introduction of the steam-engine; and Watt, in 1784, patented an invention which not only had the credit of being the earliest, but up to recent times, the most reliable and accurate parallel motion in existence. This system was a great advance on the huge chains and arches which were affixed to the working beam of the engine for the purpose of obtaining the desired motion; and it has proved to be sufficiently accurate for all practical purposes. The construction is simple, consisting of three bars: two, rotating round fixed centres, and connected at their other extremities to the third bar. A point in this bar, either within or without the points of junction, according as the centres are on opposite sides, or on one side of the connecting rod, moves in a straight line. The simple explanation of the underlying principle is that the curvature in one direction is modified by the curvature in an opposite direction, due to the motion of the other bar. In both the original and the modified form of this three-bar motion there is a divergence from the straight line, which though inappreciable for small angles, becomes somewhat more apparent for larger ones.

The determination of the parallel point is given in the formula

$$QF : FP :: AP : BQ$$

In the figures 1 and 2, AP and BQ are the arms; PQ the con-

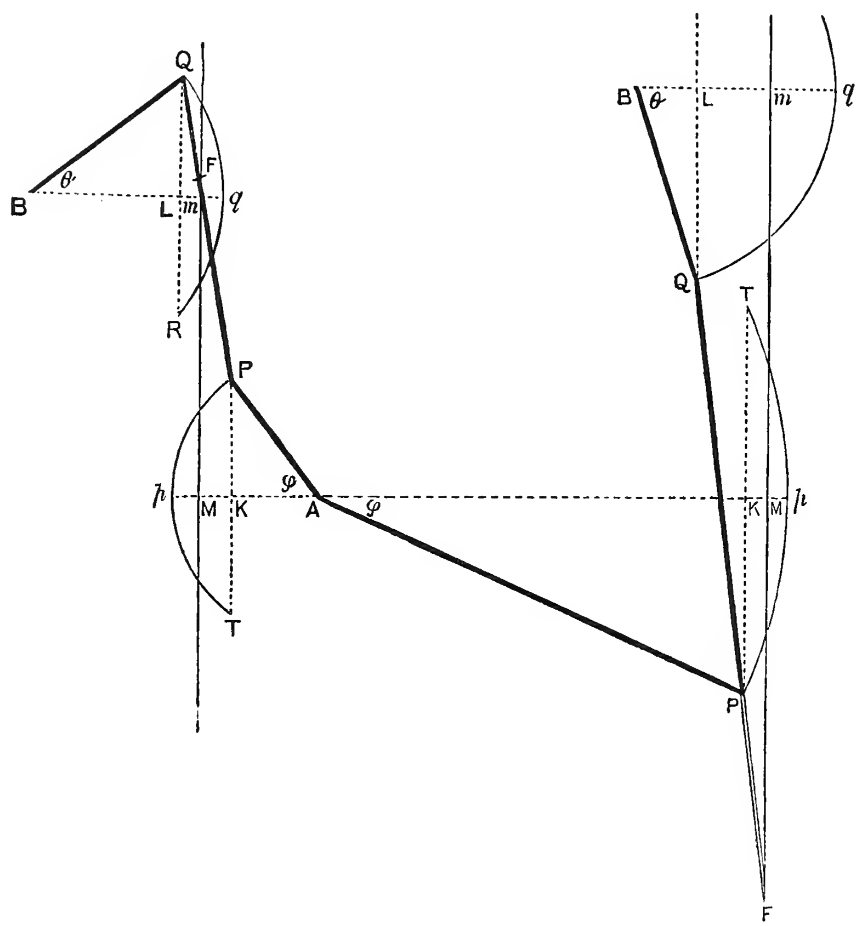


Fig. 1. Fig. 2.

necting link. Let the centres A and B be chosen so that the line mM shall bisect the versed sines of BQ and AP; the throw QR being equal to PT.

$$QF : FP :: Lm : MK : Lq : Kp$$
$$:: BQ\,(1-\cos\theta) : AP\,(1-\cos\varphi)$$
$$:: BQ\cdot 2\sin^2\frac{\theta}{2} : AP\cdot 2\sin^2\frac{\varphi}{2}$$
$$:: BQ\cdot\theta^2 \quad : AP\cdot\varphi^2 \quad . \; . \; . \; . \quad (1)$$
$$:: BQ\cdot AP^2 \quad : AP\cdot BQ^2 \quad . \; . \; . \quad (2)$$
$$:: AP \quad : BQ$$

(1) For small angles, $\sin\frac{\theta}{2}=\frac{\theta}{2}$ nearly.

(2) Ultimately, $BQ \sin\theta = AP \sin\phi$, or, $BQ : AP :: \sin\phi : \sin\theta :: \phi : \theta$.

In connection with Watt's system we are led to consider the motion of the pantagraph, which has been a means of extending the parallel motion. Thus, while to the point F is attached the air-pump rod of an engine; by means of the pantagraph, whose property is that it describes similar curves on a smaller or larger scale, another point will trace another parallel line, and to it therefore may be fixed the end of the piston-rod. By superadding the parallelogram of bars to the Watt parallel motion, and making E,—the angle of the parallelogram—the connecting point of the piston rod, so as to concentrate the force; we have the point E describing a curve similar to F, when

$$QF : BQ :: PQ \text{ or } HE : BH. \quad \text{(Fig. 3.)}$$

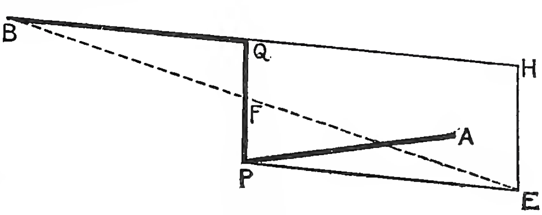


Fig. 3.

The problem of the pantagraph may be proved as follows. Let the initial position be at an angle of 90°, the arms lying along the axes of x and y. (Fig. 4.)

Given the fulcrum A and the ratio AB : AC, to find B_1, the position which B the pencil assumes when the tracer C moves to C_1. When the position of C_{11} is given, the angle C_1D_1A is known, for AD_1 and CD_1 are constants. Join BB_1 and CC_1; AB_1 and AC_1 (which latter are not assumed to be coincident).

In the triangles C_1D_1A and B_1F_1A, the angle B_1F_1A is equal to the angle C_1D_1A; and the sides about these angles are proportional, viz., $B_1F_1 : F_1A :: C_1D_1 : D_1A$. Therefore the triangles are equiangular (Euc. VI. : 6).

Wherefore the angle F_1AB_1 is equal to the angle D_1AC_1, that is they are identical; and AB_1C_1 is a straight line. Now in the triangle ACC_1, — $AB : AC :: AB_1 : AC_1$. Therefore BB_1 is parallel to CC_1; and $BB_1 : CC_1 :: AB : AC$, which is the given ratio.

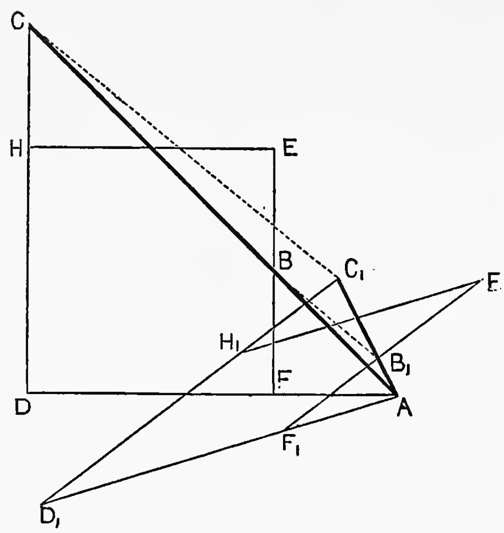


Fig. 4.

Wherefore the position of B_1 is found. And any other position such as Cn will have its projection in similar ratio to the given one $AB : AC$ or $AF : AD$.

II. *The Reciprocator.*—About 1864 M. Peaucellier communicated to the Société Philomathique a paper anent a newly-discovered means of producing parallel motion; but, at the time, little notice was taken of the subject. When Lipkin, a pupil of Tchebicheff, rediscovered it a few years later, it awakened the attention of the most eminent mathematicians of the day; and now it promises to become a power in the field of higher analytical investigation. The marvellous extension of the problem is due chiefly to the research of Professor Sylvester, who was the first in this country to direct attention to it.

The fact of pure linkwork constituted the difference between Peaucellier's and all other attempts at parallel motion. These have been only approximate, or if exact, the slot has been called into action; while in the case before us the motion is entirely due to the grouping of links around fixed centres.

The system consists of seven links. (Fig. 5.) A cell or rhombus MPNC is jointed at two opposite angles to two links AM and AN, which are called connectors. To one of the remaining angles is attached a radial bar BP. Of the two fixed points, A is called the fulcrum; B, the pivot. The points C and P are the poles of the cell. The variable distances $\overline{CA}$ and $\overline{AP}$ are called the arms of the cell, while the difference between the squares of AM and MP is termed the modulus.

When A, the fulcrum, lies without the rhombus, the cell is called a positive cell; when it falls within, the cell is negative.

The following results can easily be verified :—

(1.) $\overline{APC}$ is a straight line.

(2.) $\overline{AP} \cdot \overline{AC}$ is a constant quantity (Euclid III. 36), $\overline{N}$ being at every moment the centre of a circle with a radius $\overline{NP}$ or $\overline{NC}$. This result is expressed by saying that the curves described by P and C are the inverse of one another. Now the inverse of a circle is generally another circle. If, then, one of the poles of the cell be made to revolve in a circle round B, the other pole will describe a circle. There is however an exception, for when the fulcrum is in the circumference of the circle described by P or C, the other pole describes a straight line.

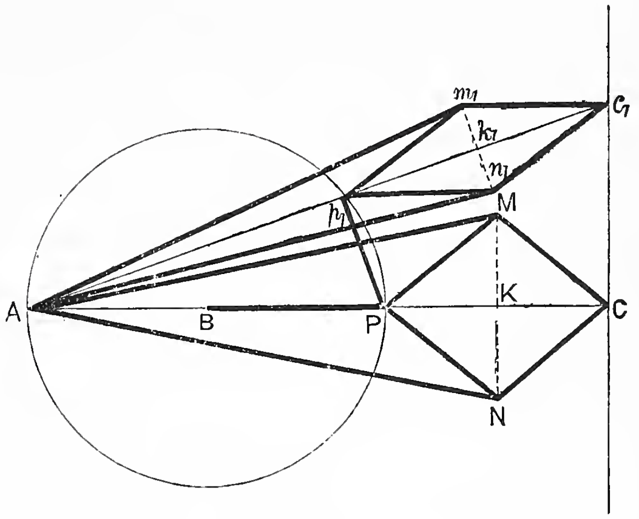


Fig. 5.

The following is the geometrical proof :—

Let the positions of P be p_1, p_2, p_3, &c., and the corresponding positions of C, c_1, c_2, c_3, &c.; and suppose that the initial position of the system is when AC lies along the axis of x, that is, in a straight line with the centres A and B. Join P and p_1; C and c_1. Join the corresponding pairs Pp_2, Cc_2, &c.

By Eucl. II.: 12. $AM^2 = MP^2 + AP^2 + 2AP \cdot PK$

$$Am^2 = mp^2 + Ap^2 + 2Ap \cdot pk$$

$$\therefore AP\,[AP + 2PK] = Ap\,[Ap + 2pk]$$

$$AP \cdot AC = Ap \cdot Ac.$$

$$\text{or } \frac{AP}{Ap} = \frac{Ac}{AC}$$

In the triangles APp and ACc the angle A is common; and the sides about it are proportional, viz.,

$$AP : Ap :: Ac : AC$$

Therefore the triangles are equiangular (Euc. VI. 6), wherefore the angle $c_1CA = Pp_1A$. But Pp_1A, the angle in a semicircle, is a right angle (Euc. III. 31), therefore c_1CA is also a right angle.

With any other position p_2 of P, C will take up a corresponding one c_2, by joining which with C a right angle is formed. Wherefore the point C moves in a straight line at right angles to AB produced.

When the ratio subsisting between the length of the radial bar and the distance of the pivot from the fulcrum is not one of equality, the position of the parallel point is a circle, which is concave or convex with respect to the fulcrum, according as BP is greater or less than AB. The results, as determined by methods of analysis, show that the equation is the same for both the symmetrical and non-symmetrical forms of the cell; that is, for both the ordinary cell, where the pivot B in the initial position lies in a line with the fulcrum and the poles,—and for that form where the line passing the poles makes in the initial position a tangent to the circle described by the radial bar. Thus, if R were the centre of the circle described by C, its distance from the fulcrum A,

is $RA = \frac{AB\,[AM^2 - MP^2]}{BP^2 - AB^2}$; and the length of the radius

is $RC = \frac{BP\,[AM^2 - MP^2]}{BP^2 - AB^2}$

The determination of the position of R, which is evident in the non-symmetrical form of the cell, is simply given in the ratio RC : RA :: BP : AB.

Thus, if $AM = 7$; $MP = 5$; $BP = 3$; and $AB = 1$

then $$RC = \frac{BP\,[AM^2 - MP^2]}{BP^2 - AB^2} = \frac{3(49 - 25)}{9 - 1} = 9$$

$$RA = \frac{AB}{BP} \cdot RC = 3$$

One form of the cell has given rise to a discussion of the question whether the parallel motion of Peaucellier is not simply a modification of the pantagraph. The resemblance between the two systems is not noticeable at first sight; and one would be inclined to deny any connection between them.

In this system the connectors are joined not at the opposite angles of the rhombus but at such points in the adjacent sides produced that at every moment they are parallel to the remaining sides (fig. 6). APC is a straight line. AM = CM since AM||LP. Let $\overline{\text{LP}}$ be produced to F, making $\overline{\text{LF}} = \overline{\text{LM}}$, and complete the rhombus $\overline{\text{LFKM}}$. The bars $\overline{\text{AN}}$, $\overline{\text{CN}}$, and $\overline{\text{IP}}$ can be removed without interfering with the motion of $\overline{\text{C}}$ and $\overline{\text{P}}$. Thus AM, MC, with LF, FK form a pantagraph, and for every position of $\overline{\text{C}}$, $\overline{\text{P}}$ takes another, equiangular and proportional.

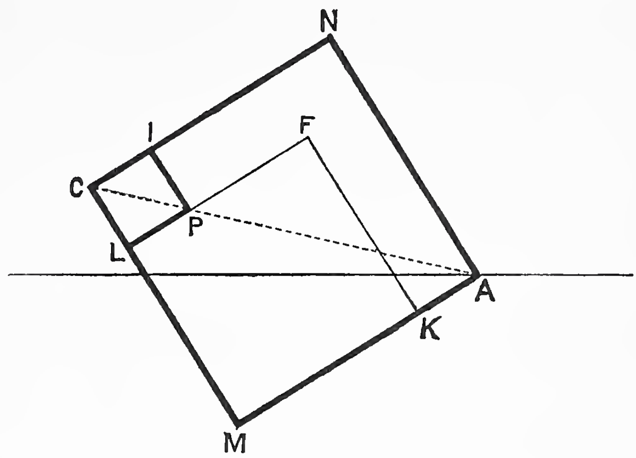


Fig. 6.

III. *The Gorgon Linkage.*—The parallel motion of Mr Scott Russell is exact, and constitutes a two-bar motion. From the fact that it was fitted by Mr Seaward to the engines of the "Gorgon," it may conveniently be called a Gorgon Linkage. A link AB is bisected in C, and at this point another link CD, equal to CA or CB is attached. D is the centre of revolution. If one end A of the link AB be guided along the axis of x, the motion of B is at every moment in the axis of y from D.

For every position C is the centre of a circle $\overline{\text{ADB}}$, and ADB is the angle in a semi-circle, that is, a right angle. Hence B describes a straight line.

This system of links derives additional interest from the discovery of the Peaucellier cell, as by it the motion of the parallel point can be thrown in a direction at right angles to itself; that is, parallel to the line joining fulcrum and pivot; and this can be transferred to the line of centres by means of the pantagraph.

IV. *Hypocycloidal Parallel Motion.*—Another very interesting case of the problem of Parallel Motion is that produced by a hypocycloidal movement. When one circle is made to revolve on the concave circumference of another circle, any point in it describes a curve, which is called the hypotrochoid or hypocycloid. If the diameter of the revolving circle be equal to the radius of the circle in which it revolves, the hypotrochoid becomes an ellipse; and if the point be *on* the circumference of the smaller circle, the ellipse degenerates into a straight line.

Let the circle BPC (fig. 7), diameter BC = AC, revolve in the circle ABM, radius AC. If the initial position of the describing circle be such that AC be its diameter along the axis of y, the point A will move along the diameter AM. When the circle has moved round to B, let P be the position of A; then AB = BP. Join BKC and KP. Let ACB = θ; BKP = φ. The arc BP = AB $\therefore$ BK $\cdot \varphi$ = AC $\cdot \theta$ = 2 BK $\cdot \theta$. Hence $\varphi = 2\,\theta$. Hence P must lie on AC. (Euc I. 32).

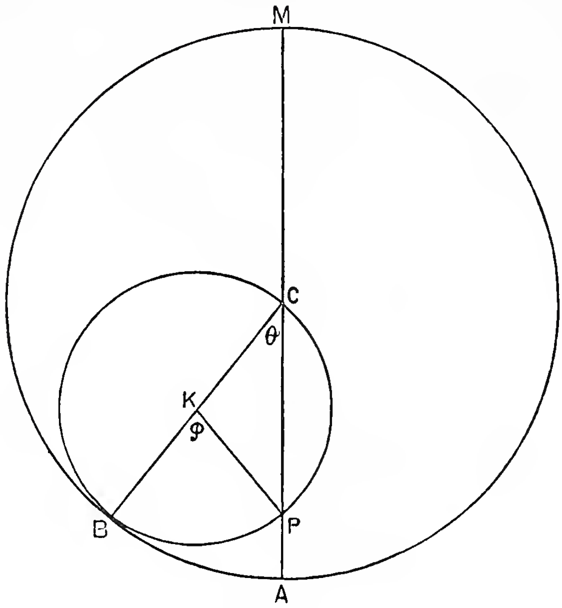


Fig. 7.

The difficulty of constructing, and the inconvenience experienced in using an arrangement in which the annular wheel comes into action, are so great that it is seldom employed. The following is a simple method of obtaining the hypocycloidal motion without requiring the annular wheel. On the arm ACB (fig. 8), which is made to revolve in a circle MLP round A, attach two toothed wheels LNF and FEM, in gear with each other and with a fixed wheel EOH, whose radius AE is double that of NLF. The centre wheel FEM is used merely for the purpose of reversing the motion of NFL, hence it is immaterial how many teeth it has; the only postulate of the problem being that LFN revolve on its axis at double the speed of the

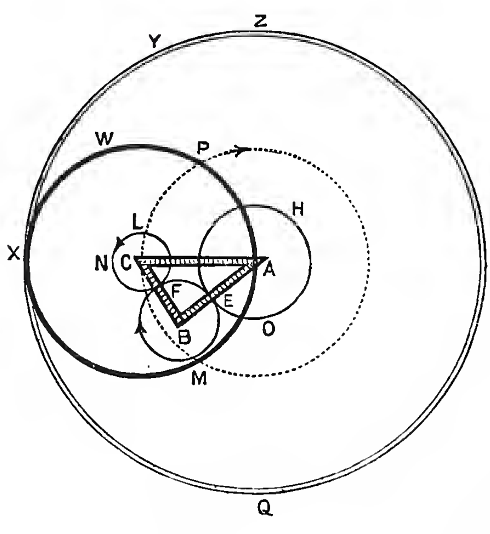


Fig. 8.

arm AC, and in a contrary direction. The number of teeth on HEO is twice the number on NLF. Hence NLF makes two revolutions round its axis, while the arm causes it to revolve round the fixed centre-circle HEO. The axis C is rigidly connected with a wheel XWP, revolving on the upper surface of the plate-arm ABC, whose circumference continually passes through A. Thus while the plate-arm ABC makes one revolution round A in the direction of the hands of a watch, XWPA, which is rigidly connected to the axis C of NLP and partakes of its motion, makes two revolutions in the opposite direction. Thus it revolves round an imaginary annular wheel XYZQ, whose diameter ZQ is double that of its own; and in virtue of hypocycloidal motion, if XWP be the initial position, the point A in the circumference of XWPA will move along ZQ.

In the annexed figure (fig. 9), the *modus operandi* of the machine is sketched. The system is analogous to the sun and planet wheel invented by Watt.

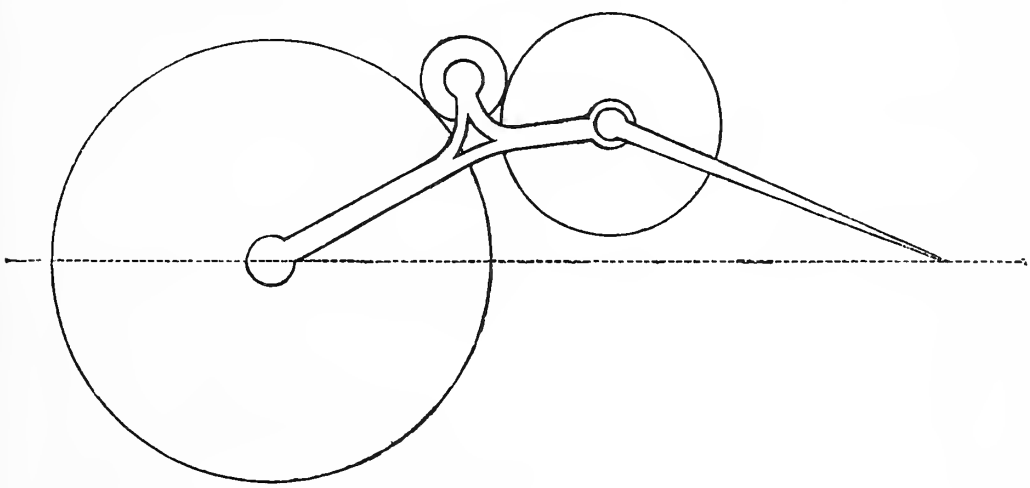


Fig. 9.

It would be going beyond the limits of this paper to direct attention to the two modifications of the Peaucellier cell, which have been called respectively the Quadratic-binomial Extractor, and the Conicograph. The introduction of these into the sphere of mathematical investigation has given several indications of valuable results to be obtained therefrom. For a brief outline of these, we refer the reader to the pamphlet of Professor Sylvester, who not only was the first in this country to direct attention to the general problem, but also had the credit of demonstrating the higher vantage ground opened to the mathematician by means of Peaucellier's discovery. The extent of our own obligation to him is great.

Not less are we indebted to Professor Kelland, whom we have known both as a teacher and a friend. The valuable hints and suggestions he has given us on this subject we are glad to take this opportunity of acknowledging.

3. Laboratory Notes. By Professor Tait.

(*a.*) On the Passage of the Electric Current from Amalgamated Zinc to Zinc Sulphate Solution. By J. G. MacGregor, M.A.

(*b.*) On the Thermo-Electric Properties of Cobalt, &c. By Messrs Knott, MacGregor, and C. M. Smith.

(*c.*) Measurements of the Potentials required for Long Sparks of a Holtz Machine. By Messrs Macfarlane and Paton.

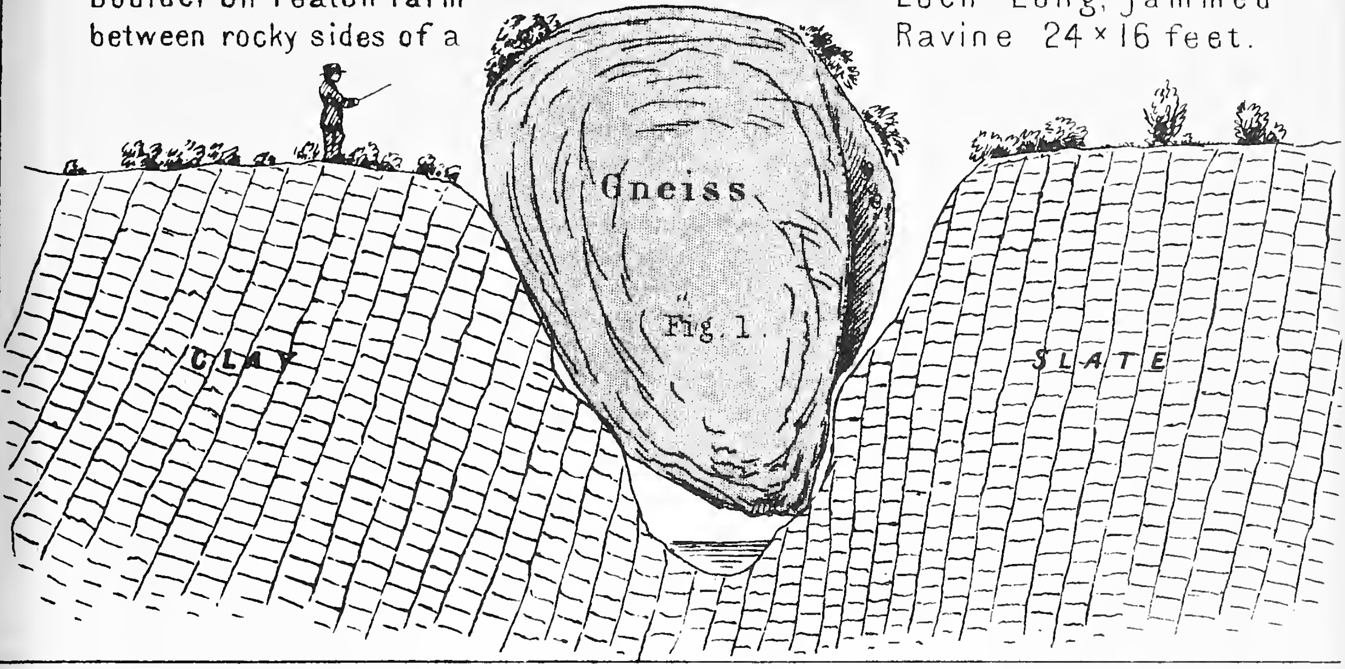
4. Note on Orthogonal Isothermal Surfaces. By Professor Tait.

5. Notice of some recent Atmospheric Phenomena. By Professor Tait.

6. Report by the Society's Boulder Committee. (Plates II. and III.)

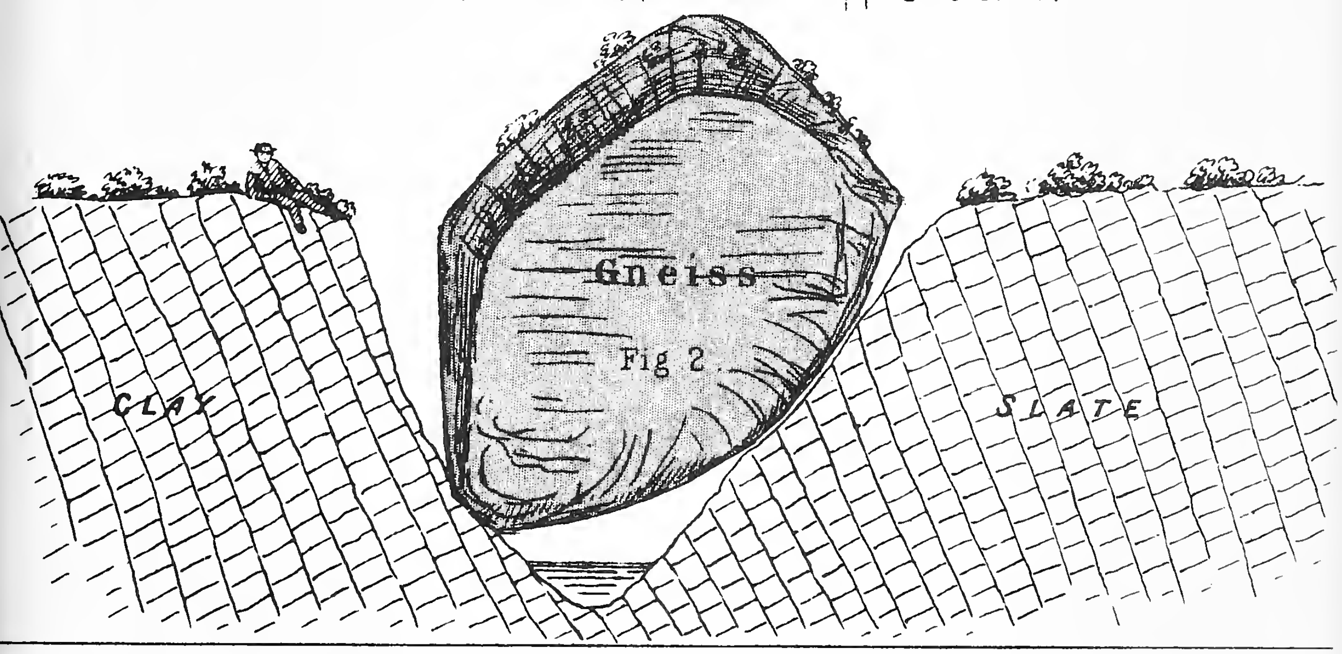
Mr David Milne Home gave in the Third Report of the Society's Boulder Committee, from which the following are extracts:—In November 1875, on the invitation of Sir John Douglas of Glenfinnart, the Convener went to visit him at that place, to have an opportunity of examining several remarkable boulders reported to the Committee as situated in that part of Argyllshire.

1. On the east side of Lochlong, opposite to Ardentinny, there is the farm of Peaton. On this farm, a burn descends from a steepish hill which faces the north. A gneiss boulder lies in a gorge cut by the burn through rocks of clay slate. The boulder



Boulder on Peaton Farm between rocky sides of a Loch Long, jammed Ravine 24 × 16 feet.

Same Boulder viewed from opposite side.



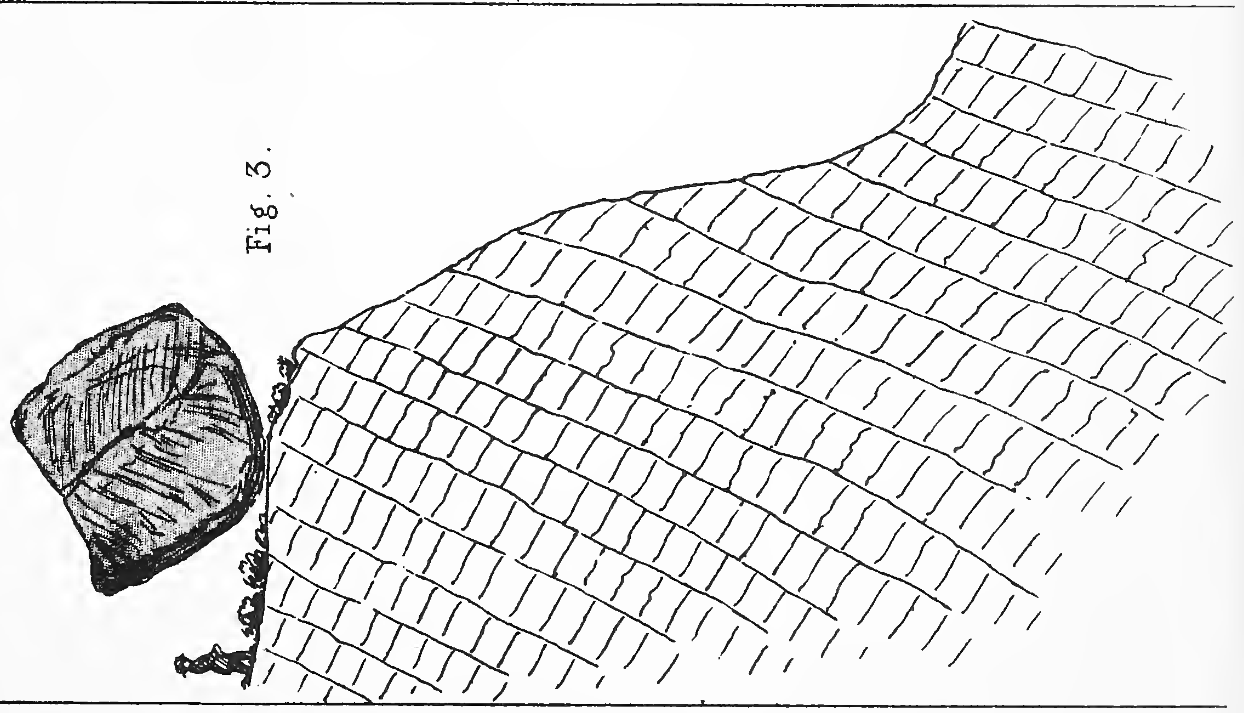
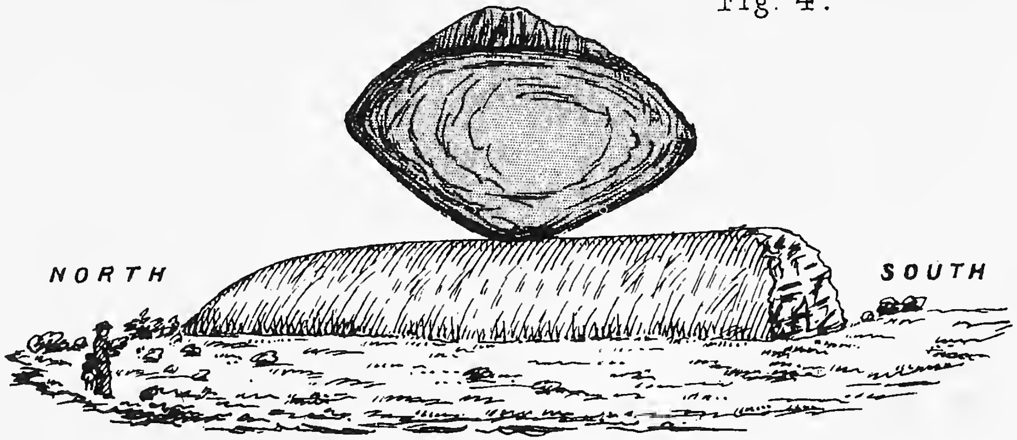


Fig. 3.

Clach Udelain Boulder of Gneiss 1526 ft above Sea, on edge of a cliff about 300 ft high

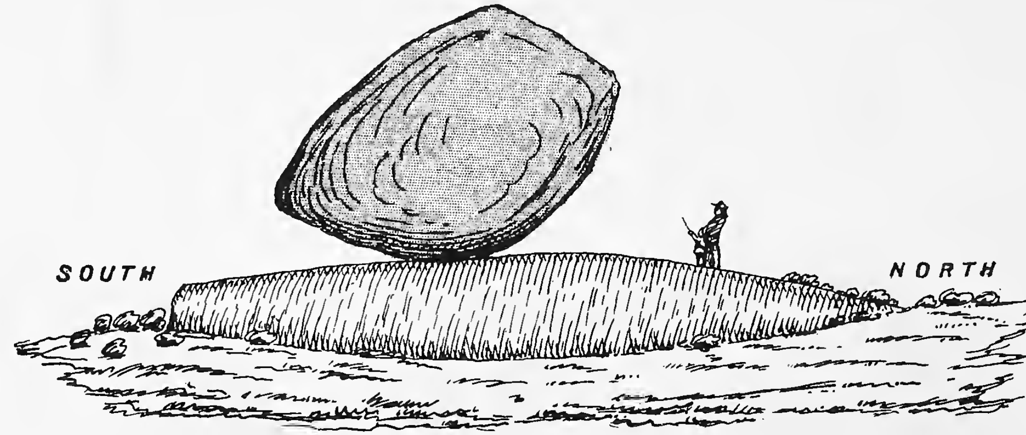
Boulder of Gneiss on a rock of Clay Slate Rock, smoothed from the North, as shown by rough parts facing the South.

Fig. 4.



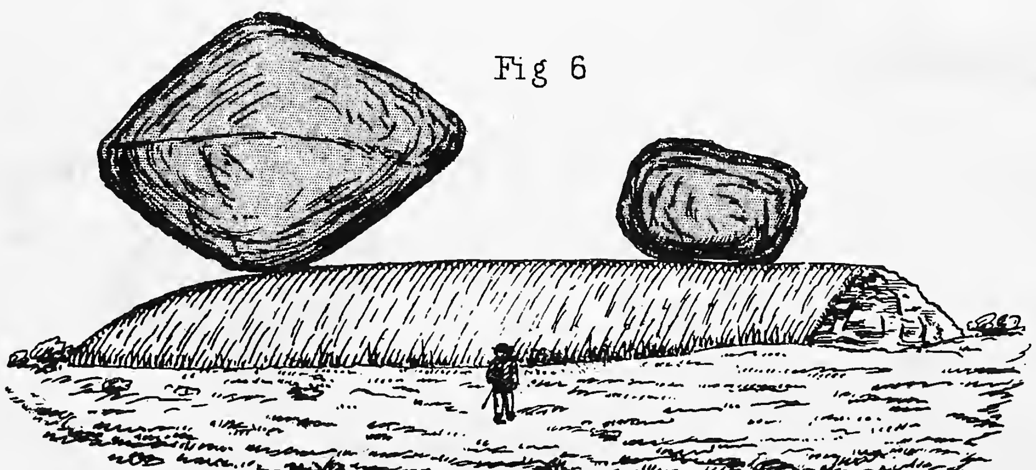
The Same Boulder viewed from opposite side. Local name "Giants putting stone."

Fig 5.



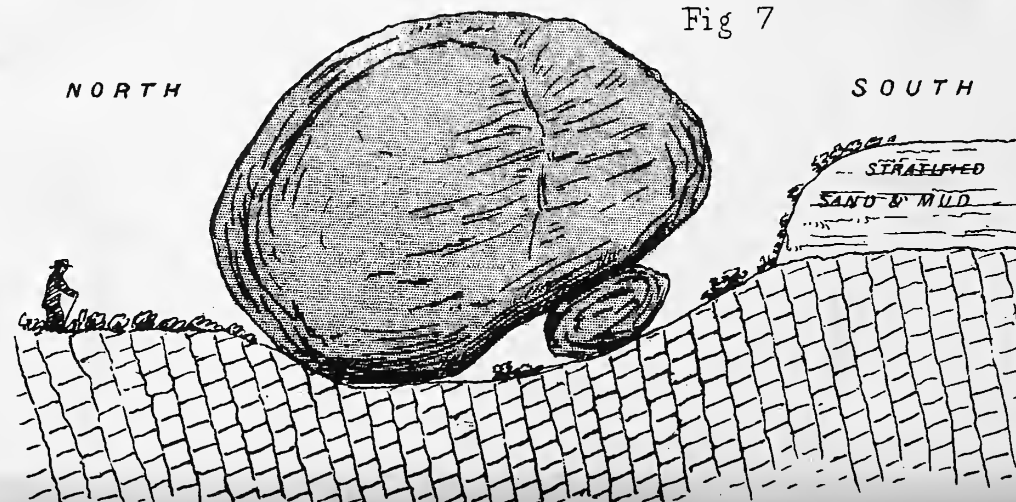
Gneiss Boulders North of Knap Farm House on Loch Long resting on smoothed clay slate rocks

Fig 6



Pulag Boulder, near Glenfinnart, on Loch Long, 824 feet above Sea Its South side rests on a small Boulder

Fig 7



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rests on the rocky sides of the burn, jammed in between the sides. The boulder has some local name like "Jenny Meullen," meaning "House on a knoll." The height of the boulder above the sea is about 226 feet. Its distance from the sea-beach is about three-quarters of a mile. The size of the boulder is about 24 × 18 × 12 feet. It is most probable that the boulder was transported across the loch from the north or north-west, and was arrested in its further progress southward by the hill, on the north side of which it stands.

Two sketches of this boulder are given on Plate II. figs. 1 and 2.

2. Between the site of this boulder and the sea-beach an old sea margin occurs at a height of about 45 feet above the sea (medium level). A number of boulders lie along the line of this sea margin. There is an old sea margin on the opposite or west side of the loch, at exactly the same height—viz., 45 feet.

3. Close to the beach in this part of Lochlong—*i.e.*, about 8 or 9 feet above high-water mark—at a place called "Letter," there lies another gneiss boulder, 12 × 8 × 8 feet. Its long diameter points N.W. by N.—viz., to Glenfinnart Valley.

4. Very near this boulder (about 100 yards to the north) the clay slate rocks have been ground down and smoothed. Their smoothed surfaces show numerous striæ pointing N., 2° or 3° W. (magnetic). The smooth surface dips towards the north at an angle of 3° or 4°.

5. On the hill above Carrick Castle, situated on Lochgoil, there is a boulder called "Clach udelain," or the "Stone nicely balanced." It is at a height of 1526 feet above the sea. This boulder is of gneiss, and lies on rocks of clay slate. It lies on a bare rock, the face of which slopes to N.N.E.—*i.e.*, towards Lochgoil. The boulder is within three or four yards of the edge of a precipitous rocky cliff, which goes vertically down about 500 or 600 feet. The block is of enormous size. Unfortunately the note taken of its dimensions has been lost. This boulder, from its position, could not have fallen from any hill. There is no hill near it from which it could have fallen.

A sketch of this boulder is given on Plate II. fig. 3.

6. The next boulder visited is about two miles to the eastward of the last-mentioned, and is within a quarter of a mile of Lochgoil,

near its junction with Lochlong. It is about 450 feet above the sea. It may be observed, that all the rocks in this district have their smooth faces towards the north, their rough faces towards the south. This boulder has received the name of the "Giant's Putting-Stone," from a legend which alleges that in former times there were giants who inhabited the district on both sides of Lochlong, and who were in the practice of amusing themselves by throwing these huge boulders across the loch. The rock on which it rests slopes gently N. by E. This rock presents a large surface, ground down and smoothed. The space of rock occupied by the superimposed boulder is only 18 inches by 12 inches. It would not be difficult for two men with strong levers to move the boulder from its narrow resting-place, in which case the boulder would probably roll down the steep hillside into the loch.

Two sketches of this boulder are given on Plate III. figs. 4 and 5.

7. To the north of Knap Farm-house, there is a small hill, on or very near the top of which eight or ten boulders are clustered. They suggest the idea that this hill has arrested or interrupted the body, whatever that body was, which transported the boulders, and caused them to be stranded here.

8. There is another hill lower down the valley of Knap (about 480 feet above the sea), the top of which consists of clay slate rocks, rounded and smoothed by some agent passing over from the north. It has received the popular name of the "Pig's Back." Several boulders lie on this ridge. The largest rest on a very small portion of rock.

A sketch of this ridge of rock, with boulders on it, is given on Plate III. fig. 6.

9. Pulag boulder is near the top of a hill to the west of Glenfinnart, about 824 feet above the sea. It is a large block of gneiss, about 7 feet high. There are many other smaller boulders lying near it. The large boulder is almost on the edge of a precipice which goes down at least 200 feet. It could not have been rolled or pushed to its present position. The levels of the district show the greatest openings towards the north—a circumstance which suggests that the boulder came from the north. Moreover, its south end rests on a smaller boulder, which seems to have stopped its progress further south.

A sketch of this boulder is given on Plate III. fig. 7.

10. Along various parts of the hills in this district where their highest ranges are seen against the sky, and at a height of about 2000 feet above the sea, boulders are discernible from a distance, lying on the ridges. It would be very desirable to obtain particular accounts of boulders at so high an elevation.

11. In the last Report of the Committee, notice was taken of a boulder in Ayrshire called the "Hunterston Boulder." Along the same coast, and especially on the property of Mr Alexander of Boydstone, several very large boulders may yet be seen. One, called the "Boydstone Rock or Stone," is situated about two miles north-west of Ardrossan. Some chips of the boulder, sent to the Convener in a letter, show that it is porphyritic. The rocks on this part of the coast are Old Red Sandstone. The boulder is in length about 19 feet, and in breadth about the same. It is partly buried in the mud of the shore. Its highest point is $9\frac{1}{2}$ feet above the shore. It is said to contain 40 cubic yards above the shore line. It is supposed that the boulder is buried to the depth of 5 feet. The tide at high water leaves about 3 feet of the boulder visible. This boulder has inspired the poetic genius of an Ayrshire letter-carrier (Malcolm Kerr, post-messenger between Ardrossan and West Kilbride), who, through Mr Weir of Kirkhall, has sent to the Convener the following stirring address :—

To the Great Boulder on the shore opposite to the lands of Boydstone, two miles north of Ardrossan, Ayrshire.

"Can'st thou speak, old grey stone,
Unto me ?
List thou to the ocean's moan,
I to thee :
Music sweet ! Spirits string
Wild ditties, as they cling
To the big waves which swing
Around thee.

Stranger ! whence didst thou come
To this shore ?
Art thou an Arctic crumb,
Which of yore

On some huge iceberg side
From thy first home did glide,
A wanderer on the tide,
To this shore?

Many eyes with wonder,
Ages gone,
Looked on thee! What number
Yet unknown,
Will gaze with curious eye,
Seeking to know thy history,
And solve a hidden mystery,
Old grey stone."

Besides the boulder which inspired these verses, there are two others, also on Mr Alexander's property—one of them, as Mr Weir states, "even larger than the big stone at Brigurd, at Hunterston." This larger one it was proposed to split up for building purposes at Ardrossan; but Mr Alexander interfered, and saved it. There are many other boulders along the Ayrshire sea-coast, but none so large as the Boydstone boulder. A chip of one of these sent, shows that it is of gneiss, indicating a northern origin.

12. A report has been received of a gneiss boulder near Dunblane, on the property of Cromlix, belonging to the Hon. Captain Drummond. The length of the boulder is stated to be $17\frac{1}{2}$ feet, its breadth (on an average) 10, its height about 5 feet. Its longer axis lies in a direction south-west and north-east. At its south end it dips into the ground at an angle of 45°. Its weight above ground is estimated at 34 tons. Its height above the sea is about 450 feet. It is about four miles south from the Grampians. The same reporter (Henry Wilkie, Ashfield Works, Dunblane) refers to a group of four boulders in the parish of Redgorton, at the west end of a gravel ridge on the farm of Bertha, the property of Murray Graham, Esq. of Murrayshall. Three of these Redgorton boulders are within an area of 30 yards. They are angular; flat on the top, and some of them square. They seem to be Silurian rocks. They are distant about twelve miles from the Grampians.

13. Notice has been received by the Convener from Mr Robertson, C.E., who is in charge of the Albert New Docks at Leith, that boulder clay was found in excavating for the docks beneath the shore line, to the depth of about 70 feet. The clay was full of

large blocks. Some of these were of sandstone, weighing 10 or 12 tons, and appeared to be of the same rock as that worked at Granton and Craigleith Quarries to the westward. Beneath the boulder clay, there was found what the engineers call a "running sand" lying over strata of shale and sandstone.

Before concluding, the Committee may advert to the circumstance that a part of the district comprehended in this Report was many years ago described by an eminent Scotch geologist—the late Charles M'Laren—and with reference to the very matters embraced in this report. Mr M'Laren read papers in this Society, in the years 1846–47, describing the boulders then existing on the shores of the Gairloch, and on the hills between that loch and Lochlong. Even then the destruction of boulders in that quarter had begun, being appropriated, as Mr M'Laren states, to building purposes; and probably by this time they have all been annihilated. Mr M'Laren in these papers described also the striation and smoothings of the rocks, which he found from the sea-shore up to the tops of the ridges, between the Gairloch and Lochlong, at heights of about 1000 feet above the sea. It is due alike to the memory of our Associate, and to the interests of geological science, to mention, that the boulders referred to by him, as found on the Gairloch, consisted of grey granite, of which he counted above a hundred, one-third of them exceeding 30 tons in weight; as also mica slate, which, though less numerous, had had among them blocks of 60 and 80 tons in weight. As the rocks of the Gairloch *in situ*, are of a more recent kind—viz. clay slate—Mr M'Laren justly inferred that the boulders were of northern origin. For those of granite, he pointed to Ben Cruachan, a mountain exceeding 3000 feet in height, situated to the N.N.W., and distant about thirty miles. The mica slate hills are also in the same direction, somewhat more distant. From his study of the boulders and other phenomena in this district, Mr M'Laren drew two important conclusions. One conclusion was, that the boulders must have been brought to the district from the parent mountains, across valleys and ranges of hills, on ice floating on a sea which stood from 1500 to 2000 feet above the present sea-level, and in which a strong current had prevailed from the N.W. This conclusion, it will be noticed, is confirmed by the facts specified in the present Report, and also in former Reports by the Committee. The numerous instances given in these Reports of huge boulders shown by their composition to be of northern rocks, clustered frequently on the summits or peaks of hills, at heights of 1500 and 2000 feet above the present sea-level, seem to leave no doubt regarding the soundness of *that* conclusion to which Mr M'Laren had come to thirty years ago.

The other conclusion to which Mr M'Laren came, and which many good geologists of the present day hold, was this, that local glaciers had at one time existed in all those valleys. This he inferred from observing, that the striations on the rocks were all, as he thought, exactly parallel with the axis of the valley, and which here generally runs in a direction north-west and south-east; and also from discovering accumulations of gravel and clay in the form of elongated embankments—some across the valleys, others parallel with the valleys, reminding him of the lateral and terminal moraines of Switzerland.

As to the soundness of this second conclusion, or the correctness of the observations on which it was founded, the Committee give no opinion. None of the members of the Committee have visited the localities, and their function as a Committee has been chiefly to collect facts connected with the boulders. But it may not be out of place to record the fact, that another of our Associates, the late Robert Chambers, who had also given much attention to this branch of geological science, went to examine the districts referred to by Mr M'Laren, and expressed doubts regarding these last-mentioned views. In two papers read by him in this Society in December 1852, Chambers states, that on the rocky ridge between Lochlong and Holy Loch, at a height of 600 feet above the sea, the direction of the striations was not parallel with the valley, but "slanting over the hill;" and as the striations were not merely on the sides of the valleys, but on the tops of the hilly ranges dividing the valleys, he thought they were more probably due to a general glaciation of the country than to local glaciers. Chambers quotes also the opinion of Sir Roderick Murchison, "that there is no imaginable centre for the issue of glaciers of the ordinary kind down the Gairloch." On this point the Committee offer no opinion, and wish only to advert to the investigations of eminent Scotch geologists who in former days attempted a solution of these difficult questions. The Committee would venture to suggest a re-examination of the localities, as the fuller knowledge now possessed on these subjects may possibly throw a clearer light on the phenomena.

The Committee wish only to add, that if any geologists, whether fellows of this Society or not, happen, in the course of their rambles through the country, to fall in with or hear of any boulders remarkable for size, position, or composition, not yet mentioned in the Committee's Reports, the Convener will be happy to receive a notice of them.

After some conversation, in which Mr Ferguson of Kinmundy, Dr Bryce, the Rev. T. Brown, and others took part, the Report was adopted, and the proceedings terminated.

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Almanach der königlichen Bayerischen Akademie der Wissenschaften. Jahr 1875. 12mo.—*From the Academy.*

Sitzungsberichte der königl. bayer. Akademie der Wissenschaften. Philosophisch-Philologischen und Historischen Classe 1875—Band II. Heft 1-4; Band I. Heft 1. Mathematisch-Phisykalischen Classe—1875, Heft 2, 3; 1876, Band I. Heft. 8vo.—*From the Academy.*

Neuchatel.—Bulletin de la Société des Sciences Naturelles de Neuchatel. Tome X. Part 2. 8vo.—*From the Society.*

New Haven (*U.S.*)—Journal (American) of Science and Art, conducted by Benjamin Silliman. Vol. X. Nos. 58–60; XI. Nos. 61–66; XII. Nos. 67, 68. 8vo.—*From the Editor.*

Transactions of the Connecticut Academy of Arts and Sciences. Vol. III. Part I. 8vo.—*From the Academy.*

New York.—Fifty-Sixth Annual Report of the Trustees of the New York State Library for the year 1873; Fifty-Seventh Annual Report for the year 1874. 8vo.—*From the Trustees.*

Eighty-Sixth Annual Report of the Regents of the University for 1873; Eighty-Seventh Annual Report for 1874. 8vo.—*From the University.*

New Zealand.—Statistics of the Colony of New Zealand, 1874. Folio.—*From the New Zealand Government.*

Tenth Annual Report of the Colonial Museum and Laboratory. 1874–75. By Dr Hector. 8vo.—*From the Geological Survey.*

Nijmegen.—Nederlandsch Kruidkundog Archief. Deel II. Stuk 1. 8vo.—*From the Editors.*

Oxford.—Astronomical and Meteorological Observations made at the Radcliffe Observatory, Oxford, in the year 1873. Vol. XXXIII. 8vo.—*From the Observatory.*

Paris.—Annales des Mines. Tome VIII. Liv. 4^e, 5^e, 6^e; Tome IX. Liv. 1^e. 8vo.—*From the Ecole des Mines.*

Bulletin de la Société de Géographie. 1875, Septembre, Octobre, Novembre, Decembre; 1876, Janvier, Fevrier, Mars, Avril, Mai, Juin. 8vo.—*From the Society.*

Paris.—Bulletin de la Société Mathématique de France. Tome III. Nos. 3, 5, 6. 8vo.—*From the Society.*

Nouvelles Archives du Museum d Histoire Naturelle. Tom X. Fasc. 1–2. 4to.—*From the Natural History Museum.*

Publications of the Dépôt de la Marine (with Charts). 8vo. —*From the Depôt.*

Revue Historique Dirigée. Par M.M. G. Monod et G. Fagniez. Tome Primier, No. 1. 1876. 8vo.—*From the Editors.*

Revue Philosophique de la France et de l'Étranger Dirigée. Par Th. Ribot. Premiere, No. 1. 1876. 8vo.—*From the Editor.*

Philadelphia.—Proceedings of the American Philosophical Society. Vol. XIV., Nos. 94 and 95. 8vo.—*From the Society.*

Transactions of the American Philosophical Society. Vol. XV. New Series, Part 2. 4to.—*From the Society.*

Rome.—Bolletino del Comitato Geologico d'Italia. Nos. 1–6. 8vo.—*From the Society.*

Salem (*U.S.*)—The American Naturalist. Vols. VIII. and IX. 8vo.—*From the Peabody Academy of Science.*

Memoirs of the American Association for the Advancement of Science. Vol. I. 4to.—*From the Association.*

Memoirs of the Peabody Academy of Science. Vol. I. No. 4. 8vo.—*From the Academy.*

Sixth Annual Report of the Trustees of the Peabody Academy of Science, for the year 1873. 8vo.—*From the Academy.*

Sidney.—Proceedings of the Linnean Society of New South Wales. Vol. I. Part 1. 8vo.—*From the Society.*

Stockholm.—Kongliga Svenska Ventenskaps-Akademienes Handlingar Ny Folja Attonde. Bandet 1870–71–73. 4to. —*From the Academy.*

Meteorologiska Jakttagelser i Sverige utgifna of Köngl. Svenska Vetenskaps-Akademiens Anställda och Bearbetade af Er-Edlund. Tolefte Bandet 1870; Trellonde Bandet 1871; Fjertonde Bandet 1872. 4to.—*From the Academy.*

Stockholm.—Ofversigt af Köngl. Vetenskaps Akademiens Förhandlingar Tjugondeattonde. Argangen 1871–1872; Tretionde Argangen 1873; Tretiondeforsla Argangen 1874. 8vo. —*From the Academy.*

Bihang till Köngl. Svenska Vetenskaps-Akademiens Handlingar. Första Bandet, Hafte 1–2; Andra Bandet, Hafte 1–2. 8vo.—*From the Academy.*

Lefnadsteckningar ofver Köngl. Svenska Vetenskaps-Akademienstefter ar 1854, Aflidna Ledamoter. Band I. Hefte 3. 8vo.—*From the Academy.*

St Petersburg.—Annalen Physikalischen Central Observatoriums. 1874. 4to.—*From the Russian Government.*

Bulletin de l'Académie Impériale des Sciences de St Petersbourg. Tome XXI. No. 5. 4to.—*From the Academy.*

Repertorium für Meteorologie. Band IV. 4to.—*From the Royal Academy.*

Tableau General Methodique et Alphabetique des Matieres Contenues dans les Publications de l'Académie Impériale des Sciences de St Pétersbourg. 1872, 1re Partie. 8vo. —*From the Academy.*

Toronto.—Canadian Journal of Science, Literature, and History. Vol. XIV. Nos. 5, 6; Vol. XV. Nos. 1, 2. 8vo.—*From the Editor.*

Turin.—Atti della Reale Accademia della Scienze de Torino. Vol. X. Dispensa 1–8. 8vo.—*From the Academy.*

Bolletini dell Osservatorio della Regia Universita de Torino. 1875. 4to.—*From the University.*

R. Osservatorio Astronomico di Torino Effemride del Sole, Della Luna e die Principali Pianeti calcolate per Torino, in Tempo Medio Civile di Roma, per l'Anno 1876–1877. 8vo.—*From the University.*

Bolletino Meteorologico ed Astronomico del Regio Ossorvatorio della Universita di Torino. 1873–1874. 4to.—*From the University.*

Upsala.—Nova Acta Regiæ Societatis Scientiarum Upsaliensis. Vol. IX. Fasc. 2. 4to.—*From the Society.*

Bulletin Météorologique Mensuel de l'Observatoire de l'Université d'Upsal. Vol. VI. Annee 1874. 4to.—*From the University.*

Utrecht.—Nederlandsch Meteorologisch Jaarboek voor 1870 and 1874. 4to.—*From the Meteorological Institute.*

Verslag van het Verhandelde in de Algemeene Vergadering van het Provinciaal Utrechtsche genootscap van Kunsten en Wetenscheppen. 1874. 8vo.—*From the Society of Sciences and Arts.*

Victoria.—Statistical Register of the Colony of Victoria for 1874. Fol. Population—Part 6. Statistical Register Interchange—Part 5. Reports on the Mining Surveyors and Registrars, 1875.—Fol. Patents and Patentees—Vol. VIII.—4to. Statistics of Friendly Societies, 1874.

Statistical Register of the Colony of Victoria.—Part 7. Accumulation. Fol.—*From the Registrar General.*

Vienna.—Das Gebirge um Hallstatt. Eine Geologisch-Paláontologische Studie aus den Alpen. Abhundlungen der k. k. Geologischen Reichsanstalt. Band VI. Heft 2. Edmund Mojsisvocs v. Mojsvár. 4to.—*From the Society.*

Die Congerien und Paludinenscichten Slavoniens und Eeren Faunen, von D. M. Neumayl und C. M. Paul. Band VII. Heft 3. 4to.—*From the Society.*

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Sitzungsberichte der kaiserlichen Academie der Wissenschaften.—Min. Bot Zool. Geo. Pal. Classe. Band LXX. Hefts 3, 4, 5. LXXI. Hefts 1, bis 5.—Phys. Anat. Classe. Band LXX. Hefts 3, bis 5; LXXI. Hefts 1, 2. —Phil. Hist. Classe. LXXVIII. Hefts 2, 3; LXXX. Hefts 1, 2, 3; LXXIX. Hefts 1, 2, 3.—Math. Nat. Classe. LXX. Hefts 3, 4 bis 5; LXXI. Hefts 1, bis 5. 8vo.—*From the Academy.*

Almanach der Kaiserlichen Academie der Wissenschaften. 1875. 8vo.—*From the Academy.*

Jahrbuch der kaiserlich-königlichen Geologischen Reichsanstalt. Band XXV. Nos. 3, 4; XXVI. No. 1. 8vo.—*From the Society.*

Vienna.—Verhandlungen der kaiserlich-königlichen Zoologisch-botanischen Gesellsschaft in Wien. Band XXV. 8vo.—*From the Society.*

Verhandlungen der kaiserlich-königlichen Geologischen Reichsanstalt. 1875, 1–5; 11, 12, 13. Nos. 14–18. 1876. 8vo.—*From the Society.*

Festschrift zur feier des Funfundzwanzigjahrigen Bestehens k. k. Zoologisch-Botanischen Gesellschaft in Wein. 4to. —*From the Society.*

Warwick.—Fortieth Annual Report (1876) of the Natural History and Archæological Society. 8vo.—*From the Society.*

Proceedings of the Warwickshire Naturalists' and Archæologists' Field Club. 1875. 8vo.—*From the Society.*

Washington.—Annual Report of the Board of Regents of the Smithsonian Institution, for the year 1874. 8vo.—*From the Institution.*

Astronomical and Meteorological Observations made during the year 1873. 4to.—*From the U. S Naval Observatory.*

Bulletin of the United States Geological and Geographical Survey of the Territories. 1875. Nos. 2, 3. 8vo.—*From the Geological Survey.*

Report of the Geographical and Geological Surveys West of the Mississippi. 1875. 8vo.—*From the Geological Survey.*

Report of the United States Geological Survey of the Territories. By F. V. Hayden. Vol. II. 4to.—*From the Geological Survey.*

Annual Report of the United States Geological and Geographical Survey of the Territories, embracing Colorado and parts of adjacent Territories, being a Report of the Exploration for the year 1874. By F. V. Hayden. 8vo. —*From the Geological Survey.*

Watford.—Transactions of the Watford Natural History and Hertfordshire Field Club. Vol. I. Part 1–3. 8vo.—*From the Society.*

Wellington (N.Z.)—Reports on the Durability of New Zealand Timber in Constructive Works. 8vo.—*From the Agent General.*

Wellington (*N.Z.*)—Transactions and Proceedings of the New Zealand Institute. 1875. Vol. VIII. 8vo.—*From the Institute.*

Whitby.—Report of the Literary and Philosophical Society. 1875. 8vo.—*From the Society.*

York.—Annual Report of the Yorkshire Philosophical Society for 1874. 8vo.—*From the Society.*

Zurich.—Schweizerische Meteorologische Beobachtungen. 4to.—*From the Observatory.*

Vierteljahrschrift der Naturförschenden Gesellschaft in Zurich. Neunter Jahrgang, Heft 1–2. 8vo.—*From the Society.*

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. IX. | 1876–77. | No. 96.

Ninety-Fourth Session.

Monday, 27th November 1876.

Sir WILLIAM THOMSON, President, in the Chair.

The following Council were elected :—

President.
Sir WILLIAM THOMSON, Knt., LL.D.

Honorary Vice-Presidents, having passed the Chair.
His Grace the DUKE of ARGYLL.
Sir ROBERT CHRISTISON, Bart., M.D.

Vice-Presidents.
Professor Kelland.
Rev. W. Lindsay Alexander, D.D.
David Stevenson, Esq., C.E.
The Hon Lord Neaves.
The Right Rev. Bishop Cotterill.
Principal Sir Alex. Grant, Bart.

General Secretary—Dr John Hutton Balfour.

Secretaries to Ordinary Meetings.
Professor Tait.
Professor Turner.

Treasurer—David Smith, Esq.

Curator of Library and Museum—Dr Maclagan.

Councillors.
Dr Andrew Fleming.
Dr Charles Morehead.
Alexander Buchan, M.A.
Robert Wyld, LL.D.
Dr Ramsay H. Traquair.
Dr Thomas Harvey.
Dr John G. M'Kendrick.
Dr J. Matthews Duncan.
Sir T. C. Wyville Thomson.
D. Milne Home, LL.D.
Professor Crum Brown.
James Bryce, LL.D.

Monday, 4th December 1876.

The Rev. W. LINDSAY ALEXANDER, D.D., one of the Vice-Presidents, at the request of the Council, gave the following Opening Address :—

GENTLEMEN,—The Council having appointed me to deliver the address at the opening of this the ninety-fourth session of the Royal Society of Edinburgh, I have not felt myself at liberty to decline the appointment, though deeply conscious of my inability to discharge adequately the duty which has thus been laid upon me.

I have, in the first instance, to advert to the changes which have taken place in the fellowship of the Society in the course of the past year. During that period the Society has lost by death *nine* of its Ordinary Fellows and *three* of its Foreign Honorary Fellows. These are by name—

Thomas Login, Esq., C.E., India.
Sir George Harvey, Kt., P.R.S.A.
James Warburton Begbie, M.D., F.R.C.P.E.
Lewis D. B. Gordon, Esq., C.E.
David Bryce, Esq., Architect.
George Stirling Home-Drummond of Blair-Drummond, Esq.
Alexander Russel, Esq.
Thomas Laycock, M.D., F.R.C.P.E.
The Most Noble the Marquis of Tweeddale, K.T., G.C.B., &c.
Adolphe Pictet, Geneva.
Adolphe Theodore Brongniart, Paris.
Christian Gottfried Ehrenberg, Berlin.

Fifteen new members were elected during the past year.

The total number of Fellows of the Society at this date is 419, viz.—

Ordinary Fellows, ...	363
Honorary Fellows, ...	55
Non-resident Fellow under the old law,	1
	419

Following a practice which has grown to be a usage and a rule, I proceed to lay before the Society brief biographical sketches of the deceased members, so far as I have been enabled to gather materials for this purpose. I shall take them in the order in which they have been enumerated above.

Thomas Login,* born at Stromness, in Orkney, in 1823, studied engineering at Dundee. In 1844 he obtained an appointment in the Public Works Department in India, and was engaged from 1847 to 1854 in the construction of the Ganges Canal under the late Sir P. T. Cautley, who has most cordially acknowledged that to Mr Login's advice and assistance "he was greatly indebted in designing and executing that work."

After this period Mr Login was invalided and came to England. In 1857 he returned to India, where he acted successively as executive engineer of the Darjeeling and Roorkee Roads and of the Northern Division of the Ganges Canal. In 1868 he again came to England, returning to India in 1870 to occupy the post of officiating superintending engineer at Umballa, where his labours were varied and arduous, and a return of his illness cut short his useful career on 5th June 1874, while engaged in an inspection of the Thibet Road in the Punjaub.

While resident in India Mr Login made several communications on engineering, among which may be mentioned his paper on the "Benefits of irrigation in India, and on the proper construction of irrigating canals," for which he received a Telford Premium from the Institution of Civil Engineers; his "Description of the Ganges Canal," and recommendations for "Roads, Railways, and Canals for India;" and his paper "On the Delta of the Irrawaddy," communicated to this Society in 1857.

Mr Login was a member of the Institution of Civil Engineers, and was elected a Fellow of this Society in 1857.

George Harvey was a native of St Ninians, near Stirling, where he was born February 1806. His father, a respectable tradesman in that city, and a man of intelligence and piety, trained up his children in the fear of God, and in a strict regard to the claims of

* This sketch has been furnished by David Stevenson, Esq., C.E.

honour and duty, and sent them forth to the business of life well educated and thoroughly imbued with upright and virtuous principles. From an early period George showed that his bent was towards the pictorial art; and his boyish efforts in that direction proved that he had the natural gifts which, under due cultivation, promised to lead to eminence. In order to obtain this necessary culture he came in 1823 to Edinburgh, where he studied at the drawing school connected with the Institution. His first pictures were exhibited in 1826, and attracted much attention. In his earlier efforts he seems to have had Wilkie before him as his model; at any rate, his subjects were selected from scenes of humble Scottish life, such as Wilkie delighted to delineate. These Harvey reproduced with scrupulous truthfulness, and his pictures show a keener sense of the more humorous side of his subject even than those of Wilkie. The scenes he preferred to delineate were such as he had himself witnessed, and the features of which remained vividly impressed on his memory. One of his earliest pictures represented a village school during school hours,—the respectable master engaged in hearing a lesson from a class of boys and girls, and the rest of the pupils employed either in working at their slates or conning their lessons, or in weary vacuity waiting for the season of dismissal, or, as their bent inclined, playing tricks and working mischief out of the master's sight. This picture attracted the attention of an eminent patron of art, who desired to purchase it; but Harvey, having promised it to a friend, would not consent to sell it, even though his friend urged him not to lose the advantage, so important to a beginner, of getting his picture placed in the gallery of a celebrated collector. To the village school Harvey resorted oftener than once, even in the more advanced stages of his career, for the subject of a picture, as his well-known pictures of "The Examination" and "The Skule Skailin'" show. Other scenes of ordinary Scottish life were depicted by him at this time, such as "The Leisure Hour," "Disputing the Billet," "The Small-Debt Court," and in later years his "Curlers," his "Highland Funeral," his "Penny Bank," show the undying interest which the habits, pursuits, and manners of his countrymen had for him. As a religious man, the religious history of his country could not fail powerfully to engage his regards, and in connection with this some of the noblest efforts of his pencil were produced; his Covenanter pictures—The Preaching, Baptism, and Communion, as well as "The Battle of Drumclog"—attesting how deep was his sympathy with those who in evil days had to seek their spiritual sustenance and contend for their spiritual liberty at the peril of their lives; while his "Reading of the Bible in Old St Paul's," his "Bunyan in Prison," and his "Bunyan selling laces on Bedford Bridge," show that it was not to the religious history of his own country, or the struggles and sufferings of his own countrymen, that his sympathies were restricted. In the ecclesiastical movements of his own time, also, he took a deep interest; and his "Quitting of the Manse" remains to show how he could appreciate a noble sacrifice for conscience' sake on the part of those with whom he himself had no ecclesiastical connection. In the wider field of general historical painting Harvey did not attempt the delineation of great and stirring events, but contented himself with depicting scenes and actions of individual life or personal enterprise. As among his most powerful efforts in this wider field of his art may be mentioned his "Dawn revealing the New World to Columbus," his "Shakespeare before Sir Thomas Lucy," his "Robbers melting Plate," his "Castaway," and his "Dr Guthrie Preaching in the Highlands." He was fond also of painting groups of children, whose ways he had carefully observed, into whose affections and sympathies he lovingly entered, and from whose mimic sports he could draw lessons which by his pencil he sought to impress on older folks. It is only necessary to name his "Children blowing Bubbles in Greyfriars' Churchyard," and his "Wise and Foolish Builders," to illustrate his success in this department of his art. In his later years he betook himself to the painting of landscapes; and here, in the judgment of those most qualified to judge, he was at his best. As a delineator of Scottish pastoral scenery, whether in the Lowlands or in the Highlands, he in many respects stands without a rival. In portrait-painting he was less successful; still some noble portraits, that, for instance, of the late Professor Wilson, came from his easel; and in several of his historical pictures characteristic likenesses of eminent men living at the time are introduced.

I cannot pretend to offer a critical estimate of Harvey's merits as an artist. Even an unskilled observer, however, could not fail to be struck with the more prominent qualities of his works,—the truthfulness of representation, the clearness of conception, the power and precision of execution, and the freshness and thoughtfulness by which his pictures are characterised. A peculiar excellence attaching to the productions of his pencil is derived from their high moral character. Of his historical paintings each depicts some scene of deep moral interest, or illustrates and enforces some great moral or religious lesson; and when he ceased to occupy himself with such subjects, it was to Nature in her serener and grander aspects that he turned, or among the dwellers in quiet pastoral regions that he sought the objects of his art. In scenes of calm natural beauty, amidst the solemn silence of the everlasting hills, he delighted to roam, and such scenes he sought especially to transfer to his canvas. They lifted up his own soul to God, and he sought by his art to make them the means of producing the same effect on others. He delighted also to depict scenes in the common life of men, scenes which had powerfully touched that chord of human sympathy which so strongly vibrated in his own soul. With a deep sense of humour, and with an eye for the ludicrous both in form and action, he never stooped to cater for the mere amusement of the public, nor did he ever use his skill in such a way as to offend the sensibilities of the least refined. Nothing mean, nothing trivial, nothing fantastic, engaged his pencil. In all he did a high moral purpose is discernible, and his works, if they have secured for him the reputation of a great artist, no less commend him as a great moral teacher. This is wholly in keeping with his general character. Endowed with genius and keen susceptibilities, he was at the same time a man of high principle, of a vigorous and manly intellect, of simple and natural tastes, of broad sympathies, of warm affections, and of a kindly and genial spirit.

Not long after exhibiting his first pictures Harvey became an Associate of the Academy. From that time forward he took a lively interest in that Institution, and to his zeal and energy it was in no small measure indebted for its establishment and early success. In 1870 he appeared as its historian in a volume in which the origin and progress of the Academy are narrated, and many interesting documents connected with its history are given; the volume is entitled "Notes on the Early History of the Royal Scottish Academy." In due time he became an Academician and a member of Council, and on the death of the late Sir John Watson Gordon he succeeded him as President of the Academy. Some time afterwards he received the honour of knighthood from the Queen.

Endowed with a constitution robust and sound, Sir George Harvey was able up to an advanced period of life to pursue without serious interruption the work of his profession. As he approached his seventieth year, however, his health began to give way, and symptoms of a serious kind soon showed themselves. In the spring of 1874 his illness assumed so serious a form that his life was despaired of; and though he rallied so far as to be able to leave his room, take short drives, and receive his more intimate friends, it became increasingly manifest that his work was done, and that his earthly career must soon reach its close. Calmly, serenely, surrounded by loving friends, and tended with affectionate solicitude by those nearest and dearest to him, he for several months awaited his end. This came to him on the evening of the 22d of January 1876, when he peacefully breathed his last.

Sir George Harvey became a Fellow of this Society in 1867.

James Warburton Begbie was born at Edinburgh on the 19th of November 1826. He was the second son of Dr James Begbie, for many years well known and held in much repute as a consulting physician in this city. Destined to follow his father's profession, young Begbie was carefully educated with that view. His professional studies were prosecuted at the University and the Surgeons' Hall, Edinburgh, and afterwards at several of the continental schools, including those of Paris, Vienna, and Italy; and he had all along, while pursuing his studies, the advantage of his father's constant superintendence, instruction, and counsel. His degree of M.D. was taken in 1847, and in 1852 he was elected a Fellow of the Royal College of Physicians. In 1855 he was appointed a physician and clinical lecturer in the Royal Infirmary; and much about the same time he began to lecture on the practice of physic in the Extra-Academical School. His period of service at the Infirmary expired in 1865, and at this time he gave up lecturing, as his private practice had so increased that he could no longer give the time required for this. For the same reason he was obliged to resign the office of Examiner in Medicine for the Edinburgh University, to which he had been appointed in 1869. On relinquishing his connection with the Infirmary he devoted himself to private practice as a physician; but after the death of his father, which took place in 1870, he found it necessary to retire from ordinary practice and devote himself to the work of a consulting physician. In this capacity he continued to labour till the close of his life; and in it he attained a position which has been described as almost unique in his profession, his consulting room, on days when he was to be seen at home, being crowded with patients of all classes, and his services being in request, not only in all parts of Scotland, but frequently also in England. For this he was indebted, in some degree, to the urbanity and kindliness of his manner, in a greater degree to the excellent footing on which he stood with members of his own profession, but most of all to his undoubted skill, knowledge, and experience as a physician.

In the midst of his professional engagements, Dr Warburton Begbie found time to furnish several valuable contributions to the literature and science of his profession. These appeared, for the most part, in the pages of the "Edinburgh Medical Journal," to which he was for many years a frequent contributor. He published also, at an early stage in his career, a little work, entitled "Handy Book of Medical Information and Advice. By a Physician." This, though published anonymously, speedily obtained an extensive circulation, which was much increased when the authorship of the work became generally known. In the literature of his profession he was deeply versed, and his extensive knowledge of the history of medicine enabled him to illustrate his own writings by appropriate citations from his predecessors in the same field of inquiry and observation. To his other accomplishments he added that of being an excellent linguist; with the classical writers of Greece and Rome he was familiar, and he was able to converse freely in several of the modern European tongues. As a lecturer on medicine he was distinguished by the accuracy and fulness of his knowledge, by the perspicuity of his style, and by the minuteness with which he entered into the exposition of his subject, both in its theoretical aspects and in its practical details; and the same features are discernible in the published productions of his pen. When the British Medical Association met in Edinburgh in the autumn of 1875 Dr Begbie was selected to act as orator of a section of that body, and on that occasion he delivered an address on the practice of medicine in ancient and modern times, which displayed all the excellent qualities of his manner as a lecturer and a writer, and was not only well received by his professional brethren, but welcomed with hearty applause by the general public.

The incessant demands made upon him in the exercise of his profession, and his unsparing devotion to the case of all who sought his aid, his innumerable and often exhausting journeys, and the toil, physical and mental, which he endured, began to tell upon a constitution originally sound and healthy but not remarkably robust. For some years before his death he had, at intervals, suffered from weakness in the action of the heart; and these attacks had come to be so frequent that he found it necessary, in the beginning of the present year, to seek relief by retiring for a season from the active duties of his profession. With this view he left Edinburgh in the beginning of February last, intending to proceed by easy stages to the south of England and ultimately to Italy. He had not, however, reached farther than to Carlisle, when the symptoms of his illness showed themselves in so aggravated a form that he saw his case had become one of imminent danger. He accordingly returned home. Here, at first, he seemed to rally, but after a few days his strength gradually sank, and though several of the most eminent of the medical practitioners in the city were in constant attendance upon him, it soon became evident that a fatal issue could not be averted. He died on Friday the 26th of February 1876. His funeral took place on the following Thursday, and was attended by some of the most distinguished members of the legal and medical professions, and by a large company of the citizens.

Dr Warburton Begbie had all the qualifications of a great physician. To natural abilities and excellent general culture he added profound knowledge of his profession, extensive experience in the practice of it, great skill in the discernment of disease, and a happy facility in suggesting fitting methods of treatment. His high moral character also, and the known conscientiousness with which he devoted himself to the case of those who consulted him, tended to inspire confidence in him on the part of the public, as well as to encourage his professional brethren to appeal to his advice in cases of emergency; nor were there wanting that "*hilaris vultus*," that kindly demeanour, and that gentle manner, which Celsus tells us are such excellent qualities in the "peritus medicus."* Pleasant looks and cheerful words will not of themselves, it is true, effect a cure; but every experienced physician knows how materially they are helpful to the end he seeks to gain with his patient:—

> "Sunt verba et voces quibus hunc lenire dolorum
> Possis, et magnam morbi depellere partem"†

Few men have passed away more sincerely lamented than was Dr Warburton Begbie, as well by his professional brethren as by the general public. As a skilled physician, as a man of varied culture, high moral character and amiable manners, as a sincere Christian and a generous benefactor to the poor, he has left behind him a reputation which will not soon be obliterated.

Dr Begbie became a Fellow of this Society in 1870.

Lewis D. B. Gordon,‡ son of Joseph Gordon, writer to the Signet, was born at Edinburgh in 1815, and received his early education at the High School and University of Edinburgh.

Having determined to follow the profession of engineering, young Gordon was first sent to Dundee, where he had the benefit of working at the bench and studying engine-fitting at the Dundee Foundry. His next introduction to practical work was at the Thames Tunnel, where, by the kindness of Mr Brunel, he had an opportunity of seeing all the details of that unique work. Finally, he completed his studies at the Royal Mining Academy, Fribourg, and the Ecole Polytechnique at Paris.

* Periti medici est non protinus ut venit apprehendere manu brachium, sed primum residere hilari vultu, percunctarique quemadmodum se habeat, et si quis ejus metus est eum probabili sermone lenire.—Cels. *De Re Medica*, b. iii. c. 6.

† Hor. Epist. I. 1. 35.

‡ This sketch has been contributed by D. Stevenson, Esq., C.E.

On his return to Scotland he commenced practice as a civil engineer in Glasgow, in partnership with Mr Lawrence Hill.

In 1840 the Government determined to establish a professorship of civil engineering in the University of Glasgow, for which Gordon became a candidate, and so high were his recommendations that he received the appointment. There can be no doubt that at so young an age, and with the strict sense of duty which ever animated him, Gordon felt the task of organising the new chair to be one that called forth all his energies. No man could be more fully alive to the importance of his new office, or knew better the large amount of knowledge—scientific and practical—that was required of its occupant, and his sensitive mind felt the responsibility he had undertaken very keenly. But he had a spirit that was not easily daunted by difficulties, and in anticipation of his appointed work he produced the syllabus of a course of study, embracing a very wide field of engineering, under the following general heads:—

The Mechanical Effect produced by Forces and its Measure.
Physical and Mechanical Properties of Materials.
Results of Experiments on the Resistance of Materials.
Friction.
Doctrine of Mechanics.
Animal-power and its Recipient Machines.
Water-power and its Recipient Machines.
Steam-power and the Steam-engine.

After delivering his lectures he had the satisfaction to find that he had got through the first session with comfort to himself and profit to his pupils.

It may here be noticed that the skeleton syllabus of his opening course of lectures was afterwards, in more matured form, published in 1847, under the unassuming title of "Engineering Aphorisms and Memoranda," and ultimately, in 1849, with further additions, it was published, under the title of "A Synopsis of Lectures on Civil Engineering and Mechanics."

He, however, knew that he had to *make* his professional character as an engineer, and that this was not possible were all his energies to be given to his professorial duties, and therefore, during the time he occupied the chair, he invariably, when the session was completed, devoted his whole time to the practice of his profession as an engineer,—first in conjunction with Mr Lawrence Hill, and latterly with Messrs Liddell and Newall, with whom he entered into a second copartnery.

He ultimately found, as his engineering business increased, that he could not fulfil the duties of his chair to his own satisfation, and, in 1855, he resigned his professorship, in which he was succeeded by the late Professor Rankine.

During the time of his first partnership he, in conjunction with Mr Hill, was employed in general engineering business in Scotland, and reference may, in particular, be made to the investigation for the water supply of Glasgow in 1845, when they came to the conclusion "that the nearest adequate supply of pure water, that can be brought to Glasgow by gravitation, is what is afforded by the overflow of Loch Katrine." This project was revived in 1852 by Professor Rankine and Mr John Thomson, and was ultimately, as is well known, successfully carried out by Mr J. F. Bateman. Among other works, Messrs Gordon and Hill were employed in advising the Marquis of Breadalbane in his mining operations at Tyndrum, and in constructing the great chimney for Messrs Tennants' works at St Rollox, which was, at that early period, considered to be a work as bold as it was successful.

But it was in connection with Messrs Liddell and Newall that most of Gordon's engineering work was done. Liddell and Gordon were engineers for several railways in England and Wales, and designed and executed many iron bridges, among which may be mentioned the Hereford, the Usk, and especially the Crumlin Viaduct in South Wales, consisting of 10 spans of 150 feet,—a structure of marvellous lightness, and withal of requisite strength and rigidity. Their firm was rendered famous by the introduction of wire ropes, which Gordon had seen in use at the mines in Germany, and introduced into England in 1840, under a patent taken out by Gordon, Newall, and Liddell. The uses of these ropes became highly important when they were ultimately so largely employed in protecting the electric wires for submarine cables,—a new and large field of work was thus opened up for the firm, which was designated R. S Newall and Co. of Gateshead. The firm manufactured and laid upwards of 4500 miles of cable in different parts of the world. It was on one of his numerous voyages in connection with marine telegraphs that he, in 1859, had the misfortune to be shipwrecked in the Red Sea, when he suffered exposure on a barren rock for four days, and contracted illness from which he never recovered. Latterly he suffered from paralysis, which for many years prevented his engaging in active business.

In preparing his lectures for the Glasgow professorship, Gordon felt that at his early age he had much knowledge to acquire, and he seems to have had no difficulty in giving the result of his investigations to the Glasgow Philosophical Society, of which he was a member, and to which he communicated, between 1840 and 1844, many papers.

In 1841, in a paper "On the Determination of the Melting Points of Metals," he gave an account of the experiments of Plattner of Fribourg. In the same year, under the title of "Dynamometrical Apparatus," he detailed the investigations of M. Morin of Metz, and, under the "Temperature of the Earth," he gave an elaborate account of the thermometric observations of Forbes and Herr Dove. He also made communications on "The Flow of Water through Pipes," the "Measure of Impact by Pressure or Weight," and other subjects of interest to the Society.

Of papers and pamphlets, on subjects of general engineering, the following imperfect list may be given:—

"Description of the Great Chimney of St Rollox at Glasgow." 1844.

"On the Supply of the city of Glasgow with Water from Loch Katrine." 1845.

"Railway Economy,—an Exposition of the advantages of Locomotion by Locomotive Carriages instead of the present system of Steam-tugs." 1849.

"Railway Economy,—Use of Counter-pressure Steam in the Locomotive Engine as a Brake. Translated from M. L. Le Chatelier, Ingenieur en Chef des Mines." 1869.

"Exposition of a Plan for a Metropolitan Water Supply."

"On the most Advantageous Use of Steam." 1845.

"Short Description of the Plans of Captain J. Vetch, R.E., for the Sewerage of the Metropolis." 1851.

In 1848 he translated from the German the "Principles of the Mechanics of Machinery and Engineering," by J. Weisbach, of the Royal Mining Academy, Fribourg; and as a supplement added some original appendices "On the Strength of Materials," "Tubular Bridges," and the "Rigidity of Cordage."

Even after being laid aside from active business he continued to take a lively interest in engineering, and never failed to answer, in carefully considered letters, whatever his friends in the profession submitted for his friendly advice, which was always promptly and ungrudgingly given.

In proof of what I may call the *disinterested interest* he took in his profession and his professional brethren, it is pleasant to know that the latest work in which Gordon was engaged was a labour of love and regard for the memory of a professional brother.

About the close of 1875 it occurred to Mr James R. Napier of Glasgow and Gordon, that a memoir of the late Professor Rankine, and a republication of his contributions on scientific subjects to Societies and journals would be a task agreeable to his friends, useful to his former pupils, and acceptable to men of science. His last letter to Mr Napier on the subject is dated 24th February 1876, just two months before his death. But his correspondence, which had been going on three months, then ceased, and his friendly desire—so like his nature—to give his time and strength as one of the editors of the works of his successor in the chair of engineering at Glasgow came to an end. He died at Poynters Grove, near London, on the 28th April 1876. He became a Fellow of the Society in 1845.

Resignation to the will of God was the ruling principle of the last years of Gordon's long illness, and this gave cheerfulness to his daily intercourse, which was the admiration, and indeed the envy, of the many friends who now lament his death.

David Bryce was a native of Edinburgh, where he was born in 1803, and where he received his early education, principally at the High School. The son of an architect he determined to follow his father's profession, for which from an early age he had shown special aptitude. After serving an apprenticeship in his father's office, where he laid the foundation of his future eminence as a designer, and acquired that technical skill by which he was distinguished, he while yet young became assistant to Mr Burn, a well-known architect, by whom he was soon after received into partnership. On Mr Burn's removal to London in 1844, Mr Bryce commenced business for himself, and soon earned a high reputation in his profession. For a short time he was in partnership with Mr Robert Anderson, and a few years before his death he was joined by his nephew, Mr John Bryce, in conjunction with whom he carried on business to the close of his life.

During his lengthened career Mr Bryce executed many important works, which remain to attest his ability and skill. In style he was cosmopolitan, and, accordingly, the buildings he designed are in various styles of architecture. In Edinburgh the Life Assurance Company's Office, the Scottish Widows' Fund Office (originally the Western Bank), the Subscription Library, the Surgical Hospital, the Standard Assurance Office, the Sheriff Courts, the Union Bank and Free St George's Church are in the Italian style, as are also the Western Bank and the Scottish Widows' Fund Office in Glasgow; the British Linen Company's Bank and the Clydesdale Bank, in Edinburgh, are in the Palladian manner; and the Fettes College, Edinburgh, and the Dundee Exchange, are in French Gothic. For churches he generally adopted the Gothic, rarely the Norman, and still more rarely the Italian. For a few public buildings he employed the old Scottish style, as in the New Royal Infirmary, Edinburgh, and the Sheriff Courts, Kirkwall; but he reserved this chiefly for private mansions. Of these he erected and altered a vast number throughout the country; indeed, it has been affirmed on good authority, that "perhaps no man in the kingdom has altered more mansion-houses than Mr Bryce."* Here, it may be said, his chief success was achieved, and here his peculiar genius was chiefly manifested. "Many an inconvenient, comfortless dwelling has been by him converted into one of the most comfortable residences, and many a tame, uninteresting, commonplace mansion rendered a picturesque feature in the landscape."† Of the houses of which he was the architect the most noteworthy are Cortachy House, the seat of the Earl of Airlie; Glen House, the property of Mr C. Tennent; Ballinkinrain House, the property of Mr Orr-Ewing, M.P.; Hartrig House, near Jedburgh, the residence of Lord Campbell; Castlemilk, near Lockerbie, the property of Mr

* "The Builder," for May 27, 1876, p. 507. † Ibid.

Robert Jardine; Kimmerghame, the residence of Mr Campbell Swinton; Broadstone, the residence of Mr J. Birkmyre; and Woodcroft, the residence of Colonel Davidson. All these are in the old baronial style. In other styles are Panmure House (Elizabethan); Langton, near Dunse, the seat of Lady Elizabeth Pringle (Elizabethan); Portmore, the residence of Mr Mackenzie (Elizabethan); Kinnaird Castle (French); Belladrum, the property of Mr Merry, M.P. (French); Eastburgh, the residence of Mr Carnegy (French); and Kincaid Castle, the seat of the Earl of Southesk. In adopting these different styles Mr Bryce was not a mere copyist; he impressed upon all his work the stamp of his own taste and genius. Essentially an artist, he had a fine perception of harmony, and he sought always to bring his buildings into fitness for the place they had to occupy, and into harmony with the surrounding scenery, so as to make them part of one great picture. "When he had a work on hand where anything like scope was allowed to his powers, he wrought upon it as a painter does upon his easel, recurring to it again and again, altering proportions and rearranging the grouping of masses, and in doing this he did not hesitate occasionally to obliterate what had cost him much labour."*

Mr Bryce stood high in his profession, and his name will remain, along with those of Adam, Hamilton, and Playfair, among those of the greatest of modern architects. He was Grand Architect for Scotland, a Fellow of the Royal Institute of British Architects, and a member of the Royal Scottish Academy, as well as a Fellow of this Society. He was a man of varied acquirements, of high integrity, and of a genial disposition. Under a blunt manner, and somewhat rough exterior, there lay in him a kindly nature and a generous heart; and, whilst his society was much sought after by his friends, he drew to him the esteem alike of his servants and his employers.

He died on the 7th of May last, in his seventy-fourth year. He became a Fellow of the Royal Society in 1856.

George Stirling Home-Drummond was the eldest son of the late Henry Home-Drummond, Esq. of Blair-Drummond, and great-grandson of the celebrated Henry Home Lord Kames. His father was for many years member of Parliament, first, before the passing

* "The Builder," as above.

of the Reform Bill, for Stirlingshire, subsequently for Perthshire; in which capacity he rendered important services, particularly by the laws which he procured to be enacted, of which the Act for the Regulation of Public Houses in Scotland, and that for the Small Debts Jurisdiction in the Sheriff Courts, may be mentioned as specially valuable. He died in 1867, and was then succeeded in his estates by his son, the subject of this notice. This gentleman was born in Edinburgh on the 1st of March 1813. He received his university education at Oxford, where he was entered at Christ Church College. Some time after leaving the university, he travelled through Palestine and other parts of the East. Of this tour he wrote an account, which was printed for private circulation, but never published.

Mr Home-Drummond was a man of extensive culture and varied pursuits. With the languages and literature of ancient Greece and Rome he was familiar; of several of the languages of modern Europe he was accurately master, especially French and Italian, which he wrote and spoke with ease and fluency; and in several branches of natural science, he was proficient. He was chiefly interested in antiquarian research, and became especially skilled in the deciphering of ancient documents. One of the fruits of his labours in this field was the collection of a mass of notes and papers relative to the Earldom of Monteith, which, it is understood, is now in the possession of a learned legal antiquary, with the view of being used in the preparation of a historical volume.

Though interested in literary and scientific pursuits, Mr Home-Drummond did not neglect the duties of a large landholder and country gentleman. In this capacity as well as in the relations of private life, he was much respected and esteemed. He died somewhat suddenly in London, on the 3d of June in the present year. He was elected a Fellow of this Society in 1869. He was also a Fellow of the Society of Scottish Antiquaries and a Fellow of the Geological Society.

Alexander Russel was a native of Edinburgh, where he was born on the 10th of December 1814. His father was a solicitor practising in that city, and his mother, from whom it is said he derived much of his mental vigour and character, was the daughter of a Mr Somerville, whose interest in politics and stanch adherence to his party may be said also to have been inherited by his descendant. After receiving education at several schools in Edinburgh Mr Russel was apprenticed to the printing trade with Mr John Johnstone, a gentleman well known in connection with literature as an editor and author still more than as a printer. To young Russel the acquaintance and society of this gentleman and his gifted and accomplished wife were of great advantage. They soon discovered his abilities, and they did much to foster his taste for literature as well as to direct his studies. By them he was encouraged to the use of his pen in composition, and several of his early essays were introduced by Mrs Johnstone into "Tait's Magazine," of which she was at that time the editor. From the outset, however, his bent was to politics rather than to literature; and having been at an unusually early age appointed editor of the "Berwick Advertiser," he was enabled thenceforward to devote himself to a career to which his tastes inclined him, and for which he was by natural abilities and acquired facilities and resources peculiarly fitted. After some years he exchanged the editorship of the "Advertiser" for that of the "Fife Herald," which post he continued to hold till towards the close of 1844. By this time his reputation as an able writer and skilful editor was established; and in 1845 he, after a connection of short duration with a paper in Kilmarnock, was invited to become the colleague of the late Mr Maclaren as editor of the "Scotsman," then, as it is still, the leading political journal in Scotland. In connection with Mr Maclaren he continued to edit this journal, taking upon him almost the entire management, until 1849, when Mr Maclaren retired, and Mr Russel became sole editor. In this position he remained up to the time of his death, though latterly, through failing health, he was obliged to devolve upon others much of that work which for many years he had with Herculean vigour done alone.

The "Scotsman" newspaper is, as is well known, the recognised principal organ of the Whig party in Scotland; and for the advocacy and diffusion of the principles held by that party it was designed. Of that party Mr Russel was a stanch adherent, of its principles he was the bold, uncompromising, and consistent advocate, and its interests he zealously laboured to advance. The services he rendered to his party were immense. Nor were the members of the party slow to acknowledge this; as was attested in many ways, but especially by the testimonial which was presented to him in 1859, "in recognition of his services and as a mark of respect for his honourable and independent conduct in public and private life," and by his being spontaneously elected a member of the Reform Club in London, which has been described as "the central organisation of the Liberal party." Nor was it by persons of that party alone that his merits were acknowledged, and the tribute of respect to his consistency and integrity rendered. Perhaps no provincial newspaper was so generally read by men of all parties as the "Scotsman" whilst under Mr Russel's management; and however much some of its readers might dissent from his opinions or dislike his principles, there were none who did not admire the boldness, the steadfastness, and the consummate ability with which these were maintained and advocated. For the conduct of a public journal he was eminently qualified as well by the brilliancy of his faculties as by the extent and variety of his knowledge, his sound sense, and the manly vigour which characterised all his utterances on questions of public interest. Of him may it be said, with much greater justice than of the person to whom Addison first addressed the line, that he was

> "For wit, for humour, and for judgment famed."

If sometimes his wit was over keen, if now and then in his humour there was a flavour of coarseness, if occasionally he jested where jesting was "not convenient," if the showers of ridicule with which he assailed the objects of his criticism were often more copious than deserved, there is this to be said on the other side, that his judgment was seldom at fault, that in his satire there was no malevolence, that his antagonism was open and manly, that he scorned to use any base arts of insinuation, vituperation, or slander, and that when he wielded the scourge there was so much cleverness in his manipulation, such felicity of expression and illustration, and withal such a joyous hilarity pervading the whole, that even those who were the objects of his most pungent strictures found it impossible to withhold their admiration, or to resent with bitterness the castigation they had received.

Those who have been born and brought up in towns, and whose avocations restrict them to a residence in "the busy haunts of men," have often little or no relish for natural scenery, and no inclination towards the pursuits or pastimes of country life. Dr Johnson, it is well known, thought Fleet Street much to be preferred to Greenwich Park, and maintained that no man would live in the country who could help it. Boswell, in recording this with approval, "shelters" himself under the authority of one whom he describes as a man of fashion, but distinguished also by a love of literature, who declared that he preferred the smell of a flambeau at the playhouse to the fragrance of a May evening in the country.* Madame de Stael, when an enthusiastic friend was expatiating on the delight which such a heart as hers must take in green fields and gentle streams, replied—"Ah! il n'y a pour moi de ruisseau qui vaille celui de la Rue de Bac," a street in the Faubourg St Germain, through which a paltry rivulet flows. There are many who, without having a distaste for the country so pronounced as this, are yet supremely indifferent to it, and either never care to visit it, or if they are seduced into it, are never happy till they turn their back on it and find themselves once more among streets and houses. Mr Russel was not of this class. Though born in a town, and from his youth up a dweller in towns, he had a passionate love for the country, and found no relief from his toils so refreshing and exhilarating as a ramble among the hills and by the streams of his native land. He was also enthusiastically devoted to angling, and there are few of the angling waters of Scotland with which he was not acquainted. With the Gala and the Tweed he was especially familiar, and often sought on their banks that recreation which their streams afforded to him in the pursuit of his favourite pastime. With the habits of the tenants of the waters he was careful to make himself acquainted, and he knew them all well, from the trout to the salmon. His observations were embodied in an article which appeared in the "Quarterly Review," and which he afterwards expanded into a volume; and on the subject of fishing he became so much of an authority, that he was repeatedly examined before Parliamentary Commissions appointed to inquire into this matter. Besides tra-

* Life of Dr Johnson, i. p. 438, 8vo edit. Lond. 1807.

versing widely his own country, Mr Russel in 1850 made a tour in Ireland; in 1863 he for the first time visited the Continent; and in 1869 he went to Egypt, and was a witness of the ceremony at the opening of the Suez Canal. In 1872, after his first serious illness, he made a lengthened tour through the south of Europe, visiting several places in France, Portugal, Spain, and Northern Italy.

The illness under which Mr Russel suffered was a form of heart disease, and by this he was for some years before his death so seriously affected that he was obliged to retire to a great extent from the literary work of the "Scotsman." Towards the end of last year the attacks became more frequent and severe, and his strong constitution at length sank under them. He died on the 18th of July last.

Mr Russel became a Fellow of the Royal Society in 1870.

Thomas Laycock was born at Witherby, in Yorkshire, on the 10th of August 1812. He was the son of a Wesleyan minister, and received his early education at the Wesleyan Academy, Woodhouse Grove. Destined for the medical profession, he was, when fifteen years of age, apprenticed to Mr Spence, surgeon, of Bedale. He afterwards prosecuted professional studies at University College, London, where he followed the full curriculum; subsequently, he went to Paris, where he studied under Velpeau and Lisfranc; and from this to Göttingen, where he took his M.D. degree "summâ cum laude." On his return from the Continent he settled at York as a general medical practitioner. In 1841 he was appointed physician to the York Dispensary; in 1844 he acted as Statistical Secretary to the British Association, which met that year at York; and in 1846 he became Lecturer on the Theory and Practice of Medicine in the York Medical School. On the retirement of the late Professor Alison in 1855, he was elected Professor of the Practice of Medicine and of Clinical Medicine in the University of Edinburgh. In 1869 he was appointed Physician to the Queen in Scotland. He was a Fellow of the Royal College of Physicians as well as of the Royal Society of Edinburgh.

Endowed with great mental ability, and possessed of indomitable energy, Dr Laycock contributed largely to the literature and science of his profession. He was an excellent linguist, and kept himself abreast of the most important advances in medical science and speculation on the Continent. He translated Prochasta's Treatise on the Nervous System and Unger's Physiology. His own contributions to various branches of medical science were many, and most of great value. Besides numerous papers in the medical journals and in the transactions of learned societies, he published a Treatise on the Nervous Diseases of Women, in which he first developed his views as to the scientific data of unconscious and involuntary brain function, and explained thereby the phenomena of mesmerism, dreaming, and insanity. The views advanced in this work he afterwards extended and completed as a system of Practical Philosophy, in a work in two volumes, entitled "Mind and Brain, or the Correlation of Consciousness and Organisation," of which the first edition appeared in 1860 and the second in 1869. He also devoted much attention to the subject of epidemiology, and his papers on this and on questions of sanitary reform contributed much to turn attention to the important matter of public health, and to direct to the use of measures best fitted to secure and promote it.

The natural bent of Dr Laycock's mind was towards speculation and theory; but he had what many theorists and speculative thinkers have not, a capacity of minute observation and a patience of details which have enabled him to give to his writings a value and importance independent of the theories which they are intended to advocate. As to the soundness of these theories there may be, and I presume are, differences of opinion, but there can be but one opinion as to the conscientious carefulness and exactitude of his observations, and as to the value of the facts which he has collected and described. His doctrine concerning the reflex function of the brain is now, I believe, universally recognised by physiologists, and its importance both in a scientific and a practical relation acknowledged to be great. To the subject of mental disease Dr Laycock devoted much attention; and he did not a little to advance the science of mental pathology, and thereby to place on a firmer basis the theory of the treatment of the insane. As a professor his aim was not merely to communicate knowledge to his students, but still more to awaken them to thought, to stimulate them to inquiry, and to urge them to use their faculties in the independent pursuit of truth. Complaints have sometimes been made of his want of lucidity as a lecturer, but the fault here may have been rather on the part of the student than on the part of the professor; for where the mind has not been previously trained to processes of thinking, the most lucid exposition of doctrines which are speculative and abstract will fail of conveying to the hearer a clear and just conception of the teacher's meaning. Dr Laycock's lectures were always thoughtful and instructive, and if they often required an effort of close attention and thought on the part of the student to apprehend them, this was never rendered without great advantage to the student thence accruing.

In outward manner Dr Laycock was somewhat cold and formal, and there was in his addresss what had the appearance, though slight, of hauteur. But under all this there lay an extreme kindliness of disposition, which manifested itself in many acts of generous beneficence and tender sympathy.

For some years before his death Dr Laycock's health had begun to fail. In 1857 he had been visited with a threatening of phthisis; and though he seemed entirely to recover from this, the insidious malady continued lurking in his system. In 1866 disease of the left knee-joint rendered amputation of that limb necessary; and this gave a shock to his system from which he never wholly recovered. He was able, however, in the enjoyment of an apparently fair measure of health and strength, to continue his ordinary avocations and to fulfil his professorial functions up to the close of last winter session, when an accession of his old malady laid him aside. As the summer advanced pulmonary consumption gradually extended its ravages in his frame, and on the 21st of September he breathed his last.

Dr Laycock became a Fellow of this Society in 1856, the year after his appointment to the Chair in the University.

George Hay, Marquis of Tweeddale, was born at Yester House, on the 1st of February 1787; and succeeded his father as eighth marquis in 1804. In the same year he entered the army, and he was engaged in active service during the whole of the Peninsular War. He served as aid-de-camp to the Duke of Wellington, by whom he was highly esteemed as a brave soldier and an able officer. He afterwards served in Canada. He was wounded at the battle of

Busaco, and he received other wounds in subsequent engagements; and when, in 1814, he returned home invalided, he was so lame from the effects of his wounds that he was obliged to use crutches. As a young man he was remarkable for his great strength, and many stories are told of his powers both on the field and in pugilistic encounters. He wielded a sabre longer by several inches than the regulation standard; he was an excellent horseman, and a notable whip. It is recorded of him that he once drove the mail coach from London to Haddington without leaving the box or resigning the reins for a single stage. Even in extreme old age he was to be seen on the streets of this city driving a pair of spirited horses with the vigour and skill of an experienced charioteer.

In 1842 the Marquis went out to India as Governor and Commander-in-Chief at Madras. Here he remained for six years faithfully discharging the duties of his high and responsible office. As one who had been trained under Wellington he could not but set himself to restrain and cure the lax discipline which had been allowed to creep into the Indian army; and the severity of some of the measures he took with this view exposed him to no small obloquy and censure. Events, however, proved that he was right in the course he pursued, and that his action was alike necessary and salutary.

On his retirement from active service in the army the Marquis devoted himself to the duties and occupations of a country gentleman, and these he continued to pursue to the end of his life with the exception of the six years he was in India. He engaged largely in agricultural pursuits, retaining in his own hands an extensive farm, which he cultivated with the utmost assiduity, sparing no expense of money, or labour, or skill, not only to render it productive to the highest possible degree, but to make it a model of scientific farming, from which others engaged in the same pursuit might derive profitable instruction. He introduced many important improvements both in the practice of farming and in the implements used in agriculture; so great, indeed, were the improvements he introduced, both in the methods and in the means of tillage, that he may be said to have revolutionised Scottish agriculture. In 1836 he received the gold medal of the Highland and Agricultural Society for the invention of a machine for the making of tiles to be used in drainage. He invented also the Tweeddale plough, an implement which has been found of immense use, as enabling the cultivator with the minimum of draught to turn up the subsoil so as to break it up and subject it to the action of the atmosphere. The system of deep ploughing which he introduced prepared the way for the use of the steam-plough, which he was the first to bring under the notice of the Society and to introduce into general use. As has been justly said, "the agricultural world was under a deep obligation to the Marquis for the time, research, and large pecuniary expenditure he devoted to the practical solution of the steam plough question."*

To the agriculturist the condition and changes of the atmosphere are hardly of less interest than is the soil which he has to till. As might be expected, therefore, in one so interested in agriculture, the Marquis of Tweeddale attached great importance to meteorology, and devoted much time, labour, and money to the fostering of that science. Of the National Meteorological Society of Scotland he was from the first, and continued to be, the main support. From time to time he offered prizes for the best essay or series of observations on meteorological phenomena; which had the effect of engaging the attention of competent inquirers to these phenomena, and drawing forth some communications of the greatest importance to those engaged in agriculture and allied pursuits.

Besides holding the hereditary honours and dignities of his house, some of which had come down to him from a remote ancestry, Lord Tweeddale possessed many personal distinctions. He was a K.T. and G.C.B., a representative Peer of Scotland, Lord-Lieutenant of Haddingtonshire, Colonel successively of the 30th Regiment, the 42nd Regiment, and the 2nd Regiment of Life Guards, a General in the army, and a Field Marshal. He died at Yester House on the 10th of October last, in his 90th year.

The Marquis of Tweeddale became a Fellow of this Society in 1849.

Adolphe Pictet was born at Geneva on the 11th of September 1799. He received his education at the Hofwyl institution

* Stephens & Slight, Book of Farm Implements.

founded by Fellenberg, who was an intimate friend of his father Charles Pictet de Rochamont. He afterwards studied at Paris; and from that he went to Germany, where he made the acquaintance of Schlegel, Schelling, Hegel, and Goethe, as he had at Paris that of Victor Cousin and other men of eminence. A vast mass of letters remain to testify to the intimacy of his relations with these and other distinguished men among his contemporaries.

He began to write in the "Bibliotheque Universelle," a journal founded by members of his own family, and of which he became the proprietor in 1825. In 1838 or 1839 he commenced to lecture on æsthetics in the Academy at Geneva, and three years afterwards he was appointed professor there of æsthetics, modern literature and linguistic. This appointment he did not hold long; circumstances led to his engaging in other pursuits, and for a time he left Geneva and fixed his abode at Turin. Obliged, as every Swiss citizen is, to serve as a soldier, he for some time was in the army, where he rose to the rank of colonel of artillery. Directing his attention to the implements of artillery warfare, he introduced such improvements in these that the Austrian Government purchased for 25,000 francs the secret of bombs of his invention. Returning to Geneva, he devoted himself to his favourite studies, and from time to time issued works in various branches of philosophy, and especially in comparative philology, which brought him reputation much more in other countries than his own. In Geneva, indeed, he was very little known, the abstruse character of his writings rendering them acceptable only to the few, and the deafness with which he was afflicted preventing him from mingling in general society. In 1839 the Institute of France adjudged to him the Volney prize for an essay on the affinity of the Celtic language with the Sanscrit; and twenty years afterwards the same prize was assigned to him for a work in which he developed at length his views as to the primary relations of the Indo-European tongues generally. This work, which is entitled "Les Origines Indo-Européennes, ou Les Aryas primitifs; Essai de Paléontologie linguistique," is one of great learning and acuteness, and has commanded the applause of philologists and ethnologists in all parts of the learned world. In 1856 he published a work scarcely less remarkable in its kind, entitled "Du Beau dans la Nature, l'art, et la poesie, études d'Esthétique." His other works are, "Course à Chamonix, conte fantastique" (1838); "Essai sur les propriétés et la tactique des fusées de guerre" (Turin, 1848); "Mystère des Bardes de l'Isle de Bretagne;" two "Essais sur des Inscriptions Gauloises," and many articles in reviews and other periodicals.

M. Pictet was a Member of the Royal Irish Academy, of the Ethnographic Society of New York, of the Academie Stanislas of Nancy, as well as a Fellow of this Society. From Napoleon III. he received the decorations of the Legion of Honour and of St Maurice and St Lazare. He was elected an Honorary Fellow in 1864. He died 20th December 1875.

Adolphe-Theodore Brongniart was born at Paris on the 14th January 1801. He was the son of Alexandre Brongniart, the celebrated naturalist and associate of Cuvier, who died in 1847. Devoted from childhood to the study of the natural sciences, in which he was encouraged as well by the example as by the counsels of his father, Adolphe, while preparing to take his degree of doctor of medicine, cultivated at the same time with earnestness and success botany and geology. In 1822 he published a monograph on the classification and distribution of fossil vegetables; three years later he published a work on *Champignons*, in which he embraced the whole of that family in a natural classification of its genera; and in 1826 he presented to the Académie des Sciences a "Memoire sur la génération et le dévéloppement de l'embryon végétal," for which he received the first prize for experimental physiology. In this work a new light was thrown on the most important fact in the life of plants; and if it cannot be said that he entirely lifted the veil which had hitherto covered the mystery of fecundation, he advanced very near to the complete explanation of that phenomenon. The course on which he had thus entered of scientific investigation he pursued with unabated energy to the close of his life. The number of investigations which he conducted, and the papers which he presented to scientific societies and to the scientific world through the press, indicate an almost unprecedented amount of activity on his part. His most important work is his "Histoire des Végétaux Fossiles," which unfortunately was left unfinished by him. In this work new light is thrown on both botany and geology, and it may be regarded as a work of standard authority on vegetable palæontology.

M. Brongniart was elected professor of botany and vegetable physiology in the Museum of Natural History at Paris in 1833; and from 1852 he was inspector-general of the University for the sciences. He was a Member of the Academie des Sciences and an officer of the Legion of Honour. He was elected an Honorary Fellow of this Society in 1872. He died in February last.

Christian Gottfried Ehrenberg was born at Delitsch, in Prussia, on the 19th of April 1795. He studied chiefly at Leipsic, where he took his doctor's degree in medicine. At Berlin, in 1815, he devoted himself to microscopic researches in physiology, and through these became known to the scientific world. In 1820 he was sent by the Academy of Sciences, along with Hemprich, on a scientific expedition to Egypt, in the course of which he traversed Egypt, Abyssinia, and a considerable part of Africa. His companion having succumbed under the fatigues of the journey, Ehrenberg prosecuted it alone, and returned bringing with him a large collection of animals and plants until then unknown. He was named professor extraordinary in the Faculty of Medicine at Berlin; but he preferred going with Humboldt to explore Central Asia, and more particularly the plateau of the Altaï. On his return he settled at Berlin, where he was in 1842 made principal secretary to the Academy of Science. Here he devoted himself chiefly to microscopic researches on the Infusoria, and in this field made many discoveries, which added materially to the knowledge of these minute forms of animal life, and suggested the explanation of many previously unexplained phenomena, such as phosphorescence on the sea, blood-rain, red snow on the Alps, and the blood-red spots which, to the terror of the ignorant and superstitious, sometimes appear on bread. To the heaps of infusoria, also, he attributed "the existence of vegetable soil, and according to his observations these infinitely small creatures have formed entire mountain-chains, and played an important part in the formation of the crust of the earth."* His great work, entitled "Die Infusiorsthierchen als vollkommen Organismen" (Leipz. 1838), contains his classification

* Men of the Time, p. 328, 8th edit.

of these minute organisms, a classification, however, which naturalists have not universally adopted.

Herr Ehrenberg was a member of most of the learned Societies of Europe. He was elected an Honorary Fellow of this Society in 1845. He died in April of this year.

In the course of the past session the number of "papers" read at the meetings of the Society was forty-five, besides the address delivered at the opening of the session by the senior Vice-President, Mr Milne-Home. Of the papers read, twenty-three were in the department of mathematics and physics, three in chemistry, four in meteorology, one in anatomy, five in geology, two in geography, five in botany and natural history, and two on literary subjects. Some of these papers were of considerable length, and most were of importance and permanent interest.

The Keith Prize for the biennial period 1873–75 was awarded to Professor Crum Brown for his "Researches on the Sense of Rotation and on the Anatomical Relations of the Semi-Circular Canals of the Internal Ear;" and was presented to him by the President at the meeting on the 15th of May.

The Society is to be congratulated on the evidence which the number and variety of the papers read last session afford of the continued zeal and activity of its members in various spheres of scientific inquiry and literary research. At the same time, I venture to express a wish that the range of the Society's activity were widened, so that incursions were made into fields of inquiry and research which, so far as our Proceedings show, are wholly neglected by the members of the Society. With the exception of the two literary papers, all the papers read last session have to do either with the exact sciences or the sciences of observation. Besides these, however, there are the moral sciences, the science of experience or consciousness, and the history of science, in all of which there are questions of high interest and importance which yet remain unsettled, and which await and invite the investigation of members of a learned body such as this. How little, for instance, is known of the history of philosophic thought and scientific speculation among the Arabians and the Jews in the Middle Ages, or among the theosophists, mystics, and speculatists of the East!

In ethics, in political economy, in jurisprudence, in international law, how many points of profound interest are still in dispute or are involved in obscurity! In psychology, how many new theories and methods have recently emerged, both in this country and on the Continent, which may change the entire aspect of that science, and which, at any rate, challenge the careful consideration of all philosophic thinkers! A new science, indeed, has of late sprung up in this department,—the science, so-called, of physiological psychology,—an infelicitous designation, as I cannot but think, seeing that as physiology can never become psychology nor psychology physiology, the union of the two words in one designation is almost tantamount to a negation of the possibility of the science so designated. This science has found so many enthusiastic and able cultivators that its claims on the attention and study of scientific inquirers cannot be neglected. I confess I am not myself sanguine of any great advantage accruing to psychology from its being approached through the medium of physiology, for this, were there no other reason, that, as on the one hand, the physiologist must first learn from consciousness or experience the existence of any faculty before he can search for or find an organ for that faculty in the body; so, on the other hand, he is unable from the mere observation of the bodily organs or functions to throw any light upon the mental faculties, seeing all he can accomplish at the utmost is to point to a connection of some sort between the mental faculty and the bodily organ. At the same time, I defer to the judgment of the eminent men who have appeared as the cultivators of this science, and claim for it a place among the objects which engage the attention of the members of this Society.

"Through desire a man, having separated himself, seeketh and intermeddleth with all wisdom" (Prov. xviii. 1). This saying of the Hebrew sage might be adopted as a fitting motto for such a Society as this. By joining it the members separate or set themselves apart, under the impulse of a master desire, to the pursuit of knowledge; and it beseems them, as so separated, to take all wisdom for their province. It is true that in the present day knowledge is no longer confined to the few, or locked up within Societies of those who give themselves to the pursuit of it as their business and occupation. It is true that "science has now left her retreats, her shades, her selected company of votaries" (Channing); and that many who are engaged in other occupations are at the same time diligent cultivators of science, and often come forward to add contributions to its stores. Still, it is to those who set themselves apart as men of science or as literary men that the world looks for the steady pursuit of knowledge, and for the guidance of others to the solution of those questions which from time to time press upon the interest or affect the welfare of the community. And where Societies of such are formed men look to them to intermeddle with all wisdom, and leave no part of the wide field of knowledge unexplored.*

On the value and utility of such Societies it would be idle in me to expatiate in such an assembly as this. Who knows not that by intercourse with others men have their faculties sharpened, are helped better to understand themselves, and to bring to precision and definiteness their own cogitations, as well as stimulated to explore new fields of inquiry and guided to make new discoveries? It is long ago since Homer said—

> " By mutual confidence and mutual aid
> Great deeds are done and great discov'ries made.
> The wise new wisdom from the wise acquire,
> And one brave hero fans another's fire"—†

and ample experience has showed that this holds true no less of those who go in quest of truth than of those who engage in military adventure; for as Plato, after referring to this passage in Homer, says, " in society we all are somehow more alert in deed and word and thought;"‡ and Aristotle, also referring to this passage, says that by society " those engaged in great undertakings are rendered more potent to think and to act."§ I content myself with congratulating the Society on its past achievements and its present flourishing condition; and expressing my confidence that the energy which has characterised the members in the past will be no less displayed, and with equally satisfactory results, during the session on which we now enter.

* Naturæ rerum vis atque majestas in omnibus momentis fide caret si quis modo partes ejus ac non totam complectitur animo.—Plin. Hist. Nat. vii. 1.

† Iliad, x. 265, in Pope's version.

‡ Protag., p. 348, D.

§ Nicom. Eth., viii. 1.

We need have no misgivings as to the worthiness of the pursuits to which this Society is devoted. To search after truth, to strive, as "the minister and interpreter of Nature," not only to discover all her facts but to elicit their meaning, to educe the principles which underlie them, and to arrive at the apprehension of the laws which they obey,—

> "Und was in schwankenden Erscheinung schwebt
> Befestigen mit dauerden Gedanken"*—

is an occupation than which none can be more noble, more worthy of the faculties with which we have been endowed, or more pleasing to Him by whom these faculties have been conferred,—for He Himself is Truth; and in proportion as men with sincerity and earnestness seek after truth, in that proportion do they put on the similitude of God and come into sympathy with Him. The visible universe, moreover, is as Goethe has expressed it, "Der Gottheit lebendiger Kleid" (the living mantle of the Deity); and by it He, who is himself invisible, reveals himself to us, making known to us, as the apostle tells us, by the things that are made his eternal power and Godhead (Rom. i. 20). Philosophy, if it follow its normal tendency, leads up to God; for the progress of all true philosophic thought is from the many to the one, from facts to principles, from the relative to the absolute, from phenomena to essence; and it is illegitimately arrested in its proper course, and defrauded of its proper issue, if it be stopped short of the Supreme Essence in whose infinite mind are the archeal types of all existences—"the forms eternal of created things." Nor is the value of literary and scientific study as a moral discipline to be overlooked. To those engaged in such pursuits there arises an influence which, like some subtle essence, pervades their whole inner nature, and unconsciously, perhaps, to themselves elevates, purifies, and refines it. The study of philosophy, of literature, and of science thus becomes a great moral therapeutic, an instrument of spiritual culture:—"Animum format et fabricat, vitam disponit, actiones regit, agenda et omittenda demonstrat, sedit ad gubernacubum et per ancipitia fluctuentium dirigit cursum."†

"The true philosopher (I use the words of Sir Humphry Davy) sees good in all the diversified forms of the external world. Whilst

* Goethe, Faust, Prol.

† Seneca, Ep. xvi.

he investigates the operations of infinite power guided by infinite wisdom all low prejudices, all mean superstitions disappear from his mind. He sees man an atom amidst atoms fixed upon a point in space, and yet modifying the laws that are around him by understanding them; and gaining, as it were, a kind of dominion over time and an empire in material space, and creating on a scale infinitely small a power seeming a sort of shadow or reflection of a creative energy, and which entitles him to the distinction of being made in the image of God and animated by a spark of the Divine mind." *

The following statement, in regard to the number of the present Fellows of the Society, has been drawn up by the Secretary:—

1. Honorary Fellows:—

Royal Personage—

His Royal Highness the Prince of Wales, . . . 1

British Subjects—

John Couch Adams, Esq., Cambridge; Sir George Biddell Airy, Greenwich; Thomas Andrews, M.D., Belfast (Queen's College); Thomas Carlyle, Esq., London; Arthur Cayley, Esq., Cambridge; Charles Darwin, Esq., Down, Bromley, Kent; John Anthony Froude, Esq., London; Thomas Henry Huxley, London; James Prescott Joule, LL.D., Cliffpoint, Higher Broughton, Manchester; William Lassell, Esq., Liverpool; Rev. Dr Humphrey Lloyd, Dublin; William Hallowes Miller, LL.D., Cambridge; Richard Owen, Esq., London; Thomas Romney Robinson, D.D., Armagh; Lieut.-General Edward Sabine, R.A., London; Henry John Stephen Smith, Oxford; George Gabriel Stokes, Esq., Cambridge; James Joseph Sylvester, LL.D., London; William Henry Fox Talbot, Esq., Lacock Abbey, Wiltshire; Alfred Tennyson, Esq., Freshwater, Isle of Wight. 20

Foreign—

Claude Bernard, Paris; Robert Wilhelm Bunsen, Heidelberg; Michael Eugene Chevreul, Paris; James D. Dana, LL.D., Newhaven, Connecticut; Heinrich Wilhelm Dove, Berlin; Jean Baptiste

Carry forward, 21

* Consolation in Travel, Works, vol. ix. p. 361.

Brought forward, 21

Dumas, Paris; Charles Dupin, Paris; Elias Fries, Upsala; Herman Helmholtz, Berlin; August Kekule, Bonn; Gustav Robert Kirchhoff, Heidelberg; Herman Kolbe, Leipzig; Albert Kölliker, Wurzburg; Ernst Eduard Kummer, Berlin; Johann von Lamont, Munich; Richard Lepsius, Berlin; Ferdinand de Lesseps, Paris; Rudolph Leuckart, Leipzig; Urbain Jean Joseph Leverrier, Paris; Joseph Liouville, Paris; Carl Ludwig, Leipzig; Henry Milne-Edwards, Paris; Theodore Mommsen, Berlin; John Lothrop Motley, United States; Louis Pasteur, Paris; Prof. Benjamin Peirce, United States Survey; Henry Victor Regnault, Paris; Angelo Secchi, Rome; Karl Theodor von Siebold, Munich; Bernard Studer, Berne; Otto Torell, Lund; Rudolph Virchow, Berlin; Wilhelm Eduard Weber, Gottingen; Friedrich Wohler, Gottingen, ... 34

The following are the Honorary Fellows deceased during the year:—

British—Sir William E. Logan.

Foreign—Adolphe-Theodore Brongniart, Christian Gottfried Ehrenberg.

2. Non-Resident Fellow under the Old Laws:—

Sir Richard Griffiths, ... 1

Total Honorary and Non-Resident Fellows at 4th December 1876, ... 56

3. Ordinary Fellows:—

Ordinary Fellows at November 1875, ... 358

New Fellows, 1875–76.—Rev. Francis Edward Belcombe; Bruce Allen Bremner, M.D.; Rev. John Gibson Cazenove, D.D.; Peter Denny; James Douglas H. Dickson, M.A.; James Duncan; J. S. Fleming; J. Ballantine Hannay; M. Forster Heddle; Rev. Norman Macleod, D.D.; John Macmillan, M.A.; J. F. Rodger, S.S.C.; William Skinner, W.S.; William Thomson, F.C.S.; Rev. Francis Le Grix White, M.A., ... 15

Carry forward, 373

	Brought forward,	373
Deduct Deceased—Dr James Warburton Begbie; David Bryce; G. Stirling Home Drummond; Lewis D. B. Gordon, C.E.; Sir George Harvey; Dr Laycock; Thomas Login, C.E.; Alexander Russel; The Marquis of Tweeddale, ...	9	
Resigned—Hon. Charles Baillie (Lord Jerviswoode) .	1	
		10
Total number of Ordinary Fellows at November 1876, .		363
Add Honorary and Non-Resident Fellows, . .		56
Total Honorary and Ordinary Fellows at commencement of Session 1876–77, ...		419

Monday, 18th December 1876.

Sir WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. On the Roots of the Equation V $\rho\phi\rho = 0$. By Gustav Plarr. Communicated by Professor Tait.

2. Applications of the Theorem that Two Closed Plane Curves intersect an even number of times. By Prof. Tait.

(*Abstract.*)

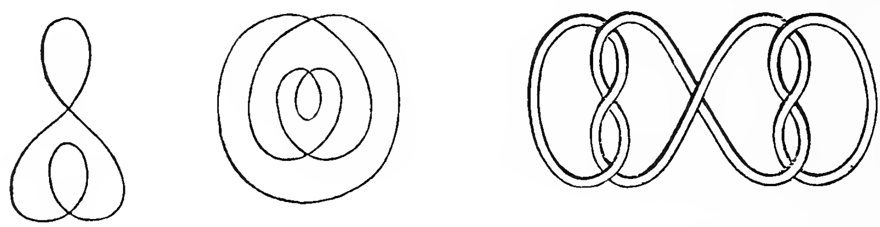
The theorem itself may be considered obvious, and is easily applied, as I showed at the late meeting of the *British Association*, to prove that in passing from any one double point of a plane closed curve continuously along the curve to the same point again, an *even* number of intersections must be passed through. Hence, if we suppose the curve to be constructed of cord or wire, and restrict the crossings to *double points*, we may arrange them throughout so that, in following the wire continuously, it goes alternately over and under each branch it meets. When this is done it is obviously as completely knotted as its scheme (defined below) will admit of, and except in a special class of cases cannot have the number of crossings reduced by any possible deformation. The excepted class is that in which the intersections are either wholly or partially nugatory, *i.e.*, not in reality contributing to the knot, whether on account of the order of their arrangement or their signs. All nugatory intersections can be detected at once by the scheme itself, and may be struck out. As will be understood from what follows, the schemes

A A B B C C and A C B B C A

are wholly nugatory, while in

A C B D C B D A E G F E G F

only the intersection A is necessarily nugatory. In fact a group like C B D C B D, when not itself nugatory by reason of its signs, is self-contained, and forms a special knot which may be drawn tight so as to present only a roughness in the string. The following sketches illustrate these essentially nugatory crossings:—



I. Given the number of its double points, to find all the essentially different forms which a closed curve can assume.

(*a*.) Going round the curve continuously, call the first, third, &c., intersections A, B, C, &c. In this category we evidently exhaust all the intersections. The complete scheme is then to be formed by properly interpolating the same letters in the even places; and the form of the curve depends solely upon the way in which this is done.

(*b*) It cannot, however, be done at random. For instance, the scheme

A *D* B *E* C *A* D *B* E *C* | A

is lawful, but

A *D* B *A* C *E* D *C* E *B* | A

is not.

The former, in fact, may be treated as the result of superposing two closed (and not self-intersecting) curves, both denoted by the letters A D B E C A, so as to make them cross one another at the points marked B, C, D, E, then cutting them open at A, and joining the free ends so as to make a continuous circuit with a crossing at A.

But in the latter scheme above, we have to deal with the curves A D B A and C E C E, and in the last of these we cannot have the junctions alternately + and – as required by our fundamental principle. In fact, the scheme would require the point C to lie simultaneously inside and outside the closed circuit A D B A.

Or we may treat A D B A and C E D C as closed curves intersecting one another and yet having only one point, D, in common.

(*c*) Thus, to test any arrangement, we may strike out from the whole scheme all the letters of any one closed part as A——*A*, and the remaining letters must satisfy the fundamental principle.

Or we may strike out all the letters of any two sets which begin and end similarly, *e.g.*, A . . . X, X . . . A, the two together being treated as one closed curve, and the test must still apply.

More generally, we may take the sides of any closed polygon as A – X, X – Y, Y – Z, Z – A, and apply them in the same way. But in this, as in the simpler case just given, the sides must all be taken the same way round in the scheme itself.

(*d.*) Such schemes as the latter of the two in (*b*) above may be made algebraically possible by slightly changing our assumptions. Thus, for instance, we might admit of a triple point, and agree not to reckon it as an intersection on a continuous oval provided one of the remaining branches goes *into* the oval, and the other comes out of it, these two not necessarily intersecting one another. In the case specified the triple point would be E supposed to lie *on* the oval A D B A, and not to be counted as an intersection while we pass round that oval. But this is a mere algebraic escape from a geometrical difficulty, and will not necessarily help us when we deal with knots on actual cords or wires.

II. A possible scheme being thus made, with the requisite number of intersections, let it be constructed in cord, with the intersections as above alternately + and –. Then [since all schemes involving nugatory points, like those above mentioned, must be got rid of, as they do not really possess the requisite number of intersections] no deformation which the cord can suffer will reduce, though it may increase, the number of double points. If it *do* increase the number, the added terms will be of the nugatory character presently to be explained. If it do not increase that number, the scheme will in general still represent the altered figure. Hence the scheme is a complete and definite statement of the nature of the knot.

(*a.*) One illustration depends upon the fact that all deformations of such a cord or wire may be considered as being effected by bending at a time only a limited portion of the wire, the rest being held fixed. This corresponds to changing the point of view *finitely* with regard to the part altered, and yet *infinitesimally* with regard to all the rest. This, it is clear, can always be done, as the *relative* dimensions of the various coils may be altered to any extent without altering the character of the knot All such deformations may be obtained by altering the position of a luminous *point*, and the plane on which it casts a shadow of the knot. Any addition to the normal number of intersections which may be produced by this process is essentially nugatory.

Another mode, really depending on the same principles, consists in fixing temporarily one or more of the crossings, and considering the impossibility of unlocking in any way what is now virtually two or more *separate* interlacing closed curves, or a single closed curve with full knottings, but with fewer intersections than the original one.

Another depends upon the study of cases of knots in which one or more crossings can be got rid of. Here, it is proved that *continuations* of sign are in general lost when an intersection is lost; so that, as our system has no continuations of sign, it can lose no intersections.

(*b.*) Practical processes for producing all such deformations graphically are given at once by various simple mechanisms. Thus, taking O any fixed point whatever, let p, a point in the deformed curve, be found from its corresponding point, P, by joining PO and producing it so that

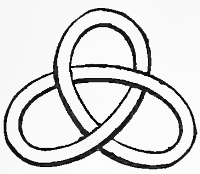
$$\mathrm{PO} \cdot \mathrm{O}p = a^2,$$

or so that

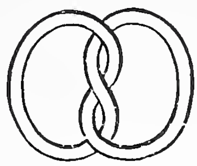
$$\mathrm{PO} + \mathrm{O}p = a, \text{ \&c., \&c.}$$

The essential thing is that points near O should have images distant from O, and *vice versâ*. And p must be taken in OP *produced*, else the distorted knot is altered from a right-handed to a left-handed one, and *vice versâ*. This distinction is shown in the cuts

1 and 3 below, where it will be seen that one turn of the coil may be regarded as wound round the other—the screw being right-handed in 3 and left-handed in 1.



1.



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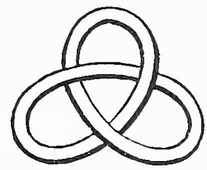
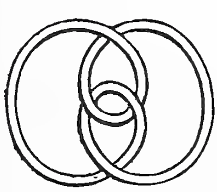


3.

It is easy to show that these methods give merely different views of the same knot. The simplest way of doing this is to suppose the knot projected on a sphere, the eye being at the centre. Arrange so that one closed branch, *e.g.*, A——A, forms nearly a great circle. Shifting the eye to opposite sides of the plane of this great circle the coil presents exactly the two appearances related to one another by the deformation processes given above. What was inside the closed branch from the one point of view is outside it from the other, and *vice versâ*.

Thus 1 and 2 above are the *only* forms with three non-nugatory intersections. 2 may be formed from 1 by putting O in either of the three border areas, each of which has two sides only. If O be placed in external space, or in the inner three-sided area, 1 is reproduced. Similar remarks apply to the deformation of 2.



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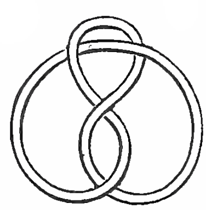
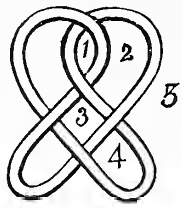


5.

Figures 4 and 5 are the only forms with four valid intersections. Like 1 and 2, the first of them is a *clear* coil (see below), the second not clear. And, of course, any deformation of either produces the other, or reproduces itself. 6, 7, 8, 9, are forms of an essentially not-clear arrangement, with five intersections. The numbers inserted in 6 show which form is produced by placing O in the corresponding area. The only other forms having five intersections, are the clear coil of two turns, whose scheme is the first given in I (*b*) above, and its solitary deformation.



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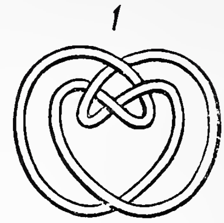

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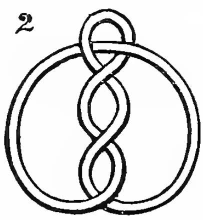

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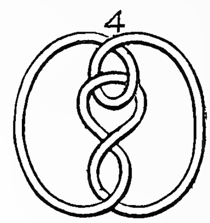


9.

(*c.*) Hence to draw a scheme, select in it any closed circuit, *e.g.*, A . . . . A—the more extensive the better, provided it do not include any less extensive one. Draw this, and build upon it the rest of the scheme; commencing always with the common point A, and passing each way from this to the *next occurring* of the junctions named in the closed circuit. [It is better to construct both parts of the rest of the scheme *inside*, and then invert one of them, as we thus avoid some puzzling ambiguities.] Inversions with respect to various origins will now give all possible forms of the scheme.

III. Thus the scheme is perfectly definite as to the general shape of the curve, if we take the possible deformations into account. And the spherical projection, already mentioned, will in general allow us to regard and exhibit the knot as a more or less perfect *plait*. It does so always when the coil is *clear*, *i.e.*, when all the turns of the cord may be regarded as passing in the same direction round a common axis thrust through the knot. When the coil is not clear some of the cords of the plait are doubled back on themselves. Thus by drawing the plait from a given scheme we can tell at once whether one of its forms is a clear coil or not.

From this point of view another notation for clear coils is given in the form

$$\begin{array}{c} \alpha\,\gamma\,\beta\,\alpha \\ \beta\,\alpha\,\gamma\,\beta \end{array} \cdots\cdots$$

Here α, β, γ . . . are, in order, the several strings plaited—so that in the coil β is the prolongation of α, γ that of β, &c., and α that of the last of the series. The expression $\genfrac{}{}{0pt}{}{\alpha}{\beta}$ means that α crosses *over* β. It is sometimes useful to indicate whether a crossing takes place to the right or left. This is done by putting + or − over the symbol. Thus the four crossings above may be more fully written as

$$\begin{matrix} + & - & + & - \\ \alpha & \gamma & \beta & \alpha \\ \beta & \alpha & \gamma & \beta \end{matrix} \cdots\cdot$$

The properties of this notation are examined in detail. It is shown, that the combination just written cannot be simplified in itself; but that

$$\begin{matrix} + & - & - & - \\ \alpha & \gamma & \gamma & \alpha \\ \beta & \alpha & \beta & \beta \end{matrix} = \begin{matrix} - & - \\ \gamma & \gamma \\ \beta & \alpha \end{matrix}, \text{ \&c.}$$

This notation requires care. For instance, the terms

$$\begin{matrix} \alpha & \alpha \\ \beta & \beta \end{matrix}$$

are simply nugatory, and may be written off. But, on the other hand, the terms

$$\begin{matrix} \alpha & \beta \\ \beta & \alpha \end{matrix}$$

usually add to the beknottedness of the whole scheme.

When the scheme is not compatible with a clear coil there occur terms of the form

$$\begin{matrix} \alpha \\ \alpha \end{matrix},$$

and the application of this method becomes very troublesome.

(*a.*) When the scheme has no merely nugatory intersections, the most complete knotting is secured by alternate crossings above and below; or, as we may write,

$$\begin{matrix} A & X & B & Y & C & \ldots\ \text{\&c.} \\ + & - & + & - & + & \end{matrix}$$

and here there is *no continuation of sign.*

(*b.*) Cases in which there is no knot at all may be obtained for any scheme by making each letter positive on its first appearance. The various coils are then, as it were, paid out over one another. This process will give rise, in general, to but few changes of sign:

but the number of such will usually depend upon the particular intersection with which we commence the scheme.

Additional changes of sign, still without any knotting, may be introduced by various processes, of which the following is the simplest:—When two letters appear together twice, not necessarily in the same order, but with like signs, these signs may be changed. Thus, the following parts of a scheme

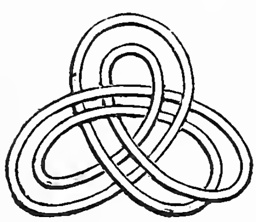
$$\underset{-}{P}\underset{-}{Q}\ldots\ldots\underset{+}{Q}\underset{+}{P}$$

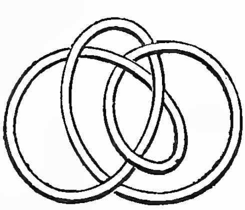
may be changed to

$$\underset{+}{P}\underset{+}{Q}\ldots\ldots\underset{-}{Q}\underset{-}{P}$$

and the statements already made about nugatory intersections can be applied to these and other combinations even when they occur separately once only in each of two separate knots on the same cord. This, and a great number of similar theorems, allow of a great special extension of the nugatory test already given—but an extension which cannot be made in any case until the *signs* of the intersections are given as well as the order of their occurrence.

Again, though, as has been said above, continuations of sign disappear when an intersection is lost, it does not follow that if a scheme have continuations of sign it must necessarily be reducible. The annexed diagram is an excellent instance. Its scheme contains fourteen continuations, and only twelve changes, of sign, and yet the knot is irreducible. But if we suppose it cut across twice at the single unsymmetrically placed crossing, and the ends joined so as still to preserve continuity in the string, the scheme has still fourteen continuations, but only ten changes, of sign; and it does not involve any real beknottedness.





The remaining figure illustrates a fully knotted scheme, where there are no continuations of sign, but in which the mere change of sign of one of the intersections produces four continuations of sign, and the whole beknottedness disappears. Similar remarks apply to most of the preceding figures.

IV. A great many other deductions from the fundamental proposition are given—for instance,

A closed plane curve, intersecting itself, divides the plane into separate areas whose number is greater by 2 than the number of intersections.

Regarding the curve as a wall, dividing the plane into a number of fields, if we walk along the wall and drop a coin into each field as we *reach* it, each field will get as many coins as it has corners, but those fields only will have a coin in each corner whose sides are all described in the same direction round. The number of coins is four times the number of intersections—and two coins are in each corner bounded by sides by each of which you enter—none in those bounded by sides by each of which you leave.

Cut off at any intersection and remove a portion of the curve forming a closed (not self-cutting) circuit. You thus abolish an odd number of intersections. Hence if there is an even number of coils, whether the whole be clear or not, there is an odd number of intersections, and *vice versâ*.

To form the symmetrical clear coil of two turns and of any (odd) number of intersections, make the wire into a helix, and bring one end through the axis in the same direction as the helix (not in the opposite direction, as in Ampère's *Solenoids*), then join the ends. [The solenoidal arrangement, regarded from any point of view, has only nugatory intersections.] An excellent mode of forming this coil is to twist a long strip of paper through an odd number of half-turns, and then paste its ends together,—the two longer edges become parts of one continuous curve which is the clear coil in question. This result is applied to the study of the form of soap-films obtained by Plateau's process on clear coils of wire.

V. Another question treated is the numbers of possible arrangements of given numbers of intersections in which the *cyclical* order of the letters in the 2nd, 4th, 6th, &c. places of the scheme shall be the same as that in the 1st, 3rd, 5th, &c., *i.e*, the alphabetical. Instances of such have already been given above. In the first of I (*b*), for example, the letters in the even places are

D E A B C.

Here the cyclical order of the alphabet is maintained, but A is postponed by two places.

Whatever be the number of intersections a postponement of *no* places leads to nugatory results.

A postponement of one place is possible for three and for four intersections only.

Postponement of two places is possible only for (*four*), five, and eight—three for seven and ten—four for nine and fourteen—five for (*eight*), eleven and sixteen,—six for (*ten*), thirteen, and twenty, &c. Generally there are in all cases n postponements for $2n+1$ intersections:—and for $3n+2$, or $3n+1$ intersections, according as n is even or odd. The numbers which are italicised and put in brackets above, arise from the fact that a postponement of r places, when there are n intersections, gives the same result as a postponement of $n-r-1$ places. [It will be observed that this cyclical order of the letters in the even places is possible for *any* number of intersections which is not 6 or a multiple of 6.]

When there are n postponements with $2n+1$ intersections the curve is the symmetrical double coil—*i.e.*, the plait is a simple *twist.*

The case with $3n+2$ or $3n+1$ intersections is a clear coil of three turns, corresponding to a regular plait of three strands.

VI. Numerous examples are given of the application of various methods of reduction. For instance, the scheme

A *E* B *F* C *G* D *A* E *K* F *L* G *D* H *B* K *C* L *H* | A
− + + + − + − + − + − + − + − − − + − +

which is rendered irreducible by changing the sign of B, is reduced by successive stages as follows:—

A B C *G* D *A* *K* *L* G *D* H K *B* *C* L *H* | A,
− + − + − + + + − + − − − + − +

B C A *G* D *A* G *D* H *B* *C* *H* | B,
+ − − + − + − + − − + +

C A *G* D *A* G *D* *C* | C;
− − + − + − + +

and, finally,

A *G* D *A* G *D* | A,
− + − + − +

which is the simple irreducible knot of figures 1 and 3 above.

3. On the Distribution of Volcanic Debris over the Floor of the Ocean,—its Character, Source, and some of the Products of its Disintegration and Decomposition. By John Murray, Esq. Communicated by Sir C. Wyville Thomson.

During the present session I propose to lay before the Society several papers on subjects connected with the deposits which were found at the bottom of the oceans and seas visited by H.M.S. Challenger in the years 1872, 1873, 1874, 1875, and 1876.

Instruments in use for obtaining information of the Deposits.

It will be convenient to introduce this first communication with a brief description of the instruments and methods employed on board H.M.S. Challenger with the view of obtaining information and specimens of these ocean deposits. The instrument in most frequent use was the tube or cylinder forming part of the sounding apparatus.

During the first six months of the cruise this cylinder was one having less than an inch bore, and was so arranged with respect to the weights or sinkers that it projected about six inches beneath them. The lower end of the cylinder was fitted with a common butterfly valve. This arrangement gave us a very small sample of the bottom.

In July 1873 this small cylinder was replaced by one having a two-inch bore, and it was also made to project fully eighteen inches below the weights. This was a great improvement, as it gave a much greater quantity of the bottom in most soundings.

The tube was, in the clays, frequently forced nearly two feet into the bottom. On its return to the ship, the butterfly valves were removed, and a roll of the clay or mud, sometimes eighteen inches in length, could be forced from it. In this way we learned that the deeper layers were very frequently different from those occupying the surface.

In the organic oozes—as the Globigerina, Pteropod, Radiolarian, and Diatom oozes—the tube did not usually penetrate the bottom over six or seven inches, these deposits offering more resistance than the clays and muds. Occasionally the tube came up without anything in it, but the outside was marked with streaks of the black oxide of manganese. In about thirteen out of nearly four hundred soundings we did not get any information of a reliable nature about the deposit.

The dredge in use was a heavy modification of Ball's naturalists' dredge, and the trawl was the ordinary beam trawl of the fishermen.

Both of these instruments had generally a bag of canvas or other coarse cloth sewed into the bottom of the netting, to prevent the soft clay or ooze from being entirely washed out. In this way we, at many stations, got, along with animals, a large quantity of ooze, clay, stones, or manganese nodules.

While trawling or dredging the ship often shifted her position a mile or two, but we could not tell whether the dredge or trawl had been working over all that distance, or had merely taken a dip into the deposits. This should be remembered when comparing the captures in one locality with those of another.

Altogether there is much uncertainty about the behaviour of the trawl and dredge in deep water. It occasionally happened that when the greatest care was taken, and when it was believed that the trawl had been dragging for some hours, it came up without anything in it, or any evidence upon it or in the attached tow-nets to show that it had been on the bottom.

During the last year of the cruise a tow-net was attached to the dredging line just below the weights, which last were placed a few hundred fathoms in front of the trawl or dredge. Tow-nets were also attached to the trawl and dredge. These nets frequently came up nearly full of mud, and almost always contained minute things and fragments from the surface layers of the bottom.

At times the water-bottle attached to the sounding line came up with clay or ooze in it, or had some of the deposit adhering to its under-surface.

These then were the means and methods employed for getting information concerning ocean deposits, and collectively they have furnished us with a large amount of material. A careful examination of the specimens procured has already much increased our knowledge of the nature and distribution of ocean deposits, of the sources of the materials of which they are built up, and of the chemical processes taking place in the deep waters and on the floor of the ocean.

The Volcanic Debris in Ocean Deposits and some of the Products of its Disintegration and Decomposition.

In a preliminary report to Professor Wyville Thomson, which has been published in the Proceedings of the Royal Society of London, I pointed out the wide-spread distribution of volcanic debris in ocean deposits and its probable influence in the formation of deep sea clays, and manganese nodules or depositions. In this paper I propose to treat of these subjects in more detail, and to give some of the results of observations which have been made since the above report was written.

Pumice Stones.

The form of volcanic debris most frequently met with in ocean deposits is pumice stone.

Specimens of these stones, varying from the size of a pea to that of a foot-ball, have been taken in dredging at eighty of our stations. I have placed the position of these stations on a map, from which it will be seen that they occur all along our route.

Near volcanic centres the dredge has frequently brought them up in great numbers, as off the Azores in the Atlantic, off New Zealand and the Kermadec Islands, at several places among the Philippine Islands, off the coast of Japan, and elsewhere. As a rule, they are not numerous in shore deposits when these are distant from volcanic regions. In deposits far from land they are most abundant in deep-sea clays, from which the shells and skeletons of surface organisms have been all or nearly all removed.

In the North Pacific the trawl brought up bushels of them from depths of 2300 and 2900 fathoms. Perhaps in no single instance have we trawled successfully on any of our deep-sea clays without getting numbers of these stones. If there be an exception it is in the North Atlantic. But here it is to be remembered that while we were investigating the conditions of the North Atlantic our attention had not yet been directed to the importance of detecting the presence of pumice, and we have not preserved such large samples of the North Atlantic deposits as of those of other regions.

On the whole, pumice stones are more numerous in the Pacific than in the Atlantic deposits.

In the Globigerina and other organic oozes, they are abundan or otherwise according as the deposit is near or far removed from volcanoes. In these oozes they never occur so abundantly as in the clays. They are more or less masked and covered up by the accumulated remains of foraminifera, diatoms, or other surface organisms. In like manner they are obscured in shore deposits by river and coast detritus. Besides those specimens which are sufficiently large to be examined by the hand, we detected with the microscope minute particles of feldspar in all our ocean deposits.

An inspection of the specimens which I have placed on the table will show that the majority of these pumice stones have a rolled appearance. Some of them have undergone much decomposition, while others are little altered. Some are coated with the peroxide of manganese, or have streaks of this substance running through them. They are the most frequent nucleus of the manganese nodules, to which I shall presently refer. Some specimens which were dredged from a depth of over three miles will, when dried, float for weeks in a basin of water; others, which have undergone partial decomposition, sink at once.

They present a great variety of texture and composition. They are white, grey, green, or black in colour. They are highly vesicular, or rather compact and fibrous. There would appear to be every gradation from common feldspathic to dark green pyroxenic kinds.

We find in them crystals of sanidin, augite, hornblende, olivine, quartz, lucite, magnetite, and titaniferous iron. Magnetic iron ore was found in all the specimens examined, either in crystals or in the form of dust. The other minerals vary in kind and abundance in the different specimens. The same crystals which we find in the pumice occur in all the kinds of ocean deposits.

Sources of the Pumice Stones.

The pumice stones which we find at the bottom of the sea have most likely all been formed in the air. Some of them may have fallen upon the sea; but the great majority seem to have fallen on land, and been subsequently washed and floated out to sea by rains and rivers. After floating about for a longer or shorter time they have become water-logged and have sunk to the bottom. Both in the North Atlantic and Pacific small pieces of pumice were several times taken on the surface of the ocean by means of the tow-net.

Over the surface of some of these serpulæ and algæ were growing, and crystals of sinadin projected, or were imbedded in the feldspar. During our visit to Ascension there was a very heavy fall of rain, such as had not been experienced by the inhabitants for many years. For several days after, many pieces of scoriæ, cinders, and the like were noticed floating about on the surface of the sea near the island. Such fragments may be transported to great distances by currents.

On the shores of Bermuda, where the rock is composed of blown calcareous sand, we picked up fragments of travelled volcanic rocks. The same observation was made by General Nelson at the Bahamas. Mr Darwin noticed pieces of pumice on the shore of Patagonia, and Prof. L. Agassiz and his companions noticed them on the reefs of Brazil. During a recent eruption in Iceland, the ferry of a river is said to have been blocked for several day by the large quantity of pumice floating down the river and out to sea. All the pumice which we find need not be of quite recent origin. Mr Bates informs me that great quantities of pumice are continually being floated down the Amazon. These come from near the foot of the Andes, where the head-waters cut their way through fields of pumice stones. In the province of Wellington, New Zealand, two of the rivers run through areas covered with pumice, and during floods bear great quantities out to sea.

Prof. Alex. Agassiz has kindly furnished me with the following note:—

"The river Chile, which flows through Arequipa, Peru, has cut its way for some thirty miles through the extensive deposits of volcanic ashes which form the base of the extinct volcano Misti. Some of the gorges are even 500 feet in depth, forming regular cãnons. The whole length of the river bottom is covered by well-rolled pieces of pumice from the size of a walnut to that of a man's head. In the dry season (winter) there is but little water flowing, but in the summer, or rainy season, the river, which has a very considerable fall (7000 feet in a distance of about 90 miles), drives down annually a large mass of these rolled pumice stones to the Pacific. The volcanic ashes are not recent. There is no tradition among the Indians of any eruption within historic times."

Captain Evans, the present hydrographer to the navy, informs me that he frequently picked up pumice on the Great Barrier Reef of Australia.

Volcanic Ashes.

Near volcanic centres, and sometimes at great distances from land, we find much volcanic matter in a very fine state of division at the bottom of the sea. This consists of minute particles of feldspar, hornblende, augite, olivine, magnetite, and other volcanic minerals. In the South Pacific, many hundred miles from land, and from a depth of 2300 fathoms, the trawl brought up a number of pieces of tufa entirely composed of these comminuted fragments. These particles appear to me to have been carried to the areas where we find them, by winds, in the form of what is known as volcanic dust or ashes. Sir Rawson Rawson sent to Sir Wyville Thomson a packet of the volcanic ashes which fell on the Island of Barbadoes, after an eruption in 1812 on the Island of St Vincent, W. I., one hundred and sixty miles distant. I have examined this, and find it made up of fragments of the same character as those in the tufa to which I have just referred, some of the particles being perhaps a little larger. We have sometimes found this ash in considerable abundance mixed up with the shells in a globigerina ooze. In the deposits for hundreds of miles about the Sandwich Islands there are many fragments of pyroxenic lava, which I believe have been borne by the winds, either as ashes, or in the form of Pele's hair.

At Honolulu we were informed that threads of Pele's hair were picked up in the gardens there after an eruption of Kilauea, one hundred and eighty miles from the volcano. This Pele's hair bears along with it small crystals of olivine.

Obsidian and Lava Fragments.

Small pieces of obsidian and of feldspathic and basaltic lavas were frequently found in deposits near volcanic islands.

At two stations in the South Pacific, many hundred miles from land, we dredged pieces of this nature of considerable size larger than ordinary marbles. It is difficult to account for the transference of these fragments to the places where they were found. It is, however, in this region, and this alone, that it may be necessary to bring in a submarine eruption to account for the condition of things at the bottom.

A consideration of these observations, and the specimens which are laid on the table, will, I think, justify the conclusion that volcanic materials, either in the form of pumice-stones, ashes, or other fragments, are universally distributed in ocean deposits.

They have been found abundantly or otherwise in our dredgings, according as these have been near or far from volcanoes, or as there has been much or little river and coast detritus, or few or many remains of surface organisms in the deposits.

Some of the Products of the Decomposition of Volcanic Debris.

Clay.—Pure clay, as is well known, is a product of the decomposition of feldspar, and the clay which we find in ocean deposits appears to have had a similar origin.

In the deposits far from land the greater part of the clay originates, I believe, from the decomposition of the feldspar of fragmental volcanic material, which we have seen to be so universally distributed.

Pumice-stone is largely made up of feldspar, and from its areolar structure is peculiarly liable to decomposition. Being permeated by sea water holding carbonic acid in solution, a part of the silica and the alkalies are carried away, water is taken up, and a hydrated silicate of alumina or clay results.

Like most clays our ocean clays contain many impurities, these last being as varied as the sources whence the materials of the deposits are derived.

Let us briefly enumerate the sources of these materials.

We have *first* the matters derived from the wear of coasts, and those brought to the sea by rivers, either in a state of suspension or solution. The material in suspension appears to be almost entirely deposited within two hundred miles of the land.

Where great rivers enter the sea, and where we have strong currents, as in the North Atlantic, some of the fine detritus may be carried to a greater distance, but its amount can never be very large. In oceans affected with floating ice we have land debris carried to greater distances than above stated; for instance we can detect such materials in the deposits of the North Atlantic as far south as the 40th parallel N., and in the South Pacific as far north as the 40th parallel S.

Some of the substances in solution, as carbonate of lime and silica, are extracted by animals and plants to form their shells and skeletons; these last, falling to the bottom, form a globigerina, a pteropod, a radiolarian, or a diatom ooze. We have also the bones of mammals and fish mixed up in different kinds of deposits. These, as well as animal and vegetable tissues, generally are a source of phosphates, fluorides, some oxide of iron, and possibly of other inorganic material.

Sir Wyville Thomson, early in the cruise, suggested that much of the inorganic material in deposits is derived from the source to which I have just alluded. Our subsequent observations have, I think, shown that originally Sir Wyville gave too much importance to this as a source of the materials in our deep deposits.

Second.—We have the dust of deserts, which is carried great distances by the winds, and which, falling upon the ocean, sinks to the bottom and adds to the depositions taking place. In the trade wind regions of the North Atlantic we have a very red-coloured clay, in deep water, which is largely made up of dust from the Sahara. Such dust frequently falls in this region as what is called blood-rain.

Third.—We have the loose volcanic materials, which have been shown to be universally distributed as floating pumice, or as ashes carried by the wind.

This short review shows that the clay in shore deposits is chiefly derived from river and coast detritus. As we pass beyond about one hundred and fifty miles from the shores of a continent the character of the clayey matter changes. It loses its usual blue colour, and becomes reddish or brown, and particles of mica and rounded pieces of quartz give place to pumice, crystals of sanidin, augite, olivine, &c. All this goes, I think, to show that in deposits far from land the clay is chiefly derived from volcanic debris, though in the region of the North Atlantic trade winds much of it may be derived from the feldspar in the dust of the Sahara.

The pumice which floats about on the surface of the sea must be continually weathering, and the clay which results and the crystals which it contains will fall to the bottom, mingling with the deposit which is in course of formation. In our purest globigerina ooze this clay and these crystals are present. If a few of the shells, say thirty foraminifera, are taken from such a deposit, and carefully washed, and then dissolved away with weak acid, a residue remains which is red-brown or grey in colour, according to the region from which the ooze came. If the same number of shells be collected from the surface and dissolved away in the same manner, no perceptible residue is observed. The clayey matter would therefore seem to have infiltrated into the shells soon after they fell to the bottom.

I have already mentioned several instances of pumice-stones having been found on coral reefs. Many more instances could be given. These stones, undergoing disintegration in these positions, add clay, crystals of augite, hornblende, magnetic iron ore, &c., to the limestones which the coral animals are building up.

I have found these crystals in the limestones and red earth of Bermuda, and in a specimen of the limestone from Jamaica.

This observation, it appears to me, points out that the red earth of Bermuda, Bahamas, Jamaica, and some other limestones, may originally have been largely derived from fragmental volcanic materials, which were carried to the limestone while yet in the course of formation. There are also small particles of the peroxide of manganese in the red earth of Bermuda.

Peroxide of Manganese.

Peroxide of Manganese occurs widely in ocean deposits, either as nodules, incrustations, or as depositions on the bottom itself. It has been found most frequently in the nodular form in the deep-sea clays far from land. It also occurs in the organic oozes, when these contain much volcanic debris, or are near volcanic centres.

In shallow water, near some volcanic islands, it covers shells and pieces of coral or pumice with a light brown incrustation. It has been met with very sparingly, if at all, in shore deposits removed from volcanic centres.

In my preliminary report above referred to, I stated that further investigation might show that manganese nodules and depositions abound in these regions where we have much of the debris of augitic or heavy lavas.

A re-examination of specimens since our return confirms this view. Wherever we have pumice containing much magnetite, olivine, augite, or hornblende, and these apparently undergoing decomposition and alteration, or where we have evidence of great showers of volcanic ash, there we find the manganese in greatest abundance. This correspondence between the distribution of the manganese and volcanic debris appears to me very significant of the origin of the former. I regard the manganese, as we find it, as one of the secondary products arising from the decomposition of volcanic minerals.

Manganese is as frequent as iron in lavas, being usually associated with it, though in very much smaller amount. In magnetite and in some varieties of augite and hornblende the protoxide of iron is at times partially replaced by that of manganese.

In the manganese of these minerals, and in the carbonic acid and oxygen of ocean waters, we have the requisite conditions for the decomposition of the minerals, the solution of the manganese, and its subsequent deposition as a peroxide.

The carbonic acid converts the silicates of the protoxides of manganese, and the protoxides of manganese into carbonate of manganese, and thus prepares the way for oxidation by the oxygen of the water.

It is probable that the action of the carbonic acid is not apparent, and that the manganese is at once deposited as a high oxide if not as the peroxide. This theory is essentially the same as that which Bischof gives for manganese ores generally. I have laid a series of these manganese depositions on the table. An inspection of these and their localities will show that in the clays and oozes the depositions are nodular in form. If a section be made of one of these, a number of concentric layers will be observed arranged around a central nucleus—the same as in a urinary calculus. When the peroxide of manganese is removed by strong hydrochloric acid, there remains a clayey skeleton which still more strongly resembles a urinary calculus.

This skeleton contains crystals of olivine, quartz, augite, magnetite or any other materials which were contained in the clay from which the nodule was taken. In the process of its deposition around a nucleus, the peroxide of manganese has inclosed and incorporated in the nodule the clay and crystals and other materials in which the nucleus was imbedded. The clayey skeleton thus varies with the clay or ooze in which it was formed. Those from a fine clay usually adhere well together; those from a globigerina ooze have an areolar appearance; those from a clay with many fine sandy particles usually fall to pieces. Mr Buchanan informs me that the purest portions of these nodules, that is those portions made up of closely packed concentric layers, contain from 30 to 34 per cent. of the peroxide.

Taking the nodule as a whole, it will of course contain very much less than this. The nucleus varies in each nodule, and that part of a nodule which is made up of concentric layers will vary with each locality, and with the depth from which it comes. We may expect therefore that analysis will show considerable variations in the amount of alumina, silica, and metals, lime, &c., in the nodules from different stations. At some places in the Pacific the nodules show periods of deposition very distinctly. We have first a very compact nodule which may have a shark's tooth for a nucleus, and which appears to have been formed slowly. Then there would seem to have been a shower of ashes. After a time manganese was again deposited, inclosing in the nodule a layer of these ashes. The most frequent nucleus in the nodules is a piece of pumice or other volcanic fragment.

In deep-sea clays, far from land, sharks' teeth, ear-bones of whales, and fragments of other bones are very often the nucleus around which the manganese is deposited. In one instance a piece of siliceous sponge forms the nucleus. In a globigerina ooze a portion of the deposit has apparently formed the nucleus. In these we have perfect casts of the foraminifera, but all the carbonate of lime has been removed. The volcanic fragments which have formed the nuclei of nodules appear frequently to have undergone peculiar alterations. For instance, obsidian is usually surrounded by beautiful agate bands.

When we found the bottom composed almost entirely of volcanic ashes, or so hard from other reasons that the sounding tube did not penetrate it, the manganese was deposited in layers over the bottom itself. Large pieces of this nature were taken several times.

The escape of carbonic acid through the floor of the ocean near volcanic islands may in these regions greatly accelerate the processes which end in the deposition of the peroxide of manganese, and account for the great abundance of it in some such localities where we found it.

Native Iron and Cosmic Dust.

While examining the deposits during the cruise I frequently observed among the magnetic particles from our deep-sea clays small round black coloured particles which were attracted by the magnet, and I found it difficult to account for the origin of these.

On our return home I entered into a more careful examination of the magnetic particles. By means of a magnet carefully covered with paper I extracted these particles from the deposits, from the pumice-stones, and from the manganese nodules of many regions. The great majority of these magnetic particles are magnetic iron ore and titaniferous iron, either in the form of crystals, or as fine dust. In the clays, and in the manganese nodules from stations far from land and in deep water, there were again noticed many small, round spheres among the magnetic particles.

On mentioning this to Professor Geikie, he suggested that I should try the method employed by Professor Andrews of Belfast for detecting minute particles of native iron.

This process consists in moistening the magnetic particles, which have been extracted by means of the magnet, with an acid solution of sulphate of copper, when copper is at once deposited on any native particles which may be present. In this way I have detected native iron in many of our deposits, in the powdered portions of manganese nodules, and in pumice-stones.

Professor Andrews tells me that there can be little doubt that the particles on which copper is deposited are native iron, as he has found that it is not deposited on nickel, and the chances of cobalt being present are very slight. Professor Andrews warned me on the extreme precautions necessary in conducting these observations, that no iron from a hammer or other instrument should get at the specimen under observation.

It is true that all specimens of our deposits have been obtained by means of dredges and iron gear, and some of these particles may be from this source.

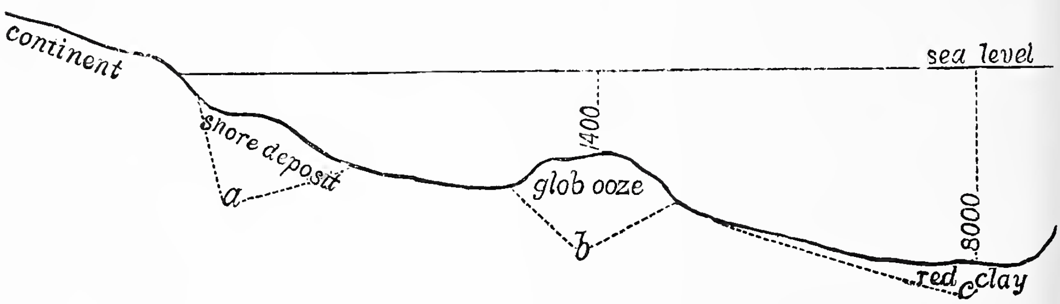
Many of the particles must have another origin. I have taken two of our manganese nodules, and washing them carefully, taking care to let no iron instrument come near them, have broken them by rapping them together. Then taking only the interior parts of these nodules I have pulverized them in a porcelain mortar. The magnetic particles were afterwards extracted by a magnet covered with paper. Now, placing these particles on a glass slide under the microscope, and adding the sulphate of copper solution, there was in a few moments a deposit of copper on several small perfect spherules, varying in size from the $\frac{1}{1000}$ to the $\frac{1}{300}$ of an inch in diameter. I have placed some of these spherules under the microscope and now show them to the Society. It will be noticed that on one the copper is not deposited all over the sphere, but in ramified spider-like lines. On the cut surface of a meteorite, from Professor Sir Wyville Thomson's collection, which I also exhibit, the copper is precipitated in precisely the same manner as on the little sphere from the manganese nodule. Besides the spherules on which the copper is deposited, there are others generally of a larger size and dark colour. These are, so far as microscopic examination shows, quite like the particles on the mammillated outer surface of this Cape meteorite, also from Sir Wyville's collection.

These spherules have hitherto only been noticed in those deposits in deep water far from land, and where for many reasons we believe the rate of formation of deposits to be very slow.

They occur also only in those manganese nodules which come from the same deep-sea clays or deposits far from land.

The particles of native iron found in pumice-stones are not numerous, and never take the form of spherules so far as observed. Some of these particles of native iron may then come from the dredge. Other particles come from the pumice and the volcanic materials. Professor Andrews long since showed that minute particles of native iron existed in basalt and other rocks. And lastly the spherules of which I have been speaking appear to have a cosmic origin.

The reason, for these spherules occurring only in deposits far from land and in deep water, may be more apparent by reference to the annexed diagram, which might represent a section from the west coast of South America out into the Pacific 500 miles. Along the shores of the continent as at *a* we have an accumulation of river and coast detritus. At *b* in depths from 1400 to 2200 fathoms we have a globigerina ooze mostly made up of surface shells. At *c*, in a depth of 2300 to 3000 fathoms, all the surface shells are removed from the bottom. No coast detritus reaches this area, and we find in the deposit pumice stones, some volcanic ashes, manganese nodules, sharks' teeth, and ear bones of whales. It is only in areas like this that we find sharks' teeth and ear bones of cetaceans in any numbers. Some of them from the same haul are deeply surrounded with manganese deposit, and contain little animal matter; while others have no deposit on them, and seem quite recent. These, and other facts which might be mentioned, all argue for an exceedingly slow rate of deposition. Now it is in these same areas that the spherules of native iron and other magnetic spherules are found, both in the deposits and in the manganese nodules from them.



Finding them in this situation favours the idea that they are of cosmic origin, for in such places they are least likely to be covered up or washed away. It is certainly difficult to understand why the spherules on which the copper is precipitated have not become oxidised. If nickel be present in them, this may retard oxidation to some extent.

The manganese depositions in our ocean deposits are very different in structure and composition from any of the ores of manganese I have had an opportunity of examining, and the deposits of the deep sea far from land have not, so far as I know, any equivalents in the geological series of rocks.

All the subjects treated of in this paper are still under investigation, and at some future time I hope to present a much more detailed account.

These observations seem to me to give ground for the following conclusions:—

First, That volcanic debris, either in the form of pumice stones, ashes, or ejected fragments, are universally distributed in ocean deposits.

Second, That pumice stones are continually being carried into the sea by rivers and rains, and are constantly floating on the surface of the ocean far from land.

Third, That the clayey matter in deposits far from land is principally derived from the decomposition of the feldspar in fragmental volcanic rocks, though in the trade wind region of the North Atlantic the dust of the Sahara contributes much material for clay.

Fourth, That the red earth of Bermuda, Bahamas, Jamaica, and other limestone countries, is most probably originally derived from the decomposition of pumice stone, while these limestones were in the process of formation.

Fifth, That the peroxide of manganese is probably a secondary product of the decomposition of the volcanic rocks and minerals present in the areas where the nodules of manganese are found.

Sixth, That there are many minute particles of native iron in deposits far from land; that some of these particles are little spherules; that these last, as well as some other spherules which are magnetic, have probably a cosmic origin.

Seventh, That the peroxide of manganese depositions in the deep sea are different in structure and composition from known ores of manganese.

Eighth.—That we do not appear to have equivalents of the rocks, now forming in the deep sea far from land, in the geological series.

In conclusion, I have to acknowledge much assistance in these investigations from all my colleagues, especially my indebtedness to Sir Wyville Thomson and Mr Buchanan.

Since my return I have received many hints from Professors Tait, Geikie, Turner, Dr Purves, Mr Morrison, and other gentlemen.

In much of the mechanical work which an examination of these deposits has entailed, I have, both during the cruise and since my return, had the assistance of Frederick Pearcey.

4. On New and Little-known Fossil Fishes from the Edinburgh District, No. I. By R. H. Traquair, M.D., F.G.S.

FAMILY PALÆONISCIDÆ.

Genus *Nematoptychius*, Traquair, 1875.

This genus was instituted by the author for the reception of the *Pygopterus Greenockii* of Agassiz, and characterised in the "Annals and Magazine of Natural History" for April 1875. It differs from *Pygopterus* in the form of the scales of the flank, which are much higher than broad, and having their articular spine arising from the whole, or nearly the whole of the upper margin; in the structure of the pectoral fin, in which all the rays are articulated for the greater part of their extent; and in the form of the anal, which is in shape like the dorsal, and is not produced backwards in the peculiar fringe-like manner characteristic of *Pygopterus*.

Since the publication of the notice above referred to, remains of *N. Greenockii* have turned up in two other localities near Edinburgh, viz., near Juniper Green in the horizon of the Wardie Shales (Museum of Science and Art), and at Raw Camps, near Mid Calder, in that of the Burdiehouse Limestone (Collection of the Geological Survey of Scotland).

The following is new to science.

Nematoptychius gracilis, sp. nov. Traquair.

Of this two specimens have occurred at Gilmerton. The more perfect of these, compressed on its side, displays the entire contour of the fish, including all the fins, and measures 9 inches in total length, by $1\frac{1}{8}$ inch in depth between the head and the ventrals; the length of the head is contained about $4\frac{1}{2}$ times in the total. The ventral fins arise opposite a point $3\frac{1}{2}$ inches distant from the tip of the snout; the commencement of the anal is midway between that of the ventral and of the caudal; the dorsal is situated nearly opposite the anal, commencing only $\frac{1}{4}$ inch in front of it. The form of the fish is thus elongated and slender, gradually tapering from behind the shoulder towards the tail, the dorsal fin being situated very far back. The other and slightly longer specimen lies compressed on its back, and shows the under surface of the head, both pectoral and both ventral fins, with traces also of the dorsal, anal, and caudal. Its length is $9\frac{1}{2}$ inches, but the fish must originally have been a little longer, as its caudal extremity is imperfect.

The scales are very small, and much disarranged, as is the case in nearly all the fishes occurring in the same bed, but their configuration is apparently the same as in *N. Greenockii*, and their external ornamentation consists, as in that species, of very fine oblique, thread-like, branching, and anastomising ridges; the median ridge-scales, extending from behind the dorsal fin along the upper margin of the caudal body-prolongation, are very large and pronounced. The bones of the head are much crushed and broken, so that only a few of them can be made out. The suspensorium is very oblique and the gape extensive, the length of the lower jaw being, in the first mentioned specimen, $1\frac{1}{2}$ inch. The external surface of the lower jaw is ornamented with a minute and very close tuberculation; the dental margin of the maxilla is also tuberculated, but the rest of its surface is marked with delicate ridges, which run for the most part parallel with its superior and posterior margins. Large conical teeth occur in both jaws, with a few of the external series of smaller ones. One of the larger teeth in the mandible of specimen No. 2 measures a little over $\frac{1}{8}$ inch in length by $\frac{1}{16}$ in diameter at the base; its form is regularly and acutely conical, with well-marked smooth enamel-cap at the apex, below which the surface is delicately and beautifully striated, the striæ being more pronounced than is usually the case in *N. Greenockii*. The opercular bones are not visible, but some of the branchiostegal rays may be detected; their outer surfaces are ornamented by very fine flexuous ridges. The paired fins are moderate in size; indeed they might safely be termed small. The pectoral of No. 1 measures one inch in length; in neither specimen can its number of rays be ascertained, but it is evident that the principal ones were unarticulated for about $\frac{1}{3}$ of their length. The length of the ventral in the first specimen is $\frac{3}{4}$ inch; its rays, probably 25 in number, are delicate, smooth, and with their transverse articulations rather far apart. The dorsal and anal fins resemble each other in form, but the latter is apparently slightly the larger; both are triangular, acuminate, high in front, with concavely cut out posterior margin; the height of the dorsal in front is $1\frac{1}{3}$ inch, its base of origin is disturbed; the apex of the anal is imperfect, its base measures $\frac{7}{8}$ inch. Their rays are numerous, slender, and smooth, dichotomising only near their extremities, and having their transverse articulations comparatively distant. The caudal, as it lies before us, can hardly be called inequilobate, but as the upper lobe appears somewhat crumpled, its entire length is probably not accurately exhibited. It is deeply bifurcated, the lower lobe being narrow and of great length, measuring $1\frac{3}{4}$ inch, though its extreme point is cut off; its rays are like those of the dorsal and anal, being slender, smooth, and with rather distant articulations, the joints being at first more than twice as broad, but getting a little closer towards the end of the rays; in the upper lobe the rays are, as usual, short and very delicate. Small and closely set fulcra are observable on the anterior margin of all the fins.

Remarks.—It is of course at once evident that the present species is closely allied to the powerful *Nematoptychius Greenockii*, Ag. sp., of the Wardie Shales; it differs, however, from the latter in several particulars beside its smaller size. The transverse articulations of all the fin-rays are considerably more distant; and the principal rays of the pectoral are unarticulated for a greater distance than is the case in *N. Greenockii.* The form of the laniary teeth differs also somewhat in the two species. In *N. gracilis* these are more regularly conical, gradually tapering from the base to the apex; whereas in *N. Greenockii* they assume a more slender form, owing to the more sudden narrowing of the base upwards into the body of the tooth, although the relative proportion of the diameter of the base to the length of the tooth is about the same. The specific name "gracilis" is bestowed on the present species in allusion to the slender and elegant shape of the body.

Geological Position and Locality.—The two specimens above described were found in the blackband ironstone at present wrought in Venturefair Colliery, Gilmerton, near Edinburgh, and are both preserved in the Museum of Science and Art, Edinburgh. This ironstone, well known for its *Rhizodus* remains, is a member of the Lower Limestone group of the Carboniferous Limestone series of the Lothians, and occurs along with the North Greens coal-seam, the lowest wrought coal in the Edinburgh district, between the first and second Marine Limestone beds in ascending order.

Gonatodus, gen. nov., Traquair.
Amblypterus, Agassiz (pars.).

The body is fusiform, the scales rather large, rhomboidal, ornamented with striæ and punctures, sometimes nearly perfectly smooth. Rays of pectoral fin articulated; base of ventrals short; dorsal and anal large, triangular, dorsal situated behind the middle of the back so that the middle of its base is opposite the commencement of the anal; caudal following closely on the anal, powerful and deeply cleft with well developed body-prolongation along the upper lobe; rays in all the fins numerous, and closely jointed. Suspensorium not quite so oblique as in *Palæoniscus*, *Elonichthys*, and most other genera of the family, but more so than in *Amblypterus;* operculum large, oblong, suboperculum wanting, interoperculum * quadrate; branchiostegal rays numerous, with a median lozenge-shaped plate behind the symphysis of the mandible. Jaws stout; teeth closely set and of very peculiar form, being cylindro-conical, first inclined slightly inwards, then bent outwards at an obtuse angle, the apex coming then by another curvature to point upwards (or downwards in the case of the upper jaw).

Gonatodus punctatus, Agass. sp.

Amblypterus punctatus (pars.) Agassiz, Poissons Fossiles, vol. ii. pt. 2, p. 109; Atlas, vol. ii. tab. 4 c, fig. 4, but *not* figs. 3, 5, 6, 7, 8.

(?) ———— *anconoæchmodus*, Walker, Trans. Geol. Soc. Edin., vol. ii. pt. 1 (1872), pp. 119–124.

The length of the entire specimens varies from $5\frac{1}{2}$ to $7\frac{1}{4}$ inches, but in none is the extreme point of the upper lobe of the tail preserved. The length of the head is contained about four times, the greatest depth of the body about three times in the total length up to the bifurcation of the caudal fin. The head is short, with bluntly

* The bony plate which I here denominate "interoperculum" is the same as that which, in the Palæoniscidæ, has hitherto been considered as "suboperculum." In *Rhabdolepis* there occurs between it and the operculum another and smaller plate, which I interpret as "suboperculum," and which is wanting in most of the genera of this family. (See the author's account of the structure of the Palæoniscidæ in the Memoirs of the Palæontographical Society for 1877).

rounded snout; the external ornament of the bones of the cranium proper is rugose in character. The operculum is oblong, broader above than below, and ornamented with delicate ridges which radiate from the anterior-superior angle downwards and backwards over its surface; the interoperculum is nearly square, and marked with ridges which for the most part pass horizontally over its surface from before backwards; the præoperculum is well developed. The jaws are stout; the broad part of the maxilla is ornamented with delicate and not very closely placed branching and anastomosing ridges, which for the most part tend to run parallel with its superior and posterior borders; the striation of the mandible is close, the ridges running longitudinally, but the dentary margin is finely tuberculated. The teeth with which the jaws are armed are most peculiar in form and arrangement; they are about $\frac{1}{24}$ to $\frac{1}{20}$ inch in length, slender-cylindrical in shape, but suddenly narrowed near the extremity to a sharply conical apex. Each tooth is first inclined a little inwards, then bent outwards at an obtuse angle, and again bent so that the apex comes to point upwards (or downwards in the case of the maxillary teeth). They are nearly uniform, and arranged in one closely set row, inside which there are no larger teeth, nor have I seen any evidence of smaller ones outside. The branchiostegal rays are thirteen on each side, the anterior of each series being broader than those behind, besides which there is a median lozenge-shaped plate in front. The bones of the shoulder girdle are well shown in most of the specimens, and like those of the face are ornamented externally with flexuous branching and anastomosing ridges.

The scales of the flank are slightly higher than broad, with gently concave upper, and convex lower margin; the articular spine is well marked. On the under surface, the socket for the spine of the scale below is distinct, but the keel is obsolete; the latter appears as we proceed backwards towards the tail, while the spine and its socket diminish and ultimately disappears. The posterior margin of the scale is finely and obliquely serrated. On the outer surface the margin overlapped by the scale next in front is very narrow; the exposed area is in the most anteriorly situated scales, is ornamented with delicate and rather feebly marked ridges, best marked along the anterior and inferior margins with which they run parallel, and usually becoming speedily obsolete over the rest of the scale, so as to leave a considerable space above and behind, marked only with tolerable coarse punctures, though in many cases shallow grooves also extend for some distance forwards from the notches between the denticulations of the posterior border. These striæ are much more pronounced in some specimens than in others; except on the small scales at the base of the dorsal fin, where they are pretty well marked, they cease to be observable before the origin of the ventrals, whence backwards the only scale-ornament consists of scattered punctures passing into short oblique furrows, especially near the interior margin, these punctures and furrows being persistent even on the small lozenge-shaped scales clothing the sides of the caudal body-prolongation. The scales also vary considerably in size on different parts of the body, becoming rapidly smaller posteriorly. One from the middle of the flank between the head and ventral fins in a specimen of 6 inches, measures nearly $\frac{1}{5}$ inch in height by $\frac{3}{20}$ in breadth, while the breadth of one from the side of the posterior part of the body opposite the termination of the dorsal fin is only equal to one-half that of the former. The scales immediately below and adjoining the base of origin of the dorsal fin are of particularly small size. The usual large scales are seen in front of the dorsal and anal fins, and the squamation of the prolongation of the body axis along the upper lobe of the caudal present nothing specially worthy of note.

The pectoral fins are acutely pointed, and of considerable expanse. The length of each is equal to about three-quarters that of the head; the ventrals are smaller; the base of each is rather short. They are likewise pointed in shape, and have their posterior margins considerably cut out. The dorsal is placed behind the middle of the back, so that the centre of its base is nearly opposite the commencement of the anal; both these fins are large and triangular; the caudal is powerful and deeply cleft. The rays of all the fins are very numerous and delicate; in the pectoral these cannot be less than 30; in the ventral, 23; in the dorsal and anal, 45 each; those of the caudal cannot be counted. The articulations of the fin rays are also tolerably close, especially in the finer rays of the posterior part of each fin, where the joints appear nearly square, being in the other rays rather longer than broad; the articulations are more than usually distant towards the proximal extremities of the principal rays of the pectoral, and also, though to a less extent, in the lower lobe of the caudal. The joints are scale-like in general aspect; the distal margin of each is notched or concave, the proximal correspondingly convex; and the outer surface is in most cases marked with at least one delicate furrow parallel with the anterior and posterior borders; near the bases of the dorsal and anal fins the joints present indeed an appearance as of fine striation. The fulcra of the anterior margins of the fins are closely set and very minute, being only observable with the aid of a lens.

Remarks.—Under the name of *Amblypterus punctatus*, three imperfect specimens of fossil fish from the shales of Wardie were described and figured by Agassiz in the "Poissons Fossiles." One of these is a head with the anterior part of the body (Atlas, vol. ii. tab. 4 B, fig. 4); the second (*ib.* fig. 5) wants the head and shoulders and the extremity of the tail; the third (*ib.* fig. 3) displays the entire caudal fin, but is obliquely cut off just in front of the dorsal and anal. The principal characters which he assigned to this species were—the considerable depth of the anterior part of the body; the character of the teeth, which had the form of small obtuse cones apparently disposed in several rows; and the ornament of the scales, which consisted, in those in front, of oblique undulating lines closer together at the anterior part of each scale and mingled with punctures, the latter more numerous towards the posterior margin; while on the scales of the hinder part of the body those lines were less crowded, and the punctures more numerous, the former entirely disappearing on the scales of the pedicle of the tail, where only a few scattered points were left.

For some time after commencing the study of the fossil fishes of Wardie, this species "*punctatus*," stated by Agassiz to be common in that locality, was to me a complete enigma, as among the numerous *entire* specimens of fish which I had the opportunity of examining, I could not find one which agreed in all its characters with Agassiz's description, though in some the *anterior*, and in others the *posterior* part of the fish answered well enough. The mystery was, however, at once cleared up by an examination of the figured specimens, of which two, collected by Lord Greenock, are in the collection of the Royal Society of Edinburgh; while the third, collected by Dr Buckland, and contained in the Oxford University Museum, was forwarded to me with great liberality by Professor Prestwich. A comparison of these specimens with a series of entire fishes from the Wardie beds establishes the fact, that the *Amblypterus punctatus* of Agassiz was founded upon fragments of two distinct species, the specimen with the head, but without the hinder part of the body, being even generically distinct from the other two, which display the hinder part, but without the head,—the fishes to which they respectively appertain differing not only in dentition, but also in many other particulars connected with the head, the scales, and the fins.

The peculiar form and arrangement of the teeth in the first of these render necessary the institution of a new genus, to which I have given the name *Gonatodus*.* These teeth were somewhat incorrectly described by Agassiz as being "en cones obtus," this appearance in the specimen he examined being due to their being there only seen in antero-posterior vertical section, their peculiar flexures and pointed apices being invisible; their being disposed "sur plusieurs rangées" seems also to be an error as far as the mandibles and maxilla are concerned, though probably there were additional teeth on the margin of the palate. Perfect examples of the species to which the other two type specimens belong show that the teeth are in it acutely conical, incurved, and of different sizes, large and small, and that in these and other respects the fish is closely allied to the *Amblypterus nemopterus*, *Palæoniscus striolatus*, and *P. Robisoni* of Agassiz, along with which forms it is, in my opinion, referable to the genus *Elonichthys* of Giebel.† It now, however, becomes a question for which of these two fishes the specific term "*punctatus*" should be retained. Now, although the enlarged representations of scales given by Agassiz (Pois. Foss. Atlas, vol. ii. tab. 4 c. figs. 6 and 7) are taken from the second species (*Elonichthys*), yet the term is indeed applicable to both, and as the characters of the head and teeth are

* γονυ, *knee;* and οδους, *tooth.*

† The reasons for removing these forms from the genera *Amblypterus* and *Palæoniscus*, and uniting them with *Elonichthys*, will be given in my next communication.

those which more especially distinguished Agassiz's conception of "*Amblypterus punctatus*" from his *A. nemopterus* with which he contrasted it as occurring in the same beds, it seemed to me more appropriate to retain his name "*punctatus*" for the species of which these peculiarities are characteristic. For the other I propose the name *Elonichthys intermedius*; it is very closely allied both to *E. nemopterus* and *E. striolatus*, and will be described in my next communication on the fossil fishes of the Edinburgh district.

The peculiar dentition of *Gonatodus* was, however, first correctly described by Mr R. Walker* in a fish from the oil shales of Pitcorthie, Fifeshire, to which he gave the name of *Amblypterus anconoœchmodus*, the horizon in which it occurred being probably that of the Burdiehouse Limestone. Mr Walker's fish undoubtedly belongs to the same genus with that described above, and may possibly be the same with *G. punctatus*—if not, it is certainly very closely allied, —but I have had no opportunity of instituting a comparison by means of actual specimens. Mr Walker, however, makes no mention of punctures as a scale ornament, and his figures represent the entire surface of the scale covered—as he says in the text—"with fine striæ which run parallel with the anterior and lower margins," and "are more conspicuous on the scales of some specimens than on those of others." † Regarding the arrangement of the teeth in the lower jaw, Mr Walker also states that they "are placed alternately one close to the outside margin, the next to it is fully half its own thickness farther in, and so on the whole length of the bone,"—an appearance of which I have observed some slight indications in *G. punctatus*, but not with the regularity described.

Geological Position and Locality.—The specimens from which the above description of *Gonatodus punctatus* has been drawn up are in the Museum of the Royal Society of Edinburgh and in the private collection of the author. They are all preserved in nodules of clay

* "On a New Species of *Amblypterus* and other fossil fish remains from Pitcorthie, Fife." Trans. Edin. Geol. Soc. vol. ii. pt. 1 (1872), pp 119–124.

† In the Wardie specimens the scales appear for the most part dull, and delicately striated all over; this is, however, *internal structure*, not external sculpture, and is due to the flaking off of the external ganoine layer. When this is preserved *in situ*, as it is here and there in many specimens, the surface of the scale is brilliant, largely punctured, and the appearance of striation more or less limited or obsolete, as already described.

ironstone, from the lower Carboniferous shales of Wardie, near Edinburgh, belonging to the Cement-stone group of the Calciferous sandstone series.

The next species is new, and from a higher horizon.

Gonatodus macrolepis sp., nov. Traquair.

Description.—The usual length of examples of this species is from four to six or seven inches, but although a considerable number of specimens more or less perfect have occurred in no two do the proportional measurement agree, owing to the greater or less amount of alteration of form, frequently amounting to positive distortion, which they have undergone, apparently both soon after death, and also during the consolidation of their ironstone matrix. Seldom do the scales, save on the caudal body-prolongation, remain in their original relations to each other on any considerable part of the body, but are always more or less jumbled up, even though the contour of the fish may remain tolerably regular, and the shape and structure of more or fewer of the fins be quite intact. As remarked in the description of *Nematoptychius gracilis*, this condition affects nearly all the small fishes occurring in the Gilmerton ironstone. In one example of the present species in my collection, the apex of the anal fin appears neatly cut off, and dislocated a quarter of an inch forward from the rest of the fin.

The most perfect specimen in my collection is $6\frac{1}{2}$ inches in length by $1\frac{3}{4}$ in depth at the ventral fins, and is nearly perfect. In no specimens are the bones of the head well shown, these being always more or less crushed and broken; what can be seen of them shows that they were conformed essentially as in the preceding species, and sculptured much in the same way, though perhaps a little more coarsely. The configuration of the teeth is also essentially the same, though they appear to be a little more clumsy in shape, and not quite so regular in arrangement. But the most salient peculiarity of the present species lies in its scales, which are considerably larger than in *G. punctatus*, except on the caudal body-prolongation, where they are equally small. Their outer and brilliantly-polished surfaces are also devoid of ornament, and might indeed be described as completely smooth, only a few delicate punctures being discernible by careful examination with the lens;

their posterior margins are finely denticulated as in the preceding species. The form and position of the fins is the same as in *G. punctatus*, but their rays are slightly coarser and proportionally fewer in number, though it is difficult to ascertain with accuracy their numbers in the various fins. The articulations of the rays are also a little closer, but the configuration of the joints is the same, these being emarginate distally, convex proximally, and with a little furrow parallel with their anterior and posterior margins, but I have not observed any additional furrows or striæ than those near the bases of the dorsal and anal as in the Wardie species.

Geological Position and Locality.—A considerable number of examples of this species have occurred in the Blackband ironstone at Venturefair Colliery, Gilmerton, and are contained in the Edinburgh Museum of Science and Art, and in the private collection of the author. A fragment in the Hunterian Museum of the University of Glasgow, from the ironstone of Possil, also a member of the Carboniferous Limestone series, though higher in position than that of Gilmerton, is probably also referable to the same.

5. On the Ruff (*Machetes pugnax*). By Professor Duns.

The exceedingly beautiful bird now on the table was forwarded to me on the 1st of September last by Mr Wilson, Edington Mains, Chirnside, Berwickshire. It had been shot two days previously. Mr Wilson says, "On comparing it with all the Waders figured by Bewick (the only work of the kind which I possess), I find none that correspond; whence I infer that it is really a *rara avis.*" He adds, "When noticed by the edge of the pond by my children it had a young one with it, which they saw it feeding. The young one, they said, was much lighter in its plumage, but was old enough to fly strongly." The note led me to expect a full-grown female wader and young one. But the size of the bird and its general features of maturity showed it to be a male, in full winter plumage. Its companion, described as young, doubtless from its size, seems to have been the female, or reeve, which is little more than half the size of the male. In this respect the ruff differs from most of the sub-family *Tringinæ*, in which the females are generally larger than the males.

The ruff is polygamous. After the breeding season a separation takes place between the sexes, which continues till the approach of the following spring. The example now before us is thus exceptional, as occurring along with one female long after the breeding season was past, which is late in the spring, or in May. At one time these birds were met with in great numbers in the low-lying districts of Norfolk and Lincoln, where they bred. Even in these localities they are now seldom met with. There are no instances on record of their nesting in Scotland, where they are even more rare than in England. "On the east coast of Scotland," says Macgillivray, "they usually appear about the middle of September and depart in about a fortnight; but I have never seen an adult male killed there, the little flocks that occur being young birds and females." "Except in a very few instances," says Mr Gray, "I have never met with the ruff in the western counties. One or two occasionally penetrate as far as the Clyde, but these are mere stragglers."* The forms now noticed appeared at a considerable distance from the coast, in a district, however, which at one time must have presented as good nesting ground as could be found in Norfolk or Lincoln, but which by drainage and many other features of high farming has become almost free from conditions suitable to their habits. It would almost seem as if some birds have a transmitted instinct towards certain localities, which at long intervals finds high expression in certain pairs. Might not this account for the occasional occurrence of the bittern, the night heron, and many other forms in districts where they are now regarded as stragglers? I know, moreover, one locality where the names of places show how common the raven had been at one time, but where only one pair is known to have appeared in forty years. Many instances of this kind might be given. The habits of the ruff have been, I might almost say, so exhaustively described by Montague, Rennie, Selby, and others, as to make it unnecessary to refer to them here. I have, however, placed on the table other specimens than that now noticed, with

* Dr John Alexander Smith has noticed specimens from Carnwath, Alloa, Portobello, and Fenton Barns, East Lothian. As many as a dozen were recently seen near Grangemouth by Mr Harvie Brown, five of which were shot.

the view of indicating the relative size of male and female, of pointing out the profuse and almost grotesque ornamentation of the males at the breeding season, and of showing the wide variation in the plumage of male birds especially. During the breeding season numerous papillæ, or fleshy tubercules, appear on the face, a couple of ear tufts slope gracefully to the hind head, and a large frill or ruff of elongated feathers surrounds the neck. These features continue till about the middle of June, when the birds moult. They then put on the autumn and winter garb. It is next to impossible to find two males ornamented alike. But it is known that the colouring of the frill first assumed is retained through all the changes of plumage that follow in after years. If red originally, it will have that colour as successive breeding times come round. If black, or white, these colours reappear in their season. The fact is curious and interesting from the physiological point of view. The theory of sexual selection does not help to explain it. The birds are polygamous, and the male has to fight to obtain the female. It is not, however, the size or colour of the frill, but the strength of beak and of leg that conquers. Then what is it that determines the colour of the frill in different birds of the same species, and what keeps the hue persistent throughout all the yearly changes of plumage? The questions are easily put. One would like if they were as easily answered.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. IX. 1876–77. No. 97.

Ninety-Fourth Session.

Monday, 15th January 1877.

Sir William Thomson, President, in the Chair.

The following Communications were read:—

1. Communication from the President, relative to the Administration of the Government Fund of £4000.

2. On New and Little-known Fossil Fishes from the Edinburgh District. No. II. By R. H. Traquair, M.D., F.G.S.

Genus *Elonichthys*, Giebel, 1848.

Amblypterus (pars), Agassiz.
Palæoniscus (pars), Agassiz.
Pygopterus (pars), Agassiz.

Body fusiform, sometimes rather deep; median fins large, the caudal deeply cleft, inequilobate; dorsal and anal triangular, acuminate, the dorsal situated nearly opposite the interval between the ventrals and the anal; base of ventrals not extended; pectoral rays articulated (except a varying amount of the commencement of the principal ones). Fin rays ganoid, closely set, striated; fulcra closely set, minute. Scales sculptured, of moderate size, rhomboidal, their posterior borders frequently denticulated or serrated; the anterior overlapped area very narrow, reduced to a mere margin. Suspensorium very oblique; gape extensive; operculum well developed, oblong; interoperculum square-shaped; no suboperculum. Branchiostegal rays numerous; the anterior one of each lateral series broader than the rest a median lozenge-shaped plate in front. Jaws stout; teeth acutely conical, sharp, of two sizes, large and small, the larger ones being placed in a row internal to the more closely set outer series of smaller teeth. Ornament of cranial bones tubercular or rugose-tubercular; facial bones, and those of the shoulder-girdle striated; the jaws usually also tuberculated just at the dental margin.

The name *Elonichthys* was first proposed by Giebel* for certain fishes (*E. Germari*, *crassidens* and *lævis*) from the coal-measures of Wettin, near Halle, which he considered intermediate in generic character between *Amblypterus* and *Palæoniscus*, as defined by Agassiz. They were said to resemble *Palæoniscus* in their fulcrated fins, but to differ from it in the absence of the scaly covering to the rays, affirmed by Agassiz to exist in some Palæonisci, while to certain "*Amblypteri*" they showed an affinity in the striation of their scales, and to "*Amblypterus*" in general in the large size of the fins. Their special characteristics were to be found in the dentition, which consisted of an external series of minute teeth comparable to the "Bürstenzähne' of *Amblypterus*, between which there were large conical teeth, "wie ich dieselben weder bei den Palæonisken noch Amblypteren finde." Unfortunately, however, for this diagnosis, the fin-rays of *Palæoniscus* are no more covered with scales than those of any other genus belonging to this family; nor are the fins of Agassiz's "*Amblypteri*" destitute of fulcra "except on the upper lobe of the tail," as has been so repeatedly stated by compilers, who, copying this error from the "Tableau synoptique des Genres et des Espèces" have apparently overlooked the correction of it made by Agassiz himself a few pages further on in his general description of the genus.† The dentition, too, of Giebel's *Elonichthys* is essentially similar to that of Agassiz's *Amblyterus macropterus* (*Rhabdolepis* Troschel) in which large conical teeth were shown to exist by Goldfuss in 1847,‡ and again by

* Fauna der Vorwelt, 1, 3, p. 249.

† Poissons Fossiles, vol. ii. pt. 1. p. 29.

‡ Beiträge zur vorweltlichen Fauna des Steinkohlen-gebirges. Bonn 1847, p. 20.

Troschel in 1857,* the latter author using this character to separate the striated-scaled "*Amblypteri*" of Saarbrücken and Lebach, under the name of *Rhabdolepis* (*R. macropterus*, *R. eupterygius*) from their smooth-scaled associates with minute slender teeth, for which the term *Amblypterus* was retained (*A. latus*, *A. lateralis*).

An examination of the type-specimens of *Elonichthys* in the Mineralogical Museum at Halle has, however, convinced me that the genus is tenable, and that to it is referable an extensive series of Palæoniscidæ, which will include, besides Giebel's originals and several new species, various forms referred by Agassiz and other writers to *Amblypterus*, *Palæoniscus*, and *Pygopterus*. Several species also, whose position, from want of sufficient information, is somewhat doubtful, may best be placed here provisionally.

Elonichthys most closely resembles *Acrolepis* and *Rhabdolepis*, but differs from the former in the anterior covered area of the scales being reduced to a very narrow margin, and from the latter in the absence of the subopercular plate. From *Amblypterus*, as restricted by Troschel, the greater obliquity of the suspensorium, and the dentition, are obvious marks of distinction; the teeth in the true *Amblypteri* being minute, slender, without differentiated laniaries, and more meriting the title "en brosse" than those of any other genus of Palæoniscidæ. From *Palæoniscus* it is distinguished by the large size of the median fins, and by the possession of large conical teeth in the jaws, the teeth of *Palæoniscus*, as that genus must now be restricted, being small, and though, of different sizes, without conspicuous laniaries. It is also widely separated from *Pygopterus* by the structure of the pectoral fin, in which the principal rays are articulated for the greater part of their length, whereas in the last-named genus, they are unarticulated till towards their terminations; by the position of the dorsal fin, which, in relation to the anal, is further forward; and by the form of the anal, which is not produced backwards in a fringe-like manner along the lower margin of the body.

Accordingly *Amblypterus nemopterus* of Agassiz, and one of the two species included in his *A. punctatus*, must be transferred to *Elonichthys*, as well as his *Palæoniscus Robisoni*, *P. striolatus*, *P. Egertoni*, and *Pygopterus Bucklandi*. The mutual resemblances of

* "Verh. des naturhist. ver. d. preuss. Rheinlande," xiv. (1857) p. 12.

these forms are so close that it seems almost unaccountable that they should have been placed by Agassiz in different genera; his having done so seems, indeed, to indicate the arbitrary nature of the distinction which he drew between *Amblypterus* and *Palæoniscus*, as well as his not having fully realised the nature of the essentially distinctive characters of the true *Pygopteri*.

Amblypterus Portlockii of Sir Philip Grey-Egerton * seems to belong to this genus, as probably also does *Palæoniscus Brownii* of Jackson, judging from the figure given.† The same must also be said of *P. peltigerus* of Newberry,‡ which was indeed first described by that author as an *Elonichthys*.§

As above defined, *Elonichthys* is pre-eminently a carboniferous genus, and well represented in strata of that age in Great Britain and other countries. The necessary restriction of the limits of the genera *Palæcniscus*, *Amblypterus*, and *Pygopterus*, and the transference of the carboniferous species formerly referred to them to *Elonichthys* and other genera (*Nematoptychius*, *Rhadinichthys*) leave the three first named without representatives in the carboniferous formation, so far as our present knowledge goes,—a conclusion which may appear at first somewhat startling to geologists, who have been so long accustomed to look upon them as characteristic of that era, as well as of the succeeding Permian.‖

Elonichthys nemopterus, Agass. sp.

Amblypterus nemopterus, Agassiz, Poissons Foss. vol. ii. p. 1 p. 107; Atlas, vol. ii. pl. 46, figs. 1 and 2.

To Sir Walter C. Trevelyan, Bart., of Wallington, Northumberland, I am indebted for the loan of the original example of this species figured by Agassiz; the only other specimen, which I can with certainty identify with it, is contained in my n collection. Its principal specific characters may be summed up as follows :—

Length from 4 to 5 inches; scales rather small, their posterior

* Quar. Jour. Geol. Soc. vi. 1850, p. 2.

† Report on the Albert Coal Mine, New Brunswick, p. 52, pl. i. fig. 2.

‡ Geol. Survey of Ohio, Palæontology, vol. i. p. 345, pl. xxxviii. fig. 1.

§ Proc. Acad. Nat. Sc. Philad., 1856; pp. 96–100.

‖ The strata at Saarbrücken and Lebach, containing the typical *Amblypteri*, as well as the fish-bearing beds of Münster-Appel, Kreuznach, Goldlauter, &c., in Germany, and of Autun in France, long believed to belong to the carboniferous formation, are now referred by continental geologists to the Lower Permian ("unteres Rothliegendes").

margins finely denticulated, ornamented with delicate ridges, which are mostly parallel with the anterior and lower margins, some above being parallel with the upper one, and which in the hinder part of the fish, tend, on the posterior part of the scale, to become replaced by punctures; dorsal and anal fins very high in front sharply acuminated; the rays of all the fins delicate, with rather distant articulations (in the principal rays at least twice as long as broad).

By Agassiz the rays of the dorsal fin are described as being "tous bifurqués a plusieures reprises jusque vers leur milieu." It seems to me, however, that the dichotomisation of the longer rays, as is usual in this genus, does not commence till towards their terminations. He also described the surface of the scales as being "orneé de petites rides saillantes, disposées à-peu-près comme les lignes d'accroissement en losanges concentriques plus on moins régulières et un peu obliques, de telle sorte que leurs angles aigus sont tournés vers les angles supérieur-antérieur et inférieur-postérieur de chaque écaille,"—a description which is hardly correct, as there are no striæ parallel with the posterior margin of the scale. The scales of the type-specimen are, however, in very bad preservation, the exterior ganoine layer having everywhere disappeared, and their markings can only be made out from the impressions on the counterpart. Agassiz seems also to have been much struck by its resemblance to the *Amblypterus* (*Rhabdolepis*) *macropterus* of Saarbrücken; to me, however, although the bones of the head are very badly preserved, its affinities to the following species seem so plain and obvious, that I have no hesitation in referring it to the same genus.

Geological Position and Locality. In ironstone nodules from the Lower Carboniferous shales of Wardie, near Edinburgh.

Elonichthys intermedius, sp. nov. Traquair.

Amblypterus punctatus (pars) Agassiz "Poissons Fossiles," vol. ii. pt. 2, p. 109. Atlas vol. ii. tab. 4c. figs. 3, 5, 6, 7, 8, but *not* fig. 4.

Eliminating from the three figured specimens of "*Amblypterus punctatus*" Ag., the one showing a head, and which in my last communication to the Society I have made the type of a new genus, *Gonatodus*, retaining the original specific name *punctatus*, the two remaining headless specimens seem undoubtedly to agree with each other, and also with a series of more or less perfect fishes in my own collection, which belong to a species perfectly distinct, even generically, from the first. The presence in the jaws of this species of sharp conical teeth of different sizes, as well as its entire aspect, shows that it is referable to the same genus as *Elonichthys striolatus*, (*Palæoniscus striolatus*, Agassiz) with which it is indeed closely allied.

Description.—The largest specimen in my collection measures $6\frac{4}{5}$ inches in length, but, as it is a little contorted laterally, it must have been originally somewhat longer; the more ordinary length is from $5\frac{1}{2}$ to $6\frac{1}{2}$ inches. The form of the body is fusiform, but as hardly any specimen is absolutely free from some amount of distortion, it is difficult to ascertain the proportional measurements with absolute accuracy; two very good examples, however, agree in this, that the length of the head is contained a little more than four times, and the greatest depth of the body (just in front of the dorsal fin) a little more than three times in the total length, up to the bifurcation of the caudal fin.

Many of the details of the structure of the head are clearly shown. The outer surfaces of the bones of the cranial shield display a fine tubercular ornamentation, the tubercles frequently passing into short rugæ; the superethmoidal forms as usual a blunt prominence projecting over the front of the mouth. The suspensorium is very oblique, and the gape consequently of great extent. The operculum is rather narrow, its posterior-superior angle is rounded off, the posterior-inferior one acutely pointed, its outer surface is marked with delicate and somewhat distant ridges, which mostly follow the direction of the lines of growth. The interoperculum is square-shaped and apparently similarly ornamented, though it is difficult to obtain a satisfactory view of its external surface. The exact number of the branchiostegal rays is hardly ascertainable; in many specimens the anterior one of each lateral series is distinctly shown to be much broader than the rest, and a median lozenge-shaped plate is also conspicuously present between these, and behind the symphysis of the mandible. The maxilla is of the usual form; its broad portion is ornamented with delicate raised striæ placed rather widely apart, and mostly parallel with the superior and posterior margins; they are closer and more pronounced along the dentary margin. The mandible is moderately stout, and closely striated externally with delicate ridges, which run in a direction slightly oblique to its upper border. The teeth are rarely seen, being usually covered up by the matrix, whose intense hardness defies all attempts at working them out; in some specimens, however, a few are exhibited both on the mandible and maxilla. They are conical, sharp, and incurved, and of different sizes, the largest measuring about $\frac{1}{30}$ inch in length, though in one specimen a broken tooth is perceptible which must have been larger, probably attaining about $\frac{1}{20}$ inch in length. The bones of the shoulder girdle, usually well seen, present nothing remarkable in their configuration; they are ornamented externally with closely set, wavy, branching, and anastomosing ridges.

The scales of the body are of medium size; as usual they diminish from before backwards, but not in so very marked a manner as in *Gonatodus punctatus*. Externally they are marked with oblique furrows passing into isolated coarse punctures in the posterior part of the scale. In the anterior part of the fish, and along the line of the back in front of the dorsal fin, these furrows are closer, producing sometimes a ridged appearance between them, and extend more over the surface than is the case further back, where they are shorter, and more limited to the anterior margin, so that the greater part of the exposed area of the scale is simply punctured; the furrows become, indeed, almost entirely lost on the scales of the tail pedicle. The posterior margins of the scales are finely denticulated.

The length of the pectoral fin is equal to about $\frac{2}{3}$ that of the head; it is sharply acuminated, and contains about 30 rays, which at the medial edge of the fin become very short and delicate. The first three rays on the lateral edge, which are short and do not reach the apex, seem entirely unarticulated; the articulations of the others are distant proximally, but becoming rapidly closer distally, so that over the greater part of the expanse of the fin the joints are square-shaped, becoming at length even shorter than broad. The ventrals are situated midway between the pectorals and the anal; they are also acuminate in form, each containing about 15 rays, articulated from their origins, their joints also diminishing in length from their proximal towards their distal extremities. The dorsal is situated nearly opposite the interval between it and the anal; both of these fins are very similar in shape, being triangular, very high in front, acutely pointed, and with the posterior border gently concave. Their rays appear coarse, compared with those of *Elonichthys nemopterus*, and especially with those of *Gonatodus punctatus*, so that had Agassiz only seen the fins of the latter he could not have confounded it with the present species. Each consists of about 30 rays, finely striated externally in the direction of their length; their transverse joints are conspicuously longer than broad in the principal rays, but become shorter in the posterior part of the fin, and also in the longer rays towards their extremities. The longer rays begin also to bifurcate towards their terminations, the process gradually creeping up on the posterior part of the fin till in the delicate hindermost rays it takes place about their middle,—a very general characteristic of the fins in the species of this genus. The caudal is very powerful; the rays of its lower lobe are divided by rather distant articulations, and dichotomise towards their extremities; those of the upper lobe divide about their middle, and are characterised by the extreme closeness of their articulations, the joints being rather shorter than the rays are broad. The fulcra of all the fins are minute and closely set.

Remarks.—The close resemblance which this species bears to *E. striolatus* is quite apparent; their specific distinction is also sufficiently pronounced. In *E. striolatus* the articulations of the principal rays of the dorsal and anal fins are much closer; the dorsal and anal fins do not seem to be quite so sharply acuminate in form; the laniary teeth are larger; the scale ornament, though belonging to the same general type, is more delicate in character. To *E. nemopterus* its resemblances are also very strong; it differs, however, from it in the greater coarseness of the fin rays, with closer articulations, this being especially marked in the case of the pectoral, as well as in the nature of the scale ornament. The aspect of the fin rays is somewhat intermediate between their condition in the two other species named, hence the specific term which I have adopted for the present fish.

Geological Position and Locality.—In ironstone nodules from the Lower Carboniferous shales of Wardie, near Edinburgh. In the collection of James Linn, Esq., there is also a specimen from Dechmont in Linlithgowshire, in very indifferent preservation, but which I am inclined to refer to the same species.

3. Note on Clapeyron's Formula. By Professor Dewar.

4. Note on the Specific Gravity of Ocean Water. By J. Y. Buchanan.

During the cruise of the Challenger the specific gravity of the surface water was observed daily while at sea, and that of water from the bottom and intermediate depths as often as opportunity offered. These observations were almost entirely confined to the region between the parallels of 40° N. and 40° S. latitudes, which, including as it does the regions of the equatorial calms, of the trade winds, and to a certain extent of both the northern and southern westerly winds is decidedly the most interesting field for such investigations. Many valuable observations have been made by other observers, and I have availed myself more especially of the series of observations made by Lenz during Kotzebue's voyage to the Pacific and back. His observations were almost entirely confined to the surface water.

The density of sea water depends on the amount of salt which it contains, on its temperature, and on the pressure to which it is subject. Away from the disturbing influences of the shore, the water of the ocean is subject to comparatively slight variations in saltnesss, so slight indeed that different waters may without sensible error be assumed to be equally affected by changes of temperature and pressure. As the remarks which are offered in the present paper have reference to the salinity or concentration of the water, the specific gravities have all been reduced to their value at 60° F., the unit being that of distilled water at its temperature of maximum density. The pressure is that of one atmosphere.

There are two meteorological causes which affect the saltness of the sea; the one is the formation of vapour and the precipitation of it as rain, and the other is the freezing of the water and melting of the ice thus formed. Vast quantities of solid matter, chiefly carbonate of lime and silica, are removed from the water by living organisms, and these substances form the chief components of the solid matter brought down in solution by rivers. Although this is a very important cause of the transmigration of rock, it does not sensibly affect the specific gravity of the sea-water, because only a very limited amount either of silica or of carbonate of lime can be in solution at any one time. The general distribution of saltness, as indicated by the specific gravity of the water at a common temperature of 60° F. at the surface, is shown on a chart on which localities of equal saltness are connected by lines, and the spaces between successive lines coloured distinctively. The vertical distribution is shown in diagrams in sections along certain lines of route, the depths at which the same saltness is observed being also connected by lines. The variations of saltness at the surface on the lines of section are further shown by curves having for ordinates the reduced specific gravity, and for abscissæ degrees of latitude or longitude according to circumstances.

These diagrams show at once how great are the differences in the saltness of the water in different depths and at different localities on the surface, and it must be remembered that we are here considering *bona fide ocean* water, litoral waters so much influenced by the drainage of the land being reserved for separate treatment. Looking at the chart we see that specific gravities above 1·0270 occur in three localities only—namely, in the trade-wind regions of the North and South Atlantic, and of the central South Pacific. In the North Pacific the specific gravity was nowhere as high as 1·0265, whereas large tracts both of the North and South Atlantic reached 1·0275. The greater part of the year 1873 was passed in the North Atlantic, and the central part was crossed on the homeward voyage. On the eastern side and in the track of outward bound ships, the maximum of 1·02763 was reached in latitude 24°. In the central part the maximum was 1·02768 in latitude 25°, and in the western part it was 1·02743 in 27°30′; so that between the meridians of 20° W. and 70° W. the line of maximum saltness would appear to run nearly due east and west, with a slight northerly inclination towards the west. In the central South Atlantic the maximum of 1·02768 was observed in latitude 17°26′, and in the western part 1·02783, in latitude 15°15′, so that here also the line of maximum saltness would appear to have a slight northerly trend. Between these two lines the specific gravity falls off until a minimum of 1·0260 is reached at about 3° N. latitude, and following the equator from east to west the specific gravity appears to increase pretty steadily. In the Central Pacific the maximum saltness is observed in latitude 18° S., where the specific gravity is 1·02725; in the north the maximum is 1·0263, in latitude 22° N. The minimum of 1·02488 was found in latitude 7°26′ N. In the Western Pacific the series of observations was not so continuous, there being a considerable gap between latitude 15° S. and 2 S.; the northern portion, however, is complete. Here, again, the absolute maximum is in the south, reaching 1·0268, in 22°30° S.; in the north the maximum is 1·0263 in 22° N., but the dense water of the Southern Pacific penetrates into the northern part of the ocean, giving another and higher maximum of 1·0266 in lat. 2′ N. The actual maximum observed was 1·0242 close to Humboldt Bay, in New Guinea. Other very low specific gravities were observed occasionally even at considerable distances from the coast; but the fact that there was frequently drift-wood floating about, and that the light water was confined to a stratum of 10 to 20 fathoms thickness, showed that we had here to do with shore water. If the general run of the curve be followed, the oceanic minimum will be found to occur about 10° N., marking 1·0258.

The saltness, which increases on both sides of the equator until a maximum is reached in the trade wind districts, decreases again towards the poles and the whole of the ocean to the southward of latitude 40° S., has a specific gravity below 10260, and for the greater part below 1·0255. In the neighbourhood of ice in summer great and sudden variations are frequently observed, the specific gravity amongst loose park ice frequently falling as low as 1·0240.

Confining our consideration, then, in the meantime, to the surface, we find that the maxima in the Southern Pacific and Atlantic are situated nearer the equator than those in the northern parts of these oceans; that the maxima in the Atlantic are higher than the corresponding ones in the Pacific; and that the *maximum maximorum* occurs in the South Atlantic about latitude 15° S. The equatorial minima in both oceans are to the northward of the equator; and from all these facts we see how intimately the concentration of the sea-water is connected with the distribution of the trade winds.

If we turn now to the consideration of the vertical distribution of density, we see that the influence of atmospheric conditions does not cease when we leave the surface. In the trade-wind districts we find the excessive concentration extending to greater or less depths, according to the ocean which we are considering, and, as might have been expected, this effect is much more sensible in the Atlantic than in the Pacific. In the Atlantic, again, the amount of concentration is much greater in the northern than in the southern portion, while in the Pacific the reverse is the case. The great amount of concentration in the North Atlantic is doubtless due chiefly to the form of its basin. The south-east trade which blows with a preponderating force drives constantly fresh supplies of water in to the northern basin, where a large portion of it is kept constantly circulating by the prevailing winds in a region where the atmosphere is ever greedy of moisture. This circulation, assisted by the yearly oscillations of temperature, bring about a constant concentration of the deep waters, and at the same time, as I have pointed out, a heating of the deeper waters by convection. The effect of this concentration of the water is to render it heavier than an equal bulk of water further to the south, and consequently it forces its way southwards along the bottom. This under outflow from the North Atlantic was conjectured by Captain Tizard from a consideration of the temperature of the deep water of the western basin of the South Atlantic, and a glance at the vertical section of the Atlantic is sufficient to show the great likelihood of its existence. From a consideration of this section it would appear probable that at some position about 40° north latitude a tolerably uniform density of from 1·0265 to 1·0270 will be found, and, indeed, in the neighbourhood of the Azores the following bottom specific gravities were observed :—

Depth in fathoms, . .	1675	1000	900	750
Specific gravity at 15·56, .	1·02670	1·02693	1·02691	1·02679

Although we find that in all tropical and temperate regions the temperature of the water is highest at the surface, and decreases as the depth increases, we do not by any means invariably find the highest specific gravity at the surface, nor does it decrease regularly with increasing depth. The water is usually least salt at between 500 and 1000 fathoms from the surface. The maximum specific gravity at any locality is frequently observed at depths of from 25 to 150 fathoms from the surface, and it is not unusual to find besides the normal minimum between 500 and 1000 fathoms, a second minimum at 25 or 50 fathoms from the surface. These maxima and minima are caused in some cases by currents, and in others are the evidence of past wet or dry seasons. In the Southern Ocean the surface specific gravity varies between 1·0240 and 1·0250, being nearly constant at 1·0250 in localities free from ice; it increases, however, with the depth, the bottom specific gravity being usually 1·0255 or 1·0256.

The following Gentlemen were elected Foreign Honorary Fellows, to fill up the vacancies caused by the death of Adolphe Theodore Brongniart and of Christian Gottfried Ehrenberg:—

Alphonso De Candolle, Geneva.
Professor Carl Gegenbaur, Heidelberg.

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

John Murray.
Walter Noel Hartley, Chemical Demonstrator, King's College, London.
Walter Weldon, F.C.S., London.

Monday, 29th January 1877.

Professor KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. Note on the Manganese Nodules found on the Bed of the Ocean. By J. Y. Buchanan.

The manganese nodules occur in greater or less quantity all over the ocean-bed, and most abundantly in the Pacific. They occur of all sizes, from minute grains to masses of a pound weight, and even greater, and form nodular concretions of concentric shells, round a nucleus, which is very frequently a piece of pumice or a shark's tooth. Their outside has a peculiar and very characteristic mammillated surface, which enables them to be identified at a glance. When freshly brought up they are very soft, being easily scraped to powder with a knife. They gradually get harder on exposure to the air.

The powder, heated in a closed tube, gives out water which reacts alkaline, and has an empyreumatic odour. Heated with strong hydrochloric acid, it liberates abundance of chlorine, and the residue which remains is white, consisting of silica, clay, and sand, the sand being the same as is found in the bottom mud from the same locality. Their composition varies greatly, different nodules containing different quantities of mechanically admixed mud, and the number of different elements found in them is very large. Copper, iron, cobalt, nickel, manganese, alumina, lime, magnesia, silica, and phosphoric acid have been detected in a large number; but I have not as yet been able to make a completed analysis of any of them. I have, however, made a few determinations of the most important component substances. For this purpose the outside and densest layers of the nodules were selected, and portions of them pulverised and dried for ten or twelve hours at 140° C. The amount of chlorine liberated on treatment with hydrochloric acid was determined by Bunsen's method, and the iron was determined by titration with stannous chloride. The samples analysed were from four different localities.

Nos. 2, 4, and 5 were from the same place, No. 2 being the matter collected round a shark's tooth as nucleus; Nos. 4 and 5 being the outside rinds of ordinary nodules.

The results are given in the following table, the numbers being in many cases the means of several observations :—

Locality.			A	B	C	D	E	F	G	
Lat.	Long.	No.	Insoluble Residue.	O	MnO_2	MnO	Fe_2O_3	Al_2O_3	H_2O	Na_2O
13° 52′ S.	149° 17′ W.	2	17·55	6·13	33·30	27·18	...	...	...	...
...	...	4	15·30	5·92	32·23	...	23·86	...	...	...
...	...	5	15·30	6·49	35·28	...	24 85	...	10·2	...
37° 52′ N.	160° 17′ W.	6	36·24	6.49	24·41	...	20·16	3·83	7·70	5·98
42° 42′ S.	134° 10′ E.	7	17·98	7·54	41·11	33·53	18·04	2·55	7·31	...
22° 21′ S.	150° 17′ W.	8	21·74	5.19	28·20	...	24·52	7·67	8·54	8.5

A is the residue which remains undissolved after treating the mineral with strong hydrochloric acid, evaporating to dryness and redissolving. In No. 5 it contains 85·16 per cent. silica, and in No. 6, 82·27 per cent.

B is the available oxygen determined by Bunsen's method.

C is the MnO_2 equivalent to the available oxygen.

D is the MnO found by weighing as Mn_3O_4.

E is the Fe_2O_3 found by titration with $SnCl_2$.

F is the alumina found by subtracting the Fe_2O_3 found in E from the weight of the precipitate with acetate of soda.

G is the water expelled on ignition; it is obtained by deducting two-thirds of the oxygen found in B from the loss of weight by ignition.

It will be seen from the results given in the above table that the nodules from different localities vary greatly in composition, though in the same locality they have similar composition, irrespectively of the nature of the nodules. The insoluble residue contains, besides silica and clay, sand of the same mineral nature as is found in the bottom at the same locality. The manganese is present wholly as MnO_2, and the iron as Fe_2O_3. In No. 6 there is 3 per cent. of cobalt; this metal, along with copper and a little nickel, is present in all of them. Zinc was not found in any of the above specimens.

2. Note on the Measure of Beknottedness. By Prof. Tait.

In drawing the various closed curves which have a given number of double points, I found it desirable to have some simple mode of ascertaining whether a particular form was a new one, or only a deformation of one of those I had already obtained. Of course the *schemes* (as described in my former paper) contain the desired information, but it may sometimes be difficult to obtain in this way; for, when the number of intersections is large, we may have to change the crossing which is taken as the initial one several times before we hit upon the same notation for like crossings (if such exist) in the two schemes compared. And the methods of deformation already given often present their results in forms so distorted that it is not easy at once to recognise their identity with other drawings of the very same curves.

The method of treatment described in my paper, which depends upon the study of the *plait*, supplies (by the + and − signs *over* the various crossings) exactly the sort of information we require, though it may leave ambiguities. But some simple mode of applying it is requisite.

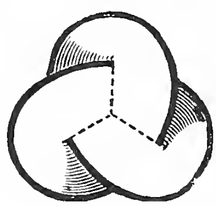
I first tried a modification of the process (formerly described) of going round the curve and pitching a coin into each field or cell as it is reached. To make the required distinction between crossing *over* and crossing *under*, we may suppose the two coins to be of different kinds,—silver and copper for instance. Let the rule be :—silver to the right when crossing over, to the left when crossing under. Then, however the path be arranged, of the four angles at each crossing, one will have no coins, the vertical or opposite corner will have *two* silver or *two* copper coins, the others *one* copper or *one* silver coin each.

It is easily seen that a reversal of the direction of going round leaves the single coins as they were, but shifts the pair of coins into the angle formerly vacant; also that in the deformed figures the circumstances are exactly the same as in the original. Hence we may divide the crossings into silver and copper ones, according as two silver or two copper coins come together. And the excess of the silver over the copper crossings, or *vice versâ*, furnishes an exceedingly simple and readily applied test (not however, as will soon be seen, in itself absolutely conclusive of identity, though absolutely conclusive against it), which is of great value in arranging in family groups (those of each family having the same number of silver crossings), the various knots having a given number of intersections.

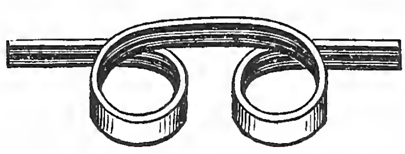
I soon saw that this process, so limited, was intimately connected with that required for the estimation of the work necessary to carry a magnetic pole along the curve, the curve being supposed to be traversed by an electric current, and it occurred to me that we might possibly obtain a definite measurement of beknottedness in terms of such a physical quantity: as it obviously must be always the same for the same knot, and must vanish when there is no beknottedness. The measure may be made more complete by recording the numbers of non-nugatory silver and copper crossings separately, with the number to be deducted as due merely to the *coiling* of the figure. I shall recur to this point later.

When unit current circulates in a circuit, the work required to carry unit pole once round any closed curve once linked with the circuit is $\pm 4\pi$. Instead of the current we may substitute a uniformly and normally magnetised surface bounded by the circuit. The potential energy of the pole in any position is always measured by the spherical opening subtended by the circuit; but its sign depends upon whether the north or south polar side is turned to the pole. Hence there is no potential energy when the pole is situated in the plane of the circuit but external to it, and the value is $\pm 2\pi$ when the pole just reaches the plane of the circuit internally. Gauss gave from these results the value of a remarkable double integral extending over each of any two closed curves linked together in space. Clerk-Maxwell (*Electricity*, § 422) has shown that this integral may vanish even for a complex linking of the two circuits; and a similar difficulty is met with in the single circuits with which we are now dealing, so that a special set of rules must be made for determining the beknottedness in terms of the silver and copper junctions. But the difficulty just mentioned leads, as will be seen, to a very curious result.

To construct the magnetised surface which shall exert the same external action on a pole as a current in any given closed circuit does, we may either suppose a surface extending to infinity in one direction (say, for definiteness, upwards from the plane of the paper), and having the circuit for its edge; or we may form, as in the figure, a finite autotomic surface of one sheet, having the circuit for its edge. The only difficulty in estimating the work lies in the definite statement of how the pole is to move along the curve itself. For, if its path screw round the curve, $\pm 4\pi$ must be added to the work for each such complete turn.

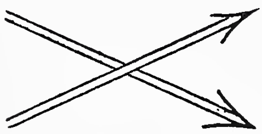


As an illustration, suppose we bend an india-rubber band coloured black on one side, as in the figure, so that the black is always the concave surface, we find on pulling it out straight that it has no twist. If both loops be made by *over*laying, when pulled out it becomes twisted through two whole turns. This is an instance of the kinematic principle that spiral springs act by torsion.



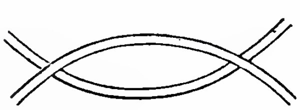
Perhaps the most simple definite condition is that which I first employed, viz., to make the pole move along the curve, keeping always in the osculating plane and on the convex side. But we have then to arrange beforehand what is to be done at a point of inflexion.

A practical rule, however, may easily be given from the consideration of the magnetised surface above mentioned. Go round the curve, marking an arrow head after each crossing to show the direction in which you passed it. Then a junction like the following gives $+ 2\pi$ at each time of crossing; or, if we use the infinite surface spoken of above, it gives $+ 4\pi$ for the upper branch, and nothing for the lower (which, on this supposition, does not pass through the magnetic sheet). Change the crossing from *over* to *under*, and these quantities change sign.

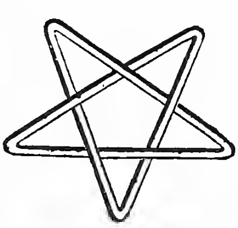


The junction figured above would, in our first illustration, be a silver one. But a still simpler process is to go round putting a dot to the *right* after each crossing *over*, and *vice versâ*. Silver crossings have two dots in one angle; copper one in each of two vertical angles.

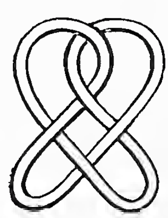
Now, in order that our rule may be such as to give no work where there is no beknottedness, we must make the required expression such as to vanish whenever all the intersections are nugatory. Those which are nugatory only in consequence of their signs are in pairs, silver and copper, and will take care of themselves, as we see by special examples like the following, in which the reversal of one of the directions simply reverses the signs. Hence the only part to correct for is that depending on the number of whole turns, and the sketch of the india-rubber band above shows that the work at the vertex of each such partial closed circuit is simply not to be counted—*i.e.*, that the $\pm 4\pi$, which would be reckoned for each crossing by our rule, is to be considered as made up for by the corresponding screwing of the pole round the curve.



To illustrate the application of this process, let us consider again the two distinct forms with five non-nugatory intersections



1.



2.

(the first being a modified form of the "pentacle," the second, fig. 6

of my paper, which, for the comparison below, must be supposed to have all its signs changed) whose schemes are, respectively—

$$\begin{array}{cccccccccc|l} - & - & - & - & - & - & - & - & - & - & \\ A & D & B & E & C & A & D & B & E & C & A, \\ - & + & - & + & - & + & - & + & - & + & \end{array}$$

$$\begin{array}{cccccccccc|l} - & - & - & - & - & - & - & - & - & - & \\ A & D & B & E & C & A & D & C & E & B & A. \\ - & + & - & + & - & + & - & + & - & + & \end{array}$$

The lower signs refer to over or under, the upper to the electromagnetic work, or to the silver-copper distinction. These two instances, in which both series of signs are absolutely identical, each with each, show at once that we cannot take the two sets of signs alone as fully descriptive of the knot.

To determine the electromagnetic work for any knot, we must divide the scheme into independent circuits, no one of which includes a less extensive one; and omit from the reckoning the work for the terminal of each such circuit, and for each of the intersections which is not included in any one of the separate circuits. The particular closed circuits chosen do not affect the final result, as is easily seen by thinking of the various deformations of each figure.

In the first of the two schemes above there is but one independent non-autotomic circuit, which may be taken as

A D B E C A.

In this all the intersections are included, so that the whole work is to be found by leaving out that for A only; *i.e.*, it is -16π.

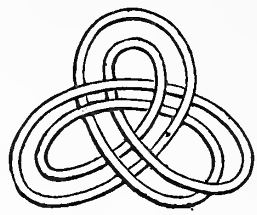
But in the second scheme we may take the two circuits

B A D B and C A D C,

and E is not included in either. Hence we must leave out of count the work for B, C, and E; and thus the whole work is only -8π.

In fact, the figures show that to untie the first knot we must not only have the signs such that we can slip off B and E, but also C and D, *i.e.*, *two* signs must be changed; while the second loses all its beknottedness if A and D could be got rid of, that is if *one* sign only be changed. This is an instance in which the estimate by the electro-magnetic process exactly agrees with the result of simpler considerations. And it is probable after all that the true measure of beknottedness is the smallest number of signs in a scheme which must be altered in order that the wire may cease to be knotted.

It will be found that the alteration of five signs is sufficient to remove the knotting from the annexed figure, and the stages of



operation of the various modes of reduction show that this form can be regarded as made up of simpler knots intersecting one another on the same string. In such a case it is not easy to give a strict definition of the beknottedness in any other way than by defining it as the smallest number of changes of sign which will take off all the knotting. For the separate knots are virtually independent, and to change *all* the signs in any one of them does not in every case necessarily simplify the knot. Uncorrected the work is $-13 \times 4\pi$. Corrected it is $-10 \times 4\pi$, which agrees with the removal of the beknottedness by change of *five* signs only.

If the sign of the one unsymmetrical crossing be altered, four changes of sign will suffice; for the uncorrected work is $-11 \times 4\pi$; corrected it is $-8 \times 4\pi$, corresponding to four changes of sign.

The various modes alluded to in my paper of adjusting the (lower) signs so that there shall be no beknottedness, follow at once from these remarks. For we may make all the free letters in each circuit +, save those which we have taken, *in pairs*, in some previous circuit.

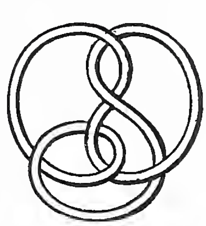
This test, though extremely useful as above explained in classifying knots with the same number of intersections, is not fully descriptive of a knot, being ambiguous whenever there is more than one class of knots with the same number of intersections and with the same excess or defect of silver as regards copper crossings. This consideration, which promises to clear up some obscure and difficult parts of the subject, has led me to some very curious results. The most important of these is when a knot, whatever be its number of intersections, has equal numbers of silver and of copper crossings, or when the uncorrected expression for the electro-magnetic work vanishes. Thinking of this in connection with the fact that a change from right-handed to left-handed in a knot simply changes silver to copper, or *vice versâ*, *i.e.*, reverses the sign of the electro-magnetic work, I was led to see that there is a class of knots which are *capable of being changed from right to left-handed, without change of form*, by the ordinary processes of deformation. Of course this implies that there is a mode of interchanging the letters, two and two, in the scheme, so that their order remains unaltered; or, what comes to the same thing, that we shall get exactly the same scheme (signs not included) by taking either of two different crossings as A, and lettering as usual from it in the same direction round the curve.

It will be readily found by trial that this can be done with the only forms which have four valid intersections—as they are figured in my former paper—if only the wire or cord be so twisted about that, while the *form* is preserved, the junctions B, D be brought into the positions relative to the figure which were formerly occupied by A, C. For the scheme

$$\begin{array}{cccccccccc} + & - & - & + & + & - & - & + & & \\ A & D & B & A & C & B & D & C & | & A \\ - & + & - & + & - & + & - & + & & \end{array}$$

remains the same, so far as the letters are concerned, if we keep the same cyclical order of letters, but write A for B, B for C, &c. In estimating the electromagnetic work, by the rule above, we find we may leave out either A, C, or B, D. So that the work is $\pm 8\pi$, *i.e.*, one degree of beknottedness.

The following case, with six intersections, is very instructive. Either figure is formed from the other by throwing the lower coil over the top of the whole.



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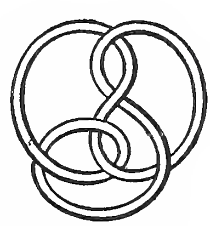


4

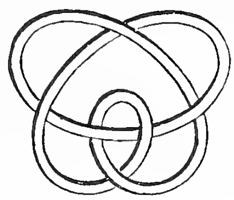
It will be seen that each of these forms may be regarded as a simple loop passed unsymmetrically through a simple knot of three intersections (figures 1 and 3 of my former paper), and that the knot and loop are interchangeable between two groups of three intersections. The knot is right-handed when transferred to the second group; left-handed in the first. But the figures plainly show that they may also be regarded as a right and a left-handed simple knot having a part common to each, so that neither can be pulled tight, subject to our convention that there shall be nothing higher than a double point. And *here* the peculiar difficulty associated with the amphicheiral forms comes in; for, in estimating the electromagnetic work, we find we must leave out one copper and one silver junction—the result being $+8\pi-8\pi$. This is to be treated as $\pm 16\pi$ (or two degrees of beknottedness) because the portions with different signs belong to what are, virtually at least, two separate knots.*

The possibility of such *amphicheiral* forms is obvious from one of the first illustrations in my former paper; where we have only to suppose the irrelevant crossing removed, and one of the separate simple knots (which are both right-handed in the cut) made left-handed. But I was not at first prepared to find this property in any knot not separable into detached, self-contained portions; so that it is possible that some of my former statements may require modification.

It may be well to notice that when, in a slight variation of the

* *Feb.* 19.—This is not correct. There is but *one* degree of beknottedness, for the two knots are not "virtually separate," as they have a part in common, while one is right-handed and the other left-handed. In fact, the figures above are mere transformations of the last cut in my former paper —which is shown to be capable of being opened up by a single change of sign. This can be done in the figures above, at either end of the lower coil where it forms part of the external boundary. But if, without altering the outline of the figure, we change all the signs in either of the two component knots, so as to make them both right-handed, or both left-handed, the whole acquires the double degree of beknottedness wrongly assigned to it in the text. But it has now continuations of sign, and in virtue of these it happens to be reducible. In fact, when we make it into a clear coil after these changes of sign it becomes the pentacle (fig. 1 above), the only knot with fewer than six crossings which possesses, as we have seen, *two* degrees of beknottedness. I stated in my first paper, that when the signs in any non-nugatory arrangement are alternately + and − the cord "is obviously as completely knotted as its scheme will admit of." This completeness must

arrangement just described, the loop passes symmetrically through the simple knot, we have another six-crossing form, very much resembling the last, but which is essentially not amphicheiral. It is figured below in one of its forms—the others may be got by deformation—and the schemes of the two kinds are appended for comparison.



5

Figs. 3 and 4	A *D* B *E* C *A* D *F* E *C* F *B* | A.
Fig. 5	A *D* B *E* C *F* D *A* E *C* F *B* | A.

It will be seen that the sole difference between the amphicheiral knot and that last figured, lies in the inversion of the positions of A and F in their *even* places in the scheme.

It appears, then, that none of these abbreviated methods, however useful as temporary aids to classification, can take the place of the scheme in fully describing the form of a knot and in measuring the amount of beknottedness in general. Especially is the scheme required in order to calculate the beknottedness in terms of the electromagnetic work. And this conclusion might, I think, have been inferred from the prominent part which the arrangement of the letters in the even places plays in determining the form of a knot; an arrangement of which only traces are left when we substitute the sign of the work at each junction for the letter attached to it, thus losing all control of the amount to be added or subtracted on account of the mere number of coils.

[*Added Jan. 27th.*]—Professor Clerk-Maxwell, to whom I sent some of the above results (and to whom, as well as to Sir W.

be understood of what may be called *Knottiness*, not of *Beknottedness*. For it has just been shown by a particular case that we can occasionally increase the degree of beknottedness, while diminishing knottiness, *i.e.*, losing crossings by so altering their signs as to make some of them nugatory. The point thus raised, *i.e.*, the distinction between Knottiness and Beknottedness, is a very troublesome and delicate one, and is obviously related to several of the difficulties pointed out in the present paper.

Thomson, I am indebted for various hints, usually in the especially valuable form of criticisms and reasons for doubt), has lately called my attention to a paper by Listing, of date 1847, part of which is devoted to the subject of knots. I have this morning obtained it from the Cambridge University Library, but have not yet thoroughly read it. As was to be expected, I find that the author has anticipated some of the contents of my papers; and he mentions at least one very curious fact, which I had thought possible, but had not observed, though it is very directly connected with one of the results of the present note. He virtually shows, by giving a particular case, that the method of deformation which I employ does not always give all possible forms of a completely knotted wire. I believe that this depends on the fact that a part of the scheme is amphicheiral. I propose to give the Society an account of Listing's method and results on the earliest opportunity.

3. Note on the Effect of Heat on Infusible Impalpable Powders. By Professor Tait.

Several years ago Professor Dewar gave me a specimen of silica in a state of exceedingly minute division, which had been produced in Dr Playfair's laboratory in the preparation of fluosilicic acid. I noticed at the time how much its great mobility is increased by heating—so that it behaves almost like a liquid. And I fancied that I observed close to the surface a thin stratum of what might by the same analogy be called a vapour; consisting of particles thrown up and falling back again, like the little drops thrown up at the surface of soda-water. I was inclined to ascribe these phenomena to heat directly—supposing that the particles were fine enough to behave, though in a very imperfect way, as the kinetic theory assumes the particles of a gas to behave. However this may be, the extreme mobility of such powders when heated on a platinum dish; and the fact, noticed by chemists, that a bath of calcined magnesia is capable of propagating waves when heated; seem to show that valuable results might be obtained by seeking for evidence of inter-diffusion as the result of experiments made by very long-continued heating of vessels containing fine silica and magnesia originally in separate strata. I have brought this before the Society in the hope that (as it can hardly be classed as a laboratory experiment) some of the Fellows, who may have access to a suitable furnace which is in activity the greater part of the year, may be induced to give the experiment a fair trial.

4. Abstract of Additional Memoir on the Parallel Roads of Lochaber. By D. Milne Home, LL.D.

Mr Milne Home stated that he had been induced to resume his researches in the Lochaber district, in consequence of a lecture, in June last, by Dr Tyndall of London, at the Royal Institution, Albemarle Street, and subsequently printed and published. In this lecture, Dr Tyndall controverted the detrital theory of lake barriers, and advocated the glacier theory;—supporting his views by a reference to observations which Dr Tyndall said he had recently made on a special visit to the Parallel Roads.

Mr Milne Home said that on account of Dr Tyndall's reputation as a man of science, and his great knowledge of glacier action, he had thought it right to reconsider his own opinions, and go back to Lochaber with Dr Tyndall's lecture in his hand.

During this recent visit, Mr Milne Home said he had obtained farther information bearing on the question, which he hoped might be deemed sufficiently important to be communicated to the Society.

He would first mention the import of this information, and then offer remarks on Dr Tyndall's lecture.

After recapitulating the grounds on which he had in his last Memoir suggested that the barriers of the old lakes had been composed of the natural detritus of the district, he mentioned the following facts, in corroboration of these grounds—

1. He specified several additional cases of lakes, in Stratherrick (a district not far from Lochaber), now kept in by detrital blockage, and which, owing to a partial lowering of this blockage, had subsided from higher levels,—these higher levels being indicated by horizonal lines of cliff.

2. He specified the occurrence of thick beds of detritus, consisting of clay, gravel, and sand, along the hills between Stratherrick and Strathspey, at levels of from 2000 to 2500 ft. above the sea,—heights far greater than the sites of the barriers of the old Lochaber lakes.

3. He next indicated on a map the probable sites of the barriers by which the lakes of Glen Roy and Glen Spean had been respectively confined, till the barriers were broken down, and gave reasons for fixing on these sites.

4. While in his last Memoir he had attributed the breaking down of the barriers to the agency of the rivers discharging from the lakes, and also of streams flowing upon the barriers from the adjoining mountains, he now added another probable cause of injury, to the barriers at the mouths of Glen Spean and Glen Glusy in the agency of the sea, there being probable evidence that when these barriers existed, the sea was standing at a level of about 500 or 600 feet higher than at present; and that the blockages of these two glens were so near the sea, when at that height, as to form sea-cliffs.

He explained that this evidence consisted, *first*, in the occurrence of sea shells, said to have been found at two places outside the Glen Spean barrier, in beds of clay from 200 to 500 feet above the sea, by persons whose trusworthiness he had ascertained; and, *second*, in the existence of a series of terraces, in the district about Spean Bridge, from 400 to 500 feet above the sea, which he considered to be marine.

5. Mr Milne Home then adverted to the grounds on which the glacier theory rested, and stated that by another visit made last autumn to Corry N'Eoin and Glen Treig, the only two valleys, where a glacier is supposed to have existed, when Glens Spean and Roy were occupied by lakes, he had discovered that these valleys, instead of being then filled with ice, must have been occupied by water, viz., the water of the Glen Spean Lake; inasmuch as there are beach lines round the whole of L oc Treig, and also at the mouth of Corry N'Eoin.

6. Referring to what had been called the Moraines of the alleged Treig glacier—viz., at Murlaggan, and in the district between the Rough Burn and Fersit,—he said that at Murlaggan

the knolls referred to were composed entirely of beds of sand and fine gravel,—manifestly deposited by water, and not brought by a glacier.

The supposed Moraines, between Rough Burn and Fersit, consist of enormous embankments of gravel, apparently sub-marine, and formed when the sea stood some thousands of feet above its present level. The boulders strewed over this district, and heaped on these embankments, had probably been brought by floating ice, in a current from the westward, flowing up the valley of Spean towards Strathspey.

7. In regard to Dr Tyndall's lecture, Mr Milne Home pointed out, that when impugning the soundness of the detrital barrier theory, Dr Tyndall had given a representation of that theory only from Sir Thomas D. Lauder's Memoirs, written fifty-nine years ago, and not from the more recently published Memoirs, which stated facts and arguments not to be found in Sir Thomas Lauder's paper. Moreover, Dr Tyndall had unfortunately misapprehended Sir Thomas Lauder's views regarding the formation of the barriers, ascribing to him views on this subject disparaging to the detrital theory, which neither Sir Thomas nor any other author entertained.

The only specific objection to the detrital theory suggested by Dr Tyndall, was the alleged absence of any traces of barriers. Mr Milne Home stated that he thought there were traces; and that, even if there were none, the want of them would be no valid objection to the theory.

With regard to the grounds on which Dr Tyndall maintained the glacier theory, Mr Milne Home stated that Dr Tyndall ought to have shown, how there could be such a difference in the climatic condition of the two sides of Spean Valley, that while the glens on the south side were filled with ice, the glens on the north side were filled with water;—this, however, Dr Tyndall had failed to do.

Mr Milne Home farther stated that though it was undoubtedly true that some traces of glacier action were visible in various parts of Lochaber, these, as it seemed to him, belonged to a period in the world's history long antecedent to the lakes of Glen Roy and Glen Spean. When these lakes existed, the water of the Spean lake penetrated into the two glens (Corry N'Eoin and Treig), supposed to have been occupied by glaciers. It was therefore a physical impossibility, that there could have been glaciers in these two glens at the time that there were lakes in Glen Roy and Glen Spean.

Even if there had been glaciers in these glens, these glaciers, to reach the position of the Glen Roy barriers, would, after emerging from their parent glens, have had to travel seven or eight miles across uneven ground, push themselves round the corners of several hills, and rise up to a level several hundred feet above these glens. If it was possible to suppose that the tongues of these glaciers could reach the required spots, the notion of their forming solid and permanent barriers at these spots seems quite untenable.

Monday, 5th February 1877.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read :—

1. On some Effects of Heat on Electrostatic Attraction. By Professor Tait.

2. On the Curves produced by Reflection from a Polished Revolving Wire. By Edward Sang, Esq.

When a polished thin straight wire turns on a fixed point in space, the point at which light coming from a fixed source is reflected, moves in a curved surface. In this paper the motion of the wire was supposed to be restricted to the plane passing through the eye and the source of light. The curve was shown to be of the third order, having a straight line as a symptote both ways, and to depend for its form upon a characterising angle. The interest of the subject lay chiefly in the remarkable transformations of the curve.

3. On an Ammonia-Cupric Zinc Chloride. By E. W. Prevost, Ph.D.

The following is but a short and incomplete account of a compound formed on the carbon and binding screws of a makeshift Leclanché battery. The cells employed were ordinary Bunsen elements, of which the carbon was embedded in manganese dioxide;

the substance in question grew on the brass binding screws, and as a thin layer on the carbon. The external appearance of the mass was reniform, the colour ranging from a pale blue to a dark purple and lilac tint; the portions from the carbon were of a pale green shade, and of no special shape. The quantity being small, it was impossible to make more than one analysis of each substance and that only with very small quantities. The exterior portions which showed no crystalline structure, and were efflorescent, appeared to contain less water than the interior portions, which possessed a paler colour, the composition being otherwise the same. The interior was not homogeneous, as particles of some white substance could be distinguished scattered through the mass. The dark purple and green portions, when heated to 100° C, give off ammonia and water in varying quantities (NH_3 2·02–0·87 per cent.), and left a bluish green powder, which was only slightly soluble in water or in dilute sulphuric acid, but easily dissolved with effervescence by nitric or hydrochloric acid. The original substance treated with water formed a blue alkaline solution, resembling that produced by the solution of cupric hydrate in excess of ammonia, a green residue being left undissolved, which, with potassium hydrate, yielded ammonia.

The analyses point to a formula,

$$Cu_2Zn_4Cl_6(NH_3)_4CO_3\,.$$

If the carbonic anhydride be present as zinc carbonate, and in all probability the above-mentioned white particles are ($ZnCO_3$), we have present the substances,

$$(2NH_3Cu)Cl_2 + (2NH_3Zn)Cl_2 + ZnCO_3 + ZnCl_2\,.$$

Also was found $(NH_4)_2(NH_3)_3Cl_5Cu_2Zn_3CO_3$.

And in another analysis, no carbon dioxide being present,

$$Cu_2Zn_2(NH_3)_3Cl_4\,.$$

In contact with a portion of the substances analysed was metallic copper; this may probably account for the varying quantities of combined copper in its immediate neighbourhood, some of the results obtained indicating the presence of an ammonio-cuprous, as well as of an ammonio-cupric compound. No trace of metallic zinc could be found.

Analyses.

The following are analyses of the substances, but much reliance cannot be placed on them, as they are each from a single analysis of a very small quantity of substance.

I. Purple and lilac portion from interior, changing on exposure, giving off ammonia, insoluble in water, and depositing brown powder.

II. Pale blue, interspersed with white particles.

	I.	II.
Cu	21·67	26·77
Zn	32·62	33·76
Cl	27·08	26·34
CO_3 (from CO_2) . . .	10·10	6·16
NH_4	6·39	6·84
NH_3	2·12	...
	99·98	99·87

4. On a Peculiarity of the Diurnal Hygrometric Curve at Geneva. By Alexander Buchan, Esq.

In a work recently published by Professor Plantamour on the climate of Geneva, the hourly means of the aqueous vapour of the atmosphere for each month is given, deduced from observations made during the twenty-seven years ending with 1875. The curves drawn from these figures are undoubtedly the most remarkable of the meteorological specialties of the climate of Geneva.

Professor Plantamour endeavours to explain the facts of the diurnal hygrometric curves at Geneva by the ascending and descending currents of air consequent on the diurnal march of the temperature. These systems of air currents serve to explain a part of the phenomena in question,—but only a part, and we think a very small part. A reference to the curves at other places is sufficient to show that this explanation is insufficient. There is another cause to which the characteristic feature of the curves is due, and that cause is the diurnal changes of the wind, which occur at Geneva, arising from its position with reference to Lake Geneva, and from the qualities of these winds as determined by the relative size of the lake.

The hourly variations in the direction of the wind show that the land and lake breezes are strongly marked—the breeze from the lake prevailing during those hours of the day when the temperature of the land is in excess of that of the lake, the land breeze during the rest of the day. In December, when the land is at no hour of the day warmer than the lake, no breeze from the lake sets in over Geneva, and also in January the breeze from the lake is but slight and of short continuance. These breezes leave their impress in a most distinct manner on the curves of the hourly variation of the vapour tension. During the winter months, when no breeze from the lake, or but a feeble one, sets in, the vapour curves show only one daily minimum, occurring about sunrise, and one maximum, about 2 P.M., whereas during the other months, from March to October, the vapour curves exhibit two daily minima, one about or shortly before sunrise, and the other from 2 to 4 P.M., and two maxima, one from 8 to 11 A.M., and the other from 6 to 10 P.M., according to season. Equally marked are the curves of the hourly variations of cloud, the maximum during the winter months occurring about sunrise, and the minimum about sunset. On the other hand, during the warm months there are two maxima and minima —the first maximum occurring about or shortly after sunrise, and the second, but by far the larger maximum, about 6 P.M., and the two minima shortly after midnight, and from 9 to 11 A.M.

The explanation of these instructive phenomena is doubtless to be found in the size of Lake Geneva, which is large enough to occasion a strong breeze during the day from the lake all round its shores. On the setting in of the breeze, the air conveyed by it, having been resting sometime previously on the surface of the lake, is necessarily moist, and while this state of matters continues the first daily maximum of vapour tension is reached. As the breeze continues, the current is necessarily drawn from higher strata of the atmosphere, and being thus but a brief interval in contact with the lake the air becomes constantly drier till the second daily minimum occurs, from 2 to 4 P.M. The breeze then gradually diminishes in force, and the air consequently becomes moister, till the maximum vapour tension of the day occurs about the time when the breeze from the lake falls to a calm, and before the land breeze springs up.

A cursory examination of the curves suffices to show that there is a close connection between their critical phases and the corresponding phases of atmospheric pressure and temperature; and the idea is suggested that in this great contribution of Plantamour's to the meteorology of Geneva, we are put in possession of data of the utmost importance as regards the relations of the vapour of the atmosphere and its movements to changes of atmospheric pressure in a way such as could be done by the observations of few observatories.

5. On Knots. By Professor Tait.

(*Abstract.*)

At the last meeting of the Society I stated that I had just procured a remarkable essay by Listing, part of which bears on the subject of knots, and that I had found in it an example of a change of form not producible by the modes of deformation I had employed.

It had for some time struck me as very singular that, thoug I could easily prove that (when nugatory intersections are removed) a knot in which the crossings are alternately over and under is not farther reducible, I could not prove all its possible deformations to be producible by inversions or projections of the kinds specified in my paper; but, as soon as I recognised the existence of amphicheiral forms, I saw that it was probable that they would furnish a key to my difficulty. I immediately set to work to classify the simpler of such forms; and while I was thus engaged I got the *Göttinger Studien* for 1847, in which is Listing's paper, with the title *Vorstudien zur Topologie*.

By this title Listing means *qualitative* as distinguished from *quantitative* space-relations. He commences with a study of inversion (*Umkehrung*) and perversion (*Verkehrung*),—the first being the effect of a rotation through two right angles about any axis, the second the result of reflexion in a plane mirror.

He next treats of screwing of all kinds, including twisting and plaiting.

He then applies the notion of lines winding or screwing round one another to the projection of a knot on a surface, and shows that we can thus obtain a knowledge of the relative situation of the various coils. At each crossing portions of the two branches may be regarded as small parts of lines twisted round one another. Of the four angles so formed, two vertical or opposite ones are bounded towards the *right*, the other two towards the *left*, by that line which passes *over* the other. We thus distinguish these pairs of vertical angles into right- and left-handed. [Listing uses right-handed for what we should call left-handed in screwing, but the difference is of no importance, so far as his results are concerned.]

Next he shows that perversion, but not inversion, changes right-handed into left-handed angles.

He then gives the complete knot with three intersections, and shows that when it is in a *reduced* (as distinguished by him from a *reducible*) form, all the angles in each separate mesh have a common character; but that, when it is reducible, some of the meshes have angles of both kinds. He distinguishes between the right- and left-handed forms of the reduced knot, and shows that they are not convertible into one another; also that (including external space, or the *Amplexum*) there are three meshes with two corners each, and two with three corners; one class being right-handed, the other left. And he states that the Amplex may be made to change its character from right to left by being changed from a three-cornered to a two-cornered mesh, or *vice versa*.

He points out that a loop (*i.e.*, a mesh with only one corner) does not appear in the reduced form, and then writes, as the type-symbol of the reduced right-handed knot of three intersections, the expression

$$\left.\begin{matrix} 3r^2 \\ 2l^3 \end{matrix}\right\},$$

denoting three right-handed meshes with two corners each (*Oesen*), and two left-handed meshes with three corners each (*Maschen*). [The perverted, or left-handed, form is of course represented by the same symbols, with the interchange of r and l.]

The sum of the numerical coefficients in the symbol is the number of meshes (the Amplex included), and is greater by two than the number of crossings. The sum of the products of the coefficients into the corresponding exponents gives, in each of the two parts of the symbol, double the number of crossings. These symbols contain the topologic character of any particular knotting.

Listing next points out that the reduced three-crossing knot may be obtained by three half-turns about one another of two originally parallel cords whose ends are afterwards joined into one ring, and that the character depends upon the direction of the torsion.

He proceeds to give a symmetrical knot, with seven crossings, in two different reducible, and one reduced, forms. The reduction of the first gives the three crossing knot, that of the second the four forms of the essentially non-clear coil of five intersections. (Figs. 6-9 of my first paper.)

Their common symbol he writes as

$$\left.\begin{matrix} 2r^4 + r^2 \\ 2l^3 + 2l^2 \end{matrix}\right\},$$

and he points out that, in this case, the Amplex belongs to each in succession of these four kinds of meshes.

He then states that the symbol

$$\left.\begin{matrix} 2r^4 + 3r^2 \\ 2l^5 + 2l^2 \end{matrix}\right\}$$

gives five different reduced forms, each with seven crossings; while the symbol

$$\left.\begin{matrix} r^5 + 3r^3 \\ l^4 + 2l^3 + 2l^2 \end{matrix}\right\}$$

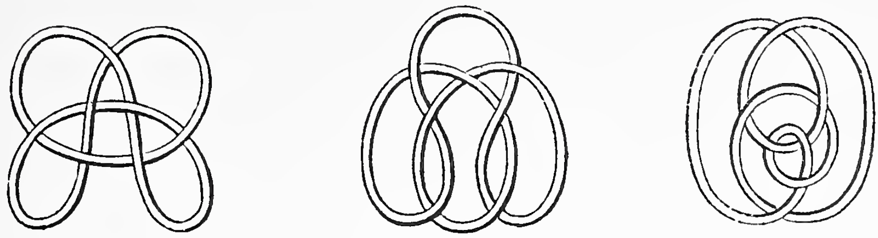
has six distinct forms.

But he adds the following extremely important remark:—"In certain cases one symbol is equivalent to another, so that the reduced forms of the one can be transformed into those of the other." He states that this is the case with the last written symbol and the following:—

$$\left.\begin{matrix} 2r^4 + 2r^3 \\ l^4 + 2l^3 + 2l^2 \end{matrix}\right\}$$

which has five reduced forms.

Thus there are, in all, *eleven* reduced forms of these kinds, all equivalent to one another, and all having seven crossings. The following are his figures: the first and second belonging to the former of the two equivalent symbols, the third to the latter—



He concludes this part of his Essay by saying that "these examples (confined as they are to single, closed lines), and the remarks made upon them, serve to show that the fundamental conception of twisting of lines is capable of being applied to the most complex space relations."

It may be added that these very elegant and important results are given as statements merely, without any hint of how they were arrived at, or how they may be extended. In fact brevity has been sedulously studied, for all that is given about knots forms a comparatively small part of the whole of Listing's extremely valuable, but too brief, Essay.

The rest treats, rather more fully, the whole subjects of inversion and perversion, screws of various kinds, plaiting and twisting, (with their applications to vegetable spirals, &c.), the numbers of lines joining given sets of points, the extensions of the meaning of the word *Area*, &c., &c.

The above abstract, which contains almost all of Listing's remarks on knots, shows that he has long ago anticipated a very great deal of what I have lately sent to the Society. For myself, however, I may say that I have had to learn only two things (about *knots*) from Listing, viz. (1), a special case (which will be examined immediately), in which two forms are equivalent, though not transformable into one another by the methods given in my first paper; and (2), to value more highly than I had hitherto done the method of classifying forms by the numbers of each kind of mesh, and the right-handed or left-handed character of each.

My first paper, as sent to the Society, was essentially confined (as indeed its title indicates) to the results deducible from a special elementary theorem,—one of two which occurred to me long ago when designing Vortex-Atoms of various forms, and which I gave to Section A at the late meeting of the British Association. The second of these theorems (as will be seen by reference to my British Association paper, *Messenger of Mathematics*, Jan. 1877), was virtually the same as Listing's division of the meshes of a reduced knot into right- and left-handed,—only I called them black and white, but as I did not see how to connect this theorem directly with the measure of beknottedness, I did not formally introduce it into my papers read to the Society. It is, of course, virtually included in the statements regarding coins thrown into the corners of cells,—for, taking the case of the silver and copper coins, the pair of left-handed vertical angles are those in which, or in one of them, there is silver—the right-handed, copper.

Nothing can be clearer than Listing's statements on several parts of the subject: it is greatly to be desired that he had made many more. Still, with a cordial recognition of the great value of all that is to be found in Listing's paper, I adhere to what I said in my last communication, to the effect that the full character of a knot cannot be learned except from its "scheme," or something equivalent to it. That the type-symbol (when such a representation is possible) is ultimately equivalent to the scheme may possibly be true,* especially when we consider that it virtually contains *two independent* descriptions of a knot (*i.e.* in terms of its right-handed and its left-handed meshes separately); that for purposes of classification it is superior is, I think, obvious, but I think it equally obvious that for the purpose of drawing the knot it is inferior. And the scheme for a reducible knot is quite as simple as that for a reduced one, while it is not easy to see what would be called the symbol of a reducible knot. Nor can I represent by

* (*Added Feb.* 7.)—I have just found symbols for which this is not the case. The following single instance is sufficient, for the present, to show that the type-symbol is not always equivalent to the scheme. The symbol

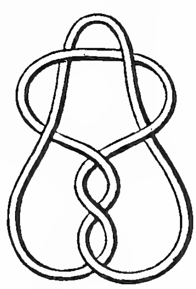
$$\left.\begin{matrix} r^4+2r^3+2r^2 \\ l^5+l^4+l^3+l^2 \end{matrix}\right\}$$

may represent either a continuous curve with 7 intersections, or a complex system consisting of a circle intersected at six points by a skewed figure of 8. I shall discuss the subject fully in a paper "On Links," which I have in preparation.

Listing's notation the double trefoil knot which has appeared in each of my papers; for, although irreducible (at least so far as I am aware), it contains several meshes which have angles of essentially different characters. Listing's avowed object was to simplify notation as far as possible. My impression is that, in one respect at least, he has carried simplification a little too far; for it cost me some little time and trouble to draw, from his type-symbol, the one knot which he speaks of, but leaves undrawn, viz., as above,—

$$\left.\begin{matrix} 2r^4 + 3r^2 \\ 2l^5 + 2l^2 \end{matrix}\right\} .$$

Here is one of its forms: transformation will give the four others.



In fact the type-symbol, even in this specially simple and symmetrical case, where it is much condensed, contains just as many separate typographical characters as the scheme; and I think there can be no doubt whatever that it is almost incomparably more easy to draw the figure from the scheme than from the symbol. Given the scheme, the symbol can be formed from it in a moment; while the finding of the scheme from the symbol is very troublesome. But in such a matter experience is the only guide, and I have had almost no practice in trying to draw from the symbol. Listing's type-symbol leads directly, however, to an inquiry not even suggested by the scheme; (for the latter, as I have given it, is essentially confined to a single closed curve),—viz., the forms of more than one closed curve, intersecting one another or not, which jointly divide an unlimited plane into given numbers of meshes with given numbers of sides.

The first idea of this was suggested to me when I endeavoured to draw the curve with six intersections, whose type symbol is

$$\left.\begin{array}{l} 3r^4 \\ 2l^3+3l^2 \end{array}\right\}.$$

This symbol obviously satisfies the three numerical conditions; but, on trying to draw the corresponding figure, I found that it always came out as a species of endless chain of three separate links. One of its forms, from which the others can be found by transformation, is three circles, every two of which intersect.

Two figures of 8, linked together at each end, give the symbol

$$\left.\begin{array}{l} 4r^3 \\ 2l^4+2l^2 \end{array}\right\}.$$

And by shifting the twist from one to the other, as explained in the latter part of this paper, the symbol may be changed to

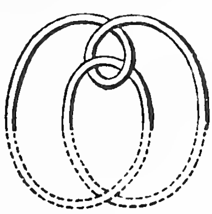
$$\left.\begin{array}{l} r^4+2r^3+r^2 \\ 2l^4+2l^2 \end{array}\right\}.$$

I have not as yet studied the theory of type-symbols, as it differs so much from my own method; but it is obviously desirable to find the criterion by which to distinguish from one another the type-symbols necessarily denoting one closed curve, and those necessarily denoting two or more intersecting curves. It is probable that there are symbols which may represent either kind of figure. The inquiry will no doubt be found very simple, if only approached from the proper side.

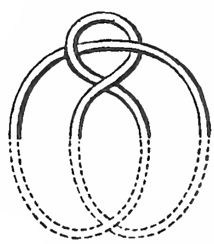
I now pass to the sole point of Listing's paper which (so far as knots are concerned) was thoroughly new to me, though not unexpected; and I shall lead up gradually to the special case which he gives, using for the purpose the properties of the amphicheiral knots mentioned in my last paper.

To apply the amphicheiral property to the production of new forms, we may begin by studying under what conditions the internal arrangements of a knot can be altered while *four* points of its contour, and *two* of the parts of the cord or wire joining pairs of these, are fixed. [The reason for the number *four* is, that when two only are employed to mark off separate parts of a knot, it is either (*a*) virtually unconstrained, or (*b*) it is divisible (and is actually divided) into two separate knots.] If, under these circumstances, changes can be made on one side of the fixtures, they will be practicable also *in whatever way these points be connected by the rest of the string*, provided always that it be not led through the amphicheiral part. Hence, if there be an amphicheiral part of any knot, it may often be transformed *in situ*; the rest of the knot being unaltered, but the amphicheiral part being made to present, as it were, another hand to the rest.

To begin with a very simple case, let us take the simplest amphicheiral form—the complete knot with four crossings—as below.



1.



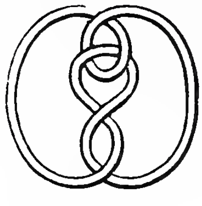
2.

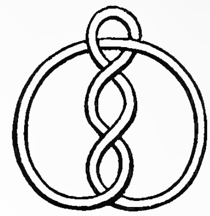
When the first of these is inverted, O being within either of the *interior* three-sided meshes, the second is produced: if O be in one of the *boundary* three-side meshes the perversion of the second is produced. But we may easily convert 1 into 2 by fixing the lower crossing (*i.e.*, by fixing a point near it in each of the four lines diverging from it, so that the dotted part remains fixed), and making the upper loop rotate so as to banish the upper crossing. Thus the upper parts of these two figures are equivalent.

And we can now suppose the (dotted) portions between the fixed points to be cut open and reknotted in any way we choose,—subject, of course, always to the rule of alternate over and under, else the knot would in general be reducible. Of course, unless we wish to study *linking*, the ends must be joined so as to preserve continuity throughout the string.

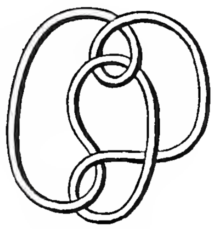
The simplest modes of joining (without additional intersections) give us at once two different aspects of the same "trefoil" knot: —with one crossing additional we have the original figures

(1 and 2) above: with two additional we have the following,—figured in my first paper.

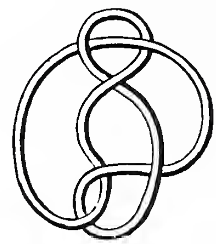




With three we have two apparently different forms,

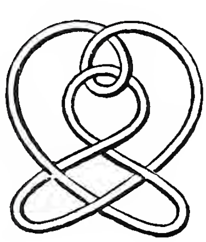


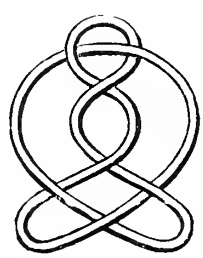
and



These are, however, only transformations of the six-crossing amphicheiral form (figs. 3 and 4 of my last paper), and are directly transformable into one another. They are, of course, unchanged if the lower part be reversed, for the upper parts are symmetrical.

But when we add four new crossings, as below, instead of the single crossing removed, we get the two equivalent figures





which are not transformable into one another by the processes of my first paper. In fact the *schemes* will be found to be incompatible, begin each where we choose. In Listing's notation their type-symbols would stand respectively, thus—

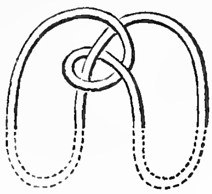
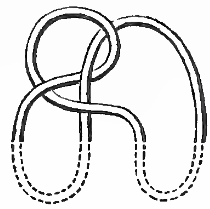
$$\left.\begin{matrix} 2r^4+2r^3 \\ 2l^4+3l^2 \end{matrix}\right\} \quad \text{and} \quad \left\{\begin{matrix} r^5+r^4+r^3+r^2 \\ 2l^4+3l^2 \end{matrix}\right.$$

The schemes are

A*E*B*F*C*B*D*A*E*G*F*C*G*D* | A and A*D*B*F*C*A*D*G*E*B*F*E*G*C* | A.

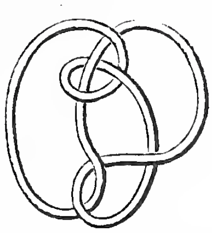
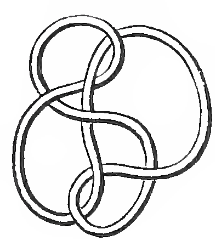
But even this simplest amphicheiral form has other applications.

Thus, it will easily be seen that the figures below are mere *distortions* of fig. 2 above; and that, the dotted parts being fixed, they can be changed on an actual cord into one another, and even reversed (as from left to right), thus giving four distinguishable forms, of which I figure only two,—

Each of these may be changed without undoing the fixtures to its reverse (from left to right) by the first simple process just described, the loop, in fact, being transferred from one branch of the (undotted) cord to the other.

Of course the number of ways in which the dotted part can be varied is infinite. I therefore give here only that which reproduces the forms already quoted from Listing as equivalent.

A glance shows that their type-symbols are

$$\left.\begin{matrix} r^5 + 3r^3 \\ l^4 + 2l^3 + 2l^2 \end{matrix}\right\} \quad \text{and} \quad \left\{\begin{matrix} 2r^4 + 2r^3 \\ l^4 + 2l^3 + 2l^2 \end{matrix}\right.$$

which are those already quoted from Listing.

In fact, looking at Listing's figures above, we see that in each there is a part of the curve, containing four crossings, exactly the same as one or other of the two (partly dotted) distortions of the 4-crossing knot above.

The paper contains many other instances of these applications of amphicheiral forms.

In conclusion, it appears that the problem of finding all the absolutely distinct forms of knots, with a given number of intersections, is a much more difficult one than I at first thought; and it is so because the number of really distinct species of each order is very much *less* than I was prepared to find it. The question now belongs more to quantitative than to qualitative relations. It resembles, in fact, the species of problem originally suggested by Crum Brown, and resolved by Sylvester and Cayley, of determining the number of conceivable Hydrocarbons under given conditions of limitation. And here I am glad to leave it, for at this stage it is entirely out of my usual sphere of work, and it has already occupied too much of my time.

[*April* 11*th.*—Prof. Listing, to whom I sent in "proof" the above abstract of part of his paper, has kindly written some remarks upon it, from which I extract the following very interesting passages, which increase our regret that such a master has published so little on a subject which he has evidently made his own:—

"In dem . . . proof . . . sollte die Aufmerksamkeit vorzugsweise auf den allerdings sehr kurzen Theil gelenkt werden, welcher sich auf die Knoten oder Curvenverschlingungen bezieht. Anderenfalls würde ich gewünscht haben, dass das—obwohl sehr elementäre—Capitel über die 'Position' etwas näher besprochen worden wäre, zumal darin das Motiv zu finden, warum ich unsere gewöhnliche Schraube laeotrop nenne statt dexiotrop, wie Sie p. 307 rügen. Ich habe längst die Benennungen rechts und links bei Schraubenwindungen, sowie die lateinischen oder griechischen Ausdrücke, welche stets zu den widerwärtigsten Verwechselungen Anlass geben, durch die Namen *Delta-* und *Lambda-*Windung ersetzt, welche, wie p. 42 meiner Schrift erwähnt ist, mnemonish und intuitiv die Vorstellungen fixiren. Der Botaniker—und einige haben in der That diesen Modus befolgt—würde also dem Humulus lupulus Deltawindung, dem Convolvulus Lambdawindung zuschreiben, so wie unsere künstliche Schrauben der Technik meistens Lambda-Schrauben und nur in seltneren Fällen Delta-Schrauben sind. Die Unterscheidung zwischen Inversion und Perversion im Anschluss an sogenannte positive und negative Positionen ist nur ein gelegentliches specielles Ergebniss."

"Die Verschlingungen cyclischer Curven im Raume d. h. die Knoten betreffend, so sollten, wie in den 'Vorstudien' es nicht anders erwartet werden durfte, die Geometer auf die Bedeutsamkeit dieses überaus schwierigen Theils der Topologie durch Aufzeigung einiger der einfachsten Anfänge aufmerksam gemacht werden, z. B. durch Andeutungen über mögliche Bezeichnungsmethoden, Symbole und dgl. durch welche wir, wie es die Wissenschaft in allen analogen Fällen zu erstreben hat, von der Intuition zu den Begriffen fortzuschreiten vermögen. Die von mir empfohlenen Symbole sollen nichts weiter sein als ein derartiger Fingerzeig, und wenn ich auch ganz mit Ihnen darin übereinstimme dass das Schema einen Nodalcomplex leichter construirbar macht als das Symbol, so bleibt doch das Schema viel weiter von dem Begriffe entfernt als das Symbol, auch abgesehen von den auf beiden Seiten noch übrig bleibenden Vieldeutigkeiten. Wie fragmentär das, was ich vor 30 Jahren darüber angedeutet habe, gewesen, kann ich Ihnen unter vielen an dem einen Punkte erläutern, dass ich mich dort nur auf die Verknotung einer einzigen cyklischen oder einer cyklodischen Curve beschränkte, und nur auf solche Verschlingungen unter der Benennung 'reducirt' einging, deren sämmtliche Parcellen (incl. Amplexum) monotyp sind, wiewohl Ihnen selbst bekannt ist, dass es Knoten giebt mit Maschen, deren Ecken promiscue δ und λ heissen, ohne auf einfachere Formen, d. h. eine geringere Zahl von crossings reducirbar zu sein. . . . Es versteht sich von selbst, das die in den Vorstudien angeführten Symbole nur auf monotype reducirte Complexe anwendbar sind, und durch Verallgemeinerung andere Gestalten annehmen, worauf ich indessen hier weiter einzugehen unterlasse."

I have thought it better to give Listing's own comments on my remarks than to seek permission from the Council to alter these remarks in the text.]

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH

VOL. IX. 1876–77. No. 98.

Ninety-Fourth Session.

Monday, 19th February 1877.

DAVID STEVENSON, Esq., Vice-President, in the Chair.

The Chairman reported that the Council had awarded the Makdougall-Brisbane Prize for the biennial period 1874–76, to Alexander Buchan, M.A., for his paper "On the Diurnal Oscillation of the Barometer," which forms one of an important series of Contributions by Mr Buchan to the Advancement of Meteorological Science.

The following Communications were read:—

1. On the Action of Sulphuretted Hydrogen on the Hydrate and on the Carbonate of Trimethyl-sulphine. By Professor Crum Brown.

(*Abstract.*)

The investigation, of which this paper contains the first part, was undertaken with the view of throwing light on the constitution of salts of trimethl-sulphine.

These salts have been represented in two different ways—1st, as compound of tetratomic sulphur, and 2d, as molecular combinations of sulphide of methyl with methylic ethers, just as the ammonia salts have been represented as compounds of pentad nitrogen, or as molecular combinations of ammonia with hydric salts.

It appeared to the author that facts having an important bearing on the question, which of these is the better representation of the constitution of such bodies, might be obtained by the study of the sulphur compounds of trimethyl-sulphine, of the corresponding selenium compounds, and of the intermediate substances.

The following list, in which each substance is formulated—*a*, on the assumption of tetrad sulphur and selenium, and *b*, on the supposition of molecular union illustrates this idea.

	a.		*b.*
(1.)	$(CH_3)_3S—SH$	...	$(CH_3)_2S$, CH_3HS.
(2.)	$((CH_3)_3S)_2S$	...	$(CH_3)_2S$, $(CH_3)_2S$, $(CH_3)_2S$.
(3.)	$(CH_3)_3Se—SH$	...	$(CH_3)_2Se$, CH_3HS.
(4.)	$((CH_3)_3Se)_2S$	...	$(CH_3)_2Se$, $(CH_3)_2Se$, $(CH_3)_2S$.
(5.)	$\begin{matrix}(CH_3)_3S \\ (CH_3)_3Se\end{matrix} S$	...	$(CH_3)_2S$, $(CH_3)_2Se$, $(CH_3)_2S$.
(6.)	$(CH_3)_3S—SeH$	...	$(CH_3)_2S$, CH_3HSe.
(7.)	$((CH_3)_3S)_2Se$	...	$(CH_3)_2S$, $(CH_3)_2S$, $(CH_3)_2Se$.
(8.)	$(CH_3)_3Se—SeH$	...	$(CH_3)_2Se$, CH_3HSe.
(9.)	$((CH_3)_3Se)_2Se$	...	$(CH_3)_2Se$, $(CH_3)_2Se$, $(CH_3)_2Se$.
(10)	$\begin{matrix}(CH_3)_3S \\ (CH_3)_3Se\end{matrix} Se$	...	$(CH_3)_2S$, $(CH_3)_2Se$, $(CH_3)_2Se$.

Of these it is obvious that (3) and (6), (5) and (7), and (4) and (10) form three pairs of isomers.

Upon any theory (3) and (6) may be expected to be different, but it is not so with the other two pairs. They ought to be different, if the assumption of tetrad sulphur and selenium is correct, but on the theory of molecular combination it is difficult to see how a difference of properties could be accounted for.

The hydrate and also the carbonate of trimethyl-sulphine are readily acted upon in the dry condition by sulphuretted hydrogen, and the product is colourless if air has been rigidly excluded. The characteristic reaction with nitroprusside shows the product to be a sulphide or sulphhydrate, but the action of oxygen upon it is so rapid that it has not yet been obtained in a condition fit for analysis. Before attempting to prepare it in a state of purity, it was thought best to examine the products of its oxidation. These are—(1), an orange polysulphide, which in the presence of moisture and oxygen is further oxidised, with separation of yellow sulphur, yielding (2) hyposulphite of trimethyl-sulphine.

The action is thus similar to that of sulphuretted hydrogen on potash or carbonate of potash, but takes place with much greater rapidity.

The examination of the sulphide and polysulphide of trimethyl-sulphine will form the subject of the next part of the paper.

2. On Links. By Professor Tait.

(*Abstract.*)

Though in my former papers on knots I have made but little allusion to cases in which two or more closed curves are linked together, the method I have employed is easily and directly applicable to them. I stated to the British Association that the number of intersections passed through in going continuously along a curve, from any intersection to the same again, is always even—whether it be linked with other curves or not Hence, even when a number of closed curves are linked together, the intersections may be so arranged as to be alternately over and under along each of the curves.

When this is done, each of the meshes has all its angles right or left handed; so that Listing's type-symbols may be employed, just as for a single knotted curve. The scheme, however, consists of as many parts as there are intersecting curves—each part containing, along with each of its own crossings *twice*, each of its intersections with other curves *once*.

Thus AB | A AB | A

or

$$\left.\begin{matrix} 2r^2 \\ 2l^2 \end{matrix}\right\}$$

represents a couple of ovals linked together.

When three ovals are joined, so as to form an endless chain, we have

ABCD | A DCEF | D FEBA | F

or

$$\left.\begin{matrix} 2r^3 + 3r^2 \\ 3l_4 \end{matrix}\right\}$$

Of course such figures can be transformed or deformed according to the methods given in my first paper—the scheme and the type-symbol alike remaining unaltered. And alterations of both scheme and symbol are, in various classes of cases, producible by the processes of my last paper without any change of links or linking.

The genesis of the scheme of a link may be most easily studied by forming a knot into a link. This is done by cutting both turns of the wire at any junction, and joining them again so as to make two closed curves instead of one. No intersections are lost by this process, except that which was cut across, provided, of course, that the original knot had no nugatory intersections, and that none are rendered nugatory by the operation of cutting the whole across.

Any crossing with four adjacent crossings when the turns of the coil pass alternately over and under one another will appear in a scheme as follows :—

$$\begin{array}{ccccc} \ldots\ldots & A\,X\,B & \ldots\ldots & C\,X\,D & \ldots\ldots \\ & -+- & & +-+ & \end{array}$$

implying that from X through B and C back to X forms one continuous circuit; similarly from X through D and A back to X.

There are but two ways in which continuity can be kept up if we cut the cord twice at X, and reunite the ends in a different arrangement from the original one.

It is obvious that if we pass from C to B, by way of X (abolished), and similarly with the rest, we divide the continuous closed curve into two separate (but generally inter-linked) closed curves. If we pass from A, by way of X (abolished) to C, we pass thence in time to B, and finally by way of D to A. Thus the curve remains continuous, but with one intersection less than at first. And, in either case, the alternate order of the signs of the crossings will be maintained throughout.

In the former of these modes we take the part containing C and B (and we may, if we please, also take the rest) in the same order as before the change. The scheme is therefore, without any other change, simply divided into two parts, which are separated from one another by the (abolished) junction X in its two positions.

In the second mode, it is obvious that the letters in one of the two parts separated from one another by the mark X in its two places are simply to be inverted in order without change.

The process presents no difficulties, so that I shall give only two simple examples. Thus the scheme of the pentacle, viz. :—

A D B E C A D B E C | A

is divided at A (in this case it does not matter which junction we take) into the two superposed non-autotomic ovals

D B E C | D, D B E C | D,

by the first mode :—, and is simplified into

D B E C C E B D | D

(*i.e.*, a wholly nugatory scheme) by the second.

The type-symbols in the original state, and in the two altered states, are, respectively,

$$\left.\begin{matrix} 2r^5 \\ 5l^2 \end{matrix}\right\}$$

$$\left.\begin{matrix} 2r^4 \\ 4l^2 \end{matrix}\right\}$$

$$\left.\begin{matrix} r^4 \\ 3l^2 + 2l \end{matrix}\right\}$$

The last of these is virtually nothing. In fact, terms in r or l to the first power are rejected by Listing. And, when these loops are taken off by untwisting or by opening up, the scheme becomes

$$\left.\begin{matrix} r^4 \\ l^2 + 2l \end{matrix}\right\}$$

and a second application of the process removes the whole.

Operating in a similar way upon the only other figure with five non-nugatory intersections—viz. :—

$$A_4 D_4 B_2 E_2 C_2 A_4 D_4 C_2 E_2 B_2 \mid A$$

or

$$\left.\begin{matrix} 2r^4 + r^2 \\ 2l^3 + l^2 \end{matrix}\right\}$$

we find three classes of cases, according to the particular intersection operated on.

[I may here introduce, though it involves a slight digression, a method which I have found very convenient as an assistance in finding which intersections have similar properties as regards the figures which will be obtained when they are made in turn the point of section. In the scheme above written the suffixes express the numbers of letters which intervene, in the scheme, between the two appearances of the same letter. If n be the whole number of letters, the suffix may of course be either $2r$ or $2n-2r-2$. It is convenient to write always that one of these two numbers which is not greater than the other. When a particular suffix occurs only once, the corresponding crossing has evidently different properties from the others; if twice, we find in general that the corresponding crossings have similar properties. If three times, two of them have usually like properties, but the third not—and so on. This method is useful, but it is in certain cases misleading. In fact, we must look not only at the suffix itself, but at the place which it occupies relatively to the whole group of suffixes, in order to obtain absolutely definite information. Something similar to this is obviously hinted at in Listing's paper, where he seems to determine the number of possible transformations of the figure representing a symbol, by treating the numerical coefficients much as I have here treated the suffixes. But this is mere conjecture on my part.]

By this method then, or by examining the diagram, we see that A and D are similar, so are B and C, while E may possibly possess distinct properties of its own. We need, therefore, take only three cases, A, B, and E.

a.) Divide at A. Then we have either

$$\text{D B E C} \mid \text{D} \quad \text{D C E B} \mid \text{D}$$

$$\left.\begin{matrix} 2r^4 \\ 4l^2 \end{matrix}\right\}$$

two ovals crossing one another, one taken right handed, the other left; or

$$\text{D B E C B E C D} \mid \text{D} = \text{B E C B E C} \mid \text{B}$$

$$\left.\begin{matrix} 2r^3 \\ 3l^2 \end{matrix}\right\}$$

the trefoil knot; for D becomes nugatory.

b.) Divide at B. We have either

$$\text{A D} \mid \text{A} \qquad \text{E C A D C E} \mid \text{E} = \text{D A} \mid \text{D}$$

$$\left.\begin{matrix} 2r^2 \\ 2l^2 \end{matrix}\right\}$$

two linked ovals, C and E having become nugatory; or

E C A D C E D A | E

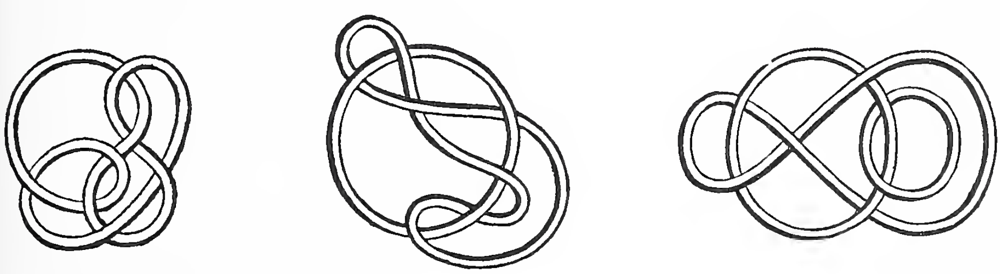
$$\left.\begin{matrix} 2r^3 + r^2 \\ 2l^3 + l^2 \end{matrix}\right\}$$

an amphicheiral knot, the only knot with 4 intersections.

c.) Dividing at E we find the same results as for B and C.

From the rules just given for removing an intersection, it is of course easy to pass to those required for the introduction of a new intersection.

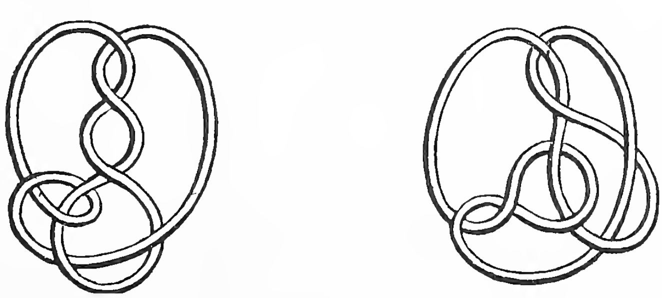
In endeavouring to frame a general method of determining whether a particular type-symbol necessarily denotes one continuous curve, or a superposition of two or more curves, I was completely unsuccessful. But, as indicated in a note to my last paper, I found the reason to be that *no such distinction necessarily exists.* And by the application of the methods of adding or removing intersections given, I found a number of instances in which the same type-symbol may represent many entirely different kinds of figures. Thus the following



are all represented alike by the symbol

$$\left.\begin{matrix} r^5 + r^4 + r^3 + r^2 \\ l^4 + 2l^3 + 2l^2 \end{matrix}\right\}$$

But I have since succeeded in obtaining cases in which the same type-symbol represents two perfectly distinct single closed curves. One instructive example is the following



The common type-symbol is

$$\left.\begin{array}{l} r^5 + r^4 + r^3 + 2r^2 \\ l^5 + l^4 + l^3 + 2l^2 \end{array}\right\}$$

But the schemes are

$$A_6E_6B_4G_6C_6H_6D_6B_4E_6A_6F_2C_6G_6F_2H_6D_6 \mid A$$

and

$$A_6D_4B_2H_4C_6F_4D_4A_6E_4G_2F_4C_6G_2E_4A_4B_2 \mid A$$

Now no change in lettering can affect the suffixes, so that the two schemes are essentially different. In fact the sum of the suffixes is 84 in the first scheme, but only 64 in the second. The first has only one degree of beknottedness, the second has two. The first is not amphicheiral, the second is.

There is no connection between the type-symbol, as Listing gives it, and the singleness or complexity of the curve represented, but it is possible to make analogous symbols capable of expressing everything of this kind. Only we must now adopt something very much resembling Crum Brown's Graphical Formulæ for chemical composition. Some very remarkable relations follow from this process, but I can only allude to a few of the simpler of them in this abstract.

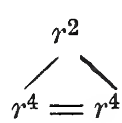
The only necessary relations among the numbers forming the right or left part of a symbol are satisfied if no one is greater than the sum of the others, and if the sum of all is even. With any set of numbers subject to these conditions, we can form the right or left-hand side of a symbol—and from that we can form the other when we know the grouping.

An example or two will make this clear. Take, for instance, the symbol

$$\left.\begin{array}{l} 2r^4 + r^2 \\ 2l^3 + l^2 \end{array}\right\}$$

which represents the five-crossing knot of p. 242 above.

A glance at the figure shows that the following is the arrangement of the right-handed meshes.



the single mesh with two corners having one of these corners in common with each of the two four-sided meshes, which again have three corners in common. Hence in this notation *the joining lines represent the crossings.* Hence also the characters of the left-hand meshes are obvious from the figure. Outer space has the three external lines for corners—inside there is one triangle and two spaces bounded by two lines each (*i.e.*, with two corners). Thus we reproduce the left-hand part of Listing's symbol. But the figure also shows us which lines (corners) each pair of these has in common, and enables us at once to draw the annexed figure

$$\left(\begin{array}{c} l^3 \\ | \\ l^2 \\ | \\ l^2 \\ | \\ l^3 \end{array}\right)$$

which gives us exactly the same information as the first, only from a different point of view.

The connections in the former figure cannot be varied, so that, in this particular case, Listing's symbol for the right-handed meshes alone suffices to draw the figure; at least if nugatory crossings be rejected. Such would arise, for instance, if we tried to draw the symbol in the form

$$\begin{array}{c} r^2 \\ \| \\ r^4 \\ \| \\ r^4 \\ \mathrm{O} \end{array}$$

which would give three ovals joined like the links of a chain—the last having an internal nugatory loop. In this case the second part of the symbol would be

$$l^5 + 2l^2 + l$$

where the nugatory character of one intersection is clearly exhibited.

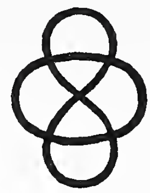
But, if we had merely the left-hand part of the symbol given us, we might adjust it thus

$$\begin{array}{ccc} & l^2 & \\ / & & \backslash \\ l^3 & \text{———} & l^3 \\ \backslash & & / \\ & l^2 & \end{array}$$

which would correspond to

$$r^4 + 2r^3$$

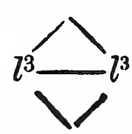
for the right-handed part, and would give us the form



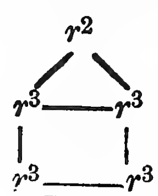
or one of its deformations.

The criterion by which to distinguish at once whether such symbolic representations as those just given represent knots or links is easy to find. If we remember that each of the (even number of) crossings lying on a closed curve is a corner of one black and of one white mesh (contained within the curve)—while each of the crossings lying within it is a corner of each of two white and of two black meshes—we see that unless we can enclose a part of the graphic symbol in such a way that the sum of the exponents within the enclosure, and that formed by the doubling of the number of the joining lines which are wholly within the enclosure, and adding it to the number of those which cut the boundary, are *equal even numbers*—the figure is necessarily a knot. But if we can enclose such a part, it requires to be farther examined to test whether the figure consists of links or is a single knot.

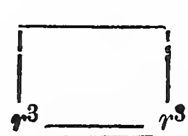
Thus, in the example just given, the part



is a simple oval divided by two intersecting chords into three-cornered meshes—but in the following formula



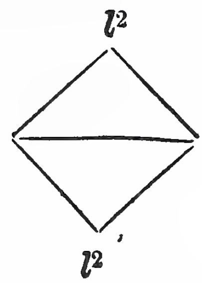
although the par



seems to fulfil the conditions above, it does not represent a separate closed curve. In fact, the upper line represents a crossing *on the boundary*, at which there is (internally) only a left-handed mesh, which is impossible if the boundary were a closed curve.

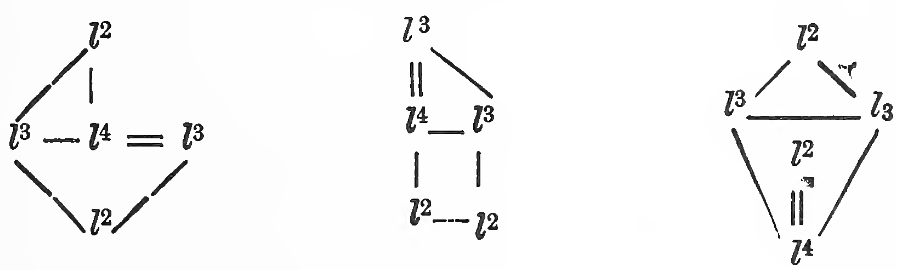
And the lowest line in the figure is a point in the boundary which forms a common vertex of *three* (internal) meshes, two right and one left-handed. This, also, is inconsistent with the boundary's being a closed curve.

There is only one other case which may cause a little trouble. It can easily be seen by the fig. of last page. For we may take out the following part of the symbol

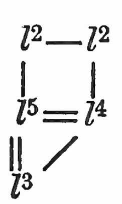


which must obviously represent the lemniscate in the figure. Its exponents and lines do not satisfy our condition : but they will do so if we remove the diagonal line—which corresponds to what is (in the lemniscate when alone) a nugatory intersection.

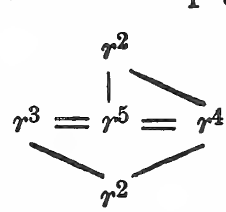
I conclude by giving the representations, according to the method just explained, of some of the preceding figures. Thus the three first figs. of p. 325 are, respectively,



while the pair of common-symbol knots on the same page are



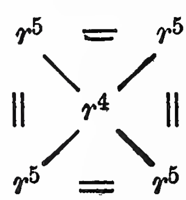
and

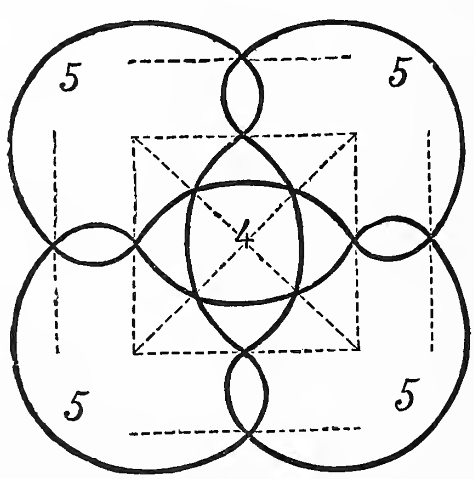


It may be observed that the present method gives great facilities for the study of cases in which knots are reduced, or are changed into links, by the removal of an intersection. For, to take off an intersection is easily seen to be equivalent simply to rubbing out one connecting line in the figure, and simultaneously diminishing by unity each of the exponents at its ends. If it be the only line connecting these exponents, they are (after reduction by unit each), to be added together. And this consideration enables us to obtain, even more simply than before, the rules for distinguishing a knot from a link. I propose, when I have sufficient leisure, to re-investigate the whole subject from this point of view.

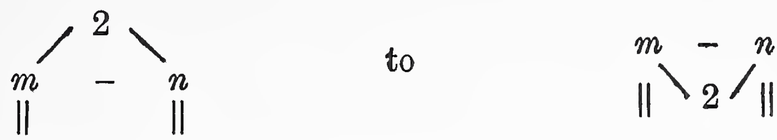
Meanwhile I may notice that it is exceedingly easy to draw the outline of any knot or link by this method. All that is necessary is to select a point in each of the lines in the figure, and join (two and two) all these points which are in the boundary of each closed area. The four lines which will thus be drawn to each of the chosen points must be treated as pairs of continuous lines *intersecting* at these points, and at these only. When there are only two sides—and, therefore, only two points—in an area, two separate lines must be drawn between them, and these must *cross* one another at each of the two points.

The annexed diagram shows the result of this process as applied to the following symbol





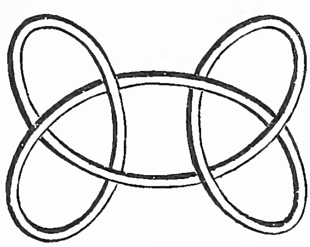
This method also clears up in a remarkable manner the whole subject of change of scheme of a given knotting which was discussed in my last paper. To give only a very simple instance, notice that the first of the changes there mentioned is merely that from



where the double lines may stand for any numbers of connection whatever.

I conclude by stating, in illustration of the remarks made at the end of my last paper, that I have hastily (though I hope correctly) investigated the nature of all the valid combinations among 720 which are possible in the even places of a scheme corresponding to 6 intersections (only 80 of these are not obviously nugatory) —and that I find *only four really distinct forms.* They are

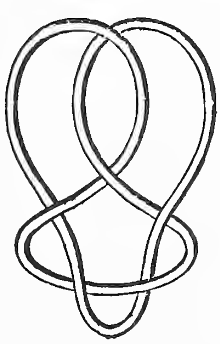
1. Two separate trefoil knots. Here there are two degrees of beknottedness.



2. The amphicheiral form. (Figured on p. 295 of my *Note on Beknottedness.* Also in a clear form in the last cut of my first paper.)

3. Fig. p. 297 of the same paper. These two forms are essentially made up of a trefoil knot and a loop intersecting it.

4. The following knot, which belongs to a species found with every possible number of crossings from 3 upwards. This species furnishes the unique knots with 3 and with 4 crossings, and one of the only two kinds possible with 5.



Its symbol is always of the form

$$\left.\begin{matrix} 2r^3+(n-3)r^2 \\ 2l^{n-1}+l^2 \end{matrix}\right\} \text{ or more fully } \begin{array}{ccc} & 2 & \\ \diagup & & \diagdown \\ (n-1) & \equiv & (n-1) \end{array},$$

there being n—2 lines in the lower group.

The three last forms have each essentially only one degree of beknottedness. In certain cases (see the foot note *ante* p. 296) we may give two degrees of beknottedness by altering some of the signs —but the knot has then one nugatory intersection, and falls into the class with five crossings.

A number of curious problems are suggested by the process which I employed in the investigation of these six-crossing forms. I give the following as an instance.

Take any arrangement whatever of the first n letters :—Say, for instance,

C N D A . . . L E.

change each to the next in (cyclical order, so that A becomes B, B becomes C, ... , N becomes A) and bring the last of the row to the beginning. The result is

F D A E B . . . . M.

After performing this operation n times we obviously get back the arrangement from which we started. [Thus in seeking all the different forms of knots of a given number of crossings, *one alone* of this set of n need be kept.] The problem is to find sets such that the original combination is repeated after m operations like that above. It is obvious that if m is to be less than n it must be an aliquot part of it, and thus n must be a composite number.

[*April* 11.—The references to Listing's type-symbol here given must be taken in connection with the extracts from his letter, *ante*, p. 316.]

3. Laboratory Notes. By Professor Tait.

(*a.*) Measurement of the Potential, required to produce Sparks of various lengths, in Air at different pressures, by a Holtz machine. By Messrs Macfarlane and Paton.

The general result of these strictly preliminary experiments appears to show that for sparks not exceeding a decimetre in length

(L), taken in air at different pressures (P), between two metal balls of $7^{mm} \cdot 5$ radius, the requisite potential (V), is expressed by the formula

$$V \propto P \sqrt{L}.$$

The Holtz machine employed is a double one, made by Ruhmkorff, and it was used with its small Leyden jars attached. The measurements had to be made with a divided-ring electrometer, so that two insulated balls, at a considerable distance from one another, were connected, one with the machine, the other with the electrometer. With P constant the curve for V closely resembles a parabola between $L = 0$ and $L = 60^{mm}$, but for higher values of L it appears to tend towards an asymptote at a finite distance from the axis. Thus it would seem that to double the length of very long sparks under these circumstances, a comparatively small percentage increase of potential will suffice. This may enable us to explain some singular peculiarities of lightning. Mr Macfarlane intends to work up this subject very thoroughly, with the help of Thomson's Long Range Electrometer.

(*b.*) The Thermal Conductivity of Gas Coke. By Messrs Knott and Macfarlane.

The method employed was the same as that described (Proc. VIII. 623), but the high conducting power of gas coke for electricity made the experimental work very difficult. The results, so far, are not very consistent with one another, but they appear to point to a diminution of conductivity by rise of temperature.

(*c.*) Preliminary Experiments on the Currents produced by contact of Wires of the same metal at different Temperatures. By Messrs Knott and Macfarlane.

These experiments, so far as they go, confirm the results formerly obtained by Mr Durham, Proc. VII. 788.

(*d.*) On the Relative Percentages of the Atmosphere and of the Ocean which would flow into a given Rent in the Earth's Surface. By Professor Tait.

The note had reference to some sensational statements recently made under the the title "Are we drying up." But it led also to a curious hydrostatical question as to the equilibrium arrangement of water poured into a shaft already full of air, and supposed to be so deep that in its lower parts the air is compressed to a density exceeding that of water. This suggested numerous questions, such as: What addition to the atmosphere would raise the sea from the earth's surface?

Monday, 5th March 1877.

Sir WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. On the Biliary Secretion with reference to the Action of Cholagogues. By Prof. Rutherford, F.R.SS. L. & E., and M. Vignal.

(*Abstract.*)

Notwithstanding the fact that substances supposed to affect the flow of the bile have been administered to man for more than 2000 years, there is still very great uncertainty as to what does and what does not augment the biliary excretion. The indefinite state of knowledge regarding this point is due to the circumstance that variations in the flow of bile from the human liver are estimated by simply observing the colour of the dejections,—a method that is of necessity exceedingly rough, for it is impossible thus to detect slight variations in the excretion of bile, especially when, as in the case of rhubarb, the substance gives to the dejections a colour similar to that of the bile, and where, as in the case of sodium sulphate, the substance gives rise to copious dejections of a watery character, whereby the colour is diluted.

Even in the case of those substances generally believed to augment the biliary flow, *e.g.*, podophylline, observations on man have entirely failed to determine whether they merely occasion an expulsion of bile from the gall bladder, or an increased secretion by the liver. The determination of the point is of great importance in scientific medicine, for it is calculated to advance our knowledge of the nature of the diseased condition relieved or cured by the cholagogue.

By experiments on animals, both of the above-mentioned difficulties may be overcome, and definite knowledge arrived at.

The want of precise knowledge in this department of therapeutics induced Nasse, Kölliker, and Müller, Scott, Hughes Bennett, and Röhrig to perform some experiments; but the results have been limited and unsatisfactory, owing to the faultiness of the experimental methods adopted.

In the present research the experiments have been performed on dogs, fasting and curarised, in order that the secretion of bile might be rendered constant. The bile was continuously collected from the common bile duct, and measured every fifteen minutes, and the flow of bile into or out of the gall bladder was eliminated by clamping the cystic duct. The results obtained are as follows:—

1. In a curarised dog that has fasted 18 hours, the secretion of bile is tolerably uniform during the first four or five hours after the commencement of the experiment, but falls slightly as a longer period elapses. Its composition remains constant.

2. Croton oil is a cholagogue of feeble power. The high place assigned to it by Röhrig was probably the result of his imperfect method of experiment.

3. Podophylline is a very powerful stimulant of the liver. During the increased secretion of bile, the percentage amount of the special bile solids is not diminished. If the dose be too large, the secretion of bile is not increased. It is a powerful intestinal irritant.

4. Aloes is a powerful hepatic stimulant. It renders the bile more watery, but at the same time increases the excretion of biliary matter by the liver.

5. Rhubarb is a certain though not a powerful hepatic stimulant. The bile secreted under its influence has the normal composition.

6. Senna is a hepatic stimulant of very feeble power. It renders the bile more watery.

7. Colchicum increases to a considerable extent the amount of biliary matter excreted by the liver, although it renders the bile more watery.

8. Taraxacum is a very feeble hepatic stimulant.

9. Scammony is a very feeble hepatic stimulant.

10. "Euonymin," an impure resinous matter prepared from the bark of *Euonymus atropurpureus*, is a powerful hepatic stimulant. It is not nearly so powerful an irritant of the intestine as podophylline.

11. "Sanguinarin," an impure resinous matter prepared from the *Sanguinaria canadensis*, is a powerful hepatic stimulant. It also stimulates the intestine, but not nearly so powerfully as podophylline.

12. "Iridin," an impure resinous matter prepared from the root of *Iris versicolor*, is a powerful hepatic stimulant. It also stimulates the intestine, but not so powerfully as podophylline.

13. Leptandria is a hepatic stimulant of moderate power. It is a feeble intestinal irritant.

14. Ipecacuan is a powerful hepatic stimulant. It increases slightly the secretion of intestinal mucus; but has no other apparent stimulant effect on the intestine. The bile secreted under the influence of ipecacuan has the normal composition.

15. Colocynth is a powerful hepatic as well as intestinal stimulant. It renders the bile more watery, but increases the secretion of biliary matter.

16. Jalap is a powerful hepatic as well as intestinal stimulant.

17. Sodium sulphate is a hepatic stimulant of considerable power. It also stimulates the intestinal glands.

18. Magnesium sulphate is an intestinal but not a hepatic stimulant.

19. Potassium sulphate is a hepatic and intestinal stimulant of considerable power. Its action on the liver is, however, uncertain, probably owing to its sparing solubility.

20. Sodium phosphate is a powerful hepatic, and a moderately powerful intestinal stimulant.

21. Rochelle salt is a feeble hepatic, but a powerful intestinal stimulant.

22. Sodium chloride is a very feeble hepatic stimulant.

23. Sodium bicarbonate has scarcely any appreciable effect as a hepatic stimulant.

24. Potassium bicarbonate has scarcely any appreciable effect as a hepatic stimulant.

25. Mercuric chloride is a powerful hepatic stimulant. Its stimulant effect on the intestinal glands is feeble.

26. Calomel is a powerful stimulant of the intestinal glands, but does not increase the secretion of bile.

27. Ammonium chloride has no stimulating effect on the liver of the dog.

28. Castor oil stimulates the intestinal glands, but not the liver.

29. Gamboge stimulates the intestinal glands, but not the liver.

The foregoing results, although adding greatly to our knowledge, are in complete harmony with observations on man in every case, save those of calomel and ammonium chloride; but the want of harmony is probably more apparent than real, for there is no evidence that in man these substances really do excite the hepatic cells. They may possibly occasion merely *expulsion* of bile already secreted, or may act on the liver in other ways. The general conclusions of the research are as follows:—

1. That, as the liver of the dog is affected by medicinal agents in the same sense as the human liver, this animal is suitable for observations that cannot be made on man.

2. The attention of medical men is hereby directed to a number of valuable cholagogues, such as euonymin, sanguinarin, iridin, ipecacuan, sodium phosphate, sodium sulphate, &c., hitherto but little or not at all employed as such, owing to the absence of positive information regarding their actions.

3. As regards the whole question of hepatic pharmacology, definite is hereby substituted for indefinite knowledge; for it is not only shown what substances really do act as cholagogues, but it is proved that they all, excepting calomel and ammonium chloride, really do stimulate the liver to secrete more bile.

4. It is shown that, as such substances as magnesium sulphate, gamboge, and castor oil do not increase the secretion of bile, although they irritate the mucous membrane of the duodenum and the remainder of the intestine, the action of stimulants on the secreting apparatus of the liver is probably not reflex from the intestinal mucous membrane, but a direct action either upon the hepatic cells or upon their nerves.

5. It is shown that when a purgative substance is not a cholagogue it diminishes the biliary secretion. The importance of a knowledge of this fact is indicated.

2. Specimen of Auriferous Quartz. By Patrick Dudgeon.

The specimen was found near Wanlockhead, Dumfriesshire, in 1872, by Andrew Gemmell, miner, who unfortunately broke up the piece, and disposed of the fragments to different parties in the locality. Mr Dudgeon was enabled to get possession of all the fragments, and has restored the mass to its original form. The specimen is a very interesting one, being the largest known piece of auriferous quartz found in Scotland; and Mr Dudgeon thought it would interest the fellows of the Society to exhibit it at the meeting before placing it in the Museum of Science and Art, which has now been done with the consent of the different owners.

The following Gentlemen were elected Fellows of the Society:—

George Broadrick, C.E., Claremont Cottage, Leith.
John Napier, Engineer, Lancefield House, Glasgow.
James King, of Campsie, 12 Claremont Terrace, Glasgow.
George Cunningham, C.E., 2 Ainslie Place.

Monday, 19th March 1877.

Sir WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. On a Problem of Arrangements. By Professor Cayley.

It is a well-known problem to find for n letters the number of the arrangements in which no letter occupies its original place; and the solution of it is given by the following general theorem:—viz., the number of the arrangements which satisfy any r conditions is

$$(1-1)(1-2)\ldots\ldots(1-r),$$
$$=1-\Sigma(1)+\Sigma(12)\ldots\pm(12\ldots r),$$

where 1 denotes the whole number of arrangements; (1) the number of them which fail in regard to the first condition; (2) the number which fail in regard to the second condition; (12) the number which fail in regard to the first condition, and also in regard to the second condition; and so on: $\Sigma(1)$ means $(1)+(2) \ldots +(r)$: $\Sigma(12)$ means $(12)+(13)+(2r) \ldots +(r-1, r)$; and so on, up to $(12 \ldots r)$, which denotes the number failing in regard to each of the r conditions.

Thus, in the special problem, the first condition is that the letter in the first place shall not be *a;* the second condition is that the letter in the second place shall not be *b*; and so on; and taking $r=n$, we have the known result, No. =

$$\Pi n - \frac{n}{1}\Pi(n-1) + \frac{n \cdot n-1}{1.2}\Pi\ (n-2). + \ldots \pm \quad \frac{n \cdot n-1 \ldots 2 \cdot 1}{1 \cdot 2 \ldots n},$$

$$=1.2.3 \ldots n \left\{ 1 - \frac{1}{1} + \frac{1}{1.2} - \frac{1}{1 \cdot 2 \cdot 3} \ldots \pm \frac{1}{1 \cdot 2 \cdot 3 \ldots n} \right\},$$

giving for the several cases

$$n = 2, 3, 4, \ 5, \ 6, \ 7, \ldots$$
$$\text{No.} = 1, 2, 9, 44, 265, 1854 \ \ldots$$

I proceed to consider the following problem, suggested to me by Professor Tait, in connexion with his theory of knots: to find the number of the arrangements of n letters $a\ b\ c \ldots j\ k$, when the letter in the first place is not a or b, the letter in the second place not b or c, ... the letter in the last place not k or a.

Numbering the conditions $1, 2, 3 \ldots n$, according to the places to which they relate, a single condition is called [1]; two conditions are called [2] or [1, 1], according as the numbers are consecutive or non-consecutive: three conditions are called [3], [2, 1] or [1, 1, 1], according as the numbers are all three consecutive, two consecutive and one not consecutive, or all non-consecutive; and so on: the numbers which refer to the conditions being always written in their natural order, and it being understood that they follow each other cyclically, so that 1 is consecutive to n. Thus, $n=6$, the set 126 of conditions is [3], as consisting of 3 consecutive conditions; and similarly 1346 is [2, 2].

Consider a single condition [1], say this is 1; the arrangements which fail in regard to this condition are those which contain in the first place a or b; whichever it be, the other $n-1$ letters may be arranged in any order whatever; and there are thus $2\ \Pi(n-1)$ failing arrangements.

Next for two conditions; these may be [2], say the conditions are 1 and 2, or else [1 , 1] say they are 1 and 3. In the former case the arrangements which fail are those which contain in the first and second places ab, ac, or bc, and for each of these the other $n-2$ letters may be arranged in any order whatever; there are thus $3\,\Pi\,(n-2)$ failing arrangements. In the latter case the failing arrangements have in the first place a or b, and in the third place c or d,—viz., the letters in these two places are $a\,.\,c$, $a\,.\,d$, $b\,.\,c$, or $b\,.\,d$, and in each case the other $n-2$ letters may be arranged in any order whatever: the number of failing arrangements is thus $= 2\,.\,2\,.\,\Pi\,(n-2)$. And so in general when the conditions are $[\alpha, \beta, \gamma\,.\,.]$, the number of failing arrangements is $=(\alpha+1)(\beta+1)(\gamma+1)\,.\,.\,\Pi(n-\alpha-\beta-\gamma\,.\,.)$. But for $[n]$, that is for the entire system of the n conditions, the number of failing arrangements is (not as by the rule it should be $=n+1$, but) $=2$, —viz., the only arrangements which fail in regard to each of the n conditions are (as is at once seen), $abc\,.\,.\,jk$, and $bc\,.\,.\,.\,jka$.

Changing now the notation so that [1], [2], [1,1], &c., shall denote the *number* of the conditions [1], [2], [1,1], &c., respectively, it is easy to see the form of the general result: if, for greater clearness, we write $n=6$, we have

$$\begin{array}{llll} 1 & -\Sigma(1) & +\Sigma(12) & -\Sigma(123) \\ \text{No.} = 720 & -\{([1]=6)2\}120 & + \left\{\begin{array}{l} ([2]\ \ =6)3 \\ +([1,1]=9)2.2 \end{array}\right\}24 & - \left\{\begin{array}{l} ([3]\ \ \ =6)4 \\ +([2,1]\ =12)3.2 \\ +([1,1,1]=\ 2.2.2 \end{array}\right\} \end{array}$$

$$\begin{array}{lll} +\Sigma(1234) & -\Sigma(12345) & +(123456) \\ + \left\{\begin{array}{l} ([4\ \ ]=6)5 \\ +([3,1]=6)4.2 \\ +([2,2]=3)3.3 \end{array}\right\}2 & -\{([5]=6)6\}1 & +\{([6]=1)2\} \end{array}$$

or, reducing into numbers, this is

$$\text{No.} = 720 - 1440 + 1296 - 672 + 210 - 36 + 2\,, \qquad = 80\,.$$

The formula for the next succeeding case, $n=7$, gives

$$\text{No.} = 5040 - 10080 + 9240 - 5040 + 1764 - 392 + 49 - 2\,, = 579\,.$$

Those for the preceding cases, $n=3$, 4, 5, respectively are

$$\text{No.} = \;\; 6 - \;\; 12 + \;\; 9 - \;\; 2 \qquad = \;\; 1$$
$$\text{No.} = 24 - \;\; 48 + \;\; 40 - \;\; 16 + 2 \qquad = \;\; 2$$
$$\text{No.} = 120 - 240 + 210 - 100 + 25 - 2 \qquad = \;\; 13\,.$$

We have in general $[1]=n$, $[2]=n$, $[1,\,1]=\frac{1}{2}n(n-3)$; and in the several columns of the formulæ the sums of the numbers thus represented are equal to the coefficients of $(1+1)^2$, thus, $n=6$ as above, the sums are 6, 15, 20, 15, 6, 1. As regards the calculation of the numbers in question, any symbol $[\alpha,\,\beta,\,\gamma]$ is a sum of symbols $[\alpha-\alpha'+\beta-\beta'+\gamma-\gamma'\,.\,.]$, where $\alpha'+\beta'+\gamma'\,.\,.$ is any partition of $n-(\alpha+\beta+\gamma\,.\,.)$; read, of the series of numbers $1,\,2,\,3\,\ldots\,n$, taken in cyclical order beginning with any number, retain α, omit α', retain β, omit β', retain γ, omit γ', . . Thus in particular, $n=6$, $[1,\,1]$ is a sum of symbols $[1-3+1-1]$ and $[1-2+1-2]$; it is clear that any such symbol $[\alpha'-\alpha'+\beta-\beta'\,.\,.]$ is $=n$ or a submultiple of n (in particular if n be prime, the symbol is always $=n$): and we thus in every case obtain the value of $[\alpha,\,\beta,\,\gamma\,.\,.]$ by taking for the negative numbers the several partitions of $n-(\alpha+\beta+\gamma\,.\,.)$ and for each symbol $[\alpha-\alpha'+\beta-\beta'+\gamma-\gamma'\,.\,.]$, writing its value, $=n$ or a given submultiple of n, as just mentioned. There would, I think, be no use in pursuing the matter further, by seeking to obtain an analytical expression for the symbols $[\alpha,\,\beta,\,\gamma\,.\,.]$.

For the actual formation of the required arrangements, it is of course easy, when all the arrangements are written down, to strike out those which do not satisfy the prescribed conditions, and so obtain the system in question. Or introducing the notion of substitutions,* and accordingly considering each arrangement as derived by a substitution from the primitive arrangement *abcd . . . jk*, we can write down the substitutions which give the system of arrangements in which no letter occupies its original place: viz., we must for this purpose partition the n letters into parts, no part less than 2, and then in each set taking one letter (say the first in alphabetical order) as fixed, permute in every possible way the

* In explanation of the notation of substitutions, observe that (*abcde*) means that *a* is to be changed into *b*, *b* into *c*, *c* into *d*, *d* into *e*, *e* into *a*; and similarly (*ab*)(*cde*) means that *a* is to be changed into *b*, *b* into *a*, *c* into *d*, *d* into *e*, *e* into *c*.

other letters of the set; we thus obtain all the substitutions which move every letter. Thus $n=5$, we obtain the 44 substitutions for the letters *abcde*, viz., these are

(*abcde*), &c., 24 symbols obtained by permuting in every way the four letters *b*, *c*, *d*, *e*.

(*ab*)(*cde*), &c., 20 symbols corresponding to the 10 partitions *ab*, *cde*, and for each of them 2 arrangements such as *cde*, *ced*.

And then if we reject those symbols which contain in any ( ) two consecutive letters, we have the substitutions which give the arrangements wherein the letter in the first place is not *a* or *b*, that in the second place not *b* or *c*, and so on. In particular $n=5$, rejecting the substitutions which contain in any ( ), *ab*, *bc*, *cd*, *de*, or *ea*, we have 13 substitutions, which may be thus arranged:—

(*acbed*), (*ac*)(*bed*), (*acebd*), (*adbec*), (*aedbc*),
(*aedbc*), (*bd*)(*aec*),
(*acedb*), (*ce*)(*adb*),
(*aecbd*), (*ad*)(*bec*),
(*adceb*), (*be*)(*adc*).

Here in the first column performing on the symbol (*acbed*) the substitution (*abcde*), we obtain (*bdcae*), =(*aebdc*), the second symbol; and so again and again operating with (*abcde*) we obtain the remaining symbols of the column; these are for this reason said to be of the same type. In like manner symbols of the second column are of the same type; but the symbols in the remaining three columns are each of them a type by itself; viz., operating with (*abcde*) upon (*acebd*) we obtain (*bdace*), =(*acebd*); and the like as regards (*adbec*) and (*aedbc*) respectively. The 13 substitutions are thus of 5 different types, or say the arrangements to which they belong, viz.,

cebad, *ceabd*, *cdeab*, *deabc*, *eabcd*,
edacb, *edabc*,
caebd, *daebc*,
edbac, *debac*,
daecb, *deacb*.

are of 5 different types. The question to determine for any value of *n*, the number of the different types, is, it would appear, a difficult one, and I do not at present enter upon it.

2. On the Construction of the Canon of Sines, for the Decimal Division of the Quadrant. Edward Sang, Esq.

The convenience of having only one system of numeration is so well recognised, that there is no need for any discussion. Already the numerical nomenclatures of all nations having any culture have been converted to one, namely, the decimal system, and traces of the ancient use of dozens, scores, fifteens, or sixties, can be found in only a very few of them. Although we count our hours in sixty minutes, we do not date the present as the year *thirty-one sixties and seventeen.* Yet, in the matters of measure, weight, and value, the old and irksome divisions continue to be used; nay, even in those departments of science which most need laborious calculation, we continue to employ the ancient scale of division.

It is, indeed, remarkable that, while men of business are striving for uniformity in the modes of counting and of measuring, trigonometers and astronomers should remain unconcerned as to the subdivision of arcs and of time. We are rapidly approaching the anomalous position of using the decimal division of the earth's quadrant as the source of our standards of weights and measures, and of yet rejecting that division in our notation of angles.

The want of trigonometrical tables suitable to the new division is the real cause of this backwardness; the construction of these tables is essentially the first step to the universal employment of the decimal system. This has been long and well recognised. In the end of the last century, Borda computed the decimal canon; this computation was superseded by that which the French Government caused to be made under the superintendence of Prony; but neither of these has been put to press. The only centesimal table available to the trigonometer is that given for each minute of the quadrant, in Callet's "Tables Portatives;" it was collated with the manuscripts of Borda and of Prony, but is presented in a most inconvenient form.

The eminent astronomer, Laplace, adopted the decimal division of arcs, of distance and of time, in his "Mécanique Celeste," published in the last year of the century. The force of this illustrious example might long since have gained universal acceptance for the system, had not the non-publication of the requisite canon prevented all progress in this direction.

Notwithstanding many solicitations, and even the offer by the English Government to defray a share of the expense, the tables computed in the Bureau du Cadastre remain unpublished; and the fact of their existence remains a discouragement to other computers.

Now, fifty years ago, having fallen upon a method of approximating very rapidly to the roots of numerical equations, published in 1829 under the title, "Solution of Algebraic Equations of all Orders," I applied it to the quinquisection of an arc, and thus obtained directly the sine of any proposed decimal division of the quadrant. After proceeding a short way in the construction of the canon by this method, I laid it aside, from the conviction that the labour could, at best, only produce a repetition of what had already been accomplished.

Many years ago, after having felt for long the want of a table of logarithms more extensive than any existing, I designed a nine-place table up to one million; and having carried the manuscript fifteen-place table up to 300 000, laid it before this Society, whose Council did me the exceedingly great favour of presenting to Government a memorial soliciting aid in the completion and publication.

One of our scientific periodicals, in noticing this memorial, supplied to Government a most potent reason for not acceding to the request, in this, that the labours of Prony in the Bureau du Cadastre had already anticipated and surpassed all that can be done in future in this department of calculation.

Forced thus into a critical examination of the Cadastre Tables, so far as that could be accomplished by help of published documents, I arrived at most unexpected conclusions.

In the first place, the fundamental table, that of the Logarithms of the Prime Numbers, as given by Legendre in his work on Elliptic Functions, was found to be correct up to 2000; that is, as far as Abraham Sharp and other ancient computers had gone. But of the primes between 2000 and 10 000 computed in the Bureau, only five have their logarithms correctly given, while almost all of the other logarthims err on one side.

In the second place, Henry Brigg's original table had been collated by Prony's assistants, and errors in the tenth and higher places had been found; yet no notice had been taken of the very many errors in the fourteenth and even in the thirteenth place.

Thirdly, the system of computation adopted was so imperfect that, although differences of the sixth order were extended to the thirty-sixth place, the results were liable to errors up to the twentieth figure.

Lastly; the Cadastre calculations were used by M. Lefort for correcting Adrian Vlacq's ten-place table (that table of which all the subsequently published seven-place tables are abridgments); with the result of sometimes putting Vlacq in the wrong when he is right. That is to say, the Cadastre calculations cannot be trusted for the compilation of a ten-place logarithmic table.

Such being the case with that part of Prony's great work which was comparable with previously published tables, we are unable to place confidence in the trigonometrical portion, which necessarily is almost entirely new; and we are forced, when contemplating the compilation of the Canon of Sines, to hold the Cadastre Tables as non-existent, or at best, as useful for controlling palpable errors of the press.

In actual trigonometrical calculations we very seldom use the sines and tangents themselves, but employ their logarithms instead; wherefore, both the Canon of Sines and the Logarithmic Canon are needed as the joint foundation of our working tables. Having carried the table of logarithms as far as to 370 000, and being satisfied of the insufficiency of the work done in the Bureau du Cadastre, I resumed the computation of the sines, and have now proceeded to such a length as to be able to submit the methods employed to the Society, and through it to the mathematical public.

The decimal division of the quadrant is effected by bisections and quinquisections, and the first thing to be determined is the order in which these operations should be taken. Now, we obtain the sine of the half arc, not from the sine of the whole arc, but from its cosine, or rather from its co-versed-sine; when the arc is small the co-versed-sine, or defect of the cosine from the radius, is represented by a very small decimal fraction, the number of whose effective figures is less than the total number of figures in the calculations; we have to extract the square root of its half, which square root, can only be true to as many effective figures, and thus cannot be extended to the full number. If, then, we desire to obtain accuracy to a specified number of decimal places, we must extend our first calculations far beyond. Whereas the sine of an odd sub-multiple is deduced directly from the sine of the whole arc, and the computation is such as to retain the full number of effective figures. Hence we must proceed by first making all the requisite bisections, and thereafter the quinquisections.

There is, however, an obvious exception. The sines of 20^c, 40^c, 60^c and 80^c are obtained by the solution of quadratic equations, so that this quinquisection naturally comes first; the bisections afterwards, and then the remaining quinquisections.

Having prescribed unit in the thirtieth decimal place as the limit of inexactitude, and taken three figures more to obviate the accumulation of the minute errors inevitable in all approximate work, I computed the sines of these four arcs to thirty-three places.

The bisection of the quadrant gave the sine of 50^c; that of 60^c gave for 30^c and for its complement 70^c; that of 20^c gave for 10^c and 90^c, thus completing the table for each tenth part of the quadrant.

In the same way the bisections of 10^c, 30^c, 50^c, 70^c, and 90^c gave the sines of the intermediate fifth degrees.

The bisections were continued in this way until the quadrant was divided into 80 equal parts.

In performing this work, each of the roots

$$\sin a = \sqrt{\{\tfrac{1}{2} - \tfrac{1}{2}\cos 2a\}}, \qquad \cos a = \sqrt{\{\tfrac{1}{2} + \tfrac{1}{2}\cos 2a\}}$$

was extracted twice, and sin a was also found from the division

$$\sin a = \frac{\sin 2a}{2\cos a},$$

by which means the values of $\sin a$ and $\cos a$ were securely got, and the previous computations of $\sin 2a$, $\cos 2a$ again verified. There were, as a matter of course, small errors in the last places, but the results, to the thirtieth place, were made sure.

The quinquisections were obtained by help of the equation

$$16\ \sin a^5 - 20 \sin a^3 + 5 \sin a - \sin 5a = 0 = \mathrm{E}\,.$$

Regarding E as a function of $\sin a$, its successive derivatives are

$$\begin{aligned} {}_1\mathrm{E} &= 80.\sin a^4 - 60.\sin a^2 + 5 \\ {}_2\mathrm{E} &= 320.\sin a^3 - 120.\sin a \\ {}_3\mathrm{E} &= 960.\sin a^2 - 120 \\ {}_4\mathrm{E} &= 1920.\sin a \\ {}_5\mathrm{E} &= 1920\ ; \end{aligned}$$

now, while resolving the equation, we get the values of all the derivatives; so, taking advantage of these, we have

$$\cos a^2 = \frac{7}{8} - \frac{1}{960}\,{}_3\mathrm{E}$$

$$\cos 2a = \frac{3}{4} - \frac{1}{480}\,{}_3\mathrm{E}$$

$$\sin 3a = \frac{3}{2}\ \sin a - \frac{1}{80}\,{}_2\mathrm{E}$$

$$\cos 4a = \frac{1}{4} - \frac{1}{480}\,{}_3\mathrm{E} + \frac{1}{10}\,{}_1\mathrm{E}\,;$$

the first of these gives us, by extracting the square root, cos a.

Applying this method to the arc $1^c\ 25'$, the eightieth part of the quadrant, I obtained the sine and cosine of $25'$, and proceeded to compute the functions of its multiples by help of second differences, according to the well-known formula.

$$\sin(n+1)\,a - 2\sin na + \sin(a-1)\,a = \sin n\,a.2\ \mathrm{ver}\,a\,;$$

and, because the multiplier 2 ver$a$ is to be repeatedly used, a table of its multiples was constructed, in the case of $a = 25'$, up to the hundredth, in the cases of $a = 5'$ and $a = 1'$, up to the thousandth multiple.

The sines of these multiples being computed continuously, an error in any part of the work, propagated subsequent errors, which could not possibly be overlooked in comparing the results at each fifth step with the previously computed test values. In this way immunity from error was obtained, excepting in the last places, where small errors are inevitable.

In performing the multiplication sin na.2 ver a stopping at the thirty-third place, the last figure of each partial product may err in excess or in defect by $\frac{1}{2}$; now it is possible, though not likely, that all of these errors may be on one side, and therefore there is a *possible* error in the last place of the total product, of half as many units as there are lines in the multiplication. By using the table of multiples up to one thousand, we reduce the number of lines to one third, and therefore the possible amount of residual error in the same ratio; so that the auxiliary table both saves labour and augments the exactitude of the result.

These final-place errors were corrected at each fifth step by altering the last figures of the second differences, and thus the accumulation of those errors was prevented. In the whole calculation of the sines of the quarter degrees, it was not found necessary to alter any one second difference to so much as the limit of possible error, and therefore we may hold that the manuscript table of the sines of these arcs is absolutely correct to within the prescribed degree of precision, namely, unit in the thirtieth decimal place.

The next quinquisection, conducted in the same way, gave the sine and cosine of 5′; the functions of whose multiples were obtained as before, and compared, at each fifth step, with the previous work. The table of sines to every fifth minute is already well advanced.

The third quinquisection gave the sine and cosine of the single minute of the decimal division. A table of the multiples of 2 ver 1′ has been constructed up to 1000, and has been used in forming a good beginning of the canon of sines to each single minute.

For the purpose of preventing all error in the record of these calculations, the second differences only were copied from the duplicate scroll calculations, and the successive first differences and sines were thence recomputed on the record sheet. Since any error in copying, or even in the original computation, was necessarily continued and extended into the after part of the record, its detection was rendered certain, so that the recorded results may be implicitly relied on. To make the record more secure, each page was copied on thin transfer paper, on which no alteration can be made without being obvious.

The method of computing might have been extended to differences of the fourth order, in which case the common multiplier, 4 ver a^2, would have been much smaller, and the terms of the products fewer. But, on the other hand, the entries in the record would have been more, and the effects of the residual errors would have been much greater and more troublesome in correction. For the interpolation of each quarter degree, the saving obtained by the use of fourth differences would have been unappreciable; for those of each fifth and of each single minute, it would have been hardly such as to compensate for the inconveniences just mentioned; but for the future interpolations of each tenth second and of each single second, the fourth differences may be advantageously used.

When the canon of sines shall have been completed, the computation of the working table, that of logarithmic sines, will be easy, particularly by help of my fifteen-place table of logarithms.

Beginning, as John Nepair did, at the sine of the whole quadrant and proceeding downwards, the computed logarithmic sines may be verified by their differences, which are small. When the work has been brought down to the log sin of the half-quadrant, the farther progress is easy and rapid, for the formula

$$2 \sin a . \cos a = \sin 2 a$$

gives $\log \sin a = \log \frac{1}{2} + \log \sin 2 a - \log \cos a$, so that the differences of any order may be got at once from the previously tabulated differences of that order, and, what is most worthy of remark, may be used without the fear of an accumulation of residual error.

The table of logarithmic tangents follows as a matter of course.

3. On the Precautions to be taken in recording and using the Records of Original Computations. Edward Sang, Esq.

The real utility of tables of numerical results is only secured by making them accessible to those computers who may require them;

and the essence of their utility lies in this, that the labour of a single computer saves that of many others.

It is indispensable that those who use the tables be able to rely implicitly on the accuracy of the tabulated numbers, and that they have a ready means of detecting any error should the existence of one be suspected.

I do not mean, at present, to say a word on the mechanical arrangements of setting up the types, of stereotyping, revising the proofs, and printing; these have already often been discussed, but I shall take the matter up at this critical point:—The investigator has in his hands a set of printed or of manuscript tables, which he means to use in his researches, and he wishes to know whether the individual book be or be not to be trusted. His confidence must necessarily be influenced by the history of the book and by its co-relatives. Thus, if it be a stereotype copy of a work in extensive circulation, he may accept the general opinion as to its accuracy; but if the table be one seldom used, such as those which serve as the foundation of working tables, this source of confidence fails him.

The nature of the case may be most clearly seen from an example:—

We propose to extend the logarithmic canon beyond the limits to which it has been already printed; this extension must be founded on the logarithms of the prime numbers; now Abraham Sharp computed, to 61 places, the logarithm of every prime number up to 1097; these were printed in Sherwin's collection, and thence reprinted by Callet in his Tables Portatives; shall we then build our more extensive tables on the computation by Sharp? Sharp was known as a most zealous and careful computer; both Sherwin and Callet would take care that the numbers be correctly copied; yet for all that, we cannot venture to found on Sharp's work because there is an essential omission.

If we were to proceed to compute, by help of these, the logarithms of larger primes, and if, after a lengthened series of operations, we were to find a disagreement, we should be left in doubt as to which of the many logarithms that had been used may be in fault; we should have to recompute such of Sharp's logarithms as might be implicated, while the labour and irksomeness of the search would become intolerable.

In all such calculations we seek to arrive at the result by two independent processes. All the use, therefore, that can be made of Sharp's tables, is to hold his work as constituting one of these processes; a great use certainly, yet, at best, only half of what it might have been.

Now, in the computation, to twenty-eight places, of the logarithms of the prime numbers, no error whatever was discovered among those given by Callet; so here we have an instance of records, in themselves quite exact, and yet insufficient to obviate subsequent recomputation.

The means of readily verifying the record are awanting; these means must necessarily vary with the nature of the tabulated functions.

In the volumes containing the computation of the logarithms to twenty-eight places of all primes below ten thousand, which was laid before the Society, the articulate steps of every calculation are recorded and indexed, so that if an error be suspected in any one logarithm, we have the means of instantly verifying the table, or of detecting the source of the error. Had such a record accompanied Sharp's admirable table, the need for subsequent re-calculation would have been entirely obviated.

The vast majority of tables have their arguments arranged with equal differences, consequently the functions progress gradually; and, for the most part, these tables have been constructed by help of differences. It is then sufficient to record the differences along with the values of the functions. For the canon of sines I have found it convenient to place the first difference, with the sign +, and then the second difference, with the sign −, below the preceding sine, as shown below:—

Arc.	*Sine.*
1° 12′	.01759 20113 41099 72108 93607 04490
	+ 15 70551 06706 95090 77046 38118
	− 4 37940 65992 07711 07444
1...13	.01774 90664 47806 67199 70653 42608
	+ 15 70546 68766 29098 69335 30674
	− 4 41815 82853 79716 53726
1...14	.01790 61211 16572 96298 39988 73281

This arrangement enables the computer to examine any sin which he wishes to extract, so as to guard against any typographical error; and, if the table of the multiples of 2 ver 1′ were appended, to check readily the computation itself. When, however, the differences have only a few figures, the ordinary method, of placing them in separate columns, is to be preferred; it saves room, while the practised calculator has no difficulty in performing the requisite additions or subtractions. The utility of this arrangement has been long recognised.

In the case of tables for common use, which are, in general, abbreviations of original calculations to a greater number of places, it is enough to give the first differences when these vary much.

When such means of verification have been provided, the user of the table can make sure that the number which he extracts contains no error; and if all users were to habitually make this examination, printed tables, and above all those printed from stereotype, would be gradually freed from errors of press.

4. On an Unnamed Palæozoic Annelid. By Professor Duns. (Plate IV.)

CYMADERMA* (nov. gen.).—*Generis Characteres:*—Corpus cylindricum, elongatum, nudum, striatum; striæ tenues, in ordinem undulatæ, et ubique corpus cingentes, ita ut cutis subrugosa videatur; linea dorsualis continua, alternasque cavaturas ovatus et vertices ostendens; nulla verorum articulamentorum indicia; vestigium tortuosum, bisulcum.

CYMADERMA (new genus).—*Generic Characters:*—Body cylindrical, elongated, naked, striated; striæ minute, waved symmetrically, encompassing the form in all parts, and giving to the skin a subrugose appearance; a continuous dorsal line, showing alternate oval depressions and slight ridges; no traces of true articulations; track, tortuous bisulcate.

This annuloid fossil was obtained from upper Carboniferous strata, in a cutting on the Midland Railway, valley of the Ribble, near Settle, Yorkshire. Peculiarities characteristic of the striation and the median dorsal line led me to conclude that the form was probably rare, or one that had not been described. It was thus of importance to get the other part of the slab on which the impres-

* Etymol. *κῦμα, unda, δέρμα, cutis.*

sion, or *intaglio*, corresponding to the rounded relief, might be expected. The friend who had forwarded the fossil made diligent search, but found the upper part of the slab had been destroyed. In answer to a request for any animal tracks, or traces of organisms that might be met with in the same, or closely related deposits, he sent me several slabs of compact dark-coloured micaceous, sandy shale, on which some markings occurred. On splitting these, they were found crowded with tracks and casts of forms closely resembling that first sent, though the matrix is lithologically different. These shales intervene between bands of the so-called gritstone, in which the form now noticed was found. Its position was about 16 feet from the surface, in a gritstone bed 4 feet thick, having above a deposit of alternating shales and gritstones nearly 15 feet thick and below a bed of limestone.

The dark-coloured slabs present features of much interest. They contain traces of three species, the prevailing one being identical with that now before us, though none of the characteristic marks are so sharply outlined. They make it clear, however, that the surface on which the median line occurs is dorsal, and they establish the bisulcate character of the track. On the reverse of one slab, the *intaglio* I was anxious to obtain shows distinctly that the dorsal line is ridged. This is also very well marked in another. In both the slight dorsal ridge is represented by a corresponding depression or furrow in the overlying shale. Even the outlines of these tracks are suggestive to the ichnologist. Some of them are comparatively deep, some shallow; in some the bisulcate character is well seen, while in others the lower surface of the track is flat. The explanation of this is obvious. If, for example, we look at the tracks, say, of our common whelks (*Littorinæ*), we see that those formed in shallow shore-pools are flat; those on very wet sand are slightly hollow, their edges presenting a corrugated appearance; while those made on fine sand beginning to dry, are marked at regular intervals with distinct transverse lines. It is not unusual to observe all these crossing one another, or seeming to form loops with each other. The waved whelk (*Buccinum undatem*) makes a bisulcate track on the sand when it is partially dry. Occasionally, however, it trails the sharp edge of its *operculum* in such a way as to give three *sulci*. Ichnologists may thus have the track of only one form before them, when they describe three or four apparently different ones, and trace them to different species. Thus, such terms as *unisulcus*, *bisulcus*, *trisulcus*, *unisulcus corrugatus*, and the like, instead of indicating so many distinct species, may, in reality, express the varying form of the track of one only. Perhaps, even the Hitchcocks' in their magnificent works on ichnology have not given due weight to this consideration.*

The slabs laid on the table have been examined with great care, in the hope of detecting traces of hairs, setæ, or any other characteristic marks of true recent annelids. But neither hairs nor bristles have been found. On most of the slabs, however, markings occur, which I am inclined to regard as the outlines of organs analogous to the gill-leaves of such forms as *Phyllodoce*. These are worthy the attention of naturalists. They are numerous, and have not, I think, been observed before. While occurring on the same surface as the outlines and tracks of the organisms, only in one instance they seem attached to them, but even in this case the association is doubtful. See Plate IV. fig. 2, for an outline of some of these markings. In one case, in which a good duplicate was obtained, distinct traces as of a fringe appear. An enlarged rough outline of part of this fringe is shown on the Plate, IV. fig. 4. On the same specimen a good many annulate pointed objects occur, two of which are represented at *a* and *b* fig. 2.

There can be no doubt that both tube inhabiting and errant Annelida existed in palæozoic time—traces of some being found even in the Laurentian rocks—though little is known of their true nature and relations. The genera *conchiolites*, *cornulites*, *serpulites*, *spirorbis*, and *trachyderma*, may be said to complete the list of the former, and the genera *arenicolites*, *crossopodia*, *myrianites*, and *nereites* that of the latter. But even some of these generic designations are open to criticism, or cumbered with doubt, and it is questionable if, in any of these cases, we have a true representation of the animal itself. Such considerations add to the value of the specimen now under notice. It has no resemblance to any of the forms just named. The first examples on record having some

* "Ichnology of Massachusetts." By Edward Hitchcock, Boston, 1858. "Supplement to the Ichnology of New England." Edited by C. H. Hitchcock, Boston, 1865.

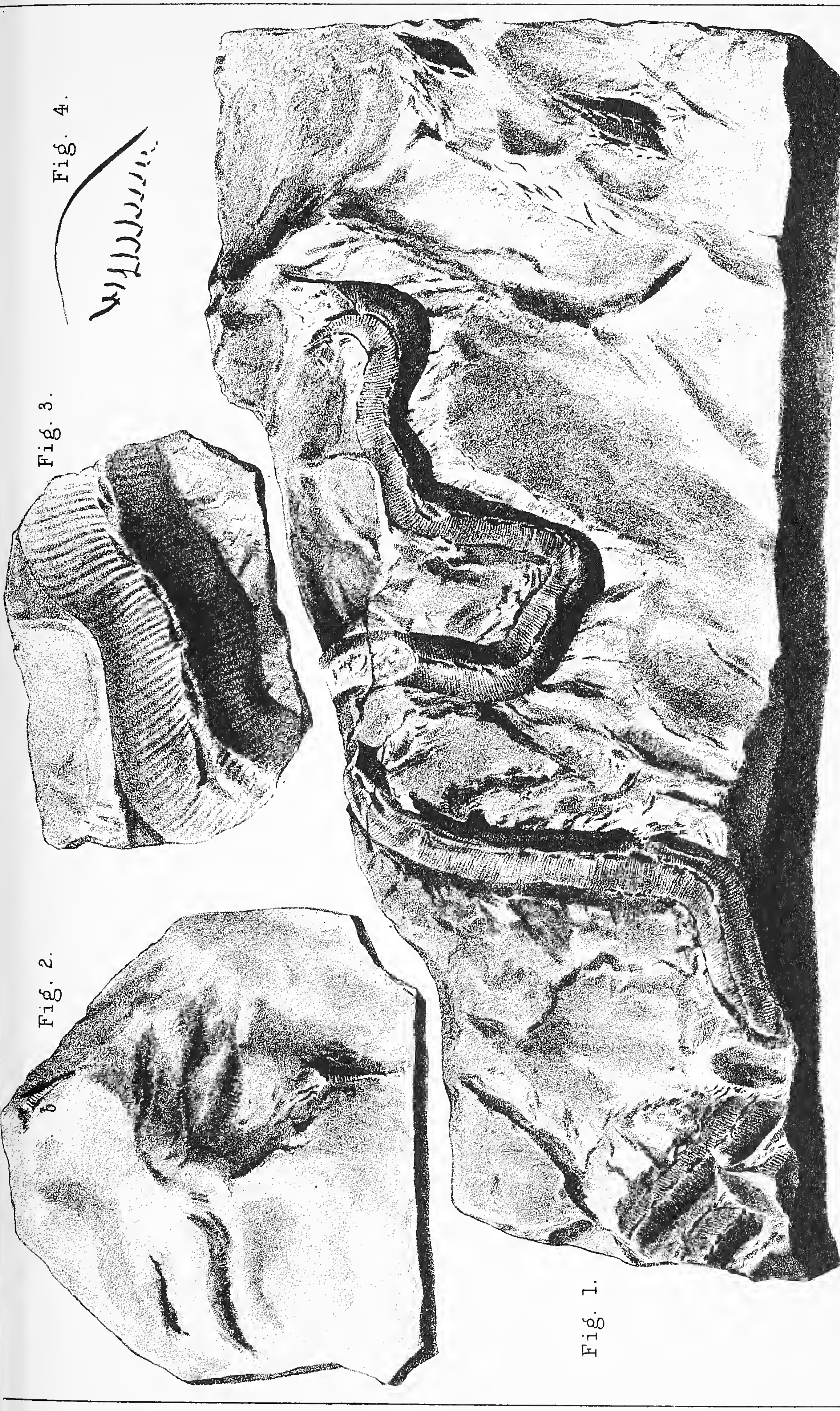


Fig. 1. Fig. 2. Fig. 3. Fig. 4.

B. N. Peach, delt.

M^{c}Farlane & Erskine, Lith^{rs} Edin^{r}

likeness to it were discovered by Mr Dixon, of Unthank,* Northumberland, in fine-grained micaceus carboniferous slabs, in 1838, and sent by him to the Newcastle Museum. In 1844, Dr Emmons,† of Albany, U.S., published an account of annelids found by him in Lower Silurian strata, but none of his figures bear the least resemblance to this. An account is given in the first volume of "The Naturalist" of corresponding markings obtained by Mr Ed. Wood, in 1850, from the Northumberland strata, specimens of which were sent to the Museum, Jermyn Street, which Edward Forbes marked "Casts of Annelid Tracks." M'Coy, who gives in his "British Palæontology"‡ careful descriptions of the genera already named, does seem to have had his attention turned to these. In 1858, Mr Albany Hancock contributed an able paper to the Annals of Natural History, entitled "Remarks on certain Vermiform Fossils found in the Mountain Limestone Districts of the North of England."§ This paper is illustrated by six well-executed plates, the last two of which (xviii. and xix.) bear on the present inquiry. The other figures are undoubtedly mere tracks, and Mr Hancock believes them to be those of crustaceans, but the grounds for this belief are far from clear or satisfactory. Referring to a species of Amphiopoda—*Sulcator arenarius*—he says,—"While forming its track, the animal is never seen; it moves along a little below the surface of the sand, which it pushes upwards with its back, and the arch or tunnel thus formed partially subsides as the creature passes forward, and breaking along the centre the median groove is produced." Now all that this observation shows is, that a median groove is formed in the line of this crustacean's track, though the probability of the realisation of a form like this in such a tunnel cannot be admitted. It would imply that the tunnel

* Since this paper was written I have received from a Fellow of the Society, P. Dudgeon, Esq. of Cargen, several annulose specimens, one of which, from Upper Carboniferous strata, Haltwhistle, Northumberland, not far from this locality, bears a close resemblance to the forms figured by Mr Hancock, and referred to below.

† "The Taconic System." By Eb. Emmons, Abany, 1844.

‡ "Contributions to British Palæontology." By Frederick M'Coy, F.G.S., Cambridge: Macmillan & Co., 1854.

§ Annals and Magazine of Natural History, vol. ii. December 1858. (Plates xviii. and xix.)

in wet sand was kept open till, by gentle infiltration, the hollow was filled, and an exact likeness of the interior of the tunnel realised on the infiltrated matter! A very good example of the forms figured by Mr Hancock occurs on one of the slabs on the table.

At a meeting of the Dublin Geological Society in 1858, Professor Haughton, the president, read a paper "On the Occurrence of some New and Rare Forms of Annelidoid Tracks in the Coal Measures, Lugacurren, Queen's County."* A year later, and again in 1860, he returned to the subject, indicating his belief that the Lugacurren tracks resembled those described by Mr Hancock, and ultimately accepting his theory of their crustacean origin. As Professor Haughton's papers were illustrated by lithographic plates *ad naturam*, they, like Mr Hancock's, are available for comparison with the specimen before us. The Lugacurren specimens differ from the Northumberland forms in having at one part four, in one case, and in another five circular depressions in the median line. They differ from that on the table in having circular depressions confined to a small part of the form, and a median groove, instead of, as here, oval depressions running the whole length of the body, and a median ridge. In neither the English nor the Irish specimens is the characteristic striation so conspicuous as in this. These, however, seem all to be specimens of closely related species. Both Mr Hancock and Professor Haughton think that the depressions in the median groove may have been made by the pygidium of a carboniferous trilobite, the print of the tail being the only trace the animal has left of its having had a place in this deposit! Now such marks as those on the Lugacurren specimens could only have been made by the pygidium being set down vertically, then lifted for a time, and so placed at regular intervals, their being no evidence of dragging,—most unlikely, if not impossible conditions. Fortunately, a good representation of the form figured by Professor Haughton occurs also on these slabs. From this it is evident that the characteristic circular punctures run the whole length of the body.

It seems to me that the reference to the track of *Sulcator arenarius* has been misleading. It can, indeed, have no bearing on any

* Journal of the Geological Society of Dublin, vol. viii. 1857–1860.

of these closely related forms, being unisulcus, destitute of striæ, and not more than three-eighths of an inch wide, while all these are striated and seven-eighths of an inch wide; that under notice being moreover distinctly bisulcate. It is acknowledged that the relations of this to recent forms are obscure. The features which chiefly claim attention are—

First, The outline of the animal.—A glance at the specimen is sufficient to convince us that we have here not a track merely, but the representation of an annulose form. (See Plate IV. fig. 1.) On one of the slabs this is associated with several inches of the track over which it has passed. The median dorsal line is fully exposed above, while the median ridge, which makes the track bisulcate, is precisely what would be formed by the ventral groove of a nereis—*Alitta virens* (Sars), for example. The striæ which pass round the body leave no traces of their outline in this track; but in another, from which the representation of the animal was removed, these striæ are well marked. Again, on the bulged sides of the tortuous outline, the striæ are wider than on the opposite side, while in the comparatively straight parts they are symmetrical. So far as I know, similar markings do not occur on any recent annelid. They are, however, represented, though not so distinctly as here, on a small annelid—*Epitrachys rugosis*—figured by Ehler of Erlangen in his paper on the "Fossil Worms of the Lithographic States of Bavaria."*

Second, The tracks.—They differ widely from the tracks both of *mollusca* and *crustacea*—those of the former being, for the most part, sharp in their turns, and those of the latter consist generally of lines more or less straight, not tortuous. In addition, they have two furrows divided by a distinct median ridge. I am sure that had the able observers named above seen such specimens of the tracks, and also of the *intaglios* of the rounded dorsal surface as are now on the table, they would not have questioned the true annulose character of this organism.

Third, The median dorsal line.—This is exceedingly well represented, not only on the outline of the animal itself, but also in one of the intaglios referred to. It consists of small, shallow, oval

* Ueber fossile Würmer aus dem lithographischen Schiefer in Bayern Cassel, 1869.

depressions, deepest in the centre, and narrowing at each end, where they meet a slight ridge which stretches between the depressions, giving to the line, looked at from a short distance, a chain-like appearance. Were the branchial tufts of some recent annelids plucked out, we would have a somewhat similar appearance.

Fourth, The characteristic striation.—This is most distinctly and even sharply marked on the form in the gritstone slab. It is also, though less definitely, marked on some of the softer micaceous slabs. Mr Hancock says, with reference to his specimen which has most resemblance to this—"The transverse striæ on the surface of the grooved form certainly gives it much the appearance of some organism;" but the value of this acknowledgment is lost by the supposition that the striæ might have been "produced by the intermitting progress of the animal." Now it is simply impossible that such striation as is seen here could have been produced in this manner.

The expression "*ondeleusement et symmétriquement*," used by Cuvier in describing the striæ on the shell of a cephalopode, very well indicates a leading feature of this striation. Indeed, the symmetry of these beautifully regular undulating striæ may be best understood by comparing them with the striæ on the shells both of recent and fossil nautilidæ. Fig 3 is intended for a representation, *ad naturam*, of the characteristic striation, but the striæ are sharper and better defined than shown on the figure.

In conclusion, it will be seen that the distinctive features of the specimen now brought under the notice of the Society are the median dorsal line and the waved striation. In the generic features set down at the head of this paper, I have described the former thus,—linea dorsualis continua, alternasque cavaturas ovatas et vertices ortendens. As, however, uncertainty attaches to the nature of this line, the latter—the striation—may be taken as the outstanding generic feature,—striæ tenues, in ordinem undulatæ, et ubique corpus cingentes, ita ut cutis subrugosa videatur. The term Cymaderma, or wave-skin, is proposed for a genus, whose true zoological position is as yet uncertain. Should the examination of other specimens show that the oval depressions in the median dorsal line are only specific marks, not points of insertion of organs, and the striæ mere lines formed by the contraction of the *cutis*—a most unlikely circumstance—the organism would have closer nemertean than annelid relations. But, if proof be ultimately obtained that the branchiæ-like organs referred to above were connected with the oval depressions, and that the transverse markings are really not striæ but annuli, the zoological position of the animal will be among true annelids, characterised, however, by structural features widely divergent from recent forms.

5. On Eisenstein's Continued Fraction. By Thomas Muir, M.A.

6. Note on an Infinitude of Operations. By Thomas Muir, M.A.

The assumption that there is a limit in Professor Tait's problem regarding the interpretation of $\text{Lim}_{n=\infty}(\cos^n x)$ means that if we start with an angle x, and find the number which is its cosine, then the number which is the cosine of this number, and so on, we shall at last come to a limiting result ω, such that $\cos\omega = \omega$. The problem is thus transformed into the solution of the equation $\omega = \cos\omega$, which may be accomplished as follows:—

$$\omega = \cos\omega,$$

$$\therefore \quad \omega > 1 - \frac{\omega}{2},$$

whence

$$\omega > \sqrt{3} - 1,$$

$$> \cdot 73205 \ldots .$$

Taking therefore ·733 as a first approximation, we find that with it

$$\omega - \cos\omega = -\cdot 01$$

and taking ·75 or $\frac{3}{4}$ as another approximation, which is readily seen to differ from the former by erring in excess, we find that with it

$$\omega - \omega = \cos + \cdot 019 \ldots .$$

from which two results by the regula falsi we derive a better approximation, viz.,

$$\cdot 733 - \frac{\cdot 75 - \cdot 733}{\cdot 019 + \cdot 01} \times \cdot 01$$

i.e., $\cdot 7388 \ldots .$

Continuing in this way, it is found that

$$\mathrm{Lim}_{n=\infty}(\cos_n x) = \cdot 7390852,$$

a result correct to at least the fifth place.

This peculiar constant we should expect to find connected in some way with π, and that such a connection exists is easily seen from the formula—

$$\cos x = \left(1 - \frac{4x^2}{\pi^2}\right)\left(1 - \frac{4x^2}{3^2\pi^2}\right)\left(1 - \frac{4x^2}{5^2\pi^2}\right) \ldots .$$

from which we have

$$\omega = \left(1 - \frac{4\omega^2}{\pi^2}\right)\left(1 - \frac{4\omega^2}{3^2\pi^2}\right)\left(1 - \frac{4\omega^2}{5^2\pi^2}\right) \ldots .$$

An *explicit* expression of the same relation is obtained from the identity

$$\cos^{-1} x = \frac{\pi}{2} - x - \frac{1}{2} \cdot \frac{x^3}{3} - \frac{1 \cdot 3}{2 . 4} \frac{x^5}{5} - \ldots .$$

which gives

$$\omega = \frac{\pi}{2} - \omega - \frac{1}{2} \cdot \frac{\omega^3}{3} - \frac{1 \cdot 3}{2 \cdot 4} \cdot \frac{\omega^5}{5} - \ldots .$$

whence $$\pi = 4\omega + \frac{\omega^3}{3} + \frac{3}{4} \cdot \frac{\omega^5}{5} + \ldots .$$

the result of the "reversion" of which is

$$\cos^{\infty} x = \frac{\pi}{4} - \frac{1}{3 . 4}\left(\frac{\pi}{4}\right)^3 - \frac{1}{.3 . 4 . 5}\left(\frac{\pi}{4}\right)^5 - \ldots .$$

The corresponding limit in the case of other functions may be similarly interpreted; that is to say, the limit of $\phi^n(x)$, if such be possible, when n is indefinitely increased, is a root of the equation $\phi(x)=x$. We may view such limits as implying an *infinitude of operations;* and we have seen that when this infinitude of operations is carried out upon any value (within certain limits) of the independent variable, the result is always the same; in other words, we have seen that those functions, in the case of which the limit is possible, are *levelling* functions, having the property of bringing everything they act on to the same dead level. In this respect they may be compared with Euler's expression

$$\frac{1}{2}x + \sin x + \frac{1}{2}\sin 2x + \frac{1}{3}\sin 3x + \ldots.$$

which for all values of x within certain limits equals $\frac{\pi}{2}$; and this suggests the possibility of finding a similar expression for $\cos^{\infty}x$ by means of Lagrange's or Fourier's theorem.

7. Note on Determinant Expressions for the Sum of a Harmonical Progression. By Thomas Muir, M.A.

(Received February 27—Read March 1877.)

Taking the harmonical progression

$$\frac{1}{a}+\frac{1}{a+b}+\frac{1}{a+2b}+\frac{1}{a+3b}+\ldots.$$

and denoting the sum of n terms of it by S_n we have by means of Euler's transformation,

$$S_n=\cfrac{1}{a-\cfrac{a^2}{2a+b-\cfrac{(a+b)^2}{2a+3b-\cfrac{(a+2b)^2}{2a+5b-\cdots-\cfrac{\{a+(n-2)b\}^2}{2a+(2n-3)b}}}}}$$

and therefore from the theory of continuants,

$$S^n = \frac{\begin{vmatrix} 2a+b & (a+b)^2 & 0 & 0 & \cdots \\ 1 & 2a+3b & (a+2b)^2 & 0 & \\ 0 & 1 & 2a+5b & (a+3b)^2 & \cdots \\ 0 & 0 & 1 & 2a+7b & \cdots \\ 0 & 0 & 0 & 1 & \cdots \\ \cdots & \cdots & \cdots & \cdots & \cdots \end{vmatrix}}{\begin{vmatrix} a & a^2 & 0 & 0 & \cdots \\ 1 & 2a+b & (a+b)^2 & 0 & \\ 0 & 1 & 2a+3b & (a+2b)^2 & \cdots \\ 0 & 0 & 1 & 2a+5b & \cdots \\ 0 & 0 & 0 & 1 & \cdots \\ \cdots & \cdots & \cdots & \cdots & \cdots \end{vmatrix}}$$

where the denominator is of the n^{th} order and the numerator of the $(n-1)^{th}$, being formed from the denominator by the omission of the first row and first column.

There is, however, another such expression for S_n of more interest and less likely to occur to one, viz.,

$$\frac{\begin{vmatrix} 1 & -b & -b & -b & -b & \cdots\cdots & -b \\ 1 & na & -2b & -3b & -4b & \cdots\cdots & -(n-1)b \\ 1 & (n-1)a & a & -2b & -3b & \cdots\cdots & -(n-1)b \\ 1 & (n-2)a & a & a & -2b & \cdots\cdots & -(n-1)b \\ 1 & (n-3)a & a & a & a & \cdots\cdots & -(n-1)b \\ \cdots & \cdots & \cdots & \cdots & \cdots & \cdots & \cdots \\ 1 & 2a & a & a & a & \cdots\cdots & a \end{vmatrix}}{a(a+b)(a+2b)\cdots\cdots\{a+(n-1)b\}}$$

This is easily verified for the cases where $n=2$ and $n=3$. When $n=4$ we have

$$a(a+b)(a+2b)(a+3b)S_4 = \begin{vmatrix} 1 & -b & -b & -b \\ 1 & 4a & -2b & -3b \\ 1 & 3a & a & -3b \\ 1 & 2a & a & a \end{vmatrix} = \begin{vmatrix} 1 & -b & -b & 2b \\ 1 & 4a & -2b & 0 \\ 1 & 3a & a & 0 \\ 1 & 2a & a & a+3b \end{vmatrix}$$

$$= (a+3b)\begin{vmatrix} 1 & -b & -b \\ 1 & 4a & -2b \\ 1 & 3a & a \end{vmatrix} - 2b\begin{vmatrix} 1 & 4a-2b \\ 1 & 3a & a \\ 1 & 2a & a \end{vmatrix}$$

$$= (a+3b,\begin{vmatrix} 1 & -b & -b \\ 1 & 3a & -2b \\ 1 & 2a & a \end{vmatrix} + (a+3b)\begin{vmatrix} 1 & 0 & -b \\ 1 & a & -2b \\ 1 & a & a \end{vmatrix} - 2b\begin{vmatrix} 1 & 4a-2b \\ 1 & 3a & a \\ 1 & 2a & a \end{vmatrix}$$

$$= (a+3b,\begin{vmatrix} 1 & -b & -b \\ 1 & 3a & - \\ 1 & 2a & a \end{vmatrix} + (a+b)a(a+2b)\,,$$

for the last two determinants in the preceding line are each equal to $a(a+2b)$. Thus we have

$$S_4 = \frac{\begin{vmatrix} 1 & -b & -b \\ 1 & 3a & -2b \\ 1 & 2a & a \end{vmatrix}}{a(a+b)(a+2b)} + \frac{1}{a+3b}$$

$$= S_3 + \frac{1}{a+3b}$$

as it should be. And this method of showing that the validity of the fourth case is dependent on that of the third is applicable in other case.

8. Sevenfold Knottiness. By Prof. Tait.

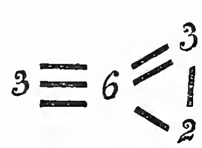
(Abstract.)

From the point of view of the Hypothesis of Vortex Atoms, it becomes a question of great importance to find how many distinct forms there are of knots with a given amount of knottiness. The enormous numbers of lines in the spectra of certain elementary substances show that the form of the corresponding Vortex Atoms cannot be regarded as very simple. But this is no objection against, it is rather an argument in favour of the truth of, the Hypothesis.

For not only are the great majority of possible knots not stable forms for vortices; but altogether independently of the question of kinetic stability, the number of distinct forms with each degree of knottiness is exceedingly small,—very much smaller than I was prepared to find it. I have already stated that for three, four, five, and sixfold knottiness, the numbers are only 1, 1, 2, 4. For a reason given in my first paper, knots whose number of crossings is a multiple of 6 form an exceptional class: so I thought it might be useful to discover and to figure all the distinct forms with seven-fold knottiness. Eight and higher numbers are not likely to be attacked by a rigorous process until the methods are immensely simplified. The method of partitions, supplemented by the graphic formulæ of my last paper, is to some extent tentative. I have verified the present results by means of it, and have extended it to 8-fold knottiness, but I am not certain that I have got *all* the possible forms of the latter.

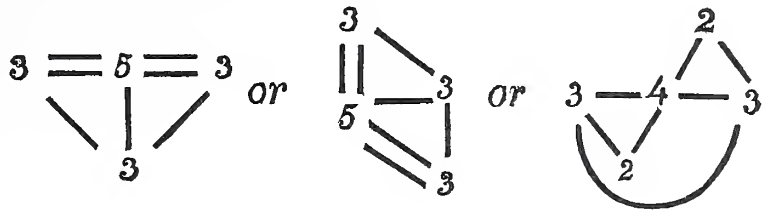
As I did not see how to abridge the process, I wrote out all the admissible permutations of the seven letters in the even places of the scheme. These I found to be 579, five of which were, of course, unique. The others (as 7 is a prime number) were divisible into 82 groups—those of each group being mutually equivalent. On examination, it was found that only 22 of the 87 selected arrangements satisfied the criterion for possible knots (see I § (*b*) of my paper, *ante* p. 238), and several even of these were repetitions. These repetitions were of two kinds—1st, the mere inversion of the order of the scheme; 2d, the relative positions of a 3-fold and a 4-fold knot which in certain cases were found combined as a 7-fold form. Clearing off these repetitions, and along with them a form really belonging to 6-fold knots (because consisting of two trefoil knots and one nugatory intersection), there remain only *eleven* distinct forms of the 7th order. These are as follows:—

1.



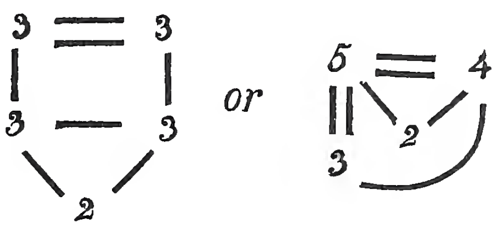
This has a great many forms, with correspondingly different symbols, being a mere compound of a 3-fold and a 4-fold knot, which may have *any* relative positions on the string.

2.

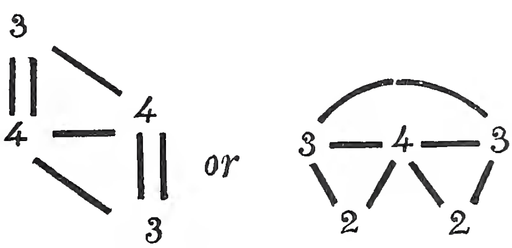


This is one of Listing's knots.

3.

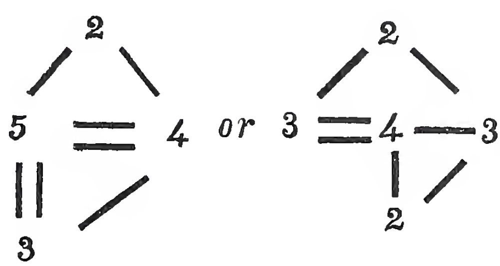


4.



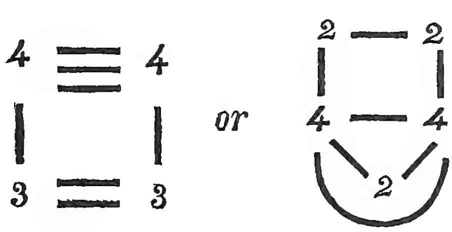
Listing has shown that this is deformable into 2 above.

5.

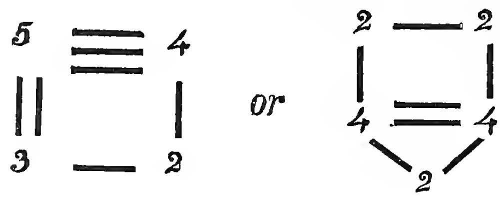


I find that this can be deformed into 3 above. It is figured in my paper on *Links*, *ante*, p. 325, first woodcut.

6.

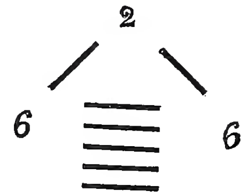


7.



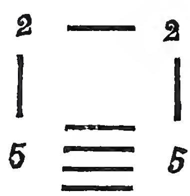
This can be deformed into 6 above

8.



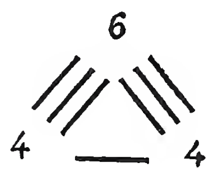
This species of knot occurs for *all* numbers of intersections greater than 2.

9.



This is the 7 knot which Listing does not sketch. *Ante*, p. 311.

10.



11.

This is the simple twist, which occurs for every *odd* number of intersections.

As 2 and 4, 3 and 5, 6 and 7 are capable of being deformed into one another, three of them are not independent forms, and thus the number of distinct forms of seven-fold knots is only *eight*.

Drawings of various forms of each of these knots were given, as well as indications of the modes in which they can be formed from knottinesses of lower orders.

Monday, 2d April 1877.

Sir WILLIAM THOMSON, President, in the Chair.

The following Communications were read :—

1. Professor Geikie exhibited a large Map showing the progress of the Geological Survey of Scotland.

2. Notice of a Saline Water from the Volcanic Rocks of Linlithgow. By Professor Geikie, F.R.S.

From a boring which has recently been made about a mile west from the town of Linlithgow, water has been obtained differing so much in character from that of the usual wells and springs of the district that some notice of it deserves to be placed on record. When the fact was communicated to me I was asked to explain by what means sea-water could obtain access to underground rocks in an inland district. On visiting the ground I found the site of the bore to be among some hollows of the gravel and sand which cover the country between Falkirk and Linlithgow, its height being about 165 feet above the sea, from which it was distant about three miles. The ordinary wells of the district are situated in the superficial deposits, and supply good potable water, though the supply is necessarily limited. A more copious flow being desired the bore was sunk through the sands, gravels, and clays (here rather more than 100 feet thick), and then entered upon a succession of green, brown, blue, and red "whinstone." After a depth of 317 feet had been passed through, consisting entirely of these alternations of "whinstone," a sample of the tolerably copious supply of water which had now appeared was drawn up and sent for analysis to Dr Stevenson Macadam, whose results were as follow :—

Chloride of sodium,	118·76	grains per	imperial gallon.
Sulphate of lime,	7·94	„	„
Chloride of calcium,	6·78	„	„
Chloride of magnesium,	7·83	„	„
Chloride of potassium,	1·46	„	„

Carbonate of lime,	20·23	grains per	imperial gallon.
Carbonate of magnesia,	3·48	,,	,,
Oxide of iron,	0·34	,,	,,
Phosphates,	0·71	,,	,,
Soluble silica,	0·64	,,	,,
Organic and volatile matters,	2·37	,,	,,
	170·54		

This large admixture of saline ingredients rendered the boring unavailable for the increase of the water-supply; but the boring-rods were driven 30 feet further in the hope that possibly this mineral water might find some other subterranean means of escape. The hope was of course disappointed, and the operation terminated at a depth of 451 feet from the surface, the lower 348 feet of the boring having been found to pass wholly through "whinstone" in numerous bands of varying hardness and colour. Two samples of the water, taken when the ultimate depth had been reached, were submitted for analysis to different chemists, and gave nearly similar results. The proportions of salts obtained by Mr Robert M'Alley, Falkirk, were the following:—

Carbonate of lime,	7	grains per	gallon.
Carbonate of magnesia,	·60	,,	,,
Sulphate of lime,	·92	,,	,,
Chloride of calcium,	1·22	,,	,,
Common salt,	134·76	,,	,,
Alumina,	·20	,,	,
Siliceous matter,	1·20	,,	,,
Volatile and organic matter,	1·60	,,	,,
	147·50		

One character of the water not noticed in the analyses, but distinctly perceptible to me in a freshly drawn sample, was the odour of sulphuretted hydrogen. I may add that the bore was begun from the bottom of a previously made well 18 feet from the surface, and that I found the water flowing out abundantly from the top of the bore-tube into the well from which it was temporarily pumped away.

It was evident that the idea of any subterranean communication with the sea was quite inadmissible. In the first place, the locality is three miles from the nearest part of the sea-coast, with a ridge of high ground intervening. In the second place, between it and the sea-margin lie the numerous deep pits of the Borrowstounness and Kinneil coal-fields, which, it may be supposed, must prevent at least any superficial water-communication. And in the third place, the proportions of the various salts contained in the water are quite different from those which would have resulted from the mere access of ordinary sea-water. The salts can only have been derived from the subterranean rocks traversed by the water.

An acquaintance with the geology of the district enables me to recognise the various kinds of "whinstone" found in the bore. They are successive beds of the dull green and brown, usually more or less decomposed, dolerites (diabases) forming the long volcanic range between Bathgate and the sea, and which were poured out partly as submarine and partly as subaërial lavas during the deposition of the Carboniferous Limestone, or Lower Coal series of Linlithgowshire. Occasional bands of green and red tuff mark intervals between the lava-flows. Since the water from the superficial gravels was of the usual potable quality there can be no doubt that the saline impregnation comes from the underlying rocks.

So far as I am aware, the first detection of soda as a chemical constituent of rocks was made by Dr Robert Kennedy, and announced to this Society as far back as December 1798. He analysed the specimens of whinstone employed by Sir James Hall in his classical experiments upon the fusibility of whinstone and lava, and found the constant presence of soda to the extent of four or five per cent., with one per cent. of muriatic acid. He further examined pieces of sandstone from the neighbourhood of Edinburgh and elsewhere, with the invariable result of detecting an appreciable quantity of common salt in them. Since the early date of his researches chloride of sodium has been found very widely diffused among the minerals and rocks of the earth's crust.

There seems to be three chief sources from which rocks derive their chloride of sodium:—1. Rain; 2. The evaporated water of old salt lakes and inland seas; 3. Volcanic sublimations.

1. *Rain.*—The researches of Dr Angus Smith * into the compo-

* See his *Air and Rain.*

sition of rain in different parts of the country have shown the very general prevalence of chlorides, and particularly of chloride of sodium, in the air. As might be expected, the proportion of chlorides is greatest nearest to the sea, though abnormally large quantities are found in the air of manufacturing towns as the result of the combustion of coal, &c. On the west side of Britain the proportion of hydrochloric acid in rain was found sometimes to amount to nearly four grains per gallon, or 56 parts in a million. On the east side of Scotland the proportion sinks to ·9 grain per gallon, or 12 parts in the million. There can be little doubt that most of this is chloride of sodium. Dr Smith has pointed out the curious fact that this salt cannot be conveyed into the atmosphere merely in spray driven from the surface of the sea by high winds, for if that were the case the composition of the rain should be approximately like that of sea-water. But the saline ingredients do not occur at all in similar proportions. It seems reasonable to suppose that the superficial parts of rocks liable to be saturated with rain-water must thereby receive an appreciable amount of chloride of sodium. In this case it is evident that much care should be exercised in procuring for analysis portions of rock which lie beyond the reach of this surface saturation. Possibly in some of the instances cited by Kennedy, in the paper already referred to, the common salt may have been introduced by the action of rain.

2. *The Deposit of Salts on the Floor of Old Lakes and Inland Seas.*—This mode of origin is doubtless by much the most important source of the chloride of sodium in rocks. When we consider the large proportion of marine strata in the stratified part of the earth's crust it is surprising, as De la Beche remarked long ago,* that saline waters are not more abundant than they really are. Dr Sterry Hunt has pointed to the mineral waters of Canada and the North-Eastern States as probably deriving their salts from the original sea-water of palæozoic times still imprisoned within the pores of the rocks.† I need not refer to the abundant deposits of rock-salt and gypsum, as well as of saliferous and gypsiferous clays, which occur in so many districts of the world.

3. *Volcanic Sublimations.*—The occurrence of incrustations and

* Researches in Theoretical Geology.

† "Essays in Chemical Geology, 1875."

stalactites of common salt upon the walls and slopes of a volcanic crater after eruption, and the appearance of the same substance upon the surface or within the chinks of recently-consolidated lava streams, have long been observed. On Vesuvius vast quantities of salt have been gathered again and again, and have been used by the inhabitants at the foot of the mountain. Among the Icelandic volcanoes the same fact has occurred. But besides this outward and conspicuous manifestation, chemical analysis has revealed the presence of chloride of sodium in many volcanic rocks, either diffused through their crystalline matrix or entangled within their individual constituent minerals. In such volcanic minerals as Hauyne, Nosean and Sodalite it has been found to be noticeably present. It has been extracted even by pure water from clinkstones, basalts, and from various plutonic rocks.

There can be little doubt that it is to this volcanic origin that the large admixture of salt in the Linlithgow water is to be traced. The bore may have reached a stratum of ancient lava or tuff, originally permeated or incrusted with salt, which may have been dissolved by percolating water, and yet have remained in solution below for want of any ready means by which the water could escape to the surface. This escape is now provided by the bore, and the water is consequently now removing the salt from the rock.

It may be mentioned that some of the best known saline waters of Scotland rise from volcanic rocks. At the Bridge of Allan, water containing 95 grains of chloride of sodium in the gallon takes its rise from near the top of the enormous mass of lavas and tuffs erupted during the Lower Old Red Sandstone period, and forming now the chain of the Ochil Hills. At Pitcaithley, Bridge of Earn, water containing 114 grains of the same salt in the gallon rises from the same volcanic band, while at Dunblane, water with 48 grains of common salt in the gallon comes through the lower parts of the red sandstones of that district, which lie upon the same great volcanic series. In most of these cases the rise of the saline water appears to have been determined by an accident, such as the sinking of a bore for water, or, as at Bridge of Allan, during the search for minerals.

3. On the Arrangement and Relations of the Great Nerve-Cords in the Marine Annelids. By W. C. M'Intosh, M.D., F.R.S.E., F.L.S.

(*Abstract.*)

Fam. Euphrosynidæ.—In *Euphrosyne foliosa*, Aud. and Ed., the separate nerve-cords are comparatively large, and lie quite within the body-wall, the oblique muscles, which generally bound the longitudinal ventral muscles, decussating beneath them.

Amphinomidæ.—The cords are somewhat small and flattened in *Chloeia*, and occupy an area bounded internally by a transverse band of fibres, and externally by the circular muscular layer and the hypodermic basement-tissue. The oblique muscles are attached at the outer border of each trunk.

Aphroditidæ.—The nerve-cords in *Aphrodita aculeata*, L., occur in a transversely elongated space between the ventral attachments of the oblique muscles, and bounded externally by the hypodermic basement-tissue and the cuticle. In *Hermione hystrix*, Sav., again, they lie—as distinct trunks—in a well-defined hypodermic area within the dense cuticle, and separated by an interval from the attachment of the oblique muscle on each side.

Polynoidæ.—In *Lepidonotus squamatus*, L., the cords occupy a hypodermic area between the ventral longitudinal muscles. The oblique muscles pierce the vertical at the upper and outer angle of the space, and are attached to the hypodermic basement-tissue, external and superior to the cords. A belt of hypoderm intervenes between the latter and the cuticle. In *Polynoë scolopendrina*, Sav., the interval between the ventral attachments of the oblique muscles is less, and the cords are bounded superiorly by a special longitudinal muscle.

Acœtidæ.—In *Panthalis Œrstedi*, Kbg., the trunks are situated in the hypodermic region between the ventral longitudinal muscles, a thin layer of the former tissue occurring between them and the cuticle. The great oblique muscles pass down to their upper and outer border. The space between the ventral longitudinal muscles is less than in the Polynoidæ.

Sigalionidæ.—The space between the ventral longitudinal muscles anteriorly in *Sthenelais boa*, Johnst., is still more narrowed

than in *Panthalis*, and the hypodermic area for the nerve is thus increased in depth. Superiorly the arch is completely covered by the insertions of the vertical and oblique muscles; and it is interesting that the latter do not pierce the former (which occupy the middle line), but are attached to the basement-tissue below them, on each side of the nerve-area. The cords are almond-shaped in transverse section, whereas in *S. Mathildæ*, Aud. and Ed., they are round, and the hypodermic area enclosing them is much more expanded inferiorly.

Nephthydidæ.—In *Nephthys cœca*, Fabr., the combined oblique and vertical are attached along the entire arch of basement-tissue, above the nerve-area. A broad hypodermic belt exists above the nerve trunks, and a narrower between them and the cuticle.

Phyllodocidæ.—The nerves in *Phyllodoce grœnlandica*, Œrst., are situated within the circular muscular coat, and above the insertions of the oblique muscles (which decussate in the middle line), as well as such fibres of the vertical muscles as are inserted into the basement-tissue of the ventral hypoderm. In *Eteone picta*, De Quatref., and *Eulalia viridis*, O. F. Müller, certain fibres of the oblique pass at intervals right over the cords, so as to form a continuous band from side to side, and in the former an interval between the oblique muscles is indicated.

In *Alciope* the cords also lie within the circular muscular layer—in the interval between the longitudinal ventral. The oblique pass below the cords, but do not appear to meet in the middle line. The sole specimen, however, is indifferently preserved for microscopic work.

Hesionidæ.—The trunks in *Ophiodromus vittatus*, Sars., have in the anterior region passed below the ventral attachment of the oblique muscles to the basement-tissue. A thin stratum of longitudinal fibres occurs superiorly, while externally is a thickened hypoderm. The nerves seem to be proportionally large.

Syllidæ.—In *Syllis armillaris*, O. F. Müller, the nerve-cords are also comparatively large. A considerable depth of the closely approximated ventral longitudinal muscles shuts them from the very thin hypodermic elements within the thickened cuticle, except in the intervals between the ganglia, where there is a slender pedicle. The oblique muscles bend below the cords to be attached to the raphe in the same intervals. Fibres from the walls of the alimentary canal also descend to the raphe on each side of the trunks.

NEREIDÆ.—In *Nereis pelagica*, L., the nerve-cords lie rather above the attachments of the oblique muscles to the hypodermic basement-tissue, the area being continued to the hypoderm by a central pedicle. *Nereis* (*Alitta*) *virens*, Sars., an epitocous form, likewise has the oblique muscles attached on each side of the pedicle of the nerve-area, while the vertical are inserted in a chitinous arch at the upper and lateral regions. Two well-marked longitudinal muscles lie over the cords. Several neural canals exist,—viz., two large infero-lateral, a single superior median, and a smaller, a little below the latter, on each side. In *Nereis diversicolor*, O. F. Müller, each cord has a neural canal of considerable size towards its inferior border.

STAUROCEPHALIDÆ.—The cords are large in *Staurocephalus rubrovittatus*, Grube, and occur in the somewhat wide interval between the ventral attachments of the oblique muscles. Externally are the basement-tissue, hypoderm, and cuticle.

LUMBRINEREIDÆ.—In a *Lumbriconereis* from Herm, a large neural canal exists above the cords, which are carried inward by the approximation of the great longitudinal ventral muscles. The oblique meet over the neural canal. Externally are basement-tissue, hypoderm, and cuticle. The nerves are pressed further inward in *Notocirrus tricolor*, Johnst., the oblique muscles being attached to the summit of the nerve-area (laterally), outside the fibres forming the special chamber for the vessel. Some fibres pass down each side, and cross below the nerve-area.

The neural canal in the posterior third of *Lysidice ninetta*, Aud. and Ed., is situated toward the ventral border of the ganglia, but between the cords in the intervals. The oblique muscles pass below the cords. The latter takes place likewise in *Palolo viridis*, Gray.

EUNICIDÆ.—The cords in *Marphysa sanguinea*, Mont., lie between the greatly developed longitudinal ventral muscles, and present the aspect of a margin to the large median neural canal. The fibres of the oblique muscles are attached to the upper and outer border of the region,—a few fibres passing downward to curve outward at the circular coat of the body-wall. Externally, between the latter coat (and a layer of somewhat isolated longitudinal fibres within it), are the hypoderm and cuticle. In *Eunice norvegica*, L., a similar arrangement occurs in the anterior third, and a large median neural canal lies below the cords. The strong oblique muscles pass down to the ventral hypoderm. Distinct muscular bands also enclose the ventral blood-vessel and nerve trunk in a tunnel. A little behind the middle of *Eunice Harassii*, Aud. and Ed., the cords present the same external coverings, only, from the great size of the oblique and vertical muscles, they are somewhat supported by the latter inferiorly. The same arrangement occurs in a large male *Eunice* from the "Porcupine." The constancy of the muscular tunnel for the nerve-cords and ventral blood-vessel is interesting.

ONUPHIDIDÆ.—The oblique muscles in both *Nothria conchylega*, Sars., and *Hyalinœcia tubicola*, O. F. Müller, are well developed, and not only meet, but slightly cross, in the middle line. The nerve-cords lie in the angle of decussation superiorly, and have a single neural canal of considerable size towards the lower border. Besides the oblique are externally the circular muscular coat (which, in *H. tubicola*, is specially developed in the median line), a narrow band of hypoderm, and the dense cuticle.

GONIADIDÆ.—In the anterior region of *Goniada maculata*, Œrst., the powerful oblique muscles sweep from below the bristle-bundles on each side, with a slight inclination downward and inward, and meet for insertion on each side of the hypodermic wedge above the nerves. The latter occupy a somewhat triangular area of the hypoderm, and each has a small neural canal toward the upper and narrow part. In *Eone Nordmanni*, Mgrn., the nerves are proportionally smaller, and the hypodermic area less.

GLYCERIDÆ.—The cords at the anterior third of *Glycera capitata*, Œrst., are large, and occur in a hypodermic region, wedged between the great longitudinal ventral muscles, which touch in the middle line, so as to form an arch over the nerves. The great external circular muscular layer ceases before reaching the nerve-area, so that externally the latter has only the hypoderm and the specially thickened cuticle. The oblique muscles are very slightly developed in this species, and their terminations over the cords can only be seen occasionally. Each cord has a neural canal at its upper third, near the middle line. *Glycera Gœsi*, Mgrn., shows nearly the same arrangements; while *G. setosa*, Œrst., exhibits more evident separation of the cords. The insertions of the oblique muscles over the nerve-area are best seen in *G. alba*, Rathke, a large species, in which the cords are proportionally less.

Ariciidæ.—In the anterior region of *Aricia latreillii*, Aud. and Ed., the cords are situated at the upper part of a somewhat triangular hypodermic area, bounded laterally by that part of the circular coat clasping the ventral longitudinal muscles. Powerful oblique muscles meet for insertion over each cord. In the same region of *Scolopos armiger*, O. F. Müller, the cords also lie beneath the point of union of the greatly developed and nearly horizontal oblique muscles, and supported laterally by the massive edges of the ventral longitudinal. A single neural canal exists superiorly. Between the cords and the hypoderm is the thick circular muscular coat. In the middle of the body the nerves are thrust upward by the great ventral muscles, which are only separated by the narrow pedicle of the area.

Opheliidæ.—Throughout the greater part of *Ammotrypane aulogaster*, H. R., a peculiar modification of the body-wall exists, in the form of a constriction between the dorsal and ventral longitudinal muscles. The intermediate pedicle is formed apparently by the metamorphosed vertical muscles, which have coalesced over the nerve cords. The oblique muscles pass from the outer edge of the ventral longitudinal to the middle line below the nerve-area. A small and indistinct neural canal appears toward the upper part of the latter. In *Ophelia limacina*, H. R., the cords in the smoothly rounded anterior third lie in the middle of the long interval between the ventral longitudinal muscles. The oblique are inserted far outside the cords. Externally are a granular area, a series of transverse fibres, and the cuticle. Toward the posterior part of the body, where the ventral ridges are well-developed, the cords, by an intricate change in the relationships of the muscles, get above the median (ventral) insertion of the vertical, the fibres of which extend outward and somewhat downward into each pedicle. The true oblique is with difficulty seen; but it appears to be a slender muscle passing from the outer border of the ventral longitudinal. In many sections a neural canal is seen somewhat above the middle of the area. A muscle passes from the bristle-tuft upward in a slanting direction through each pedicle to the raphe below the nerve. In *Travisia Forbesii*, Johnst., the nerve-trunks are situated between, and somewhat above, the insertions of the oblique muscle. Externally are the circular coat, a translucent basement-tissue, a granular hypoderm, and a very thin cuticle.

Scalibregmidæ.—In *Eumenia Jeffreysii* the cords in the anterior region occur toward the inner aspect of the thick layer of translucent basement-tissue, below and between the insertions of the two long oblique muscles. Toward the middle of the body the trunks still indent the basement-tissue, the circular muscular coat forming their inner boundary. A series of strong transverse fibres occurs at intervals as an arch over the cords. In *Scalibregma inflatum*, H. R., the cords (in the posterior region) lie below the circular muscular fibres, which occur beneath the commissure of the oblique in the middle line. Externally are the hypoderm and cuticle.

Telethusæ.—In the anterior third of *Arenicola marina*, L., the cords form a comparatively small ovoid mass—clasped by the great longitudinal muscles, and connected with the circular coat by a narrow granular pedicle.

Sphærodoridæ.—While there is a very thick cuticle in *Ephesia gracilis*, H. R., the hypoderm is slightly developed below the nerves. The oblique muscles pass from the inferior border of the bristle-tufts to the outer margin of the nerve-trunks.

Chloræmidæ.—In *Trophonia plumosa*, O. F. Müller, the nerves lie above the median decussation of the oblique muscles, the cords being distinct in the intervals between the ganglia. Externally are also the circular coat, a narrow hypoderm and a roughly papillose cuticle.*

Chætopteridæ.—In *Chætopterus norvegicus*, Sars., the cords are placed wide apart in front, in consonance with the peculiarly modified muscular arrangements of the body-wall. They are hypodermic. By the great increase of the median system of muscles

* Next the Chloræmidæ Dr Malmgren places the Sternaspididæ, but in structure Sternaspis is Gephyrean.

the homologues of the ventral longitudinal are pushed outward. The oblique preserve their usual relations with the latter and the nerve-cords, over which their ventral termination occurs. Another muscle, probably the homologue of the transverse, passes from the termination of the oblique inward to the middle line on each side. As the late M. Claparède has shown, the cords approach each other in the posterior region in *Chœtopterus*, while in *Telepsavus* they remain separate throughout.*

Spionidæ.—As pointed out last session,† the cords in this family are hypodermic in position. Their relation to the ventral insertions of the oblique muscles are also well shown, since they follow the latter in their gradual progress inward from the sides of the body in front until the cords touch and the muscles meet over them. The neural canals are largely developed.

Cirratulidæ.—In *Cirratulus cirratus*, O. F. Müller, the cords in the anterior region lie in the median line ventrally within a thick hypodermic area. The oblique muscles are inserted at the summit of the area, and the sides of the ventral longitudinal muscles overlap its upper arch. The circular muscular coat is continuous over (internal to) the cords. In *Dodecaceria concharum*, Œrst., a thick median mass of blackish hypoderm protects the cords, and the relations of the oblique and longitudinal ventral muscles are similar.

Halelminthidæ.—This family approaches the Lumbricidæ in having the nerve-cords placed within the great and nearly continuous longitudinal muscles of the body-wall. Externally (in the ventral region of *Capitella capitata*, Fabr.) are in addition a dense circular muscular coat, a thin layer of longitudinal fibres, basement-tissue, hypoderm, and cuticle. Anteriorly there are two divisions of the ventral longitudinal muscular fibres under the nerves, but in the middle region of the body they have coalesced into a single mass. A neural canal occurs superiorly in the ovoid nervous area. Two vertical muscles bound those above which the nerves lie, but the oblique are not recognisable.

Maldaidnæ.—In the anterior region of *Praxilla prœtermissa*, Mgrn., the nerve-cords lie beneath (outside) the circular muscular

* Annélides Sédent. p. 127, &c.

† Proceed. R.S.E., vol. ix. No. 94, p. 124, &c.

coat, at the gap between the ventral longitudinal muscles. The area is small and flattened, and has a large neural canal in the centre superiorly. The oblique muscles are attached above the area. A similar arrangement occurs in *Nicomache lumbricalis*, Fabr., and in a large Canadian species of the same genus.

Ammocharidæ.—About a quarter of an inch behind the snout the nerve-area in *Owenia filiformis*, D. Ch., forms an ovoid mass outside the tough basement-tissue bounding the great longitudinal muscular layer. The area is entirely hypodermic, and as very little of this tissue remains in the majority of the preparations, the cords are often bare. The oblique muscles are not visible, and the longitudinal are only separated in the median line dorsally and ventrally.

Hermellidæ.—In *Sabellaria spinulosa*, R. Leuckart, the cords remain quite separate throughout their entire length. Anteriorly each is placed in the substance of the great ventral muscle, near its upper and inner border. No distinct oblique muscles appear in this form. Posteriorly the nerves occupy the same relative position in the diminished muscles, and each has a large neural canal at its inner border.

Amphictenidæ.—In *Cystenides hyperborea*, Mgrn., the united cords in the anterior region occur as an ovoid mass over the transverse muscular fibres in the ventral median line. The large oblique muscles are widely separated from the nerve-trunks.

Ampharetidæ.—In *Amphicteis Gunneri*, from Canada, the nerves (in transverse section of the body-wall) appear as two minute separate bodies enveloped in a common neurilemma lying in the thick hypoderm of the median ventral region. Internally are the fibres of the circular coat and the insertions of the oblique and vertical muscles.

Terebellidæ.—Anteriorly in *Terebella nebulosa*, Mont., the nerve-cords are placed outside the transverse band of fibres (part of the circular coat) between the oblique muscles, and therefore are hypodermic. The same arrangement occurs posteriorly. In *Polycirrus aurantiacus*, Gr., the cords have the same relative position, only they are considerably larger—a feature of interest in connection with the phosphorescent properties of the species. Within the circular coat is a small median longitudinal muscle. In *Terebel-*

lides strœmi, Sars., the cords are small, and lie within the hypoderm and strong circular coat in a line between the ventral longitudinal muscles. The slender oblique are attached on each side of the trunks, and must be carefully distinguished from the much larger muscles which cut off the great lateral longitudinal muscle externally.

Sabellidæ.—In *Sabella pavonina*, Sav., the nerves occur in front as widely separated cords—each situated at the inner border of the ventral longitudinal muscle. A large neural canal lies above each. Externally are the circular muscular coat, the greatly thickened hypoderm and the cuticle. The feeble oblique muscles are partly inserted above the neural canal and partly into the basement-layer below and internal to the nerve. Proceeding backward, the neural canal increases very much in size, so that it occupies the whole area on each side of the central region, and presses the ovoid nerve-cord to the exterior. The canals appear to be filled with a slightly yellowish substance—consolidated in the preparations. The same relative position is maintained posteriorly, though the neural canal is smaller, and, as in front, placed superiorly. The arrangement in *Dasychone bombyx*, Dalyell, agrees in most respects with the foregoing.

Chone and *Euchone* seem to approach the next family in the arrangement of the nerve-cords and in the remarkable structure of the great longitudinal muscles. In *Chone infundibulum*, Mont., the nerve-trunks occur between the ventral longitudinal muscles, and placed much more closely together than in *Sabella*. A large neural canal lies over each nerve. Externally are the strong circular coat, the thick hypoderm, and the cuticle. The oblique muscles are attached to the sides of the median vessel above the neural canals, and to the summit of the latter on each side. While the cords are lost in the ganglia, the neural canals remain distinct. In *Euchone* a similar condition exists, for though the cords are distinct anteriorly they are closely approximated posteriorly.

Eriographididæ.—In the anterior third of *Myxicola infundibulum*, Renier, the cords are approximated in the interval between the great longitudinal ventral muscles, and beneath the large blood-vessel. A neural canal appears to exist toward the superior border. The slender oblique muscles are inserted into the summit of the nerve-area. Externally are circular coat, hypoderm, and cuticle. Posteriorly the cords are connate, and a single neural canal of considerable size exists superiorly.

Serpulidæ.—In *Protula protensa* the cords anteriorly are situated under the great longitudinal dorsal muscles as widely separated trunks. They then fall into position and approach the increasing longitudinal ventral muscles—a large neural canal being at the inner border. Posteriorly the neural canal is proportionally larger than in front, but the relations of the nerves are similar. At their commencement in front the nerve-cords are internal, and have beneath them all the tissues under the dorsal muscles. After reaching the inner border of the ventral longitudinal muscles they have externally (*i.e.*, inferiorly) a thin layer of longitudinal muscular fibres, the circular coat, hypoderm, and cuticle. A transverse band bounds them superiorly. Posteriorly the first-mentioned (longitudinal) layer is absent externally. In *Serpula vermicularis*, L., a similar arrangement occurs, but the neural canal is external, that is, next the ventral longitudinal muscle, and the thin longitudinal stratum is internal.

4. On the Application of Graphic Methods to the Determination of the Efficiency of Machinery. By Professor Fleeming Jenkin.

(*Abstract.*)

The general scope of the paper was to show how by graphic methods we might find the relation between effort exerted at one part of a machine and the resistance overcome at another part. It was shown that for a given machine at any instant a linear frame might be substituted, such that the stresses in the links corresponded to the pressures at the joints between the elements of the machine, including the pressures due to driving effort and resistance. Numerous examples were given of the application of this frame, called the dynamic frame, to the solution of the above problem, the friction, inertia, and weight of all the parts being taken rigidly into account.

5. On Professor Tait's Problem of Arrangement. By Thomas Muir, M.A.

The problem in question is—To find the number of possible arrangements of a set of n things, subject to the conditions that the first is not to be in the last or first place, the second not in the first or second place, the third not in the second or third place, and so on.

A little consideration serves to show that we may with advantage shift the ground of the problem to the theory of determinants. For the sake of definiteness take the case of *five* things, A, B, C, D, E. Here A may be in the second, third, or fourth places only; B in the third, fourth, or fifth places only; and similarly of the others—a result which we may tabulate thus:—

.	A	A	A	.
.	.	B	B	B
C	.	.	C	C
D	D	.	.	D
E	E	E	.	.

an A being written in the places which it is possible for A to occupy, and a dot signifying that the letter found in the same line with it may not occupy its place. Hence, to obtain the various arrangements, we see that for the first place we may have any letter that is in the first column; for the second place any letter that is in the second column, provided it be not in the same line with the letter taken from the first column; for the third place, any letter that is in the third column, provided it be not in the same line with either of the letters previously taken, and so on. This law of formation, however, is identical with that in accordance with which the terms of a determinant are got from the elements of the matrix; so that the problem we are concerned with is transformed into this: Find the number of terms of the determinant of the nth order of the form,—

$$\left|\begin{array}{ccccc} \cdot & * & * & * & \cdot \\ \cdot & \cdot & * & * & * \\ * & \cdot & \cdot & * & * \\ * & * & \cdot & \cdot & * \\ * & * & * & \cdot & \cdot \end{array}\right|$$

where the dots and asterisks denote zero elements and non-zero elements respectively.

In the solution of this, determinants of a set of other forms require to be considered, viz.,—

$$\left|\begin{array}{ccccc} \cdot & * & * & * & * \\ \cdot & \cdot & * & * & * \\ * & \cdot & \cdot & * & * \\ * & * & \cdot & \cdot & * \\ * & * & * & \cdot & \cdot \end{array}\right| , \quad \left|\begin{array}{ccccc} \cdot & * & * & * & * \\ * & \cdot & * & * & * \\ * & \cdot & \cdot & * & * \\ * & * & \cdot & \cdot & * \\ * & * & * & \cdot & \cdot \end{array}\right| , \quad \left|\begin{array}{ccccc} \cdot & * & * & * & * \\ \cdot & \cdot & * & * & * \\ * & * & \cdot & * & * \\ * & * & \cdot & \cdot & * \\ * & * & * & \cdot & \cdot \end{array}\right| , \ \&c.$$

in all of which the elements of the main diagonal are zero; the elements of the adjacent minor diagonal being in the first also all zero, in the second all zero except the first element, in the third all zero except the second element, and so on. Let us denote the number of terms in these when they are of the nth order by

$$\chi_0(n)\,,\ \chi_1(n)\,,\ \chi_2(n)\,,\ \ldots .$$

and let the number of terms sought be

$$\Psi(n)\,.$$

Further, let any determinant-form with dots and asterisks stand for *the number of terms* in such a determinant; the four forms above, for example, being thus symbols equivalent to $\Psi(5)$, $\chi_0(5)$, $\chi_1(5)$, $\chi_2(5)$, respectively.

Beginning with the first of the χ forms we see it to be transformable into

$$\begin{vmatrix} \cdot & * & * & * & \cdot \\ \cdot & \cdot & * & * & * \\ * & \cdot & \cdot & * & * \\ * & * & \cdot & \cdot & * \\ * & * & * & \cdot & \cdot \end{vmatrix} + \begin{vmatrix} \cdot & \cdot & * & * \\ * & \cdot & \cdot & * \\ * & * & \cdot & \cdot \\ * & * & * & \cdot \end{vmatrix}$$

and the second term of this may in like manner be changed into

$$\begin{vmatrix} \cdot & \cdot & * & * \\ * & \cdot & \cdot & * \\ * & * & \cdot & \cdot \\ \cdot & * & * & \cdot \end{vmatrix} + \begin{vmatrix} \cdot & * & * \\ \cdot & \cdot & * \\ * & \cdot & \cdot \end{vmatrix}$$

and the second term of this again into

$$\begin{vmatrix} \cdot & * & \cdot \\ \cdot & \cdot & * \\ * & \cdot & \cdot \end{vmatrix} + \begin{vmatrix} \cdot & \cdot \\ * & \cdot \end{vmatrix}$$

the second term of which is zero. Hence we have the result,

$$\chi_0(n) = \Psi(n) + \Psi(n-1) + \Psi(n-2) + \ldots + \Psi(3), \quad . \quad . \quad . \quad (a)$$

Of the other χ forms it is clear to begin with that

$$\chi_1 = \chi_{n-1}, \qquad \chi_2 = \chi_{n-2}, \quad \ldots\ldots$$

and treating the distinct cases as we have treated χ_0, we equally readily see that

$$\begin{aligned} \chi_1(n) &= \chi_0(n) + \chi_0(n-1) + \chi_0(n-2) \\ \chi_2(n) &= \chi_0(n) + \chi_0(n-1) + \chi_1(n-2) \\ \chi_3(n) &= \chi_0(n) + \chi_0(n-1) + \chi_2(n-2) \\ &\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots \\ \chi_{n-2}(n) &= \chi_0(n) + \chi_0(n-1) + \chi_1(n-2) \\ \chi_{n-1}(n) &= \chi_0(n) + \chi_0(n-1) + \chi_0(n-2) \end{aligned}$$

which, on eliminating χ_1, χ_2, ... from the right-hand members, become

$$\chi_1(n) = \chi_0(n) + \chi_0(n-1) + \chi_0(n-2)$$
$$\chi_2(n) = \chi_0(n) + \chi_0(n-1) + \chi_0(n-2) + \chi_0(n-3) + \chi_0(n-4)$$
$$\chi_3(n) = \chi_0(n) + \chi_0(n-1) + \chi_0(n-2) + \chi_0(n-3) + \chi_0(n-4) + \chi_0(n-5) + \chi_0(n-6)$$

....

$$\chi_{n-2}(n) = \chi_0(n) + \chi_0(n-1) + \chi_0(n-2) + \chi_0(n-3) + \chi_0(n-4)$$
$$\chi_{n-1}(n) = \chi_0(n) + \chi_0(n-1) + \chi_0(n-2) .$$

Here the second of the series of right-hand members has two terms more than the first, the third two terms more than the second, and so on until we approach the middle of the series, when, if n be odd, the two middle right-hand members are found to be the same as the one preceding or the one following them, the whole four ending thus—

$$\ldots . + \chi_0(5) + \chi_0(4) + \chi_0(3) ;$$

and if n be even, the one middle right-hand member is found to be greater by unity than the one preceding or the one following it, and to end thus—

$$\ldots . + \chi_0(5) + \chi_0(4) + \chi_0(3) + 1 ,$$

the 1 arising from the fact that the above process of reduction, in the case of $\chi_{\frac{1}{2}n}(n)$, leads us finally, not to

$$\begin{vmatrix} \cdot & * & * \\ \cdot & \cdot & * \\ * & \cdot & \cdot \end{vmatrix} + \begin{vmatrix} * & * \\ \cdot & \cdot \end{vmatrix} , \text{ but to } \begin{vmatrix} \cdot & * & * \\ \cdot & \cdot & * \\ * & \cdot & \cdot \end{vmatrix} + \begin{vmatrix} \cdot & * \\ * & \cdot \end{vmatrix}$$

i.e., to $\chi_0(3) + 1$.

Returning now to the Ψ form, and taking

$$\begin{vmatrix} \cdot & * & * & * & \cdot \\ \cdot & \cdot & * & * & * \\ * & \cdot & \cdot & * & * \\ * & * & \cdot & \cdot & * \\ * & * & * & \cdot & \cdot \end{vmatrix}$$

we transform it into

$$\begin{vmatrix} \cdot & * & * & * \\ * & \cdot & * & * \\ * & \cdot & \cdot & * \\ * & * & \cdot & \cdot \end{vmatrix} + \begin{vmatrix} \cdot & \cdot & * & * \\ * & \cdot & * & * \\ * & * & \cdot & * \\ * & * & \cdot & \cdot \end{vmatrix} + \begin{vmatrix} \cdot & \cdot & * & * \\ * & \cdot & \cdot & * \\ * & * & \cdot & * \\ * & * & * & \cdot \end{vmatrix}$$

the middle term of which becomes by transposition of the first two rows, and the subsequent transposition of the first two columns,

$$\begin{vmatrix} \cdot & * & * & * \\ \cdot & \cdot & * & * \\ * & * & \cdot & * \\ * & * & \cdot & \cdot \end{vmatrix}$$

Consequently we have

$$\Psi(5) = \chi_1(4) + \chi_2(4) + \chi_3(4) ,$$

and it is easily seen that a similar transformation is possible in every case, giving

$$\Psi(n) = \chi_1(n-1) + \chi_2(n-1) + \chi_3(n-1) + \dots\dots + \chi_{n-2}(n-1) .$$

Expressing χ_2, χ_3, ... in terms of χ_0 by means of what precedes, we have

$$\begin{aligned} \Psi(n) = \quad & \chi_0(n-1) + \chi_0(n-2) + \chi_0(n-3) \\ & + \chi_0(n-1) + \chi_0(n-2) + \chi_0(n-3) + \chi_0(n-4) + \chi_0(n-5) \\ & \dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots \\ & + \chi_0(n-1) + \chi_0(n-2) + \chi_0(n-3) + \chi_0(n-4) + \chi_0(n-5) \\ & + \chi_0(n-1) + \chi_0(n-2) + \chi_0(n-3) , \end{aligned}$$

and now using (*a*), to express χ_0 in terms of Ψ, we find

$$\begin{aligned}\Psi(n) = \quad & \Psi(n-1)+2\Psi(n-2)+3\{\Psi(n-3)+ \ldots\ldots\ldots\ldots\ldots +\Psi(3)\} \\ & +\Psi(n-1)+2\Psi(n-2)+3\Psi(n-3)+4\Psi(n-4) \\ & \qquad\qquad +5\{\Psi(n-5)+ \ldots\ldots\ldots\ldots\ldots +\Psi(3)\} \\ & \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ & +\Psi(n-1)+2\Psi(n-2)+3\Psi(n-3)+4\Psi(n-4) \\ & \qquad\qquad +5\{\Psi(n-5)+ \ldots\ldots\ldots\ldots\ldots +\Psi(3)\} \\ & +\Psi(n-1)+2\Psi(n-2)+3\{\Psi(n-3)+ \ldots\ldots\ldots\ldots\ldots +\Psi(3)\}\end{aligned}$$

where, on the first line the coefficient of the third and all the following terms is 3, on the second line the coefficient of the fifth and all the following terms is 5, on the third line the coefficient of the seventh and all the following terms is 7, and so on, the middle term (when such occurs) having a 1 superadded.

Hence, for the determination of $\Psi(n)$ when $\Psi(n-1)$, $\Psi(n-2)$, ... are known, we have

$$\begin{aligned}\Psi(n) = (n-2)\Psi(n-1)+(2n-\ 4)\Psi(n-2)+(3n-\ 6)\Psi(n-3) \\ +(4n-10)\Psi(n-4)+(5n-14)\Psi(n-6) \\ +(6n-20)\Psi(n-6)+(7n-26)\Psi(n-7) \\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ +\frac{1-(-1)^n}{2},\end{aligned}$$

where the coefficients proceed for two terms with the common difference $n-2$, for the next two terms with the common difference $n-4$, for the next two terms with the common difference $n-6$, and so on.

And as it is self-evident that $\Psi(2)=0$, we obtain

$$\begin{aligned}\Psi(3) &= 1\Psi(2)+1 &&= 1 \\ \Psi(4) &= 2\Psi(3) &&= 2 \\ \Psi(5) &= 3\Psi(4)+6\Psi(3)+1 &&= 13 \\ \Psi(6) &= 4\Psi(5)+8\Psi(4)+12\Psi(3) &&= 80 \\ \Psi(7) &= 5\Psi(6)+10\Psi(5)+15\Psi(4)+18\Psi(3)+1 &&= 579 \\ \Psi(8) &= 6\Psi(7)+12\Psi(6)+18\Psi(5)+22\Psi(4)+26\Psi(3) &&= 4738\end{aligned}$$

and so forth.

To the foregoing Professor Cayley has kindly made the following additions :—

The investigation may be carried further: writing for shortness u_3, u_4, &c., in place of $\Psi(3)$, $\Psi(4)$, &c., the equations are

$$\begin{aligned}
u_3 &= 1\,, \\
u_4 &= 2u_3\,, \\
u_5 &= 3u_4 + \ \ 6u_3 + \ \ 1\,, \\
u_6 &= 4u_5 + \ \ 8u_4 + 12u_3 \\
u_7 &= 5u_6 + 10u_5 + 15u_4 + 18u_3 + 1\,,
\end{aligned}$$

and hence assuming

$$u = u_3 + u_4 x + u_5 x^2 + u_5 x^3 + u_7 x^4 \ldots$$

we have

$$\begin{aligned}
u = \frac{1}{1-x^2} &+ u_3(2x + 6x^2 + 12x^3 + 18x^4 + \ldots \\
&+ u_4(3x^2 + 8x^3 + 15x^4 + 22x^5 + \ldots \\
&+ u_5(4x^3 + 10x^4 + 18x^5 + 26x^6 + \ldots \\
&+ u_6(5x^4 + 12x^5 + 21x^6 + 30x_7 + \ ;
\end{aligned}$$

and hence forming the equation

$$\begin{aligned}
u'\frac{x^2}{(1-x)^2} = &\ \ u_4(\ x^2 + 2x^3 + 3x^4 + 4x^5 + \ldots) \\
&+ u_5(2x^3 + 4x^4 + 6x^5 + 8x^6 + \ldots) \\
&+ u_6(3x^4 + 6x^4 + 9x^6 + 12x^7 + \ldots)\,;
\end{aligned}$$

where u' denotes $\frac{du}{dx}$, we have

$$\begin{aligned}
u - u'\frac{x^3}{(1-x)^2} &= \frac{1}{1-x^2} + (u_3 + u_4 x + u_5 x^2 \ldots)(2x + 6x^2 + 12x^3 + 18x^4 \ldots) \\
&= \frac{1}{1-x^2} + u(2x + 6x^2 + 12x^3 + 18x^4 + \ldots)\,;
\end{aligned}$$

or, what is the same thing,

$$u - u'\frac{x^2}{(1-x)^2} = \frac{1}{1-x^2} + u\left\{\frac{2x}{(1-x)^3} - \frac{2x^4}{(1-x)^3(1+x)}\right\}\,;$$

that is,

$$\left\{1-\frac{2x}{(1-x)^3}+\frac{2x^4}{(1-x)^3(1+x)}\right\}u-\frac{x^2}{(1-x)^2}u'=\frac{1}{1-x^2}.$$

This equation may be simplified: write

$$u=-\frac{1-x^2}{x^4}Q\,,\ =\left(-\frac{1}{x^4}+\frac{1}{x^2}\right)Q\,,$$

then

$$u'=\left(\frac{4}{x^5}-\frac{2}{x^3}\right)Q+\frac{1-x^2}{x^4}Q'\,,$$

and the equation is

$$\left\{-\frac{1-x^2}{x^4}+\frac{2}{x^3}\frac{1+x}{(1-x)^2}-\frac{2}{(1-x)^2}\right.$$
$$\left.-\frac{4}{x^3}\frac{1}{(1-x)^2}+\frac{2}{x(1-x)^2}\right\}Q+\frac{1+x}{(1-x)x^2}Q'=\frac{1}{1-x^2};$$

that is,

$$\left\{-\frac{1}{x^4}+\frac{1}{x^2}-\frac{2}{x^3(1-x)^2}+\frac{2}{x^2(1-x)^2}+\frac{2}{x(1-x)^2}-\frac{2}{(1-x)^2}\right\}Q$$
$$+\frac{1+x}{(1-x)x^2}Q'=\frac{1}{1-x^2}\,,$$

viz., this is

$$\left\{-\frac{(1-x)^2}{x^4}+\frac{(1-x)^2}{x^2}-\frac{2}{x^3}+\frac{2}{x^2}+\frac{2}{x}-2\right\}Q+\frac{1-x^2}{x^2}Q'=\frac{1-x}{1+x}\,,$$

that is

$$\left\{-\frac{1}{x^4}+\frac{2}{x^2}-1\right\}Q+\frac{1-x^2}{x^2}Q'=\frac{1-x}{1+x};$$

or

$$-\frac{(1-x^2)^2}{x^4}Q+\frac{1-x^2}{x^2}Q'=\frac{1-x}{1+x};$$

or finally,

$$Q\left(1-\frac{1}{x^2}\right)+Q'=\frac{x^2}{(1+x)^2}\,,$$

giving

$$Q=e^{-\left(x+\frac{1}{x}\right)}\int\frac{x^2}{(x+1)^2}e^{x+\frac{1}{x}}dx\,,$$

and thence

$$u = \frac{x^2-1}{a^4} e^{-\left(x+\frac{1}{x}\right)} \int \frac{x^2}{(x+1)^2} e^{\left(x+\frac{1}{x}\right)} dx ,$$

which is the value of the generating function

$$u = u_3 + u_4 x + u_5 x^2 + \&c.$$

But for the purpose of calculation it is best to integrate by a series the differential equation for Q: assume

$$Q = -q_3 x^4 - q_4 x^5 - q_5 x^6 - \ldots$$

then we find

$$\begin{aligned} q_4 &= 4q_3 & & -2, \\ q_5 &= 5q_4 & +q_3 & +3, \\ q_6 &= 6q_5 & +q_4 & -4, \\ q_7 &= 7q_6 & +q_5 & +5, \\ &\vdots \\ q_n &= nq_{n-1} + q_{n-2} + (-)^{n-1}(n-2) . \end{aligned}$$

We have thus for $q_3, q_4, q_5 \ldots$ the values 1, 2, 14, 82, 593, 4820, and thence

$$u = (1-x^2)(1 + 2x + 14x^2 + 82x^3 + 593x^4 + 4820x^5 + \ldots) ,$$

viz., writing

$$\begin{array}{rrrrrr} 1 & 2 & 14 & 82 & 593 & 4820 \ldots \\ & & -1 & -2 & -14 & -82 \\ \hline 1, & 2, & 13, & 80, & 579, & 4738 \ldots \end{array}$$

the values of $u_3, u_4 \ldots$ are 1, 2, 13, 80, 579, 4738 ... agreeing with the results found above.

In the more simple problem, where the arrangements of the n things are such that no one of them occupies its original place, if u_n be the number of arrangements, we have

$$\begin{aligned} u_2 &= 1 & &= 1 \\ u_3 &= 2u_2 & ,&= 2 \\ u_4 &= 3(u_3+u_2) & ,&= 9 \\ u_5 &= 4(u_4+u_3) & ,&= 44 \\ &\vdots \\ u_{n+1} &= n(u_n + u_{n-1}) , \end{aligned}$$

and writing

$$u = u_2 + u_3 x + u_4 x^2 + \ldots$$

we find

$$u = 1 + (2x + 3x^2)u + (x^2 + x^3)u' ;$$

that is

$$(-1 + 2x + 3x^2)u + (x^2 + x^3)u' = -1 ;$$

or, what is the same thing,

$$u' + \left(\frac{3}{x} - \frac{1}{x^2}\right)u = -\frac{1}{x^2(1+x)} ,$$

whence

$$u = x^{-3} e^{-\frac{1}{x}} \int \frac{-x}{1+x} e^{\frac{1}{x}} dx ,$$

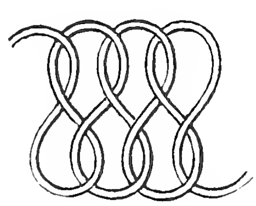
but the calculation is most easily performed by means of the foregoing equation of differences, itself obtained from the differential equation written in the foregoing form,

$$(-1 + 2x + 3x^2)u + (x^2 + x^3)u' = -1 .$$

6. On Amphicheiral Forms and their Relations. By Professor Tait.

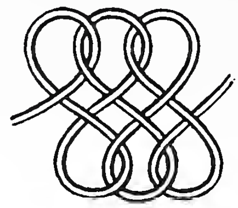
(*Abstract.*)

If a cord be knotted, any number of times, according to the pattern below

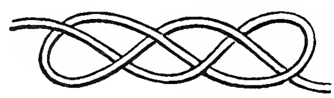


it is obviously *perverted* by simple *inversion*. Hence, when the free ends are joined it is an amphicheiral knot. Its simplest form is that of 4-fold knottiness. All its forms have knottiness expressible as $4n$.

The following pattern gives amphicheiral knots of knottiness $2+6n$.



And on the following pattern may be formed amphicheiral knots of all the orders included in $6n$ and $4+6n$.

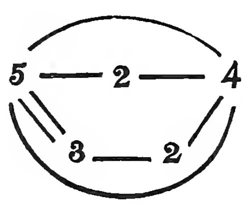


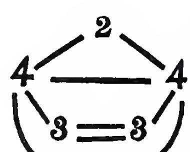
Among them these forms contain all the even numbers, so that *there is at least one amphicheiral form of every even order.*

Many more complex forms are given in the paper, several of which are closely connected with knitting, &c.

In one of my former papers I gave examples of type-symbol which individually represent two perfectly different knots.

I now give examples of the same knot represented by type-symbols which have neither right nor left-handed parts in common. One of the most remarkable of these is





which can be analysed (but not separated) into a combination of the two forms of the 4-fold amphicheiral knot.

The following Gentlemen were elected Fellows of the Society :—

William Jolly, H.M. Inspector of Schools, Inverness.
Rob. Milner Morrison, 21 Manor Place.
John Gibson, Ph.D., 12 Greenhill Gardens.
Chas. E. Underhill, B.A., M.B., 8 Coates Crescent.
Charles Edward Wilson, M.A., LL.D., 19 Palmerston Place.
George Carr Robinson, East Preston Street.

Monday, 16*th April* 1877.

Professor KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. On the Toothing of Un-round Discs which are intended to roll upon each other. By Edward Sang.

(*Abstract.*)

This paper contained an extension to discs of any shape whatever of the principles explained in the "New General Theory of the Teeth of Wheels," as applicable to circular discs.

2. On the Mineralogy of Scotland.—Chapter II. By Professor Heddle.

In this chapter Professor Heddle submitted the results of the analyses of Orthoclase from fifteen localities; of Albite from four; of Oligoclase from eight; of Labradorite from eleven; of Andesiel from five; of Anorthite from three; and of Latrobite from two.

The three last minerals being now, for the first time, recognised as British species.

Dr Heddle also described a peculiar association of Orthoclase with Oligoclase, in crystals nearly of the form of the former, from certain localities; he drew the conclusion from his researches that the above felspars are all well individualised, if not by physical, at least by chemical characters; while they are probably more or less special to certain rocks.

3. Least Roots of Equations. By J. D. Hamilton Dickson.

Monday, 7th May 1877.

DAVID STEVENSON, Esq., Vice-President, in the Chair.

The following Communications were read:—

1. On new and little-known Fossil Fishes from the Edinburgh District. No. III. By Dr R. H. Traquair.

2. On Ocean Circulation. By John Aitken.

It is with extreme reluctance that I venture to disturb the present repose of the much-contested field of ocean circulation. My object is not, however, to provoke discussion on the general theory of ocean circulation, as I am sure all will agree in thinking that the subject has already been discussed far beyond the point at which it is likely to be benefited by discussion. My object is simply to call attention to certain influences at work in the ocean, the effects of which seem to have been totally overlooked.

The first of them to which I wish to refer is the influence of the winds on the ocean. The extreme holders of the wind theory of ocean circulation consider that the action of the wind is quite sufficient to account for all the currents which we find in the ocean. That the wind is *a* cause of ocean currents no one can doubt. If we examine a lake when the wind is blowing over it, we shall find that the plants growing in the shallow water near the surface are all bending in the direction of the wind, indicating that there is a current at the surface flowing in the direction of the wind,—the appearance of the bending plants in the lake reminding one of a slow-running river. To supply the water for this surface current there must, of course, be another current, flowing in the opposite direction underneath. The lake and the ocean are not, however, parallel cases. In the case of the lake, the wind is blowing in the same direction all over it, so that the return current is forced to flow underneath the surface, as it cannot get back any other way; whereas, in the ocean, the wind blows in one direction at one part, and in a different direction at another part, and the question now comes to be: What is the effect of the difference in the two cases? If the wind does not blow in the same direction at all parts over the surface of the water, will the return current flow underneath the surface current, or will it return by some other route?

It is extremely difficult to get a satisfactory experimental answer to this question. The following attempts were, however, made:—A trough with glass sides was filled with water, the water being well stirred just before the experiment was made, to prevent difference of density due to temperature having any effect on the result. When all was again at rest, a solution of colouring matter was dropped into the water at different points. Each drop of colouring matter, as it sunk to the bottom, left a vertical coloured streak in the water. A jet of air urged by a pair of bellows was now directed along the surface of the water, so as to act on only a small part of the surface, near the middle of the breadth of the trough. The upper part of the coloured streaks, situated underneath the air-jet, at once indicated a current in the surface water in the direction of the air-jet. The return current did not, however, flow *underneath* the air-driven current, as in the case of the lake, but the air-driven current divided in two at the far end of the trough, and *flowed back on the surface,* one current on each side of the air-driven current.*

When the air current was first started, the water currents were confined to the surface; but after the motion had been kept up some time, the depth of the currents gradually increased, till all the water in the trough was in motion,—the direction of the motion of the water at any part of the bottom being the same as at the surface vertically over it. The water showed not the smallest tendency to take up a "vertical" circulation, similar to the circulation produced by difference of density. The surface water simply circulated to different parts of the surface, and the bottom water to different parts of the bottom, almost the whole of the motion taking

* The trough in which the experiment is made must have sloping and not vertical ends, because if the wind-driven current strikes against a vertical surface, it raises a "head," which causes a vertical current to descend at the end of the trough, in addition to the two return surface currents.

place in a horizontal direction. In order to get quit, as much as possible, of the effects of friction on the sides and bottom of the vessel, the experiment was repeated in a pond, with a like result.

From these experiments we may conclude that, save under very exceptional circumstances, the wind can only give rise to a horizontal circulation of the oceanic waters,—the exceptional cases being, when the wind-driven current is deflected by the irregularities in the outline of the bed of the ocean, or strikes against a deep and nearly vertical coast; or when the north wind drives the waters of the Antarctic Sea against the great barrier of ice-cliffs which surround the south pole. The depth to which the vertical currents will descend in these cases will depend much on the relative densities of the water in the currents and of the water surrounding them.

It may possibly be objected that we are not entitled to come to any conclusion, from experiments made on so small a scale, as to what takes place in the ocean. Such objections would be perfectly valid, if there was not some evidence, in the conditions we find in the ocean, to support these conclusions. If the return currents in the ocean flowed underneath the wind-driven surface currents, then we should be perfectly justified in expecting some evidence of their presence. For instance, we would expect that the water near the bottom, underneath the wind-driven currents near the equator, would be hotter than the water at corresponding depths at other parts of the ocean. An examination, however, of the temperature sections of the Atlantic Ocean, taken by the "Challenger," shows no evidence whatever of the return current by this route; we are therefore compelled to conclude that the water must return by the surface, and that the *wind* does only produce *horizontal currents*, and therefore cannot account for the presence of the cold water which we find all over the bottom of the ocean, from the poles to the equator.

The second point I wish to refer to, is the effect of these wind-driven surface currents on the cold water underneath them. I have said that almost the whole of the motion of the wind-driven currents takes place in a horizontal direction. Such was the general result given by the experiment. But, in addition to this, there is another point to which I must refer. The wind driven horizontal currents have an influence on the water underneath them which, for the sake of clearness, I have reserved for separate consideration here. Let us draw an imaginary section across a wind-driven current, at a point near its source, that is, near where the wind begins to act on it. And let us imagine another section of this same current, at a point some distance farther "down" the stream. As the current is acted upon by the wind between these two sections, it will be much deeper at the second section than at the first, and will also be going at a greater velocity. There will, therefore, be much more water passing the second section than passed the first, and the water necessary to supply this growing stream must be supplied to it between the two imaginary sections. The result is, part of the necessary supply rushes in at the sides, but part of it rises from the still water underneath the surface stream. This lifting of the deeper water by the surface current was very evident in the experiment already referred to, so long as the surface current was shallow, and gradually became less, as might be expected, when the current deepened.

From these considerations, we are naturally led to expect that the hot surface water at those parts of the ocean over which winds are constantly blowing, will be much reduced in depth, and that the cold bottom water will be found at a less depth underneath these surface currents than at any other part of the ocean. Part of this cold water will, in all probability, get mixed up with the bottom water of the surface current. And further, we would expect that this wind-driven hot surface water, after it passes beyond the windy regions, will gradually lose its motion and increase in depth.

These expectations are in a remarkable manner supported by the evidence of the temperature sections of the Atlantic taken by the "Challenger," and given in Dr Carpenter's paper in the "Proceedings of the Royal Geographical Society," vol. xviii. No. iv., 1874. Take, for instance, the section between Tenerife and St Thomas. In the first part of the journey there is not much alteration in the relative position of the isotherms, but after crossing the Tropic of Cancer and getting into the region of the north-east trade winds, the hot surface isotherms gradually approach each other, and the isotherm of 40°, which off the coast of Tenerife was at a depth of nearly 1000 fathoms, rises to about 700 fathoms before it arrives at St Thomas. Again, in the passage north from St Thomas to Bermuda, and on to Halifax and New York, the temperature sections show that after getting out of the region of the trade winds, the drifted hot surface water has gradully lost its motion and increased in depth. This refers to the great mass of ocean water, and not to the comparatively shallow Gulf-Stream. For instance, the isotherm of 60°, which at St Thomas was found at a depth of only 200 fathoms, was found at a depth of 330 fathoms for hundreds of miles all round Bermuda, notwithstanding a considerable reduction in the temperature of the surface water.

The temperature sections of the South Atlantic do not illustrate these points so well as the temperature sections of the North Atlantic, partly because a very large part of the hot surface water of the south equatorial current does not return to the South Atlantic, but is driven into the North Atlantic, and partly on account of the great amount of cold surface water of the Antarctic drift, which gets mixed np with the return current; and further, the temperature sections are not taken at the best places for our present purpose. The only two available sections, however, point to the same conclusion as the North Atlantic sections. There is no section of the south equatorial current, but we may suppose it to be somewhat similar to the section taken between St Paul's Rocks and Pernambuco, which gives the isotherms of the branch of the south equatorial current which passes into the North Atlantic. If we compare this section with the part of the stream which has flowed southwards, as given in the section taken between Abrolhos Island and Tristan d'Acunha, we find that the hot surface water, in flowing southwards beyond the region of the south-east trade winds, has deepened, notwithstanding a considerable fall in the temperature of the surface water. The isotherm of 40° which was found at a depth of 300 fathoms off Pernambuco, sunk to a depth of between 400 and 500 fathoms between Abrolhos Island and Tristan d'Acunha. We might have expected that this hot surface water would have kept its depth all the way to the Cape of Good Hope. It, however, does not do so, probably on account of the cold surface water of the Antarctic drift.

I am aware Dr Carpenter has offered a different explanation of the rising of the glacial water under the equator. He considers that the rising of the glacial water under the Line is due to the meeting of the Arctic and Antarctic underflows. That part of the effect is due to this cause is very probable, but when we consider the very great area of section of the under glacial currents, and the small amount of water that can be carried by them, it is evident that their rate of motion must be very slow, and it is very doubtful how far the whole phenomena can be explained in this way. Our doubts on this subject are somewhat confirmed by the consideration of the fact that the Arctic underflow is warmer than the Antarctic underflow, and would therefore—other things being equal—tend simply to *overflow* the Antarctic underflow, and not to rise vertically as Dr Carpenter supposes. That the wind-driven currents, so long as they are increasing in volume, have the power of drawing the bottom water upwards cannot be doubted, and I have already said was most marked in the experimental illustration. We should not, however, expect to find this lifting power so marked in the ocean, as the ocean currents are so much deeper and do not so rapidly increase in volume as in the experiment. And further, the bottom water in the ocean is denser than the surface water, and does not rise so easily as the bottom water in the experiment.

If we take the evidence of the temperature sections of the Atlantic on the subject, we shall find that they also point to the wind as one of the causes of the rising of the cold bottom water. If we take the section between Madeira and lat. 3° N. and long. 15° W, we find that the isotherm of 40°, which at Madeira lies at a depth of about 900 fathoms, rises to a depth of only 300 fathoms at the equatorial position, and further, neither this section nor the section between lat. 3° N. and long. 15° W. and Pernambuco, show the least evidence of the presence of Antarctic water so far north as the equator, in the eastern basin of the Atlantic. *This rising, then, of the glacial water on the eastern side of the Atlantic cannot therefore be due to the meeting of the glacial streams*, and in the absence of further evidence we may suppose it to be due to the wind-driven currents. We thus see, that though the great bulk of the wind-driven circulation is a horizontal one, yet there is also produced in a comparatively very small degree a modified vertical circulation.

It may here be asked, Is this lifting power of the surface currents sufficient to account for the vertical circulation which we find in the ocean? In all probability it is not. There seems to be no reason why this vertically rising current under the equator should draw its supplies from the furthest limits of the ocean, which it would require to do to explain the conditions we find existing. Yet there can be no doubt but that these horizontal surface currents really do assist in producing a vertical circulation.

3. On a New Investigation of the Series for the Sine and Cosine of an Arc. By Edward Sang.

The sines of the successive equidifferent arcs form a progression having for its general character the relation

$$\phi_{n-1} - 2\phi n + \phi_{n+1} - \phi n \,.\, v\,,$$

and the properties of sines may be deduced from this general formula. Viewed in this light, the angular functions become cases only of more general ones.

If we suppose A, B, C to be three consecutive terms of such a progression we must have

$$A - 2B + C = vB\,,$$

from which, when three of the four quantities, A, B, C, v are given, the fourth may be found. Let then A and B, the first and second terms of the progression, and v the common coefficient, be given; the succeeding terms may be computed thus:—

$$\begin{array}{lll}
\phi 0 = & A & \\
 & -A & +B \\
 & & +Bv \\
\hline
\phi 1 = & & B \\
 & -A & +B(1+v) \\
 & -Av & +B(\ \ +2v+v^2) \\
\hline
\phi 2 = & -A & +B(2+v) \\
 & -A(1+v) & +B(1+3v+v^2) \\
 & -A(\ \ +2v+v^2) & +B(\ \ +3v+4v^2+v^3) \\
\hline
\end{array}$$

$$\begin{array}{lll} \phi 3 = & -A(2+v) & +B(3+4v+v^2) \\ & -A(1+3v+v^2) & +B(1+6v5v^2+v^3) \\ \hline \phi 4 = & -A(3+4v+v^2) & +B(4+10v+6v^2+v^3)\,, \end{array}$$

from which it is obvious that the coefficient of $-A$ in the expression for ϕn, is a transcript of that of B in the preceding expression for $\phi(n-1)$. Hence, for the present, we may confine our attention to the latter.

The coefficients of B form the following progression:—

in $\phi 0$ 0
in $\phi 1$ 1
in $\phi 2$ $2+v$
in $\phi 3$ $3+4v+v^2$
in $\phi 4$ $4+10v+6v^2+v^3$
in $\phi 5$ $5+20v+21v^2+8v^3+v^4$
in $\phi 6$ $6+35v+56v^2+36v^3+10v^4+v^5$
in $\phi 7$ $7+56v+126v^2+120v^3+55v^4+12v^5+v^6$
in $\phi 8$ $8+84v+252v^2\times 330v^3+220v^4+78v^5+14v^5+v^7$
in $\phi 9$ $9+120v+462v^2+792v^3+715v^4+364v^5+105v^6+16v^7+$ &c.

and in general

$$\text{in } \phi n \quad \frac{n}{1}+\frac{n-1}{1}\,\frac{n}{2}\,\frac{n+1}{3}\,v+\frac{n-2}{1}\,\frac{n-1}{2}\,\frac{n}{3}\,\frac{n+1}{4}\,\frac{n+2}{5}\,v^2+\&c.$$

When v is positive the formulæ belong to the class of catenarian functions; when v is negative, to the circular ones.

If we put sin pa for $\phi 0$, sin $\overline{p+1}\,a$ for $\phi 1$, and $-$chord a for v, we obtain

$$\sin(p+n)a = -\sin pa\left\{\frac{n-1}{1}-\frac{n-2}{1}\,\frac{n-1}{2}\,\frac{n}{3}\text{ cho } a^2+\ \&c.\right\}$$

$$+\sin(p+1)a\left\{\frac{n}{1}-\frac{n-1}{1}\,\frac{n}{2}\,\frac{n+1}{3}\text{ cho } a^2+\ \&c.\right\}$$

and in thus putting $p=0$

$$\sin na = n\sin a\left\{1-\frac{n^2-1}{1.2}\,\frac{\text{cho } a^2}{3}+\frac{n^2-1}{1.2}\,\frac{n^2-4}{3.4}\,\frac{\text{cho } a^3}{5}-\&c.\right\}$$

And again writing $na = \text{A}$, $a = \frac{\text{A}}{n}$, this takes the form

$$\sin \text{A} = n \sin \frac{\text{A}}{n}\left\{1 - \frac{n^2-1}{1.2}\frac{1}{3}\left(\text{cho}\frac{\text{A}}{n}\right)^2 + \frac{n^2-1}{1.2}\frac{n^2-4}{3.4}\frac{1}{5}\left(\text{cho}\frac{\text{A}}{n}\right)^2 - \&\text{c.}\right\}$$

Now when n becomes indefinitely great, $n \sin \frac{\text{A}}{n}$ becomes A, so also $n \text{ cho} \frac{\text{A}}{n}$, wherefore $\sin \text{A} = \frac{\text{A}}{1} = \frac{\text{A}^3}{1.2.3} + \frac{\text{A}^5}{1.2.3.4.5} - \&\text{c.}$

In order to obtain the series for the cosine we must put $pa = \frac{\pi}{2}$, and therefore $\sin (p+1)a = \cos a$, which gives

$$\cos na = -1\left\{\frac{n-1}{1} - \frac{n-2}{1}\frac{n-1}{2}\frac{n}{3}\text{ cho } a^2 + \&\text{c.}\right\}$$

$$+ \cos a\left\{\frac{n}{1} - \frac{n-1}{1}\frac{1}{2}\frac{n+1}{3}\text{ cho } a^2 + \&\text{c.}\right\}$$

and if, in this, we substitute for $\cos a$, its value $1 - \frac{1}{2}\text{ cho } a^2$,

$$\cos na = 1 - \frac{n}{1}\frac{n-2}{2}\text{ cho } a^2 + \frac{n-1}{1}\frac{n}{2}\frac{n+1}{3}\frac{n-4}{4}\text{ cho } a^4 \ \&\text{c.}$$

whence, proceeding as before,

$$\cos \text{A} = 1 - \frac{\text{A}^2}{1.2} + \frac{\text{A}^4}{1\ldots.4} - \frac{\text{A}^6}{1\ldots..6} + \&\text{c.}$$

4. Note on the Bifilar Magnetometer. By J. A. Broun, F.R.S. Communicated by Professor Tait.

5. Addition to the paper "On the establishment of the Elementary Principles of Quaternions," by G. Plarr,—published in Vol. XXVII. of the Transactions of the Society. Communicated by Professor Tait.

6. Note on Mr Muir's Solution of a "Problem of Arrangement." By Professor Cayley.

This note has been printed along with Mr Muir's paper, *ante*, p. 382.

7. Preliminary Note on a New Method of Investigating the Properties of Knots. By Professor Tait.

As we cannot have knots in two dimensions, and as Prof. Klein has proved that they cannot exist in space of four dimensions, it would appear that the investigation of their properties belongs to that class of problems for which the methods of quaternions were specially devised. The equation

$$\rho = \phi(s),$$

where ϕ is a periodic function, of course represents any endless curve whatever. Now the only condition to which variations of this function (looked on as corresponding to *deformations* of the knot) is subject, is that *no two values of* ρ *shall ever be equal* even at a *stage* of the deformation. Subject to this proviso, ϕ may suffer any changes whatever—retaining of course its periodicity. Some of the simpler results of a study of this novel problem in the theory of equations were given,—among others the complete representation of any knot whatever by three closed plane curves, non-autotomic and (if required) non-intersecting.

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

Robert A. Macfie, Dreghorn, Colinton.
William Stirling, Sc.D., M.D.

Monday, 21st May 1877.

Professor KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. On the Cranial Osteology of Rhizodopsis, and on some points in the Structure of Rhizodus. By Dr R. H. Traquair.

2. Notice of Recent Earthquake Shocks in Argyleshire in 1877. By David Stevenson, Civil Engineer.

Two earthquake shocks have lately occurred in Argyleshire of so decided a character that a description of their effects, as observed at four of the Lighthouse stations on the west coast of Scotland, will, it is thought, be interesting to the Society.

The first shock occurred on the 11th March, and was observed at the Lighthouse station of Hynish in the island of Tyree, and at Sound of Mull, near Tobermory, the distance between the two places being about 34 statute miles.

The report from Tyree states:—"On the 11th current (March), at half-past 11 o'clock A.M., a smart shock of earthquake was felt all along the island; a great many people both heard the noise and at the same time felt the earth to tremble. It was heard and felt very distinctly at the station." Bar. 30·18 at 9 A.M.

That from the Sound of Mull says:—"On the 11th, at 11.30 A.M., this district was visited by a smart shock of earthquake. It began by a rumbling noise like distant thunder. When the noise was at its height the houses, and everything about them, shook, and the slates on the roof rattled. The shaking was not of long duration, but the noise was heard a considerable time before and after the trembling of the earth." Bar. 29·92 at 9 A.M.

The second shock, which seems to have been more severe, took place on the 23d April, and was observed at the island of Phladda, off Easdale, and at Lismore, at the eastern entrance to the Sound of Mull, the distance between the two stations being about 73 statute miles.

The report from Phladda states:—"At 3.40 A.M. the Principal Keeper on the watch felt a severe shock of earthquake. The tower and dwelling-houses shook very much. All the neighbouring islands felt it at the same time." Bar. 29·74 at 9 A.M.

At Lismore the lightkeeper describes the effect as follows (the lighthouse clock had been under repair):—"I beg leave to report that on the morning of the 23d, at 3.30 A.M., while I was standing on the grating inside the lightroom I felt a heavy shock on the tower, with a strange rumbling sound of noise which lasted some seconds, and made everything in the lightroom shake at an alarming rate. It awoke all the inmates of the dwelling-houses. Mr M'Leod jumped out of bed, thinking the tower had fallen, but afterwards thought it was a peal of thunder. I do not think it was thunder. I saw no lightning, and the wind was light at the time. There is no damage done to anything about the station so far as I can see." Bar. 29·43 at 9 A.M.

These observations are valuable because of their trustworthiness as coming from wholly independent observers, and because few more sensible earthquake shocks have, so far as I know, been observed in Scotland. It is also remarkable that they do not appear to have been felt at any other lighthouse stations, although there are several others in the immediate vicinity. A record of them may therefore be useful to those engaged in seismic investigations.

3. Additional Remarks on Knots. By Professor Tait.

The author, in laying before the Society a revised and condensed version of the various papers recently communicated by him, took occasion to make some additional remarks. Of these only one need be given here. He pointed out that another fundamental term is requisite besides those already used viz., *Knots* and *Links*. For three endless cords may be inseparably entangled with one another, or locked together, even if no one of them be knotted and no two interlinked.

Monday, 4th June 1877.

DAVID STEVENSON, Esq., C.E., Vice-President, in the Chair.

The following Communications were read:—

1. On the Structure and Relations of the Genus Holopus. By Sir C. Wyville Thomson, F.R.S.

(*Abstract.*)

The "Challenger" Expedition had no opportunity of visiting Barbadoes, and this I regretted greatly, as Sir Rawson Rawson, who was at that time governor of the island, had paid great attention to the marine fauna, and was anxious to introduce us to his fine collection, which included many specimens of the rare and

remarkable forms for which the sea of the Antilles is famous. However, although the "Challenger" was not there in person, on her return Sir Rawson Rawson most kindly and liberally placed the finest of his specimens at my disposal for examination and description; and it is through his liberality that I now have it in my power to exhibit the very singular creature which is the subject of these notes. In 1837 M. Alcide d'Orbigny described and figured in the "Magasin de Zoologie," under the name of *Holopus*, a new recent genus of fixed Crinoids; and as the description of this distinguished palæontologist indicated an undescribed form of great interest, it was speedily reproduced in the "Annales des Sciences Naturelles" and in Wiegmann's "Archiv."

The specimen described by D'Orbigny, which was for a long time unique, was brought from Martinique by M. Sander-Rang. It subsequently passed into the possession of M. D'Orbigny, who described it under the name of *H. rangii*. D'Orbigny's account was very clear and intelligible, and his determination was fully borne out by his figures; and in Bronn's "Classen und Ordnungen des Thier-Reichs," published somewhere about 1861, the description and figures are repeated, and a distinct family, Holopidæ, is adopted for the reception of the single species. It is very singular that in the "Historie Naturelle des Zoophytes Echinodermes," by Dujardin and Hupé, published as a volume of the "Suites à Buffon" in 1862, the authors express their opinion that *Holopus* is not a Crinoid, but some totally different thing, probably a Cirriped, and they profess to have been unable to find D'Orbigny's specimen.

At M. D'Orbigny's death his whole museum was bought by the Jardin des Plantes, and in the year 1867, through the courtesy of M. Fischer, I had an opportunity of examining the original specimen there; and although it was in a very dilapidated condition, I had no difficulty in satisfying myself that it was a true Crinoid of a very peculiar type.

Professor Louis Agassiz called at Barbadoes in the "Hassler" in 1873, and he there saw a second specimen of *Holopus* in Sir Rawson Rawson's collection. Professor Agassiz intended to have published a full description of the specimen, which was lent to him for that purpose by Sir Rawson Rawson, but he was prevented from doing so by failing health, and after his death the figures which he had prepared were published by Alexander Agassiz, with a short note by Count Pourtales, in the "Zoological Results of the 'Hassler' Expedition."

During the last few years three specimens of *Holopus rangii* have fallen into Sir Rawson Rawson's hands, and from these we will be able to give a pretty good account of the hind parts. All were brought up on fishermen's lines from deep water off Barbadoes. One is very complete in all important points, wanting only the two "bivial" arms, but retaining the mouth-valves. The second is a little larger; it wants the mouth-valves, and again the bivial arms; and with Sir Rawson Rawson's sanction I boiled this specimen down, to figure and describe the separate parts. The third specimen is quite perfect, the arms closely curled in, in their normal position when contracted; but it is very young, only about 8 m.m. in height. Besides the four examples mentioned I am aware of only another, which I have not yet seen; it was shown at the Philadelphia Exhibition, and was afterwards bought by the Museum of Comparative Zoology at Cambridge, Mass.

Holopus is distinguished from all other recent Crinoids by having the basal plates, and the first and probably also the second radials fused together, forming the wall of a tube-like body-chamber, which is cemented beneath to the foreign body to which the Crinoid is attached, by an irregularly expanded calcareous base. This mode of attachment also occurs in the fossil genus *Apiocrinus*, and in many other forms of the Apiocrinidæ and Cyathocrinidæ, but in these, of course, the cement matter is thrown out at the base of a jointed stem.

The upper portion of this hollow column expands slightly, and its thickened upper border is divided into five strongly-marked facets for the articulation of five arms. Each facet is traversed by a transverse articulating ridge, a little in front of which there is the mouth of the tube which lodges the sarcode axis of the joints, and a little behind its centre there is a somewhat longer aperture which appears to lead into the cancellated structure of the outer part of the wall. There are two large shallow muscular impressions on the surface of the facet on the proximal aspect of the transverse ridge. These facets, I conclude, represent the upper surfaces of second radials, but if so, they differ from the second radials of all other recent Crinoids in being connected with the radial axillaries by a true muscular joint instead of by a syzygy. The alternative is that they may be the upper articulating surfaces of the first radials, in which case the next joints may be formed of the second and third radials coalesced, and the syzygy between them obliterated; or, finally, there may be only two radials. There is no trace of any further division of the wall of the column, and the cavity is continued contracting gradually to the bottom. A vertical mark, sometimes a groove and sometimes a ridge, runs from the centre of each articulating facet down the inside of the wall for about two-thirds of the depth of the cavity, when it is lost. The upper border of the cup, bearing the facets, is very irregular in thickness; and in all the specimens which I have seen, including D'Orbigny's, one side of the border is much thicker and considerably higher than the other side, and the three arms articulated to it are much larger than those articulated to the opposite side. There is thus a very marked division into "bivium" and "trivium," and consequently a bilateral symmetry underlies the radiated arrangement of the antimeres. Singularly enough, the specimen described by D'Orbigny was abnormal, only four arms being developed, a circumstance which no doubt greatly conduced to the doubt with which its determination as an echinoderm was received.

Five "radial-axillary" plates, three larger and two smaller, articulate by corresponding ridges and muscular impressions with the facets of the border of the cup, and each of these bears distally two facets, sloping outwards and downwards, for the insertion of the first brachials. The outer surfaces of the radial axillaries are very gibbous, thrown out into almost hemispherical projections, studded with low tubercles; a deep groove runs up the centre of the inner aspect of the joint, and the two sides send inwards very strong projecting processes, which abut against the corresponding processes of the contiguous joints on either side, and lock into them by a system of corresponding ridges and grooves, so that there appears to be little or no motion between these joints.

The radial-axillaries are each succeeded by two series of about eight similar, thick, wedge-shaped brachial joints, very convex externally, and giving off laterally on each side of the arm alternating, very flat broad pinnules each consisting of about six plate-like joints. The brachials are also provided with strong lateral processes forming a wall on either side of the radial groove, and the sides of adjacent series of these first eight arm-joints are marked with corresponding grooves and ridges, so that, although from the presence of articulating ridges of varying degrees of obliquity, and of muscular impressions; the proximal portions of the arms must be capable of some motion, that motion would appear to be slight. After about the eighth the joints suddenly contract in size and become greatly compressed, and this narrow series extends to about sixteen in number, gradually tapering to the end of the arm.

At the bases of the arms, just above the edge of the cup, five thick calcareous bosses, each composed of the contiguous lateral processes of two radial-axillary joints, project interradially into the cup; and opposite these five rather large triangular plates, meeting in the centre of the disk, form a low pyramid covering the mouth. The oral plates are interradial, and the spaces between them radial corresponding with the arm-grooves.

D'Orbigny described the animal as possessing no anal opening, and this is probably the case, but the material is still too scanty to admit of the full examination of a complete specimen of the skeleton, and the soft parts are unknown.

All the specimens of *Holopus* which have been hitherto procured are in a very peculiar condition; the thick-walled foot, and massive, somewhat rudely shaped cup and arm-joints are formed of a loose spongy calcified areolar tissue deeply stained with a black-green pigment. There is no appearance of any separate organic matter, either on the outer surface of the skeleton, which is very delicately sculptured like shagreen, or on the articulating surfaces of separated plates; indeed, the whole body is so perfectly hard and rigid that at first sight I thought it might be semi-fossil. It is without doubt recent, but I suspect that the tissues are very imperfectly differentiated, almost protoplasmic. When an arm is put into boiling water it falls into pieces at once, the joints simply coming asunder, and showing no trace of muscular or other organic connection except the axial cords of the joints, which sometimes keep two joints hanging in connection for a little.

Holopus is thus specially characterised among living Crinoids by the absence of an articulated stem or its representative the centro-dorsal plate; by its viscera being lodged in a hollow peduncle with a continuously calcified wall; and by the absence of an anal opening.

In 1846 Professor Steenstrup described under the name of *Cyathidium*, a genus of fossil Crinoid from the chalk of Faxoe, which occupies a debatable position between the base of the Eocene Tertiaries and the top of the Cretaceous series. The only portion yet described of *Cyathidium* is a deep cup or tube with a spreading base of attachment and a thickened rim with articulating facets for five arms. The cup of *Cyathidium* is somewhat more symmetrical and coralloid than that of the recent West Indian form; but I see no distinction between them of generic value, and I think we must accept *Holopus* as another of the links which recent investigations have made so numerous between the faunæ of later geological periods and that of the present time.

2. On the Diurnal Oscillations of the Barometer.—Part II. By Alex. Buchan, M.A.

In this communication the author stated that he limited his remarks on the present occasion to some of the more prominent results he has arrived at in the course of this investigation. The paper itself, with the tables, will be submitted when the computations have been finished and thoroughly revised,—a work which must necessarily yet take some considerable time.

It is proposed that Part II. consist chiefly of tables showing the arithmetic mean values of the hourly variations of the different months of the year, with remarks on the more evident conclusions which may be drawn from them. The number of places for which data of more or less completeness have now been obtained exceed 130, situated in different parts of the globe. To these it is proposed to add the results of observations made at sea, chiefly those made by the "Challenger" and "Novara" expeditions.

As regards temperate regions, such as Great Britain, periods of no more than three years' observations give only the broadest characteristics of the diurnal barometric curve. From 20 to 25 years will probably be found to be required to show the peculiarities of the curve with such completeness as to exhibit the variations impressed on the curve by season and by geographical position, particularly as regards masses of land and extended sheets of water. In lower latitudes a comparatively short time is required; but even here, where a general regularity in the phenomena is perhaps the most striking fact in the meteorology of equatorial regions, the variations which do occur from year to year ought to be carefully observed from their important bearing on the whole theory of the movements of the atmosphere. The summer months of the northern hemisphere, as regards the diurnal oscillations of the barometer, that is the period when the influence of the sun is at the maximum as regards its effects on these phenomena, are May, June, and July, and the winter months, November, December, and January, both corresponding with the sun's declination; that is, the effects are not cumulative, as in the case of the temperature of the air or that of the sea, by which the critical periods are retarded from one to two months.

Among the many interesting features of the curves which were pointed out may be noted the enormous influence of latitude and of land and sea respectively in determining the amount and time of occurrence of the different phases of the oscillations, and a diagram was exhibited showing the curves of a large number of places, from which it appeared that as regards the summer the A.M. maximum occurs at any time from 6–7 A.M. to 2 P.M., and the P.M. minimum from 3 to 8 P.M.—the stations selected showing a regular gradation between these extremes, a gradation dependent on geographical position. The tendency of assimilation of the curves for certain elevated stations and those for strictly sea-side stations was pointed out, and attention was drawn to the striking fact that the summer curves of inland stations within lat. 30° N. and S. essentially differed from those of higher latitudes—a difference which the varying declination of the sun with season failed to obliterate.

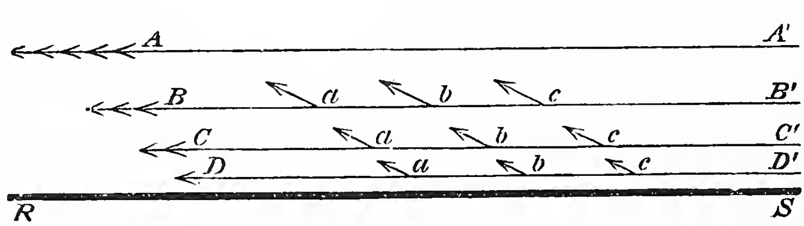
An examination of the different theories yet propounded shows that none of them are in accordance with the facts which have been collected. It would not be difficult by a proper selection of stations to bring proof in support of any of these theories. The truth is, however, that as more facts are obtained the difficulty of framing a satisfactory theory can scarcely be said to be materially lessened, even though it becomes easy to arrive at a close approximation to the curves of a place from its geographical position alone, before determining the curves by working them out numerically.

An examination of these curves by the harmonic analysis and a similar examination of the temperature, hygrometric, wind, magnetic, and electric curves, will in all likelihood be required before the true theory of the diurnal barometric oscillations can be stated.

3. On the Air dissolved in Sea-Water. By J. Y. Buchanan.

4. Why the Barometer does not always indicate real Vertical Pressure: A continuation of the Paper laid before the Royal Society of Edinburgh in July 1875, in which, in addition to several other points, this was attempted to be shown. It is now more fully written out. By Robert Tennent.

The barometer only indicates real pressure when the atmosphere is in a state of perfect rest. It may then be represented a existing in vertical columns, but when it moves over a resisting surface its lower surface currents will be greatly retarded, while those aloft will move comparatively free and unimpeded. In this state it may be represented as moving in columns inclined in the direction towards which it moves. The atmosphere may thus be conceived as being divided into a number of spheroidal concentric layers, each of which is possessed of a different rate of speed, and moves more rapidly than the one beneath it: an increasing amount of friction will take place betwixt the layers as they approach the surface where its influence is greatest. What takes place may be represented in this way. Let R S be the resisting surface, and let



A A′, B B′, C C′, and D D′ represent different layers of air moving at different rates of speed. Those being most rapid where the arrow heads are most numerous. Let these layers also exhibit equal masses of air, but of different volumes, which increase in size from the ground upwards, accompanied also by much greater mobility, as shown by Tyndall in his experiments at Chamouni and the summit of Mount Blanc. An important point is this. The surface current D D′ has only a horizontal source of supply from the direction D′ from which it is fed, while the upper current A A′ has not only a horizontal source of supply from A′, but it also derives supply from the slower moving current B B′ beneath it, which will be drawn upwards, or "lifted," in the direction of the small inclined arrows *a b c d.* To enable this upper current to supply itself in this way, while it possesses the same amount of mass as that beneath it, it must have a greater velocity, and therefore greater momentum than that of the current beneath it, from which it draws its supply. Each of the lower currents, as they approach the surface, are also supplied in the same manner, but in a decreasing ratio, from those beneath them, until the lowest layer is reached, which is that which is most retarded, not only owing to its proximity to a resisting surface, but also to the scarcity of supply, which can now only be derived from a horizontal source, and not from beneath, as was the case with those above it. A tendency in the air to accumulate aloft will now take place, by "lifting," in the direction of the small inclined arrows *a b c d.* Pressure is thus diminished at the surface, while it is abnormally increased higher up.

The effect of this will be that the surface barometer will exhibit diminished or fictitious pressure, while the real weight of the atmosphere remains unaltered. A partial vacuum is found on the lee-side of a wall, over the top of which a strong wind blows: it withdraws the air there to such an extent, that it causes removal to exceed restoration, but it does not affect the real vertical mass of the column overhead except to an infinitesimal degree. In this, as in the former case, real pressure cannot be ascertained by the surface barometer. It is only to be obtained from the result of an observation of a series of barometers placed vertically above each other, and not very far apart. From the results thus obtained, it would be found that the normal upward diminution of pressure which takes place when the atmosphere is at rest would be greatly altered when its upper portion is in rapid motion.

Retarded surface currents and rapid upper currents which move in inclined columns and produce these effects, can only be found with an imperfect fluid, and on a resisting surface, into which the element of friction enters. On a frictionless surface this could not take place. The atmosphere would there move in vertical, not in inclined columns, no "lifting" would take place, pressure would be real or statical; its upward diminution would be normal, as when it is at rest, and horizontal movement would not take off vertical pressure.

Barometric pressure must hence be regarded in two points of view—1st as being a cause; and 2d, as being an effect. When, as in the first case, it is real or statical, it operates as a cause due to gravitation, which is unresisted. When, as in the second instance, there is an introduction of the dynamical element, surface gravitation is diminished by "lifting," and it must then to a certain extent be regarded as an effect. The practical conclusion from this is obvious. On weather charts the constant rise and fall of the barometer, which is there reported, *is to a large extent simply due to the passage of air over a resisting surface.* Over a surface devoid of friction these mechanical effects would be entirely removed: its rise and fall would be greatly reduced, and might be considered as being solely dependent on the effects of heat and vapour. The gradients and isobars which are represented by these movements of the barometer would consequently require also to be similarly corrected.

The barometer does not indicate the real weight of the atmosphere, it only exhibits the amount of its elasticity, from which its real weight can only be deduced when the dynamical element of motion does not enter into its currents. The two cases above mentioned may illustrate this point. The surface barometer there indicates fictitious pressure, or in other words, *the amount of pressure due to the elasticity of the air, but not to that of its real weight*, which is there diminished by "lifting," and as lifting can increase or diminish in amount, so also can the elasticity of the air, while its real weight remains unaltered.

As a general rule, in the British Isles, equatorial winds are accompanied by these rapid upper movements, while polar winds move with a greater uniformity in the velocity of their various layers, and sometimes even those on the surface move more rapidly when copiously supplied from a vertical source. There is thus a *remarkable difference in their mode of inflow.* Equatorial winds, as they increase in force, are hence accompanied by "lifting" and a fall of the barometer. Polar winds are not attended by "lifting," and if their supply is copious and partly from a vertical source, their increase in force is accompanied by a rise of the barometer.

The range of the thermometer is equally great, both above and below its mean; but with the barometer the extent of its range above the mean is not more than one-half of that which takes place when it is below it. When it is below the mean, equatorial winds generally prevail, which are accompanied by lifting and extensive range. When above the mean, polar winds prevail, which are not attended by lifting or such extensive fluctuations. Hence, as a general rule, equatorial winds exhibit fictitious or dynamical pressure, while polar winds possess more nearly, real or statical pressure, being less accompanied by rapid upper currents, and by the mechanical oscillations due to the passage of air over a resisting surface.

Observations of a general description and illustrations will probably be afterwards introduced to exemplify the above conclusions.

It is to this *difference in the mode of inflow* that an attempt has been made to explain the causes why depressions move in an easterly direction.

5. Laboratory Notes. By Professor Tait.

(*a*) On an Effect of Heat on Electro-Static Action.

By means of a very delicate galvanometer, transient currents were detected when one of two plates of the same, or of different, metals, separated by a sheet of mica or glass, was suddenly heated.

(*b*) On Dr Blair's *Scientific Aphorisms* in connection with the Ultra-Mundane Particles of Le Sage.

Accident has recently called my attention to a work entitled *Essays on Scientific Subjects*, by Robert Blair, Regius Professor of

Practical Astronomy in the University of Edin. (Edin. 1818). In the University Library there is a second edition of a part of the same work with the title *Scientific Aphorisms* (Edin. 1827). I bring them before the notice of the Society, as they contain an explanation of gravitation, &c., almost identical with that of Le Sage, to which our attention was lately recalled by our President. Professor Blair seems to have invented this explanation for himself—because, though he gives frequent references to other authors, whose results he quotes, he makes, so far as I have seen, no reference to Le Sage.

On a future occasion I may enter on a discussion of the points of resemblance and difference of these two theories.

5. Note on an Identity. By Professor Tait.

Whatever be p and q it is obvious that

$$\frac{1}{p} = \frac{1}{q} + \frac{q-p}{q} \cdot \frac{1}{p}$$

Hence

$$\frac{1}{p} = \frac{1}{q_1} + \frac{q_1-p}{q_1}\left(\frac{1}{q_2} + \frac{q_2-p}{q_2} \cdot \frac{1}{p}\right)$$

and so on. Finally we see that

$$\frac{1}{p} = \frac{1}{q_1} + \frac{q_1-p}{q_1} \cdot \frac{1}{q_2} + \frac{q_1-p}{q_1} \cdot \frac{q_2-p}{q_2} \cdot \frac{1}{q_3} + \ldots\ldots$$

$$\ldots + \frac{q_1-p}{q_1} \cdot \frac{q_2-p}{q_2} \ldots \frac{q_{n-1}-p}{q_{n-1}} \cdot \frac{1}{q_n} + \frac{q_1-p}{q_1} \cdot \frac{q_2-p}{q_2} \ldots \frac{q_n-p}{q_n} \cdot \frac{1}{p} .$$

absolutely without any restriction on the values of the quantities involved.

It is obvious that an immense number of curious results in the form of sums of series, &c. can be derived with great ease from this expression and from various modifications of it. I give, therefore, only a few very simple examples.

Take q_1, q_2, &c., as the first n of the natural numbers, and the series becomes

$$\frac{1}{p} = 1 - \frac{p-1}{2} + \frac{p-1}{2} \cdot \frac{p-2}{3} - \ldots .$$

$$(-)^{n-1} \frac{p-1}{2} \cdot \frac{p-2}{3} \ldots \frac{p-n-1}{n} (-)^n \frac{p-1}{1} \cdot \frac{p-2}{2} \ldots \frac{p-n}{n} \frac{1}{p},$$

whence at once the sum of the first $n+1$ terms of the expansion of $(1-1)^p$ is seen to be

$$(-)^n \frac{p-1}{1} \cdot \frac{p-2}{2} \cdots \cdot \frac{p-n}{n} .$$

We obtain merely the same result if we take q_1, q_2, &c., as any set of consecutive whole numbers; but from the theorem itself it is easy to obtain the equality,

$$\frac{p}{r}\left\{1 + \frac{p+r}{r+1} + \frac{p+r}{r+1} \cdot \frac{p+r+1}{r+2} + \cdots + \frac{p+r}{r+1} \cdots \frac{p+s-1}{s}\right\}$$

$$= \frac{p+r}{r} \cdot \frac{p+r+1}{r+1} \cdots \frac{p+s}{s} .$$

Next, write the general identity as follows:—

$$\frac{1}{p} = \frac{1}{q_1} + \frac{p}{q_2}\left(\frac{1}{p} - \frac{1}{q_1}\right) + \frac{p^2}{q_3}\left(\frac{1}{p} - \frac{1}{q_1}\right)\left(\frac{1}{p} - \frac{1}{q_2}\right) + \cdots$$

$$+ \frac{p^{n-1}}{q_n}\left(\frac{1}{p} - \frac{1}{q_1}\right) \cdots \left(\frac{1}{p} - \frac{1}{q_{n-1}}\right) + p^{n-1}\left(\frac{1}{p} - \frac{1}{q_1}\right) \cdots \left(\frac{1}{p} - \frac{1}{q_n}\right).$$

If in this we write each letter for its reciprocal we have

$$p = q_1 + \frac{q_2}{p}(p - q_1) + \frac{q_3}{p^2}(p-q_1)(p-q_2) + \&c.,$$

of which a particular case is the curious formula

$$p = 1 + 2\left(1 - \frac{1}{p}\right) + 3\left(1 - \frac{1}{p}\right)\left(1 - \frac{2}{p}\right) + \cdots$$

$$+ n\left(1 - \frac{1}{p}\right)\left(1 - \frac{2}{p}\right) \cdots \left(1 - \frac{n-1}{p}\right) + p\left(1 - \frac{1}{p}\right) \cdots \left(1 - \frac{n}{p}\right).$$

Another is

$$1 = \cos\theta + \cos 2\theta(1-\cos\theta) + \cos 3\theta(1-\cos\theta)(1-\cos 2\theta) + \ldots$$

$$+ \cos n\theta(1-\cos\theta)\ldots(1-\cos(n-1)\theta)$$

$$+ (1-\cos\theta)(1-\cos 2\theta)\ldots(1-\cos n\theta) ,$$

of which a very interesting case is given by $n\theta = 2\pi$.

As a final example we have the singular formula,

$$\frac{1}{x-y} = \frac{1}{x} + \frac{y}{x(x+1)} + \frac{y(y+1)}{x(x+1)(x+2)} + \ldots\ldots \&c.$$

whence it follows that, subject to the introduction of the remainders as above (which vanish if the series are extended to infinity, and if $x > y$),

$$\left(\frac{1}{x} + \frac{y}{x(x+1)} + \frac{y(y+1)}{x(x+1)(x+2)} + \ldots\right)\left(\frac{1}{x} - \frac{y}{x(x+1)} + \frac{y(y-1)}{x(x+1)(x+2)} + \ldots\right)$$

$$= \frac{1}{x^2} + \frac{y^2}{x^2(x^2+1)} + \frac{y^2(y^2+1)}{x^2(x^2+1)(x^2+2)} + \ldots\ldots$$

By another application of the formula we may easily obtain finite expressions for the sum of the series of which two successive terms are

$$\frac{y(y+1)\ldots\ldots(y+r-1)}{x(x+1)\ldots\ldots(x+s-1)}, \text{ and } \frac{y(y+1)\ldots\ldots(y+r)}{x(x+1)\ldots\ldots(x+s)}.$$

I obtained the first expression above by integrating *by parts* a power such as x^{p-1}, but the following mode of obtaining it shows at once its nature.

Let there be a number of independent events, A, B, ... N, whose separate probabilities are $\alpha, \beta, \ldots \nu$. Then the chance that one at least of them occurs is

$$1-(1-\alpha)(1-\beta)\ldots\ldots(1-\nu).$$

But we may obtain another expression for the same result by writing the chance that any one (say A) occurs, adding to that the chance that another (say B) occurs while A does not occur, then that C occurs and neither A nor B, &c. This gives

$$\alpha+\beta(1-\alpha)+\gamma(1-\alpha)(1-\beta)+\ldots\ldots$$

Equating these two expressions we get an identity which is easily transformed into that first given.

But its truth is much more easily seen if we write α' for $(1-\alpha)$, &c., when the last given form becomes

$$1-\alpha'\beta'\gamma'\ldots\ldots = 1-\alpha'+(1-\beta')\alpha'+(1-\gamma')\alpha'\beta'+\ldots\ldots$$

which is an obvious truism. The method seems well worth the attention of any one with leisure and some analytical skill.

July 24.—Mr Muir has kindly given me a reference to Crelle, vol. xii. p. 354, where it is stated that the above identity in one of its forms is in Schweins' "Analyse," p. 237. This work I have not seen. Mr Muir adds that no developments or applications of the theorem are made.

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

1. James Stevenson, 4 Woodside Crescent, Glasgow.
2. James Roberton, LL.D., Professor of Conveyancing, 1 Park Terrace East, Glasgow.
3. George A. Panton, 24 Bennet's Hill, Birmingham.
4. Isaac Bayley Balfour, Sc.D., 27 Inverleith Row.
5. Sir Daniel Macnee, 6 Learmonth Terrace.
6. William Pole, Mus.Doc., Memb. Inst. Civil Engineers, 31 Parliament Street, Westminster.

PROCEEDINGS

OF THE

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APPENDIX.

(The MSS. of the following papers were unfortunately mislaid, so that the titles only were printed in last year's "Proceedings.")

3. The Thermo-Electric Properties of Cobalt. By Messrs C. G. Knott, J. Gordon MacGregor, and C. Michie Smith.

(Read 5th June 1876.)

As we could not find pure cobalt for sale in the state of foil or wire suitable for thermo-electric experiments, we resolved to prepare it by electrolysis of chloride of cobalt in the way recommended by Mr G. W. Beardslee of Brooklyn, N.Y.* For this there is required pure chloride of cobalt of a certain strength to form the electrolyte, and spongy metallic cobalt to form the positive electrode. By the kindness of Dr Crum Brown these were prepared with very great care in the chemical laboratory by Mr Robinson, to whom we are much indebted. The following note of the method of preparation employed has been supplied by Mr Robinson:—

"Oxide of cobalt was dissolved by heat in hydrochloric acid; the acid solution was treated with ammonia in great excess, the precipitate at first formed being then dissolved; the ammoniacal solution of chloride of cobalt was then oxidised by passing clean, dry air through it till the colour was no longer altered, *i.e.*, till the purple colour was succeeded by a reddish-brown, and till it admitted of precipitation by hydrochloric acid. This was done.

* *Les Mondes*, vol. xxxv. p. 206.

The precipitate purpureo-cobaltic chloride was collected on a filter, well washed with very dilute hydrochloric acid, and dried carefully, great care being taken to avoid contamination by iron. The dried purpureo-cobaltic chloride was then reduced in a current of hydrogen in small reducing crucibles, and fine skeletons of spongy cobalt were obtained. 26 gms. of cobalt were made in this way; the remainder of the purpureo-cobaltic chloride was heated to expel chloride of ammonium, dissolved in hydrochloric acid and water, and when evaporated to dryness, yielded 32 gms. of chloride of cobalt. This was made up to 900 c.c. with distilled water."

Having obtained these, some experiments were then made as to the substitution of aluminium for the carbon-electrode used by Mr Beardslee, and as to the strength of current required. The aluminium was found to answer admirably. These preliminary experiments over, we set about preparing such a bar as would serve us instead of a wire in our thermo-electric work. For this end we took a strip of aluminium about 100 mm. long, and 8 mm. broad, carefully covered it with wax on one side, and used it as the negative electrode. To form the positive electrode a piece of glass tube, 10 mm. wide, and 20 mm. long, was taken and closed at one end, through which a platinum wire was passed; the one end of this was connected with the positive pole of the battery, while the other end was coiled into a spiral inside the tube, into which some of the spongy cobalt was tightly packed so as to make good contact with the wire. These electrodes were placed in a porcelain bath containing some of the chloride of cobalt solution, rendered very slightly alkaline by the addition of ammonia. The battery found most suitable consisted of two large Bunsen cells; when less was used the deposition went on too slowly, while pieces prepared with a stronger battery-power were incoherent. The action was usually allowed to go on for two or three days, at the end of which time the metal had a considerable thickness. The cobalt was separated from the aluminium by boiling it in a strong solution of caustic soda. The pieces of cobalt thus prepared were of very great hardness and compactness, being with difficulty filed.

It was with these pieces of cobalt that the thermo-electric observations were made. The first observations showed that the cobalt line lay very far down on the thermo-electric diagram, while subsequent observations showed that it lay below all the lines formerly obtained. Hence it lies in a region in which it is very difficult accurately to determine its position. This difficulty is increased by the circumstance that the line which lies nearest it, namely, that of nickel, is not a straight line. Another difficulty in carrying out the experiments arose from the shortness of the cobalt bars, but this was in a great measure overcome by the use of a slight modification of the apparatus devised for the determination of the thermo-electric properties of sodium and potassium, formerly described to this Society.* The following is a brief account of the experiments and their results:—

In the beginning of March one of the bars was ready for use. Preliminary observations carried on with hot oil having shown that no marked peculiarities existed within the temperature of boiling oil, a quadruple junction of M, N,† nickel and cobalt was formed and heated by means of a red-hot iron cylinder. The nickel-cobalt deflections obtained from this experiment, when plotted in terms of the M-N deflections, gave a line showing clearly the nickel peculiarities, and supplied us with an approximate position for cobalt.

On the opening of the laboratory for the summer session experiments with the hot cylinder were resumed, and a quadruple junction of M, N, palladium, cobalt, was used. This was connected with the galvanometer in such a way that the deflections due to the thermo-electric pairs Pd-Co, M-N, M-Pd, could be read separately, and in rapid succession. The M-N junction supplied us with a temperature scale as in former experiments, and the lines got by plotting the other deflections in terms of this gave an easy method of fixing the relative positions of cobalt, palladium and M, and hence of fixing the position of cobalt on the thermo-electric diagram." Owing to the much greater electro-motive force in the Pd-Co and M-Pd circuits than in the M-N circuit, a resistance of 192 ohms had to be inserted between the palladium wire and the galvanometer. The M-Pd curve presented the usual well-known parabolic

* Proc. Roy. Soc. Edin. 1873–4, p. 350.

† Trans. R. S. E. 1872–73, p. 138.

appearance without any marked peculiarity, but the Pd-Co curve, though parabolic at comparatively low temperatures, showed at the higher temperatures an apparent tendency to straighten, thus indicating a possible point of contrary-flexure, *i.e.*, a bend in the cobalt line at a temperature somewhere about or above a bright red heat. The approximate position of the cobalt line may be gathered from these facts: that the line of 15° C is divided by the points of intersection of the cobalt, palladium and M lines in the ratio of 11 : 8, and that the cobalt line is inclined to the horizontal at an angle not quite double of the angle of inclination of palladium.

In experiments made to-day with a double circuit of Co-Pd against nickel, with some resistance thrown into the cobalt arm, a neutral point has been got at a temperature of about 500° F. This shows us that by the aid of cobalt we shall be able to investigate the peculiarities of nickel more accurately, while from the parts of the nickel line which are already known we shall be able to fix definitely the position of the cobalt. It is worthy of notice that even at the high temperatures of the red-hot or even white-hot cylinder the cobalt shows no tendency to oxidise.

Electric Conductivity.—During the last few days we have been carrying on a series of experiments on the change of the electric conductivity of cobalt with change of temperature. For this we prepared a very narrow strip of cobalt by depositing it on the edge of a piece of aluminium. To the ends of the strip so prepared stout copper wires were soldered, and the experiments were then carried on as in the case of nickel (Proc. Roy. Soc. Edin., 1875–6, p. 120). The line obtained was not straight but slightly curved, showing apparently a gradual alteration in the rate of change of conductivity. Within the temperature of boiling oil, however, there is no indication of a sudden change, and the curve in the line may be due to several disturbing causes which we have not as yet been able to eliminate.

5. Notice of some Recent Atmospheric Phenomena. By Professor Tait.

(Read 5th June 1876.)

On March 25th, at 8.15 P.M., very few clouds being visible, there appeared a luminous arch in a vertical plane, passing *true* E. and W. nearly through the zenith. This arch, to my surprise, gave everywhere a continuous spectrum. I had, in company with Professor Dewar, observed a phenomenon similar in all respects (continuous spectrum included) some seven years before, and was then inclined to fancy the arch a mere low-lying cloud, and its light due to the town lamps. But on the present occasion, after about forty minutes, when the arch had disappeared, an aurora, showing unmistakably in the spectroscope the characteristic green band, appeared near the magnetic north. And a few days afterwards there appeared in the Edinburgh newspapers several descriptions of a similar arch seen at almost exactly the same time at Colinton, &c., and even so far off as Dunfermline. Most of the observers remarked the entire absence of coruscations as a proof that the phenomenon could not be auroral. The facts stated seem, however, to leave no doubt that it was connected with the aurora, and the peculiarity of its spectrum remains to be explained.

On May 10th, the sky cloudless but hazy, from 11 A.M. till nearly 3 P.M., I observed a magnificent halo of 22°, with its tangent arcs forming almost a complete external oval. Various observers in different parts of Scotland seem to have noticed the halo itself from 2 P.M. till sunset, but none have mentioned the external oval, which at midday was brighter than the halo.

As showing that the ice-crystals in the air were very widely spread about this period, I append an extract from a letter I have just received from Mr Aitken of Darroch.

"I have sent you a sketch I made of an atmospheric phenomenon which I saw at Cadenabbia, Lago di Como, on Sunday the 30th April, and hope it may be useful to you to-night.

"The weather during the previous day was very bad; it rained heavilv all day. The wind was northerly, and the temperature low. In the evening there was a thunderstorm, accompanied by a good deal of lightning. Sunday morning broke clear and fine;

without a cloud in the sky, almost no wind, and barometer high. At 10.45 A.M., when I first noticed the phenomenon, the sky was covered with a very thin uniform white haze, and no clouds visible.

"When first observed, the large circle which passed through the sun was complete, but only the upper half of the small circle which surrounded the sun was visible. There was no colour in the large circle, but the small circle was fringed with colour, the outside being of a bluish-violet tint, the inside reddish. In the centre of the band, between the two colours, no colour could be distinguished; it looked pure white. On each side of the small circle there was a short strip of colour, cutting the large circle at an acute angle. This short band was visible on the white band of the large circle, and only extended to a short distance on each side of it. The small coloured circle was never very perfect, only the top part remained steadily brilliant, and at this part the colours were more brilliant than at any other part. The sides were particularly always rather undefined.

"For part of the time, while the phenomenon lasted, there was an extraordinary change in its appearance. There appeared to be two circles of nearly the same diameter surrounding the sun, neither of which was concentric with the sun, and were separated from each other horizontally, as shown in the lower of the two sketches. While this was the case the circles were never complete, only little more than the upper halves of them being visible. Another curious point was that the coloured circle did not seem a perfect circle. The horizontal axis seemed longer than the vertical; but of this I am not certain, as I had no instruments to take any measurements. The only means of observation I had was a circular piece of paper I hurriedly cut, and held between my eye and the sun, in such a position that the sun shone through a small hole in the centre. When this disc was held at such a distance from the eye that the upper part of the coloured circle was just seen past the edge, the lower part and the sides were seen to project some distance beyond it.

"About 11.5 A.M. the haze began to collect into clouds, and the phenomenon ended at 11.20. As the day advanced, clouds continued to form, and rain began to descend at 2 P.M. Towards evening it became very wet, and it continued to rain every day for more than a week."

1. On New and Little-known Fossil Fishes from the Edinburgh District. No. III. By R. H. Traquair, M.D., F.G.S.

(Read 7th May 1877.)

Elonichthys ovatus, sp. nov. Traquair.

Of this I have only seen one specimen, from the limestone of Burdiehouse, and preserved in the Edinburgh Museum of Science and Art.

Description.—Allowing for the anterior part of the head, which is deficient, the entire length of the specimen to the extreme point of the upper lobe of the caudal fin would be about $5\frac{5}{8}$ inches; the greatest depth of the body in front of the dorsal fin is $1\frac{1}{2}$ inch. The distance from the origin of the pectoral fin to that of the ventral is a little over 1 inch, to opposite the commencement of the dorsal $1\frac{1}{4}$ inch, to opposite that of the anal $1\frac{1}{2}$ inch, and to opposite that of the lower lobe of the caudal nearly 3 inches. The general form of the fish is thus remarkably short, deep, and ovoid, and its general appearance does not indicate that its peculiar form is due to *post mortem* distortion or change.

Nothing can be said regarding the osteology of the head, which is hopelessly crushed, and its anterior part cut off by the edge of the stone. The scales are of moderate size, and ornamented externally, like those of *E. striolatus*, with delicate punctures and oblique furrows, the latter mostly observable towards the anterior margins. The paired fins are small, the pectorals only equalling $\frac{3}{4}$ inch in length, the ventrals being a little shorter; the median fins, on the other hand, are very largely developed. The dorsal and anal fins contain about 35 rays each, as nearly as can be ascertained. The former fin is the larger, and is very high and acuminate in front, the length of its longest ray being $1\frac{1}{2}$ inch, while its base equals 1 inch in extent. The anal fin commences opposite the posterior third of the dorsal; it is not so acutely pointed, for while the length of its base is, as in the dorsal, 1 inch, that of its longest rays is the same; its posterior margin is also a little more concave. The principal rays of these two fins have their transverse joints rather longer than they are broad. As usual, they begin to dichotomise towards their extremities; this process, however, occurs proportionally sooner in the shorter rays behind. The caudal is very powerful;

inequilobate. The anterior rays of the lower lobe have, for a considerable extent of their length, their transverse articulations more distant than those of its other rays and of the whole upper lobe, in which the joints are very short.

Remarks.—This species closely resembles *E. striolatus*, but is distinguishable from it by the very short and deep form of the body, and the more largely developed median fins, of which the dorsal is especially striking, from its great size. Although fully aware of the danger, in dealing with Palæozoic fishes, of defining or founding species upon proportional measurements, yet, seeing that, as already observed, there seems to be no evidence that the present specimen has undergone any material change of form by *post mortem* processes, I have, after due consideration, thought it best to bestow upon it a new specific designation.

Geological Position and Locality.—From Burdiehouse, in limestone of the calciferous sandstone series.

Elonichthys Bucklandi, Agass. sp.

Pygopterus Bucklandi, Agassiz, "Poissons Fossiles," vol ii. pt. 2, p. 77 (1835).
,, ,, Hibbert (Agassiz), Trans. Roy. Soc. Ed., vol. xiii. pp. 216–217, pl. vii. fig. 2 (1834–35).

I have not seen the original type of this species, figured by Dr Hibbert in his celebrated memoir on the Burdiehouse limestone, and stated by Agassiz to be in the collection of the Royal Society of Edinburgh. This may, indeed, have been a mistake, and the specimen may possibly have been in the private collection of Dr Hibbert, now unfortunately dispersed. From Hibbert's figure, however, and the very brief notice of this species in the "Poissons Fossiles," I feel pretty confident in referring to it a number of mostly fragmentary remains of a large Palæoniscoid fish from Burdiehouse contained in the Edinburgh Museum of Science and Art.

The only entire specimen I have seen is $11\frac{1}{2}$ inches in length. It is, most unfortunately, crushed on its back. It displays, however, the right pectoral and ventral fins. The former has its principal rays articulated throughout; the ventral is of moderate size. The median fins are large. The dorsal is not shown in any of the specimens belonging to the Edinburgh Museum, but in Dr Hibbert's figure it is seen to resemble the anal, and is evidently placed nearly opposite the interval between that fin and the ventrals, although the latter are not shown in the figure. The anal is, however, well shown in one specimen; it is large, triangular, and acuminate, and is closely followed by the caudal, which is very powerful. The fin rays are externally ganoid and finely striated; their transverse articulations are very close; the fulcra are closely set, and minute for the size of the fish. The scales of the body are proportionally small. Those of the front part of the body are apparently nearly equilateral, but posteriorly, and more especially towards the ventral margin, their form is low and narrow. Their anterior covered area is very narrow; the posterior margin is very finely denticulated. The exposed area is covered with a delicate yet sharply-defined ornamentation, consisting of fine sub-parallel ridges which pass from before backwards across the scale in a gently sigmoid direction, tending to become intermixed with punctures posteriorly, especially above the diagonal between the two acute angles of the scale. Towards the tail the ridges become less marked on the posterior part of the scale, giving way to the thickly dotted punctures, till, on the caudal body-prolongation, the former, after lingering at the anterior margin, altogether disappear, and punctures alone remain.

Very little can be made out concerning the bones of the head; however, in the above mentioned entire specimen the lower jaw is seen to be very stout, and ornamented externally with fine, sharp, closely set, wavy, branching, and anastomosing and interrupted ridges running in a longitudinal direction. The laniary teeth are very strong, incurved, and smooth, with apical enamel cap; similar teeth are seen on the maxilla, the dental margin of which is finely tuberculated.

Remarks.—The structure of the pectoral fin, the relative position of the dorsal and anal fins, and the form of the latter, show clearly that this species is not a "*Pygopterus*," to which genus it was referred by Agassiz. On the other hand, its affinities to *Elonichthys striolatus* are unmistakable, though it attains a larger size. In this and in some other respects it resembles considerably the three large species from the Coal-measures of North Staffordshire, which I have recently described as *Elonichthys semistriatus*, *caudalis*, and *oblongus*.*

*Memoirs of the Palaeontographical Society for 1877, "Carboniferous Ganoids," pp. 49–57.

Elonichthys (?) *pectinatus*, sp. nov. Traquair.

Description.—Flank scales about $\frac{1}{3}$ inch broad, and usually a little higher, exclusive of the articular spine; moderately thick. Upper margin with prominent articular spine; anterior covered area narrow; exposed surface brilliantly ganoid, sculptured with oblique, sub-parallel, prominent ridges, occasionally branching, anastomising, and intercalated, and terminating behind in delicate denticulations of the posterior margin; about 7 or 8 of these ridges in the space of $\frac{1}{8}$ inch. Under surface of scale with feebly marked keel; a narrow area along the posterior margin is obliquely grooved, the short grooves terminating between the denticulations of the margin, so as to produce a pectinated appearance. Scales from apparently the ventral aspect are lower than broad, and with more produced anterior-superior angles; others, from their more regularly rhomboidal shape, and scanty development or absence of the articular spine, and more prominent keel of the attached surface, were probably situated more towards the caudal extremity. In all cases the external sculpture is similar, and the under surface displays the same peculiar pectinated appearance at the posterior margin.

Associated with these scales there is on a specimen from Carluke in the collection of Mr Grossart, of Salsburgh, a small fragment of the edge of what must have been a pretty large jaw. This fragment is nearly $1\frac{1}{2}$ inch in length, and displays the stumps of 5 stout conical teeth, with traces of smaller ones external to them; the external surface of the bone is beautifully tuberculated.

Remarks.—Though as yet I only know this species from detached and dislocated scales, often occurring together in masses, and, from the small jaw fragment described above, there can be no doubt as to its distinctness and novelty as a species. In external sculpture these scales bear a considerable resemblance to those of *Acrolepis Rankinei* (*Gyrolepis Rankinei*, Ag.), with which, on superficial examination, they might perhaps be confounded, but from the latter they may be at once distinguished by the narrowness of the anterior covered marginal area, and the grooved pectination of the posterior margin of the internal aspect. Of course, until further information regarding its fins and head be obtained there must be some uncertainty as to the genus to which it belongs. Meanwhile, I have assigned this species to *Elonichthys* on account of the general configuration of the scales. Should this opinion prove correct, it is certainly the largest species of the genus hitherto described.

Geological Position and Localities.—In the blackband ironstone of Gilmerton belonging to the Lower Carboniferous Limestone series; in bituminous shale from a similar horizon at Carluke, in the collections of Dr Rankin of Carluke, and Mr Grossart of Salsburgh; in calcareous shale accompanying the limestone of Levenseat in the Upper Carboniferous Limestone group. I am indebted to Mr Grossart for specimens from the last-named locality.

Rhadinichthys, gen. nov. Traquair.

Palæoniscus (pars), Agassiz.

Body more or less elongated; scales moderate, sometimes rather large, variously ornamented or nearly smooth, their posterior margins serrated. Caudal body prolongation slender. Dorsal fin placed rather far back, commencing only very slightly in front of the anal; the principal rays of the pectoral unarticulated till towards their terminations. Suspensorium very oblique; gape very wide; jaws armed with a row of incurved conical laniaries, outside which there is a series of smaller teeth.

I propose to institute the genus *Rhadinichthys* (ῥαδινός, slender, and ἰχθύς) for the *Palæoniscus ornatissimus* of Agassiz, and a number of other species from carboniferous rocks, many of which have hitherto been undescribed. These fishes differ markedly from *Palæoniscus* in the structure of the pectoral and in the position of the dorsal fin, in these respects approaching *Pygopterus*, from which they are, however, distinguished by their scales being proportionally larger and thicker, the anal n not being prolonged backwards in a fringe-like manner, and the caudal body prolongation being much less powerful. None of the species of *Rhadinichthys* attain large dimensions; most of them are, indeed, fishes of very small size.

This genus is, so far as is yet known, confined to the carboniferous formation, and is abundantly represented in rocks of that age in Great Britain. Judging from the figures given, *Palæoniscus Cairnsii* of Jackson, and some of the other small Palæoniscidæ from

New Brunswick figured but not named by him,* seem to be referable to *Rhadinichthys*, as is possibly also the case with his *Palæoniscus Albertii*, though I regret that I have not seen any specimens of the latter sufficiently perfect to enable me to come to a definite conclusion as to whether or not it may belong to still another genus—most certainly, however, it is not a "*Palæoniscus*" in the sense in which that term ought now to be restricted.

Rhadinichthys ornatissimus, Agass. sp.

Palæoniscus ornatissimus (pars), Agassiz, "Poissons Foss." vol. ii. pt. ii. pp. 92–93 (1835); "Atlas," vol. ii. pl. 10*a*, fig. 6, *but not* figs. 5 and 7.

"*Palæoniscus*" *ornatissimus* was described by Agassiz from three specimens, all of which are figured in the "Poissons Fossiles." Two of these from Burdiehouse are in the collection of the Royal Society of Edinburgh; they are in a very bad state of preservation, nevertheless enough is seen of their structure to convince me that they belong to two different species. The original of fig. 5, tab. 10*a* ("Atlas," vol. ii.), seems to me to be only a peculiarly crushed specimen of *Elonichthys Robisoni*. The other, represented in fig. 6, is of larger dimensions, and though the fins are very imperfectly shown, yet, from the general form of the body, and from the sculpture of the scales and cranial bones, I have no hesitation in identifying with it a number of specimens of a fish, which is not uncommon in the rocks of the calciferous sandstone series of the neighbourhood of Edinburgh. The specimen represented in fig. 7 of the same plate of Agassiz's work does not seem to me to belong to the same species, and, unfortunately, there seems now to be little hope of tracing the original to its present possessor, as it belonged to the late Mr Jamieson Torrie, whose collection was, after his death many years ago, sold by public auction and dispersed. As type of "*ornatissimus*" we are therefore justified in taking the original of fig. 6 of the plate referred to.

Description.—The length of the type specimen, which is so bent round that the tip of the tail nearly touches the snout, is 5 inches; doubtless, its original length was greater, previous to the strange contortion which has thus affected its form. It is, nevertheless, small, the most perfect specimen in the Hugh Miller Collection

* Report on the Albert Coal Mine, New Brunswick.

attaining a length of 8 inches, while another, unfortunately wanting the head, would probably have measured no less than 10 inches had it been entire. The head seems rather large, and would probably be contained about $\frac{1}{5}$ in the total; the greatest depth of the body is at the shoulder, whence it gradually tapers to the tail pedicle, which is rather narrow. The suspensorium is very oblique, the gape extensive; the lower jaw is of a tapering form. Traces of sharp conical teeth are seen in many specimens, and are, as usual, arranged in two sets—internal larger and external smaller. The operculum is large and oblong, with acute anterior-superior and posterior-inferior angles; the interoperculum is of the usual quadrate shape. A specimen from Wardie, obliquely compressed upon its back, shows on the right side a beautiful series of 15 branchiostegal rays, along with the anterior 7 of those of the other side, besides which there is a small median lozenge-shaped plate behind the symphysis of the jaw, the anterior plate of each lateral series being also much broader than the others. All the bones of the head are finely and closely striated.

The scales are rather large on the front of the flank. In a Burdiehouse specimen, which probably measured originally about 10 inches in length, one of these scales measures $\frac{2}{5}$ inch in height by $\frac{3}{10}$ in breadth; they get much smaller posteriorly, and those situated on the belly are rather low and narrow. Their external ornament consists of sharp yet delicate slightly sigmoidally curved ridges, mostly parallel with the superior and inferior margins. In the furrows between these, numerous punctures may be observed, and very often about the centre of the scale the ridges are over a small space nearly obsolete, so that the punctures come more prominently into view. Proceeding towards the tail, the ridges become less and less prominent, especially in the middle of the scale, so that the ornament ultimately appears to consist only of short furrows and punctures, which are most marked towards the anterior and posterior margins. The posterior margin of the scale is finely denticulated; the keel of the under surface is feebly marked in the scales of the front of the body, but posteriorly it becomes more apparent.

The paired fins are rather small, the pectoral attaining only about one-half the length of the head. It consists of about 30 rays, of which the principal ones are, as in *Pygopterus* and *Oxygnathus*, unarticulated till towards their terminations. The ventrals are not well exhibited in any of the specimens, though in two cases portions of them are seen; they seem to have been small, with delicate rays, whose transverse articulations are rather distant. The dorsal and anal fins are moderate, the former being placed far back, and arising only slightly in front of the anal. Both are acuminate, triangular, with considerable concave posterior margins. The base of the anal is slightly more extended than that of the dorsal, and a well-marked interval occurs between its termination and the commencement of the caudal. It is not possible to ascertain the number of rays in those fins. They are rather delicate, and with their transverse joints about twice as long as broad—at least, in the longer rays before bifurcation. When the rays are *in situ* their articulations appear proportionally still more distant, owing to the usual imbrication of the demi-rays. The anterior rays of the lower lobe of the caudal are stouter than those of the rest of the fin, and have their transverse articulations closer, the joints appearing nearly square as seen from the outside; as the lower lobe passes into the upper one the rays become more delicate, and their articulations more distant. The outer surfaces of the rays of the pectoral are nowhere seen, but in the other fins the rays are ganoid externally, and mostly smooth, though showing here and there, especially towards their proximal extremities, traces of longitudinal striation. The fulcra are small, though very distinctly visible under an ordinary lens.

Geological Position and Localities.—The specimens which I have included under the foregoing description are all from the Calciferous Sandstone series of Midlothian, in which the species seems to be widely distributed, though not very abundant as regards number of specimens. It occurs in the Wardie shales, at Wardie (Museum of Science and Art and collection of the author); near Slateford (collection of the Geological Survey of Scotland); near Juniper Green (Mr John Henderson); in the horizon of the Burdiehouse limestone, at Burdiehouse and at South Queensferry. Its occurrence in strata of similar age in Fifeshire is extremely probable, though the specimen from Burntisland, figured as *Palæoniscus ornatissimus* by Agassiz, seems now to be unfortunately lost.

Rhadinichthys ferox, sp. nov. Traquair.

Of this new species there is one tolerably entire example from Wardie in the Hugh Miller Collection in the Edinburgh Museum of Science and Art. A short time ago also the Museum acquired a fragment of the head and anterior part of the body, collected by Mr C. W. Peach, and quite recently another fragment, a portion of the body, was presented by Mr David Grieve. In the following description these specimens will be referred to as Nos. 1, 2, and 3 respectively.

Description.—The specimen in the Hugh Miller Collection has undergone a twist about the middle of the body, and shows, moreover, only a portion of the caudal fin, so that it is difficult to estimate its original length. As it is, it measures $5\frac{3}{4}$ inches in length, but it is quite evident that when alive it must have been considerably longer.

The head equals $1\frac{3}{4}$ inch in length, and shows superiorly a cast of the greater part of the inner surface of the cranial buckler, with the lines of demarcation between the parietal, frontal, squamosal, and post-frontal bones. The opercular bones are not seen, but a portion of the maxilla, and the greater part of the mandible are exhibited, the latter bearing sharp conical teeth of different sizes, those externally placed being very small, while one large laniary, $\frac{1}{10}$ inch in length, is conspicuous. In specimen No. 2 the impressions are seen of the parietal, squamosal, opercular, præopercular, post-temporal, and supra-clavicular bones, and of the posterior part of the maxilla, these impressions clearly showing that the external sculpture of the bones in question was of a highly ornate character, consisting of sharply defined, flexuous, branching, anastomising, and interrupted ridges, tending to pass into tubercles at the inner margin of each parietal.

The scales are of moderate size on the flanks, becoming rather small posteriorly, while on the belly they are low and narrow. Over the whole body their outer surfaces are highly ornate, the ornamentation consisting of fine sharply defined ridges, forming a pattern which is usually, as it were, divided by a diagonal passing from the anterior-superior to the posterior-inferior angle of the scale. On the anterior-inferior half of the scale the ridges run parallel with the anterior and inferior margins, impinging posteriorly on the diagonal, while on the posterior-superior half they pass obliquely downwards and backwards, mostly parallel with the diagonal, ending in the denticulations of the posterior margin ; in many cases they are interrupted before reaching that margin, and are replaced by other shorter ones springing up between them. In some cases, also, a few of the ridges of the upper half, near the anterior-superior angle of the scale, likewise impinge upon the diagonal, consequently meeting those of the lower half at acute angles.

The pectoral fin is well displayed in specimen No. 1, in which it measures rather more than 1 inch in length. It is of considerable expanse, and consists of at least 30 rays, of which the stronger ones, the first 15 counting from the lateral margin, are unarticulated till towards their terminations, where bifurcation sets in. The ventrals are not shown. Portion of the dorsal is seen in No. 1, in which it seems to arise opposite the anterior part of the anal, and its entire contour is exhibited in No. 3. It is triangular and acuminate, very high in front; its rays, which cannot be counted, are rather delicate, and their transverse articulations considerably distant. In No. 1 the anal is seen to be of large size, and of the same triangular acuminate form as the dorsal. Its base is $1\frac{1}{10}$ inch in extent, and its longer anterior rays seem to have exceeded that measurement, though they are cut off at the apex ; the fin is, moreover, badly preserved, and its anterior margin somewhat distorted and injured. In the same specimen the origin of the lower lobe of the caudal is also shown, consisting of exceedingly closely set deeply imbricating rays, of which I count at least 40 up to where they begin to pass into those of the upper lobe, but here the tail is unfortunately truncated by the edge of the nodule. Interspinous bones of great strength are seen supporting this part of the caudal fin.

Remarks.—This is a most distinctly-marked species, differing obviously from *R. ornatissimus* in the proportions and sculpture of the scales, and in the large size of the fins. I know of no other species with which it can at all be confounded. The first time I saw the most perfect of the three specimens described above, I was struck with the general resemblance which it bore to the Permian genus *Pygopterus*, but the larger proportional size of the scales, and the anal fin not being prolonged backwards, indicate that its place is in *Rhadinichthys.*

Geological Position and Locality.—In ironstone nodules from the shales of Wardie, near Edinburgh, belonging to the Calciferous Sandstone series.

Rhadinichthys lepturus, sp. nov. Traquair.

I am indebted to Dr Paterson, of Leith, and to Mr R. Etheridge, jun., for the loan of two specimens of a small *Rhadinichthys* from Grange Quarry, Burntisland, which appears to belong to a new species. With these I can identify a detached tail recently found by myself in the same locality.

Description.—The length of Dr Paterson's specimen is $3\frac{1}{2}$ inches, allowing for the extremity of the snout, which is deficient. The depth of the body, midway between the head and the dorsal fin, is $\frac{11}{12}$ inch; that of the tail pedicle at the commencement of the lower lobe of the caudal is $\frac{7}{24}$ inch. It is, however, just possible that the anterior part of the fish may be a little shortened up by one of the common forms of distortion, as, on the other hand, the whole shape of the other specimen belonging to Mr Etheridge certainly is unnaturally attenuated by a similar cause. Nevertheless, we have before us a small fish of a fusiform shape, elegantly tapering posteriorly to a very narrow tail pedicle, the dorsal and anal fins being nearly opposite each other, and a well-marked interval occurring between the latter and the commencement of the caudal, which is rather small and of a delicate appearance. Nothing can be said regarding the head, which is hopelessly crushed. The scales are of medium size, becoming rapidly smaller posteriorly. Those of the anterior part of the body have their outer surfaces entirely covered by a delicate sculpture consisting of fine furrows and ridges passing across the scale, and ending in the denticulations of the posterior margin, those of the upper and posterior part of the area being more oblique than those of the lower and anterior. On the posterior part of the fish the scales are seen only from their inner or attached surfaces. The origin of the pectoral fin is seen in the second specimen referred to, but it is in very bad preservation. Returning to Dr Paterson's specimen, a few rays of the ventral are seen, but the real size and shape of the fin are not shown. The dorsal and anal fins are moderate, and of the same form, triangular, concavely cut out behind, with delicate rays, whose transverse joints are considerably longer than they are broad. The dorsal commences only very slightly in front of the origin of the anal, and an interval, equal to $\frac{2}{3}$ the length of the base of the anal, occurs between its posterior termination and the commencement of the lower lobe of the caudal.

The last-named fin is unusually delicate in appearance, the upper lobe only very slightly exceeding the lower in length, with a slender body prolongation. In the lower lobe the transverse joints of the principal rays are longer than they are broad, except just towards their terminations.

Remarks.—In the contour at least of the hinder part of the body this species strongly resembles *R. carinatus*, save that the caudal fin is more delicate—the greater apparent depth of the body in front of the dorsal fin may be, as already indicated, perhaps accounted for by *post mortem* distortion. The sculpture of the scales, which is very different from what we find in *R. carinatus*, reminds us of *R. ornatissimus*, from which, however, the present species is obviously distinguished by the greater delicacy of the rays of the lower lobe of the caudal, with their more distant transverse articulations, as well as the entire aspect of the fin. It is, however, very possible that the imperfect specimen from Burntisland referred by Agassiz to "*Palæoniscus*" *ornatissimus* (Poisson's "Fossiles Atlas," vol. ii. pl. 10*a*, fig. 7), and in which the tail is deficient, may belong to *R. lepturus*, but his figure being by no means good, it is impossible to decide, in the absence of the original specimen.

Geological Position and Locality.—In calcareous shale overlying the "Burdiehouse" limestone exposed in Grange Quarry, Burntisland (Calciferous Sandstone series).

Rhadinichthys Geikiei, sp. nov. Traquair.

Through the kindness of Professors Ramsay and Geikie I have enjoyed the opportunity of examining a number of small specimens of Palæoniscidæ collected by the Geological Survey from the horizon of the Wardie shales near Redhall, among which there is at least one beautiful little species of *Rhadinichthys* hitherto undescribed. This I have much pleasure in dedicating to Professor Geikie, to whom I am much indebted for cordial assistance in prosecuting the study of Scottish Palæozoic ichthyology.

Description.—The specimens are rather fragmentary, except two in which the entire fish is shown. Of these the larger has undergone an unfortunate twist, but its length may be estimated at $3\frac{1}{4}$ inches; the other, measuring only $2\frac{1}{8}$ inches, seems to me to be a younger example of the same species. The body is fusiform, the head rather large, being contained about 4 times in the total. The cranial bones are beautifully ornamented with delicate wavy ridges, running mostly in a longitudinal direction. The jaws are armed with sharp conical teeth of different sizes. The scales are of moderate size, gradually diminishing posteriorly, though those on the flanks are not of specially large dimensions. The flank scales are nearly equilateral in shape; the free surface of each is ornamented with 5 or 6 pretty well-marked oblique ridges, running downwards and backwards, and terminating in the denticulations of the posterior margin. Below there are a few more delicate striæ parallel with the lower margin. Along the back, between the head and the dorsal fin, the scales are smaller, and the ornamentation takes the form of a few sharply-marked striæ passing diagonally across the scale between the two acute angles. Posteriorly, as the scales become smaller, the ridges are fewer, and the ornamentation becomes more and more feebly marked, till on the tail pedicle the scales are nearly smooth, and entirely so on the caudal body prolongation, which is delicate. Some amount of individual variation occurs in the scales of different specimens, which I see no good reason for referring to different species. In two fragments representing portions of rather larger fishes than the rest, the scales of the back, between the head and the dorsal fin, are rather smoother, while those of the flank show a greater number of ridges and denticulations of the hinder margin; on the other hand, in the smallest specimen of the series, the ridging on the dorsal scales is very well marked, and rather feeble in those of the flanks.

The pectoral fin is shown in one specimen, but not well, being a little obscured by a thin film of the matrix; the ventrals are visible in none. The dorsal and anal fins are situated nearly opposite each other, the anal commencing only a little further back. The entire contour of these fins is not clearly exhibited, but their rays are smooth, delicate, and slender, with distant transverse articulations. The caudal is inequilobate, its rays are likewise delicate and smooth, and with distant articulations; the caudal body prolongation is slender.

Remarks.—In its general proportions *R. Geikiei* is not so slender as most species of the genus, and there is not the usual interval between the anal and caudal fins; in these respects it agrees with

R brevis, from which it may, however, be distinguished by the nature of the ornamentation on the scales and cranial roof bones.

Geological Position and Locality. From the Wardie shales in the Calciferous Sandstone series. The specimens were collected by Mr James Bennie, of the Geological Survey of Scotland, in a very fissile shale exposed near Redhall, to the west of Edinburgh, in a cutting made for the Caledonian Railway loop line to Juniper Green and Midcalder.

Rhadinichthys brevis, sp. nov. Traquair.

Description.—The length of the most perfect specimen in my collection is $3\frac{1}{2}$ inches; the extremity of the caudal fin is not, however, preserved. Another specimen, more badly preserved posteriorly, must have been a little longer, judging from the size of the head and body. The head is elegantly shaped, with bluntly pointed muzzle. The opercular bones are rather small; the jaws pretty stout; the dentition is not visible. The bones of the cranial roof are sculptured, with contorted and rather flattened rugæ; those of the face are ornamented with sharp and closely set wavy ridges, which, on the operculum, pass obliquely downwards and backwards over its surface. The scales are of moderate, indeed, rather small size, and though, of course, diminishing gradually towards the tail, those in the flanks are not marked by any very special excess in size. The flank scales are nearly equilateral; the posterior margin of each shows 5 or 6 prominent oblique denticulations; the outer surface shows a few feebly marked, nearly obsolete ridges, passing obliquely backwards and a little downwards, and not very regularly placed. Immediately behind the clavicle, and above the origin of the pectoral fin, the sculpture is, however, more strongly pronounced, the ridges being finer, sharper, and closer together, but posteriorly the scales soon become nearly quite smooth. The paired fins are rather small, the principal rays of the pectoral are clearly seen to be unarticulated till towards their terminations; the ventrals are situated midway between the pectorals and the origin of the anal, their rays are delicate, and their transverse joints appear at least three times as long as broad. The dorsal and anal fins are large, and situated nearly opposite each other, the former commencing, as usual in this genus, slightly in front of the latter. Both have the same form, being high and acuminate in front, and concavely cut out behind; their rays are rather coarser than those of the ventrals, and their transverse joints are considerably longer than broad. In no case is the caudal fin well preserved, its rays being more or less disjointed and broken up, but it is clear that the prolongation of the body in the upper lobe was delicate, the rays slender, and with distant articulations.

Remarks.—As already indicated, this species agrees with *R. Geikiei* in the general form of the body, and, along with it, differs from all the other species of the genus with which I am acquainted in the closer approximation of the anal fin to the caudal; in both, also, the flank scales are comparatively small. But the action of the scale sculpture is essentially different. In *R. brevis* such ridges as are seen on the flank scales have not the same short, sharp, straight, and parallel character which distinguishes those which in *R. Geikiei* pass to the denticulations of the posterior margin; the rugæ ornamenting the cranial roof bones are also coarser, more flattened, and less capable of being described as "striæ."

Geological Position and Localities..—From the Calciferous Sandstone series. Five specimens are in my possession, four of which are in ironstone nodules from Wardie beach; the fifth I found in a detached block of slaty carbonaceous ironstone, on the shore near the mouth of the Kenly Burn, between St Andrews and Kingsbarns.

Rhadinichthys carinatus, Agassiz sp.

Palæoniscus carinatus, Agassiz; "Poissons Fossiles," vol. ii. pt. i. p. 104 (1835); "Atlas," vol. ii. tab. 4*b*, figs. 1 and 2.

The form of the body is slender, the length of the head being contained about $4\frac{3}{4}$ times in the total up to the bifurcation of the caudal fin, and equalling the greatest depth of the body at the origin of the ventral fins. The head is very elegantly shaped, rather depressed above, but pointed when seen in profile. The bones of the cranial shield are ornamented with sharp, delicate, wavy ridges or "striæ." A specimen from Wardie in my collection shows most clearly the presence of extensive ossification in the periotic and alisphenoid regions of the cranium, and, moreover, a cast of the posterior semi-circular canal of the ear is most distinctly and beautifully preserved. The opercular bones are rather small; the jaws remarkably slender and delicate; the external ornament of the facial bones consists, like that of the cranial shield, of delicate wavy ridges. No specimen I have as yet seen affords, however, the smallest trace of teeth. The paired fins are rather small. The pectoral is in no case well preserved, but there is sufficient evidence that its longer rays were not articulated till towards their terminations. The ventrals are still smaller, and are equidistant in position between the pectorals and the commencement of the anal. The median fins are, on the contrary, rather large in proportion to the size of the fish. The anal commences rather in front of a point equidistant between the origin of the ventrals and that of the lower lobe of the caudal; it is high and acuminate in front, and pretty deeply cut out behind; the length of its base is equal to that of its longest rays. The dorsal is similar in shape to the anal, and commences only slightly in front of the origin of the latter. The caudal is pretty large, and deeply cleft, the body prolongation along the upper lobe being, however, comparatively delicate. The rays of all these fins are slender, ganoid, and smooth externally; their articulations tolerably distant; the fulcra on their anterior margins obvious, though minute.

The scales are remarkable for their large size on the flanks, while on the belly they suddenly became very low and narrow; they diminish rapidly in size posteriorly, and on the tail pedicle are rather small. Externally they are brilliantly polished and nearly smooth; on the flank scales, however, a few shallow furrows extend a little way forwards from the posterior margin, which is, as usual, sharply denticulated.

Remarks.—The original specimen, collected by Lord Greenock at Wardie, and figured by Agassiz, is in the collection of the Royal Society of Edinburgh. It is very imperfect, showing no fins save the mere origins of the anal and caudal, the former of which seems to have been overlooked by Agassiz. On the other hand, I cannot, by the most careful examination, verify his statement regarding the presence of small teeth, "*en brosse*" upon the jaw. I am indebted to Professor Turner for the loan of a specimen from Cornceres, in Fifeshire, which belonged to the late Professor Goodsir; it is possible that this may be the "*Catopterus fusiformis*" which Mr

Robert Walker states was described by Professor Goodsir in 1838 before a meeting of the Literary and Philosophical Society of St Andrews, and which, according to Mr Walker, he characterised as closely allied to *Palæoniscus*, but differing from it "in wanting the scaling or false rays along the anterior ray of the fins, and also in the dorsal fin being opposite the anal."* In this specimen the dorsal is, indeed, nearly opposite the anal (as in others of the same genus as well as species), but, on close examination, fulcra are distinctly observable. I have not been able to obtain Professor Goodsir's description, or even to ascertain exactly where it is to be found, but as it appears to have been published only in a provincial newspaper, the generic name *Catopterus* must remain with the Triassic semi-heterocercal Lepidosteids, to which it has been applied by the Messrs Redfield.

Geological Position and Localities.—From the Calciferous Sandstone series. In ironstone nodules from the shore at Wardie, near Edinburgh ; on the shore near Pittenweem, in Fifeshire, in the collection of Mr J. W. Kirkby ; in a compact grey limestone at Cornceres near Kilrenny, collected by the late Professor Goodsir.

Rhadinichthys tenuicauda, sp. nov. Traquair.

Length, about 2½ to 3 inches ; form slender, narrowly tapering posteriorly. Head bones sculptured externally, with closely set flexuous ridges ; dentition not visible. Scales rather large for the small size of the fish, and especially large on the sides of the abdomen, becoming smaller on the back and towards the tail. The posterior margins of the scales are denticulated, their external surfaces nearly smooth along the sides of the body and on the tail pedicle, but along the back, especially between the dorsal fin and the head, they are marked by a few rather prominent oblique ridges ; a similar ornamentation, though rather finer and closer in character, makes its appearance also on the scales along the ventral margin. The pectoral fin is small ; the non-articulation of its stouter rays seems to prevail over a less extent of the fin than is usual in this genus. The ventral is still smaller. The dorsal and anal are of medium size, and nearly opposite each other ; their rays are fine

* Trans. Geol. Soc. of Edinburgh, vol. ii. pt 1. (1872), p. 124.

and distantly jointed. Between the anal and caudal fins there is a considerable interval. The caudal is inequilobate, with attenuated body prolongation ; its rays are slender, smooth, and with distantly placed articulations. The fulcra are very obvious in all the fins, notwithstanding the small size of the fish.

Remarks.—This species is allied to *R. carinatus* in general form, and in the large size of the scales of the flank, but differs obviously from it in the general larger proportional size of the scales, in the ornate character of those along the dorsal and ventral margins, and in the greater delicacy of the fins, and smaller number of their rays. The head seems also to be larger in proportion to the size of the fish.

Geological Position and Localities.—A fish of the middle or "Edge Coal" group of the Carboniferous Limestone series, the specimens above described being from Wallyford, near Tranent, in the collections of Mr D. J. Brown ; and from Possil, near Glasgow, in the collection of Messrs James Thomson and John Young, F.G.S. To the kindness of these gentlemen I am indebted for the opportunity of describing this most distinct and interesting little form.

On the Cranial Osteology of *Rhizodopsis,* and on some points in the Structure of *Rhizodus.* By Dr R. H. Traquair.

(*Abstract.*)

Read May 21, 1877.

This paper contains an account of the cranial structure of *Rhizodopsis* as far as this can be elucidated by an examination of a suite of specimens from the Coal Measures of Fenton, Staffordshire, in the collection of Mr Ward of Longton, supplemented by additional specimens from various other localities.

The cranial shield of *Rhizodopsis* resembles in general form and structure that of the Saurodipterini, though the surface of the bone is elaborately sculptured instead of presenting the smooth glistening aspect characteristic of the external plates and scales of the last named group. The posterior part of the shield is composed of two elongated *parietals*, and having on its outer margin two smaller plates, anterior and posterior, which may be considered as the *post-frontal* and *squamosal* respectively. The anterior or fronto-ethmoidal

portion forms the depressed rounded snout, the front margin of which above the mouth, is formed by two small dentigerous *præmaxillary* bones entirely differing in form from the bones described as such by Messrs Hancock and Atthey. The gape is wide, the *hyomandibular* bone being slightly inclined backwards; there is apparently no *symplectic.* The form of the *maxilla* is well known; the structure of the *mandible* is exceedingly complex. Its dentary element is found to correspond with the bone interpreted by Messrs Hancock and Atthey as "*præmaxilla,*" but turned in a contrary direction, *i.e.,* with its toothed margin upwards instead of downwards as supposed by them; it bears the anterior laniary tooth and the outer row of small teeth. The laniaries posterior to the front one are borne on separate *internal dentary* ossicles, the presence of which is clearly proved by a portion of a jaw of a large specimen of *Rhizodopsis* in the Edinburgh Museum. The external aspect of the lower jaw is completed by at least two *infra-dentary* plates situated below the inferior margin of the dentary, behind which there is another covering externally the articular and angular region of the jaw, and which is probably the equivalent of the *angular* of other ganoids. On the internal aspect of the mandible there is, besides the internal dentary or laniary ossicles, a well-marked *splenial.* The orbit is very anteriorly placed; as in *Megalichthys,* three plates cover the cheek behind the suborbitals, corresponding to the single large one in *Osteolepis.* The author does not find Professor Young's statements that the jugular plates of *Rhizodopsis* are "in two pairs, principal and posterior," and that there is "no trace of median or of lateral plates," corroborated by the specimens under examination. On the other hand, he finds *one pair* of large *principal* jugulars with at least four *lateral* ones on each side, as well as very distinct evidence of the presence of a median plate behind the symphysis of the jaw. The shoulder girdle is provided with well-developed infraclavicular plates.

The lower jaw of *Rhizodus Hibberti* is found to have essentially the same structure as that of *Rhizodopsis.* The dentary element is of the same form, and bears the anterior large laniary tooth and the outer range of small teeth, the posterior laniaries being borne on separate ossicles, which are sometimes found entirely detached.

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Preliminary Report of the U.S. Geological Survey of Wyoming and contiguous Territories. Conducted by F. V. Hayden, 1870. 8vo. Washington, 1871.—*From the United States Geological Survey.*

Report of the U.S. Coast Survey, showing progress during 1869, 1870, 1871, 1872, 1873.—*From the United States Government.*

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Washington.—Ninth Census of the United States. Statistics of the Population.—Statistics of Wealth and Industry. 3 vols. 4to. 1872.—*From the United States Government.*

Medical Essays compiled from Reports to the Bureau of Medicine and Surgery, by Medical Officers of the U.S. Navy. 8vo. 1872.—*From the United States Government.*

The Practical Use of Meteorological Reports and Weather Maps. 8vo. 1871.—*From the United States Government.*

Tables and Formulæ useful in Surveying, Geodesy, and Practical Astronomy, including Projection of Maps. 8vo. 1873.—*From the United States Government.*

Directions for Water Level and Meteorological Observations, and the Registry of Periodical Phenomena. 8vo. 1871. —*From the United States Government.*

Reports of Explorations in 1869–72 of Colorado River of the West and its Tributaries, by Professor J. W. Powell. 8vo.—*From the United States Government.*

Reports of Explorations and Surveys to ascertain the Practicability of a Ship Canal between the Atlantic and Pacific Oceans by the way of Tehuantepec. 4to. 1872.—*From Sir Charles Hartley.*

Report on the Difference of Longitude between Washington and Ogden, Utah, by J. R. Eastman. 4to. 1876.—*From the U.S. Observatory.*

Papers relating to the Transit of Venus in 1874, prepared under the direction of the Commission authorised by Congress. Part 2. 4to.—*From the U.S. Naval Observatory.*

Investigation of Corrections to Hansen's Tables of the Moon; with Tables of their application. 4to. 1876.—*From the U.S. Observatory.*

Besides the donations above mentioned, the Society has received from the United States Government many important works, comprising Journals and Reports of Congress and its Committees, Reports issued by the various Departments of the State, and miscellaneous Papers relating to the civil, military, and naval administration of the United States. These works, though too numerous to specify here, are duly entered in the General Catalogue of the Library.

Wellington.—Statistics of New Zealand. 1874, 1875. Folio.—*From the New Zealand Government.*

Meteorological Report for 1875, including Returns for 1873–74, and Abstracts for previous years. 8vo.—*From the Geological Survey Department.*

Ninth, Tenth, and Eleventh Annual Reports of the Colonial Museum of New Zealand. 8vo.—*From the Geological Survey Department.*

Transactions and Proceedings of the New Zealand Institute. Vols. I.–IX., 1868–76. 8vo.—*From the Institute.*

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Vierteljahrsschrift der Naturforschenden Gessellschaft zu Zürich. Jahrg. XIX., XX. 8vo.—*From the Society.*

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PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. IX. 1877–78. No. 100.

Ninety-Fifth Session.

Monday, 26th November 1877.

Sir WILLIAM THOMSON, President, in the Chair.

The following Council were elected:—

President.
Sir WILLIAM THOMSON, Knt., LL.D.

Honorary Vice-Presidents, having passed the Chair.
His Grace the DUKE of ARGYLL.
Sir ROBERT CHRISTISON, Bart., M.D.

Vice-Presidents.

Rev. W. Lindsay Alexander, D.D.	Principal Sir Alex. Grant, Bart.
David Stevenson, Memb. In. C.E.	David Milne Home, LL.D.
The Right Rev. Bishop Cotterill.	Sir C. Wyville Thomson, LL.D.

General Secretary—Dr John Hutton Balfour.

Secretaries to Ordinary Meetings.
Professor Tait.
Professor Turner.

Treasurer—David Smith, W.S.

Curator of Library and Museum—Dr Maclagan.

Councillors.

Dr Ramsay H. Traquair.	Rev. R. Boog Watson.
Thomas Harvey, LL.D.	Dr Hugh Cleghorn.
Professor John G. M'Kendrick.	Professor T. R. Fraser.
Professor Crum Brown.	Professor Rutherford.
Professor Kelland.	Dr R. M. Ferguson.
Professor Fleeming Jenkin.	Sheriff Hallard.

Monday, 3d December 1877.

Principal Sir ALEXANDER GRANT, Bart., one of the Vice-Presidents of the Society, read the following Opening Address:—

GENTLEMEN,—I find it recorded* that in the year 1662, which was the first year of the incorporation of the Royal Society of London, the celebrated mathematician, Robert Hooke, drew up "Proposals for the good of the Royal Society," the third article of which was as follows:—"That every member of the Society shall be equally obliged to promote the ends thereof by paying 52s. yearly, and by doing some one duty that shall be charged on him by the Council once a year, or, if his occasions will not permit, to pay 52s. more per annum." This proposed salutary rule does not seem ever to have been enacted by the Royal Society of London, nor do I believe that any analogous article forms part of the statutes of this Society, and yet it is in accordance with the spirit of such a rule that I appear before you this evening.

When the Council of this Society requested me, only four weeks ago, to open the ensuing session by addressing you, I at once, though perhaps imprudently, resolved to obey them, and to do "the one duty charged upon me by the Council." But in the meantime I have become more and more conscious of the fact, that probably no length of preparation would have enabled me to offer you an address worthy of this occasion and of my predecessors in this chair, and that it would be a simple impossibility to accomplish this within a few weeks at a period of the year when the distractions are so manifold that I can scarcely get a clear morning, not to speak of a clear day. I must therefore ask you to accept for the nonce some discourse on matters which are a very old story now. I know that the Royal Society is like those Athenians of whom it was said that they "spent their time in nothing else, but either to tell or to hear some new thing." Therefore, an idea to be suitable for the Royal Society ought to be brand new. But I shelter myself under the reflection, that the topic which has suggested itself to my mind for this evening is one which might perhaps always claim a certain welcome in this room. For I thought of whiling away the opening half-hour of our

* In Weld's *History of the Royal Society*, vol. i. p. 139. To this excellent history the following paper is much indebted.

new session with some remarks upon our connection with him whom I cannot but regard as our lineal progenitor and virtual founder, Francis Bacon of Verulam. In the Harveian Society, I believe that there is an annual eulogy of the great discoverer of the circulation of the blood; and how often in medical societies has the opening address been given up to a panegyric of Hippocrates, the ancient father of medicine? In societies like this, whose main function is the promotion of experimental research, it can never be out of place to do honour to Lord Bacon, the august herald and prophet of modern science. I trust that you will concede me this proposition, and that you will excuse the introduction of a trite subject on the present occasion, and that you will bear with me if I repeat many things which are quite familiar to you.

Principal Forbes, in an excellent address which he delivered in this place fifteen years ago, traces the origin of the Royal Society of London to the example of similar societies which sprang up in Italy during the 16th and 17th centuries, especially the Society of *Lincei*, or Lynx-eyed Ones. founded in 1604, of which Galileo was a member, and the Florentine Academy of Experiment ('del cimento'), founded in 1657. This view was, broadly speaking, correct; but I think that it ignores too much the forces working within England itself. During the 15th and 16th centuries there was carried out the most important revolution (except the introduction of Christianity) that ever occurred among the human race—namely, the overthrow of scholasticism, and the introduction into all departments of knowledge of the modern spirit of inquiry. This revolution was far greater and more important in its consequences than the downfall of any dynasty, or the destruction of any empire; greater and more important than the Renaissance and the Reformation which jointly led to it; far greater in its scope and results than the much-vaunted French Revolution, of which, to the present day, it seems so hard for France to reap the fruits. In contributing to this greatest of all changes, in ushering in the new era, Italy may claim to have played a glorious part. But the new spirit pervaded all Europe. Men of the new era seemed to spring up everywhere. Columbus in Spain, Copernicus and Kepler in Germany, Lionardo da Vinci and Galileo in Italy, Tycho Brahe in Denmark, Gassendi and Descartes in France, Harvey and Gilbert in England,—these and many more, as

if inspired by some special impulse, had each started on a separate road of discovery. They each shone like separate lights, and their united effulgence created the new day. Countries, I think, as well as individuals, moved independently of one another. At all events, I think that England moved independently of Italy in respect of the formation of scientific societies. In the eighth volume of your "Transactions" there is a learned paper contributed by Professor Macvey Napier fifty-nine years ago, in which he collects from writers of the 17th century, evidences of the influence exercised by Lord Bacon's works, and of the high esteem in which they were held. Professor Napier's case is fully made out, and he not only establishes the general fact that Bacon made a deep impression on his own age and the succeeding times, but also he proves, in particular, the immediate and direct connection between Lord Bacon's writings and the foundation of the Royal Society. The original founders of the Royal Society had those writings continually in their thoughts. Bacon's splendid aspirations, clothed in some of the stateliest prose that the world has ever seen, had struck upon men's minds and filled them with enthusiasm. Bacon was not content with setting forth his views in the abstract, but in his *New Atlantis* he exhibited in concrete, but imaginary, form, the benefits which would result, and the state of things which would arise, when the new philosophy had been thoroughly welcomed, and experimental science had been thoroughly established among men. The chief feature in Bacon's undiscovered island of the fancy was "Solomon's House," in which the employments of the Fellows are described as follows:—
"We have twelve that sail into foreign countries, who bring in the books and patterns of experiments of all other parts. These we call Merchants of Light. We have three that collect the experiments which are in all books. These we call Depredators. We have three that collect experiments of all mechanical arts, and also of liberal sciences, and also of practices which are not brought into arts. These we call Mystery-men. We have three that try new experiments, such as themselves think good. These we call Pioneers or Miners. We have three that draw the experiments of the former four into titles and tablets, to give the better light for the drawing of observations and axioms out of them. These we call Compilers. We have three that bind themselves to looking into the experiments

of their fellows, and cast about how to draw out of them things of use and practice for man's life and knowledge. These we call Dowry-men, or Benefactors. Then, after divers meetings and consults of our whole number, to consider of the former labours and collections, we have three that take care, out of them, to direct new experiments, of a higher light, more penetrating into nature than the former. These we call Lamps. We have three others that do execute the experiments so directed, and report them. These we call Inoculators. Lastly, we have three that raise the former discoveries by experiments into greater observations, axioms, and aphorisms. These we call Interpreters of Nature."

Rawley, Lord Bacon's chaplain and literary executor, says in his preface to the *New Atlantis*, which was published in 1627, soon after Bacon's death—"This fable my lord devised, to the end that hee might exhibite therein a modell or description of a college, instituted for the interpreting of nature, and the producing of great and marvellous works for the benefit of men, under the name of 'Solomon's House, or the College of the Six Dayes' Works.'"

Bacon's imaginative conception, being so consonant as it was to the spirit of the age, took root and fructified in the minds of many of his countrymen, both those who were passing through the trying scenes of the civil war in England, and those who had accompanied Charles II. into his exile. The first attempt to realize the conception seems to have been made about nineteen years after Bacon's death, in the year 1645, by the formation in London of a scientific society called the "Philosophical College," which was also called the "Invisible College." Dr Wallis, in his autobiography, speaks of this society as consisting of "divers worthy persons, inquisitive into natural philosophy and other parts of human learning, and particularly of what hath been called the New Philosophy, or Experimental Philosophy."

Several eminent Oxford men who were in London in 1645, owing to the interruption of university work caused by the civil wars, became members of the "Invisible College;" and in 1649, returning to their *Alma Mater*, they founded the Philosophical Society of Oxford, which used to meet in the house of Dr Wilkins, then Warden of Wadham College, and afterwards Bishop of Chester, or in the apartments of the Hon. Robert Boyle, who was then residing in Oxford.

Ten years later, in 1659, the accomplished and philanthropic Evelyn addressed to Robert Boyle a letter containing his proposal for the erection of "a Philosophic-Mathematic College," which letter is full of Baconian inspiration. In it he says that, "since we are not to hope for a Solomon's house in this sad *catalysis* (or dissolution of things), and *inter los armorum strepitus*, a period so uncharitable and perverse, why not might some gentlemen, whose geniuses are greatly suitable, and who desire nothing more than to give a good example, preserve science, and cultivate themselves, join together in society, and resolve upon some orders and economy to be mutually observed, such as shall best become the end of their union?" He then offers cheerfully to devote his fortune to the carrying out of this scheme; and he proceeds to say, in the Baconian manner:—"I propose the purchasing of thirty or forty acres of land in some healthy place not above twenty-five miles from London, of which a good part should be tall wood, and the rest upland pastures or downs, sweetly irrigated. We would erect upon the most convenient site of this, near the wood, our building, viz., one handsome pavilion, containing a refectory, library, and withdrawing-room;" this is the first storey. The second storey was to contain "a fair lodging-chamber, a pallet-room (*i.e.* a second-class bedroom), and gallery, all of which should be well and very nobly furnished, for any worthy person that might desire to stay any time, and for the reputation of the college." There were to be six members of the college, each of whom was to have his cell and a private garden, "somewhat after the manner of the Carthusians." Many details followed as to the mode of life to be pursued in this institution, "the principal end" of which was to be the "promotion of experimental knowledge." Of course, this dream of Evelyn's was never carried out; it is only mentioned as showing the sort of ideas which the study of Bacon had engendered in men's minds, and which finally resulted in the establishment of the Royal Society.

A somewhat similar scheme was published about the same time when Evelyn's letter was being written, by the poet Cowley, under the title of a *Proposition for the Advancement of Experimental Philosophy.* This was to be effected by the creation of a Philosophical College, with a revenue of £4000 a year, and a staff of 64 persons, among whom are included 20 philosophers or professors;

16 young scholars, servants to the professors; a chaplain; a chirurgeon; "2 lungs, or chymical servants,"* and 4 old women to tend the chambers. This college was to examine all existing theories of nature, and to separate the true coins from those "false moneys with which the world has been paid and cheated so long." Secondly, it was to recover the lost inventions of the ancients. Thirdly, it was to improve all arts which we now have, and discover others which we yet have not.

These Baconian imaginings never obtained a literal fulfilment. But in the next year, in 1660, immediately after the restoration of Charles II., they received a modified realisation in the establishment of the Royal Society. It is a point of some little interest that the chief agent in effecting this result was a Scotsman. This was the good Sir Robert Moray, who had accompanied the king in exile, and who, at the Restoration, was made a Privy Councillor, and much consulted on affairs of State. He was an ardent naturalist, and on the king's return to London in June 1660, he appears to have joined the meetings of the "Invisible College," at one of which, in November of the same year, the schemes for a Physico-Mathematical College were discussed, and it was resolved by the members present to give themselves a more definite and regular constitution, "according to the manner in other countries, where there were voluntary associations of men in academies for the advancement of various parts of learning, so they might do something answerable here for the promoting of experimental philosophy." Sir Robert Moray was present at this meeting, and in his intercourse with the king he took occasion to communicate the proposal to His Majesty. At their next meeting, a week later, he was able to state to his friends that the king "did well approve of their design, and would be ready to give encouragement to it." The members at once constituted themselves the "Royal Society," and Sir Robert Moray became their first president *ad interim*. About a year and a half later, in July 1662, the Society received its charter of incorporation. Lord Brouncker, who was a discoverer in mathematics,—"being the first to introduce continual fractions, and to give a series for the quadrature of a portion of the equilateral hyperbola,"—was named president in the Royal Charter. The king continued to take an interest in

* The "lung" was so called because he blew the fire for his master.

the Society, and was often present at their experiments in his gardens, his parks, or on the Thames; and he even condescended to chide them "on the slowness of their proceedings."

In 1663 he presented them with their mace. The charter and the mace are the only substantial favours which Charles II. appears to have conferred upon the Royal Society, and for a long period they worked on, by means of the genuine enthusiasm of the fellows, with no funds at their disposal for scientific purposes. I think it is amusing to observe that Sir Robert Moray so far contrived to give a Scottish character to the Society, that their mace was decorated all over with the thistle, and that St Andrew was chosen as their patron saint.

As soon as the Royal Society was established, its members connected it with the name of Bacon. Bishop Sprat, who published a history of the Society in 1667, writes:—"The Royal Society was a work well becoming the largeness of Bacon's wit to devise, and the greatness of Clarendon's prudence to establish." Oldenburg, the first secretary of the Society, says:—"The enrichment of the storehouse of natural philosophy was a work begun by the single care and conduct of the excellent Lord Verulam, and now prosecuted by the joint undertakings of the Royal Society." Glanvil, in his work called *Plus Ultra, or the Advancement of Knowledge since the days of Aristotle*, after alluding to the *New Atlantis*, says:—"This the great man desired, and formed a society of experimenters in a romantic model; but he could do no more—his time was not ripe for such performances. These things, therefore, were considered by later virtuosi, who several of them combined together, and set themselves to work upon his grand design." Professor Napier quotes several other more obscure authorities, to show the same opinion of Lord Bacon entertained in the minds of Englishmen. One Dr Power, in 1664, in a work on the discoveries of Galileo, Torricelli, and Pascal, calls Bacon "the patriarch of experimental philosophy." Another writer, of the same year, with the unfortunate name of Mr Havers, says that "Lord Bacon's way of experiment, as now prosecuted by sundry English gentlemen, affords more probabilities of glorious and profitable fruits than the attempts of any age or nation whatsoever;" and Dr Childrey entitled his work on the *Natural Rarities of England*, "*Britannia Baconica*," in order to indicate its connection with those studies which Bacon had originated. Lastly, I may perhaps be allowed to quote the well-known lines from the *Ode to the Royal Society*, by Cowley, himself one of the earliest fellows, in which, after detailing the futility of the early philosophy, he says:—

> "From these and all long errors of the way,
>   In which our wandering predecessors went,
> And, like th' old Hebrews, many years did stray
>   In deserts but of small extent,
> Bacon, like Moses, led us forth at last,
> The barren wilderness he past,
> Did on the very border stand
> Of the blest promised land,
> And from the mountain-top of his exalted wit,
> Saw it himself, and showed us it."

Kuno Fischer says that Bacon was an "epoch-making" thinker, who put men into a new attitude towards things, and opened a new world of possibilities. I think that it would be more strictly true to say that Bacon was an "epoch-marking" man, a herald and fugle-man, created and sent forward by his epoch. He was himself unconscious of that; he thought of himself as a solitary worker; he fancied that he derived nothing from others. He mentions his own case as a source of encouragement, and says:—"Some hope might, I think, be afforded by my own example; and I do not say this for the sake of boasting, but because it may be useful. If any feel a want of confidence, let them look at me, who of all my contemporaries have been most engaged in public affairs, who am of somewhat infirm health (which of itself occasions great loss of time), and who in this matter have assuredly been the first explorer, neither following in the steps of another, nor communicating with a single individual, but who, nevertheless, by entering firmly on the right way, and in submitting my mind to *things*, have, methinks, to some extent advanced these matters." It is true that Bacon, in solitary meditation, during the intervals of the law courts and State affairs, built up the fabric of his philosophy, as a poet builds up an epic poem or a drama. In 1605 he published his *Advancement of Learning*, which was meant to be a map of the existing state of human knowledge in all its various provinces, with a note of the facts in which knowledge was deficient, and an exhortation to supply these deficiencies. The whole was to be a prelude to his *Instauratio Magna*, or entire remodelling of the sciences, and was intended especially to catch the mind of James I., who was, however, of too pedantic an intellect, and too much absorbed in theology, to rise to the level of Bacon's great argument. In 1620, when Lord Chancellor of England and Baron Verulam, he brought out his *Novum Organum*, or new method of the sciences, in two divisions, of which the first was destructive of scholasticism, and the second constructive of a better way. The *Novum Organum* contained the chief pith of Bacon's philosophy, but of this only the first or destructive part was complete. No praise can be too high for the manner in which scholasticism is for ever annihilated in this treatise. The trenchant criticism, the large and luminous good sense, the grand and true conceptions of man's relation to nature, the striking metaphors—now as familiar as household words—which pervade the first part of Bacon's *Organum*, have rendered it an immortal work, from the point of view of literature as well as of philosophy. But it would be ludicrous to maintain that, in writing it, Bacon followed the footsteps of no one. There had been a host of attacks upon Aristotle and the Aristotelians. Ramus, in Paris, as a young man, had sustained the thesis, that "everything which Aristotle had said was false." Patrizzi, in Venice, had published an assault upon Aristotle characterised by the utmost malignity. Galileo, at Pisa, had roused the anger of the Aristotelians by demonstrating that Aristotle was in error when he said that heavy bodies fall faster than light ones. The air in Europe was full of revolt against scholasticism, and yet Bacon thought that when he laid down, in his brilliant way, that "Truth is the daughter of Time, and not of authority;" that the "ancients were the children of the world;" and that "the syllogism is unequal to the subtlety of things," be was stating something original, and not following in the footsteps of any one. Bacon, having imbibed, with the quick apprehension of genius, numberless suggestions from his contemporaries and predecessors, at once kicked down the ladders on which his mind had risen; he spoke with the greatest disdain of Ramus and Patrizzi; and he so little appreciated Galileo that, when informed of some of the labours upon which Galileo was engaged, he said to his informant:—"I wish you would persuade those Italian astronomers to give up amusing us with their idle stories, and stick a little closer to the experience of the senses." It is a most curious circumstance that Bacon, with all his great intellect, could not stomach the Copernican theory. Indeed, in some things he was even behind his age. He spoke slightingly of Gilbert as a mere specialist—Gilbert, whose discoveries in magnetism have, I believe, been scarcely superseded at the present day. He accords no just tribute to Harvey, who had been his own physician. Harvey, on the other hand, appears to have set no great store by Bacon. Harvey is reported to have said :—" He writes on philosophy like a Lord Chancellor." Now that I have begun with Bacon's intellectual defects, I may as well finish with them. Bacon was strikingly deficient in the power of looking at things historically; indeed, it never seems to have occurred to him to do so. He treated Aristotle and Plato as if they were men of his own time, who differed from him in opinion. In his work called the *Wisdom of the Ancients*, he undertakes to show that under each of the fables of the Greek mythology there was wrapped up and concealed some piece of physical, metaphysical, or political philosophy. His interpretations of the myths are rich in ingenuity, but at the same time they are perfectly arbitrary and reckless; and they ignore the primary question of all, which is, How are we to conceive myths originally to have been formed and established in the minds of a people? To this question the quite recent science of comparative mythology offers some sort of solution. Bacon's view of the myths was very prosaic; he regarded them as didactic stories for the improvement of child-like minds. His view of poetry itself is of the same character. He describes it as "a part of learning," and evidently values it only so far as it is useful—that is to say, didactic. It is with a covert sneer that he says :—" Poesy was ever thought to have some participation of divineness, because it doth raise and erect the mind, by submitting the shows of things to the desires of the mind; whereas reason doth buckle and bow the mind unto the nature of things." He divides poetry into Narrative, Representative, and Allusive. In speaking of Representative poetry — that is, the drama—he makes no mention of Shakespeare, whom indeed he did not appreciate. He cares most for Allusive poetry, which conveys truth under the form of allegory. He excluded Lyrical poetry from the category of poetry altogether. He says :—" We exclude satires, elegies, epigrams, odes, and the like, from our discourse, and class them with philosophy and the arts of oratory." In short, Bacon, though he had himself many of the characteristics of a poet, utterly misconceived the true nature of poetry, and in his theory reduced it to the level of prose.

It is perhaps more excusable in Bacon that his outlook on the future of human knowledge was, after all, quite limited. He admitted that the instauration of the sciences would require some ages for its completion; but he thought that, within the compass of no very long period, that completion would be effected. Bacon would not have realized what was implied by Goethe's phrase, *unendliche Natur*. If you had asked him, he would probably have prophesied that, before the year 1877, all the mysteries of nature would have been cut and dried and garnered. He had some dim notion of the science of chemistry; of the sciences of geology, palæontology, comparative philology, and political economy, which have done so much to change modern ways of thinking, he had not an inkling. Of the theory of evolution or of spectrum analysis, it is needless to speak. Bacon had not even heard of gravitation, and he held to the Ptolemaic system of the universe. After all, men's imagination is restricted to what they have had experience of, or to some short flights of analogy beyond it, and we cannot in the least anticipate or set bounds to what our successors may be thinking of some few centuries hence.

Bacon has been charged with having led the way, by his philosophy, to empiricism, materialism, positivism, and sundry other amiable "isms." But this charge is rather unfair; for though, in his physical philosophy, he seems to have tended more and more to an atomistic theory, and though in the interval between his *Advancement of Learning* and his *Novum Organum* he seems to have abandoned the idea of metaphysics as a useful science, still he himself always maintained a great respect for revealed religion. Though he, very properly, enjoined that ethics should be studied, like other sciences, inductively, he considered that mind itself is an emanation from God, and not subject to the laws of science. He thus, as Kuno Fischer remarks, entertained a dualism not dissimilar to that of Descartes. He held that faith should go side by side with science, and that we should "render to Cæsar the things that are Cæsar's, and to God the things that are God's." Therefore, if subsequent philosophers have taken a different attitude, the responsibility surely rests with themselves.

The last reproach against Bacon is, that he made no discoveries himself, and that he directly led the way to none. Both of these allegations are, in fact, undeniable. Bacon sometimes showed brilliant intuitions into the nature of things; for example, in illustrating his method of discovery by "prerogative instances," he says:—"The phenomenon of colour is discovered most readily, and with the least heterogeneous admixture, in prisms, crystals, and dew-drops; for these have little or nothing in common with other coloured bodies, such as flowers, stones, metals, varieties of wood, &c. They are, in this respect, single instances, and from observing them we easily arrive at the result that colour is nothing but a modification of the image of the incident and absorbed light; in the former case by the different degrees of incidence, in the latter by the textures and various forms of bodies." This paragraph has been said to contain an anticipation of Goethe's theory of colours. But such intuitions were only, like those of Aristotle, the happy thoughts of a powerful intellect. Discovery, properly so called, remains as the prerogative of the specialist; and Bacon, though he killed himself by the experiment of stuffing a fowl with snow, cannot claim any discovery as his own.

Nor have discoveries been made by following the precepts of the Baconian method of induction. Some attribute this to the incompleteness of the second part of the *Novum Organum;* others say that it is because Bacon's method is based on an erroneous view of causation. Twenty years ago, Dr Whewell brought out a *Novum Organum Renovatum,* to supply Bacon's deficiencies; but we may safely venture to assert, that Whewell's book will not be a bit more fertile in producing discoveries than Bacon's has been. Logic is a useful part of education, and is necessary as a part of philosophy; but every real worker in the world seems to have an unconscious logic of his own, based on innumerable analogies of the class of facts with which he is in the habit of dealing. The study of deductive logic will never of itself create powerful reasoning in the law courts or the senate; and let the logic of inductive method be laid down never so perfectly, still discovery and the advancement of science will go on independently of it. The true method of studying nature had come into the world before Bacon wrote; it had been distinctly described, though in general terms, by Lionardo da Vinci, Tycho Brahe, and Galileo; and, quite independently of Bacon, its adoption had been signalised by several of the greatest discoveries ever made by the human race,—some of which Bacon even refused to admit. Probably no direct connection can be established between the influence of Bacon and the achievements of any separate science. But, as De Jouffroy says, "Is it nothing to have seized, at the critical moment, the idea which would open a new era to the sciences? Is it nothing, in the early dawn of an immense revolution, to have predicted almost in detail its career? Before Bacon's time no one seems to have had a true sentiment of the grandeur of nature; this sentiment, conjointly with enthusiasm for science, he preached and spread abroad. As soon as he had written, the genius of observation raised its head, and marched on an equality with the genius of thought."

I have already given evidence of Bacon's general influence upon the minds of men, and it was by this general influence, on the universities, on separate scholars, and on the reading world throughout Europe, that Bacon assisted the birth of the new era. His perceptive mind brought into one focus, beams of light which would otherwise have remained scattered; like an eagle he soared till he caught sight of the unrisen sun; he told the world what was going on and what was in store for it; he made the new era conscious of itself. In doing this, he employed the most splendid literary faculty. No architecture is, after its kind, finer than the English prose of Bacon's *Advancement of Learning*. Remusat justly recognises in Bacon's writing that quality which is termed "the great manner," and he envies him that balance of imagination with good sense, which he considers to be a specially English characteristic. Bacon, in an interval of short-sightedness, distrusted the permanence of the English language; like Dante, and others of the great moderns, he undervalued the worth and inherent vitality of his vernacular tongue; he said "these modern languages will one day play the bankrupt with books." Therefore, after his first great treatise, he preferred to write upon philosophy in Latin. This we may now regret from a literary point of view, but it doubtless extended the circle of his contemporary readers, and his Latin writings, not being in classical style, and being full of pregnant thoughts, wise and witty sayings, and the happiest metaphors, are easily translated into any language, and are a treasure for all the world. In conclusion, it is as an inspired seer, one of the greatest of men of letters, and the prose-poet of modern science, that I reverence Lord Bacon, and have ventured to make him the first topic of the evening.

Our own Royal Society doubtless borrowed its title and partly its idea from the Royal Society of London. I am proud to think that this Society is an emanation from the University of Edinburgh, from which it sprang, on the suggestion of Principal Robertson, in the latter part of 1782. Thus, in the same year when the University will celebrate its tercentenary, this Society will be able, perhaps conjointly, to celebrate its hundredth birthday. But in one essential particular we differ from the Royal Society of London, whose function was thus defined in 1663 :—"To improve the knowledge of natural things, and all useful arts, manufactures, mechanic practices, engines, and inventions, by experiments (not meddling with divinity, metaphysics, morals, politics, grammar, rhetoric, or logic)." These exclusions form no part in our constitution. On the contrary, from the first, the promotion of literature as well as science was the object of the Royal Society of Edinburgh. I may say, then, that the name of Bacon, as one of the greatest stars of literature, has a double claim upon our regard. But it has been observed that the literary element in our proceedings has been gradually dwindling away. Principal Forbes, fifteen years ago, spoke of the difficulties under which we laboured owing to a change in the manners of society, and to our having become less "clubable," or less intellectually sociable, than our ancestors used to be a hundred years ago. And he exhorted the then fellows to bestir themselves in the production of papers. I have inquired the number of papers, not connected with physical science, which have been contributed during the last fifteen years, and it appears to be considerably less than forty, or little more than two per annum. And of these, seven were contributed by Professor Blackie on classical or philosophical subjects, four by Lord Neaves chiefly on philology, three by Dr John Muir on Indian antiquities, three by Mr Wyld on metaphysical topics, two by Sir James Simpson on archæological questions, two by Mr Skene on Celtic topography and philology, and the remaining twelve or fourten by separate individuals. Thus, in the last fifteen years, out of about 370 Ordinary Fellows of the Society, only about twenty have come forward to contribute papers other than physical or mathematical. During the last session not a single paper on any literary subject was read before us. No doubt, there is much excuse for this, in the fact that the time of literary men is often much occupied, and that they can obtain a high remuneration elsewhere for the produce of their labour. It was doubtless owing to this that Sir Walter Scott, who was for twelve years President of this Society, never appears to have contributed a single paper to it, though he was so full of history and archæology and lore of all sorts.

The day may even come when the general public is sufficiently instructed to receive with interest, in ordinary periodicals, the physico-mathematical papers, which form the staple commodity of this Society, and then we shall have done our work. In the meantime I would venture, like Principal Forbes, to advocate "intellectual clubability." All tentative essays, which are not yet ripe to form a book or an article, may fitly be contributed to this Society and embodied in its Transactions, and I should have thought that not a single professor of any department in the University could go through the teaching of a session, without coming across at least some one novel point, which it would be worth while to bring forward.

In the course of last session, one of the three prizes in the gift of this Society—the Macdougall-Brisbane prize, consisting of a Gold Medal and £15, 14s. 7d., was awarded to Mr Buchan, for his paper, "On the Diurnal Oscillation of the Barometer." This important contribution to meteorological science has been published in the Transactions of the Society, and I note here with pleasure the words used in regard to it by our President, on presenting the prize:—"That it paves the way for discovering the complete thermodynamic theory of its subject, and that Mr Buchan has well followed up his laborious and most useful investigations of the meteorology of Scotland by so valuable a discussion of barometric observations from all parts of the world, collected and arranged in the very best manner for the immediate application of the harmonic analysis."

The following statement, in regard to the number of the present Fellows of the Society, has been drawn up by the Secretary :—

1. Honorary Fellows :—

Royal Personage—

His Royal Highness the Prince of Wales, . . 1

British Subjects—

John Couch Adams, Esq., Cambridge ; Sir George Biddell Airy, Greenwich ; Thomas Andrews, M.D., Belfast (Queen's College) ; Thomas Carlyle, Esq., London ; Arthur Cayley, Esq., Cambridge ; Charles Darwin, Esq., Down, Bromley, Kent ; John Anthony Froude, Esq., London ; Thomas Henry Huxley, LL.D., London ; James Prescott Joule, LL.D., Cliffpoint, Higher Broughton, Manchester ; William Lassell, Esq., Liverpool ; Rev. Dr Humphrey Lloyd, Dublin ; William Hallowes Miller, LL.D., Cambridge ; Richard Owen, Esq., London ; Thomas Romney Robinson, D.D., Armagh ; Lieut.-General Edward Sabine, R.A., London ; Henry John Stephen Smith, Esq., Oxford ; George Gabriel Stokes, Esq., Cambridge ; James Joseph Sylvester, LL.D., London ; Alfred Tennyson, Esq., Freshwater, Isle of Wight, ... 19

Foreign—

Claude Bernard, Paris ; Robert Wilhelm Bunsen, Heidelberg ; Michael Eugene Chevreul, Paris ; James D. Dana, LL.D., Newhaven, Connecticut ; Alphonse de Candolle, Geneva ; Heinrich Wilhelm Dove, Berlin ; Jean Baptiste Dumas, Paris ; Charles Dupin, Paris ; Elias Fries, Upsala ; Professor Carl Gegenbaur, Heidelberg ; Herman Helmholtz, Berlin ; August Kekule, Bonn ; Gustav Robert Kirchhoff, Heidelberg ; Herman Kolbe, Leipzig ; Albert Kölliker, Würzburg ; Ernst Eduard Kummer, Berlin ; Johann von Lamont, Munich ; Richard Lepsius, Berlin ; Ferdinand de Lesseps, Paris ; Rudolph Leuckart, Leipzig ; Joseph Liouville, Paris ; Carl Ludwig, Leipzig ; Henry Milne-Edwards, Paris ; Theodore Mommsen, Berlin ; Louis Pasteur, Paris ; Prof. Benjamin Peirce, United States Survey ; Henry Victor Regnault, Paris ; Angelo Secchi,

Carry forward, 20

Brought forward, 20

Rome; Karl Theodor von Siebold, Munich; Bernard Studer, Berne; Otto Torell, Lund; Rudolph Virchow, Berlin; Wilhelm Eduard Weber, Göttingen; Friedrich Wohler, Göttingen, ... 34

2. Non-Resident Fellow under the Old Statutes :—

Sir Richard Griffiths, ... 1

Total number of Honorary and Non-Resident Fellows at November 1877, ... — 55

The following are the Honorary Fellows deceased during the year :—

Foreign—Urbain Jean Joseph Leverrier, John Lothrop Motley, ... 2

British—William Henry Fox Talbot, ... 1

— 3

3. Ordinary Fellows :—

Ordinary Fellows at November 1876, ... 363

New Fellows, 1876–77.—Dr Isaac Bayley Balfour; George Broadrick, Esq.; George Cunningham, Esq.; Dr John Gibson; Walter Noel Hartley, Esq.; William Jolly, Esq.; James King, Esq.; Robert A. Macfie, Esq.; Sir Daniel Macnee; Dr Robert Milner Morrison; John Murray, Esq.; John Napier, Esq.; George A. Panton, Esq.; Dr William Pole; Prof. James Roberton; George Carr Robinson, Esq.; James Stevenson, Esq.; Prof. William Stirling; Charles E. Underhill, Esq.; Walter Weldon, Esq.; Charles Edward Wilson, LL.D., ... 21

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Deduct Deceased.—Dr James Bryce; Andrew Coventry, Esq.; Rev. D. T. K. Drummond; Sir David Dundas, Bart.; William Keddie, Esq.; The Hon. Lord Neaves; James Thomson, Esq., C.E., who died in August 1870, but whose death was only intimated in August 1877, 7

Resigned—Dr Alexander Hunter; Captain T. P. White, 2

Carry forward, 9 384

Brought forward,	9	384
Cancelled— Prof. Arthur Gamgee, M.D.; Ernest Bonar, Esq., cancelled in 1856, and whose name has been inserted by mistake in subsequent Lists, . .	2	
		11
Total number of Ordinary Fellows at November 1877, .		373
Add Honorary Fellows and Non-Resident Fellows, .		55
Total Ordinary and Honorary Fellows at commencement of Session 1877–78, ...		428

The following Obituary Address was read by Professor Piazzi Smyth, Astronomer-Royal for Scotland:—

Le Verrier—Urbain, Jean, Joseph—one of our most respected Honorary Members for thirty years, died on the 23d of last September, aged sixty-six.

On the 23d of that same month, but in the year 1846, an astronomical discovery was made on the Continent, which almost took men's breath away for the moment in astonishment and admiration; and which in bringing the name of Le Verrier for the first time prominently before the general public, taught them that the age of intellectual giants after the manner of Newton and La Place, was not yet closed. And thirty-one years afterwards, or on that very identical day of September in the present year 1877, the electric telegraph flashed this other mournful news over an attentive world, that the same Le Verrier (without compare the greatest gravitational astronomer of our time) had ceased to exist!

Death has been indeed too complete, for within a few days of Le Verrier's own demise, his adored Wife followed him to the tomb, where one of his sons was already laid; and we thus have the whole life-lesson of this most eminent scientist ready for impartial historical treatment, if at all.

But who shall prove equal to the task of writing the life of *such* a man. Biographical notices of him innumerable have been published, but *not* the one, full, history which posterity will require; nor is this the place to attempt anything of the kind. But what *has* appeared in print already, *sur*-abundantly relieves your Council of the necessity of any attempt on their part at formally *justifying* their advice of thirty years ago, to the Society, to enrol the name of

Le Verrier amongst its Honorary Members; and they cannot perhaps do better in the few minutes now accorded to them, than simply to describe something of the general character of, as well as progress of alteration with time in, the public career of the departed.

The *first* period of Le Verrier's virile existence may be considered to extend from his leaving the Polytechnic School in 1831, to receiving the appointment of Director of the Observatory of Paris in 1853; a joyous, a rising and expanding existence to him the whole of the time. Born in 1811 at St Lo, in the Department of Manche, (which courts the west wind of the Atlantic along the whole extent of its coast), and son of a Government official there, the young Le Verrier had received his education first in provincial, then in Parisian, schools, always manifesting a taste for, and superior power in, pure mathematics; but with a further determination also to hold his own course, and to prosecute his own *excelsior* ideals therein. Hence, on ceasing to be a college student in 1831, and beginning his career as a man, as the architect too of his own fortunes to be, he seems to have chosen, not a poorly paid, over worked, hard scientific post, but the more easily executed, and better paid employment of the ordinary civil service of his country; selecting such a branch therein as should leave him most spare time for the prosecution of his private reading; and only when, after some years, the further continuation of that official employment, the *Administration des Tabacs*, would have obliged him to leave the neighbourhood of metropolitan libraries,—did he seek to support himself by teaching mathematics, of which he then became one of the assistant professors in his old Polytechnic School.

But those pedagogic labours by day, the still youthful Le Verrier never allowed to interfere with his own private studies at night. He had already comprehended and fervently accepted his destiny; *viz.*, to succeed La Place in those applications of the higher mathematical analysis to celestial mechanics, which demand as much of continued labour, and power of abstraction, as of penetration, genius, and analytical resources. Hence his earliest paper presented to the Academy of Sciences in 1839, attacked no less than the secular inequalities in the movements of the principal planets. His second paper took up the orbit of Mercury, and the most exact calculation of the numerical amounts of the perturbations it is subject to from other planets. His third paper, presented in 1844, investigated in the most masterly style the perturbations which the strangely deflected Comet of 1770 must have experienced both before and after that epoch, from the attraction of the planet Jupiter. And his fourth paper, applied similar mathematical calculations to the more modern discovered Comet of 1843.

But those Comet papers, though they gained Le Verrier a seat and a pension in the Academy of Sciences, were not altogether in his destined line. In more immediate resumption of which, it was, that in the beginning of 1846, he brought before the Academy his fine investigation into the orbit of the planet Uranus, his computations of every perturbation it could suffer from every known member of the solar system, and his finding them all insufficient to account for the difference between theory and observation. And then, after that essential clearing of the ground, he produced his further and most remarkable of all his mathematical calculations, confessedly and by its very title, *on* "the hitherto unseen, outside planet, which, acting from without, *is* producing the apparent irregularities in the movement of Uranus, from a distance of 1500 millions of miles beyond, and *is* now in such and such a position in our sky, and *must* have such and such an apparent diameter." A note of that position and size Le Verrier sent to a German observer on the 17th of September in that year, 1846. That observer, Dr Galle by name, looked for it on the evening of the 23d of that month, and within a few minutes, with the powerful and accurately mounted equatorial telescope of the Berlin Observatory, he actually succeeded in finding Le Verrier's theoretical planet, since called Neptune, in the given direction, at the enormous distance and of the indicated size, nearly.

This was the discovery, *sensational* it has been called, but most worthy to make a sensation throughout the whole human race, which immediately brought in honours of every kind, and from many nations to Le Verrier. Which presently too, wafted him in political safety across the abyss which proved fatal to the then king and all the royal family of France; but gave the astronomer-mathematician an eminent place under the subsequent republic, and a still more eminent one under the Empire which followed. But none of those things, however flatttering personally, not even the powers invested in him to reform the Polytechnic School, and to inspect all the universities of France, nor all the immense astronomical correspondence of which he became the European centre, did he allow to interfere with his continued private research in his own appointed task. Wherefore the Academy was electrified again and again by further of his magnificent *planetary* papers, in 1849, 1850, and 1853—as, on new methods of constructing the numerical tables required for practically computing the places of the planets as subjected to all perturbations of Newtonian gravitation origin; next, on improved tables of the Sun, as representing the movements of the earth; and then on still more refined theoretical considerations touching the whole group of planetoids, between Mars and Jupiter, whatever their teeming numbers might eventually be found with the telescope.

All this time, too, Le Verrier was the most cheerful of beings, the most admired of all in scientific life, and envied by no one; for he had gained his successes by genius and force which were all his own, which no one else could attempt to claim or compete with. And he could be so smiling, so gracious, sweet as summer air, and full of happy remarks to all he met. Occasionally even rather too much so; for we have seen him at that period of his life at a meeting of the Academy, and while the very Perpetual Secretary was reading out the papers of *others*, there was M. Le Verrier joyously walking in, and about, and through all the ranks of seated savants, nodding to one, discussing with another, laughing with a third, proposing problems to a fourth, and without the slightest idea he could be doing anything wrong. While his personal appearance was in those days a tall figure, with fine upright carriage, sparkling eyes unconscious of offence, an immense dome of a head and abundant flaxen hair. The French called it "blond:" and how that man could have escaped being born an Anglo-Saxon, was more than we could then understand.

So closed the first period of Le Verrier's career; for the second and very contrasted part of it began when Louis Napoleon appointed him to the charge of the Imperial Observatory at Paris.

And why should it have been such a contrast? Was not the "Observatoire" just the place for such a man? Not a bit of it!

In our own Mrs Somerville's autobiography, unfortunately not then published, she pointedly records that when all the world was paying *her* honour, immediately after the publication of her most unexampled lady's book on astronomy, entitled "The Mechanism of the Heavens," elucidated there by her able expositions of the algebraical analysis of La Place and other great French mathematicians—one of her lady friends in Burntisland remarked so confidently, "Oh! Mrs Somerville, what a happy life you must lead, for of course you are always gazing at the stars through a telescope!" Whereupon Mrs Somerville answered to the effect, "that she had never looked through a telescope in her life, except once; and then it was not at a star." And of course! For if she had been spending her nights in instrumental star-gazing, how could she have been also getting up by midnight oil the totally different subject of algebraical equations?

Very similar too, *mutatis mutandis*, was the case of Le Verrier. Born to excel in astronomical mathematics on paper, what had he to do with telescopes! Why, the very, and most particular, praise which all the world had given him in 1846, was,—"Other astronomers have occasionally discovered a little planet, by finding in the sky with their telescopes a diminutive star, which varies its place slightly from night to night: but here, in M. Le Verrier, is a man who has discovered a planet without looking through a telescope at all; without even casting a single glance at the sky; *he* saw his grand planet at the end of his pen!" If that praise, then given so freely, was genuine and honest, why take that man away from his glorious pen work, and make him begin mechanical operations which he has no taste for: which he has shown he can transcend in their occasional results by totally different methods?

But the imperial will had ordained it; and forthwith, from 1853 to 1870, Le Verrier was known as *Director of the Observatory of Paris:* showing too, there, in many brilliant respects, how much a rigid sense of mere duty may enable a man to perform in almost any line. Though the world at large will be thankful chiefly, that in all this second portion of his career, Le Verrier never allowed himself to be entirely absorbed in the continually increasing circle of his official employments; but held as closely as ever to his soul's private task, viz., the mathematical investigations, and then the practical computations, of every perturbation which the Solar planets can suffer.

In this manner, with an energy, a constancy and an intellectual power never combined before in any one individual, Le Verrier had, by the year 1868, produced new tables of all the inner planets from Mercury to Mars ; and in the course of them had demonstrated most unexpectedly, that the hitherto universally received quantity for 70 years past, of 8·56″ for the Solar Parallax, was much too small. The effect on the astronomical world was magical. For years there *had* been a languid idea that the Solar Parallax value derived from the then last previous Venus Transit, should be improved upon by observations of Mars at opposition ; and the *Nautical Almanac* had for half a century gone on publishing the appropriate ephemerides of Mars and stars near him for Opposition after Opposition. But few persons ever observed them ; or if they did, and also, like Henderson at the Cape and Taylor at Madras, computed the results as well,—they were not attended to. But the moment it was announced that Le Verrier had found from his calculations in Physical Astronomy, that the mean Solar Parallax was so far from 8·56″ as to be nearer to 8·95″, all Astronomical Associations were shaken to their very bases. Greenwich ran to court the new quantity. The *Nautical Almanac* adopted it with fervour : and half a dozen nations were presently competing with each other in observing and re-observing Mars' oppositions, and always finding now that they gave out a quantity very like the Parallax announced by the great Gravitational Astronomer of France.

But how went on things in general at the Observatoire, all this time?

For a while, wonderfully well! If Le Verrier had no practical experience himself, he knew where he could obtain it in others. So, the Emperor always aiding and abetting, the favoured savant had only to propose schemes of improvement, and the means in both men and money were always forthcoming to carry them out. *First,* the whole Greenwich system was imported bodily, its instruments imitated, its routine copied, and observing astronomers and computers of a like order procured. Then came the concentration of Meteorological Observations by electric telegraph from all quarters of Europe, and the issue of a daily Bulletin, post free to all the world, by another corps of practical scientists newly incorporated into the establishment. Then came the rise of the "Association Scientifique de France," a voluntary popular institution after the British model, but where *all* the sciences were taken up, and Le Verrier was President of everything.

He took care verbally and with semi-military *hauteur*, to appoint his Observatory subalterns their tasks to perform; but when these *were* performed, largely by the native and peculiar, and often most exalted, skill of the workers, *they* were pushed into obscurity on every great occasion, and Le Verrier took sole ostensible authorship of all their doings. Long had they borne, but how much longer were they to continue to bear, this peculiarly affronting tyranny? To perform the most exquisite work in their own lines, but to remain unknown, trodden on, and see another, who in his heart despised such things, gain all the advantage and all the credit? Feelings, though silent, were growing more bitter and dangerously exasperated day by day at the Observatoire. Something must come of it. At length, not there, but at the Academy, the malcontents found a leader in M. Delaunay: a younger man, but rising to be a still greater mathematician in gravitational astronomy, than Le Verrier himself: and yet at the same time, was this most unique and estimable Delaunay as much distinguished by natural amiability, by the sympathetic tendencies of his disposition, and his earnest regard for others, as the Director of the Observatoire, Imperial Lord of the Ascendant, was daily becoming more and more antipathic, as well as impatient of every one in the world but himself.

Now Delaunay's grand mathematically scientific work at that period was the theory of the Moon; and he too had come therein on certain equations whence he drew a new value for the Solar parallax; making it however more nearly 8·85″, and proving Le Verrier's 8·95″ quite inadmissible. So thereupon the two great mathematicians crossed swords. Their discussions, meeting after meeting of the Academy, attained a vigour and an intensity which drew half Paris to *assist* at these re-unions; but chiefly to hear Le Verrier! He was indeed rather in the wrong scientifically; and had, long afterwards, to modify his 8·95″ to something very close to Delaunay's numbers. But what did the crowd care for minute exactitudes! They went to see how a hero fights, how he gains his successes; and Le Verrier gave them plenty of that. Though not exactly an orator, he was clear in expression, fluent in words, and ready, aye ready, at a moment's notice. Rich too in cutting sarcasm and scarifying irony, he taunted the slowness of his opponents who had to go home and think over their replies and promise to give them at the *next* meeting of the Academy,—while he stood there having answered, or at all events confounded, them on every point yet produced; he, the man of peace, who desired nothing so much as to finish the war then and there.

On such occasions Le Verrier seemed to float in positive pleasure through the combat, cutting down his enemies with ease on every side. A smile was still on his countenance; but it was now the smile which opens not the lips; a distorting, disdainful smile, betraying however within, both the consuming fires of egotism, the love of conquering, and the voluptuous rapture of triumph become a necessary accompaniment of his existence. Yet there was this untoward feature about it all the time, that with every successive conquest, Le Verrier was making a wider and wider void around himself; until, with one dazzling victory more,—there came the deadening news, that every one had left him, and the Director of the Observatory reigned over telescopes alone!

The Minister of the Interior then had to step in and give such a disorganizing Director his dismissal. Le Verrier answered by a passionate appeal for an interpellation by the Senate: but that body supported the Minister, and the tyrant-mathematician had to go. He fell; and never had there been such a catastrophe known before throughout the scientific world. But Delaunay was immediately appointed to the vacant place; the observers, computers and artisans all returned, and the public service flowed on once again.

Time passed; and how heavy a time for France, after that affair of the Observatoire in the beginning of 1870. The terrific Franco-German war began soon afterwards; the Germans invaded France with a larger army than the world had ever heard of since the days of Xerxes, and, though professing to be "Christians," and calling their country "Holy," yet armed with infinitely more terrible means for the destruction of human life than his, or Mahomet's either.

Every battle went against the French; their smaller armies were annihilated, their fairest provinces rent away: their revenues sacked, their people (even innocent and peaceable men, women, and children) starved to the very bone, their capital city taken. And then, after all these destructions from without, poor Paris fell into the hands of her own revolutionary Communists; who, when at length attacked in their turn, by the recovered forces of the nation,—most provokingly insisted on making the very Observatoire of Astronomical Science, one of their chief strategic points of defence; drawing down thereby on the grand old building, from loyal French guns too, an amount of cannon shot that riddled the Equatorial Domes through and through, and ruined much of the substantial masonry below.

But the chief part of the daily service of the Observatory was not impeded thereby. For, with remarkable prevision of coming events, Delaunay had removed a division of the working staff, long before the siege of Paris began, to a Southern Department. There they gathered up the telegraphic communications again, there they carried on their collections of all European, with a few Asiatic and some African Meteorological observations, and there also, without the cessation of a day, they issued their daily lithographed Bulletin; contriving still to give to all nations, and by a free book-post, the earliest information of every scientific event and Natural change and thus to keep up their beloved France (though bleeding from unnumbered wounds) an intellectual centre, a heralder of the way, for all the learned world.

That grand result, however, came about mainly because it was Delaunay who directed, or rather bound together with cords of love, all the broken and scattered elements of the scientific service of France. Under his sympathetic lead, every one put forth his utmost powers, and with a will and a devotion which, in immediate presence of death, was almost sublime. And when at length peace returned, and the reorganisation of the Observatory of Paris had to be undertaken, every report which M. Delaunay addressed to the Minister of the Interior was as surprising for its pellucid clearness, as for its ably comprehensive character: alike too for including every department of science, as for insuring the distinct recognition of every good worker therein. A true and happy model Republic was the Observatoire thus becoming under this rare Savant's enlightened rule, tending to the American perhaps in its almost over abundance of personal liberty, but to the Athenian in its perfection of work and glorious ideals of still higher things to come,—when, on a day,—alack the day!—one most gusty, stormy, tempestuous day; when poor Delaunay, who had gone down to the sea-side merely for a short visit, essaying to cross the harbour of Cherbourg in an open boat, a squall came on, the boat capsized in deep water, and France's first, best, rising, genius was lost for ever.

All political and scientific society stood aghast. There was no second Delaunay to be had anywhere; and even those persons who had been most active in bringing him forward only two years before,—could do nothing now but tacitly consent, in their utter dismay, to Le Verrier returning to power again.

Le Verrier returned accordingly to the Observatoire in 1872, and then began a *third* distinctly featured period of his career.

No longer the cheerful-looking Anglo-Saxon man of his earlier days; nor the tall, stern, conquering figure of his middle career; but now, bent and feeble, with a dark, jaundiced hue overspreading his countenance (symptomatic of the disorder of the liver under which he finally sank), and with the muscles of his face half withered and shrunk down to the bone, so as to show the real osseous foundation of the countenance and prove him a born Gaul, after all,—Le Verrier in his third stage pleased both friends and foes less than ever.

With the latter he did not attempt any actual open war, but there was underground work going on all the time; mining and countermining; torpedo fighting of a very bad kind. *He* chafed in spirit that he was returned to power only by the accidental death of his grand, faultless, adversary; and *they* were vexed that their whole ranks contained not another man capable of openly meeting Le Verrier. *He*, dominating at the Parisian Centre, obtained from the Minister (who was now frightened to interfere with him) power to revise and curtail as he chose almost all the other scientific Observatories of the country; and *their* Directors were furious, that when France was, as then, a Republic, they, the scientist portion thereof, should have to live under the sway of a more than Emperor, a Czar;

of one, moreover, who, while indulging himself in extending *his* Observatory even by profuse public expenditure,—sought to gain a character before the country for economy, by starving, or even abolishing, their's.

If too, the claims of *some* of those other astronomers to patriotic recognition and govermental regard were occasionally too great to be altogether and immediately overborne even by Le Verrier,—he undermined, and laid plans which must *eventually* lead to their collapse and removal. Thus, was there a new Observatory lately established in the south of Paris, where the Savant, who had carried on the Meteorological bulletins during the German war, should in future devote himself to a new kind of utilitarian meteorology for the special benefit of agriculture and the service of the public health;—or was there another new Observatory allowed to be built in the north of Paris at Meudon, near Montmartre, for the Savant who so nobly risked life and limb by sailing forth in a balloon over the heads of the truculent Germans then besieging Paris,—in order that France, even in her agony for very existence, should still be present among the nations at the observation of a total Solar Eclipse on the Northern coast of Africa,—and where, in that new Observatory, that patriot and fearless astronomer should, with no other control over him than the Minister of the Interior, seek to carry out his own very original inquiries into the Physics of Astronomy,—then Le Verrier insisted on having additions of new instruments and young subaltern observers added to his own Observatory, to accomplish the very same ends, but on a far grander scale. Or was there a new reflecting Equatorial ordered for the Observatory of Toulouse,—whereto M. Delaunay, in his short but brilliant career, had sent a young astronomer, of genius after his own heart, as Director,—Le Verrier did not actually interfere with its going there, but immediately ordered, at his country's expense, for his own Observatory, a similar reflecting telescope, one-half larger, and with all the economical fittings of the other in modest wood, replaced in his own with *ne-plus-ultra* work in fine metals.

And then his mere *manner* was sometimes so intentionally provoking.

When President Marshal Macmahon,—striving to entertain nationally, and mentally impress the Shah of Persia, then on his gorgeous visit to Paris,—applied to Le Verrier to arrange for a reception of the Eastern potentate and his bejewelled and glittering suite at the grandly restored Observatoire, he only received the following short note from the State's chief paid Astronomer :—

Marèchal,—La Science, n'illumine pas, les sauvages.

LE VERRIER.

Even in ordinary conversation it was almost terrible to see how the old-man-misanthrope, in merely describing the course of an argument with another scientist, could egregiously clench his fist and act to the very life as though delivering a regular knock-down blow *right between the eyes.* Or when the other's equations were faultless, and no other complaint could be found, he would be stigmatised in society as " that man with the big ugly mouth."

All this would soon have become unbearable, but that it was accompanied by two things, even three, which induced wise men to be exceptionally tolerant.

First, for instance, they knew that there was still a truly heroic spirit in that querulous disposition and decrepit frame. When all the rest of educated society in Paris was utterly frightened at the sudden rise of the Red Republic, and sought only to strike their colours, and disappear by putting on the aspect of being more *canaille* than the Red men themselves,—Le Verrier stood out firm in his appreciation of hereditary order, historic experience, accumulated wisdom, and preservative rule. Red-republicans indeed claim to be equal to, and on the same level with, him ! Let any Red-republican of them all essay on the grandest test of man's intellectuality, to tread with him, if they could, the mathematical foundations of the stability of the Solar system ! And they couldn't :—so were *they* to inscribe their wretched shibboleth of " Libertè, Egalitè, Fraternitè " over his door ? No ! he would have none of that sort of thing. And hence, when visitors from distant parts of the world, even from the very antipodes, came to pay their homage to the first physical Astronomer of France, and had been disgusted on passing through Paris, to see its grandest public buildings, and even the most sacred of its churches, scribbled all over, in fright, to pacify the Red men, with those three most falsifying, misleading words of their political creed and pretences,— such visitors read over the portal of the palace wherein Le Verrier resided, only this one word—"OBSERVATOIRE."

Second, within that respected building, in its private dwelling portion, *there* reigned the gentle radiance, and mild dignity of Madame Le Verrier.

In those early days of the now passed away French Emperor, when *he* made *no* mistakes, and had to pay his visit of state to London during the Crimean war, he not only took the Empress Eugènie with him ; but, while M. Le Verrier was one of *his* suite, Madame Le Verrier was one of the ladies of honour in waiting on the Empress. One of the most mentally distinguished of them too; for, after the grandest West-end reception on that occasion, this saying worthily ran through the élite of London society, "Other ladies present were expensively dressed, but Madame Le Verrier was *well* dressed." And if there is any grim philosopher who doubts whether there is much in that, let him refer to the printed discussions on this very matter which took place at the Social Science Association's brilliant meeting at Aberdeen only two months ago.

But M. Le Verrier himself, amid all his wielding the forces of science, could appreciate the beautiful in art, and even assist at the development of some of its forms ; for he fed his soul on music. But it was with a change, as his career progressed. When he visited England first, in 1847, and was asked to write something in a lady's album at Oxford, and not only composed a *chanson* then and there, but set it to original music, took a ruler, ruled the orthodox number of lines, and then wrote therein the musical notes with the precision of a copper-plate engraving,—the whole thing had the pleasant aspect of spring flowers; primroses and cowslips, daisies and buttercups in the opening months of the year. But when, as he felt impelled occasionally to do, in the long after years of 1875 or 1876, he armed himself with his violin and proceeded to draw therefrom, as it seemed almost in spite of itself, the most agonizingly beautiful strains, thrilling tones of dying grace,—tones which a Paganini might have envied, and ordinary men almost have wept that they could do nothing of the kind,—it reminded one more of the dark autumn's one, last, grand, garden flower, the Tiger-lily, bowing over its prospective grave in the earth beneath.

Lastly, in still further mitigation of much of Le Verrier's manner during the final years of his official career and life itself,—all good men were aware that his indefatigable, most original, and innate, unapproachable genius was again toilsomely engaged on the grand, self-imposed task of his life; and was even then rapidly and magnificently preparing as full mathematical theories and accurate practically-computing numerical tables of the larger, outer planets of the Solar System, as he had already done for the smaller, inner members thereof. His analytical methods too were the most comprehensive that had ever been attempted; and were specially distinguished by their capacity to take in and utilise all good observations ever made in practical astronomy on the bodies concerned, through all time, whether ancient or modern. The work was immense, and it killed him in the end, though not before he had satisfactorily finished it, even to the last page.

He had even the peculiar satisfaction before his death of seeing his most difficult researches, almost in mathematical darkness, as to the mass of the planet Mars—up to that time supposed by all the world to be "moonless Mars"—suddenly lighted up and confirmed by the splendid and most unexpected *American* discovery of this last summer, with their latest erected and largest equatorial telescope at Washington, of two moons circulating around that planet, and at rates of motion which admirably bore out his, Le Verrier's, intellectually concluded number for its mass and consequent power of gravitational attraction. But so truly immense, as well as *ultra* difficult, was the whole planetary research, that not only (as has been well said elsewhere) would it have been considered mpossible for one man to have achieved the whole, if Le Verrier had not performed it in our own times,—but, only one man has been found fully able even to describe the work to others; and that one man is,—who?

Le Verrier's early, unconscious, but unexceptionable, and equally successful rival, in his greatest astronomical calculation, prediction, and discovery, with regard to the once unknown planet, which we all now so glibly call Neptune,—viz., the then young J. C. Adams, of St John's College, Cambridge.

Professor Adams has indeed not only described, in a late Presidential address to the Royal Astronomical Society of London, Le Verrier's life long problems of the planetary orbits,—with an ability, clearness, and method which leave nothing to be desired for scientific instruction's sake,—but he has thrown into the execution of his task such a plenitude of impartiality, such generosity of feeling and goodness of heart, that his account has been enthusiastically reproduced in Paris, within the last few weeks, under the title of "The genius of Le Verrier appreciated in England."

Such then, so far as this imperfect account can go, was *Urbain, Jean, Joseph*, Le Verrier. That grand existency, who was lost to the ranks of the Honorary Members of this Society, on the 23d of last September; when his spirit returned to God who gave it.

In recording the lamented decease of the Hon. Lord Neaves, we have to record almost the greatest loss which the "Literary class" of the Royal Society of Edinburgh could have sustained. When this sad event was announced in December of last year, a universal note of lamentation was raised throughout the city. It was felt that a great figure had been taken away from our ranks; that we had lost not only a learned and weighty Judge, but an accomplished scholar, a distinguished man of letters, a wit and a conversationist who contributed salt to all social gatherings, a racy Scottish character, and "one of the last links which connected the Edinburgh of to-day with the greater Edinburgh of the past." But the loss which the Royal Society has sustained in Lord Neaves is special and peculiar, for he was rarely qualified for a literary Fellow of this Society, and as such he bravely played his part and contributed his share to our Proceedings, and he has left very few behind who can be at all compared with him. Lord Neaves possessed not only great intellectual activity and a predilection for scientific research, especially into questions of philology, but he was also largely endowed with that quality which has been, on a former occasion, referred to as necessary for the maintenance of vitality in a Society like this—the quality of "intellectual sociability." He loved to communicate with others, to tell and to hear, upon topics of literature and science. We can all call to mind his genial manner, when presiding at meetings of the Royal Society, and the dry humour with which his scholarlike papers were interspersed. An exhibition of the same manner at one of the meetings of the Social Science Congress caused the late Sir John Bowring to give Lord Neaves the nickname of "Wit and Wisdom."

Lord Neaves, who was of Forfarshire extraction, was born in Edinburgh in the year 1800. He was educated at the Edinburgh High School, where he exhibited precocious scholarship, marked intellectual tastes, and that quick, capacious, and retentive memory, "wax to receive and marble to retain," by which he was distinguished to the very end of his life. He was *dux* of the High School in 1814; and then passed through the Edinburgh University with distinction, though it does not appear that he graduated—graduation not being the custom in those days. In 1822 he was called to the bar, which then included among its numbers a brilliant band of wits and scholars;—the walls of Parliament House then resounded with the talk and the laughter of Lockhart, Professor Wilson, Professor Cheape, Sir William Hamilton, George Moir, Patrick Fraser Tytler, and Patrick Robertson; while among the older generation on the Bench were Cranstoun, Jeffrey, Murray, and Cockburn. Of the spirit of those times Lord Neaves was the last representative. In 1841 he was made Advocate-Depute in 1845, Sheriff of Orkney; in 1852, Solicitor General; in 1853, on the death of Lord Cockburn, he was appointed a Judge in the Court of Session; and in 1858 he became a Lord of Justiciary. The verdict of Lord Neaves' professional brethren upon his legal qualifications appears to be that he was a great "case lawyer"—his vast and accurate memory giving him peculiar facilities in referring to precedents, and that he was highly distinguished as a criminal Judge.

Side by side with the faithful discharge of his professional duties, Lord Neaves always maintained the part of the man of letters. From the year 1830 onwards he was a constant contributor, both of prose and verse, to "Blackwood's Magazine." He was the author of a series of articles, in that periodical, on Grimm's "Teutonic Grammar" and Grimm's "Philological Magazine," which excited much interest at the time, and the republication of which has often been urged. But his most lasting literary production will probably be his volume of "Songs and Verses, social and scientific, by an old contributor to 'Maga,'" which was first published in 1868. The motto of this volume might be "Ridentem dicere verum, Quid vetat?" In it playful and kindly satire appears in its best form, directed against the exaggerations, false sentiments, or fallacies of the day. And the songs themselves are so well written and so full of sparkling life that they may well continue to please many a future generation. His last published work was the "Greek Anthology for English Readers," and the preparation of this was with him indeed a labour of love. In it he was able to combine his scholarlike fondness for classical antiquity with his taste for the witty and the epigrammatic, and it afforded him infinite pleasure to labour at reproducing in the most terse and elegant English form the best flowers out of that garden of happy conceits and concinnities bequeathed to us by the Grecian muse. This work, though undertaken very late in life, was highly successful; the renderings were spirited, and they were accompanied with annotations, in which was much curious learning, very creditable to one with whom scholarship was not a business but a recreation.

Lord Neaves became a Fellow of the Royal Society of Edinburgh in 1856. In 1859 he delivered the opening Address of the Session, with a biographical notice of the venerable Principal Lee. Ten years later he gave another opening Address for the Session 1869–70. In the same session he read a paper on the "Primitive Affinity between the Classical and Low German Languages." In this paper he argued that the affinities between old English and Latin must have arisen from prehistoric identity or connection, and he pointed out certain limitations to which Grimm's law of affinities must be subjected. In 1871 he read a communication "On the Pentatonic and other Scales employed in Scottish Music," when airs illustrating his views were played by Mr Bridgman. In 1872 he read a paper on "Some Helps to the Study of Scoto-Celtic Philology," in which he pointed out the disguises which words belonging to the Aryan family of languages assume in passing into Gaelic, and showed the laws regulating those changes. Among other points of interest, the paper contained a discussion upon *so* and *do*, the Gaelic representatives of the Sanskrit prefixes *su* and *du*, which appear in Greek as εὐ and δυς—"well" and "ill."

In 1874 Lord Neaves furnished biographical notices of Lord Colonsay, Professor Cosmo Innes, and Mr Francis Deas; and in 1875 he read a paper "On some remarkable changes, additions, and omissions of letters in certain cognate European words." This paper dealt in a most interesting manner with the transformations of words, and showed the identity between many pairs of words in cognate languages, where the words themselves had grown to be widely dissimilar.

In this place we may surely say, "Quis desiderio sit pudor aut modus Tam cari capitis?" What he brought to us here from time to time was part of the stores which he was for ever gathering in his library, where during all leisure hours he would sit, with heaps of volumes about him, plunged and absorbed in books. In his house many of us have enjoyed his genial hospitality, where, surrounded by a bright and intellectual family, he was himself "a central warmth diffusing bliss," ever gay and radiant, a downright opponent indeed of all shams and false pretences, but yet with a large-hearted charity for all. Beneath a laughing exterior Lord Neaves concealed steadfast internal principles; the writer of this paper remembers the "sæva indignatio" with which, on one occasion, he turned upon a foreign gentleman who had uttered a flippant and infidel remark. In all the relations of life he was blameless, and by his example he has shown that it is possible to combine piety, learning, wisdom, and hard and successful work with kindliness, brightness, laughter and joy.

By the death of Mr Andrew Coventry, this Society, of which he had been for more than thirty-four years a Fellow, has lost one of the most amiable and accomplished of its members. Both Mr Coventry's father and his grandfather were men of mark. His grandfather was a Congregationalist minister of some eminence near Kelso; his father was the first Professor of Agriculture in the University of Edinburgh, and is spoken of in terms of admiration in the letters of Niebuhr, the great German scholar and historian, who at the beginning of this century was attending classes here. Mr Andrew Coventry was educated at the High School of Edinburgh, of which, when he left it, he was "second dux." He went through a course of study at the University, and seems to have paid particular attention to chemistry. In 1823 he was called to the bar, at which he made a good start, and was apparently rising, when, in 1840, he succeeded to the fortune of his uncle General Cuninghame, and relinquished professional pursuits. From this time forward he led the life of a *dilettante*, in the best sense of the word, for he really delighted both in science and in the fine arts, and exhibited no mean information in both these provinces. The first product of Mr Coventry's leisure, which has been preserved, consists in "A Letter to the Landed Interest of Scotland," advocating "the merits of the Association for employing an agricultural chemist." This pamphlet, which appeared in 1843, went through two editions. Mr Coventry travelled yearly on the Continent, taking great interest both in scenery and works of art; he collected, to a certain extent, both books and pictures, and in 1851 he had the good fortune to purchase in Westmoreland a very beautiful marble bust, which proved to be a genuine antique, and was pronounced by Mr Burgon of the British Museum to be the bust of Antonia Augusta, daughter of Mark Antony and Octavia, and mother of Germanicus and of Claudius. On this discovery Mr Coventry read a paper in 1852, being his first contribution to this Society. It may be mentioned that the bust itself is now the property of his nephew, Colonel Crighton. In 1854 he published anonymously a little treatise entitled "The Certainty of Christianity," which he maintained chiefly on historical grounds. In 1856 he delivered the "Ulbster Hall Lecture," on "Some of the most curious Inventions and Discoveries of Recent Times." This lecture gave evidence of very extensive reading and great fulness of knowledge as to the results of science, and the facts which it contained were set forth in a lively and striking manner. In 1857 Mr Coventry delivered the annual address to the students of the School of Design under the Board of Manufactures, and in so doing displayed the feeling and taste of a connoisseur. He always took a great interest in the Association for the Promotion of the Fine Arts; he was for many years a member of their managing committee, and more than once presided at their annual meeting. In later life, becoming a director of the Commercial Bank of Scotland, he began to devote himself to questions of finance, and in 1870 he read before us his second paper, which was entitled "A Method for Economizing our Currency." The idea was, that by demonetizing the sovereign and substituting for it notes, which should represent gold hoarded in the cellars of the Bank of England, a considerable saving might be effected in the wear and tear of gold. This idea, though ingeniously advocated, can hardly be said to have recommended itself to practical financiers. Of late, for many months before his death, Mr Coventry suffered from a painful and excruciating disorder, which he bore with manly fortitude and pious resignation. To the end he continued to solace himself as far as possible with his old pursuits. He was one of those who take a pride in always having read and being able to talk about the last new book. His conversation was always genial and interesting, and his loss has been felt in many circles, as it must also be in the Royal Society.

The next name in the obituary for this year is that of Sir DAVID DUNDAS, Baronet, a gentleman of that class who, without what has been called a "professional" acquaintance with science, art, or literature, are still acceptable members of a Society like this, if they show (as they must do by applying for admission) a sympathy for and interest in our pursuits. By their admission to be Fellows, a mutual compliment is at all events implied. Sir David Dundas was born in 1803. He was admitted an advocate at the Scottish bar in 1824, but never practised. He was a Deputy-Lieutenant for Perthshire, and lived the life of an active country gentleman, chiefly engaged in managing and improving his beautiful estate of Dunira. And he was respected by those who knew him as a most kind and honourable man.

By the death of Mr JOHN LOTHROP MOTLEY the Society has lost one of the most celebrated of its Honorary Fellows.

Mr Motley was born at Dorchester, Massachusetts, on the 15th of April 1814. He was educated at Harvard Unversity, where he graduated in 1831, at a very early age; after which he spent two years in Germany, residing successively in Göttingen and Berlin, and some additional time in travel in Italy and other parts of Southern Europe. On his return to America, he studied for the profession of the law; but, though he was called to the bar in 1837, he does not seem to have sought for practice. He had resolved rather that literature was his vocation. In 1839, at the age of five and twenty, he published a novel, called "Morton's Hope, or the Memoirs of a Provincial," which had little or no success, and is remembered now but slightly. In 1840 he was appointed Secretary to the American

Embassy in Russia; but, after having been for some time in St Petersburg, he resigned the appointment. Thenceforward, for nearly ten years, American biographical authorities represent him as living privately at home, engaged in miscellaneous studies and contributing miscellaneously to American periodicals. A second novel, which he published in 1849, under the title of "Merry Mount, a Romance of the Massachusetts Colony," had no better fortune than the first. Meanwhile he had been finding his true vein. The passion for history, apparent in the title and conception of this novel, had been manifesting itself more seriously and effectively in occasional historical essays. A review of Tocqueville's work on American Democracy and a paper on Peter the Great of Russia are mentioned as having attracted especial notice. By this time, indeed, it had became known among Mr Motley's friends that he had fastened on a great subject for elaborate historical treatment by himself. No one had adequately written the History of the Dutch People and their Commonwealth; there were affinities and attractions recommending this subject in a peculiar manner to an American of the United States; Mr Motley had been meditating these, and had determined to make the subject his own. To qualify himself by the necessary researches, he came to Europe again in 1850, and lived for several years at the Hague, and in Dresden and Berlin, immersed in documents. The result was the appearance, in 1856, when Mr Motley was forty-two years of age, of his "Rise of the Dutch Republic: A History," in three volumes. The work, published simultaneously in London and New York, was eagerly hailed by readers and reviewers on both sides of the Atlantic; translations of it into several European languages were at once begun; and the author at once took his place among the most eminent writers of his generation. Undisturbed by applauses and honours, Mr Motley persevered in his task. The volumes he had published, extending from the accession of Philip II. of Spain in 1555 to the death of William the Silent in 1584, had told only the story of the heroic revolt of the Dutch from the Spanish tyranny and of the triumphant foundation and beginnings of their little Republic of the Seven United Provinces. But, in 1860, there appeared the first two volumes, and in 1867, the last two, of what was virtually a sequel in four volumes, though it bore the independent title of "History of the United Netherlands from the Death of William the Silent to the Twelve Years' Truce, 1609." If of less riveting interest than the former work, these volumes were welcomed as of high historical value, and fully sustained the author's reputation. At the date of the publication of the first two volumes, and also at that of the publication of the last two, he was domiciled in London,—whence his two prefaces are dated; but, through nearly six of the intermediate years, he had been ambassador for the United States at the Court of Vienna, having been appointed to that important post in November 1861, and removed from it in 1867. On the accession of General Grant to the American presidency in 1869, Mr Motley was again in request for diplomatic service, and was made American minister in London. In consequence of some differences between him and the American Secretary of State, he retained the office only till November 1870. The rest of his life, which was passed mainly in England, though with visits to Holland, was devoted entirely to literary labour. In 1874, under the title of "The Life and Death of John of Barneveld, Advocate of Holland, with a View of the Primary Causes and Movements of the Thirty Years' War," he added two volumes more of Dutch history to his former seven, and brought his narrative down to the year 1623, when the farther fate of his beloved Dutch had became involved in the vast struggle between the opposed forces of Protestantism and Roman Catholic Absolutism all over Europe. It had been his intention to overtake this vast struggle too, and, in the form of an express history of the Thirty Years' War, to bring the Dutch safely through that great European entanglement, on to the Treaty of Westphalia in 1648, when, in the general pacification and winding up, the independence of the United Provinces was at last formally declared and guaranteed. This portion of his projected work he did not live to accomplish. He died, in an English country-house, on the 29th of May in the present year, at the age of sixty-three. Friendships and family connections, as well as tastes, had attached him much to England in his later years.

Mr Motley was a man of fine and stately presence, and of gravely courteous manners. Of his private life, as well as of his services as an American public man and diplomatist, accounts will probably be forthcoming in time. For the world at large, he lives, and will live, as the historian of the Dutch Republic. It was his distinction to have seized the great subject in the prime of his manhood, to have clung to it tenaciously through five and twenty years, and to have laboured assiduously all that while among archives and original authorities for the attainment of the adequate knowledge. It was no less his distinction to have infused into his treatment of the subject the glow and energy of his own personal convictions, the passionate and eloquent expression of his sympathies with the men and the principles that he believed to be in the right, and his detestation of those that he believed to be in the wrong, on a noted theatre of human action eight or ten generations ago. So far as there are differences of critical opinion about his writings, they chiefly depend indeed on the views that may be taken as to the limits of a historian's right to interweave his own moral and political approbations and censures with his narratives of the past. M. Guizot, who was an admirer of Mr Motley, thought that he went beyond bounds in this respect, and that his "alternations of extreme aversion and strong predilection" were irritating. The criticism has been repeated in England since Mr Motley's death. He had probably his own theory on the matter, and could have defended it well. It seems to have been an axiom with him that a historian, of competent knowledge and power of imagination, may trust himself to feel as accurately about the persons and transactions of three hundred years back as about those immediately around him, inasmuch as there are certain principles and tendencies of things that ought to be dear to all humanity, while their opposites are intolerable,—inasmuch as everywhere and in all times a Philip II. and all his belongings must be execrable, while a William of Orange or a Barneveld is to be honoured and revered. By acting on this view of a historian's duty, Mr Motley certainly increased the effect and popularity of his writings, and it does not appear that he subtracted from their permanent worth.

Shortly after the appearance of his first historical work, Mr Motley was elected a corresponding member of the Institute of France. He was also D.C.L. of Oxford, and LL.D. of Cambridge, Leyden, Harvard, and other Universities, Continental and American. He was elected an Honorary Fellow of the Royal Society of Edinburgh on the 1st of March 1875. We have lost him after a too brief connection.

The Society has this year to deplore the loss of one of the most eminent of its Honorary Members in the death of Mr Talbot. It is very rare indeed to find a man who attains to anything beyond mediocrity in more than one or two departments of knowledge. Mr Talbot is a remarkable exception to the general rule. Seldom has the world seen so many-sided a man. He attained to distinction in pure mathematics, in physics, in chemistry, in astronomy, in archæology, and in literature; nor is his name altogether absent from the records of botanical research. After studying at Harrow, Mr Talbot graduated in Cambridge in 1821; and although his name appears on the honour list in mathematics, it does not occupy so high a position as his subsequent career would have warranted us in expecting. In fact, his early promise was eminence in classical literature, especially in Greek. As an undergraduate he obtained the Porson prize for translation into Greek verse of a selected passage from an English dramatic poet. This was followed at his graduation by his obtaining one of the two Chancellor's medals, awarded "to the graduates who show themselves the greatest proficients in classical learning." Linguistic studies, though he never abandoned them, had not that prominence in his after life which might have been expected from this beginning. The year after taking his degree we find him contributing to "Gergonne's Annales" a paper on a mathematical subject, which was followed up by others in the same serial; and from that time, for upwards of fifty years, there emanated from him an uninterrupted stream of original papers on mathematics, physics, astronomy, chemistry, and archæology, as valuable as they were varied. To take even a hasty glance at his most important researches would vastly exceed the limits of this necessarily brief sketch. We must not, however, pass by without a few words his great discovery in photography. So early as 1826 he had turned his attention to the chemical action of light. The results were communicated to the "Edinburgh Journal of Science" and other periodicals. In 1833, when sketching on the shores of Lake Como, he availed himself of the *camera lucida*, and it occurred to him that images might be fixed on the paper by chemical action. The idea was not altogether novel, but it had not as yet led to any definite result. On Mr Talbot's return to England, he commenced a series of experiments on the decomposition of nitrate of silver by light, which he continued for many years. In 1835 he had reached the length of producing images on paper by the *camera obscura;* but being anxious to perfect his discovery, he contented himself with issuing brief notices of the results in the "Philosophical Magazine" and the newspapers. It was not until the 31st of January 1839 that he communicated to the Royal Society the first of the papers on a process which will always be associated with his name. The paper is entitled "Some Account of the Art of Photogenic Drawing, &c." This paper was followed up, three weeks later, by a memoir, describing the process for preparing the paper, fixing the image, &c. The following year saw Mr Talbot's crowning discovery—that which virtually established the art as it now exists—the development of an invisible image.

Daguerre in France had been at work at the same subject, using metal instead of paper, and his discoveries were given to the world almost simultaneously with those of Mr Talbot, It is not necessary here to refer to the subsequent history of calotype. It was beautiful at its very birth, and its beauty was recognised nowhere more fully than in Scotland. Many members of the Society will remember how, soon after the discovery was made known, the process was employed by Dr Adamson and Mr D. O. Hill to the production of life-like portraits in a style which has never been excelled. The simultaneous inventions of the daguerreotype and the calotype naturally created jealousies on both sides the Channel. Mr Talbot found a warm and able advocate in Sir David Brewster. These controversies may now be consigned to oblivion. If the French did injustice to Mr Talbot in the early days of photography, they made amends at a later period. At the Paris Exhibition of 1867, the Commissioners awarded him the great gold medal for his contributions to photography, although he was not an exhibitor.

It must not be inferred from the prominence given to Mr Talbot's connection with photography, that his other contributions to literature and science were unimportant. It is far otherwise. In the most recent large treatise on the differential and integral calculus, that of M. Bertrand, the reader will find the name of Mr Talbot associated with those of James Bernoulli, Fagnani, and Chasles. Mr Talbot was one of the early promoters of the Society of Biblical Archæology, and contributed largely to their Transactions. Some thirty years ago, Mr Talbot, Sir Henry Rawlinson, Dr Hinks, and M. Appert agreed to decipher independently certain cuneiform inscriptions brought from Nineveh, and to compare the results. The comparison showed a general harmony amongst the four interpretations, but by no means an identity. Mr Talbot's genius was too original and of too high an order to fail of recognition. The Royal Society of London conferred on him both their Royal and their Rumford medal. It also assigned to him the Bakerian Lecture for 1856, the subject being "Further observations on the optical phenomena of crystals." Mr Talbot's contributions to our own Transactions consist for the most part of historical sketches or short papers on subjects lying on the borders of his early investigations; such are the papers on "Fermat's Theorem," on "Fagnani's Theorem," &c. Perhaps an exception to this remark may be found in the paper "On a new mode of observing certain spectra," based on experiments made in Professor Tait's laboratory. In his brief note on "Anomalous Spectra" he shows that he long ago anticipated the wonderful discovery of Le Roux and Christiansen, but was prevented from publishing his observations by the advice of Sir D. Brewster. In private life Mr Talbot was shy and reserved to strangers, but a lively and animated talker when in the congenial society of old friends, such as Sir David Brewster, and indeed of some younger men whom he honoured with his friendship. He sat for two years in the first Reformed Parliament. Of this he was reminded by Lord Palmerston, when in 1863 the University of Edinburgh conferred on these two men, so very unlike each other, the honorary degree of LL.D. Many honours from foreign societies were conferred on Mr Talbot, but it is not easy to record them, from the fact that, with the accustomed modesty of true genius, he shunned to display them.

He died September 17, 1877, in his 78th year.

Dr James Bryce was born at Killaig, near Coleraine, in the north of Ireland, on October 22, 1806. He was the third son of a Scottish Presbyterian minister, who had settled there three years before, and obtained the earlier part of his education from his parents, who were both, in somewhat different ways, persons of remarkable intellectual gifts and cultivation. At the age of fourteen he was sent to the University of Glasgow, where his father and his eldest brother had been before him, and where he graduated M.A., receiving from it in after life the honorary degree of LL.D. There he threw himself, with characteristic ardour, into the studies of the classes, winning, among other distinctions, the Greek Blackstone prize, which was then, and appears to be still considered, the most considerable honour awarded in the linguistic classes. Although he had been from childhood fond of nature and all natural objects, the bent of his mind towards science did not definitely express itself till he accepted, at the age of twenty, the post of head of the mathematical department in the Belfast Academy, then one of the chief foundation schools in Ireland, and of which his eldest brother, the Rev. Dr Bryce of Belfast (who is still living) was then Principal. Finding the teaching of geography to belong to his department, he at once saw what hardly any one had then seen, how important a branch of that subject physical geography constitutes, and perceived that to deal adequately with it a knowledge of geology and mineralogy was necessary. From these he was led on to zoology and botany, and not only threw himself into these topics himself with characteristic ardour, but formed large classes for their study, which were attended as well by his own pupils as by other young people in the town. At the same time he had begun to connect the teaching of natural philosophy with that of mathematics, explaining and illustrating by experiments the leading principles of mechanics, pneumatics, hydrostatics, electricity, chemistry, and other cognate branches of inquiry. We have listened to so many discussions during the last few years respecting the necessity of giving to natural science a regular and important place in schools and colleges, and now see so much actually done to effect that object, that it is hard to realize the state of opinion and practice in these matters fifty years ago, when such educational reforms were unheard of, and little beyond Latin and Greek was taught in all the best schools of the United Kingdom. It needed a very warm love for science, and a very elevated view of the functions of education, to lead a young man in those days to introduce, on his own idea, so great a change in the established method of instruction. An able teacher perpetuates himself through his disciples, and many eminent scientific workers trace back to Dr Bryce the first impulse towards, the first guidance in, the study of nature which they received. His services in promoting, by example and by argument, both in Belfast and afterwards in Glasgow, the introduction of these subjects into a school course can hardly be overestimated. While inspiring others with his own tastes, he had also begun to do valuable scientific work on his own account, partly in botany and mineralogy, but chiefly in geology. He investigated and described certain interesting fossil plant beds (similar to the leaf beds of Mull) occurring on the shores of Lough Neagh, and contributed to the "Philosophical Magazine" an account of the important discovery which he had made of the remains of the Plesiosaurus in the lias of Antrim, which conclusively established its identity with the lias series of England. Pursuing his researches into the geology of the northern counties of Ireland, he was the first accurately to examine and describe the structure of the remarkable basaltic formations on the coasts, and particularly of the Giant's Causeway. A series of papers contributed to the Journals of the Geological Societies of London and Dublin, of which he had become a Fellow in 1834, attest his activity in observation; while in Belfast itself he was one of the most active members of a local scientific society, which numbered many men of eminence among its members, and exerted a powerful influence on the culture of the town, now the most prosperous in Ireland.

He removed in 1846 to Glasgow, on being appointed head of the mathematical and geographical department in the High School of that city. Here, in a larger field, he continued the same enlightened system of science teaching which he had started in Belfast. He joined the Philosophical Society, was soon placed on its Council, and was for three years its president, delivering in that capacity addresses in which he reviewed the progress of scientific inquiry, and discussed some of the main problems now lying before it with a completeness and accuracy which supplies remarkable evidence of the width of his cultivation, as well as the keenness of his observing and reflective powers. There was scarcely a department of science of whose leading principles and method he did not show himself master, a remarkable achievement in an age when every study has become so much sub-divided. His geological work during this period lay chiefly but not exclusively in the Clyde basin and its surrounding mountain groups. Papers on the "Parallel Roads of Lochaber" and the "Glacial Phenomena of the Lake District of the North of England" were followed by a systematic treatise on

"The Geology and other Natural Phenomena of Arran," which has reached a fourth edition, and constitutes the most complete and satisfactory account of that interesting island.

Latterly he had devoted himself chiefly to the geology of the North-West Highlands—first to that of Skye and Raasay, afterwards to the determination of the age of those rocks in Ross and Sutherland which have excited so much discussion among our leading geologists. In several expeditions he had visited and examined the sandstones of Loch Torridon and the remarkable limestones of Assynt and Durness. It was while on his way northward to complete these investigations that he met, at Inverfarigaig, on the shores of Loch Ness, with the accident which closed his life, by the fall of a pile of loose rocks which he was trying with his hammer.

He was then (July 11, 1877) seventy-one years of age, but still in full strength of body and mind, with his interests in science unabated. He had come in 1874, on resigning his post at Glasgow, to live among us in Edinburgh, had been elected to this Society, and seemed likely to prove one of its most zealous and useful members, when he fell by a heroic death in the service of the science to which his energies and talents had been so long consecrated.

As an observer Dr Bryce was unwearied and careful, as a describer and lecturer eminently lucid and graphic. He had also the gift of being able to communicate to others his own enthusiasm, and it was in great measure this, joined to a warm and genial manner, that made him so successful as a teacher. His love for nature and her beauties rose to a passion rare even among men of science; a ramble among woods and mountains was always to him a pleasure far more intense than any which ambition could promise or wealth purchase. His intellect was indeed an imaginative one, as appeared both in the vivid descriptions of scenery which occur here and there in his scientific papers, and in his own love for poetry and the dramatic aspects of history, branches of literature to which he had been devoted from boyhood.

Although it is as a man of science that he was chiefly known, Dr Bryce took an active part in current questions, especially those relating to education. He was one of the first—perhaps the very first—in Scotland to insist on the necessity for a reform in the constitution of the Scottish Universities, and on the recognition of the right of the graduates to a share in their government, and he organised the first association formed for that purpose. He held strongly that the educational system of Scotland ought to preserve its distinctive character, instead of being administered from London and assimilated to that of England. Among minor reforms which he advocated was that of the introduction of a decimal system of coinage, weights, and measures, on which he wrote a short but weighty treatise.

Besides the geological writings already mentioned, he was the author of several educational books, such as treatises on algebra, geometry, and the elements of astronomy, and a cyclopædia of geography. Most of these have gone through several editions.

It only remains to add that Dr Bryce's gifts as a scientific writer and teacher were not more remarkable than the simplicity and charm of his character. He retained into old age not only the fire and freshness of youth, but a perfect sweetness of temper, and an ungrudging readiness to place his time and knowledge at the service of others, which advancing years seldom spare.

His memory as that of one of the most lovable of men will be long cherished by hundreds of former pupils, with many of whom, scattered through India and the Colonies, he kept up intimate relations, and by the large circle of scientific friends whom he was accustomed to meet year after year at the meetings of the British Association and of our own learned societies.

The Rev. David Thomas Ker Drummond, descended from a very old Highland family of that name, was the youngest son of James Drummond of Strageath, and was born at Edinburgh on 25th August 1805. He was educated at the High School and the University of Edinburgh, with the view of entering the legal profession; but he subsequently proceeded to Worcester College, Oxford, where he took his bachelor's degree in 1830. He was ordained a deacon the same year by the Bishop of Llandaff, and a priest in 1831 by the Bishop of Gloucester and Bristol. For the first two years of his ministry he occupied the office of curate at Henbury and Compton, and then came to Edinburgh, where he entered into the incumbency of St Paul's Episcopal

Episcopal Chapel, Carrubber's Close. Having remained here for six years, he became joint-minister of Trinity Chapel, Dean Bridge, along with the late Mr Coventry, where he laboured with much acceptability, until his connection with the Scottish Episcopal Church was severed in 1842. He then became incumbent of St Thomas's English Episcopal Chapel, and remained in the spiritual oversight of the congregation there for upwards of thirty years. During the long period of his ministry in Edinburgh, Mr Drummond endeared himself to his congregation, both by his public and private ministrations. With the ministers of other denominations he was always on the most cordial and friendly terms, and no good work, having for its object the religious or moral improvement of the people, ever failed to elicit his co-operation and sympathy. By the general public he was long known at once as the zealous advocate and upholder of English Episcopalianism in Scotland, and as an active and valued friend of evangelical movements. His writings, also, although not extensive, served to bring his name before a considerable section of the community. The most noteworthy of these were, a series of Lectures on the 17th Chapter of St John's Gospel, a Life of Montague Stanley, and Scenes and Impressions in Switzerland and the North of Italy.

Mr Drummond became a Fellow of the Royal Society in 1868. He was fond of intercourse with the lovers of literature and science, and took much pleasure in the study of natural science, especially during his residence in the country, and his visits to the Continent of Europe. He was also an excellent photographer, and executed some fine views of Scottish scenery.

Mr Drummond, who was not a man of a strong constitution, resigned his charge with advancing years, in the autumn of 1875, and only a short time ago he changed his residence from Edinburgh to Pitlochry. He was in his usual spirits on Friday, 8th June, and went to bed in apparently good health. Early on the morning of the 9th, he was seized with sudden illness, and died in a few minutes, leaving a widow and a widowed only daughter to mourn his loss.

He was buried in the churchyard overlooking the picturesque loch at Duddingstone, in the grave where he had laid his only son forty years before. The service was read by Bishop Beckles, assisted by the Rev. W. Scott Moncrieff, vicar of Bishop-Wearmouth, who was for some years Mr Drummond's colleague in St Thomas', and a large concourse of friends testified to the affection and respect in which he had been held. His funeral sermon was preached in St Thomas' and St Vincent English Episcopal Chapels, by the Rev. C. T. Astley, vicar of Gillingham, Chatham, who had formerly assisted him at St Thomas'.

William Keddie became a Fellow of the Royal Society of Edinburgh in 1867, and died suddenly at Oban on 26th July 1877. He was born at Peebles on 22d March 1809, and with the exception of the first seven years, all his life-time was spent in Glasgow. At the age of thirteen he entered a printing establishment, and continued in it till he was twenty years of age. In 1832 he became sub-editor of the "Scottish Guardian," and contributed many valuable scientific articles to that newspaper. He attended also scientific classes at the University of Glasgow and the Andersonian College. He was a pupil of the Botanical class in the former, during the year 1843 when Dr Balfour was Professor. He ultimately became principal editor of the "Guardian." He was connected with the paper for twenty-seven years. Mr Keddie afterwards lecturerd on Natural History in the Free Church College. He made an excellent collection of geological and zoological specimens, which were handed over to the College. He was elected Secretary of the Glasgow Philosophical Society and editor of its Transactions. He published accounts of his visits to various Botanic Gardens in Britain, as well as to the Bass Rock and the Highlands of Scotland. His kind and genial manner and his fine flow of spirits made him a most pleasant companion in scientific excursions. His loss is much felt by the followers of science in Glasgow.

The following Communications were read:—

2. Report of the Deputation to Upsala. By Alexander Buchan, M.A.

The Deputation appointed by the Council to represent the Society on the occasion of the 400th anniversary of the founding of the University of Upsala consisted of Mr Sprague and myself. Professor Balfour and Professor Sir Wyville Thomson, who represented the Edinburgh University, also joined the Deputation.

The Latin Address from the Society for presentation by the Deputation having been prepared, was signed in the absence of the President by the Secretary. The Address, a copy of which accompanies the Report, is in its conception and execution, a characteristic specimen of quaint and exact Latinity. It is wholly the work of Mr Gordon, the Society's Assistant Librarian.

The Deputies assembled by previous arrangement at the Grand Central Railway Station, Stockholm, on the afternoon of Tuesday, September 4, to be conveyed by royal express train to Upsala. At 4.15 P.M. the train left the station amid the cheers and congratulations of an immense assemblage of the inhabitants. About seven o'clock Upsala was reached, and the whole of the inhabitants appeared to have assembled at the station to do honour to their guests. Of this great assemblage, the white caps of the students filled the whole central space. Young Count Hamilton, in name of the students, welcomed the Deputies, and thereafter the renowned choir of this University sang one of the Swedish national airs.

In the evening a meeting of all the Deputies was held for the purpose of deciding on the order of procedure to be observed in presenting the Addresses on the Wednesday. Since the number of learned bodies represented amounted to about seventy, it was resolved that, to save time, the different bodies be grouped into nationalities, the Deputies choosing their own representative speaker, while the Addresses would be presented without remark. The British Deputies chose Professor Balfour as their representative speaker. The British Deputies were, in addition to those already named, Professor Humphry for Cambridge University, and they were joined by Dr Curling of London, Fellow of the Royal Society, the Rev. Ch. H. H. Wright, Belfast, Bampton Lecturer elect; and the Rev. Mr Metcalfe, Senior Fellow, Lincoln College, Oxford.

The festivities of the 5th were opened with salvos of cannon from the castle and peals of bells from the cathedral, and the streets were early astir with crowds already in evening costume. By nine o'clock the side aisles and tribunes of the transept of the cathedral were filled to overflowing with gaily dressed ladies. The procession formed in the upper hall of the Carolina Rediviva—the University Hall—which is a handsome structure built on a fine site overlooking the city. It then wound its way slowly down the beautiful avenue of limes called Odin's Grove to the cathedral, headed appropriately by the students with their guests from the other Scandinavian Universities, with the appropriate banners of the thirteen nations into which the Upsala students are divided; followed in order by representatives of the Universities and learned Societies of Sweden, Iceland, Copenhagen, Helsinfors, and Christiania; representatives of the Parliaments, the officials of the University of Upsala, the Charter of its foundation being carried by the Secretary of the Academy; State Councillors and Knights of the order of the Seraphim; and other civil, military, and Court functionaries, members of Parliament, and the Honorary Doctors, the rear being brought up by the municipal and other authorities of the city, and by all the other not included in the above. When all were seated, Professor Sahlin, the rector, went out to receive the King, Crown Prince, and their suite. The cantata composed for the occasion by Mr Charles D. de Wirsén was a striking feature of the day's festivities. A Latin service having been performed by Archbishop Sundberg, the Rector welcomed the Deputies in a Latin oration, and thereafter the Deputies presented the Addresses in the inverse order in which they had joined the procession. A speech from the Rector and the concluding part of the cantata brought the ceremony to an end about 2.30 P.M.

At 3 P.M. a dinner was given by the University in a large handsome hall, specially built for the festivities in the Botanic Garden. The King presided, and covers were laid for 450 guests. By far the best speech on the occasion was the King's, in reply to the toast of his health, proposed by the Rector. He urged the advantages of a scientific and classical education to even the poorest of his subjects;

sketched with a rapid but firm hand the condition of Sweden when the University was founded, and the salient points of its subsequent history; and the important part played by the University, as seen in its brilliant history during these 400 years; gave expression to a fervent prayer that it would continue to maintain and extend its renown; and concluded a most animated and eloquent speech by announcing the gift of 40,000 kronors (2000 guineas) from the Crown to the University for the encouragement of scientific research.

The town was illuminated during the evening, and what may be called the club-houses of the various "nations" were thrown open by the students, with the view of giving the guests some idea of this characteristic phase of student life at Upsala.

The ceremonial of the Thursday was the conferring of degrees, which statedly takes place in June, at the close of the spring session, but which was appropriately deferred this year so as to form part of the quater-centenary commemoration. The festivities of this day were ushered in with the same formalities, and fortunately with the same brilliant weather as favoured those of the previous day. The doors of the cathedral were thrown open at 8.30 A.M., and the seats set apart for the ladies were rapidly filled, the best places this day being reserved, not as on the Wednesday in accordance with social position, but for the friends of those who were to be made doctors in the four faculties.

The procession set out from the Carolina Rediviva at 9.15 A.M., differing from the procession of the day before in the chief place being assigned to the four Faculties and the doctors elect; and all were in due time seated in their places in the cathedral in the same admirable order that marked the whole proceedings. The King, Crown Prince, and their suite were again present, being received in the porch by the Chancellor of the University, the Archbishop, the Rector, and the four Promoters. The cantata for the promotion was the work of Mr Victor Rydberg, a popular poet, and one of the eighteen of the Swedish Academy. This cantata, with the music set to it, was, like the cantata of the previous day, of a very high order of merit, and was admirably rendered by the choir.

The ceremony of promotion occupied about three hours, the degrees so conferred being strictly limited to persons resident in Sweden, Norway, Denmark, and Finland, the universities of these countries being in peculiarly close relationship with that of Upsala. The new doctors of medicine, law, and philosophy are nominated by these respective Faculties of the University; but the doctors of theology are nominated by the King from a list submitted to him by the Theological Faculty.

Among the many quaint traditional forms with which the ceremonial was conducted may be mentioned the firing off of a piece of ordnance instantly on the crowning of each doctor; a gold ring put on the finger of each doctor in law, medicine, and philosophy; the crowning of the doctors of philosophy with wreaths of real laurel; and the recrowning of doctors of fifty years' standing, a considerable number of such jubilee doctorates being conferred. At 3 P.M. the promotion dinner was given in the large hall in the Botanic Garden, covers being laid for 1600. At dinner and during the rest of the evening the doctors of philosophy still wore their laurel wreaths.

In the meantime the gates of the Botanic Garden had been thrown open to the dense throng of the public which had been waiting outside. Shortly thereafter the King and rest of the company repaired to the open portico of the Hall, where speeches in all languages were delivered to the assembled crowds, first in front of the fine statue of Linnæus, which was crowned with laurel on the occasion, and afterwards from the broad staircase of the building, commanding a fine view of the dense crowd which filled the broad avenue leading to the Castle. Among the speakers were Chancellor Count Hamilton, Donders of Utrecht, Topelius the popular Scandinavian poet, and Professor Balfour. The speeches were varied with songs from the students, whose white caps filled the middle space of the avenue, and whose wild but well-ordered enthusiasm forms one of the pleasantest reminiscences of the festivities. As darkness set in the gardens were illuminated, and a little later there was a display of fireworks, some of the pieces being very fine, particularly one representing the new buildings of the University which are in contemplation. The festivities of the day ended with a torchlight procession of about 1000 students, who marched with their banners to the stirring music of the choir that led the procession on to the Castle, to pay their respects to the King.

On the Friday the large hall of the Carolina Rediviva was crowded by twelve o'clock with an audience of about 4000, for the concert given by the celebrated choir of the students. The membership of the choir is limited to 500, and as all in Sweden are taught music in the elementary and secondary schools, and as it is regarded an object of ambition to be admitted a member, there is no difficulty in maintaining the choir in a state of the highest efficiency. On this, as on other high occasions, old members wearing the little rosette of membership were permitted to join the choir. It is enough to say that the concert was a very fine one, and it may be added that a degree of excellence was achieved which no existing university choir could rival. The pieces selected for the concert were essentially Scandinavian, and were remarkable for the strong patriotism and inextinguishable love of freedom which breathed through them, and for a desire for union among the three Scandinavian nations. In the evening the town gave a ball, at which the King, Crown Prince, and suite, and about 7000 guests were present in the hall in the Botanic Garden—a hall, by the way, which was levelled with the ground on the following day.

By mid-afternoon of Saturday the guests had returned to Stockholm, and at 6.30 P.M. they met at the pier to be conveyed by six steamers, specially engaged for the purpose, to Drottningholm Palace. Invitations to supper were issued by His Majesty to 700 guests. The magnificent rooms of this the stateliest of the summer palaces about Stockholm were thrown open to the guests, the King freely and cordially mingling with the company, as he did during the whole of the festivities. After supper the foreign deputies were invited to meet His Majesty in one of the larger rooms, where, after a graceful speech, to which one of the deputies replied, the King touched glasses, and shook hands with many of the deputies, and bade a cordial good-bye to all. The grounds of the palace were finely illuminated.

Thus worthily terminated the commemoration solemnities of the 400th anniversary of the founding of Upsala University, the celebration and festivities being conducted in a manner and with a munificence of which Upsala and Sweden may well be proud.

The Latin Address above referred to, which was presented by the Deputation, is as follows :—

Amplissimis Curatoribus, Rectori Magnifico, Doctissimoque Senatui Universitatis Upsaliensis.

Societas Regia Edinensis nos jussit, viri illustrissimi, vobis hoc sollemni die gratulari, quo nihil exoptatius nobis evenire potest. Itaque nobis animo perpendentibus quam excultus studiorum status hodie sit, et quantopere vos in his augendis et amplificandis excellueritis, grato animo laudanda est illa fructuosissima opera quam vos et Academia vestra in finibus scientiarum dilatandis posuistis; et admirationem observantiamque nostram in viros præclaros qui temporibus præteritis exornaverunt et in successores eorum qui nunc exornant Academiam vestram libenter enuntiamus. Ab Upsala Societas nostra Regia semper lumen exspectavit et accepit; in Astronomia, Meteorologia, rebus Botanicis et Physicis, Historia Naturali, reliquisque studiis Academicis, in quæ Socii nostri continuo incumbunt, problemata difficillima solvistis, naturæ recondita extricavistis, patefecistis et illustravistis; et vestro proventu secundo Socii nostri in difficultatibus superandis valde profecerunt. In regionibus Septentrionalibus juventutem disciplinis humanis et subtilioribus instituendo Universitas Upsaliensis de republica literaria optime meruit, et laboribus suis philosophicis de rerum natura orbi terrarum notissimis fons et origo luminis fuit, imo quidem aurora borealis, vel potius sol meridianus, Sueciæ et aliarum nationum.

Ut gloria vestra per omnia sæcula permaneat ex imo pectore optamus et precationibus ominamur.

JOANNES HUTTON BALFOUR, M.D.,
Societatis Regiae Edinensis Secretarius.

Nonis Sextilibus,
ANNO CHRISTI MDCCCLXXVII.

3. On a Method of Determining the Cohesion of Liquids. By J. B. Hannay, F.C.S.

(*Abstract.*

In this paper the author concludes that the measurement of the breaking-strain of liquids is the only universally applicable method of measuring their cohesion; and as dropping is the phenomenon in which the breaking-strain can be most easily measured, he examines the work already done in this direction. Dr Guthrie's theory that,

the increase in the size of drop, with the increase in the rate of dropping, is due to the attraction of the solid tearing more of the root of the drop in low than in high rates is put to a crucial test by an experiment in which mercury drops from a wide glass tube so arranged that the tube only acts as a support for a column of liquid from the end of which the drops fall. In this case there is no solid to reclaim by adhesion any of the drop, and yet there is the same increase in size as the rate increases. The author accounts for the increase as follows :—1. The rupture of the neck of a drop is not an instantaneous process, but lasts for a short time, and during that time liquid is flowing into the drop through the neck, and the faster the flow the greater is the increment of the drop during rupture. 2. When the rate is high the breaking neck has a longer life-time, as the stump follows after the full drop as in the beginning of the formation of a stream. Briefly stated, the quicker the rate the larger the drop, because more liquid flows into the drop after the rupture has commenced, and the longer does that flow continue. The author calls a drop when it begins to break a "normal" drop; and to find its weight he determines the decrease of weight with decrease of rate, and reduces the latter to zero when the weight of a normal drop is found. Apparatus is shown by which experiments were carried out, and inaccuracies eliminated. The normal drop is found to weigh 0·4130 grm., and as the width of the neck is found to be 3·395 mm., this gives a breaking strain of 0·0456 grm. per square millimeter for mercury at 16° C.

4. Note on Vector Conditions of Integrability. By Professor Tait.

(1.) The relation

$$d\sigma = uqd\rho q^{-1}$$

ensures that the tensor of $d\sigma$ shall always be u times that of $d\rho$ Hence, if ρ be the common vector of three series of surfaces which together cut space into cubes, σ possesses the same property. (See § 6 of my paper *On Orthogonal Isothermal Surfaces*, Trans. R.S.E., 1873–4. In what follows this paper will be referred to as Ω.)

We may suppose the tensor of q to be any constant, unity suppose. Then, from

$$Tq^2 = 1,$$

we have

$$S.dqKq = S.dqq^{-1} = 0 .$$

Thus, it appears that

$$q^{-1} . dq \text{ and its equal } - dq^{-1} . q$$

are vectors.

(2.) From the given equation we have

$$\frac{d\sigma}{dx} = uqiq^{-1} \text{ and } \frac{d\sigma}{dy} = uqjq^{-1} .$$

From these

$$q^{-1} \frac{d^2\sigma}{dxdy} q = i\frac{du}{dy} + 2uV.i\frac{dq^{-1}}{dy} q = j\frac{du}{dx} + 2uV.j\frac{dq^{-1}}{dx} q .$$

From the three equations of this form we obtain by the operations $S.i$, $S.j$, $S.k$, nine scalar equations, of which the following are three :—

$$\frac{du}{dx} = 2uS.k\frac{dq^{-1}}{dy}q ,$$

$$\frac{du}{dy} = -2uS.k\frac{dq^{-1}}{dx}q ,$$

$$S.j\frac{dq^{-1}}{dy}q = -S.i\frac{dq^{-1}}{dx}q .$$

The last of these, with its two similar equations, shows that

$$S.i\frac{dq^{-1}}{dx}q = S.j\frac{dq^{-1}}{dy}q = S.k\frac{dq^{-1}}{dz}q = 0$$

which express Dupin's theorem for this particular case.

(3.) If we put for simplicity

$$dv = \frac{du}{2u}$$

the equations of last section give at once three like

$$\frac{dq^{-1}}{dx}q = V.i\nabla v ,$$

so that

$$dq . q^{-1} = V.d\rho\nabla v , \quad . \quad . \quad . \quad . \quad (\Omega\ (33).)$$

and

$$\nabla q^{-1} . q = \Sigma . iVi\nabla v = -2\nabla v = -\frac{\nabla u}{u} ,$$

or

$$\nabla . uq^{-1} = 0 . \quad . \quad . \quad . \quad . \quad . \quad (\Omega\ (13).)$$

(4.) But we have, by differentiation, from the second equations of § (3),

$$\frac{d^2q^{-1}}{dxdy}q + \frac{dq^{-1}}{dx} \cdot \frac{dq}{dy} = \frac{d}{dy}\text{V}.i\nabla v\,,$$

$$\frac{d^2q^{-1}}{dydx}q + \frac{dq^{-1}}{dy} \cdot \frac{dq}{dx} = \frac{d}{dx}\text{V}\,j\,\nabla v\,.$$

Subtracting, and noticing that

$$\frac{dq^{-1}}{dx} \cdot \frac{dq}{dy} = -q^{-1}\frac{dq}{dx} \cdot q^{-1}\frac{dq}{dy}\,,$$

we have

$$2\text{V}\,.\,q^{-1}\frac{dq}{dx}q^{-1}\frac{dq}{dy} = \text{V}\,.\left(j\frac{d}{dx} - i\frac{d}{dy}\right)\nabla v = \text{V}.\text{V}\,(k\nabla)\,\nabla v\,,$$

or

$$2\text{S}(k\nabla v)\,.\,\nabla v = \text{V}.\text{V}\,(k\nabla)\,\nabla v\,.$$

Three like this give at once

$$(\nabla v)^2 = -\nabla^2 v$$

or

$$0 = 2u\nabla^2 u - (\nabla u)^2 = 4u^{\frac{3}{2}}\nabla^2 (u^{\frac{1}{2}}) \quad . \quad . \quad . \quad (\Omega\ (21).)$$

(5.) But if, instead of combining the last set of three we equate to zero the scalar coefficients of i, j, k separately in each, we have three equations of each of the following forms :—

$$2\frac{dv}{dx}\frac{dv}{dy} = \frac{d^2v}{dxdy}\,,\quad \frac{d^2v}{dx^2} + \frac{d^2v}{dy^2} = -2\left(\frac{dv}{dz}\right)^2.$$

Transformed to u, they become

$$2\frac{du}{dx}\frac{du}{dy} = u\frac{d^2u}{dxdy}\,,\quad \&\text{c.}$$

$$u\left(\frac{d^2u}{dx^2} + \frac{d^2u}{dx^2}\right) = \left(\frac{du}{dx}\right)^2 + \left(\frac{du}{dy}\right)^2 - \left(\frac{du}{dz}\right)^2,\quad \&\text{c.}$$

The integrals of the first three are obviously

$$\frac{1}{u^2}\frac{du}{dx} = \text{X}'\,,\quad \frac{1}{u^2}\frac{du}{dy} = \text{Y}'\,,\quad \frac{1}{u^2}\frac{du}{dz} = \text{Z}'\,.$$

where the right hand members are functions of x, y, z respectively. Thus

$$\frac{1}{u} = -\text{X} - \text{Y} - \text{Z}$$

and the first of the second set of three equations becomes

$$u^3(X''+Y''+2uX'^2+2uY'^2) = u^4(X'^2+Y'^2-Z'^2),$$

or

$$X''+Y''=-u(X'^2+Y'^2+Z'^2).$$

Thus

$$X''=Y''=Z''=C,$$

and

$$\frac{1}{u}=-\frac{C}{2}\Big[(x-a)^2+(y-b)^2+(z-c)^2\Big]+C',$$

or, as we may take the origin where we please,

$$u \propto \frac{1}{CT\rho^2+D},$$

This is, therefore, the *only* value of u which satisfies the conditions of the problem, and the last equation in § 4 above shows that either C or D must vanish. If C vanish, u and q are both constant.

(6.) If D vanish, we have by § 3 above

$$-dq\,.\,q^{-1}=V.d\rho\frac{\nabla u}{2u}=-V\frac{d\rho}{\rho}=-dU\rho\;(U\rho)^{-1}.$$

This gives

$$q=aU\rho$$

where a is any constant versor.

Also

$$d\sigma=\frac{c^2}{T\rho^2}aU\rho d\rho(U\rho)^{-1}a^{-}$$

$$=-a\,d\left(\frac{c^2}{\rho}\right)a^{-1},$$

so that σ is the *Electric Image* of ρ rotated through any angle about any axis through the centre of the reflecting sphere. (Ω § 12.)

(7.) If the equations of any three systems of *orthogonal* surfaces be

$$F_1=C_1,\ F_2=C_2,\ F_3=C_3,$$

we may obviously write for the flux of heat through each the expressions

$$\nabla F_1=u_1qiq^{-1},\ \nabla F_2=u_2qjq^{-1},\ \nabla F_3=u_3qkq^{-1};$$

so that we have three equations of the form

$$\nabla(u_1qiq^{-1})=a_1,$$

where a_1, a_2, a_3 are *scalars*, which separately vanish when the systems are *isothermal*.

Expanding the last equation we have

$$\frac{\nabla u_1}{u_1} qiq^{-1} + \nabla q \,.\, q^{-1} \,.\, qiq^{-1} + qiq^{-1}\nabla q \,.\, q^{-1} - 2\mathrm{S}\,(qiq^{-1}\nabla)\, q \,.\, q^{-1} = \frac{a_1}{u_1}$$

or, writing

$$qiq^{-1} = i' ,$$

$$\frac{\nabla u_1}{u_1} i' + 2\,\mathrm{S}.i'\nabla q\, q^{-1} - 2\,\mathrm{S}(i'\nabla)\, q \,.\, q^{-1} = \frac{a_1}{u_1} .$$

We obtain Dupin's Theorem in its most general form by operating by $\mathrm{S}.i'$, $\mathrm{S}.j'$, $\mathrm{S}.k'$ on this and the two similar equations respectively. It is thus expressed as three equations, of which one is

$$\mathrm{S}.i'\,\mathrm{S}\,(i'\nabla)\, q \,.\, q^{-1} = 0 .$$

Again, by multiplication by i', and by adding the other two equations multiplied by j' and k' respectively, we obtain also

$$\Sigma\,\frac{\nabla u}{u} + 2\Sigma i'\,\mathrm{S}i'\frac{\nabla u_1}{u_1} - 2\mathrm{V}.\nabla q\, q^{-1} + 2\nabla q \,.\, q^{-1} = \Sigma\,\frac{a_1 i'}{u_1}$$

or

$$\Sigma\,\frac{\nabla u}{u} + 2\mathrm{V}.\nabla q\; q^{-1} \;+\; 2\mathrm{V}q \,.\, q^{-1} = \;-\;\Sigma\,\frac{a_1 i'}{u_1}$$

whence

$$\mathrm{S}.\nabla q\, q^{-1} = 0 ,$$

and

$$\Sigma\,\frac{\nabla u}{u} + 4\nabla q \,.\, q^{-1} = \;-\;\Sigma\frac{a_1 i'}{u_1} .$$

When the systems are isothermal as well as orthogonal, this equation may be put in the singular form—

$$\nabla\left[\,(u_1 u_2 u_3)^{\frac{1}{4}} q\,\right] = 0 .$$

The results given in this section were laid before the Society in May 1876, but were mislaid, with other papers then read.

(8.) The great desideratum in the application of quaternions to problems such as those just treated, seems to lie in the discovery of the general solution of the equation

$$\nabla r = 0 ,$$

where r is a quaternion. Unfortunately this seems to depend ultimately upon Laplace's equation, treat it how we may. It is easily seen to be equivalent to the kinematical problem of finding a displacement which shall produce no compression, but shall produce a rotation whose vector axis itself corresponds to a displacement without compression.

The nature of the difficulty is also easily seen in another way; for, when we try to find the conditions of integrability of such an equation as

$$V \,.\, \lambda d\mu = d\nu ,$$

we may, of course, make the assumption

$$d\mu = \phi d\rho$$

where the coefficients of ϕ are functions of ρ. This gives at once

$$S\alpha d\mu = S.\phi' \alpha d\rho ,$$

so that

$$V.\nabla\phi'\alpha = 0$$

whatever constant vector be α.

Suppose this satisfied, we have the farther condition

$$V.\lambda\phi \, d\rho = d\nu ,$$

or

$$S.\phi' V(\alpha\lambda) \, d\rho = S\alpha d\nu ,$$

so that, whatever be α,

$$V.\nabla\phi' (V\alpha\lambda) = 0$$

Taken in conjunction with the former condition, this shows that ∇ may here be considered as operating on λ only.

In this very particular case, however, we find at once that λ must be constant, and that

$$d\mu = \phi d\rho = i du + j dv + k dw .$$

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH

VOL. IX. 1877–78. No. 101.

Ninety-Fifth Session.

Monday, 7th January 1878.

The Right Rev. Bishop COTTERILL, Vice-President, in the Chair.

The following Communications were read :—

1. On Gladstone's Theory of Colour-Sense in Homer. By Professor Blackie.

2. Note on a Geometrical Theorem. By Prof. Tait.

In "Trans. R.S.E." (1864–5) Fox Talbot proved very simply, by means of a species of co-ordinates depending on confocal conics, the following theorem, at the same time asking for a simple geometrical proof.

If two sets of three concentric circles, with the same common difference of radii, intersect one another—the chords of the arcs intercepted on the mean circle of each series by the extremes of the other are equal.

A properly geometrical proof may possibly be obtained by showing that the *middle points* of these arcs are equidistant from the line joining the centres. It is, of course, quite easy to build up a quasi-geometrical proof, but Talbot's would be much better.

The following investigation shows the nature of the theorem, and gives some elegant constructions.

Let d be the common difference, b and c the mean radii, and a the distance between the centres. Then the square of one of the chords is easily seen to be

$$p^2 = 2c^2\left(1 - \cos\,(\theta' \pm \theta)\right),$$

where θ' and θ are given by

$$(b-d)^2 = a^2 + c^2 - 2ac\cos\theta\,,$$
$$(b+d)^2 = a^2 + c^2 - 2ac\cos\theta'.$$

The expressions for the other chords differ only by the interchange of b and c. Elimination gives at once

$$p^2 = 2c^2\left\{1 - \frac{a^2+c^2-(b-d)^2}{2ac}\cdot\frac{a^2+c^2-(b+d)^2}{2ac}\right.$$

$$\left.\mp\left\{1-\left(\frac{a^2+c^2-(b-d)^2}{2ac}\right)^2\right\}^{\frac{1}{2}}\left\{1-\left(\frac{a^2+c^2-(b+d)^2}{2ac}\right)^2\right\}^{\frac{1}{2}}\right\}$$

$$= \frac{1}{4a^2}\left\{4(a^2+d^2)(b^2+c^2) - 2(b^2-c^2)^2 - 2(a^2-d^2)^2\right.$$

$$\left.\mp 2\left(4a^2c^2 - \left(a^2+c^2-(b-d)^2\right)^2\right)^{\frac{1}{2}}\left(4a^2c^2 - \left(a^2+c^2-(b+d)^2\right)^2\right)^{\frac{1}{2}}\right\}$$

$$= \frac{4}{a^2}\left(A^2 + A'^2 \mp 2AA'\right)$$

where A and A′ are the areas of the "inscribable" quadrilaterals, crossed and uncrossed, whose sides are a, b, c, d. This, of course, proves Talbot's theorem.

Hence

$$p^2 = 4\frac{(A' \mp A)^2}{a^2},$$

a remarkably simple expression. The two values of p are given at once by Talbot's diagram, and the rectangles under their quarter sum, and difference, respectively, with the distance between the centres, give the areas of the quadrilaterals above mentioned. Or, better, the triangles whose angular points are the middles of the arcs respectively, and the centres, have areas equal to half the sum and half the difference of the quadrilaterals.

The symmetry of these expressions shows that in Talbot's theorem any two of the four quantities employed may be interchanged—the lengths of the corresponding pairs of equal chords being always inversely as the quantity chosen for the distance between the centres of the two series of circles.

Again, it is easy to see that we have by the above equations

$$A' = ac \sin \frac{\theta'}{2} \cos \frac{\theta}{2},$$

$$A = ac \cos \frac{\theta'}{2} \sin \frac{\theta}{2},$$

so that, construct the figure how we will with four given lines, the ratio of the tangents of the halves of the pair of angles corresponding to θ, θ', is constant. This is the relation between True and Excentric Anomaly. And we have also the very simple expression

$$A\,A' = \frac{a^2c^2}{4} \sin \theta \sin \theta',$$

so that the product of the areas of the crossed and uncrossed quadrilaterals is equal to the product of the areas of the (construction) triangles whose sides are

$$a,\ c,\ b - d\,,$$

and

$$a,\ c,\ b + d\,,$$

respectively. Here again the letters may be interchanged at will; which, in itself, is a curious theorem.

While seeking a quaternion proof of the above theorem, I hit upon the following result. Given two opposite sides of a gauche quadrilateral in magnitude and direction. If one of these be fixed, and if the diagonals are to be of equal lengths, the locus of either end of the other is a plane.

Professor Tait, in consequence of the lateness of the hour, postponed his paper "On the Strength of the Currents required to work a Telephone." He said that the title given in the billet did not fully describe the contents. These referred not only to various measurements of the actual currents employed, whether produced from a cell or a Holtz machine, or by induction, but also to the mode in which the sounds are reproduced. He stated that he believed it would soon be possible to employ the instrument for the study of internal changes of form in all bodies, and also that in its construction magnets might be entirely dispensed with. He also stated that Mr James Blyth had with success substituted a copper plate for the iron disc in the receiving telephone, and had found that even wood, vulcanised india-rubber, tinfoil, and paper, might be employed for the same purpose—though not with very good results. In the transmitting instrument Mr Blyth had managed to employ a block of iron 8 inches thick.

Professor Tait also exhibited a double mouthpiece, by means of which it is easy for two players to produce chords from a French horn.

The following Gentlemen were elected Honorary Fellows of the Society:—

Otto Struve, St Petersburg.
Professor J. N. Madvig, Copenhagen.
Professor Balfour Stewart, Manchester.

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

W. H. Allchin, M.B. (Lond.), M.R.C.P., Wimpole Street, London.
Richard Norris, M.D., Professor of Physiology, Queen's College, Birmingham.
Daniel John Cunningham, M.D., 9 Gladstone Terrace.
Alan Macdougall, Memb. Inst. C.E., North British Railway, Millburn Tower, Corstorphine.
John Frederick Bateman, F.R.S., V.P. Institution C.E., 16 Great George Street, Westminster.
William King, M.A., Stewart Villa, Dean.
P. R. Scott Lang, B.Sc., St Ronan's, Viewforth.

Monday, 21st January 1878.

Sir WILLIAM THOMSON, President, in the Chair.

The Canon of Sines for each 2000th part of the quadrant, to 33 places, and true throughout to the thirtieth figure, by Edward Sang, was laid on the table.

The following Communications were read:—

1. On the Tabulation of all Fractions whose values are between two prescribed limits. By Edward Sang.

2. Can the Law of Multiple Proportions be demonstrated from Analytical Data? By W. Dittmar.

3. On the Solid Fatty Acids of Coco-Nut Oil. By G. Carr Robinson.

4. Suspension, Solution, and Chemical Combinations. By William Durham.

Some time ago I made some experiments on "Suspension of Clay in Water, and in Acid and Saline Solutions." These formed the subject of several communications to the Royal Physical Society of Edinburgh, and were afterwards published in the "Chemical News." I shall describe some of these experiments as an introduction to this paper.

1st. *Clay in Water.*—A few grains of fine white clay were stirred up with about a pint of pure water in a glass jar. The time which the water took to clear and the clay to deposit itself in the jar was noted, and found to be about 30 to 36 hours.

2d. *Clay, Water, and Acid.*—To a similar quantity of clay and water were added a few drops of acid (various acids were tried, with the same result in each case), and the time the liquid took to clear again noted. In this case, the time was from half an hour to one hour, according to the quantity of acid used. So sensitive was this action, that when the water was just touched with a glass rod dipped in sulphuric acid, the time of clearing was greatly shortened.

3d. *Clay, Water, and Salt.*—In place of acid, salts of various kinds (including common salt ($NaCl$)) were added to the clay and water, and the effect in every case was to shorten the time of precipitation of the clay and clearing of the water, according to the kind and quantity of salt used. As bearing on the precipitation of salt at the mouths of rivers, I may mention that the water taken from the end of Leith pier was about the best mixture for precipitating the clay quickly.

4th. *Clay, Water, and Alkali.*—The alkalis (potash, soda, and ammonia) were next tried with the clay and water, and when added in very *small quantities* instead of hastening the precipitation of the clay, like the acids and salts, they retarded it so that in some cases it was 90 hours before the liquid was clear. In larger quantities they acted like the acids and salts.

In endeavouring to ascertain the cause of these phenomena, I was led to experiment on various solutions, and obtained results which appear to me very interesting, and to open up a line of research likely to lead to important results as to the laws of solution and chemical affinity. The following are the more important facts as yet determined:—

1. *NaCl and HCl.*—To a saturated solution of common salt (NaCl) was added some strong hydrochloric acid (HCl). Salt was rapidly precipitated on each addition of acid. This action of HCl is usually described by saying that salt is insoluble in hydrochloric acid, but the mode of action does not seem to have attracted attention. The following experiments throw some light on the matter.

2. $Na_2SO_4 + IOH_2O$ *and HCl.*—To a saturated solution in water of sodium sulphate was added strong solution of hydrochloric acid. *Anhydrous* sodium sulphate was quickly precipitated. This precipitate was quickly dissolved on the addition of a further quantity of HCl. In this case it cannot be said that anhydrous sodium sulphate is insoluble in hydrochloric acid, because it is dissolved by it when added after its precipitation. The action is exactly analogous to what occurs when ammonia is added to a salt of zinc; the ammonia first combines with the acid of the zinc salt throwing down a precipitate of zinc oxide, then, on a further quantity of ammonia being added, the zinc oxide is dissolved. In like manner, in the case of solution of sodium sulphate the HCl first combines with the water and precipitates the anhydrous salt, and then, by a further addition of acid, the salt is again dissolved. This is made more clear by the next experiment.

3. *Crystals of* $Na_2SO_4 + 10H_2O$ *and HCl.*—Strong hydrochloric acid was poured over some undissolved crystals of sodium sulphate. Rapidly the crystals were broken up, the water uniting with the acid, and the anhydrous salt left, which, as in the former case, was dissolved by the addition of more acid.

4. $CaCl_2$ *and HCl.*—To a saturated solution in water of calcium chloride was added some strong hydrochloric acid. No action was apparent. As calcium chloride has a strong affinity for water, I concluded that the affinities were balanced in the two solutions, and therefore there was no action. To upset this balance, if it existed, I added to the hydrochloric acid solution some fragments of solid calcium chloride, which, as anticipated, rapidly dissolved with effervescence, expelling hydrochloric acid gas copiously. Further,

5. A weighed quantity of solid calcium chloride was dissolved in a measured quantity of water, and the rise in temperature noted. This was 15° Fahr.

6. The same quantity of calcium chloride was dissolved in strong hydrochloric acid and the rise in temperature also noted. This was 6° Fahr., being a difference of 9°, accounted for, no doubt, by the vapourising of the hydrochloric acid gas expelled in the latter case.

7. *NaCl and* $CaCl_2$.—Calcium chloride, both in solution and solid, was added to saturated solution of common salt, and in both cases the sodium salt was precipitated, the calcium chloride taking its place in the solution.

8. *HCl and* H_2SO_4.—Strong sulphuric acid was added to strong hydrochloric acid, when the latter as gas was expelled with effervescence.

9. $CaSO_4HCl$ *and* H_2SO_4.—Strong sulphuric acid was added to a saturated solution of calcium sulphate in hydrochloric acid, when calcium sulphate was precipitated, and hydrochloric acid gas expelled.

I next looked about for two solvents which would dissolve the same substance, and yet precipitate it when mixed. These results I found with sulphuric acid, water, and calcium sulphate.

10. $CaSO_4$, *Water, and* H_2SO_4.—To a saturated solution of calcium sulphate *in water* was added some strong sulphuric acid. On cooling, the calcium sulphate was precipitated.

11. $CaSO_4$, H_2SO_4 *and Water.*—To a saturated solution of calcium sulphate in *sulphuric acid* was added water. As before, on cooling, calcium sulphate was precipitated.

12. Two solutions (saturated) of calcium sulphate, one in water and one in sulphuric acid, were mixed when calcium sulphate was precipitated on cooling.

Three experiments similar to Nos. 10, 11, and 12 were performed, only substituting clay in suspension for calcium sulphate in solution, and the results were similar. Clay, in sulphuric acid (strong), was suspended nearly as long as in pure water, but on mixing the liquid, or on adding sulphuric acid to the water, or water to the sulphuric acid, the precipitation of the clay was greatly accelerated.

I hope to extend these experiments to other substances than those already operated upon; to enter more minutely into the various phenomena; and to determine the law of what I may call "Solution Equivalents." To know, for instance, the relation between the quantity of calcium chloride dissolved and the quantity of common salt precipitated or hydrochloric acid gas expelled from solution, and various other points which suggest themselves for investigation.

The experiments, however, so far as they go, seem to point to certain conclusions which are interesting and suggestive.

1st. There seems to be a *regular gradation of chemical attraction from that exhibited in the suspension of clay in water up to that exhibited in the attraction of sulphuric acid for water which we call chemical affinity.* The attraction of clay for water is not so strong as the attraction of salts which are dissolved in water. Then again, the attraction of salts is not so strong as that of hydrochloric acid, which almost forms a definite chemical compound with water. Then, finally, we reach sulphuric acid with the strongest attraction of all, and forming more than one definite chemical compound with water, and easily displacing from their combinations with water hydrochloric acid; salts it does not decompose and clay in suspension.

2d. That *chemical combination, solution, and suspension diffe only in degree, and are manifestations of the same force.* The few drops of sulphuric acid added to the water with clay in suspension attracts and holds the water with the same force as a salt in solution and precipitates the clay in the same manner, and as the water is evaporated increases its hold gradually on what remains until it is strong enough to form definite chemical compounds.

3d. *The attraction of chemical affinity is not, in all cases at any rate, exhausted when a definite compound is formed, but has sufficient power left to form solution or suspension compounds.* Thus calcium sulphate is a definite chemical compound, but it still possesses sufficient affinity for sulphuric acid to enter into solution with it. This view would explain the researches of M. Stus on atomic weight. He proved that Prout's law, that the atomic weights of the elements are simple multiples of that of hydrogen, is not correct, though very nearly so; the differences being very small fractional. If it be true that the attraction of affinity is not entirely exhausted in a definite compound, but that there is a fraction of it, so to speak, to spare, we should only find Prout's law to hold good if we could analyse only one molecule of any compound, but, as in any analysis we can make, there must be many molecules, such atom of the molecules having a fraction of its affinity for the other to spare, these fractions would unite and hold in combination an extra number of the other atoms, not so firmly, perhaps, but still firmly enough to make the whole appear a definite compound on analysis, and this would affect the calculation for atomic weight. Thus, suppose two atoms of Cl = 71 combine with one atom of Ca = 40 and still have $\frac{1}{100}$ part of affinity to spare, then 200 atoms of Cl would take up 101 atoms of Ca, and from this analysis we should make the atomic weight of Ca not 40 but 40·4.

4th. If chemical combination and solution are due to the same force, then solution will loosen the combination by spreading the affinity, and possibly there may be a re-arrangement of the soluble and solvent analogous to what is known to take place when two salts are mixed having different acids and bases. Hence the powerful effects of solution in promoting chemical reaction and electric conductivity.

5th. A point of practical importance may be noted regarding analysis. Many substances are added indefinitely to solution to render insoluble some body held in solution, which quantity is to be estimated. Now, if the way in which one substance renders another insoluble is by combination with the solvent, it is quite clear that if either too much or too little be added to the solvent, an error may be made in the analysis, as the whole of the precipitated body may not be thrown down.

6th. A further investigation of this subject may throw some light on the manner in which the solubility of a solid in a liquid is related to the chemical composition of the two.

5. Note on the Surface of a Body in terms of a Volume-Integral. By Professor Tait.

In § 25 of my paper on *Green's and other Allied Theorems* ("Trans. R. S. E." 1869–70) I gave the following relation between a volume and a surface integral, the limits being determined by any simply continuous closed space:—

$$\iiint \nabla \tau d\varsigma = \iint U\nu \, \tau ds.$$

If in this equation we assume τ (which is arbitrary) to be equal to $U\nu$ at every point of the surface, we have

$$\tau = U\nu = U\nabla P$$

where $P = C$ is the (scalar) equation of the surface. The equation then becomes

$$\iiint \nabla U(\nabla P) d\varsigma = -\iint ds.$$

Applied to the ellipsoid—

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1,$$

this gives for the whole surface the expression—

$$\iiint dx dy dz \frac{\frac{x^2}{a^4}\left(\frac{1}{b^2} + \frac{1}{c^2}\right) + \frac{y^2}{b^4}\left(\frac{1}{c^2} + \frac{1}{a^2}\right) + \frac{z^2}{c^4}\left(\frac{1}{a^2} + \frac{1}{b^2}\right)}{\left(\frac{x^2}{a^4} + \frac{y^2}{b^4} + \frac{z^2}{c^4}\right)^{\frac{3}{2}}}$$

the limits being given by the equation of the surface.

6. On a White Sunbow. By Sir Robert Christison, Bart.

As the phenomenon of a colourless rainbow, which was seen here in the forenoon of Thursday the 10th January, seems to be very rare, never having been witnessed before either by myself or by any of my friends to whom I have mentioned the subject, I beg to offer the Society the following description:—

On my way that forenoon to the Botanic Garden, and arriving about a quarter-past eleven at the open view of the north at the bottom of Pitt Street, my attention was arrested by the appearance of a magnificent white bow, visible in its entire arch from end to end in the northern sky.

The air was frosty, very dry, uncommonly still, and in most quarters moderately clear. The smoke of the Old Town, however, rising high in the stillness above the ridge of the High Street to the south, obscured greatly the sun, which shone through the upper region of the smoky veil without the slightest definition of its disc, white nevertheless, but so shorn of its brightness that I could easily look it in the face. I was unable to detect anywhere the slightest appearance of a shower or rain-cloud. The sun, my place of observation, and the summit of the arch, were in the same vertical plane. The summit of the arch reached about half-way to the zenith. The northern sky on which it was formed was somewhat hazy and grey in its lower region, but blue-grey, and tolerably clear in the region of the upper two-thirds of the arch.

In form the arch was identical with that of an ordinary rainbow, except that I thought it considerably broader; and its edges were in many places somewhat broken, so that it had exactly the appearance as if the sun had gathered in an arch a number of little woolly cloudlets. On minute search I could not detect any trace of colour from end to end. I asked the opinion on this point of two gentlemen whom I met at the lowest part of the road, at the wall between the road and the river, and one of them thought he could detect a very faint trace of colour over a small space at the extremity of the western limb. As the absence of colour, however, was the main phenomenon, I scanned the whole curve again and again with great attention, but could see no coloration anywhere.

At this lowest point of the road the edges of the bow were seen much more defined and sharp than when I first noticed it. As I advanced up the gentle slope from Warriston bridge towards the Botanic Garden, the summit of the arch began to break up, and to present the appearance of irregular flimsy cloudlets ascending in the sky above it. But before reaching the Garden gate the whole arch again formed an unbroken bow, and with both edges sharply defined like those of a common rainbow. At the same time a similar secondary arch had begun to form below the principal one, only half its width, and much closer to its neighbour than I remember to have seen in a double rainbow of the ordinary kind.

On returning homeward, about fifteen minutes later, I still observed, on issuing from the Garden, a sharply-defined colourless principal bow, and now a complete secondary one under every part of the bow visible from the roadway. As I proceeded southward every now and then the upper region of the bow seemed to be breaking up, and this appearance was very marked when I reached

Heriot Row, at the head of Dundas Street, the highest station in my walk. As I went westward along Heriot Row the breaking-up appeared greater and greater at every interruption of the street, which gave me a view of the northern sky; and when I reached the landing-place of my house, in Moray Place, from which, however, only the western half of the region of the bow could be seen, the whole appearances had vanished, and the sky was everywhere mottled with thin grey fleecy clouds, small and of irregular ill-defined outline. I did not again look for it, but I understand it was partly seen by others so late as two P.M.

Various particulars, which it is unnecessary to mention, led me to suppose at the time that this colourless bow had some connection with the smoky column of air through which the sun's rays penetrated. But this supposition was put an end to by learning from my son that, when at Craigiehall, five miles west from town, in a smokeless atmosphere, he observed the bow distinctly about one o'clock. Its edges were never sharply defined so long as he noticed it. But it had no colour. Another gentleman present thought there was a limited blueness at one place. But my son satisfied himself that this was owing to a patch of blue sky behind, and he is sure that there was no colour at any part of the bow visible to him.

A better explanation has been suggested to me by Professor Tait, to whose theory I subscribe. But I leave it to himself to explain his views.

By Professor Tait.

I was unfortunate in not seeing the phenomenon till nearly 2 P.M., when I was on my way from College to the Observatory. It was then very faint, but I saw at once that it differed in a marked manner from an ordinary rainbow. From what I could see, I attributed its apparent whiteness to the greatly increased *effective* surface from which tho light came. This was probably due to reflection from ice-crystals mixed with the drops of water in the thin strata of cloud which covered the whole sky. The sun's light was much dimmed, and the edge of its disc was very indistinct, as the clouds immediately round it, to the distance of at least a diameter, appeared nearly as bright as the disc itself. Hence this rainbow was probably very much less pure, while also much less bright, than the usual one.

This suggestion seems fully to explain all the appearances which I saw. It will be observed that both Sir R. Christison and Mr Buchan noticed the peculiar brightness of the clouds near the sun, so that it is probable that this explanation applies to the phenomenon as seen by them also. The peculiarities noticed in the position of the spurious bows (when seen) are of course dependent on the actual size, as well as the greater or less uniformity of size, of the drops which produced the rainbow; and these may well have been exceedingly variable as regards both time, locality, and height in the atmosphere.

[*Added, April* 8.]

I find that this nearly colourless rainbow is very easily reproduced in my class-room, when the sunlight employed to form a rainbow in fine spray is made to pass first through a large vessel with parallel glass sides, containing water and a little milk. This arrangement imitates very closely the circumstances of the 10th January.

By J. Christison, Esq.

I saw it first at Craigie Hall, five miles west of Edinburgh, about 11.30 or thereby. At first it did not make much impression, as the house front runs east and west, and I was standing at the front door, and consequently only saw half of the bow, and took it to be an accidental arrangement of the light clouds that covered most of the sky. By and by the idea of its being a rainbow struck me, and a move out from the house showed the full bow. I noticed it off and on for about an hour. Sometimes it was indistinct, but generally it was evident enough to attract attention at once. I did not, however, at any time see anything like clear definition of outline. There was always a sort of wavy indistinctness.

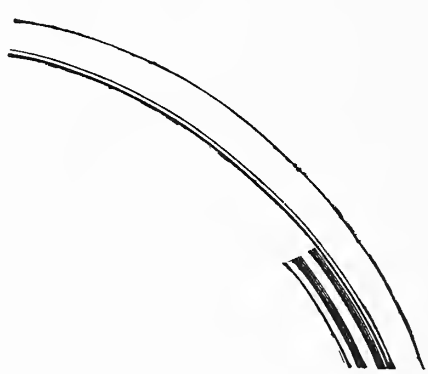
As to colour, I tried hard to convince myself I made it out, but without effect, and am quite satisfied there was none at any of the periods I looked at the bow. One of the party at Craigie Hall thought he made out colour, but on his pointing out the part where he thought it was visible I was satisfied that it was only a break in the bow, with a bit of very light blue sky showing through. I did not notice it after 12.30 or 12.45, as I was indoors.

It was pretty hard frost all the time. The only effect the sun had was to disperse a little of the hoar-frost, which was thick and heavy out there, and to make a few of the flat stones among the gravel show damp. I noticed a thermometer in the garden standing at 32. It was well placed for shelter from the sun; but it was within a few feet of two brick walls, one of them the end wall of a forcing-house. Could that affect the thermometer? The ground—grass, gravel, and earth—remained hard all day. I noticed neither rain nor snow.

The only local peculiarity on the occasion that I can remember was that the gardener was burning rubbish, off and on, all the forenoon between us and the bow. There was no smoke that I noticed after eleven, but there must have been a good deal of heat rising from the red ashes. This was close to the house, say 100 yards at most. The bow had all the appearance of being distant; but had it not been seen here (and no doubt elsewhere) there might have been room for suspicion of this appearance being a deception perhaps.

By Mr Buchan.

The rainbow described by Sir Robert Christison at meeting of the Royal Society was observed by us from the north windows of the



Scottish Meteorological Society. The above is a rough sketch of it made at the time.

The eastern limb of it rested on the tops of the houses of Leith Street, against what appeared to be a smoke-like cloud, precisely similar to what often accompanies the aurora. It was on this portion only of the phenomenon that the following points were noticed :—

1. *Spurious Bows.*—Two such bows, very distinctly marked, were seen within the primary bow. The first spurious bow was separated from the primary by an intensely black band the width of the spurious bow itself, while this spurious bow and its black band were together equal in breadth to the primary bow. The second spurious

bow was separated from the first spurious bow by an intensely black band the width of the second spurious bow, and both second spurious bow with its black band were together the width of the first spurious bow.

2. *Colour.*—Colours were seen on the portion of the limb described above as standing out against the black smoke-like cloud. The colours, though far from being well pronounced, were distinct. Miss Buchan stated she saw the colours as we looked at the bow through the window, which at the time stood in need of the window-cleaner; but I was unable to detect the colours till the window was thrown up, when they were distinctly visible.

In no other part of the bow was any development of colour visible, either when I first observed it in St Andrew Square, or afterwards at various times in the office.

The frost was keen at the time, but it is probable that a S.W. wind with thaw had then set in aloft. I understand from Mr J. Gibson-Thomson, of York Place, who called at this office shortly thereafter, that while driving out to the country in the afternoon the wind had shifted into S.W., and the tops of the mud-ridges on the road had become soft with the thaw which had set in.

The width of the primary, or its visible portion, appeared to be about a third narrower than the ordinary coloured rainbow.

The arrangement of the colours was that of the ordinary rainbow, the yellows and yellow-reds being best marked.

The appearance of the sun, as seen in St Andrew Square, was hazy, with light wisps of clouds and large patches or blurs of mist in that part of the sky, giving rise to ill-defined shadows, so that I looked about to see if there were any appearance of halos or mock-suns visible, but none were seen.

By Dr Ferguson.

Dr Ferguson stated that he had read in the *Inverness Advertiser* that a similar phenomenon had been seen at Nairn and neighbourhood on the same day from eight in the morning till midday. It is described as a pale blue arch on a white ground, having the outline and position of a rainbow, but differing from it in its remarkable fixity.

I saw the rainbow at Edinburgh from the middle of the east division of Queen Street. I think it was at eleven, but the friend in whose company I was at the time says it was twelve. I looked at the bow attentively, but not critically, as I was not aware of the exceptional character of the phenomenon. The day was fine, but there was a haze in the sky, which gave an indistinct outline to the masses of cloud which occupied the northern heaven. It looked to me like a cloud rainbow, as its continuity seemed to correspond to that of the cloudy mass on which it was seen. I remember particularly one spot to the west of the middle, where there was a partial break in the clouds and a similar defect in the bow. As regards colour, it wore the appearance of a bleached rainbow, with an indefinite stratification of tints. I did not specially mark each gradation of colour from the red to the blue, but I had a distinct impression of such. The colours were faint, and the perception of the coloured rainbow effect was as much, or more, due to contrast, than to the colours taken individually.

7. Extract of Letter to the President from H. E. Rosevelt, Esq., dated New York, Dec. 23, 1877.

"I saw the 'Phonograph' the other day, and though it is very crude I was much interested. I would briefly describe the idea as follows:—

"To the centre of an iron diaphragm is attached a metallic point resting against a strip of paper or tinfoil. You speak against diaphragm through a mouthpiece, at same time the paper being drawn under the points. The vibration of diaphragm and point indent the paper to various depths, making an undulating (as it were) mark, &c. Now, if said paper is afterwards drawn under the point, the diaphragm vibrates exactly as your voice made it vibrate, and all the sounds are reproduced exactly as you said them, making a most astonishing effect—singing, laughing, and articulate words were all reproduced. Of course it is by no means perfect, but it is very interesting. By attaching a point to one of our strong telephones we could record any messages sent. Though the phonograph is purely mechanical in its ordinary use, I mention all this to you, as you will probably soon hear of it, and naturally would not believe it a possible thing. The above is the invention of Mr Eddison, who showed it to me himself."

Monday, 4th February 1878.

Sir C. WYVILLE THOMSON, Vice-President, in the Chair.

In delivering the Neill Medal to Dr Traquair, the Chairman made the following remarks:—

Dr Ramsay Traquair,—The Neill prize for the triennial period 1874–77 has been awarded to you by the Council of this Society, nominally for your paper on the structures and affinities of *Tristichopterus alatus* (Egerton), communicated to this Society within the required period, and published in the 27th volume of the Transactions of the Society. The Council have, however, also taken into account, as they are entitled to do by the conditions of the prize, the many contributions which you have made during some years past to the knowledge of the structure of recent and fossil fishes.

I may mention as among the more important of these a memoir "On the Asymmetry of the Pleuronectidæ, as elucidated by an examination of the skeleton in the Turbot, Halibut, and Plaice," published in the Transactions of the Linnean Society of London for the year 1865; a paper "On the Cranial Osteology of Polypterus," in the *Journal of Anatomy and Physiology* for the year 1870; and a work on "The Ganoid Fishes of the British Carboniferous Formations," now in course of publication by the Palæontographical Society.

You have been fortunately placed during the last few years in a locality remarkably rich in the group of fossils to which you have paid special attention, and already three parts of a valuable series of papers "On new and little-known Fossil Fishes from the Edinburgh District" have appeared in the Proceedings of this Society.

I have much pleasure in presenting to you the Neill medal in the name of the Council of the Society.

The following Communications were read:—

1. Chapters on the Mineralogy of Scotland. Chapter III. The Garnets. By Professor Heddle.

In this third chapter Dr Heddle submits the results of his analyses of garnets from thirteen localities.

Notwithstanding the great frequency of the occurrence of the mineral, it is generally so largely contaminated with quartz that he was unable to procure, from many localities, specimens fit for examination.

Three varieties of garnet new to Scotland—one of these being altogether new—have, however, been the reward of the present investigation.

On a hill lying north of Balmoral,—Creag Mohr,—two of the new varieties were found, both in limestone.

The first, the rarer, was the *water garnet*, or colourless garnet; he second was the *grossular*, or gooseberry garnet, hitherto found only in perfection on the Wilni river in Siberia.

The *cinnamonstone* of Glen Gairn has next been analysed. This is finer in colour than the Ceylon mineral, but so flawed as to be useless for purposes of jewellery.

Not so the *pyrope* of Elie—the "Elie rubies," as they used to be termed. Dr Heddle regards these as, weight for weight, the most valuable of Scottish gems.

Analyses of *common garnet* from Yell in Shetland, Killiecrankie and Meall Luaidh in Perthshire, Knockhill in Banff, and Clach-an-Eoin in Sutherland follow.

There are, lastly, analyses of a *new* garnet from five localities—four of these in Ross-shire; the other, Ben Resipol, in Argyll.

This is a *precious garnet*, containing about fifteen per cent. of oxide of manganese. Its formula places it intermediate between the ordinary precious garnet of Bohemia and the manganesian garnet of America.

This garnet, in all the localities where it is found, is of a fine currant-red colour, due probably to the manganese. From Resipol and the Raven's Rock, near Strathpeffer, pieces large enough for cutting might probably be obtained.

The lime garnets occur in certain of the limestone beds, at the upper waters of the Don and Dee, and not in others; and Dr

Heddle thinks it probable that this fact will aid in the tracing out of the individual beds—far from an easy matter in that troubled district.

The conclusion of the chapter is devoted to a speculation upon the metamorphism of these limestones.

2. On the Strength of the Currents required to Work a Telephone. By Professor Tait.

(*Deferred from January 7th.*)

Perhaps the most singular fact connected with the telephone is the excessive feebleness of the currents which suffice to work it. I have had no opportunity of testing any but rough arrangements set up by present or former students of my own, so that I cannot judge how far my results may apply to the instrument as sold.

1. A striking illustration of the feebleness of the currents required is furnished by using a Holtz machine driven *very* slowly, without condenser, and with its terminals so close that the discharge is barely audible, and certainly invisible except in the dark. When insulated wires were led from these terminals to the telephone (placed in a distant room) the effect was very curious. The instrument gave a hissing sound, quite comparable in intensity with that which was produced directly when the terminals of the machine were widely separated, one connected with the ground and the other with a large conductor discharging by brushes into the air, the machine being turned rapidly. The telephone continued to give audible sounds with slow turning, even when the terminals of the machine (somewhat tarnished) were *pressed into contact.*

2. To measure roughly the intensity of the current, I placed one prong of an unmagnetised tuning-fork about half an inch in front of the sending telephone, and measured by a microscope and scale the extent of its vibrations when the note just ceased to be audible to a listener at the receiving telephone. Next I substituted for the receiving telephone an exceedingly delicate astatic galvanometer, with very small moment of inertia, and measured the swing produced by one definitely assigned motion of the prong of the tuning-fork. By means of a known thermo-electric couple, I determined the strength of the current corresponding to the observed

swing. The result is, of course, only a very rough approximation. It is that a single Grove's cell would produce, in a circuit of somewhere about a billion B.A. units resistance, a current sufficient, if reversed 500 times per second, to produce an audible sound in the telephone I employed.

3. Several attempts at explanation of the action of the telephone have been given here and elsewhere, and others are promised for to-night. For my own part, I think there are at least *three* separate causes at work in the telephones I have used.

There can be no doubt that the inventor's own explanation is, at least to a certain extent, correct. For we can easily dispense with the magnet in the receiving telephone, using merely a thin iron disc in front of a coil. And Mr Blyth has, I believe, found that we may make the disc, even in this case, of copper, and yet have transmission (though very feeble) of intelligible sounds.

But this cannot be the full explanation. For it does not attempt to account for the peculiar nasality of the transmitted speech. Without going more closely into the matter, the difference of quality between an open and a closed pipe suggests a certain amount of constraint as the cause. And we know that the sounds in the original telephone of Reiss were produced by molecular motions due to magnetism in soft iron. Mr Blyth has shown conclusively that molecular motion in the magnet itself has a large share in the results, because he has successfully substituted other metals than iron, and even non-conductors, for the disc, and in certain cases finds that he can dispense with the disc altogether.

Besides this, however, it seems to me that there is a third cause, which in certain cases is more effective than either of the others. This is suggested by the fact that (at least with the instruments I have tried) high notes, even of comparatively small intensity, are much more clearly transmitted than low notes,—indicating that the *rapidity* of the molecular change has a great deal to do with the result. In fact, in this respect, the telephone is really a variety of the so-called *curb-key*, giving very sudden reversals.

These considerations have led me to fancy that *rapid* change of form in matter, whether paramagnetic or not, may probably be capable of detection by the telephone, for the associated electric currents may be in certain cases powerful enough to produce audible sounds. I am at present engaged in a series of preliminary experiments on this subject.

3. Experiments with the Telephone. By James Blyth. Communicated by Professor Tait.

In the telephones used in these experiments the permanent magnets were of the ordinary horse-shoe form, about 4 inches long. No cores of soft iron were attached to the poles, the insulated wire, No. 26, being wound directly round both, in such a way that a current circulating through it followed the direction of Ampère's currents. The vibrating disc was the bottom of a shallow can of thin tinned iron, $2\frac{1}{2}$ inches in diameter, supported directly above the poles of the magnet, and almost touching them. The receiving instrument was so arranged that any kind of disc could be easily substituted for the ordinary vibrating plate, and tested by sound from the same transmitting instrument, so as to allow of a comparison being made between discs of various materials. Having first ascertained that no sound was audible when no vibrating plate was used, I tried discs of the following substances, and have arranged them approximately in the order of distinctness with which the sound was heard :—

Ferrotype plate.
Thin steel.
Thin iron.
Wire gauze (fine).
Do. (a little coarser).
Cast-iron plate, $\frac{3}{4}$ inch thick.
Sheet copper.
Sheet brass.
Sheet zinc.
Tinfoil.
Vulcanite.
Thin fir wood.
Paper.
Vulcanised India-rubber.
Cast-iron, 6 inches thick.
Slice of raw potato.
Slice of fresh butter.

With the view of testing what effect would be produced by varying the position of the wire coil on the leg of the magnet, I constructed a telephone, so that the coil could be easily slipped up and down the leg, while the sound was being sent from the transmitting instrument. Very little difference in the sound was observable till the wire coil was brought near the neutral point of the magnet. It then became very faint. I next slipped the wire coil entirely off the magnet, and simply held it touching the pole side-ways. The sounds were still quite audible, and continued to be so when the coil was removed an inch or more away from the magnet. This led me to try the effect of dispensing altogether with the steel magnet, and merely holding the helix of insulated wire opposite the centre of the vibrating plate. With an iron disc a very faint sound was heard, but it became more distinct when a disc of copper was used. From this it would seem, that the vibrations producing the sound are caused by the attraction between the currents in the helix and the induced currents in the copper disc.

Still using the same transmitting instrument, I next employed a receiver, in which a soft iron core, carrying the insulated wire, was rigidly attached to the vibrating plate. This was accomplished by rivetting the head end of a screw-nail into its centre, and winding the wire round it. The pole of a steel magnet was then fixed, just clear of actual contact, opposite the head of the nail, and, with this arrangement, sounds were distinctly audible, though not so loud as when the wire coil surrounded the magnet.

With the view of rendering the telephone an instrument for detecting the existence of very feeble currents, I constructed a pair with magnets similar to those already described, but with ferrotype discs. These were joined together by a strong semicircular spring in such a way that they could be put on, after the manner of spectacles, and stick close to the sides of the head, inclosing both ears. India-rubber rings were provided for the double purpose of excluding all extraneous sound, and avoiding any disagreeable pressure on the ear.

With this arrangement, I proceeded to test for the existence of thermo-electric currents. A copper and iron junction was inserted in the circuit of the telephone, and attached to a spring in such a way that it could be made to vibrate rapidly out and into a gas flame. A distinct grating noise was heard in the telephone. A very peculiar rasping sound was produced when the ends of a copper and iron wire were rubbed together while hot, and also when one of the wires was attached to a file, and the hot end of the other drawn rapidly along it.

4. On the Theory of the Telephone. By Prof. George Forbes.

The Telephone, invented by Mr Graham Bell, is an instrument by means of which any sounds, musical notes or spoken sentences, sounded into an instrument at the sending end of a telegraphic wire may be reproduced at the receiving end. The theory of the Telephone is two-fold. First, the mechanical theory of the nature of sounds and of speech; and secondly, the theory of the action of the instrument. The first part is well known. All sounds consist of a succession of waves propagated through the air, the rate and intensity of their succession determining the nature of the sound, in pitch, loudness, and tone. If the same succession of waves as a speaker makes in using his voice can be reproduced in the air in contact with the ear of any other person, by any means whatever, the latter person will hear a fac-simile of the sound uttered by the former. A theory of the action of the Telephone is distinctly given by the inventor in the specification of his patent. The receiving and sending instruments are identical. Round the end of a bar magnet a coil of fine wire is wound, connected with a telegraph wire on the one hand and the earth on the other. In front of these a thin iron plate is caused to vibrate by the sounds uttered in its neighbourhood. Consequently, it approaches and recedes from the pole of the magnet. This alters the magnetism in the neighbourhood. In Faraday's language, 'the lines of force are altered in position, so as to go across the coil of wire.' In consequence of this, by the well-known laws of electromagnetic action, a current of electricity is sent in one direction with each approach of the vibrating plate, and in the opposite direction with each recession. This succession of currents reaches the receiving end with a strength proportioned to the extent of vibration of the iron plate. Here they circulate through the coil, and so intensify or diminish the magnetism of the bar. Thus, the vibrating plate at the receiving end is more or less attracted, according to the direction of the current. Hence, the plate at the receiving end vibrates to and fro in vibrations which are synchronus and proportionate to those at the sending end; and so the sounds are reproduced.

This theory gives a *vera causa* for the action of the instrument in its normal form, and probably explains *in part* the true action which takes place, but the author believes that this is not the whole of the action. The fact that the inventor was working upon this theory when he designed the instrument, does not prove the theory to be correct, as is shown by his having at first employed a powerful battery with the instrument, believing it to be necessary, and it was only after diminishing the number of cells without impairing the action of the Telephone that it occurred to him to try it without a battery at all, when he found the action to be equally good.

The author's mistrust of the theory commenced when he learnt that a thick iron plate might be used, whose vibrations must be extremely minute. And when he learnt that the instrument might be used with plates of glass, wood, tinfoil, or vulcanite, or even without any plate at all, he was convinced that the theory was imperfect, and resolved to propose another theory which seemed more consistent with facts.

This theory is divided into two parts—the action at the sending end, and that at the receiving end. It is very simple, and is founded upon two well-known experiments.

The theory of the action at the sending end is founded upon an experiment by Sir William Thomson, who finds that iron subject to magnetic induction has its magnetism increased when slightly stretched in the direction of its magnetization, and we may assume that it is diminished when compressed. Now, what happens in the Telephone? Take the simplest case which has been tried, *i.e.*, where there is no vibrating plate. Sound waves (which are capable of passing through solid bodies as they do through the air) strike the end of the bar magnet, and are propagated through it. Waves of alternate compression and extension pass through the length of the magnet. Consequently, the magnet is alternately increased and diminished in intensity. Hence, currents are generated in the coil of wire, and transmitted through the telegraph wire alternately, in opposite directions, as in the theory hitherto adopted. The presence of a vibrating plate of any kind may help this action, by increasing the intensity of the waves transmitted through the bar magnet; and if the plate be of iron it will undoubtedly help to propagate these currents.

According to the author's theory, the action at the receiving end is explained by an experiment ascribed to Page. If a bar of iron be surrounded by a coil of wire connected with a battery, a chink is heard when the connection is broken, and a feebler one when the connection is re-made. If the connection be made, and broken rapidly by attaching one end of the wire to a file and drawing the other along the file, a series of chinks are heard succeeding each other at the same intervals as the contacts are made and broken. It is due to the lengthening of the bar on magnetisation and the rearrangement of the molecules of the iron. It takes place also when the iron remains partially magnetised. In this case successive contacts increase the magnetism of the bar. In the Telephone successive currents pass in opposite directions through the coil. Hence the bar is lengthened and shortened alternately, and chinks are made at intervals corresponding to the intervals between successive waves at the sending end. Hence the sound waves at the sending end are reproduced at the receiving end alike in rapidity and in intensity. When a vibrating plate of any kind is used it increases the intensity of the sound, and if it be of iron the attraction will increase the effect.

Since a chink is given off both when the magnetism is increased and diminished it might be expected that the note should be heard an octave higher. But since the one is more intense than the other, the combined effect should be to give two notes, a loud one in the tone of the speaker, and a feeble one an octave higher. This might account for the common remark that a piano sounds *wiry* through a Telephone, and that a clear lady's voice is *squeaky*. But it is not certain that this effect is produced, for the chink is produced at least in part by the lengthening and shortening of the magnet. Either of these alone will give a chink, but when following each other in rapid succession they compress and rarefy the air in contact, in periods exactly agreeing with the compressions and rarefactions originated at the sending end.

In confirmation of the supposition that the sound is produced by successive *chinks* of the material of the magnet, it was stated that in a very large and massive pair of Telephones made by Mr Wm. Bottomley, jun., when any one is speaking through one of them the sound is distinctly (though not articulately) heard by every one in the room where the other is placed, and there is no doubt that the sound seems to come *from the magnet itself.* The same is done in the case of a compound Telephone with 25 bar magnets to one iron plate exhibited by the author.

In conclusion, the author considers that the theory now explained must be taken to supplement that of Mr Graham Bell (1), because the phenomena alluded to undoubtedly take place, and (2), because it is the only theory which explains the action when the vibrating plate is not made of iron, or when there is no vibrating plate at all.

5. Some Experiments with the Telephone. By John G. M'Kendrick, M.D.

During the past two months my attention has been directed to the telephone, chiefly as an instrument which illustrates in a remarkable way the delicacy of hearing. As some of these experiments are novel, and may possibly assist those who are investigating the physical phenomena of the instrument, I beg to give a short account of them to the Society. To save circumlocution in description, the term "proximal" will be applied to the telephone receiving the stimulus, and the term "distal" to the one at the other end of the arrangement. Thus, if A talks to B, A will use the proximal, and B the distal telephone.

1. *Transmission of the Sounds of Tuning-Forks.*—At an early stage of my investigations, finding it convenient to have a constant source of sound at the proximal telephone, I placed a telephone opposite one of the limbs of a tuning-fork, kept in vibration by an electro-magnet, the current being interrupted automatically by the fork itself. At the distal telephone the sound could be distinctly heard. Then I removed the disk from the proximal telephone, and allowed the limb of the fork to play in front of the naked end of the core. The distal telephone then sounded more distinctly than before. The next step was to remove the metal disk from the distal telephone, and apply disks made of vulcanite, porcelain, glass, paper, wood, a large piece of iron $1\frac{1}{4}$ inches thick, a thick rod of iron, 8 inches in length by 1 inch in diameter. Still the sound of the proximal telephone could be heard, though faintly. There could be no doubt of the fact. To ascertain whether the effect might not be due to the communication of mechanical vibrations

from the tuning-fork beside the proximal telephone, I found, that moving the latter away 3 or 4 inches from the vibrating fork, was followed by silence of the distal telephone; indeed, to cause the latter to sound, the proximal telephone has to be placed as near the fork as possible.

The following experiment is of interest, as showing the effect of a thin metallic disk in connection with the distal telephone:—Remove the disk, and apply in its place a plate of glass; listen, and the sound of the proximal telephone will be heard faintly. Then put one of the metal disks on the glass, and on listening again, the sound will be found much intensified. It may be still further intensified by placing a second disk on the surface of the first, taking care to have a few morsels of paper interposed so as to prevent them from touching.

The next step was to ascertain whether or not sounds could be heard at the distal telephone without the use of a disk there. There can be no doubt that sounds may be heard in these circumstances. They are very faint, however, and have not the same quality as those heard with a disk.

The arrangements at the distal telephone were further modified in the following experiment:—The disk was removed from the distal telephone; and the base of a glass cylinder, 8 inches in length by $2\frac{1}{2}$ in diameter, filled with water, and closed at each end by a ground glass cover, was placed against the core. Very feeble sounds from the proximal telephone were heard, when the ear was placed at the other end of the glass cylinder. These sounds were slightly intensified by slipping in a thin metal plate between the base of the cylinder and the core; but when a second disk was placed between the ear and the other end of the cylinder, the sounds were nearly as loud as with the ordinary telephone. In this experiment, the two disks were separated by a distance of 8 inches, and it is difficult to explain the phenomena as depending merely on conduction of sound.

At this stage it occurred to me to place opposite to the core of the distal telephone a tuning-fork of the same pitch as that opposite the proximal telephone. The result was, that the one tuning-fork set the other in movement, so that the sound of the distal tuning-fork could be heard throughout the room. As is now known, the same observation was made by Dr Rontgen, and recorded by him in "Nature. It was interesting as being a proof of the actual vibration of the metallic substance at the distal tuning fork.

2. *Transmission of the Sounds of Organ Pipes.*—If an organ pipe sounding loudly be brought into close proximity with a proximal telephone, its sound is heard distinctly at the distal telephone. If then a pipe of the same pitch be placed opposite the distal telephone, and it be sounded feebly, the sound is reinforced when the pipe at the proximal telephone is also sounding; but I have never succeeded in causing one pipe actually to initiate sound in the other. Mr Aiken of Falkirk, however, has informed me by letter that he has observed this phenomenon. I found also that there was no intensifying effect when either of the disks was removed.

3. *Transmission of the Sounds of Vibrating Strings.*—I succeeded in causing a string to vibrate by means of the telephone as follows:—Two catgut strings, each having a telephonic disk cemented to the centre of the string, were tuned as accurately as possible to a pitch of about C; one disk was placed opposite a large and powerful telephone (the proximal), made by Mr William Bottomely, and the other (the distal), opposite a similar instrument; on plucking the string at the proximal telephone, the string opposite the distal also sounded, though feebly. This experiment holds out the hope, that by suitable arrangements the sounds of strings might be transmitted for musical purposes.

4. *Optical Observations of the Movements of the Disk.*—There can be no doubt (1) that sounds may be heard even when the disk is firmly pressed against the ear, so as to prevent its vibrations to a great degree; and (2) that sounds may be heard even without any disk on the distal telephone. A disk, or moving metallic body capable of magnetic action, is always necessary at the proximal telephone. On the other hand, the existence of a disk, free to vibrate, at the distal telephone always intensifies the sound, and, in particular, it appears to be almost essential for distinct articulation of language. One would therefore infer that the disk must move as a whole. These movements I have studied in various ways, and they will still be under observation.

1*st method.*—A strongly vibrating tuning-fork was kept going opposite the proximal telephone; a fine glass rod or filament, 3 inches in length, was cemented to the centre of the disk of the distal telephone; to the other end of this rod two common microscopical covering glasses, having between them a drop of blood, were also cemented so as to be in a horizontal position. The drop of blood thus fixed to the telephone disk was then brought under a Hartnack's $\frac{1}{4}$th inch, magnifying about 300 diameters, and the coloured blood corpuscles were brought into accurate focus. A key was placed near the distal telephone, by which the current might be transmitted or interrupted at pleasure. On opening the key, the circular-coloured corpuscles at once assumed an oval form, and were put somewhat out of focus, plainly the result of lateral movement. Thus, by an application of Lissajoux's method of observing vibrations optically, the movements of the disk could be seen. I found that the amplitude of the movements of the vibrating fork (moving 60 vibrations per second), near the proximal telephone, was about $\frac{1}{6}$th of an inch; the movements of the corpuscles were about half their diameter, or about $\frac{1}{6500}$th of an inch. On stopping the current by means of the key, the movements almost immediately ceased. Again, on taking away the glass rod from the disk, and applying the ear to the disk, the low booming sound of the fork could be heard. It is, therefore, evident that these sounds were produced by movements of the disk, the amplitudes of which were about the $\frac{1}{6000}$th of an inch, an interesting example of the sensitiveness of the ear. The minimum limit of excitation of the ear has been thus stated:—The faintest sound perceptible is that caused by a ball of pith, 1 milligramme in weight, falling 1 millimetre in height upon a glass plate, may be heard at a distance of 91 millimetres from the ear (*Schafhäutl*).

The telephone appears to be an instrument which illustrates the extreme sensitiveness of the ear, even better than the method of Schafhäutl just alluded to. The length of the hair cells in the cochlea (which are stated to be tolerably stiff and rigid) is about the $\frac{1}{8000}$th of an inch. One of them could therefore perform an excursion of the $\frac{1}{4000}$th of an inch. If, as Helmholtz states, the mechanical arrangements of the bones of the ear are such as to diminish to one-third the excursion of the membrana tympani, and if no further reduction took place in the internal ear itself, the oscillations of the plate I observed, and which were capable of causing a sensation of sound, would only amount, when they reached the hair cells, to about the $\frac{1}{18000}$ of an inch, and thus cause a movement of the auditory hair through about $\frac{1}{4}$th of its possible amplitude of excursion.

2*d Method.*—The movements may also be observed by throwing obliquely a beam of light on a reflecting surface on the disk, and catching the reflected ray on a screen. I did not find this method so satisfactory as the one just described.

3*d Method.*—I cemented the disk of the distal telephone to the thin membrane of one of Koenig's manometric capsules, then there was no difficulty in observing the oscillations of the flame in a rapidly revolving mirror, when a strongly vibrating tuning-fork was placed opposite the proximal telephone. I failed, however, in seeing any movements produced by speech. The method, however, is capable of more refined application than the means at present at my disposal will allow.

4*th Method.*—I have attempted to record the movements of the disk graphically by attaching to it a lever bent at right angles at one end, bringing the point on a rapidly moving surface. No oscillations could be detected. Such oscillations were, however, recorded when, instead of a disk, I used forks at the proximal and distal ends, as already described. The movements of the distal fork were then recorded, but it was observed that the sound of the fork was audible long after all oscillations could be recorded, showing that the movements of the fork were too delicate to be recorded by this method.

5. *A Mode of Intensifying the Sound of the Telephone.*—In studying the transmission of sounds of various kinds, I had occasion to use a rapidly revolving wheel opposite the proximal telephone. The wheel in my possession, which moves with greatest velocity is one about 4 inches in diameter, having placed transversely on its circumference twenty-four rectangular bars of iron. The wheel is driven by two electro-magnets, and the current employed was obtained from twenty of Sir William Thomson's tray cells. With this power it performs about eighty revolutions per second. When the proximal telephone was brought near the circumference of the wheel, the distal telephone sounded so loudly that a rattling sound like that

caused by the wheel could be heard quite distinctly all over the room. Evidently the effect of bringing the horizontal iron bars on the circumference of the wheel rapidly in front of the core of the proximal telephone was to intensify the currents of the telephones. The currents were so strong that a disk held in front of the core of the distal telephone could be felt vibrating by the hand.

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

James Alfred Ewing, 22 India Street.
Rev. John Wilson, M.A., Bannockburn Academy.
Robert Macfie Thorburn, Uddevalla, Sweden.
Andrew Peebles Aitken, Sc.D., 16 Gillespie Crescent.
John Milne, Mechanician, Trinity Grove, Edinburgh.

Monday, 18th February 1878.

Sir WILLIAM THOMSON, President, in the Chair.

The following communications were read:—

1. The application of the Graphic Method to the determination of the efficiency of a direct-acting Steam-Engine. By Professor Fleeming Jenkin.

2. On the Disruptive Discharge of Electricity. By Alexander Macfarlane, M.A., B.Sc. Communicated by Professor Tait.

(*Abstract.*)

Last summer session, with the assistance of Messrs Salvesen, Connor, and Stewart, I applied the method of measuring great differences of potential, described in a paper by Mr Paton and myself (Proc. Feb. 19, 1877), to investigate the laws of passage of the electric spark. The method essentially consists in connecting the prime conductor of the Holtz machine, not with the electrometer directly, but with an insulated spherical ball. This ball acts inductively upon another insulated spherical ball which is in connection with the electrometer.

Before proceeding with the investigation proper, I tested the accuracy of the method by applying it to determine how the induced potential of the ball in connection with the electrometer depends on the distance between the centres of the balls. I found that the equation

$$V = 6081\ r^{-1} - 42{\cdot}26,$$

where V denotes the induced potential, and r the distance, between the centres of the balls, satisfies all the observed values of V for values of r greater than twenty-four centimetres, but for smaller values of r the function requires to be corrected by being multiplied by

$$f(r) = {\cdot}524 + {\cdot}02\ r.$$

Our method, when applied to measure the difference of potential required to pass a spark through air at the atmospheric pressure between parallel metal plates at different distances, gave a result agreeing well with that which Sir William Thomson discovered to be true for small distances. The function for V, the difference of potential in terms of s, the length of the spark is

$$V = 66{\cdot}94\ \sqrt{\{s^2 + {\cdot}205\ s\}}$$

the equation of an hyperbole, whose semi-transverse axis is ·1025 centimetres, and semi-conjugate axis 6·8623 centimetre-gramme-second units. We observed, for lengths of spark, up to 1·2 centimetre.

From the above equation we infer that—

$$R = 66{\cdot}94\sqrt{\left\{1 + {\cdot}205\ \frac{1}{s}\right\}}$$

where R denotes the electrostatic force; from which it is evident that as s becomes smaller, R becomes greater. But when the discs were heated well, immediately before the taking of the observations, the curve obtained satisfies the equation—

$$V = 87{\cdot}04\ s - 19{\cdot}56\ s^2$$

a parabola; from which we deduce

$$R = 87{\cdot}04 - 19{\cdot}56\ s.$$

This result, in my opinion, establishes the truth of Clark-Maxwell's hypothesis, that the greater electromotive force required at the smaller distances is due to the existence of condensed gas on the surface of the discs. Precisely similar results were obtained when hydrogen was substituted for air.

The method, when applied to measure the difference of potential required to produce a ·5 centimetre spark at different pressures of the air, shows that for the range between the atmospheric pressure and twenty mm.,

$$V = \cdot 0458 \sqrt{\{p^2. + 203\, p\}}$$

where p denotes the pressure in millimetres of mercury.

The electric strengths of several gases were determined by comparing the differences of potential required to pass a ·5 centimetre spark through the gas at 746 mm. pressure.

Dielectric.	Electric Strength.
Air,	1
Carbonic acid,	·95
Oxygen,	·93
Hydrogen,	·63
Coal gas,	·93

Several series of observations of the difference of potential required to produce a spark between spherical surfaces for distances up to 15 centimetres confirm the result published in the paper already referred to, that the difference of potential is proportional to the square root of the length of the spark.

3. On the Compressibility of Water, Sea-Water, a four per cent. Chloride of Sodium Solution, Mercury, and Glass. By J. Y. Buchanan, M.A., F.R.S.E.

4. On the Action of Heat on some Salts of Trimethyl-Sulphine. By Professor Crum Brown and J. Adrian Blaikie, Esq.

(*Abstract.*)

The authors describe the preparation, properties, and action of heat upon the following compounds of trimethyl-sulphine :—

1st, The sulphide $[(CH_3)_3S]_2S$;

2d, The hyposulphite (thiosulphate) $[(CH_3)_3S]_2S_2O_2$, H_2O;

3d, The oxalate $[(CH_3)_3S]_2C_2O_4$, H_2O.

1st, The sulphide was prepared by dividing a strong solution of trimethyl sulphine hydrate ($[(CH_3)_3S]HO$) into two equal parts, saturating one with sulphuretted hydrogen, and then adding the other. The strong aqueous solution thus obtained was placed under a bell jar filled with coal-gas over anhydrous phosphoric acid. After a certain concentration had been attained, sulphide of methyl began to evaporate along with the water. When a solution prepared in this way was sealed up in a glass tube, a very slight rise of temperature caused the liquid to separate into two layers, the upper consisting of sulphide of methyl and the lower of aqueous solution—

$$[(CH_3)_3S]_2S = 3(CH_3)_2S.$$

This aqueous solution has all the characters of an alkaline sulphide. It dissolves sulphur, forming an orange-coloured polysulphide, it dissolves sulphide of antimony, gives the characteristic reaction with nitro-prusside, and, when treated with an acid, gives off sulphuretted hydrogen, a salt of trimethyl-sulphine being left in solution. When exposed to the air, the sulphide is rapidly oxidised, hyposulphite being produced.*

2d, The hyposulphite is best obtained by oxidation of the polysulphide, by exposure to the air.

It crystallises in clear four-sided prisms with one molecule of water of crystallisation. Analysis gave the following results :—

	I.	II.	III.	Calculated.
C, . . .	24·85	24·64	...	25·35
H, . . .	6·94	7·06	...	7·04
S, . . .	...	...	44·65	45·07

The salt is very hygroscopic, sparingly soluble in alcohol, and gives all the reactions of an alkaline hyposulphite. Over anhydrous phosphoric acid it loses 6·37 per cent. of water—the formula requires 6·33 per cent.

The anhydrous hyposulphite, when carefully heated to about 135° C., gives off sulphide of methyl—5·545 grammes, heated in this way, lost 1·308 grammes of sulphide of methyl, equal to 23·58

* See *ante*, pp. 320, 321.

per cent., and left a white crystalline substance, soluble in water, alcohol, and ether. The authors are at present engaged in the investigation of this product.

3d, The oxalate is obtained by treating the iodide with oxalate of silver. It crystallises, with one molecule of water, in clear hygroscopic plates.

Analysis gave the following results :—

	I.	II.	Calculated.
C, ...	36·95	36·78	36·92
H, ...	7·87	7·89	7·69

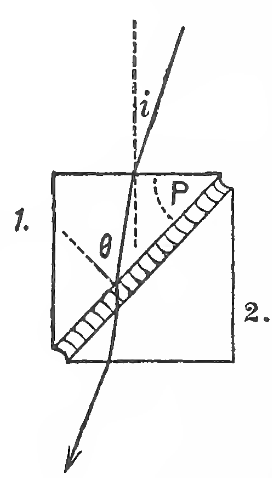
On carefully heating the salt to 110° C., the water of crystallisation is given off. At 146° C. the anhydrous salt decomposes into sulphide of methyl and pure oxalate of methyl—

$$[(CH_3)_3S]_2C_2O_4 = 2(CH_3)_2S + (CH_3)_2C_2O_4.$$

The chromate and the iodate of trimethyl-sulphine were also prepared. Heated to about 140° C. they both fuse, and almost immediately explode.

5. Extracts from two Letters by Professor Quincke on the Refractive Indexes of Glass and Quartz, as tested by Reflection from the Surface. Communicated by Sir William Thomson.

In answer to your question about the alteration of surface in quartz crystals, I place the glass or quartz plate whose refractive index χ is to be determined between two right-angled flint-glass prisms, with oil of cassia, and measure the angle θ, at which total reflection begins from the hypothenuse of the first flint-glass prism ; the angle θ can easily be calculated from i, μ the refractive index of the flint-glass, and P the angle of the prism. Sunlight falls from a collimator with a slit upon the system of flint-glass prisms, and after passing through the second prism is examined by a direct vision set of



prisms, I turn the system of flint-glass prisms until the spectrum appears to be broken off at a definite Fraunhofer line, measure the angle i, and thus obtain—

$$\operatorname{Sin} i_{,} = \frac{\sin i}{\mu}, \quad \theta = \mathrm{P} - i, \quad \chi = \mu \sin \theta .$$

Of course one can use flint-glass prisms other than right-angled ones, and, in fact, for measures of quartz, I have had flint-glass prisms made by Steinheil in Munich, in which i was only a few degrees. In a plate of quartz cut perpendicularly to the optic axis, one can easily determine in this way the refractive indices of the ordinary and extraordinary ray by interposing in front of the eye a Nichol's prism in the proper position. In fresh quartz plates I obtained almost exactly the same values as Rudberg:—

Rudberg.

B	C	D	E	F	G
1·54090	1·54181	1·54418	1·54711	1·54965	1·55425
1·54990	1·55085	1·55328	1·55631	1·55894	1·56365

Quincke ‡ zur Axe.

1·54108	1·54207	1·54412	1·54710	1·54966	1·55365
1·54987	1·55065	1·55338	1·55622	1·55892	1·57166

Quincke Quartz ⊥ zur Axe ↺

1·54022	1·54092	1·54318	1·54575	1·54845	1·55246
1·54880	1·54955	1·55245	1·55533	1·55801	1·56163

Quincke Quartz ⊥ zur Axe ↻

1·53958	1·54087	1·54335	1·54649	1·54868	1·55243
1·54789	1·54933	1·55199	1·55508	1·55758	1·56192

The differences of the individual measurements in different specimens I attribute to difference in the properties of the specimens of quartz themselves.

In old quartz surfaces which have already been about twenty years in my possession, and in others which I have found in the physical collections in Wurtzburg and Heidelberg, the refractive index for the Fraunhofer line D varied between 1·5141 and 1·5374 for the ordinary ray, and between 1·5216 and 1·5470 for the extraordinary.

I allow the first surface of the quartz to adhere by capillary attraction, and my large horizontal circle, by which μ and i are measured, reads to two seconds.

Crown glass plates from Steinheil, which had lain ten or twelve years in a press, and whose refractive index for D was 1·5245, gave, by total reflection at the surface, the refractive index 1·4903.

The alteration of the surface appears to me to be due, in quartz as in glass, to a chemical change of surface, perhaps to the vapour in the air forming a hydrate of silicic acid, or a hydrate of silic.

The above measures and arrangements have not yet been published, but they are entirely at your service if you can use them in your article on Elasticity.

I hoped to have sent you with the former ones some measures of the refractive indices of natural quartz surfaces, but then the observations have to be made with reflected light, which impairs their accuracy; besides, in spite of great trouble, I have not been able to procure any quartz whose surfaces were flat enough and complete enough for this inquiry. The only crystalline surface which I could examine seemed to have the same refractive indices as fresh polished surfaces. Besides, I am not astonished to find different refractive indices in different quartz crystals, since I have invariably found slight variations in the optical constants even for light transmitted through different specimens of crystals, for instance, in the amount of rotation of the plane of polarisation. Even if one had kept the crystal for thousands of years under the same physical conditions, for instance, at the same temperature, &c., still, according to my opinion, the mode in which it was originally formed would affect its final stationary condition. Crystals, like human beings, and like films of liquid upon heterogeneous solid or liquid surfaces, carry with them during their whole existence the mark of their origin or birth. Two bodies can only show properties extremely alike, never exactly the same.

Professor Jenkin called attention to experiments made by Mr Gott in St Pierre, and published in the Journal of the Society of Telegraph Engineers. Mr Gott converted two siphon recorders into a telephonic system by mechanically connecting the suspended coils with diaphragms. In this experiment the only conceivable mode of action was analogous to that suggested by Professor Graham Bell as the explanation of his telephone. This explanation, if not complete, was not, in Professor Jenkin's opinion, erroneous. Professor Jenkin announced that he had, with Mr J. A. Ewing's assistance, constructed one of Mr Edison's phonographs, and that this instrument, like the telephone, gave a nasal intonation to the words spoken by it.

Monday, 4th March 1878.

D. MILNE HOME, LL.D., Vice-President, in the Chair.

The following Communications were read:—

1. Proposed Theory of the Progressive Movement of Barometric Depressions or Storms; being in continuation of the Paper read before the Society on July 5, 1875. By Mr Robert Tennent.

In this paper it is not proposed to discuss in their relations to storms the effects of rain, of the earth's rotation, of areas of high and low pressure external to the storm-area, and of the prevailing westerly winds, which are doubtless occasional factors in the progressive movement of storms. What it is intended to show here is that storms possess in themselves a self-motive power, by which their onward movement over the earth's surface is determined.

It will tend to clearness if attention be pointed at the outset to two very different kinds of barometric depressions. The one which accompanies the true cyclones of the tropics is, on comparison with the height of the disturbance, of very limited extent, while the other, of which our European storms are examples, and which alone are dealt with in this paper, is of very great extent as compared with the height. The modes in which these two distinct kinds of barometric depressions tend to fill up are widely different from each other, owing particularly to the degree in which the element of friction is introduced by the forces set in motion within the storm itself.

Mode in which a Barometrical Depression of a Narrow Diameter fills up.—In this case, since the diameter of the depression is small as compared with the vertical height of the storm, it follows that the inflow of the air-currents towards the area of lower pressure will take place over a surface of comparatively small extent, and will consequently meet with but little retardation from friction. Hence the process of inflow toward the centre of the barometrical depression will take place with comparative facility in true tropical cyclones of small extent, and be therefore characterised by steep barometrical gradients.

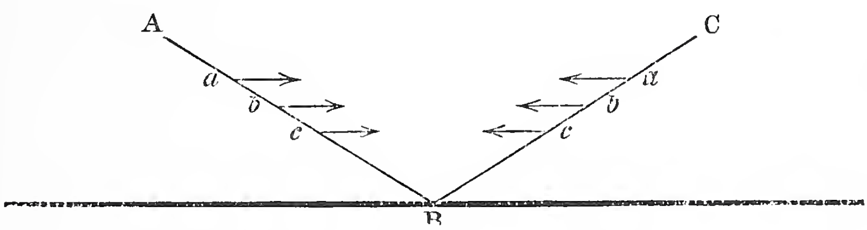


Fig. 1.

Let A B C (fig. 1) represent a vertical section of such a depression, and let the small arrows *a*, *b*, *c*, show the direction in which it will tend to fill up over a surface of comparatively small extent, accompanied by a steepening of the gradients B A and B C.

Mode in which a Barometrical Depression of Wide Diameter fills up and in the process tends to open out.—In European and American storms, the barometer at their centres falls about as low as in tropical cyclones, but the barometric gradients are much less steep, and the disturbances cover a much more extensive region. Redfield has pointed out that their vertical height is often not more than the two-hundredth part of their horizontal breadth. It follows from this essential difference between these storms and true cyclones that the mode of inflow of the air-currents towards the central low pressure can neither take place in the same manner nor with a similar facility, but, on the contrary, much friction must now be called into play, owing to the extensive resisting surface over which the air-currents are drawn.* Since friction is much greater near the earth's surface than it is aloft, it follows that such storms are characterised by greatly retarded surface currents, and rapid upper currents. Looking at the mass of the atmosphere on the outskirts and outside the area of the storm as the source whence the incurring air-currents draw their supply, it is obvious that as regards the true cyclone the source of supply is easy and copious, whereas in the case of our widespread European storms, supply is comparatively scarce, and therefore defective. In truth, as the lower and denser air-currents are, owing to the enormous extent of surface they cover, much retarded by friction, the main source of supply for the inflowing currents is chiefly to be found in the rarer, more mobile, and more rapid upper currents which flow comparatively free and unimpeded. This essential difference in the mode of inflow of the air-currents of these two types of storms is considered to be due to differences in the amount of friction, accompanied also by a great difference in the introduction of the important element of Time. The difference in velocity between the surface and upper currents is often very great. Thus Glaisher has shown from his balloon ascents that the upper currents are sometimes five or six times more rapid than the surface currents, while from observations made at the top and base of Mount Washington, it was shown that on one occasion the wind on the summit was blowing with a velocity of ninety miles an hour at the time that a calm prevailed at the base of the mountain.

The following illustration will show what is meant in this paper by the expression opening out all round. Let a barometric depression be formed several hundred miles in diameter, and let the stratum of air resting on the surface be calm, whilst aloft upper currents flow in upon the centre at the rate of ninety miles an hour. In this case the central inflow will be carried out entirely by the upper currents, and consequently there will result from this mode of inflow an outward extension or an opening outwards of the area of diminished pressure.

In fig. 2, A B C may be supposed to represent the vertical section

* "Proceedings of R.S.E." vol. viii. p. 613 and p. 614.

of the barometric minimum of a true tropical cyclone as in fig. 1. Let now D B F represent a vertical section of a barometric minimum of a very much larger diameter. When it opens up all round in the way described by means of rapid upper currents, it will be accompanied by a lowering of the surrounding gradients from the upper part of which the main source of supply is derived. The result is

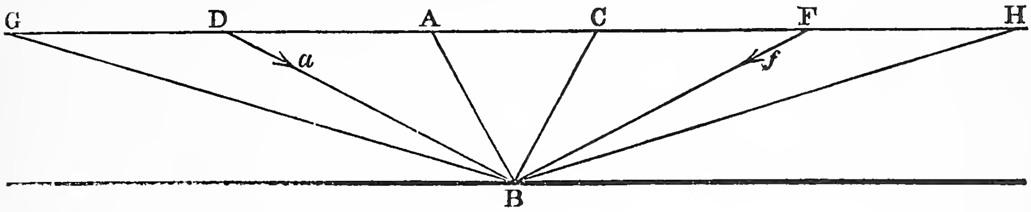


Fig. 2.

that the depression originally embracing a circular space whose diameter was D F will widen out, and the diameter extend to G H. The inflow which in this way takes place along the gradients bounded by the circle whose diameter is D F, as shown by the arrows *a* and *f*, will now have the effect of lowering the gradients B F and B D to B H and B G. The result of this may be represented by the removal of the air comprised in the spaces G B D and H B F.

When the air moves more rapidly aloft than it does near the surface, it may be conceived as moving onwards, not in vertical, but in inclined columns; and we have endeavoured to show* that this mode of inflow is attended by "lifting," and to some extent by fictitious pressure, by which is meant that although the barometer indicates correctly the elasticity of the air, still it no longer represents its real mass overhead, but a pressure more or less diminished owing to the mechanical movement of the air and to friction. The opening out or extension of the area of barometric minimum, with D F for its diameter, to a wider area, having G H for its diameter, is effected by the upper currents, indicated by *a* and *f* of fig. 2. These upper currents, which flow in upon the low central depression from a great distance all round, have their source of supply in the upper still atmosphere around and outside the circular space over which they blow. It is here where outward extension and shallowing out commences, viz., along the curve which

* "Proceedings," vol. ix. p. 412.

indicates the points where removal of air first begins to exceed restoration. This line is what has been previously designated as "the curve of outward propagation."*

This point, which is considered to be of great importance, may be better understood by an illustration. If a river flowing down an incline does so uniformly, and at an equal rate of speed, removal will equal restoration; but if in the lower part of its course a more rapid removal is inaugurated, while restoration or supply above remains as before, the curve representing the point at which the increased removal begins to travel upwards will represent the forward movement of this curve of outward propagation or extension.† Let A B C (fig. 3) represent the inclined surface of a river, A B the

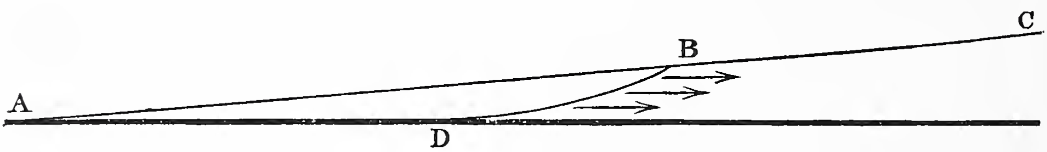


Fig. 3.

lower part of its course, in which a more rapid flow, and consequently a more rapid removal, has commenced, and D B the curved line which represents increased removal beginning to travel upward. Let us suppose that A is the low centre of a large sheet of water, towards which currents set in all round, and are there carried off. If now the inflow towards this centre there begins to increase in speed, the curve which extends all round will be propagated outwards, or in a direction opposite to that in which the currents flow. A depression may thus practically fill up by shallowing out, extending all round, and thereby lowering the gradients. When a depression fills up rapidly, it is, of course, attended by a correspondingly rapid rise of the barometer; but when it shallows out by a process of lateral extension the barometric rise is much slower. It is only in the case of an imperfect fluid, such as air, flowing over a resisting surface that the more special modes of inflow here insisted on can occur. On a frictionless surface they could not take place. When we consider the differences in the high pressures surrounding the depression on its different sides, and the different qualities of the air, as regards moisture and temperature, in different parts of the

* "Proceedings," vol. viii. p. 613. † *Ibid.* vol. viii. p. 614.

depression, it is evident that the mode of inflow cannot be uniform all round, and that horizontal extension will take place in some one particular direction. This direction will be determined by the curve of outward propagation, which thus marks the direction of the onward course of the depression or storm.

The Mode in which Opening Out takes place, accompanied by a transference of the Depression from West to East.—If a vessel be lowered into the sea a corresponding amount of water will be displaced. Let it then be supposed to move forward to the extent of its own length, an opening out ahead will take place, and a filling up astern. The original displacement will thus be closed up, but a similar displacement will be found in the new position to which the vessel has moved. Owing to its rigidity the vessel moves forward unaltered, but as the surrounding water is mobile, it cannot do so, hence it opens out and fills up. The currents which run aft on each side of the vessel represent its forward movement, while there is no real movement of the surrounding water, except its gradual transference to the rear, in the direction opposite to that in which the vessel moves. When an area of low pressure moves in an easterly direction over the British islands, and to a distance equal to that of its own diameter, it will do so in a somewhat similar manner, viz., by opening out and filling up. There is, however, an important difference to be noted; for while the tranference of the water, which enables the vessel to move forward, takes place on each side in the direction of the stern, the transference of the air, when the depression moves eastwards, takes place on the north segment from the front, where it opens out to the rear where it fills up, as all observation shows. It may be pointed out here that while depressions move forward, the aerial particles, or the mass of air, does not move forward, and viewed in this light the curve of outward propagation may be regarded as being a *purely ideal curve.*

The severe N.E. storms, which are of so frequent occurrence as to form a marked feature of the climate of the New England States, were long ago pointed out to advance on this region in an opposite direction from that in which they blow. Let the area of the storm be represented by the circle A B C F (fig. 4), then the front part A B C is the curve of outward propagation, which moves toward the N.E. in the direction of the arrow D E, this being the contrary direction to that in which in the segment A B the N.E. wind blows. Clouds apparently moving from the S.W. are often observed in this part of the storm, where N.E. wind first begins to exhibit itself. No

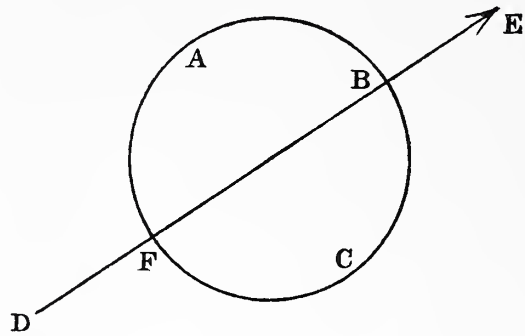


Fig. 4.

general movement of the air, however, takes place, because it is here that it first commences and continues to be generated from the still atmosphere ahead, where the curve of outward propagation advances upon it. This continued generation also represents what takes place in the clouds. Their apparent movement, which is due solely to their additional formation, is the result of atmospheric changes which must here take place. Hence occasionally they do not represent the force and direction of the wind. Similarly seamen often observe a gale approaching from some point to leeward.

It has been supposed by some writers that the progress of storms is due to the area of low pressure being impelled forward by the high pressure usually found near their rear, aided by the diminished pressure in front, which is due to heat and vapour. If this were the case, it is evident that a derangement would take place as regards the circular symmetry of the storm and the direction of the spirally inblowing winds, owing to the enormous friction which these widespread storms would encounter in being pushed forward over the earth's surface. The more evident of these derangements would be the steepening of the barometic gradients in front of the storm, a result the reverse of what observation shows to take place. The Rev. Clement Ley has shown that the isobars are widest, and gradient consequently lowest in the front of the storm.* Further, if this were the cause of the progress of storms, areas of depression would move more rapidly over the ocean than over the land. Professor Loomis has shown, however, that while the average rate of

* See "Journal of Scottish Meteorological Society," vol. iv. p. 149.

progress of depressions over the ocean is 19 miles an hour, over the land the rate is 26 miles an hour. It is also evident that if a depression moves forward accompanied by spirally inflowing winds,—considered simply as inflowing air-currents, and not as representing an efficient motive force capable of generating forward movement,—a derangement as regards their force and direction would take place, especially on those occasions when the onward march of the storm attains a speed of 70 miles an hour. But in no case has such a derangement been observed.

Proposed Theory as to the Progress of Storms.—When, however, the depression is not being impelled forward in the mode above described, but in virtue of an opening out in front, and when the air-currents, inflowing spirally upon the centre, are regarded as an effective motive force capable of causing progressive motion, no derangement can be supposed to take place as regards the force and direction of the air-currents; and no steepening of the gradients in front will take place, since the change in the position of the depression is effected with facility by the transference of the air from the front to the rear along the north segment of the depression.

A depression advances by means of these spirally inflowing currents, owing to the difference which exists in their *mode of inflow*, and consequently in their force and direction; and it is in this way that outward extension in one direction is carried out. This difference in the mode of inflow is mainly due to the introduction in a much higher degree of the element of friction into the currents of the front segment; in this way scarcity of supply is produced in this part of the depression, and the effect is intensified by the ascending tendency of the air in this segment of the depression. On the other hand, the currents which enter in at the rear are much less retarded by friction, because there is a descending tendency of the air in this segment, and, consequently, supply is copious.* The low central pressure, by travelling onwards towards the region in front, where supply is scarce, and where opening out takes place, practically overcomes this scarcity of supply, and thus uniformity of inflow is practically restored by progressive movement. If there was no progress, and if the depression was accompanied by this want of

* "Proceedings," vol. viii. p. 613–615.

uniformity in the mode of inflow, it would then tend to fill up. Progress is therefore the means by which a depression is maintained.

Since the difference in the mode of inflow is due to a difference in the amount of friction called into play, we have here, probably, the reason why storms advance more rapidly over the land than over the ocean. The deflection of the winds produced by the earth's rotation, and the irregularity in the amount of pressure at different points immediately outside the area of the depression itself, have doubtless important bearings on this whole question.

It is very generally supposed that the spiral inflow to the centre is supposed to be there carried off by means of a vast ascending current from the centre. This opinion is, however, open to serious objections, but which it would be out of place here to discuss.

The general result is, that progress is regarded as being due to an irregularity in the direction and force of the spirally inflowing currents, arising from the different degrees in which the element of friction is called into play in the different segments of the storm, accompanied or aided by high pressure on the right of the direction in which the depression moves.

Summary.—When a depression is formed with a narrow diameter, it fills up rapidly if it has a copious supply from the surrounding atmosphere, and the filling up is of course attended by a rapid rise of the low central barometer. But if the depression has a very large diameter, the element of friction must enter largely into the problem. The retarding influence due to this cause will necessarily be greatest in the denser currents near the earth's surface; while the light, rarefied, and mobile upper currents will move with great facility. Hence the main source of supply will be thrown on these freely flowing upper currents. The rapidity and facility with which they inflow give rise to horizontal extension or shallowing out aloft, and this extension would take place equally all round if the inflow were uniform in all the segments of the depression. If, however, this uniformity of inflow does not exist, but is more copious in one segment and scarce in another, then extension and shallowing out will take place towards that particular direction where supply is scarce. Hence progressive movement takes place in this direction, and is attended by filling up in the rear. When supply is equally copious in all directions, the low central barometer rises rapidly; but when the supply is not copious and uniform all round, the barometer does not rise, because, though the depression fills up in the rear, there is a corresponding opening out in the front, and the central barometer remains low —thus producing the central spiral inflow which generates the motive force to which the progressive movement is due.

2. On the Thermo-electric Properties of Charcoal and certain Alloys, with a supplementary Thermo-electric Diagram. By C. G. Knott, B.Sc., and J. G. MacGregor, D.Sc. Communicated by Professor Tait.

3. On the Auriferous Quartz of Wanlockhead. By Dr Lauder Lindsay.

4. On the Discharge of Electricity through Turpentine. By A. Macfarlane, B.Sc., and Mr R. J. S. Simpson. Communicated by Professor Tait.

5. Remarks on the Phonograph. By Professor Fleeming Jenkin and Mr J. A. Ewing.

Professor Fleeming Jenkin and Mr J. A. Ewing exhibited a phonograph which they had constructed, and also some curves drawn on paper representing the indentions produced in the tinfoil of the instrument by vowel sounds.

The phonograph exhibited had been constructed from a description of the instrument invented by Mr Edison, and consisted of a barrel about four inches diameter and four inches long, mounted on a spindle on which a square-threaded screw had been cut. One bearing of the spindle was cylindrical, and the other was a nut in which the screw worked. A fly-wheel handle turned the spindle and barrel, which advanced during each turn by a distance equal to the pitch of the screw. A helical groove, about $\frac{1}{20}$ in. in breadth, was cut on the surface of the barrel, having the same axial pitch as the screw on the spindle.

A smooth strip of tinfoil was gummed round the barrel, and indentations produced in this strip by a point attached to a vibrating disc stretched across a short brass cylinder $2\frac{1}{2}$ in. diameter.

The same vibrating disc was used to produce the indentations and to reproduce the sounds.

The usual ferrotype plate had not been tried as a vibrator, for hard metal discs were found to give a disagreeable resonance to the voice and its reproduction. A slack tinfoil disc had been used with good results; the marker was attached firmly to a small disc of stiff paper $\frac{3}{4}$ in. diameter, gummed to the tinfoil; a short piece of watch spring was also attached to the marker, so as to give the disc a rapid period of vibration.

A sentence reproduced by this disc was loud enough to be heard by many people standing round, and sentences had been heard by several persons who understood them without any previous idea of what they were. This result could not, however, always be secured. When the sentence was known to the hearers, it appeared to be given back with startling accuracy. The vowel sounds were more distinct than the consonants, but the consonants also were distinctly to be heard. The tinfoil vibrator gave more articulate sounds when slack and irregular than when it was neatly strained over the end of the tube.

An oil-silk disc had also been tried with no spring, and a simple marker attached to a disc of mica, gummed to the oil-silk. This disc gave purer sounds than the tinfoil, but they were not nearly so loud. The indentations on the tinfoil were excellent to the eye, and quite as large as with the tinfoil.

The oil-silk answered best when irregularly stretched.

An india-rubber film, with a similar mica disc and rigidly attached marker, had also been tried, and gave beautiful records on the tinfoil strip, but this disc failed to reproduce the sounds accurately, having a note of its own.

This latter disc was used to produce records which were subsequently enlarged and shown on paper in the form of curves. The india-rubber was preferred for this purpose, because the gentle and uniform pressure which it gave did not tend to obliterate the records. The curves exhibited showed the form of the indentations magnified about five hundred times. This magnification was effected by two compound levers, of which the second was a glass siphon like that of Sir William Thomson's recorder. This siphon deposited the trace of the curves on a paper strip rolled on the periphery of the large disc used by us for the experiments on friction published in the "Transactions of the Royal Society of London" for 1877.

The authors had not yet had time to analyse these curves and compare them with Helmholtz's theory of vowel sounds.

The advantage of recording the curves corresponding to the several sounds indirectly by means of the tinfoil, instead of directly, as in the case of the phonautograph, consists in the fact that with this arrangement the amplitude of the oscillations may be greatly magnified without introducing any sensible error due to the inertia of the moving parts. The paper band and tinfoil record were moved at a very slow speed, and their relative advance was rendered quite definite by a mechanical connection between the two barrels on which they were wound.

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

Charles Davidson Bell, Retired Surveyor-General, Cape of Good Hope, 19 Dean Terrace.

James Blyth, M.A., 8 Middleby Street.

R. K. Galloway, M.A., Jesus' College, Cambridge, 10 West Claremont Street.

Monday, 18th March 1878.

Sir WILLIAM THOMSON, President, in the Chair.

The following communications were read:—

1. On Thermal Conductivity. By Professor Tait.

(*Abstract.*)

This paper contains the results of a laborious series of experiments and calculations carried out during the last ten years, the object being the repetition and extension to other metals than iron of Forbes' experiments, described to the Society in 1862 and 1865.

As a check upon his work, the author first experimented upon the iron bar employed by Forbes, and reproduced, almost exactly, Forbes' results for that metal—the most notable feature of which, besides the first really trustworthy determination of absolute conductivity, was the discovery that the thermal conductivity of iron is diminished by heating, and is very nearly proportional to the reciprocal of the absolute temperature.

None of the other metals tried by the author, viz., copper (two specimens, selected as being specially good and specially bad conductors of electricity), lead, and German silver, gave at all analogous results. Their absolute conductivities have been determined; and the changes produced by heat in these conductivities were, where any, very slight, and uniformly in the direction of improved conductivity at higher temperatures.

A second part of the paper is promised, to contain comparisons of thermal and electric conductivity in the same substance, with an investigation of the "Thomson effect."

2. On the Wave Forms of Articulate Sounds. By Professor Fleeming Jenkin and Mr J. A. Ewing.

In this paper the authors gave a preliminary account of experiments made with the help of the phonograph exhibited at the last meeting. The following results have been obtained:—

1. The vowel sounds can be produced by maintaining the relative pitch of the simple tones of which they are composed constant, although the absolute pitch of those simple tones may vary greatly. The authors found that whether they turned the barrel of the phonograph at a greater speed or at a less speed than that which had been maintained while the human voice produced the indentations, the several vowels were given back with equal distinctness.

This experiment proves that within certain limits a given wave form in the tinfoil produces a given vowel independently of the rapidity with which it passes the pricker of the vibrating disc. It proves therefore that within these limits a given group of partials is competent to produce a given vowel effect, whatever be the absolute pitch of the prime tone, and of the relative partials. This result is of much importance, since Willis, Wheatstone, Donders, and Helmholtz seem all to consider that a certain absolute pitch is characteristic of each vowel.

[Note.—*May* 24.—Since the above communication was made to the Society, the authors have improved their phonograph, and also their apparatus for enlarging the record embossed on the tinfoil. For this reason they have substituted in the figures shown better curves than those which were originally presented to the Society. Subsequent experiments with the improved instrument also render it desirable to add a few words to the statement made in the text as to the effect of altered speed in the quality of the sounds given by the phonograph. The original experiment consisted in speaking the set of vowel sounds, A, E, I, O, U (pronounced in Italian fashion), to the phonograph, and then listening to the sound given when the instrument was run at various rates. The vowels were observed to maintain their relative places, so as to form the complete series in which each individual sound was by contrast at least recognisable as the vowel which had been spoken. This experiment has since been frequently repeated with perfect success. Again, if a single vowel sound, such as "*oh*," be sung to the phonograph at an ordinarily high pitch, it will be found to remain *oh* through a considerable range of speeds; but the *oh* whose pitch has been raised by quickening the phonograph seems to the authors brighter than the *oh* sung by the human voice at a correspondingly high pitch; that is to say, it resembles "*awe*" to some extent. Similarly the spoken "*awe*," when accelerated, passes into a sound like *ah*. The change appears to be greater with some vowels than with others, and also to depend partly on the pitch at which the vowel has been originally sung. The instrument is, however, even in its present improved form, not well adapted for testing any fine gradation of the quality of sound. Although the authors now feel unable to speak with any confidence as to change of quality produced by a change of speed, they consider it proved that the vowels, A, E, I, O, U, remain distinguishable by contrast when all their constituents are simultaneously raised or lowered to a considerable extent, but they do not consider that the phonograph is as yet sufficiently perfect to enable them to judge how far the definition of constant quality suggested in § 3 below would or could be accepted by the ear.]

The authors' experiments do not prove that a given vowel always produces a definite wave form.

2. A given person pronouncing or singing a vowel sound at a constant pitch produces a wave form of remarkable constancy. This proves that the relative phases of the simple tones, of which the compound tone is built up, do not alter, while a single vowel is being sung or intoned on one note. The same person or different persons pronouncing the same vowel on different occasions, will frequently reproduce wave forms closely similar, but the experiments are not yet sufficiently extended to determine whether a given voice speaking the same vowel at a given pitch will always produce the same wave form.

3. A given person singing the same vowel throughout an octave, often produces wave forms of different appearance on different notes, although he may be endeavouring to keep the quality of his vowel constant. It must on this be remarked that up to the present time there has been no standard or criterion as to constancy of quality at different pitches. This has been a mere matter of appreciation by different ears. A standard wave might, indeed, be taken and declared to be the form which gives a constant quality at all pitches. The phonograph is sufficiently distinct in its utterance to allow it to be said that such a standard *a* or *o* will be accepted within a considerable range as the same vowel, but it cannot be affirmed that all hearers would say that the quality of the vowel was constant in their judgment at all pitches. It is, however, possible that henceforth constancy of quality may be defined as that given by constancy of wave form.

4. The vowels are heard equally well, or nearly so, whether the machine is run backwards or forwards. The authors put in the saving clause "nearly" everywhere, because the sounds obtained from the phonograph are not sufficiently clear to warrant more absolute conclusions. This reversibility is a necessary consequence of Helmholtz's theory that the ear decomposes the compound tones into their harmonic constituents, and is indifferent to the phase relation of these.

5. Not only the vowels but the consonants are substantially the same when produced backwards. This result is novel to the authors, and is, they believe, quite unforeseen. There is little to wonder at

in the fact that *r*, or *s*, or *θ*, which are continuous, should sound alike backwards and forwards, but it is very remarkable that sounds like *p* or *k* at the end of the syllable should, when produced backwards, give the same effect as *p* and *k* at the beginning of a word.

The authors tested this singular fact in the following way:—They first arranged that a simple combination, such as "abafa" or "afaba," should be said to the instrument while one observer was out of the room. He then came in, and heard the word backwards. If the original word had been "abafa," he was in all cases able to say that he heard "afaba"; if the original word had been "adaka," he heard "akada." Inasmuch as he was quite uncertain whether the word spoken to the instrument was "adaka" or "akada," this experiment was in itself almost decisive. All the letters of the alphabet were tried in this way. Fearing, however, that when the alternative lay between two consonants, these might be recognised by some other quality than their true sound, the authors next proceeded to test the fact by speaking short words, such as "amma," "abba," and so forth, to the instrument. These were recognised by an observer from outside when spoken backwards. "Alla" was recognised at once; "appa" was called "abba"; "amma" was recognised at once; "atta" was called "adda."

The greatest difficulty was met with in distinguishing *s* from *f*, but the difficulty was not greater backwards than forwards. It seems due to the fact that the instrument used will not record the high tones which are obviously present in these sounds.

When the fact had been established that a consonantcould be sounded backwards between two vowels, monosyllables were next tested: "ba" was reversed and heard as "ab;" "ka" was reversed, and heard as "ak."

This is even more remarkable than the previous results, inasmuch as the mode of pronouncing *b* or *k* at the end of a word is different from that employed when it begins a word.

Finally, by pronouncing any word backwards, such as "noshaeesossa," and then reversing the instrument, the original word (association) was clearly heard. With a little practice, a sentence might be spoken backwards, and then heard intelligibly forwards.

The letter *h* was not in the phonograph to be distinguished from *ch*, and gave a single reversible sound. "Ho," when turned back, sounded distinctly "och."

Since both vowels and consonants sound alike backwards and forwards, it appears that articulate speech may be divided into a series of successive parts, each having a character of its own, and each reversible so far as sensation is concerned.

The separate reversible sensations correspond in the main to the letters of our alphabet, but our letter *i* represents at least two reversible elements, and *ch*, *ng*, or *th*, on the contrary, are single sounds. Clear, clean sounds, as spoken by educated people in all countries, seem remarkable for the small number of the elements that enter into each word. An uneducated man, speaking a dialect or *patois*, will make each nominal vowel consist of a large group of simple reversible sounds running into one another.

6. The wave form for the letter *r* has been recorded. It agrees with that obtained by Donders on the phonautograph, and consists of a series of simple curves, similar to harmonic curves, which gradually increase to a maximum, and then gradually decrease to a minimum. The number of waves separating the maximum from the minimum intensity of the sound was in the examples observed 7 or 8. The length of the whole period was therefore 14 or 16.

7. The wave forms obtained from the letter *o* in figs. 4 and 5 illustrate the difficulty which the ear, even of trained musicians, feels in determining the octave in which a given note is sung. It will be seen that each period consists of two very nearly equal parts, and that the amplitude of the second partial has obviously been large as compared with that of the prime tone. To one ear it might seem natural to call the pitch that of the second partial, since this was the loudest tone; to another the pitch would be that of the prime, because this tone was lowest in pitch. Naming a given note by the lowest of its component tones, irrespective of the relative intensity of the partials, seems to be a convenient but conventional rule.

8. The instrument as at present made does not record all sounds. This is shown by the fact that its reproduction of the French *u* is very imperfect, and *ee* is only sometimes well heard from it.

The curves laid before the Society were obtained by multiplying the traces on the tinfoil some 300 times by means of a system of levers. The lines were drawn on the paper by the plan of frictionless marking invented by Sir William Thomson, and used in his Siphon Recorder. A rise in the curve (on the page) corresponds to a hollow in the tinfoil. The arrow pointing from right to left, indicates the direction in which the tinfoil passed under the vibrating pointer when the sounds were uttered.

The authors hope soon to be able to communicate to the Society the results obtained by subjecting the experimentally drawn wave forms to harmonic analysis.

Fig. 1 gives the curve made by the sound of *oo*, as in "food," on

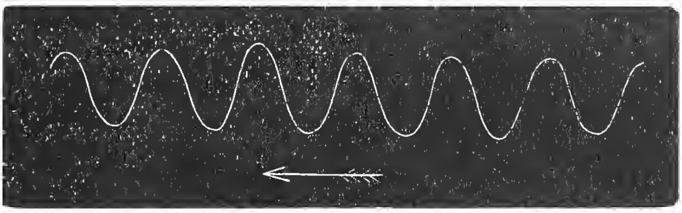


Fig. 1.

the note *b* ; fig. 2 is *oo* on *f′* by the same voice ; and fig. 3 is a still

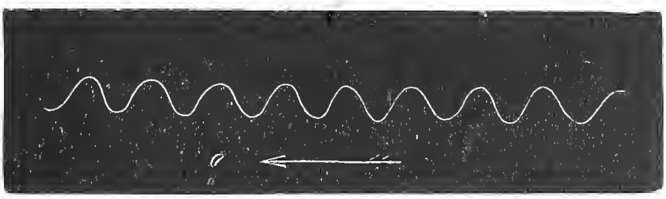


Fig. 2.

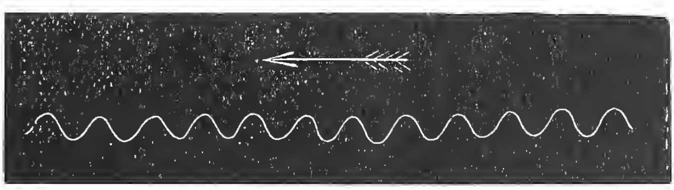


Fig. 3.

higher *oo* by another voice. As the speed at which the phonograph was turned was very nearly the same in all the examples, the relative

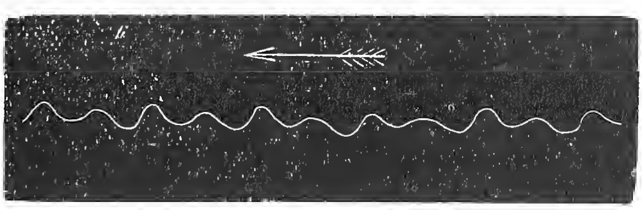


Fig. 4.

pitches are approximately given by the lengths of the periods.

Figs. 4, 5, and 6 show the waves given by the sound "*oh*," spoken at

Fig. 5.

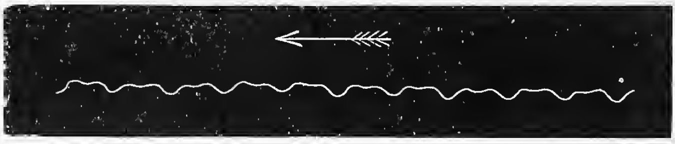


Fig. 6.

various pitches by the same voice (that of Dr Crum Brown). Figs.

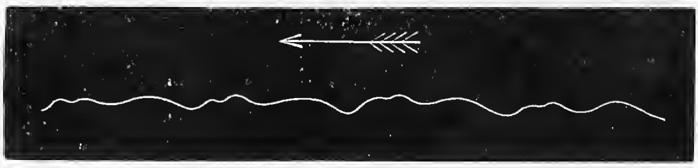


Fig. 7.

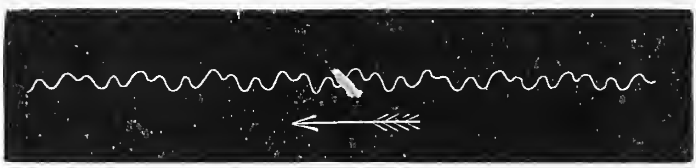
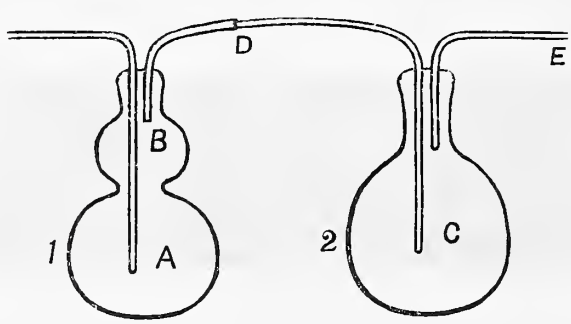


Fig. 8.

7 and 8 are given by the sound *a* in "father," the first on *c* by Sir R. Christison, the second on *c′* by Professor Jenkin.

3. On the Action of the Chlorides of Iodine upon Acetylene and Ethylene. By George M'Gowan.

Before giving the action of ethylene on the chlorides of iodine, I may shortly describe the method of preparing the latter, which I found to be the most advantageous :—



A weighed quantity of perfectly dry iodine being introduced into bulb A of flask 1, perfectly dry chlorine gas is passed over it, A being at the same time heated over a water-bath to about 60° to 80° C. The ICl_3, as it is formed, volatilises and condenses almost entirely in B, only a small portion escaping into flask 2. Chlorine is passed until no more iodine remains in bulb A. If the heating be omitted, only the outermost layers of iodine are converted into the trichloride, a kernel remaining unacted on. (Compare *Christomanos*, Berl. Ber. X. No. 5, p. 434.)

The tubes connecting the two flasks are ground into one another at D, so that no cork or caoutchouc fittings are required. This is absolutely necessary.

By attaching a small $CaCl_2$ tube to E, no moisture can enter the apparatus. The flasks can be weighed either together or separately. When passing ethylene or acetylene over either of the iodine chlorides, I found it best to cleanse and dry flask 2, leaving the gas to act on the chloride in 1. In that way any volatilised matter from 1 was condensed in 2, and the loss of weight was very trifling.

By adding the required weight of iodine to the ICl_3, and heating gently over a water-bath (the vessel being corked), ICl is easily prepared. I invariably got it in the state of long needles.*

Action of Ethylene (C_2H_4) *on* ICl_3.

By passing pure dry C_2H_4 over pure dry ICl_3 at 0° C., the two compounds *chloride* and *chloriodide* of ethylene are formed.

This reaction takes place quantitatively, thus :—

$$(C_2H_4)_2 + ICl_3 = C_2H_4Cl_2 + C_2H_4ClI.$$

	grms.	grms.
Weight of flasks, ...	169·2	
„ + ICl_3, ...	202·5	= 33·3 ICl_3
„ after saturation with C_2H_4 .	210·1	= 7·6 C_2H_4

Theory requires 7·98 grms. C_2H_4.

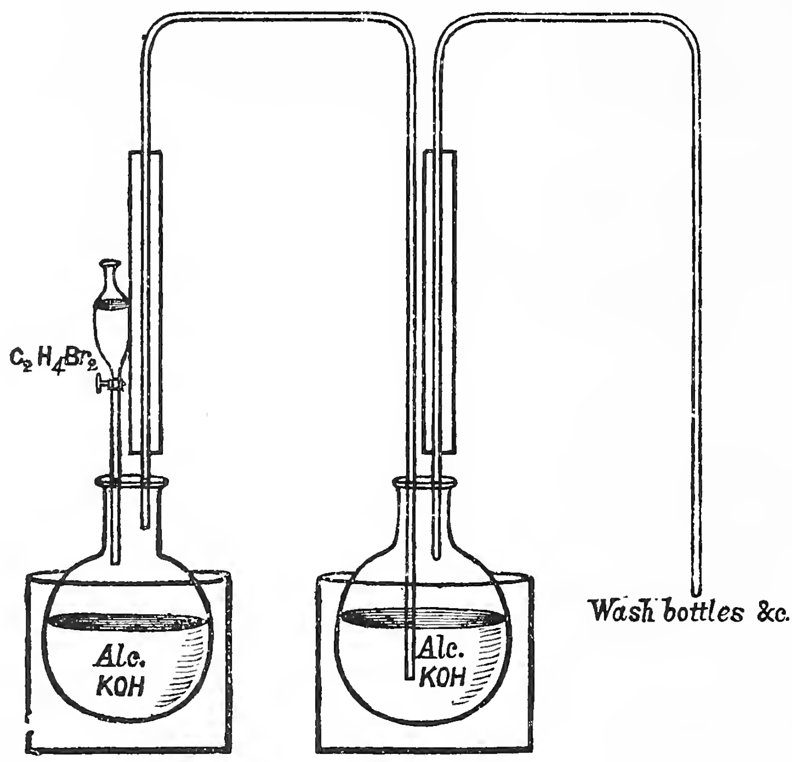
* It is generally stated that ICl dissolves in alcohol and ether, forming yellow solution. (See Watts's *Dict. of Chem.* vol. iii. p. 293, new ed.) The fact is, that if ether (perfectly free from water and alcohol) be dropped into ICl, a most violent reaction ensues. If, on the other hand, ICl be dropped into a large excess of ether (or alcohol) at 0°, it dissolves, at first forming a pale yellow-brown solution ; but, in the course of a few minutes, the ether becomes darker and darker from separation of iodine. The chlorine probably acts on the ether to form dichlor-ether ($C_4H_8Cl_2O$) for the most part. This requires to be investigated.

After saturation, the fluid was almost colourless, having only a pink tinge of iodine. The two above fluids can (after washing with dilute KOH and drying) with some difficulty be fractionated.

$C_2H_4Cl_2$ boils at 85° (Fittig).
C_2H_4ClI „ 147° (Maxwell Simpson).

Preparation of Acetylene (C_2H_2) *from Ethylene Dibromide* ($C_2H_4Br_2$).

Having prepared about a kilo. of $C_2H_4Br_2$, I decomposed it according to the method described by Sabanjeff (*Annalen*, Band 178). Briefly described, this method is as follows :—Into a thick flask of



about one litre capacity, more than half filled with a concentrated alcoholic solution of KOH, and heated over a water-bath to 100° or thereabouts, $C_2H_4Br_2$, pure or diluted with alcohol, is slowly and regularly dropped from a separating funnel. The C_2H_2 disengaged, passes off through an upright Liebig's condenser, also affixed to the flask, by which means most of the monobrom-ethylene (C_2H_3Br) formed at first, and carried away along with the C_2H_2, is returned to the flask. Some of it, however, escapes undecomposed from this flask into a second one, also containing alcoholic KOH, and fitted with an upright condenser, precisely in the same manner as the first. In this second flask it is practically entirely decomposed. The C_2H_2 is then passed through alcohol at 0° (to absorb traces of C_2H_3Br), then through two Wollf's bottles filled with water, and lastly through two $CaCl_2$ tubes. The gas may then be considered practically pure. To test this, I passed it for about half an hour through the two weighed flasks, surrounded by a mixture of ice and salt, but at the end of that time they had not gained in weight to speak of.

To prepare first cuproso vinyl oxide—

$C_4Cu_4H_2O$ (*Berthelot.*)

and then decompose this by dilute hydrochloric acid, is an extremely tedious and troublesome operation, and offers very little corresponding advantage. Sabanjeff discusses the other known methods for the preparation of C_2H_2 very fully in his paper. I also tried some of them, but found the above by far the best of all.

Action of Acetylene (C_2H_2) *on* ICl_3.

On passing C_2H_2 over pure dry ICl_3 at 0°, combination takes place with great evolution of heat. I always found more or less iodine separated out. My chief object being to try to prepare the compound, C_2H_2ClI, I did not investigate farther the products formed. Probably, if the ICl_3 were kept cold in a mixture of ice and salt, and the C_2H_2 diluted with air and passed over it very slowly, one might get the compound $C_2H_2Cl_3I$.

Action of C_2H_2 *on* ICl.

(*a.*) On pure ICl at 0°.

The evolution of heat produced by the combination was so great that more or less iodine always separated out.

(*b.*) On ICl dissolved in excess of ether at 0°.

The ether used must be perfectly free from alcohol and water. The C_2H_2 was passed as quickly as possible through this ethereal solution, to reduce as much as might be the secondary reaction of the ICl on the ether. It was absorbed greedily.

After saturation the ethereal solution was diluted with water, when a heavy liquid settled to the bottom. This was washed with dilute NaOH solution, and then with water, and dried over $CaCl_2$.

What ether remained in the above liquid was distilled off at 35° to 40° in a current of C_2H_2. Even at this temperature a slight decomposition occurred (iodine separating), so I stopped heating. It was found practicable, however, to fractionate the liquid (with very slight decomposition) in a current of steam. Repeated fractionating yielded

(1.) Ether + $C_2H_2Cl_2$, &c. (not examined).
(2.) C_2H_2ClI +.
(3.) Solid $C_2H_2I_2$.

To ascertain the composition of the above liquid I analysed it.

+ 0·6687 grm. ignited with pure CaO gave—

Tube . . .	22·9350 grms.
„ + (AgCl + AgI)	24·2482 „
„ + (AgCl + AgCl)	22·9207 „

(after converting AgI into AgCl by current of Cl).

AgCl (on filter) . . 0·0127 grm.

Calculated, this gives—

	Found.	Theory for C_2H_2ClI.
I	67·38 . .	68·77
Cl	18·83 . .	17·59

The above figures do not agree very exactly, but I had too small a quantity of the compound to be able to fractionate it perfectly. It was found impossible to determine its boiling point, as it decomposed very rapidly on heating, with separation of iodine.*

Spec. Grav. . . . 2·41 at 13·5° C.

Acetylene chlor-iodide is an ethereal liquid with much the same odour as the corresponding ethylene compound. Its vapour attacks the eyes. It does not decompose to any extent if kept in the dark.

Remarks on Acetylene Iodide ($C_2H_2I_2$).

I prepared from 50 to 60 grms. of the above compound by passing the gas through a saturated alcoholic solution of iodine. As the two

* It seemed to boil at first, on heating very rapidly, between 180° and 190° C., but this is not reliable.

combine very slowly, the iodine must be distributed in numerous small flasks, which ought to be hung on a glass rod, horizontally placed, to admit of their being shaken from time to time. (See Sabanjeff, *Annalen*, Band 178; also Semenoff (for $C_2H_4I_2$), *Zeitschrift für Chemie*, 1864.)

Sabanjeff states that he, while preparing the iodide in this manner, got at the same time a large quantity of a liquid iodide (also of the formula $C_2H_2I_2$). He supposes the one to be $\begin{matrix} CHI \\ \| \\ CHI \end{matrix}$ and the other $\begin{matrix} CH_2. \\ \| \\ CI_2. \end{matrix}$ Curiously enough, I only obtained a few drops of a liquid which could at all correspond to this second iodide. Whether it was the iodide, or a product of the action of the iodine in the alcohol, &c., I cannot say. The quantity got was too small.

The chief object which I had in view in preparing the preceding acetylene compounds (chlor-iodide, iodide, also tetrabromide, &c.), was that from them I might be able to obtain the corresponding cyanogen ones, which, if got and treated with an alkali, would probably have yielded chloro- (or iodo-) acrylic, and fumaric or maleic acids. I have not, however, as yet succeeded in obtaining them.

Whether C_2H_2 combines with iodide of cyanogen when the latter is dissolved in alcohol, I cannot yet say. It does not with dry ICN.

All attempts to obtain the cyanogen compounds of C_2H_2 by heating in sealed tubes (at temperatures ranging from 120° to 0°25) chlor-iodide or iodide of acetylene with the various cyanides {KCN, AgCN, (AgCN + KCN), $H_2(CN_2)$,} or with ICN and a metal, were fruitless.

When $H_2(CN)_2$ was used, generally no reaction occurred, and in nearly all the other cases ammonium salts were formed in large quantity. I had unfortunately only a small quantity of the chlor-iodide at my disposal.

Acetylene does not seem to be absorbed if passed into a hot solution of "nascent" formic acid, *i.e.*, a solution containing KCN and KOH.

4. On some Definite Integrals. By Professor Tait.

The integrals referred to occur in connection with applications of the Method of Electric Images. A curious special case was given to the Society in July 1875, but was not inserted in the "Proceedings." It depended on the fact that the image of a sphere, whose surface density is inversely as the cube of the distance from a point, is another sphere with a similar law of distribution. But any desired number of integrals, whose values can be at once assigned, may be obtained by various applications of the following process. Take any centrobaric distribution of electricity, and calculate directly the density induced by it at any point of an uninsulated sphere. This must be inversely as the cube of the distance from the centre of gravity of the given distribution.

Take, as a simple example, a uniformly charged sphere of non-conducting matter with unit charge, radius a, at a distance p from the centre of an uninsulated sphere of radius r. Suppose $r > a+p$, so that the inducing sphere is wholly internal. We see by the method above that the density of the induced charge at points defined by radii making an angle α with the line of centres is represented by either of the following expressions, multiplied by $\frac{1}{8\pi^2 r}$:—

$$\int_0^{\pi}\int_0^{\pi}\frac{(r^2-a^2-p^2+2ap\cos\theta)\sin\theta d\theta d\phi}{\left[r^2+a^2+p^2-2ap\cos\theta-2rp\cos\alpha+2ar(\cos\alpha\cos\theta-\sin\alpha\sin\theta\cos\phi)\right]^{\frac{3}{2}}}=\frac{2\pi(r^2-p^2)}{(r^2+p^2-2rp\cos\alpha)^{\frac{3}{2}}}$$

i.e., the double integral is independent of a.

Again, if the unit charge on the small sphere be distributed inversely as the cube of the distance from the centre of the large one, we have obviously for the induced density on the large sphere the expression—

$$\frac{a}{8\pi^2 r}\int_0^{\pi}\int_0^{\pi}\frac{(a^2-p^2)(r^2-a^2-p^2+2ap\cos\theta)\sin\theta d\theta d\phi}{\left[a^2+p^2-2ap\cos\theta\right]^{\frac{3}{2}}\left[r^2+a^2+p^2-2ap\cos\theta-2rp\cos\alpha+2ar(\cos\alpha\cos\theta-\sin\alpha\sin\theta\cos\phi)\right]^{\frac{3}{2}}}$$

But if the small sphere *include* the centre of the large one, *i.e.*, if $a > p$, the induced density is uniform; so that the value of the integral is

$$\frac{2\pi}{ar}.$$

If $a<p$, similar reasoning shows that the value is

$$\frac{2\pi\left(r^2 - \left(p - \frac{a^2}{p}\right)^2\right)}{a\left(r^2 + \left(p - \frac{a^2}{p}\right)^2 - 2r\left(p - \frac{a^2}{p}\right)\cos\alpha\right)^{\frac{3}{2}}}.$$

These values agree, as they should do, when $a=p$.

Monday, 1st April 1878.

Sir WILLIAM THOMSON, President, in the Chair.

The following communications were read:—

1. Chapters on the Mineralogy of Scotland. Chapter IV. Augite, Hornblende, and Serpentineous Change. By Professor Heddle.

(*Abstract.*)

A couple of months ago I had the honour of submitting to the Society a speculation upon the metamorphism of a particular rock mass. To-night I again return to metamorphism, submitting, however, not a speculation, but the closely elaborated process of the change which has affected another rock.

It is perhaps natural that the attention of one who approaches geology from the chemical and mineralogical sides should be immediately directed to those rocks which are either aggregates of simple minerals, or which are the products of changes effected *upon* simple minerals; natural also that consideration should be first given to those in which that change has been more immediately chemical than physical.

To no rock mass does this apply more directly than to serpentine. In my wanderings I have visited and closely observed the relationship of—with a single exception—every bit of this rock which is to be found between Unst and Tyree on the one hand,—Harris and the Black Dog Rock on the other.

In my analyses of a number of ill-defined minerals, generally believed to be products of the alteration of augite and hornblende, the subject recurred to me. In order to bring as much light to bear upon it as possible, I greatly extended the number of analyses which I undertook; and, as bearing upon it, I have now analysed 20 specimens of augite and its allomorphs, 15 of hornblende, and 21 of the products of the alteration of these two substances.

Founding upon the information gained thereby, I work out chemically the various steps and stages of the transmutation,—which transmutation will be shown and seen to be the formation of serpentine.

2. On the Old Red Sandstone of Western Europe. By Professor Geikie, F.R.S.

In a historical introduction the author gives an outline of the progress of research into the history of the Old Red Sandstone of the British area. This system is at present regarded as composed of three sub-divisions, Lower, Middle, and Upper, each characterised by a distinct suite of organic remains. From the absence of unequivocally marine fossils, and from lithological characters, it has been inferred by Mr Godwin Austin, Professor Ramsay, Professor Rupert Jones, as well as other observers, and is now very generally admitted, that the Old Red Sandstone, as distinguished from the "Devonian" rocks, probably originated in inland sheets of water. The object of the present memoir was to endeavour to trace out in that geological system of deposits the changes of physical geography which took place over Western Europe during the interval between the close of the Upper Silurian and the beginning of the Carboniferous period.

After a sketch of the probable conditions of the region previous to the commencement of the Old Red Sandstone, the author proceeds to show how the shallowing Silurian sea was converted here and there into *salinas* or inland seas, by a series of subterranean movements, which have left their indelible traces upon the upturned Silurian rocks. He divides his memoir into two parts, the first dealing with the Lower and the second with the Upper Old Red Sandstone. The present paper deals only with a portion of the first of these sections. It traces out the limits of the different basins in which the Old Red Sandstone of the British islands were deposited, and for the sake of convenience, as well as briefness of reference, proposes short geographical names for these basins, which are arranged as follows:—

Area of the Basins.	Short reference names proposed to be applied to them.
1. The Old Red Sandstone tracts of the north of Scotland, embracing the region of the Moray Firth, Caithness, the Orkney Islands, the mainland of Shetland, and perhaps part of the south-western coast of Norway.	Lake Orcadie.
2. The central valley of Scotland between the Highlands on the north and the Silurian uplands on the south, including the basin of the Firth of Clyde, and ranging across the north of Ireland to the high grounds of Donegal.	Lake Caledonia.
3. A portion of the south-east of Scotland and north of England, extending from near St Abb's Head to the Head of Liddesdale, and including the area of the Cheviot Hills.	Lake Cheviot.
4. A district in the north of Argyllshire, extending from the mouth of the Sound of Mull to Loch Awe, and perhaps up into the southern part of the Great Glen.	Lake Lorne.
5. The Old Red Sandstone region of Wales and the border counties of England, bounded on the north and west by the older palæozoic hills, the eastern and southern limits being unknown.	The Welsh Lake.

Lake Orcadie.—After describing the limits of this basin, and giving a sketch of the labours of previous observers in the Old Red Sandstone tracts of the north of Scotland, the author proceeds to examine the evidence for the threefold arrangement of the Old Red Sandstone proposed by Murchison. He shows that nowhere are the three groups, Lower, Middle, and Upper, found in consecutive order; that this so-called "Middle" division occurs only in the north of Scotland, where it lies unconformably upon the older palæozoic rocks, and is itself unconformably overlaid by the Upper Old Red Sandstone, thus occupying a position exactly similar to that of the Lower Old Red Sandstone on the southern side of the Highlands. He further points out that while some species of fishes are common to the Old Red Sandstone on the two sides of the Highland barrier, the lithological differences between the deposits of the two areas are so great as to make it evident that the rocks were laid down in distinct basins, and consequently that the fauna of each basin might be expected to be more or less peculiar, as in many analogous cases at the present day. As evidence that adjacent areas in the time of the Lower Old Red Sandstone were strongly marked off from each other in their faunas, reference is made to the contrast between the fishes and crustaceans of the Welsh region and those of Lanarkshire and Forfarshire, not a single species being common to the two countries, though some of the genera are. Reasons are then given why the argument used by Murchison from the occurrence of many of the Scottish ichthyolites in Russia could not be regarded as establishing the existence of a "Middle" division of the Old Red Sandstone.

The conclusion arrived at by the author is that the Caithness flags or "Middle Old Red Sandstone" are probably the general equivalents of the Lower Old Red Sandstone of other regions, and that this system consists in Britain of two well-marked divisions only—a Lower, which graduates in some places into the Upper Silurian rocks, and is separated by an unconformability from an Upper, which in many districts passes up into the base of the Carboniferous system.

The various districts into which the area embraced under the term Lake Orcadie may be divided are then described *seriatim*. The detailed structure of Caithness has been worked out by the author (partly with the co-operation of his colleagues in the Geological Survey, Mr B. N. Peach and Mr John Horne) as affording the most complete sections of the Old Red Sandstone in the north of Scotland. Arranged in descending order, the various stratigraphical zones stand as in the subjoined table :—

	Thickness in feet.
9. John o' Groats red sandstones, flagstones, and impure limestones and shales, . . .	2000
8. Huna flagstones, shales, and limestones, . .	1000
7. Gill's Bay red sandstones, ...	400
6. Thurso or northern group of flagstones, shales, and limestones, ...	5000
5. Wick or eastern group of flagstones, shales, and limestones passing down into red shales and sandstones, ...	5000
4. Dull red sandstones, red shales, and fine conglomerates, ...	2000
3. Brecciated conglomerates, ...	300
3. Badbea red sandstones and shales or clays, .	450
1. Coarse basement conglomerates, . . .	50
	16,200 ft.

From the four lowest sub-divisions no fossils have yet been obtained. The flagstones have yielded to Mr C. W. Peach and other observers many land plants (some of which resemble forms described by Dawson from the Gaspé sandstones) as well as *Estheria membranacea*, *Pterygotus*, sp., and many ichthyolites. Availing himself of the list of localities furnished to him by Mr Peach (to whom he cordially acknowledges his obligations), with the species of fish found at each, the author has constructed a table of the vertical distribution of the fossil fishes in Caithness. Some of the species range through almost the entire succession of beds. Some, however, are either peculiar to or very characteristic of one sub-division. Thus *Osteolepis arenatus* and *Dipterus Valenciennesi* are not noted except from the group No. 5. In the Thurso and the higher flagstones (Nos. 6, 8, and 9), *Acanthodes*, *Parexus*, *Cheiracanthus*, *Diplacanthus*, *Pterichthys*, and *Tristichopterus*—genera absent from the Wick beds—are found in greater or less abundance. These strata are further marked by peculiar species of genera which occur among the older flagstones, as *Coccosteus pusillus* and *Osteolepis microlepidotus*.

The Orkney Islands are assigned to the higher sub-divisions of the flagstone series, the protruding ridge of granite and gneiss which rises at Stromness and Gremsa being merely an indication of the irregular surface on which the deposits of Lake Orcadie were accumulated, and of the slow progressive subsidence of the area. The fossils, for which these islands have long been famous, include most of those of the upper groups of Caithness, with the addition of others which have been regarded as distinct. In the determination of these fossils, much skill is required to discriminate between the accidental differences of aspect resulting from the condition of fossilisation. The Orkney fishes, for instance, are preserved as black jet-like impressions which, often very perfect when first removed from the quarry, are apt to scale off, leaving in each case only an amorphous layer which, though it retains the contour of the fish, shows little or no trace of structure. On the shores of the Moray Firth, on the other hand, the organisms have been inclosed within calcareous nodules; their colours are sometimes brilliant, and their scales, plates, fins, and bones are often admirably preserved and remain unchanged in the Museum. Want of experience in these different modes of preservation may have led to a reduplication of species, especially in the case of the Orkney and Moray Firth fishes. Among the most interesting Orkney fossils is a portion of a *Pterygotus* (recognised by Dr H. Woodward), now in the British Museum. The occurrence there of this characteristically Upper Silurian and Lower Old Red Sandstone genus supports the view contended for in this paper as to the true horizon of the Orkney and Caithness flagstones.

The Shetland Islands contain a portion of the shore-line of Lake Orcadie, with its conglomerates and sandstones and the flagstones and shales of deeper water. Among these strata the Caithness *Estheria* occurs, with abundant stems and roots of large calamite-like plants with well-marked flutings, but without observable joints. Some ichthyolites of the Caithness type are said to have been found in Bressay. The general lithological characters are quite those of the sandy parts of the Orkney and Caithness groups. On the west side of the mainland of Shetland interesting evidence occurs to show the existence of volcanic action contemporaneous with the accumulation of the Old Red Sandstone. Beds of amygdaloidal lavas and bands of tuff occur among the sandstones, the whole being pierced by masses of pink felsite.

The south-western and southern margin of this great northern basin of the Old Red Sandstone can still be traced nearly continuously from the confines of Caithness to the borders of Aberdeenshire, its position being marked by a zone of littoral conglomerates. Beyond the edge of that zone, however, there occur some interesting outliers, which in some cases may represent long fjord-like indentations of the coast-line; in others may mark what were really independent basins lying at the base of the Grampian mountains. The author points out that probably most of the difficulty which has hitherto been experienced in understanding the sequence of beds along the southern shores of the Moray Firth, and their parallelism with those of Caithness and Orkney, is not to be attributed to the amount of detritus covering the country, but rather to the fact which has not heretofore been observed, that the Upper Old Red Sandstone, with *Holoptychius* and *Pterichthys major*, really overlaps unconformably upon the older nodular clays and conglomerates with *Coccosteus*, *Cheirolepis*, &c. This relation can be satisfactorily determined in Morayshire, and is now being worked out by Mr John Horne in the course of the Geological Survey. The author traces in great detail, from the Spey into Sutherlandshire, the development of the lower sandstones, conglomerates, and clays, which have been regarded as equivalents of the Caithness flagstones. He thinks that in no sense can this comparatively thin group of rocks (seldom 1400 feet in depth) be regarded as a mere southward attenuation of the great Caithness series, as suggested by Murchison, for that neither lithologically nor palæontologically can that view be sustained. He has been led to the conclusion that the whole of these rocks from the borders of Sutherlandshire to those of Aberdeenshire represent only the higher portions of the great Caithness series, and that they were formed during a gradual depression of the ancient high grounds whereby the waters of Lake Orcadie were allowed to creep southward over the descending land. This movement is indicated by the character of the strata, and that it took place about the time of deposit of the later flagstones of Caithness is shown by the occurrence of the fossils of that division in the nodules, flags, and clays of the Moray Firth region, while those of the Lower division are absent.

Allusion is likewise made to the discovery of two localities where contemporaneous volcanic action has recently been observed in the Moray Firth area, the whole of the basin of Lake Orcadie being otherwise remarkably free from any trace of such action except on the northern margin in Shetland. The history of the area embraced by Lake Caledonia will form the subject of the next paper.

3. On Beats of Imperfect Harmonies. By Sir William Thomson.

According to a usage which has been adopted from the German of Helmholtz by the best English scientific writers on sound, a sound is called a "simple tone,"* or without qualification a "tone," when the variation of pressure of the air in the neighbourhood of the ear which is the immediate excitant of the sense is according to a simple harmonic function of the time; that is to say, when the whole pressure of the air varies in simple proportion to the distance, from a fixed plane, of a point moving uniformly in a circle. Considering the actual sensibility of the human ear to musical sounds, we must introduce farther as a practical restriction that the period of the variation of the pressure must be less then $\frac{1}{30}$ of a second, and greater than $\frac{1}{10000}$ or $\frac{1}{20000}$ of a second. The vibrations of the air produced by a simple harmonic vibrator are either simple harmonic, or are in circular or elliptic orbits, resulting from the composition of two simple harmonic motions; and the consequent change of air-pressure in the neighbourhood of the ear follows the simple harmonic law, provided the maximum velocity of the vibrator and of the air in its neighbourhood be infinitely small in comparison with the velocity of sound. Hence the more nearly this condition is fulfilled the more nearly a simple tone is the sound heard; but it is far from being fulfilled when the vibrator, though itself performing simple harmonic motion, has sharp edges round which the

* The old musical usage, according to which the word tone denotes an interval (the major tone or minor tone, or the mean tone of the tempered scale), though it unfortunately clashes with this recent scientific use of the word tone, can scarcely be abandoned.

air is forced to rush with great velocity, or when, as in the case of free-reed organ pipes or the reeds of a harmonium, the vibrator is an elastic solid moving to and fro in a very narrow aperture. (In the case of a slapping reed, as of trumpet stops in an organ, the motion of the vibrator itself is not simple harmonic, and the sound is excessively rich in overtones, giving it its peculiarly rich or harsh character.)

A harmony is any sound of which the excitant change of air-pressure is strictly periodic, and is not a simple tone. According to Fourier's beautiful analysis* of periodic variations, to which the name of the harmonic analysis has been given, any periodically varying quantity may be regarded as the sum of quantities varying separately according to the simple harmonic law, in periods respectively equal to the main period, half the main period, a third of the main period, and so on. According to this analysis we see that the variation of air-pressure constituting a harmony may be regarded as the sum of variations constituting simple tones, one having its period equal to the period of the harmony; a second, half that of the harmony; a third, one-third that of the harmony, and so on; in other words, we may regard the harmony as compounded of these simple tones.

Practically, in musical language the term harmony is not applied when the tone of the main period predominates in the sensory impression, and in this case the sound is simply called a note; its pitch is reckoned according to the main period; and the effect of the other tones, now called overtones, which enter into its composition, are merely felt as giving it its character or quality of sound. Thus the name harmony is in musical practice restricted to cases in which there is either no tone of the main or fundamental period, or not enough to produce a predominating impression, and a sound compounded of two, three, four, or more simple tones, having commensurable periods, is heard. In ordinary musical language a harmony is not regarded as having any one pitch, but is thought of as compounded of its known constituents. The true period of

* Compare "Trans. R.S.E." April 30th, 1860, "Reduction of Observations of Underground Temperature," where a short description of Fourier's analysis is to be found.

the harmony is, however, in every case the least common multiple of the period of its constituent tones. The number of times that the period of the harmony contains the period of any one of its constituent tones is called the harmonic number of that tone. This expression is only applicable to any particular tone when viewed as one constituent of a harmony. Following the usage of Lord Rayleigh and Professor Everett, I shall employ the word "frequency" to denote the number of periods per unit of time,—per second, let us say, generally in acoustical reckonings. Thus the "frequency" of a tone or of a harmony means the number of its periods per second. Similarly the frequency of any set of beats, according to the definitions and descriptions below, will mean the number of the beats per second, and in this application of the term it will designate sometimes a proper fraction, and sometimes a small whole number with fraction.

The quality of a harmony, when the periods of its several constituent tones are given, depends upon the amplitudes of the different constituents, and on the relation of their phases. Thus, for example, consider a harmony of two tones. They may be so related in phase that at one of the instants of maximum pressure of one of the constituents there is also maximum pressure of the other constituent. The same phase-relation, if the harmonic numbers of the constituent tones be both odd, will give also coincident minimums. But when one of the harmonic numbers is even and the other odd the phase-relation of coincident maximums will also be such that there is a coincidence of minimum pressure due to one tone with maximum pressure due to the other; and again there will be an opposite phase in which there will be coincidence of minimums, and in this opposite phase there will also be a coincidence of maximum and minimum. (To avoid circumlocutions a harmony of two odd numbers will be called an odd binary harmony; a harmony of even and odd numbers will be called an even binary harmony.) Thus we see that in an odd binary harmony there is a phase-relation of coincident maximums and coincident minimums, and again an opposite phase-relation of coincident maximum minimum and minimum maximum. The former will be called the phase-relation of coincidences, the latter the phase-relation of oppositions. In an even binary harmony there is a phase-relation of coincident maxi-

I.
II.
III.
IV.
I

I.
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IV.
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I.
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I.
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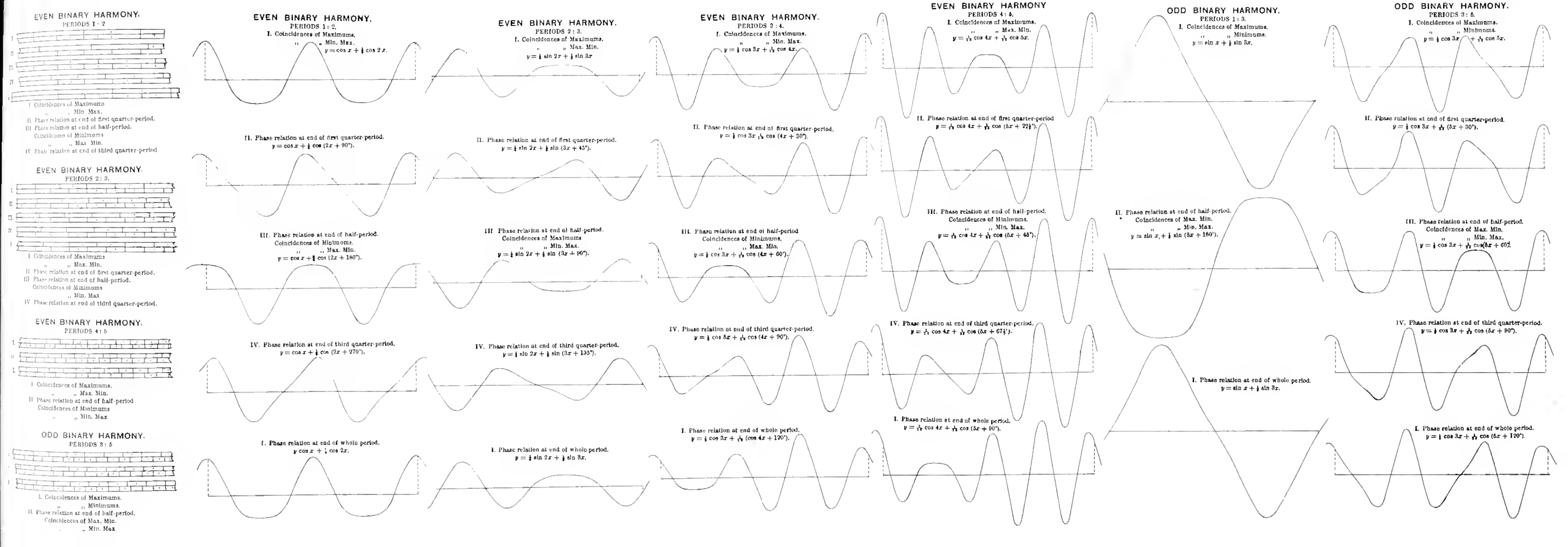

EVEN BINARY HARMONY.
PERIODS 1 : 2
I Coincidences of Maximums
" " Min. Max.
II Phase relation at end of first quarter-period.
III Phase relation at end of half-period.
Coincidences of Minimums
" " Max. Min.
IV Phase relation at end of third quarter-period
EVEN BINARY HARMONY.
PERIODS 2 : 3.
I Coincidences of Maximums
" " Max. Min.
II Phase relation at end of first quarter-period.
III Phase relation at end of half-period.
Coincidences of Minimums
" " Min. Max.
IV Phase relation at end of third quarter-period.
EVEN BINARY HARMONY.
PERIODS 4 : 5
I Coincidences of Maximums.
" " Max. Min.
II Phase relation at end of half-period
Coincidences of Minimums
" " Min. Max.
ODD BINARY HARMONY.
PERIODS 3 : 5
I. Coincidences of Maximums.
" " Minimums.
II. Phase relation at end of half-period.
Coincidences of Max. Min.
" " Min. Max.
EVEN BINARY HARMONY.
PERIODS 1 : 2.
I. Coincidences of Maximums.
" " Min. Max.
$y = \cos x + \frac{1}{2} \cos 2x$.
II. Phase relation at end of first quarter-period.
$y = \cos x + \frac{1}{2} \cos (2x + 90°)$.
III. Phase relation at end of half-period.
Coincidences of Minimums.
" " Max. Min.
$y = \cos x + \frac{1}{2} \cos (2x + 180°)$.
IV. Phase relation at end of third quarter-period.
$y = \cos x + \frac{1}{2} \cos (2x + 270°)$.
I. Phase relation at end of whole period.
$y \cos x + \frac{1}{2} \cos 2x$.
EVEN BINARY HARMONY.
PERIODS 2 : 3.
I. Coincidences of Maximums.
" " Max. Min.
$y = \frac{1}{2} \sin 2x + \frac{1}{3} \sin 3x$
II. Phase relation at end of first quarter-period.
$y = \frac{1}{2} \sin 2x + \frac{1}{3} \sin (3x + 45°)$.
III Phase relation at end of half-period.
Coincidences of Maximums
" " Min. Max.
$y = \frac{1}{2} \sin 2x + \frac{1}{3} \sin (3x + 90°)$.
IV. Phase relation at end of third quarter-period.
$y = \frac{1}{2} \sin 2x + \frac{1}{3} \sin (3x + 135°)$.
I. Phase relation at end of whole period.
$y = \frac{1}{2} \sin 2x + \frac{1}{3} \sin 3x$.
EVEN BINARY HARMONY.
PERIODS 3 : 4.
I. Coincidences of Maximums.
" " Min. Max.
$y = \frac{1}{9} \cos 3x + \frac{1}{16} \cos 4x$.
II. Phase relation at end of first quarter-period.
$y = \frac{1}{9} \cos 3x \frac{1}{16} \cos (4x + 30°)$.
III. Phase relation at end of half-period
Coincidences of Minimums.
" " Max. Min.
$y = \frac{1}{9} \cos 3x + \frac{1}{16} \cos (4x + 60°)$.
IV. Phase relation at end of third quarter-period.
$y = \frac{1}{9} \cos 3x + \frac{1}{16} \cos (4x + 90°)$.
I. Phase relation at end of whole period.
$y = \frac{1}{9} \cos 3x + \frac{1}{16} (\cos 4x + 120°)$.
EVEN BINARY HARMONY
PERIODS 4 : 5.
I. Coincidences of Maximums.
" " Max. Min.
$y = \frac{1}{16} \cos 4x + \frac{1}{25} \cos 5x$.
II. Phase relation at end of first quarter-period
$y = \frac{1}{16} \cos 4x + \frac{1}{25} \cos (5x + 22\frac{1}{2}°)$.
III. Phase relation at end of half-period.
Coincidences of Minimums.
" " Min. Max.
$y = \frac{1}{16} \cos 4x + \frac{1}{25} \cos (5x + 45°)$.
IV. Phase relation at end of third quarter-period.
$y = \frac{1}{16} \cos 4x + \frac{1}{25} \cos (5x + 67\frac{1}{2}°)$.
I. Phase relation at end of whole period.
$y = \frac{1}{16} \cos 4x + \frac{1}{25} \cos (5x + 90°)$.
ODD BINARY HARMONY.
PERIODS 1 : 3.
I. Coincidences of Maximums.
" " Minimums.
$y = \sin x + \frac{1}{9} \sin 3x$.
II. Phase relation at end of half-period.
Coincidences of Max. Min.
" " Min. Max.
$y = \sin x + \frac{1}{9} \sin (3x + 180°)$.
I. Phase relation at end of whole period.
$y = \sin x + \frac{1}{9} \sin 3x$.
ODD BINARY HARMONY.
PERIODS 3 : 5.
I. Coincidences of Maximums.
" " Minimums.
$y = \frac{1}{9} \cos 3x + \frac{1}{25} \cos 5x$.
II. Phase relation at end of first quarter-period.
$y = \frac{1}{9} \cos 3x + \frac{1}{25} (5x + 30°)$.
III. Phase relation at end of half-period.
Coincidences of Max. Min.
" " Min. Max.
$y = \frac{1}{9} \cos 3x + \frac{1}{25} \cos (5x + 60°)$.
IV. Phase relation at end of third quarter-period.
$y = \frac{1}{9} \cos 3x + \frac{1}{25} \cos (5x + 90°)$.
I. Phase relation at end of whole period.
$y = \frac{1}{9} \cos 3x + \frac{1}{25} \cos (5x + 120°)$.

mums and coincident maximum minimum; and again an opposite phase-relation of coincident maximum minimum and coincident minimums. The former will be called the phase-relation of coincident maximums, the latter the phase-relation of coincident minimums. The annexed diagrams illustrate and prove these assertions. The horizontal line in each may be either regarded as representing space at any one instant in the direction of propagation of the sound, or it may represent times at which successive phases of the motion are perceived by an ear in a fixed position. The long vertical cross-bar in each case denotes maximum of air-pressure; the short vertical cross-bar, minimum.

For lecture illustrations it is convenient to use long slips of wood with paper pasted on one side, and the short and long cross-bars marked upon it, and to support these slips of wood on a board with nails to guide them so that they may be placed in groups of two, three, or four, one over the other, and any of them moved in the direction of its length to illustrate the different phase-relations of a harmony.

Suppose now, one note of a perfect binary harmony to be very slightly sharpened or flattened: so slightly that during a large number of the periods of the perfect harmony, the phase-relation in the imperfect harmony experiences but little change. Let the two notes of the imperfect harmony be sustained long enough with perfect uniformity as to pitch and intensity:—the effect will be that of perfect harmony, modified by a slow change of its phase-relation through a cycle; which in the case of an even binary harmony is from coincident maximums gradually to coincident minimums, and thence gradually round again to coincident minimums; and in the case of an odd binary harmony is from oppositions to coincidences, and round to oppositions again; and so on in cycles. In favourable circumstances, and with careful attention, a variation of the quality of the sound recurring periodically in these successive cycles is distinctly heard, even by an unpractised ear, unless the duration of the cycle be too long or too short to suit its sensibility. It is this variation which is called the "beat" on the imperfect harmony.

The period of the beat—that is to say, the duration of the cycle described above—is most easily found by taking the reciprocal of its frequency, calculated by the following rule:—*The frequency of the* *beat is equal to the error of frequency of one note multiplied by the harmonic number of the other.* When in a harmony of three or four notes all are perfect except one, the beats due to the imperfection of the false one are to be reckoned just as if the harmony were binary, according to the following rule :—

For the two or more notes which are in perfect harmony imagine one whose period is the period of their harmony. Take this as if it were one tone of an approximate binary harmony, the false note of the given harmony being the other. Example: Let the frequencies of the three notes be 257, 320, and 384 : the common period of the two last-mentioned is $\frac{1}{64}$ of a second, and we have to calculate the beats on two notes whose frequencies are 64 and 257. The harmonic numbers of the harmonies to which these notes approximate are 1 and 4, and the error in frequency of the higher note is 1 per second; hence the beats are at the rate of 1 per second. When there is error in two or more notes of a multiple harmony, two or more sets of beats in periods not commensurable with one another are heard; but the general effect is apt to be too confused to allow any one of the sets to be distinctly counted. On a multiple harmony with only one note false the beats are in general exceedingly distinct, more so in general than in binary harmonies.

Sometimes, as for distance in reckoning the beats in the imperfect harmonies of a tempered musical scale, it is convenient to regard the two notes of an imperfect harmony as in error from two notes of a perfect harmony differing but little from them; then the rule for calculating the frequency of the beats is to take the difference of the products of the errors of the two notes, each multiplied into the harmonic number of the other. Thus, let n and n' be the harmonic numbers of the perfect harmony to which the given notes approximate, and let e and e' be the excesses of the vibrational frequencies of the two actual notes above two in perfect harmony nearly agreeing with them. The frequency of the beat of the actual notes is $n'e - ne'$.

For example, take the following table of numbers of vibrations in a perfect diatonic scale, with 256 vibrations per second for C, and in the corresponding scale of equal temperament (founded on 12 equal semitones, in each of which the interval ratio is $2^{\frac{1}{12}}$) :—

Frequencies of True.		Frequencies of Tempered.		Error of Tempered.
C	256	256	C	0·0
D	286·75	287·35	𝕯	+0·60
E	320	322·54	𝕰	+2·54
F	341·33	341·72	𝕱	+0·39
G	384	383·57	𝕲	−0·43
A	426·67	430·54	𝕬	+3·87
B	480	483·26	𝕭	+3·26
C′	512	512	C′	0·0
D′	573·50	574·70	𝕯′	+1·20
E′	640	645·08	𝕰′	+5·08

From these numbers we find the following table of beats :—

Perfect Harmonies.	Harmonic Numbers.	Imperfect Harmonies.	Errors.	Qualities of Falseness of the Intervals.	Frequencies of Beats.
G / C	3 / 2	𝕲 / C	− 0·43 / 0·0	Too small	0·86
B / E	3 / 2	𝕭 / 𝕰	+ 3·26 / + 2·54	Too small	1·10
C′ / F	3 / 2	C′ / 𝕱	0·0 / + 0·39	Too small	1·17
E′ / A	3 / 2	𝕰′ / 𝕬	+ 5·08 / + 3·87	Too small	1·45
F / C	4 / 3	𝕱 / C	+ 0·39 / 0·0	Too large	1·17
A / E	4 / 3	𝕬 / 𝕰	+ 3·87 / + 2·54	Too large	1·45
E′ / B	4 / 3	𝕰′ / 𝕭	+ 5·08 / + 3·26	Too large	2·20
E / C	5 / 4	𝕰 / C	+ 2·54 / 0·0	Too large	10·16
A / F	5 / 4	𝕬 / 𝕱	+ 3·87 / + 0·39	Too large	13·53

Perfect Harmonies.	Harmonic Numbers.	Imperfect Harmonies.		Qualities of Falseness of the Intervals.	Frequencies of Beats.
			Errors.		
G E	6 5	𝔊 𝔈	− 0·43 + 2·54	Too small	17·39
G E	6 5	G 𝔈	0·0 + 2·54	Too small	15·24
C′ A	6 5	C′ 𝔄	0·0 + 3·87	Too small	23·22
A C	5 3	𝔄 C	+ 3·87 0·0	Too large	11·61
E′ G	5 3	𝔈′ 𝔊	+ 5·08 − 0·43	Too large	17·39
E G C	5 2	𝔈 G C	+ 2·54 0·0 0·0	Too large	5·08

It is of course to be understood that the degree of falseness is the same in all the tempered harmonies of the same name (or having the same harmonic numbers); and that the different numbers shown for the frequencies of the beats are (except for the case of the 𝔈 with the untempered G) in simple proportion to the vibrational frequencies of one or other of the constituent notes. The slightness of the imperfectness in the tempered fifth (approximately 2 : 3) is indicated by the slowness of the beats, not so much as one per second on the C 𝔊. The imperfectness of the fourth (approximately 3 : 4) is even less than that of the tempered fifth, so that, notwithstanding the greater harmonic numbers, the beats are scarcely more rapid (1·17) for the C 𝔉 than (·86) for the C 𝔊. But when we go to major and minor thirds of the tempered scale, we take leave of mathematical harmony entirely. The beats on the C 𝔈 (ten per second) are too rapid to be counted, and it is only in virtue of their not being perceived, or not being disagreeably perceived, that the combination is agreeable. The same may be said still more unqualifiedly of the minor thirds, the number of beats on 𝔈 𝔊 being more than seventeen per second. It does not seem easy to explain on any physical or physiological principles the decidedly agreeable effect produced on the ear by a succession of major and minor thirds of pianoforte notes. It is, no doubt, to the slowing of the beats by the superposition of a third note upon either of the binaries C $\mathfrak{E}$ or $\mathfrak{E}$ $\mathfrak{G}$ in the ternary combination C $\mathfrak{E}$ $\mathfrak{G}$, because of its comparatively close approximation to $\overset{\frown}{\text{C G}}$ $\mathfrak{E}$ (for which the beats are only five per second), that the comparatively smooth harmoniousness of the common chord in the tempered scale is due.

It is not generally known how easily beats on approximations to other harmonies than unison are heard, even when the constituent notes are simple tones. Through the kindness of Professor M'Kendrick I have been allowed the means of testing them in very varied combinations, by aid of a series of excellent tuning-forks of Koenig's, each mounted on a wooden box resonator, after the manner of Marloye. For such experiments Koenig's tuning-forks are much superior to Marloye's, because of the greater quantity of metal in each fork, in virtue of which it gives a louder and more enduring sound. The sound proceeding from such a source is essentially a simple tone, or very nearly so. I have tested that in every case the number of beats counted is the smallest that could be according to the preceding theory; for it is to be remarked that the theory only gives the whole period of the phenomenon, but does not answer the question—Does the ear perceive a gradual variation of quality through the whole period, or does it fail to distinguish the difference of quality between two halves of the period, or between three-thirds of it, and so on? My experiments demonstrate that in every case the ear does distinguish the two halves of the period of each beat. Thus, for example, in the beat on an approximation to the harmony (1 : 2) in which the variation of air-pressure on the ear is represented by the preceding curves for four instants of the period noted, I find that the ear distinguishes the quality of the sound represented by the sharp-topped and flat-hollowed curve from that represented by the flat-topped and sharp-hollowed curve. In the one case the pressure of air close to the ear rises very suddenly to, and falls very suddenly from, its maximum, and (as in cases of tides in which there is a long hanging on low water) there is a comparatively slow variation of pressure for a few ten-thousandths of a second on each side of the instant of minimum pressure; in the opposite phase-relation there is a slow change before and after the time of maximum pressure, and a rapid change before and after the time of minimum pressure. In the former case the difference of maximum from mean exceeds the difference of minimum from mean; in the latter the difference of the minimum from mean exceeds the difference of maximum from mean. If the ear could not distinguish between two such sounds, but could distinguish between either of them and the sounds represented by the first and third quarter-period curves, the number of beats would be twice the error of frequency of the higher note. But I find that the number of beats is quite distinctly equal to the error of frequency of the higher note. I have found that the beats on the 1 : 2 harmony are most easily perceived when the intensity of the higher note is comparatively faint, as would be the case if they were explained by Helmholtz's theory that they are the beats on the approximation to unison which there is between the higher note and the first overtone of the lower note. But the simple-harmonic character of the two constituent tones at the entrance of the ear precludes the acceptance of this theory unless extended, as it has actually been by its author, to the interior mechanism of the ear.

Whatever may be the physiological theory by which the beats are to be explained, it is an interesting fact that the ear does distinguish, as it were, between push and pull on the tympanum in the manner illustrated by the preceding curves, not only for the case of approximation to the harmony 1 : 2, but for every other even binary harmony. I have heard distinctly the beats on approximations to each of the harmonies 2 : 3, 3 : 4, 4 : 5, 5 : 6, 6 : 7, 7 : 8, 1 : 3, and 3 : 5. The two last mentioned, though sometimes less easily heard than the beats on most of the others, are unmistakably distinct; and by counting the numbers of them in ten seconds or in twenty seconds, I have ascertained that they, as do all the others, fulfil the condition of having the whole period of the imperfection, and not any sub-multiple of it for their period of audible beat. They are interesting as being cases of odd binary harmonies. Before making the experiments, I thought it possible that what is heard in the beat might not make distinction between the configuration II. and IV. (first quarter phase and third quarter phase): but a *revolving character* which I perceive in the beat seems to me distinct enough to prove that the ear does distinguish between these configurations, which are one of them the same as the other taken in the reverse order of time.

In every instance except the octave, the beat on the approximation to a binary harmony is less distinct than the beat on an approximation to a ternary or higher multiple harmony with only one note false. It is not because of the comparative slowness of the beat on the multiple harmony; for by taking alternately beats with one note slightly false in a binary harmony, and the same note made more false in a ternary or multiple harmony to such a degree as to give the same number of beats, I have always found the beats in the latter case much more prominent than in the former. Thus by taking first the perfect harmony C E G (4, 5, 6), and the three binary harmonies C G (2 : 3), C E (4 : 5), E G (5 : 6), and flattening slightly any one of the three notes by screwing on a small mass of brass to either or to each prong of the tuning-fork producing it, it is easy after a little practice to count the beats on each of the binary harmonies. Thus, for example (supposing $E_{,}$ to designate a note of a slightly lower pitch than E), after a little practice it is easy to count the beats on C $E_{,}$ and on the $E_{,}$ G, and to verify that their frequencies are, the first of them four times, and the second of them six times the error of frequency of the $E_{,}$, and then to verify that the much louder beats on the ternary harmony C $E_{,}$ G, are of half the frequency of the former, and of one-third of the frequency of the latter, and to verify absolutely that they are of twice the frequency of the error of $E_{,}$.

If when the approximate harmony C $E_{,}$ is being sounded, the faintest sound of G is produced by a very gentle excitation of the fork by the bow, instantly a loud beat at half speed is heard. The phenomenon is rendered very striking by alternately touching the top of the G fork by the bow so as to stop its vibrations, and then drawing the bow very gently for a fraction of a second* along one side to re-excite them. It is marvellous how small an intensity of the sound G is required to give a smooth unbroken loud beat in the double period. I have found it difficult to excite

* In every case, to obtain regular beats, each tuning-fork, after being set in vibration by the bow, must be left to itself. The sound is sensibly graver as long as the bow is applied to augment or sustain the vibration than when the fork is left free. Thus, if two tuning-forks nearly, but not quite, in unison, are alternately acted on by the bow and left free, the beats are less rapid during the time the bow is applied to the higher fork, and more rapid while to the lower, than when both forks are vibrating freely.

the G gently enough to give the gradual transition from, let us say for example, four uniform beats per second, through the case of four beats per second with every alternate beat somewhat louder, to the case of only every second beat perceptible, or, in all, two beats per second; but it can be done, and the result is an interesting and instructive illustration of the slowing down from the quick beat of the binary harmony to half speed, or to one-third speed, or to one-fifth speed, as the case may be, by the introduction of a third note. In the several cases I have foundthat I can, by making the added note faint enough, produce a succession of beats of which every second, or every third, or every fifth, as the case may be, is louder than the others, and that, as the intensity of the added note is gradually increased, the fainter beats become imperceptible, and a regular unbroken slow beat is heard distinctly alone, always in the theoretical time of the whole imperfection of the harmony. I have verified this distinctly in the cases of 1, 2, 3; 2, 3, 4; 3, 4, 5; 4, 5, 6 (as stated above); 5, 6, 7; and 6, 7, 8. I have not succeeded in hearing the beats on the approximations to the harmonies 8 : 9 and 9 : 10. But the slow beat on the 8, 9, 10 (with vibrational frequencies 256, 288, 320), with any one of the three notes slightly flattened, is very remarkable. The sound is like that of a wheel going round with decided roughness of motion in every part of its revolution, but much rougher in one part than another, with a loudly perceptible periodic return of the roughness in the theoretical period of the approximate harmony.

The beats on the harmony C E G (vibrational frequencies 256, 320, 384), with any one of the three notes slightly flattened, are very perceptible: untrained ears hear them instantly the first time without any education, and the beat is heard almost to the very end of the sound if three of Koenig's forks, one of them, the C, for example, being slightly flattened by a brass sliding piece screwed to it, be caused to sound. The sound dies beating, the beats being distinctly heard all through a large room as long as the faintest breath of the sound is perceptible. The smooth melodious periodic moaning of the beat is particularly beautiful when the beat is slow (at the rate, for instance, of one beat in two seconds or thereabouts), being, in fact, sometimes the very last sound heard when the intensities of the three notes chance at the end to be suitably proportioned.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. IX. 1877–78. No. 102.

Ninety-Fifth Session.

Monday, 15th April 1878.

Sir WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. On Vortex Vibrations, and on Instability of Vortex Motions. By Sir William Thomson.

2. On the Theory of Vowel Sounds. By Professor M'Kendrick.

3. Preliminary Note on a Method of Detecting Fire-Damp in Coal Mines. By Professor George Forbes.

The author exhibited two instruments, both founded upon the same principles, for measuring the quantity of fire-damp present in a coal mine. The first instrument consists of a tuning-fork fixed above the open end of a resonating tube, whose other end is closed by a piston whose position (read off on a scale) regulates the length of the resonating tube. The length of the tube, which resounds to the definite pitch of the tuning-fork, depends upon the nature of the gas with which it is filled. The more fire-damp, the longer is the tube. Barometric pressure has no effect upon this instrument. The correction for temperature is made by reading off, not a fixed mark upon the piston, but the top of the mercury of a thermo meter attached thereto, of dimensions determined by actual experiment. The only source of error to which the instrument seems liable is the counteracting influence of dense carbonic acid gas in choke-damp. But it is found that the presence of choke-damp destroys the explosive character of fire-damp; and, so far as experiments go, it seems certain that, in all cases when the presence of choke-damp prevents the instrument from indicating the presence of fire-damp, the fire-damp is denuded of its explosive character.

The second instrument is a combination of a harmonium reed and an organ pipe, through which the air is driven. They are arranged so as to sound the same note when pure air is used, so that when there is a lighter gas present the organ pipe sounds a higher note, thus producing beats.

So far as the experiments have gone hitherto, the first form is by far the most accurate, being capable of detecting the presence of 1 or 2 per cent. of fire-damp.

4. Note on Electrolytic Conduction. By Professor Tait.

It is commonly said that there is a resistance to a current at the surface of contact of a solid conductor and an electrolyte. Some good authorities, however, say that we have as yet no proof of this, as the effects observed may be due to polarisation. It is obvious that, if the reverse electromotive force due to polarisation contain a term directly proportional to the strength of the current, the ordinary methods of measurement would not enable us to distinguish this from the surface resistance above mentioned. For, in the expression

$$I = \frac{\Sigma(E)}{\Sigma(R)},$$

if the numerator contain a term of the form $-eI$, it may be expunged, provided e be added to the denominator.

To clear up this point I have recently made a number of experiments. These have led me to some curious results bearing on the theory of electrolysis, which I propose to bring before the Society on a future occasion. At present I refer to them merely so far as to say that they establish fully the existence of the surface resistance above mentioned. Thus I was led to see that if a slip of platinum be inserted between the electrodes of a decomposing cell it ought, except in extreme cases, to produce almost precisely the same result as a similar and equal slip of glass or mica. This was easily verified. Here we have the singular result of a marked diminution of the current by the insertion into the electrolyte of a substance which is in itself a much superior conductor. Even when the platinum completely closes the path from one electrode to the other, so as to form two decomposing cells instead of one, a comparatively small hole made in it at once changes its function from that of common electrode to each of two decomposing cells into that of a mere *obstruction* in one cell. It is an interesting experimental inquiry to trace the intermediate stages between these two states, as a pinhole in the platinum is gradually enlarged. Whatever, then, be the behaviour of the particles of an electrolyte, they do *not* behave like little pieces of platinum.

5. Note on Thermal Conduction. By Professor Tait.

Monday, 6th May 1878.

Sir C. WYVILLE THOMSON, Vice-President, in the Chair.

The following Communications were read :—

1. On the Indications of Molecular Action in the Telephone. By R. M. Ferguson, Ph.D.

The accepted theory of the telephone represents that the vibrations of the sending plate to and from the pole of the magnet before which it is fixed is the origin of the currents generated in the pole bobbin of wire, and that these currents transmitted to the receiving telephone produce corresponding to-and-fro excursions of its plate. This theory, which is that of the inventor, may be shortly designated, in the happy words of Sir William Thomson for a kindred action, the push-and-pull theory. We have had in this session of the Society two communications of a practical nature, which seem directly confirmatory of this view. I refer to the lucid exposition of Gott's telephone experiment in the island of St Pierre, and the beautiful and successful demonstration of the action of the phonograph, both by Professor Fleeming Jenkin. In the former of these, as we learned, one end of a thread was attached to the one side of the light suspended coil of a Thomson ink recorder, and the other to the paper disc of an ordinary mechanical telephone. This was done at the two communicating stations. When the sending disc was agitated by the voice, the coil to which it was attached twisted round in the powerful and uniform magnetic field in which it was placed, and dispatched corresponding electric current waves to the receiving instrument, the coil of which was thereby moved similarly in its field, and transferred its motion to its paper disc. A more beautiful manipulation of an exquisitely designed and executed apparatus it is not easy to conceive. In the phonograph we have as it were a mechanical telephone, with the string connecting the discs cut, and nothing left of it but the two ends stiffened into pricking pins. Instead of the sending disc dealing directly with the receiving one, its energy is employed in imprinting, by means of the pricker, its vibrations on the tinfoil, and this imprint, when again vivified by the energy of the rotating drum, reproduces the vibrations which originally stamped it.

After two such demonstrations, it may be held as proved that the electric telephone is equivalent to a mechanical telephone with an electro-magnetic intervening action instead of a mechanical one. It seems therefore a hopeless task to seek for indications of molecular action where mechanical action declares itself so manifestly. The mechanical action of the voice and of the membrane of the tympanum of the ear is above question, and that mechanical vibrations are dealt to the sending instrument, and emitted by the receiving one, is equally undoubted; but the intervening electric agency, how generated in the one and how transformed in the other, is a fair field for discussion. The action is novel, and it is surely a likely inquiry to investigate whether its explanation by the first principle that comes to hand, viz., the push-and-pull of the discs, fully covers the case. The question may be raised, for instance, whether the mere impact of the waves of air on the iron disc may not affect its magnetic condition by internal change or vibration,* so as to excite currents without vibrations of the push-and-pull kind, or whether in

* Something like this was suggested by Professor Forbes.

the receiving disc the particles may not set up an action on their own account, independently of the displacement caused by the poles of the adjoining magnet. In a mechanical telephone, we do not find that it is made to sound only by the normal push or pull of the thread, the faintest rubbing on the irregularities of its surface, either on the disc or the tube to which it is attached, makes a sound loud enough to be heard, and we can easily admit that if an internal vibratory disturbance be set up in any direction in it, the same would be audible enough. In a discussion as to mechanical and molecular sounds, it may be safely admitted, where electricity or magnetism is concerned, that any action that is clearly traceable to disturbance within a body is molecular in its origin. It will, moreover, be granted that the mere smallness of any vibration does not necessarily give any clue to its origin. Infinitesimal vibrations are not necessarily molecular, nor are vibrations of molecular source free from external motion; and we can only say that a vibration comes from molecules if we can assign to it no outside cause. It may, however, be to the point that a vibration may be assumed to be molecular because of the difficulty in suppressing it, a vibration springing from within being more independent of direction than one produced from without from one quarter.

I propose in this communication to raise such questions in regard to the telephone, and though the results obtained may not be decisive, they may be some little contribution to the discussion.

I would begin with a case where internal action seems wholly absent. I refer to the action of a tuning-fork on the telephone. It has been mentioned in more than one communication to the Society, that a tuning-fork acts best without the disc. We find that the loudest sounds are sent to the listener at the receiving telephone, when one prong is brought with its flat vibrating end in front of the core or pole pin, and next to that when the prongs, if they are not too far apart, are laid with their flat sides vertical at an equal distance on each side of the pin. When the handle of the fork is laid on the core, and held upright, the resonance of the wooden frame of the telephone and the table on which it rests becomes loud, but only a faint trace of this is sent to the distant hearer. If we magnetise two like forks, one which we may call A, to be like a bar magnet having the end of the handle as one pole, and the other pole split in two in the two prongs; and another fork B, the two prongs of which are made like the poles of a horse-shoe magnet, with the handle an excrescence between, we find that while the fork A produces sounds alike with both prongs when held near the core, the two prongs of the fork B show a marked difference. The like pole to that of the core sounds much weaker than the other. All this is indicative of the ordinary magneto-electric induction at work.

If we detach the coil from the magnet, we have still further illustrations of the same. Both forks, A and B, sound loudest when placed with one prong on its flat side over the hollow at the centre (fig. 1), and both continue to sound, but with diminished force, as they are withdrawn in the same position from the middle to the margin of the coil. When laid with the plane of the prongs horizontal (fig. 2), they act differently. The A fork has its best sounding position when each prong lies symmetrically to the hollow axis, and it has a position of silence at a point between the middle and

Fig. 1.

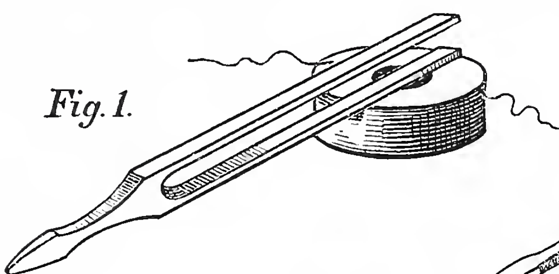


Fig. 2.

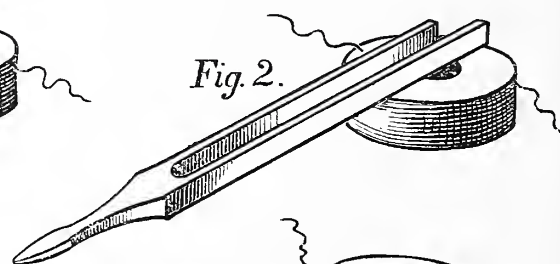


Fig. 3.

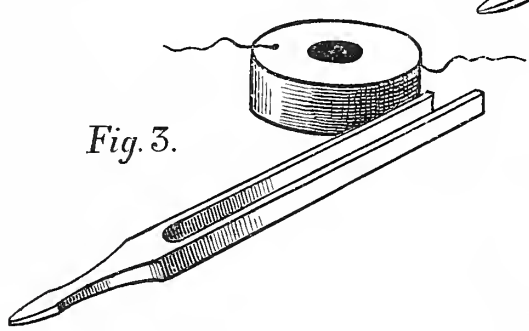
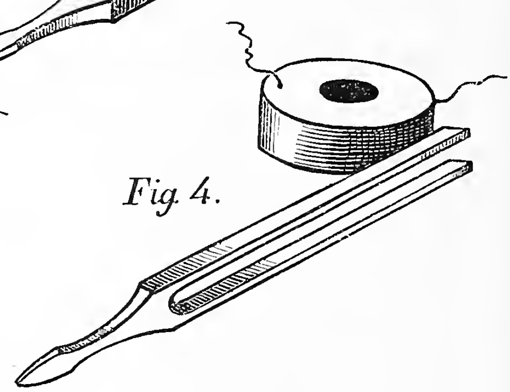


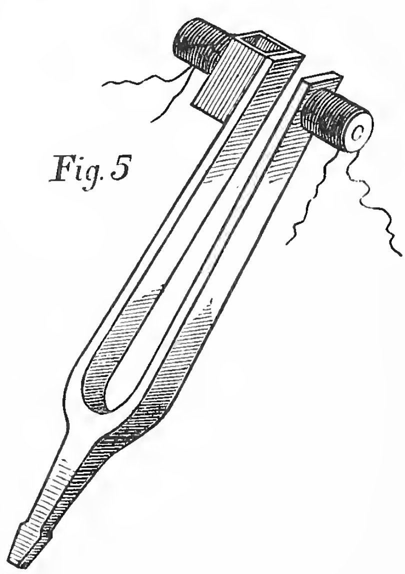
Fig. 4.



the outside, whilst the B fork in these positions acts in the opposite way. There are two positions that the forks may occupy at the side of the coils, where their similar and dissimilar actions are again shown. The first is when the plane of the fork is perpendicular to the axis (fig. 3), where both forks transmit no sound when held in the middle of the coil, but are heard when vibrating on either side. The second is when the plane is parallel to the axis (fig. 4), where A is silent when its prongs are equidistant from the middle of the coil, and the fork B loudest. All this is, as we have said, simply an illustration of a well-known action, and at the same time a beautiful demonstration of the way in which a tuning-fork vibrates. The coils I used were of fine copper wire, $1\frac{1}{2}$ inch diameter and $\frac{3}{4}$ thick, but smaller coils would do equally well, and the forks were the ordinary small A and C forks sold by the musicsellers. It is perhaps worthy of note that a coil, a magnetised fork, and a telephone form a handy combination for testing the completeness of a circuit, as the sound of the fork coming directly to the ear is immensely below that heard in the telephone in the operator's hand. When the telephone does not sound, there is a break in the circuit. In these various performances of the fork, we have evidence enough to prove that the cause assigned by Bell for the sending action of the telephone covers at least the greater part of that action. At the same time, it must be borne in mind that the vibrations of the fork, and the sounds produced by them, are immensely greater than any connected with the telephonic effect of the voice, and that it is possible that the conditions of iron vibrating under the energy treasured up in it may be different from what they may be when the iron is beaten by the air.

But even this tuning-fork performance is not quite free from ambiguity. To find whether there might not be some change of magnetic condition due to internal vibration, able to generate currents, I cemented two coils (fig. 5.) to the vibrating ends of a large tuning-fork. It was a C (256 vibrations), with prongs upwards of 6 inches long, $\frac{7}{8}$ broad, and more in average than $\frac{1}{4}$ thick. The distance between the prongs at the end inside was $\frac{7}{8}$ inch. The coils weighed $\frac{1}{2}$ oz.; they were $\frac{3}{4}$ inch diameter, 1 inch long, and were of ·007 inch copper wire. They reduced the pitch from C to A. The cement was the hard and tough black tarry compound

Fig. 5



used by electricians. On connecting one of the coils with a telephone and making the fork vibrate, I was astonished to hear the sound in the telephone astonishingly loud. I found, however, that this arose from the connecting wires, which though loose were able mechanically to transmit the vibrations of the fork. This was shown by leaving only one wire in the connection, when the sound of the fork was still heard. It is well known that the thread of a mechanical telephone delivers its message to the first fixed obstacle it meets and no further, and in this case, when the lines were held in the fingers, a mere residuum of sound was heard, which could only be properly estimated in a distant room away from the direct sound of the fork. In this, as in all subsequent experiments, I took the utmost care that no mechanical transmission was possible, and the telephone used at the operating station was put out of circuit each time a sound was made for investigation. Loud tapping was made in every conceivable place to ascertain if such could be transmitted in a mechanical way. The circuit was about 150 ft. of wire with a gas pipe return, and the two stations were in different buildings. The results obtained from this fork were not of any value other than illustrating the difficulty of insulating for transmission what may be looked upon as an internal and not an external vibration. They were these. The sound of the fork was heard much less loud than if the prong had been vibrating in front of the coils, but was louder than what was got when it vibrated at a distance equal to that of the prong on which the coil was not fixed. When the two coils were joined up consecutively, one way of joining gave a maximum, the other a minimum sound. In another arrangement the coil was attached to a hollow prism of thin brass $\frac{5}{8}$ inch inside, which in its turn was attached to the prong. The sound given by this coil was less than when it was attached immediately to the prong, but was louder than when the other prong vibrated freely at the same distance. When I mounted the fork in this way, one prong was feebly magnetised, and the other scarcely if at all.

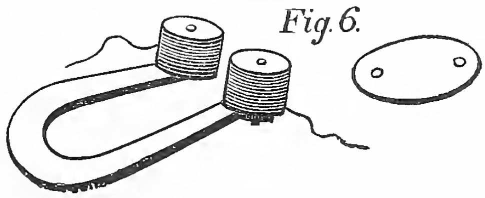
A perfect maze of reasons may turn up to account for the currents dispatched by these fixed coils. One might be their motion in the magnetic field of the earth, which is not likely, for earth-induced currents proceed from the rotation of a coil, and the faint approach to rotation in this case can hardly be looked upon as such. To exclude this cause, I put the coils in the best position for generating such currents, and connected them with a moderately sensitive reflecting galvanometer. I inserted a piece of wood between the prongs, and wedge-like drove them suddenly out, and allowed them to return, keeping time with the known pendulum-like oscillation of the mirror so as to accumulate effect, but no result was got. Again it might be that the coils, tightly as they were wound, were yielding, and we had only a case of shaking a coil in front of a magnet, instead of a magnet in front of it. That this was not the case was proved by cementing the coils to the sides of the prongs at right angles to the direction of motion, and though the forks vibrated almost unmusically with such protuberances, yet what sound they emitted was telegraphed to the listener. The most likely cause was that the coils were fixed as regarded the prongs on which they were stuck, but were affected by the distant prong which vibrated in front of them. The sounds of the vibrating coils were louder than when the coils were fixed, and the other prongs vibrated at the same distance; but that might be accounted for by the fact that the prong to which the coil was fixed was an active agent in propagating the vibrations of its companion prong to its own coil. The only chance of anything like internal action being heard here rests on the possibility that this last is not sufficient, and that is certainly a slender enough basis to build a theory upon.

In pursuit of the same internal sending action, I cemented two coils, one to the end, the other to the middle of a bar magnet. Both transmitted telephonic currents when the magnet was struck, the end one much louder than the other. Again, an unmagnetised tuning-fork was made to vibrate above a coil, and its note was feebly yet distinctly heard at the listening end. To test whether it and the other objects afterwards mentioned were magnetised, I brought them near a small active magnet about an inch in length, and before another much heavier, 9 inches in length, and watched to see if repulsion or even indifference was shown to any part of them; and lastly, I made them vibrate in front of the coil connected with the reflecting galvanometer in the position in which their sending action was afterwards tested. These tests cannot be alleged to prove complete absence of magnetism, but they go far beyond the practical application of the word unmagnetic. The fork in question answered perfectly all these tests. I next tried to get sounds out of soft iron plates kept fixed above the coil, and found no difficulty in getting them to sound when laid on a table in a horizontal position and beaten with a small hard wooden mallet, or with something that would produce with them a sharp tap. When the plates were held approximately in the plane of the magnetic meridian there was more difficulty in getting the distant listener to hear, but even in it they did not cease to sound under blows sharp and hard enough. They also sounded when the coils were cemented to them, but the hearer had his powers taxed to catch the effect. Soft iron pins placed in the hollow axis of the coil also sounded under similar treatment. I could not succeed, however, in getting a soft iron pin perfectly to answer the galvanometer test, ordinary nails and similar iron pins always caused a slight deflection, which became more marked by pendulum accumulation. Even a soft iron pin, as pure as I could get, after being exposed for half an hour to a white heat, on being made to move at right angles to the dipping needle, at least so nearly as this position could be given to it by the hand, indicated through its coil faint traces of magnetism. I had some difficulty in getting taps out of it for the listening station, but I succeeded at last by using another similar pin held in the same line as a mallet. Iron thus softened seems to lose its telephonic power more by absence of elasticity than by being less magnetic, for the said pin seldom failed in any position before being annealed, and so far as tests went it was not a bit more magnetic. A somewhat curious result was got by cementing a small coil to a strip of ferrotype plate quite near to the edge, and making that edge strike on the teeth of a syren wheel, made to revolve on a turning table, when the screeching note thus produced was distinctly telephoned. When there was any slight indication of magnetism, such as was given by the spot of light shifting even a millimetre for one quick motion, the taps were rendered with great certainty. The soft iron pins just mentioned, when put on the pole of a magnet, and very gently tapped with a common pencil, made themselves distinctly heard. This can be done in the case of any telephone on removing the disc.

Now, it may be asked, are all such sound-exciting currents to be traced to the shifting of magnetic matter in a magnetic field? or is there not ground for believing that a blow for the instant disturbs the magnetic condition of a magnet, and magnetises soft iron, in consequence of a disturbance of the molecules, enough to excite telephonic currents? And may not such magnetic changes act without the displacement in the field that generally or perhaps necessarily accompanies them, and heightens their effect? Wiedemann has shown that blows alone given at right angles to the magnetic needle, and consequently apart from the earth's magnetism, produce permanent magnetic changes, and that torsion and change of external conformation may do the same. The permanent effect also of blows in lessening the powers of a magnet, or of giving to iron under induction a magnetic set, has long been known. Now, may not the telephone reveal to us that they in every case produce a momentary effect of an intensity in some degree proportionate to the magnetisation of the iron?

The following telephonic arrangement, which is different from that of Bell's, may possibly have some bearing in the view just suggested; but knowing, from the instances discussed, the ambiguity that attends a departure from the push-and-pull theory, I quote it more as a problem for push-and-pull solution, than as a proof of blow-

Fig. 6.

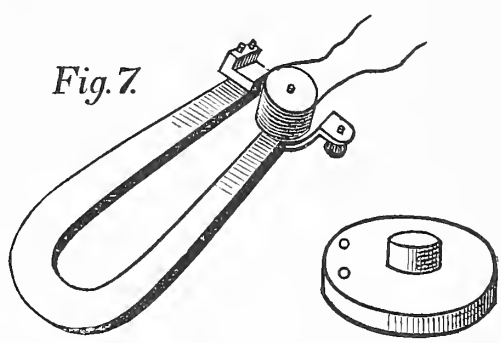


induced effects. The apparatus (fig. 6), consists of a horse-shoe magnet rather more than 5 inches long, with a pin on each pole standing at right angles to the plane of the magnet. The pins are provided each with a coil of fine copper wire, the two being joined up as in an electro-magnet. The pole pins are $1\frac{1}{2}$ inch apart, and a stiff disc (No. 28), 3 inches diameter of untempered steel, is screwed to them by large-headed screws. Such a disc, when aided by a wooden or non-metallic resonating box, can both convey and receive an articulate message, indistinctly certainly, but still it does it. When it is joined up with the galvanometer, and tapped with a pencil on any part, taking care of course not to combine the striking and lifting motions, no visible deflection is got, though the same taps on the disc of a Bell telephone make deflections of from 5 to 10 mm. of the scale. When the disc is taken off, we may ascertain the efficacy of vibrations in the line of the plate to generate currents. Taking a small thin iron rod and giving it small vertical excursions, I found that when these were made in the line of the plate no effect whatever was got when the rod was held equatorially to the line of poles, but that as the rod was brought to either pole an increasingly slight effect was seen. This might be due in part or perhaps wholly to the want of parallelism of the motion to the pins, for the slightest side or horizontal motion produced a marked effect. That even this stiff plate vibrates when spoken to there can be little doubt, but the vibrations cannot, on the ordinary push-and-pull principle, push and pull at right angles to themselves. The action of the disc may be due to the blow it gives to the pole pins as an ordinary resonator, and possibly also the action of blows just hinted at may be present. The aerial blows may directly induce currents, or they may do so in altering the shape of the plate by vibration. However this may be, the problem may safely crave a solution from the push-and-pull theorists. When the plate is not screwed down, but laid or gently pressed on the pins, and more especially if it be made the bottom of a shallow tin box with a hole on the top, its performance almost rivals that of Bell's telephone. The improved effect is no doubt due to facility of vibration which is as necessary to the sounding power of vibrations of molecular origin as to those produced mechanically.

I made an attempt also to see whether the coil itself had any sounding action. If a blow can make soft iron induce a current, possibly a closed coil, the electric analogue of soft iron, may do so under the same treatment. I first tried this by hitting the side of a coil by a small wooden hammer, then as the frame of it gave way under this usage, I cemented it to a piece of board and hit the board; but as again the whole coil split up under the blows, I lastly tied it to a strip of wood, and used it as the head of a hammer against the end of a wooden rod. In each case I succeeded once or twice in telephoning the taps, but most unaccountably, for I might tap ever so hard and so often afterwards without being again so fortunate. How the sounds were sent in these cases I cannot say. The coils used, which were the same as those employed with the tuning-fork, could send with a brisk turn of the hand a current induced by the earth, may have been so excited, but there was next to nothing rotational in the blow they got or gave.

It may be well believed that many of the sounds thus produced in the telephone were very faint, and required a sharp ear and undivided attention. I have mentioned only these results which have been repeatedly got. Possibly some of them may be wrong as regards comparative loudness, but I had in almost every case the impression of different listeners. The listener was provided with two telephones of the following kind. One pole of a horse-shoe magnet (fig. 7), carried the bobbin pin and a brass arm for an adjusting screw. To the other an iron stage was fixed, to which was screwed a box $3\frac{1}{2}$ inches in diameter and $\frac{1}{4}$ inch deep, with a hole in the top $\frac{3}{4}$ inch wide, provided with a short tube for insertion into the ear. The bottom of this box was of untempered steel plate (No. 28), and its distance from the pin could be adjusted by the screw. Such an instrument told faint tuning-fork sounds, which the ordinary one pole ferrotype plate telephone I had passed over.

Fig. 7.



Leaving now the sending station, we proceed in our search for molecular action to the receiving instrument, and we examine the sounds emitted by its electric and magnetic parts. Instead of having a person performing the irksome task of talking or singing at the sending end, let us lay aside the distant telephone, and put in its place and that of the speaker a Bunsen cell charged with water and a mercury break. Such a cell would have no effect on the telephone without the break, for it is only momentary currents that affect the instrument. The break thus furnishes us with an intermittent or discontinuous current, and is in fact an untiring and uniform speaker, uttering a series of ticks or taps which are very audible to the distant telephone hearer. Now let us begin with the coil which is the "fountain and source" of all action in the receiver. Let us detach it from the core, and hold it up to the ear. By attentive listening a faint ticking is heard, and if we wish to hear it without strain we must add another cell or two to the battery. The sounds emitted by the coil are very faint, and cannot be excited by the voice or even a tuning-fork. With small battery power the coil may be made to sound, but with a strong battery it may be heard at some distance from the ear. Thus the coil which we thrashed almost to death to send a message receives one without demur. We now insert in the coil a piece of soft iron, and we have a duet of two undistinguishable ticks, the principal part being performed by the iron. Here again the receiving action is immensely better than the sending, and leads us to suspect that our rough efforts to send were mere bungling, due to our ignorance of the right way to do it. If now we bring a magnet and place the soft iron pin on its pole, or screw it into the pole, a sudden bound is made in the loudness of the sound, and with the one cell water current we can hear a few inches off. Whatever vibrations were effected in the pin by itself now grow immensely more pronounced when it is made magnetic. We may now replace the coil in its core in the telephone, for our arrangement is nothing else than a telephone without a disc. The sending and receiving powers are now more nearly on a par, for we found that the gentle tap of a pencil can now be sent. Let us now, in imitation of Professor Tait and Mr Blyth's experiments, put a piece of glass or pasteboard, or wood or other non-conducting substance, on the core. The sound waxes thereby louder, as was explained by the plate acting as an ordinary resonator. Lift any one of these the smallest thing from the core, and it acts as a dead wall to stop the sound of the core. In place of these, let us take a series of conducting metal plates of lead, German silver, copper, and silver, all equally thick and as nearly as possible of the same temper. When we place one of these on the core we hear the same resonance as in the non-conductor, but we also perceive what appears to be a new and separate action set up by the plate on its own account, and as we change from disc to disc we find that the sound grows with the conductivity of the plate till we reach silver, which is very audible. When now we put an iron disc of similar size and temper, a sound is heard which completely eclipses the loudest of the others. All these discs when held slightly away from the core sound louder than when touching it, and the culminating receiving action, where sending and receiving are on a par, is the disc of iron in front of the pole, almost but not quite touching it—in fact, the disc of Bell's telephone.

Let us now retrace our steps, and go over the ground more minutely, beginning again with the coil. The coils I used consisted of from 30 to 100 ohms of copper wire ·007 inches, and were of various sizes and shapes. For such fine wire coils, as already stated, one cell will suffice if the listener be sharp and attentive, but two or three must be used for easy hearing. The intermittent current given off by an ordinary medical electro-magnetic machine produces very audible sounds in such coils. The electric wave of such a machine is peculiarly adapted to excite telephonic sounds. It is a double wave of opposite name, with a sharp beginning and an equally sharp termination. For thick wire coils a more powerful electromotor is required. With five fully charged Bunsen cells, I managed to get every coil I could lay hands on to render the ticking of the break, whether they had iron cores or not. The sounds were perfect telephonic performances; for it mattered not whether the wire was thick or thin, covered with silk or wool or cotton, the tick was not at all musical, but simply the reproduction of the sound of the break. We should be inclined at once to call such a rendering molecular, did we not know that in this era of mechanical telephones and phonographs, discs of tinfoil, oiled silk, paper, potato, butter, and other unlikely substances, can reproduce the tones of the human voice without peculiar accent. Coil ticking or tapping is familiar to any one who has dealt with a powerful induction coil. The sole of the instrument resounds with the primary coil rendering of the break outside the instrument.

Now, whence comes this sound in a coil? Wiedemann, who gives De la Rive the credit of first noting the sound, attributes it to the clashing of the various convolutions against each other, due to the known action that wires conveying currents in the same direction attract each other. With a view to answer this question, I wound up 15 feet of ·04 copper wire into a spiral $\frac{3}{4}$ of an inch in diameter, and sent the intermittent current of a five-celled fully charged Bunsen battery through it. On holding the end of the spiral to the ear, I heard the tapping distinctly. On drawing out the spiral, so that no two convolutions were in contact, I failed to hear any sound; but when I put two or three convolutions together and held them to the ear, the sound was again heard. I now isolated one convolution, and tied the thread of a paper telephone to it, and, although there was no neighbouring convolution in contact, I heard, by means of the telephone, the ticking distinctly, and, when the spiral was drawn out into a plain wire, the same sound continued. The current, as indicated by a tangent galvanometer of a single hoop of stout copper rod, produced a deflection of 54° for the unbroken current, and of 41° for the intermittent one. The cell resistance was under two ohms, and the exterior resistance under one ohm. The nitric acid was old, and had been frequently used. Removing the ·04 wire from the circuit, and substituting a short length of ·024 copper wire, the current interruptions were heard still better in the paper telephone. I suspected mechanical transmission of the sound, but that could scarcely be, the wires being led from the open air by a fixed course under the floor. When all mechanical connection was broken through mercury troughs, there was no alteration; and when a loop of 2 inches of ·024 wire was held firmly in a vice it did not cease to sound. I need not, however, have been so sceptical, for sounds of a similar origin had been got as early as 1845 by Beatson, De la Rive, and other observers. They may not, without the aid of the mechanical telephone, have got in the wires the same clearly rendered series of ticks that we hear by its aid. De la Rive's description of them, however, is both definite and graphic.*

* His words are :—"Quant au son lui-même, je ne peux pas mieux en donner une idée, qu'en le comparant à celui qu'on produit avec la roue de Savart. C'est une suite de bruits résultants du choc des particules métalliques les unes contre les autres, beaucoup plus qu'un son musical. On entend aussi, il est vrai, des sons musicaux. Ce sont les harmoniques du son que rendrait la tige ou le fil par l'effet des vibrations transversales; ils proviennent du mouvement vibratoire qu'éprouve le métal, mais ne sont pas un effet direct de l'influence électrique à laquelle il est soumis. On peut en effet, les faire disparaître en touchant avec la main le corps vibrant, sansque pour cela disparaisse le bruit fondamental."—"Le son que rend un fil de fer bien recuit quand il transmet le courant est un son très fort, qui ressemble beaucoup au son des cloches d'église dans le lointain. On pourrait peut-être l'employer avec avantage dans les télégraphes électriques."—"Le ton du son varie avec la vitesse avec laquelle les courants discontinus se succèdent; quand cette succession est très rapide, le son ressemble beaucoup au bruit que fait le vent lorsqu'il souffle fortement."—*Comp. Rend.*, 1845.

He details his experiments, and, unlike other observers, extended his researches to other metals besides iron. He stretched wires of various metals on a sonometer with a helix of wire round them, so that he could excite sounds by the simple passage of the current in the wire, or by the magnetising action on it of the helix. He used an electromotor of five Grove's cells. He tells us that the sounds emitted from iron and other metals, by the direct passage through them of a discontinuous current, were in no way different from those obtained by magnetisation when the same current was made to pass through the helix surrounding the wire. This was especially observable in the case of iron. He states also loosely that the loudness of the sounds emitted with the same strength of current was in proportion to the resistances offered by the wires, and that possibly this sounding action had the same conditions as heat in the galvanic circuit, so far at least as resistance was concerned. He, moreover, states that iron stood quite exceptionally among the metals in its power to give out such sounds.

The exact way in which De la Rive obtained these sounds appears to have been lost, for no subsequent observer accords with him. Wiedemann, for instance, in his "Electro-Magnetismus," in keeping with the investigations of Wertheim, states that iron alone emits sounds under the above conditions, and he quotes De la Rive's remarks on other metals within parentheses. Now, the simple device of attaching the string of a mechanical telephone to the wires when the intermittent current circulates, enables us to observe these sounding effects with perfect ease and certainty. When attached to a copper wire (·007 inches diameter) the sound is very marked; and when to an iron wire (·008) the telephone sounds many feet off, the current being that from the five-cell Bunsen battery.* Indeed, for the iron wire a telephone is not necessary, for the ticking can be heard when the wire is held in the teeth, or when it is doubled on the finger and inserted in the passage of the ear. There is only one objection to the last mode of procedure, viz., that the wire is almost too hot to be comfortable. It is not, however, necessary to insert the wire itself into the ear, for a cotton thread, tied to the wire and placed in the ear, sounds nearly

* This was distinctly heard in the auditorium of the Society when the paper was read.

as loud. A thin iron wire is therefore the most rudimentary form of a telephone. Gott's telephone has thus another explanation in addition to that of the mere twisting round in a magnetic field; for if only the coil be left without a field, or even if only one convolution of it be left, and held in the chaps of a vice, its power of moving the paper disc would not wholly disappear.

When the paper telephone was attached to the thin iron wire, and the distant break worked by the hand of an operator, a loud sound was heard when the dipper of the break entered the mercury, and another equally loud, but not louder, when it was lifted out. In this case the break was two rooms off from the listener, at a distance of from 30 to 40 feet, and when the operator was made to say "in," "out," at each motion, short as the distance was, the wire sounded before the voice. The sounds thus given had a clear metallic ring about them. This, however, disappeared when the wire was coiled tightly round the finger. The mechanical telephone enables us to hear such sounds in all portions of the wire without any interruption. We can hear, for instance, the half musical sound in a straight wire, the dull unmusical sound when the wire is tightly coiled, and the increase of this when several convolutions of a covered wire are kept tightly together. In the single wire, although the sound is loudest when it is tense, yet it is distinctly heard when perfectly loose. De la Rive tells us that the resistance in the sounding wire must be greater than that of the remaining circuit, battery included. I have found that this is not necessary, for I can hear distinctly the sound of a thin iron wire when the current is the weak one furnished by a single Bunsen cell charged with water. The Bunsen cells mentioned here have an active medium surface of 40 square inches. Again, with a small magneto-electric machine for medical purposes, almost any experiment can be demonstrated.

With reference to the strengths of current necessary to excite the wires, the characteristic of the telephone mentioned by Professor Tait, in his communication on the "Strength of a Telephonic Current," must be borne in mind, viz., that the telephonic effect does not depend so much on the actual strength of the sounding current as on the rapidity with which changes in the strength are effected. The break used in these experiments made from five to six interruptions in the second, that certainly was not very sudden. Then, in a mercury break, the spark that ensues on the separation of the dipper from the surface of the mercury is conducting, and it may only be the break of the attenuated current when the spark ceases that has telephonic power. This would bring us down to a low scale of effective action, when working with a water cell interrupted six times per second. It cannot, however, be with certainty alleged that this kind of interruption is strictly single, as there may be pulses in it preceding the final stoppage.

The relation of resistance to sound can be shown by an experiment like the following:—I measured off a yard of iron, German

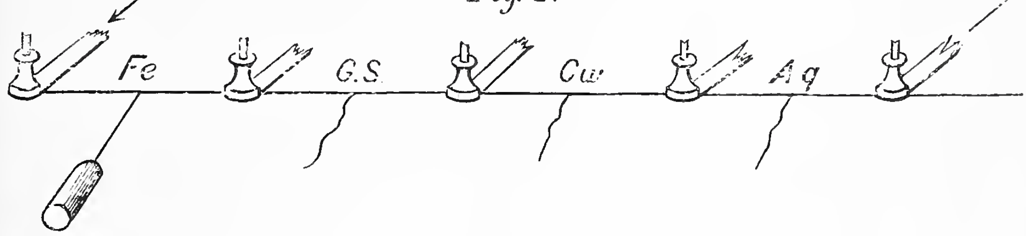


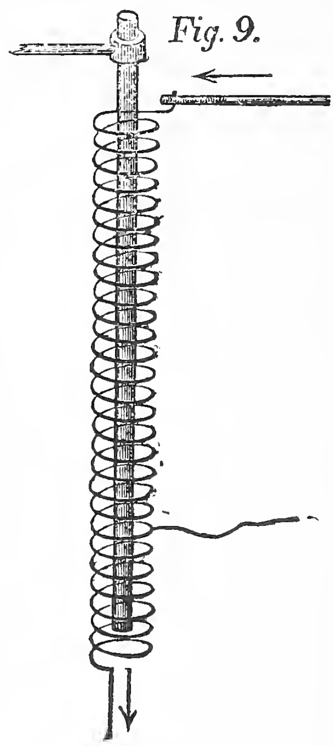
Fig. 8.

silver, copper, and silver wire, of diameters respectively of ·008, ·0086, ·007, and ·006 of an inch. The resistance of each yard was respectively 3, 8·7, ·7, and 1·4 ohms (the silver alloy was exceptionally hard). From these again I cut off a foot and soldered them end to end in a successive chain, and stretched them out (fig. 8) in a line, with their junctions firmly clumped to prevent the sound of the one running into the other. I then attached four similar mechanical telephones to them, and found, at least so far as the ear could judge, that the sounds emitted by each, when the same broken current passed through all, were proportionate to the resistance such offered, in the case of the last three metals, whilst the sound given out by the iron distanced all the others. The German silver here was much louder than the silver, which again was, one could fancy, twice as loud as the copper. This seems to clash with what we have stated with reference to the conducting discs on or near the core of the magnet of the telephone. It may be somewhat hazardous to venture an explanation, but there is one that turns up so readily at the first consideration of the matter that I may mention it. In the case of the wires the same current passed through all, but in the discs the currents induced would be, as is generally received, relatively in proportion to the conductivity of each. If it now be accepted that the sounding action be like heat proportionate to the square of the current strength, the apparent discrepancy is accounted for. Suppose the conductivity of silver to be ten times greater than that of German silver, then in the silver plate a tenfold greater current would be induced, and would have in consequence one hundredfold greater sounding power than that of the German silver in the same resistance; but as the resistance of the German silver is ten times greater than that of the silver, there would still be a tenfold louder sound in the silver than in the German silver, quite a sufficient margin to remove the discrepancy. In addition to the action within the plates here discussed, there may of course also be the external push-and-pull, as with the iron disc. In wires the length involved has little effect on the sound; for though a long stretch of wire does sound louder than a short one, the difference is by no means in keeping with the respective lengths. In thick wires no sound is got. I tried in vain with the strongest current I could conveniently use to get a sound out of a No. 14 copper wire.

Now here at least it will be admitted that we have a molecular action. It may be replied that the earth's magnetism has something to do with it. To show that it has not, we may try the somewhat inelegant feat of holding the end of the thin iron wire in the teeth, and turning it in every possible direction, when we shall find that the loudness of the ticking has no relation to direction. Again, when the string of the telephone is tied to the wire and made to go round it in a circle, we shall find no maximum or minimum points. This then is a telephonic action absolutely free from external push and pull, proceeding undoubtedly from molecular disturbance in the wire. That the action is magnetic in some way is evident from the exceptional position of iron in the series of metals. De la Rive held that the sounding action of an iron wire, when the seat of a discontinuous current was almost identical with, and must be traceable to the same ultimate cause as, that it displays when placed in a magnetising helix excited by a similar current. The parallelism between the wire telephone and the ordinary electric telephone is so striking that no theory of the latter can be held to be complete without it includes the former. It seems to suggest that the sounds we hear in plates and rods under sudden electric or magnetic changes is partially at least due to the molecular disturbances set up by induced momentary currents. Be that as it may, surely no one would be inclined to say that the ticking of a Bell telephone under a series of momentary currents is wholly mechanical, and that of a wire telephone under the same conditions wholly molecular.

After observing these sounds, my next inquiry was to ascertain how they comported themselves in a helix capable of motion. With this view I coiled about ten feet of fine iron wire (·0173 inch diameter) into a spiral about $\frac{3}{4}$ inch in diameter. I suspended it (fig. 9) by a thick wire held on a stand, and arranged that its lower end should dip into a vessel of mercury. Such a spiral is exceedingly delicate, and the up and down motion of its slender convolutions is most easily excited. When left free to itself, and there is no interruption in the mercury below, that is, no spark reaction, the motion produced by a discontinuous current of five Bunsen cells is very slight; but if a $\frac{3}{8}$-inch soft iron rod be introduced so as to be clear of the sides of the spiral, the mechanical excitement of the spiral is more marked, but still not much. But if I hold down the lower end, and it so happen that the tension and number of convolutions be sufficient, the helix divides itself into two very active ventral segments with a node in the middle. The motion, especially at the middle of each segment, is now very considerable. But if it does not vibrate thus symmetrically, by shifting the fingers a little, a point is easily found by which at least one very active ventral segment is secured. If now to a point of maximum motion I solder a very fine copper wire, to serve as the thread of a mechanical telephone and make it come out at right angles to the motion of the spiral, it does not interfere with it. By this contrivance I secure the advantage of listening to what goes on in the wire without interfering with any mechanical magnetic effect. Profiting by the experience of the fork, I found that a fine wire could hang loosely, and yet convey vibrations to

Fig. 9.

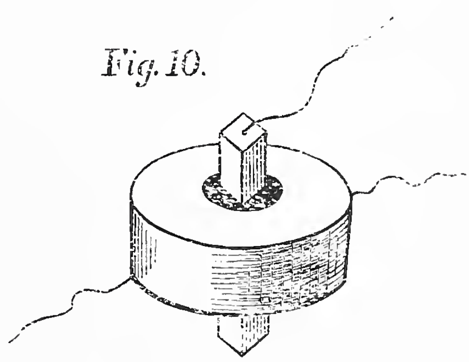


the telephone. Fine iron wire answers very well, but in this case it is inapplicable, as it sets up a clearly audible action on its own account. When the ear is now applied to the telephone the ticking that you would hear in the wire, if it were straight, is heard distinctly, and is the only distinct sound, for the up and down motion of the convolution only produces a slight jingling in the connecting wire, and possibly also in the coil. When the convolution is held tight in the fingers the ticking goes on if anything more distinctly than before. Whether the mechanical motion by the increased fixity of the helix is converted into louder ticking, could not be decided by the difference. Here we have two vibrations quite distinct from one another, the ticking in the wire and the mechanical motion due to the mutual electro-magnetic action between the rod and the coil. If this last were quick enough it would also be telephoned, but the rate of vibration being below that of musical frequency it is nothing but inaudible motion. In this helix action I would submit we have a dissected view of all receiving telephonic action—a vibration *in* the body clearly of molecular origin, and another *of* the body of a push-and-pull kind. The latter may be stopped or nearly so, but not the former. From its internal origin it is bound to make itself good, and when the body is held in the grasp of the most rigid substance it only propagates in it the minute vibrations which no elastic matter can stop. In the vibrations of coils, cores, or plates, the same thing holds. Molecular vibration is present in them all, and how far mechanical vibration chimes in in unison depends entirely on the ease with which such can be produced.

There is apparently a marked difference between these two vibrations. The one can make itself good acoustically by one impulse, the other not. I have tried in vain, while holding taut the thread of a mechanical telephone without letting the fingers slip, to produce, by a sudden pull or let go, the tick that resounds in iron or a conductor when a current suddenly begin or ends, and I could only do so by letting the string slip the slightest degree, so as to set up a short series of vibrations. Each galvanic or magnetic tick or tap may not, in the first instance, be more than a simple shock; but so sudden is it that the particles concerned do not recover at once, but continue vibrating for an infinitesimal time, and hence the pitch or musical sound that generally accompanies it. When, however, a prolonged vibration is difficult, and the impulses brace each other up by frequent repetition, we have possibly in the first case a very short series, and in the other only one vibration. Such vibrations are as capable of rendering all complex acoustic combinations as vibrations of the push-and-pull kind.

It would be a matter of mere speculation to guess how the conditions of the vibrating helix are transferred to vibrating rods or discs of iron in an intermittent magnetic field. I would only say that the same double vibration is clearly traceable in them. To illustrate this in the case of rods, I took a small coil of No. 20 wire, 2 inches in diameter and about an inch high with a hollow axis of $\frac{3}{8}$ inch, and sent the interrupted current of a five Bunsen cell battery through it. Inside the axis I put a soft iron pin 2 inches long and $\frac{1}{4}$ inch square (fig. 10). To the upper end a fine copper wire was soldered to act as the thread of a telephone. When rightly placed the pin supported itself in the hollow, and kept dancing up and down symmetrically without much friction against the inside of the bobbin hollow. Here the mechanical motion was not so clearly eliminated as in the case of the helix; yet the ticking was heard, and it alone, when the motion of the pin was stopped by the hand. The impossibility of stopping the ticking of the pin was

Fig. 10.



shown by securing its ends between the jaws of a vice and making it as tight as possible, when the vice itself took up the tale of electric interruption, and made itself heard all round. A curious change was observed in long iron rods when this coil was placed round the middle of them and when shifted to the end. In the former position the sound was a stuccato rendering of the longitudinal note of the rod, and in the latter this sound was lost in a dull tapping. In the latter position there was a pronounced tendency to push and pull.

I adopted a similar arrangement with the vibrating disc of the telephone. To the middle of one I cemented an india-rubber tube to act as a yielding handle, and the paper telephone wire was soldered near the middle. The disc was made to move in front of a telephone core excited by the coil just named. The movements of the disc were very violent, and made in all directions, making the connecting string jingle loudly, so that the isolation of the ticking sound was not so satisfactory as in the two previous cases. Still it was heard, and when the motions of the disc were kept in one direction its loudness did not grow with the extent of the motion. With a fine coil and a water cell the ticks were distinct enough; but the mechanical displacement was too small to yield the required comparison. The impossibility of stopping this ticking was illustrated in the following way:—A ferrotype plate was held tightly between two thin pieces of plate glass, the space between being filled up with sealing wax so that the whole was a solid mass of glass and wax. This was brought near a core excited by a water cell, when the sound was loudly rendered. Another illustration to the same effect was that of cementing by sealing wax the ferrotype plate of a telephone to a disc of thick microscopic glass, and putting this in the telephone with the glass side to the ear or mouth. Its articulate functions, though much impaired, still continued.

Lastly, to test whether the tick in a coil was due to electric conduction, I screwed a pin into the core of the telephone so as to act as a prolongation of the core; round this I placed a coil of fine wire, to which the string of a paper telephone was attached. There was no ticking heard so long as the circuit of the coil was broken; but the moment it was closed the ticking began. The coil was, of course, clear of the core. At the same time, however, there was mechanical action between the coil and core, illustrating the difficulty in such cases of determining by direct observation whether the single mechanical pull may not also make itself heard.

In conclusion, I would say, by way of summing up the evidence of this paper, that at the sending station the evidence of molecular action, though suggestive, is by no means conclusive; while at the receiving station the existence of molecular as well as mechanical action amounts to demonstration. How the actual performance of the receiving instrument is to be apportioned between these, it is, of course, difficult to say. Taking into account Professor Tait's calculations as to the infinitesimal strength of a current that can make a telephone tick, and assuming that that tick is purely molecular, as we have done, molecular action must be there not the less considerable.

2. Sketch of the Arrangement of Tables of Ballistic Curves in a medium resisting as the Square of the Velocity, and of the Application of these Tables to Gunnery. By EDWARD SANG.

The motion of a body in a medium whose resistance is proportional to the square of the velocity, has been the subject of many inquiries. Its intimate connection with the theory and practice of gunnery has produced for it the attention of almost every cultivator of the higher analysis; but, like several other seemingly elementary problems in mechanics, it has hitherto received no complete solution.

If nothing else than the fluid's resistance influence the motion the investigation is comparatively easy; thus, taking the time as the primary variable, the speed has its own square for its derivative, and so must be proportional to the first inverse power of the time, and consequently the distance passed over must be proportional to the logarithm of that time. Hence, if the present position, the velocity and the coefficient of resistance be given, we can compute, forwards, the position and velocity at any future time; or backwards, what those had been at previous times. But since the velocities are inversely proportional to the times elapsed from some fixed epoch, it follows that,

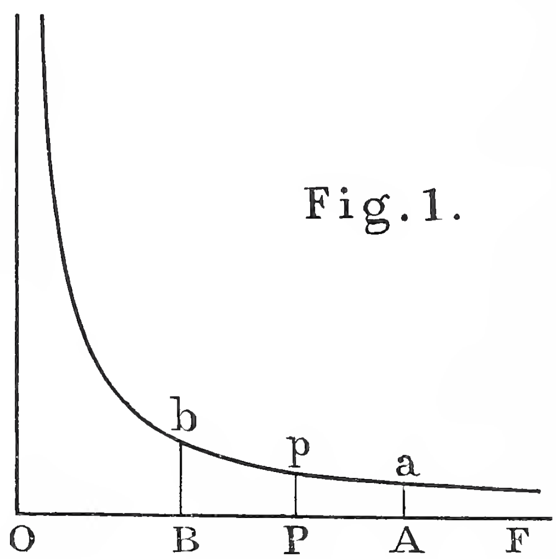


Fig. 1.

at that epoch, the velocity must have been infinite, so that although the body may have come from an infinite distance, setting out therefrom with an infinite velocity, it must have begun that motion a finite time ago.

If we represent time by distances measured along OF, one of the asymptotes of a hyperbola, the ordinates, such as Pp, drawn parallel to the other asymptote, are proportional to the corresponding velocities. Thus the present velocity being Pp, that at the future time OA will be Aa, while that at the former date OB had been Bb; and the areas BbpP, PpaA represent the distances passed over during the intervals of time BP and PA. The distance corresponding to the finite previous time OP is thus infinite, and so also must have been the velocity of projection at the date O.

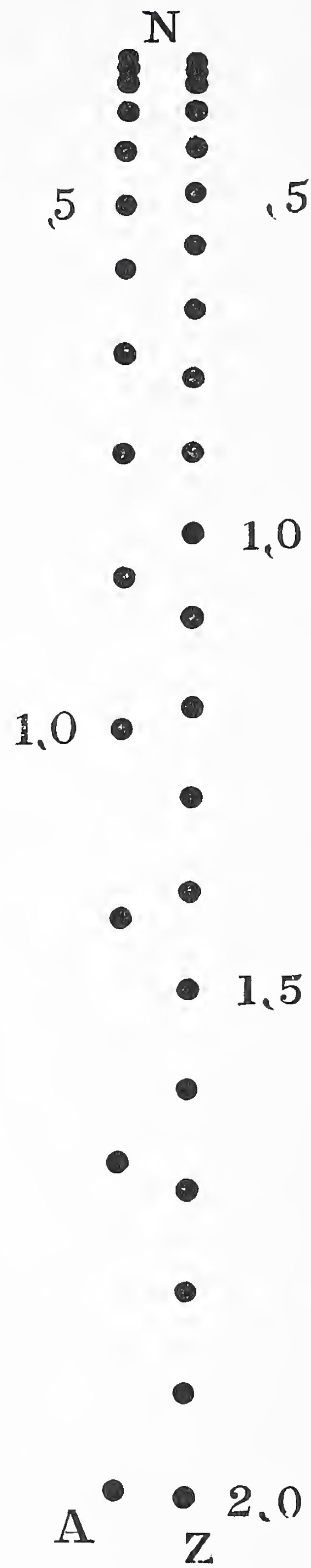
When the motion is affected by some influence other than the resistance, the investigation becomes more intricate. The case of a constant gravitation in a fixed direction is the simplest of these complications, and the simplest case of this is when the directions of the motion and of gravitation coincide.

If a stone be thrown straight upwards, its motion is impeded both by its weight and by the air's resistance; in the subsequent descent the motion is accelerated by gravity, but retarded by the air; so that, for the ascent, the soliciting influence takes the form $g+cv^2$, and for the descent becomes $g-cv^2$. Now the change in the sign of the velocity from $+v$ in the ascent to $-v$ in the descent, is not accompanied by any change in the sign of v^2, and therefore both parts of the motion cannot be represented by any one algebraic formula. Accordingly we find the upward motion to be represented by circular functions; the downward motion by the corresponding catenarian ones.

In fig. 2, the left hand row of dots represents the upward motion graduated to equal intervals of time. The stone is first shown at A as having come from some indefinite distance below; its speed, rapidly diminished, is altogether extinguished at N. In order to avoid confusion, the descent is shown on the adjoining right hand column of dots.

In descending, the acceleration due to gravity becomes less and less; it would cease altogether if the velocity could become so great as to cause a resistance equal to the weight; the tendency, therefore, is to reach a definite terminal velocity, and the stone ultimately moves almost uniformly. If it had been thrown downwards at a rate greater than this terminal velocity, its motion would have been retarded, but less and less so as it approximated to the same limit of uniform motion.

Fig. 2.



We may study the ascent by tracing it backwards from the highest point, fancying the air to have then the quality of hastening the motion. In this case the velocity would increase to become infinite; but this infinite velocity would be acquired in a finite time. In fact, the time being represented by a circular arc, the velocity would be proportional to the tangent of that arc, so that in the time corresponding to a whole quadrant, the velocity would become infinite. Thus it seems that, however rapidly a stone may be thrown upwards, its motion is extinguished within a finite time determined by the coefficient of resistance.

Each particular body has its own terminal velocity depending on the weight and on the extent and peculiarities of the surface exposed to resistance; but the motions of all follow exactly the same law, so that one diagram may serve for all, the units of comparison alone needing to be changed.

Also, one table of the positions and velocities may be made to do for all cases. In the arrangement of such a table we have to seek for the most convenient system of units; now, on contemplating the motion of a projectile independently of our measures of time and distance, we perceive that the terminal speed is the only standard with which we can compare the velocities at the various parts of the path, wherefore we adopt this terminal velocity as the tabular unit of speed.

This terminal velocity has to be considered along with the intensity of gravitation, which intensity, if acting alone, would generate velocities proportional to the times; wherefore the time in which the heavy body, falling freely, would acquire the terminal velocity, presents itself as the proper tabular unit of time, and, as a necessary accompaniment, the tabular unit of distance must be that over which the body would pass with its uniform terminal velocity in the time during which that terminal velocity was acquired; or, in other words, must be double the height of the free fall needed for the acquisition of the final velocity.

The accompanying table has been constructed for these assumed units; the details of the rise are given on the left hand, those of the fall on the right hand side, the times being reckoned before and after the instant of culmination:—

Time.	Velocity.	Rise.	Fall.	Velocity.	Time.	Time of Free Fall. / Time of Actual Fall.
−0·0	·00000	·00000	·00000	·00000	+0·0	1·00000
·1	·10033	·00501	·00499	·09967	·1	·99918
·2	·20271	·02013	·01987	·19738	·2	·99669
·3	·30934	·04569	·04434	·29131	·3	·99265
·4	·42279	·08223	·07795	·37995	·4	·98714
−0·5	·54630	·13058	·12011	·46212	+0·5	·98026
·6	·68414	·19197	·17014	·53705	·6	·97221
·7	·84209	·26809	·22727	·60437	·7	·96313
·8	1·02964	·36139	·29075	·66404	·8	·95321
·9	1·26016	·47544	·35983	·71630	·9	·94260
−1·0	1·55741	·61573	·43378	·76159	+1·0	·93143
·1	1·96476	·79055	·51194	·80050	·1	·91988
·2	2·57215	1·01512	·59369	·83365	·2	·90806
·3	3·60210	1·31864	·67850	·86172	·3	·89608
·4	5·79789	1·77215	·76570	·88552	·4	·88393
−1·5	14·10143	2·64878	·85544	·90515	+1·5	·87204
			·94681	·92167	·6	86005
			1·03968	·93541	·7	·84824
			1·13381	·94681	·8	·83637
			1·22898	·95624	·9	·82516
			1·32500	·96403	+2·0	·81394

Opposite the time 1·3 in the first column, we find the velocity 3·60210, this means that if the stone be thrown upwards with the velocity 3·60210, as at A in fig. 2, it will reach its highest point in the time 1·3, the height to which it will rise being 1·31864, as shown in the third column. The subsequent fall is shown in the fourth column, and there we find that the same level is reached just before the time 2·0; the whole time of flight being somewhat less than 3·3.

To translate this into ordinary measures, let us suppose a body whose terminal velocity is 320 feet per second; the time in which this velocity is acquired in free fall is 10 seconds, and therefore all the tabular times must be multiplied by 10, the tabular velocities by 320, and the linear distances by 3200. Hence if such a body were thrown upwards with a velocity of 1152 feet per second, it would rise in 13 seconds to a height of 4230 feet, and would thence descend in 19·93 seconds, striking the ground with the velocity of 308 feet per second.

This table enables us to interpret easily the results of experiments made on falling bodies. Thus, if the height and the time of descent be accurately observed, we may thence deduce the terminal velocity; from which, again, knowing the weight and the extent of surface, we may discover the constant of the coefficient of resistance. In order to facilitate the computations for this class of experiments, we may annex another column to our table, namely, one containing the ratio of the time of free fall to the actual time of fall. Thus the fall through the distance 1·32500, which is done in the time 2·0 with resistance, would be accomplished in 1·62788 if there were no resistance due to velocity, and the ratio, as shown in the annexed column, is ·81394.

Suppose now that a body has been dropped from a height of 400 feet, and that the observed time of the fall is 6 seconds; the time of falling freely from this height is known to be 5 seconds, and therefore the ratio is ·83333. The table shows that this ratio belongs to the tabular time 1·8271, and that, consequently, 3·284 seconds is the time in which the terminal velocity is acquired in falling freely; that terminal velocity, therefore, must be 105 feet per second.

In experiments of this kind, the disengagement at the beginning and the stroke at the end of the fall may be made, by help of the electro-magnet, to record themselves alongside of the records of the beats of a clock, and thus very great precision may be obtained.

When the stone is thrown obliquely, its path is curved; were there no air, that path would be the well-known parabola, which forms the first ideal approximation. With speeds very small in comparison with the terminal velocity, the deviation from the parabolic curve is slight; but the deviation becomes excessive in the ordinary practice of gunnery.

Beginning with the case of a body thrown obliquely upwards, we observe that its upward motion is resisted both by the air and by gravity; when it has reached its highest point and is descending, the downward motion is less rapidly accelerated than the upward motion had been retarded, so that the culminating point is reached earlier than the half-time of flight. On the other hand, the horizontal transference is retarded all along, wherefore the vertex of the curve is beyond the middle of the horizontal range, as is seen in fig. 3, which shows the positions of a projectile at equal intervals of time.

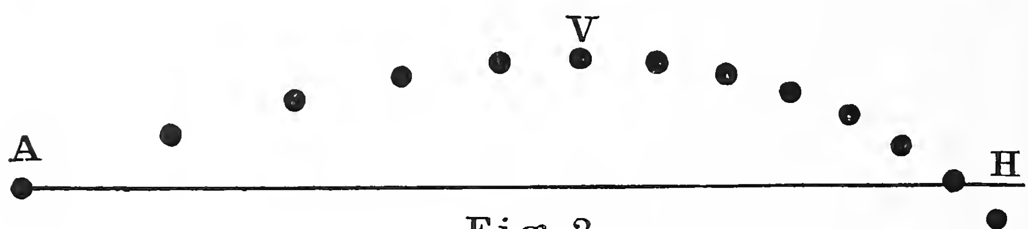


Fig. 3.

After having passed the highest point at V, the projectile bends its motion more and more downwards, until ultimately the path almost coincides with the plumb-line; the speed, at the same time, gradually approximates to the terminal velocity, and thus the curve in that direction is limited by a vertical asymptote.

Reckoning backwards on the other side of the culminating point, the speed is rapidly augmented, and would become infinite within a definite time; the inclination of the curve also tends to a definite limit, wherefore this branch, too, has a rectilineal asymptote; so that the whole curve is continued in the angle formed by the two asymptotes as represented in fig. 4. Thus the branch VB, unlimited in length, is limited in the time of its description; while the branch VC, also unlimited in length, is unlimited as to time, but is limited as to the extent of its horizontal range.

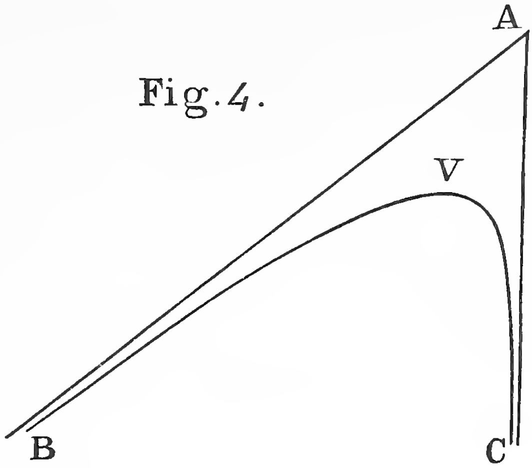


Fig. 4.

The intensity of gravitation being known; if the terminal velocity, the direction, and the velocity at any point be given, we can compute the position, the velocity, and the direction at any proposed instant. These three data serve to determine the curve, and in general any three data are sufficient, as, say, the inclination, the range, and the time of flight. The direct computation from the first-named three arguments is very operose; in the other cases it is much more so, because we can only proceed by the method of successive trials, or, what comes to the same thing, we must have recourse to tabulated results.

My object at present is to describe the arrangement of tables of this kind, and to explain their uses.

If we launch two masses at the same angle with speeds proportional to the terminal velocities, the paths are similar in shape though on different scales and performed in different times. This circumstance is the first and principal guide in the arrangement of the tables, because the computations for the one trajectory are easily made to serve for the other. We naturally select for tabulation that one in which the terminal velocity is unit.

These ballistic curves differ in character as well as in size. The characteristic of the shape may be taken as the angle A (fig. 4), made by the two asymptotes, or we may adopt the velocity at some definite direction, say the velocity at the summit V. The former is general in its application, it includes the cases of projection obliquely downwards, even to the limit of projection in the direction of gravity; but the latter has the advantage of more ready reference to practice, and lies more to hand in the calculations.

In the subjoined example of a table, the velocity at the summit is assumed to be just equal to the terminal velocity, and the details are given for intervals of one-tenth part of the tabular unit of time. For actual use these intervals must be made much smaller, and the tables should be arranged for values of the characteristic velocity, differing slightly from each other; the former table may be regarded as the beginning of the series with the title V = 0.

Since the curves extend indefinitely both ways, such tables must always be incomplete; we can do no more than carry them to the limits of probable utility.

The first part of the table contains the details of the rise; thus a body shot off at the instant − ·50, with the velocity 2·18, and at an

Velocity at Summit, V = 1·00000.				
Time.	Elevation.	Velocity.	Rise.	Hor. Distance.
·00	0°··00′	1·00000	·00000	·00000
− ·10	+ 5 ··25	1·11630	·00518	− ·10537
− ·20	+ 10 ··12	1·27188	·02158	− ·22323
− ·30	+ 14 ··17	1·48161	·05092	− ·35716
− ·40	+ 17 ··42	1·77176	·09580	− ·51262
− ·50	+ 20 ··27	2·18190	·16041	− ·69840
Time.	Depression.	Velocity.	Fall.	Hor. Distance.
·00	0°··00′	1·00000	·00000	·00000
+ ·10	− 6 ··00	·91394	·00484	+ ·09521
+ ·20	− 12 ··25	·85219	·01883	+ ·18217
+ ·30	− 19 ··03	·81041	·04127	+ ·26201
+ ·40	− 25 ··42	·78508	·07157	+ ·33561
+ ·50	− 37 ··08	·77348	·10921	+ ·40362
+ ·60	− 38 ··38	·77521	·15481	+ ·46631
+ ·70	− 44 ··48	·79801	·20628	+ ·52326

elevation of 20°··27′, gradually loses speed, as shown in the third column, and reaches the summit at the instant ·00, having there a velocity of 1·00, and being now ·160 above its original level. The details of the descent are given in the second part of the table. On consulting the column marked Fall, we find that the original level is reached between the instants + ·60 and + ·70, by interpolation at ·611, so that the whole time of its flight has been 1·111. Also, in the columns marked Hor. Distance, we find ·6984 for that covered during the rise, and ·4726 for that passed over during the fall, making the total horizontal range 1·1710. The velocity with which the ball strikes the ground is seen to be ·7777, while the impact is at an angle of 39°··19′. The squares of the initial and final velocities are nearly in the ratio of 55 to 7; that is to say, of the work done by the gunpowder in putting the ball in motion, 48 parts are spent on the air, and 7 parts only remain to represent the destructive effort.

Thus we can readily compute the range, the time of flight, and the incidence of the ball. A table of these, such as the following, forms a convenient adjunct to the fundamental table :—

Velocity at Summit = 1·00000.					
Elev.	Velocity.	Range.	Time.	Velocity.	Depr.
5°··26′	1·11630	·20372	·20347	·91142	6°··13′
10 ··12	1·27188	·41740	·41452	·84497	13 ··22
14 ··17	1·48161	·64538	·63470	·79993	21 ··26
17 ··42	1·77176	·89405	·86649	·77594	30 ··01
20 ··27	2·18190	1·17100	1·11087	·77768	39 ··19

In order to apply these results to business, we must ascertain the values of the tabular units in terms of the actual units of time and distance. This is easily done if the terminal velocity be known. As an example, let us take a bullet whose terminal velocity is 800 feet per second, in which case the tabular velocities must all be multiplied by 800. A heavy body falling freely acquires velocity at the rate of 32 feet per second for each second of time, and would acquire this velocity of 800 in 25 seconds, wherefore all the tabular times must be multiplied by 25. Lastly, the unit of distance is described with the unit velocity in the unit of time, wherefore $800 \times 25 = 20000$ feet is the actual linear unit of the tables as applied to this particular projectile. The above example, therefore, expressed in English feet and in seconds of time, becomes

Angle of elevation,	=	20°··27′.
Initial velocity,	=	1745·5 feet per sec.
Time of ascent,	=	12·5 sec.
Rise,	=	3208 feet.
Time of descent,	=	15·6 sec.
Horizontal range,	=	23420 feet.
Final velocity,	=	622 feet per sec.
Angle of incidence,	=	39°··19′.

If the same ball be shot from the same gun, but at another inclination, the shape of the path is changed and the details thereof must be sought for in another table. We search among the various tables for that one in which the given initial velocity is found opposite the proposed angle of elevation; if the tables be constructed for values of V sufficiently close, we shall find this either directly or by an easy interpolation; and then, proceeding as above, we can get all the desired information.

Similarly, if the initial velocity and the horizontal range be given, we convert these into the corresponding tabular numbers, and search among the various tables for that one in which these two are found together; the angle of inclination, the time of flight, and all the other quæsita of the problem are then to hand.

It has been stated that three data suffice to determine the path. When the terminal velocity is one of these, the solution is obtained by simple inspection; but when that velocity is one of the quæsita, the operation becomes indirect.

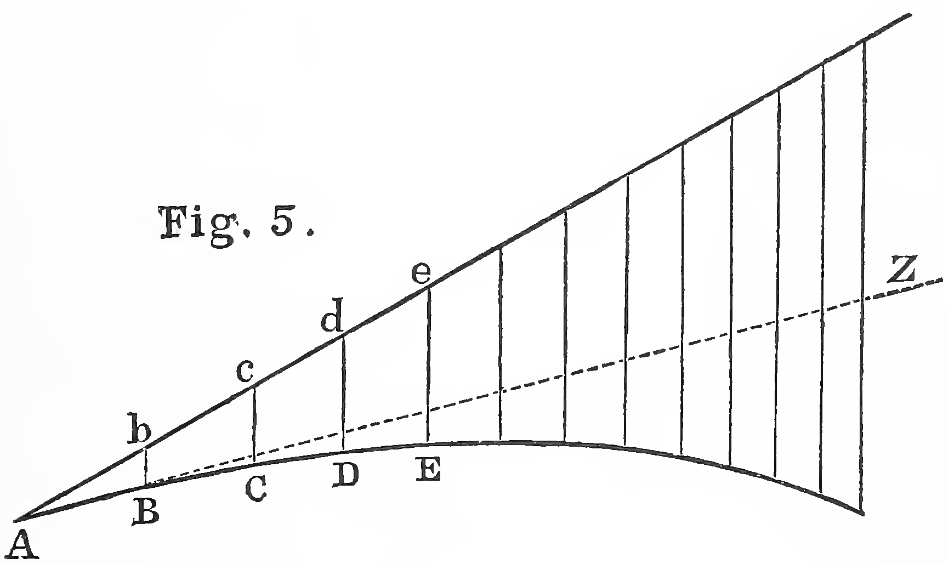
Suppose that the velocity communicated to a given shot by a specific charge of gunpowder has been ascertained, say, by help of the ballistic pendulum, we may discover the terminal velocity of that shot by observing the angle of elevation and the horizontal range. For this purpose we assume some terminal velocity, thence compute the corresponding tabular initial velocity, and thereby obtain the corresponding horizontal range. If this come out too much, we must reduce the assumed terminal velocity, and continue our trials until the computed agree with the measured distance. If the time of flight have also been carefully noted, we get a corroboration of the accuracy of the result.

Not only so, we may dispense altogether with the ballistic pendulum, and determine both the initial velocity and the resistance by accurate observations of the angle, the range, and the time of flight. For the purpose of facilitating the solutions of the various problems which may arise in practice, auxiliary tables may be derived from the fundamental ones.

When a shot is fired in a steady breeze, the direction in which the ball meets the air is not the apparent direction of the gun, it is that of the resultant of the two motions, and the computations have to be made as for that resultant.

Thus if AZ (fig. 5) represent the horizontal direction of the gun, and AB the initial velocity of the ball projected on the horizontal plane, while *b*B represents the velocity of the wind, A*b* will be the horizontal direction in which the ball meets the air. Since the vertical motion is not affected, the tangent of the elevation of the gun must be changed in the ratio of A*b* to AB in order to get the tangent of the true angle of elevation in relation to the air, and A*b* multiplied by the secant of that elevation is the true initial velocity.

Fig. 5.



If we now, using these corrected arguments, compu te the hori zontal distances corresponding to equal intervals of time, measure those along the prolongation of A*b* and from the successive points draw parallels *c*C, *d*D, &c., multiples of *b*B, we shall have the horizontal projection of the ball's path, that path being a line of double curvature.

As an example of the variety and completeness of the information conveyed by such tables, we may cite the path detailed in the preceding table and represented by fig. 3.

The ball is projected from A with the velocity of 2·18, and at once encounters a resistance $4\frac{3}{4}$ times greater than its weight. The speed is rapidly lessened, and the path is deflected to become horizontal at V, where, in the present instance, the resistance is just equal to the weight. On account of this resistance the speed is still slackened, but gravity now comes to accelerate the motion downwards, and, at about the fifth interval from V, has overcome the retardation, thereafter the velocity slowly increases, and tends ultimately to reach the limit 1·00.

Those cases in which the characteristic angle A of fig. 4 is obtuse have little or no application to gunnery; in them the path is never horizontal, but is inclined downwards all along.

The analysis of these motions is complex, and the calculations thereon following are tedious, but the results, when tabulated, are of easy application. The theory would be uninteresting to those engaged in the actual business, just as the mode of construction of trigonometric and logarithmic tables is scarcely ever thought of by the navigator or surveyor. What we have at present to consider is the advantage to be gained by the compilation of a series of tables such as those sketched out.

3. On some Physical Experiments relating to the Function of the Kidney. By David Newman, Glasgow. Communicated by Professor M'Kendrick.

(*Abstract.*)

This paper treats of the physical influences which promote the secretion of urine, as far as can be demonstrated by experiments upon animal membranes and the kidneys of animals recently killed. Before going on to consider the subject I may be permitted simply to mention the theory held regarding the means by which the

kidney performs its function, and also say a word or two in connection with the structure of that organ. As regards its histology the kidney may be said to be composed of two elements—(1) the blood-vessels, and (2) the tubuli uriniferi. (This is leaving out of account the lymphatic arrangement.) The kidney receives its supply of blood from the renal artery, which, as it passes into the substance of the kidney, penetrates the cortical portion and gives off branches. The uriniferous tubules in this part of the kidney end in globular dilatations called the capsules, or Malpighian bodies; it is into these that the branches of the renal artery pass to form convoluted coils, the glomeruli. The branches of the renal artery which pass into the glomeruli are called the afferent vessels, and the vessels that are formed by the reunion of the branches of the glomeruli are called the efferent vessels. After the efferent vessels emerge from the capsule of the Malpighian body they again subdivide to form true capillaries, most of which go to form a closely meshed network round the tubuli uriniferi. They finally unite to form the radicals of the renal vein.

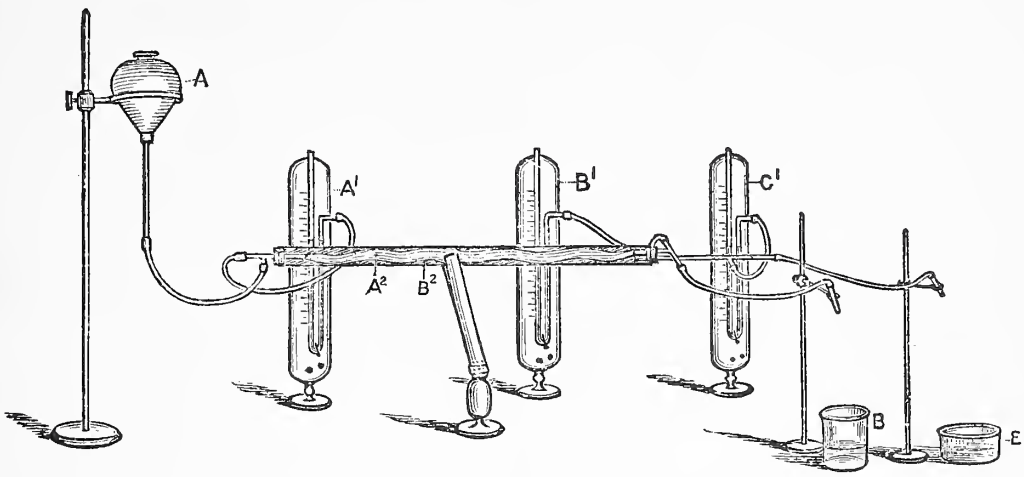
To make use of the description of Mr Bowman, "it would be difficult to conceive a disposition of parts more calculated to favour the escape of water from the blood than that of the Malpighian body. A large artery breaks up in a very direct manner into a number of minute branches, each of which suddenly opens up into an assemblage of vessels of far greater aggregate capacity than itself, and from which there is one narrow exit. Hence must arise a very abrupt retardation to the velocity of the current of blood." But besides this arrangement, by which a large volume of blood is exposed to circumstances the most conducive to free filtration of its fluid constituents, we have a condition, namely, the secondary capillary system on the distal side of the glomerulus, which, by its resistance to the onward flow of the blood, subjects the blood inside the Malpighian body to considerable pressure. It is now generally supposed that the excretion of urine takes place by filtration of a dilute solution of the soluble constituents of the urine through the glomerulus into the capsule of the Malpighian body. This weak solution then passes along the tubuli uriniferi, where it comes into close contact with the blood it has just left. It is then supposed that an interchange takes place between the blood in the capillaries surrounding the tubules and the fluid inside by which a certain amount of the water passes again into the blood, and so leaves the urine in a more concentrated state than it was when it first passed from the glomerulus into the dilated end of the urine tubes (Ludwig). It is believed, however, that the epithelium, which lines the convoluted tubes, performs certain functions in connection with the secretion of the solid constituents.

The rapidity of the secretion of urine may be said to depend upon the following factors:—(1) The relationship which exists between the pressure of the blood in the glomerulus of vessels and the urine in the capsule of the Malpighian body and in the tubuli uriniferi; (2) the state of the blood pressure in the venous system of the kidney; (3) the pressure upon the lymphatics; (4) the quality of the blood in the artery of the Malpighian tuft; (5) the state of the walls of the artery constituting the Malpighian tuft, and of the capsule itself, these being regarded as the filter through which the fluids and soluble constituents of the blood have to pass. The influence of vaso-motor nerves upon secretion must not, however, be forgotten; not only do they exert an influence upon the quantity and quality of the secretion by dilating or contracting the arterioles, but their influence upon the chemical processes, by reason of their communications with the secreting cells (Pflüger), must be remembered; (6) activity of tubular epithelium.

In the experiments I have endeavoured to imitate the conditions found in the Malpighian body of the kidney. I have not been able, however, to represent the lymphatic arrangement.

The apparatus, a drawing of which, kindly executed for me by my friend Dr Robert Moffatt, is shown in the following woodcut. It consists of a piece of rabbit's bowel A^2 enclosed in a glass tube B^2. To each end of the bowel a small glass T-tube is attached. One of those tubes is connected with a pressure-bottle A, and a manometer A^1. The pressure exercised upon the fluid inside the bowel by the pressure-bottle and indicated by the manometer A^1 will be designated the *afferent pressure.* The tube attached to the other side of the bowel conveys the fluid that passes *along* the bowel to the vessel B, and the pressure exercised upon the fluid which it contains, called the *efferent resistance,* is indicated by the manometer B^1. Through a cork at the right hand side of the large tube, another

T-tube communicates, on the one hand, with the inside of the large glass tube containing the bowel, and, on the other, the vessel E and manometer C^1. The vessel E will therefore contain the fluid that filters through the membrane A^2.



The index in the manometer A^1 will therefore represent the *afferent pressure*, and correspond to the arterial tension of the renal artery; B^1 will indicate the *efferent resistance* (when applied to the kidney, the venous resistance), while the manometer C^1 will show the resistance offered by the fluid in the tube B^2 to the transudation of the fluid inside the bowel, and therefore correspond to the tension of the urine in the capsule of the Malpighian body.

In the first series of experiments water was passed into the bowel under an afferent pressure of from 10 to 50 mm. of mercury, the efferent resistance being the same in each experiment as the afferent pressure, so that no water passed along the bowel to B. The amount of fluid which transuded through the bowel was found to increase in accordance with the pressure used. It was shown that for every 10 mm. increase in the pressure, there was ·533 c.c. more water filtered through the bowel per minute. Thus under a pressure of 10 mm. 2·133 c.c. transuded in a minute, and when a pressure of 50 mm. was applied 4·266 c.c. passed in the same time. From these results we would therefore conclude that the amount of fluid which transudes through an animal membrane is increased according to the tension of the fluid inside. In relation to this experiment, take the following:—Instead of an animal membrane the kidney of a recently killed horse was employed; in this case a three-quarters per cent. salt solution was used instead of water, as it was found that when water is passed into the vessels it almost immediately passes through the walls and causes œdema of the tissue, and the onward flow of the fluid is prevented. The salt solution seems, however, not to pass through the walls of the vessels into the lymphatic spaces so readily if the kidney is quite fresh, but still it passes from the glomeruli into the dilated end of the tubuli uriniferi. I found it very difficult to get this experiment to work satisfactorily, as the kidney requires to be used immediately after the death of the animal, and a number of precautions need to be taken which it is not necessary to mention here.

In the experiments alluded to the salt solution was passed into the artery under various pressures, the venous resistance being equal to 20 mm. in all except the first, in which case no resistance was offered to the exit of the fluid by the vein. In the first experiment the solution seemed simply to pass from the arterial into the venous system, very little being pressed into the urine tubules. When, however, the efferent resistance is raised to 20 mm., and at the same time the afferent pressure advanced to 40 mm., the increase in the amount of fluid pressed into the ureter is obvious. In the other experiments upon the kidneys of animals, the results of which I will not give in detail, a somewhat similar plan was adopted. The following are the results:—(1.) When the fluid contained in the ureter is subjected to pressure, the quantity of fluid that passes from the vein is diminished in relation to the pressure employed, and so also is the amount of fluid that transudes from the glomerulus into the capsule of the Malpighian body. (2.) The quantity of fluid that passes from the vein depends upon the amount of afferent pressure; the greatest increase takes place between 40 and 50 mm. (3.) The temperature of the fluid affects the rapidity of the flow through the vessels and the quantity that transudes into the tubuli uriniferi. The higher the temperature the greater is the amount of fluid passed from the ureter, and the more rapid the circulation through the vessels. (4.) When the fluid is pressed into the artery, it finds its way readily into the vein, but when injected into the vein, it does not escape by the artery. There must, therefore, be some arrangement in the kidney, probably in the Malpighian body, by which regurgitation of the fluid is prevented.

The results of the experiments with the bowel show (1.) that the amount of fluid that transudes is in accordance with the pressure upon the fluid inside. (2.) For every 10 mm. increase in the afferent pressure ·275 c.c. more fluid transudes per minute, and the flow along the bowel is increased; whereas, when the efferent resistance is increased, the amount of fluid that transudes is augmented by ·31 c.c. in the same time, and the flow along the bowel is diminished. Therefore the afferent pressure may be said to be expended in two ways—increasing the amount of fluid that transudes, and the quantity that passes along the bowel—but the efferent resistance exerts its whole force in pressing the fluid through the membrane, therefore, 10 mm. increase in the efferent resistance has more effect than the same increase in the afferent pressure, and for the same reason we would suppose that a given increase in the venous resistance would conduce more to rapid secretion of urine than the same increase in the arterial pressure, unless when the venous resistance is extreme when other factors come into play. (3.) The addition of urea slightly retards the transudation of water through the membrane. The filtrate contains the same percentage, whatever pressure may be employed, as the original solution. (4.) Albumen also retards the transudation of water, but it differs from urea in this respect, that the percentage of albumen in the filtrate is in relation to the pressure. The higher the pressure the larger the quantity of albumen in a given amount of the filtrate. (5.) The presence of urea in a solution of albumen assists the filtration of the albumen at the expense of the urea. The following table shows the results :—

Pressure in mm.	Albumen.		Urea.		Quantity of Solution.
	With Urea.	Without Urea.	With Albumen.	Without Albumen.	
10	·173 grms.	·131 grms.	6·5 C.grms	7·1 C.grms	14·2 c.c.
20	·460 „	·332 „	10·8 „	13·5 „	27· „
30	·750 „	·600 „	16·8 „	19·5 „	39·5 „
40	1·306 „	·900 „	22·3 „	26· „	52· „

(6.) The higher the temperature of the solution the more rapid the transudation of the fluid. Thus, when water was passed into the bowel at a temperature of 15·9° C., and under a pressure of 45 mm., 142·5 c.c. filtered through in thirty minutes; whilst, when the temperature alone was raised to 34·2° C., 197 c.c. transuded in the same time.

At the beginning of this paper I referred to Ludwig's theory regarding the secretion of urine, namely, that the blood is subjected to a high pressure inside the glomeruli, a free filtration into the dilated end of the tubuli uriniferi takes place, and this filtrate, which is at first very dilute, gradually parts with a portion of the water that holds it in solution. This is believed to take place by a process of diffusion between the fluid in the tubuli uriniferi and the blood in the veins surrounding them on all sides. Now, if it were not that albumen retards to a certain extent the passage of crystalloids (salts and urea) through an animal membrane, then the fluid in the inside of the urine tubules would be of the same concentration (in crystalloids) as the blood. But it has been shown that when a solution of albumen and urea are filtered through an animal membrane under pressure, the filtrate is less concentrated than the original solution, particularly as regards the amount of albumen, but also to a slight extent the urea. This is more especially the case when the pressure is not great. If the blood contained nothing but crystalloids (urea and salts) then the fluid inside the tubuli uriniferi would be the same as the fluid circulating in the vessels, and no diffusion would take place during the passage of the urine from the glomerulus to the pelvis of the kidney. This is, however, not the case; the blood circulating in the vessels contains a large quantity of albumen, and, if the theory above stated be correct, more urea than the fluid in the tubules, so that, putting aside any special function the epithelium may have, diffusion must result, and a portion of the water in the tubules pass back again into the blood. This diffusion will take place as the urine passes along the tubuli uriniferi either till it becomes of the same concentration as the blood outside, or makes its escape into the common ducts that convey it to the pelvis of the kidney. Therefore, the longer the urine remains in the tubuli uriniferi, other things being equal, the more concentrated will it be.

4. Note of a Method of Studying the Binocular Vision of Colour. By John G. M'Kendrick, M.D.

There are several well-known methods of mixing colours, such as the superposition of two spectra or of different parts of the same spectrum—the method of reflection, Czermak's modification of Scheiner's experiment, the use of rotating disks having coloured sectors, and the direct mixture of coloured powders or coloured liquids. In all of these cases the effect may be seen with one eye, and is due to the action of light on a definite portion of one retina. But may sensations of mixed colours be produced by binocular vision of the components? Regarding this question various well-known observers have arrived at completely opposite results. Thus, as mentioned by Helmholtz in his "Optique Physiologique," p. 976, H. Meyer, Volkmann, Meissner, Funke, and he himself fail in obtaining the sensation of the resulting colour, whilst Dove, Regnault, Brücke, Ludwig, Panum, and Hering state the reverse. In his great work, Helmholtz describes various methods by which he investigated the question, and his opinion amounts to this, that we have no true binocular perception of colour. According to him we may have a resultant sensation of a particular kind, different from that of the two components, but also unlike the sensation of the mixed colour obtained by methods appealing to one eye only.

In studying this subject I lately devised the following simple arrangement:—Take two No. 3 eye-pieces of Hartnack's microscope, or any similar eye-pieces of considerable focal length, and place one before each eye. If they be somewhat diverged, two luminous fields will be seen, and by adjustment, the edge of the one luminous field may be caused to touch the edge of the other. In these circumstances a definite area of each retina is illuminated. By converging the eye-pieces, the two fields may then be partially overlapped, and when the axes of the two eye-pieces are parallel, both fields coincide. It will then be found that the overlapped portion is intensely luminous, whilst the other portions become less luminous, as if cast into shadow. By increasing or diminishing the amount of convergence of the eye-pieces, the extent of the luminous field may be varied at pleasure, and the two fields coincide when the two images fall on the two yellow spots. If, then, a small piece of coloured glass be inserted into each eye-piece, say red into one and blue into the other, on repeating the experiment as above mentioned, I find that the overlapping portion of the two fields gives a sensation of the resultant colour. I have repeated the experiment with various coloured media, such as coloured gelatine paper, coloured paper rendered translucent by oil, &c. In showing the experiment to others, I have found that certain people do not see the resultant colour, whilst others do so readily. The cause of this and of the opposite statements of the observers above alluded to, I believe to be this : The sensation resulting from the fusion in the brain of the two impressions, one coming from each eye, appears to be capable of decomposition by a mental effort. Thus, the purple produced by red and blue appears as such to my eye so long as I simply look at it without any conscious effort; but if I wish to analyse it, I then find that the two colours, red and blue, seem to be superposed on each other, and the one appears to shine through the other. On ceasing to make any effort, they again fuse together as before. Again, by thinking of the colour opposite the right eye, say red, the field ceases to be purple and has a decided tinge of red, and on thinking of the colour before the left eye, say blue, the prevailing tone of the field is blue. Apparently, then, if corresponding points of two retinæ be simultaneously stimulated by two different colours, the impressions are fused in consciousness into the resultant colour ; but the resulting sensation may be decomposed by an act of attention. The decomposition is effected partly by strongly directing the attention to one eye, and less strongly to the other, and the result is a sensation corresponding to the colour placed before the eye to which the attention is most strongly directed. Some of the same facts may be studied with the aid of the stereoscope.

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

Alex. Macfarlane, M.A., B.Sc., 2 Roseneath Terrace.
Samuel Drew, M.D., D.Sc., Chapelton, near Sheffield.
George M'Gowan, 24 Argyll Place.
James Brunlees, Vice-Pres., Inst. C.E., 5 Victoria Street, Westminster.
John Grahame Dalziel, 95 South Street, St Andrews.

Monday, 20th May 1878.

DAVID STEVENSON, Esq., C.E., Vice-President, in the Chair.

The following Communications were read:—

1. On the Genus Rhizodus. By Dr R. H. Traquair.

(*Abstract.*)

In this paper the author first sketches the history of Rhizodus, from its discovery by Ure of Rutherglen to the most recent papers on the subject.

Placed by Agassiz among the "Coelacanthi" (*i.e.*, cycliferous Crossopterygia of modern nomenclature) it was classified by Professor Huxley in the cycliferous division of his family Glyptodipterini, along with *Holoptychius, Glyptolepis, Dendrodus.* The discovery by the author, in 1875, of its subacutely lobate pectoral fin revealed the fact that it was much more closely allied to *Rhizodopsis* than to *Holoptychius*, and that it ought, along with the former genus, to be classed in a family (Cyclodipterini) distinct from the acutely lobate Holoptychiidae. The author does not consider the identity of Leidy's genus *Apepodus* with *Rhizodus* as proved.

M'Coy admitted two species of Rhizodus—*R. Hibberti*, Agassiz, and *R. gracilis*, M'Coy, merging with the former of these the *Holoptychius Portlockii* of Agassiz. An examination of the types of *Holoptychius Portlockii*, preserved in the Museum of Practical Geology, shows, however, that this species is not only specifically but also generically distinct from *Rhizodus Hibberti*, as the teeth are devoid of the cutting edges characteristic of *Rhizodus*, and in this particular, as well as in the minute striation of the surface, they closely resemble those of Hancock and Atthey's genus *Archichthys*, to which the author proposes to transfer it, at least provisionally.

An investigation of the large store of *Rhizodus* remains from the Gilmerton ironstone, belonging to the Edinburgh Museum of Science and Art, shows the presence of two well-marked species of Rhizodus, differing in the bulk to which they attained respectively, as well as in the relative thickness of the scales, the shape of many of the

bones, and the external sculpture as well of the bones as of the scales. For the larger of these two species, there can be no doubt of the propriety of retaining the specific name *Hibberti*, as it is clear that its enormous laniary teeth, as occurring in the limestone of Burdiehouse, and at first considered by Hibbert to be reptilian, originally suggested the name *Megalichthys Hibberti* to Agassiz, which he afterwards altered to *Holoptychius Hibberti*, after eliminating the Saurodipterine remains previously confounded with them, and to which latter he then limited the term *Megalichthys*, rather unfortunately, as "*Megalichthys*" is the smaller fish! M'Coy's *R. gracilis* is certainly a synonym of *R. Hibberti*, the apparent greater slenderness of the dentary bone being due to the infra-dentary plates (not known to M'Coy) being, as is often the case, awanting in the specimen; and as regards the greater slenderness of the anterior laniary, a large series of teeth from Gilmerton shows every possible amount of gradation in that respect. For the smaller species, whose remains have hitherto been confounded with *R. Hibberti*, the author proposes the name of *R. ornatus*.

1. *Rhizodus Hibberti*, Ag. sp.

This species must have attained a gigantic size, a detached dentary bone in the Edinburgh Museum measuring no less than 25 inches in length. Externally the cranial bones are ornamented by a rather fine tuberculation, the tubercles more or less confluent with tortuous ridges. The mandible displays the same structure as in *Rhizodopsis*, the *dentary* element being narrow, pointed behind, thick in front, where it carries the anterior or symphyseal laniary tooth, the three other laniaries behind being borne upon separate *internal dentary* pieces. Below the dentary, and forming the lower margin of the jaw, are three *infra-dentary* plates, while posteriorly the articular region is covered by a large plate representing the angular element. Other determinable bones described in this paper are the *maxilla*, which, as in *Rhizodopsis*, only bears small teeth, the principal *jugular*, the *operculum*, the *clavicle*, the *infra-clavicular;* there are others also whose place in the skeleton is not easily determinable. The *clavicle* and *infra-clavicular* are not tuberculated like the cranial bones, but ornamented with delicate reticulating ridges and pits; the posterior superior angle of the infra-clavicular is produced upwards and backwards in a long slender process. The scales are comparatively thin and very large, sometimes, as noticed by Hibbert, attaining a diameter of 5 inches; usually they occur in a broken condition. Their attached surface is marked by a central boss and concentric lines of growth. The outer surface, very rarely seen, is ornamented by closely set granules, which towards the posterior border of the exposed area are confluent into wavy ridges terminating in the margin. These seem certainly to be the scales attributed to *R. Hibberti* by M'Coy, but not by Young; they are probably also the same as the undescribed '*Phyllolepsis tenuissimus*' of Agassiz.

R. Hibberti occurs throughout the Cementstone and Carboniferous Limestone series of Scotland, the most noted locality being Gilmerton, near Edinburgh.

2. *R. ornatus*, sp. nov. Traquair.

To this species, which seems never to have attained anything like the dimensions of *R. Hibberti*, belong the specimen showing the pectoral fin described by the author in 1875, the head described by Mr L. C. Miall, and in fact nearly all the specimens in which any portion of a fish with bones or scales *in situ* is shown. The ornament of the cranial bones is somewhat similar in character to that in *R. Hibberti*, but very much coarser; the same is the case with the bones of the shoulder. Of the bones the following have been recognised by the author—*dentary*, *operculum*, *principal jugular*, *clavicle*, *infra-clavicular*; there are also others whose determination is still somewhat doubtful. The clavicle differs somewhat in form from that of *R. Hibberti*, its lower portion being narrower from before backwards. The expanded portion of the infra-clavicular is also shorter than in the larger species, but the same slender process is sent backwards and upwards from its posterior superior angle. The pectoral fin is subacutely lobate. The scales are thicker than those of *R. Hibberti*. The exposed area of the external surface is marked with short, interrupted, wavy, reticulating ridges, whose direction is mainly parallel with the posterior border of the scale; in the interval between these, more delicate ridges are seen radiating from the centre. It is apparently on a scale of this species that Dr Young has founded his description of those of *R. Hibberti*.

R. ornatus occurs in the Calciferous Sandstone series of Scotland, as at Burdiehouse in Mid-Lothian, and Pittenweem in Fifeshire ; but it is especially abundant in the blackband ironstone of the Lower Carboniferous Limestone group at Gilmerton, above which horizon it has not yet been discovered.

2. On the Anatomy of a recent species of Polyodon, the *Polyodon gladius* (Martens), taken from the river Yang-tsze-Kiang, 450 miles above Woosung. Part III., being its *Viscera of Organic Life.* By P. D. Handyside, M.D.

The author proceeded with his anatomical description of the respiratory, circulatory, and pneumatic systems in this remarkable fish; referring to the differences that exist in the male, the female, the young, and the adult specimens. He also shortly noticed the alimentary and other viscera of Organic life.

Dr Handyside illustrated his paper by 24 additional drawings —including 7 microscopic views—of structure in this fish.

The fourth and last part of Dr Handyside's paper will consist of a description of the articular system and the endo-skeleton of the *Polyodon gladius.*

3. A Mechanical Illustration of the Vibrations of a Triad of Columnar Vortices. By Sir William Thomson.

4. Fourth Report of Boulder Committee, with Remarks. By D. Milne Home, Esq.

Since the last Report was drawn out and laid before the Society, the Convener has had an opportunity of inspecting a considerable number of boulders not mentioned in previous Reports. Some of these are interesting, on account not only of size, but also of shape, marks on them, and position. The Committee consider that advantage will result from a special description of these, and from woodcuts of a few.

The cases have been arranged, as in previous Reports, according to counties, to indicate the geographical position of the boulders, and enable persons desirous of inspecting them, to know where to find them.

His Grace the Duke of Argyll, at the meeting of the British Association in Glasgow in 1876, was pleased to allude in complimentary terms to the researches of the Committee, and to express a hope that a condensed abstract of all the boulders reported on, might ultimately be framed. The suggestion will receive the consideration of the Committee.

ARGYLLSHIRE.

1. *Glenelg.*—Blocks of grey and red granite occur in the drift-beds through which the river Elg has cut. The rocks of this district are not granite, but clay schists.

On the right side of the valley of the Elg, immediately above the road, about 2½ miles east of Glenelg, there is a grey granite boulder, 21 × 18 × 10 feet, as shown on figs. 1 and 2, Plate I. The sharp end points N.N.W. Its height above the road is 1020 feet, above the sea 1120 feet.

It goes by the name of the Macrae Boulder, in consequence of a prophecy by a Mackenzie of Kintail, that some day, when one of the clan Macrae is travelling on the road below, it will fall and crush him.

The boulder is on the very edge of a shelf of the hill, and projects beyond it about 6 feet, as shown in figs. 1 and 2, Plate I.

The rocks on which it lies are clay-stone schists. The boulder must therefore have been *brought* to its present position. It is said that on a hill some distance to the west there is a granite rock similar to that of the boulder. By what means, and how the boulder was deposited in its present precarious position, it is difficult to explain. Possibly, when deposited, there was no steep cliff, at the edge of which it now projects. The whole valley may have been filled with detritus up to the level of 1100 feet, and thereafter scooped out by the river, as the sea, in falling from one level to another, gave to the river more cutting power. This process of scooping might have continued for such length of time, that the cliff thereby formed at length reached the boulder.

2. *In Glen Rossdale* (about 8 miles from Glenelg), at a height of about 900 feet above the sea, there is a boulder of coarse red granite, 5 × 4½ × 3 feet, on the top of a narrow ridge of hypersthene rock, as shown on fig. 3, Plate II., on the left side of valley.

Its position also is precarious, and suggests a doubt whether, when brought here, it could have been deposited on the precise point where it now stands. There was nothing to indicate the direction from which the boulder had come.

3. There is another boulder on the right side of the valley, about 820 feet above the sea, $12 \times 15 \times 7$ feet, fig. 4, Plate I.

It lies on a shelf near the ridge of a hill, and close to a slope of the hill which rises up from the boulder, and facing the N.W. The spot suggested the idea that the boulder had been brought from the N.W., and that this hill stopped its further progress. There is towards the N.W. an opening among the hills through which it might have been floated towards its present site.

4. A little lower down the valley (Rossdale), and on the same side, at a height of about 630 feet above the sea, there is a rocky knoll somewhat flat on the top, and presenting an area of about 8 or 9 yards in diameter, on which are five or six boulders lying pretty close together, as shown on fig. 5, Plate I. The boulders are granite, the knoll is mica schist.

5. At a still lower part of the glen there is a steep hill sloping down to the river. Near the top of this hill, and on the very edge overhanging the river, a boulder rests at a height of 300 feet above the river. The boulder is of granite, about 20 feet in diameter. It rests on mica slate rocks, which form a smooth surface sloping down towards the river at an angle of about 30°. Its position indicates transport from the north, as the land there is low enough to have allowed it to be floated over, whilst high hills to the south exclude that direction.

In the valleys where these boulders lie, there are some remarkable terraces. They were made known to the Convener by Captain Burke, R.E., two years ago, when he was still at the head of the Scotch Ordnance Survey. The surveyors employed in drawing the contour lines for the maps were struck by the horizontality and continuity of the terraces. Captain Burke was so obliging as to draw on the Convener's map lines to indicate their position. As these terraces suggest important views bearing on the position of the boulders, and their mode of transport, it seems not irrelevant to record the notes supplied by Captain Burke and give a copy of the map, to show where the terraces are.

Before, however, describing these higher terraces, it may be right to refer to certain flats at the mouths of the valleys near the sea.

The town and village of Glenelg are situated on a flat which prevails all along the Scotch coasts, about 11 or 12 feet above high-water mark. Between Glenelg and Glenbeg the base of some high rocky cliffs is at the same level.

Mr Fraser, schoolmaster at Glenelg, having learned that the Convener was desirous of seeing examples of flat land, conducted him to the following spots:—

(1.) At Glenbernera, about half a mile to the north of Glenelg, there is a well-defined flat, about 44 or 45 feet above high water. A corresponding flat occurs at many other parts of the coast.

(2.) Behind and above the new schoolhouse at Glenelg, there is a considerable extent of flat land, at a height of 72 feet above high water. On the opposite, *i.e.*, the south side of the valley, which is half a mile distant, there is a flat at exactly the same height, judging by the spirit-level. The river has cut through this flat. Its original formation cannot be ascribed to river action.

Beyond the manse and church, there is another extensive flat, 88 feet above the sea.

In a higher part of the valley, there are terraces on a smaller scale. If they slope with the river, as they seem to do, *they* probably had been formed by the river, when it ran at a higher level, that is, when the sea also stood at a higher level than now.

Near the mouth of Glenbeg, about a mile from the sea, there is a great mass of detritus, through which the river Beg has cut its channel. There is a flat here also on each side of the river, the level of which is about 120 feet above high water.

Fig. 6, Plate I. is (from memory) a plan of this valley. The parts marked *a*, *a*, &c., are patches of detritus, the tops of which are all on the same level, or very nearly so, viz., 50 feet above the sea.

The whole valley apparently had been filled with detritus, when the sea stood at least 150 or 200 feet above its present level. As the sea retired, channels were cut in the detritus, not only by the main stream now occupying the valley, but by the numerous and rapid side streams from the steep mountains which enclose the valley on both sides.

At about 3 miles from the sea, the place in Glen Beg is reached, where Captain Burke states he noticed a horizontal terrace on both sides of the river, at a height of 330 feet above the sea. It is marked (A) on plan, fig. 7, Plate II.

The Convener recognised a terrace on the right bank at 338 feet, but he could see none on the opposite or left bank. At a little distance farther up, there are on the left bank gravel knolls at a somewhat higher level. At this place, the channel of the river is about 40 feet below the terrace, and is of rock, which has of course prevented any deeper cutting of the drift beds.

At the junction between Glen Beg and Glen Rossdale (B) in the plan, there are very large knolls of detritus with flattened tops.

From the highest of these knolls, the Convener, on looking across the valley in a direction by compass E. by N., descried a terrace, continuous for about 80 yards, and apparently horizontal. Its position is indicated on the plan by five small vertical strokes. When the spirit-level was turned in a direction about E. by S., it struck on another horizontal terrace, about half a mile distant. All these flats are at one height, viz., 528 feet above the sea.

Higher up Glen Rossdale on the left bank, and at a spot about $1\frac{1}{2}$ mile from (B), there is an extensive flat, which had been marked by Captain Burke. He states it at 750 feet above the sea. The Convener made it 760 feet. When a person is on the terrace, it is not distinctly traceable for more than 300 or 400 yards; but when viewed from the opposite side of the valley, at a distance of about a quarter of mile, it can be distinctly traced for more than a mile continuously; and at its east end it is seen to cross the ridge which divides Glen Beg from Glen Rossdale.

In a higher part of Glen Rossdale, and still on the left side of the glen, the Convener observed traces of a shelf at 853 feet, with a steep slope or bank below it of about 30 feet in height. The Ordnance surveyors observed traces of a horizontal terrace still higher, viz., at 1500 feet above the sea. The Convener, looking in that direction, observed, at a distance of about 3 miles, something like a horizontal line running for nearly a mile continuously at what might be about that height.

On the plan, Captain Burke indicates as existing in adjoining glens, traces of the 330 feet terrace by the cypher 0. These glens the Convener had not time to visit.

He has also put a X at the head of three several glens to indicate that at these places, and at a height of 750 feet, there are gravel heaps.

Some quotations may be made from Captain Burke's letter, dated 25th August 1876 :—

"I was up Glenelg yesterday. There is evidence of the sea having stood at more than one height, considerably above its present level.

"The only terrace of any consequence is in Glen Rossdale. It is about 300 yards in length. It has nothing in the least resembling Glen Roy.

"I will now answer your questions :—

1.—Height above the sea—

(1) Principal terrace, about	750 feet.
(2) Another, very doubtful, . .	520 ,,
(3) Some rather more apparent, .	330 ,,

"This terrace, at the head of Glen Beg, affords strong evidence of a beach, such as now exists in all sea lochs hereabouts. Beds of gravel at 330 feet are cut through by the stream running through the valley.

"On crossing the high neck, 450 feet above the sea, and descending into Glenmore, similar beds were found at the head of that valley, at the same altitude.

"The spot marked in my sketch IIIII at 530 feet is very doubtful.

"The 750 feet terrace is visible in patches in Glenbeg and Glen Rossdale. I tried to trace it down Glenmore, for I have no doubt the land between these glens was once an island. But, although I fancied I found a mound sometimes, it can't be traced.

"The longest vestige of a terrace which I saw, is in Glen Rossdale, viz., about 300 yards; for the rest, there is only a mound here and there on the hill side.

"As to the width of the terraces, the greatest I saw is about 30 yards.

"You ask how high up these hills is sand and gravel found? I saw appearance of gravel at over 1200 feet. There are gravel heaps at 750 feet at the heads of the valleys marked X, at the spots one would expect to find them, and also at ⊕, apparently washed when the country was under water, and since cut through by the streams in the valleys. In fact, all the appearance is as if these valleys were once sea lochs, just as are Lochs Nevis and Hourn at the present day. These marks are frequent throughout the Highlands. I have seen similar gravel-beds along streams in several other glens.

"Whether it will be decided to survey the shelf, I cannot say. But there is nothing definite except the bit in Glen Rossdale; and a surveyor would not find it easy, when on the hillside, to know what mounds he should show, unless he had previously run a contour at the required height.

"I have made two sketches, one of Glenbeg looking east, another of Glen Rossdale looking west, which I shall be happy to show you."

With reference to these last remarks by Captain Burke, it occurs to the Committee to express an opinion that, when the Ordnance surveyors discover on the hill sides terraces of the kind referred to, there should be some record given of them on the maps, accompanied by a contour line at the same level along the adjoining hills, so that it might be seen whether there are separate patches of gravel elsewhere at the same height. It is also desirable that when the officer at the head of the Survey verifies what the surveyors have found, and makes sketches of the terraces, these sketches should be given with the maps when published.

In walking down Glen Rossdale valley, on the right bank, the Convener fell in with a large mass of detritus, cut up into a series of knolls by the action of streams and rain. The height of these knolls above the sea was on an average 858 feet—agreeing pretty nearly with the level of the shelf already noticed as existing on the opposite side of the valley.

These remains of gravel in Glen Rossdale and the adjoining glens, looking to the height and the form in which they occur, seem conclusive as to the occupation of these valleys by the sea; and they confirm the inference derived from the position of the boulders, that the boulders were probably *floated* into these positions.

The Convener was at first puzzled to account for the circumstance that most of the large boulders which he saw in these valleys were not upon drift, but upon bare rock; and in many other parts of the country, the same thing occurs. If these boulders were floated by ice and thrown down, they must most generally have fallen upon the detrital beds then forming the sea-bottom, and not upon bare rocks. When the sea retired, the boulders would then be on the drift, or buried in it. But when the streams from the hill sides began to flow and to remove the drift, the boulders would sink until rock was reached by them, where, of course, they would remain. The denudation of the old sea-bottoms has been everywhere so extensive, that very probably most of the boulders now existing are not in precisely the exact positions which they occupied when originally deposited.

6. *In the Pass of Brander*, where the River Awe flows out of the lake of the same name, there are several boulders deserving notice.

On the right bank of the river, near the spot where there is a pier for the small lake steamer, there are two terraces on drift. Both terraces have boulders on them. The boulders are of reddish granite. The rocks in the Pass are a slaty schistose rock like greywacke. The boulders have apparently come from some distant region, as the granite of the Loch Etive hills is not red, but almost entirely grey in colour.

The height of Loch Awe above the sea is (by Ordnance Survey) 110 feet. The lowest terrace is 68 feet, the higher terrace about 120 feet above the loch.

Both terraces appear to be horizontal. They can be traced for nearly a mile continuously.

On the opposite or left bank of the river no corresponding terraces are distinguishable. That side of the valley consists of nearly bare rock, and is almost vertical, so that there cannot be expected to be on it any trace of a beach line.

Have these terraces in the Pass of Brander been formed by a lake or by the sea? In a lower part of the valley there is a large amount of detritus, and it reaches at some places to a higher level than the terraces. The valley in that lower part is narrow, so that there might have been a blockage for a lake. On the other hand, how can the granite boulders be accounted for which are on the terraces? If, as seems most probable, they have come from the north, they must have been floated by ice on a current flowing from the N.N.W.

7. *Inveraray.*—His Grace the Duke of Argyll (Nov. 1876) conducted the Convener to a small hill, about 1000 feet above the sea,

at a place called "*Brae Leckan,*" 7 miles west of Inveraray, well covered with angular boulders. The boulders were of the same nature as the rocks of the hill—a dark grey porphyry. But the boulders had evidently been transported to the hill from some other place, there being no cliff from which they could have fallen. The Duke thought they had been floated from the eastward, and in that direction certainly the land was lower than in any other direction. But the Convener observed that towards the west there was an opening among the hills low enough and wide enough for a current to have flowed to and over the hill on which the boulders rested.

8. A few miles to the *north of Inveraray* there are some huge boulders of a coarse conglomerate, quite distinct from any of the rocks in the immediate neighbourhood. The rock of these boulders is a greenish or grey coloured Silurian rock full of quartz pebbles. One of these conglomerate boulders, weighing about 60 tons, is on flat ground about 800 feet above the sea, and resting on gravel. Another, $10 \times 9\frac{1}{2} \times 7\frac{1}{2}$ feet (weighing about 48 tons), is on the left bank of the River Arey, and about 180 feet above the sea.

The gamekeeper, who pointed out these boulders, said that there was no rock of the kind composing them, except at a place about 6 miles due west. Between that spot and the sites of the boulders there were several ranges of hills and valleys.

When the subject was mentioned to the Duke of Argyll, he corroborated the gamekeeper's statement. He informed the Convener, that there is conglomerate rock, of the same character as that of the boulders, on the summit or ridge between Loch Awe and Loch Fine, which lies to the north-west of the boulders.

On the tops of several of the hills to the north-west of Inveraray, about 700 feet above the sea, boulders were noticed where it was manifest, from their peculiar position, that they could have got into it only by coming from the west. Sketches of these were taken.

9. *Oban and Neighbourhood.*—(1.) At Dunolly, close to the sea shore, there is a grey granite boulder $12 \times 8 \times 6$ feet. It is about 20 feet above high water, and rests on an old sea beach. Its longer diameter points W. by N.

The nearest rocks of grey granite are in Loch Etive, situated to the eastward. There are ranges of hills between Loch Etive and the

Fig. 1 Boulder at Glenelg. View of West Side

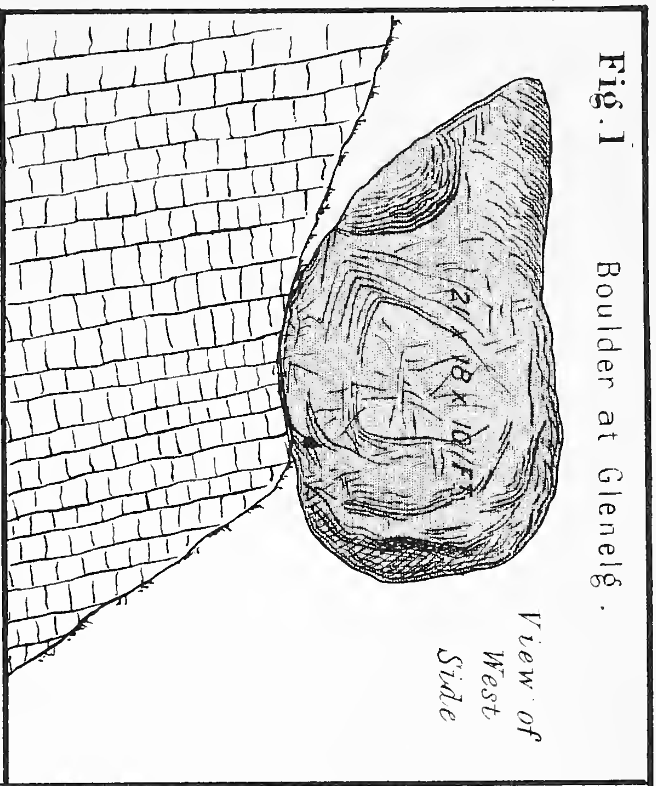


Fig. 2 Boulder at Glenelg. View of East Side

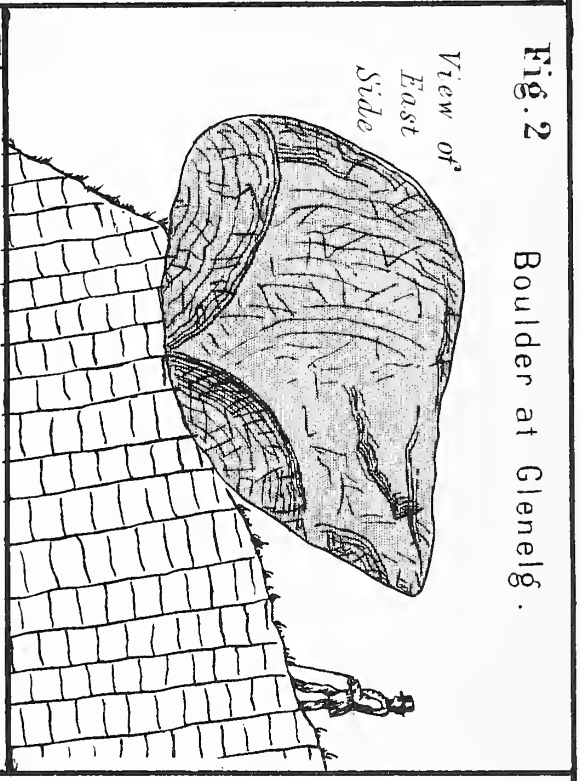


Fig. 4 Glen Rossdale.

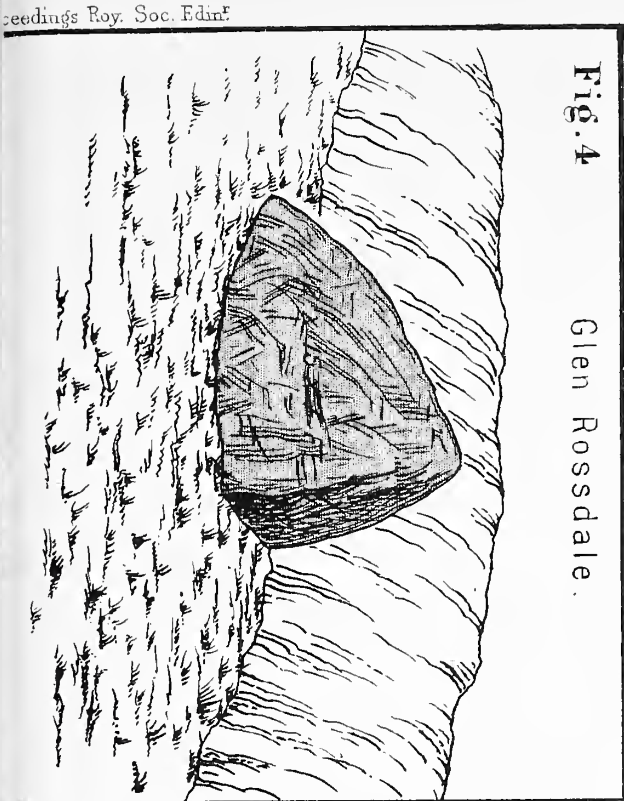


Fig. 3 Boulder at Fas-na-Cloich, Loch Creran

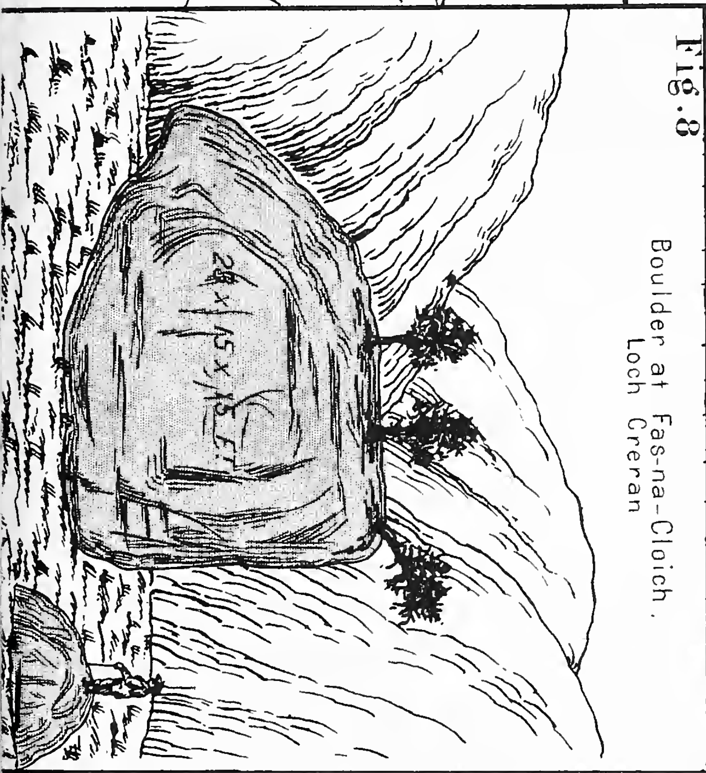


Fig. 5 Glen Rossdale. Boulders on Hill top.

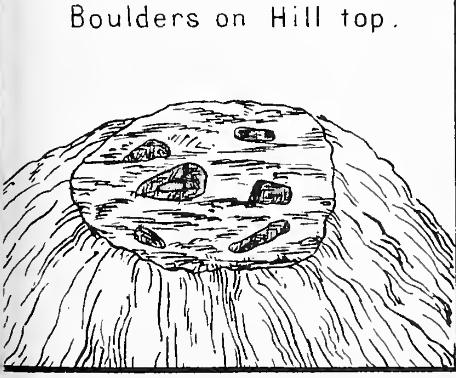
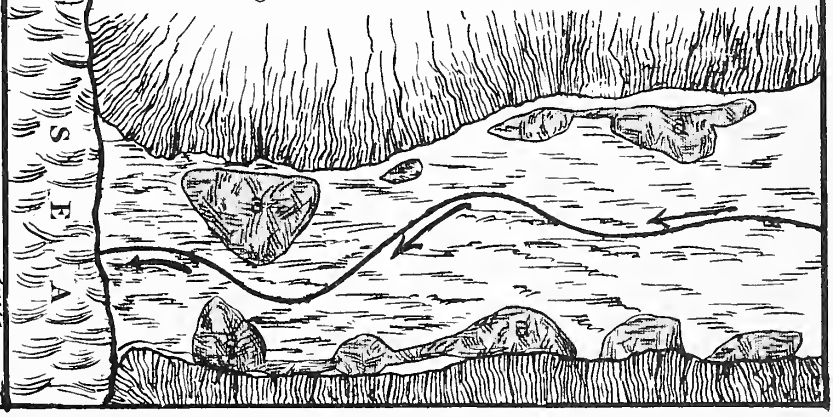


Fig. 6 River Beg with remains of old lake bottom.



W.&A.K. Johnston.

Fig. 3

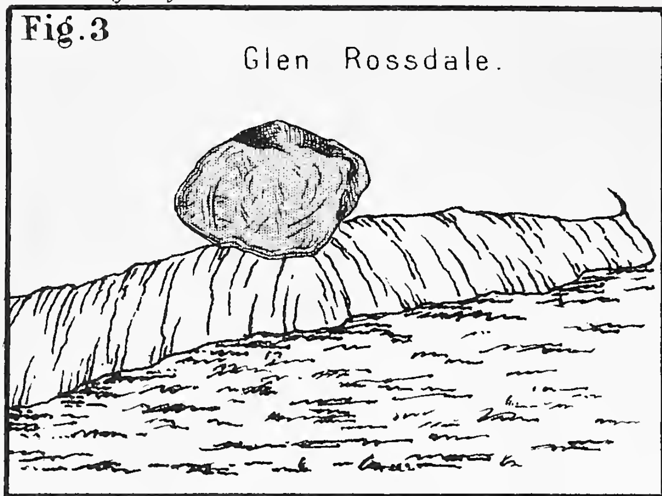


Fig. 13

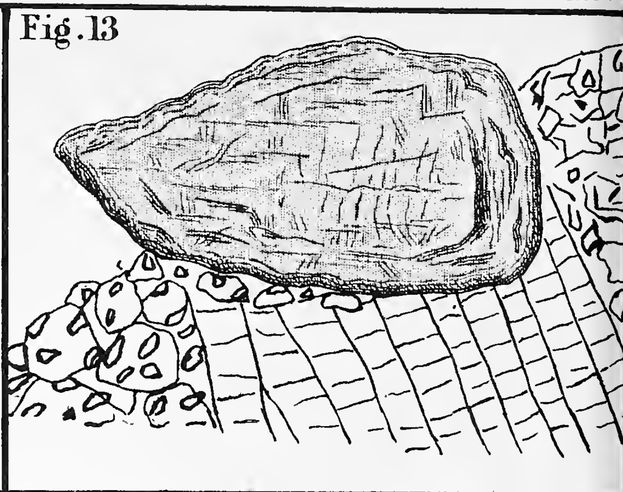


Fig. 14

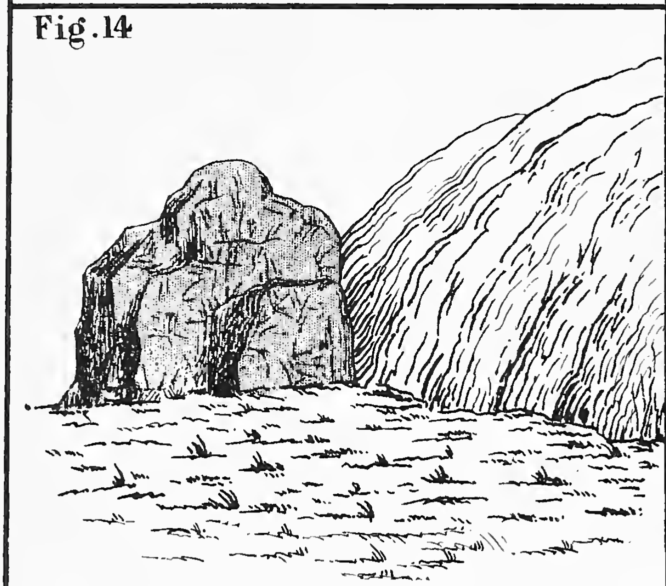


Fig. 15

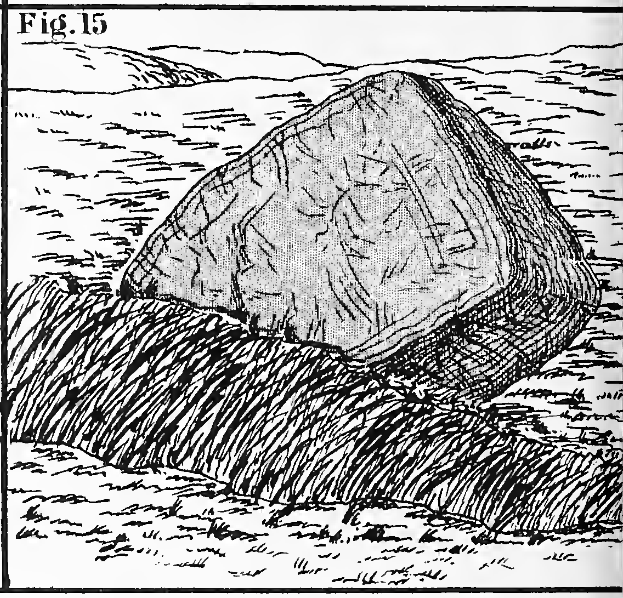
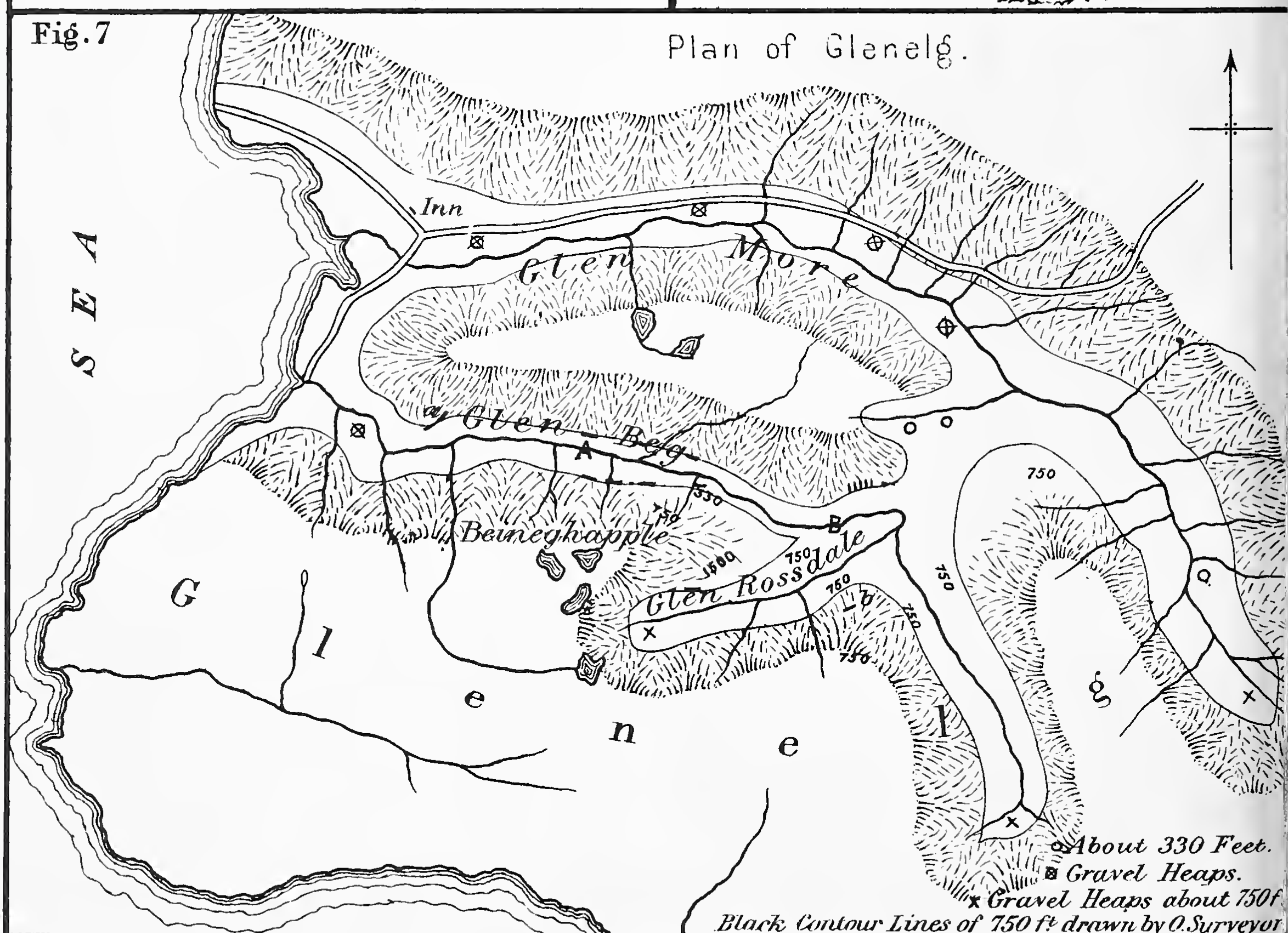


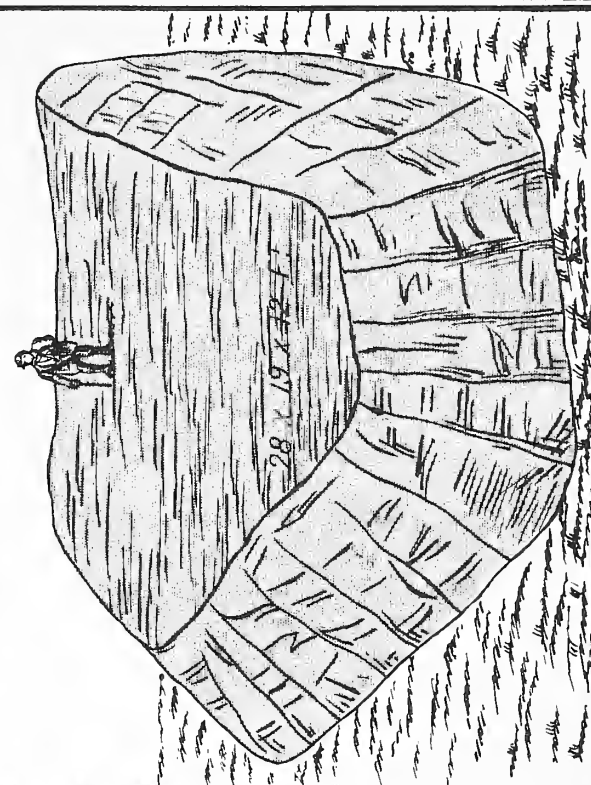
Fig. 7



W. & A. K. JOHNSTON, EDINBUR

Boulder, Kerrera, intercepted by rock & gravel cliff on S.E side.

Fig. 11 Boulder Loch Lomond.



W. & A.K. Johnston, Lithog.

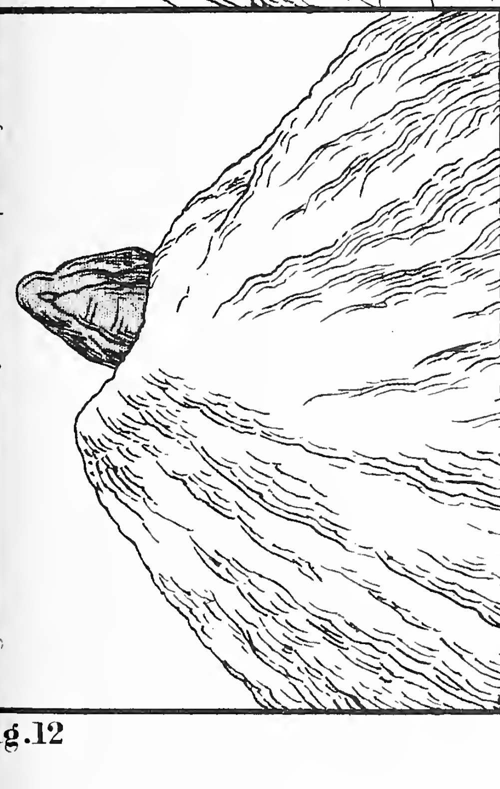


Fig. 16 Joints in rocks, smoothed by agent from West. Joints facing East rough. (Loch Tay)

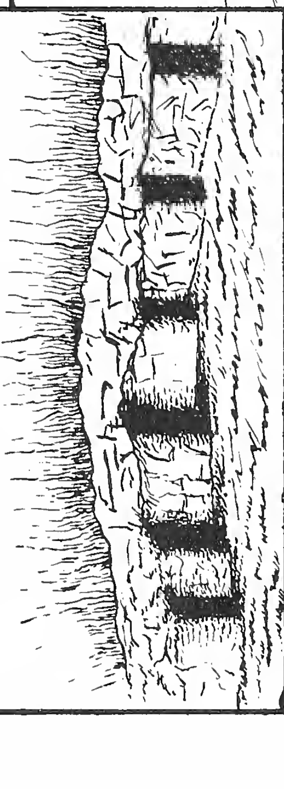
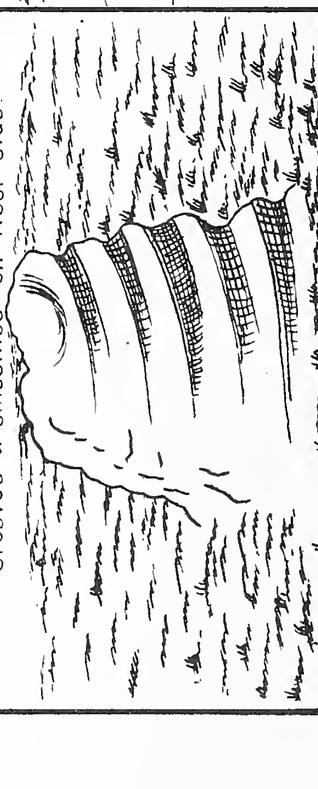
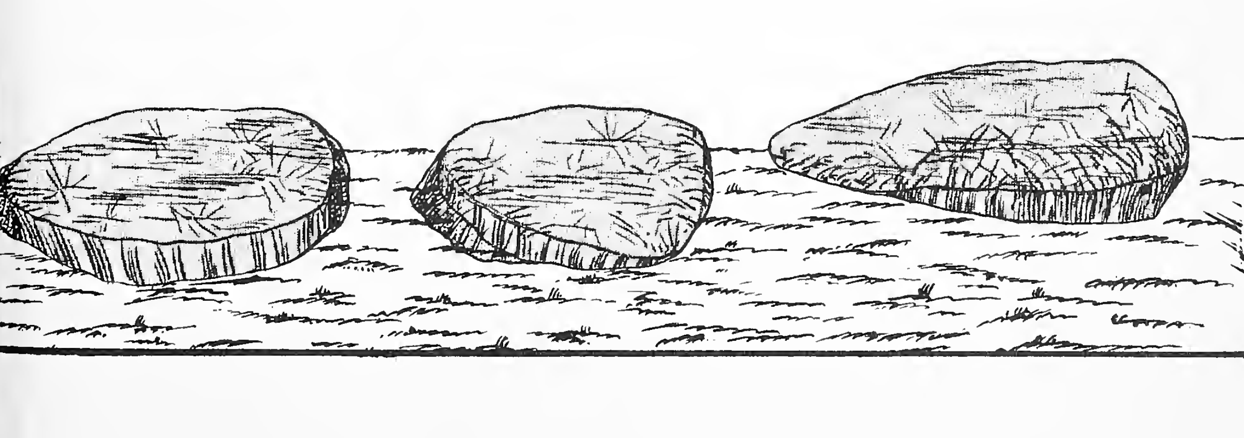


Fig. 17. Glen Dochart, Rock 1386 FT. above Sea. Grooved & smoothed on West side.



[F]ig. 12

Boulders lying on a Moor on West side of Loch Lomond



site of the boulder. Moreover, the boulder is close to the foot of a high rocky cliff, which being on the east side of the boulder, must have prevented the boulder reaching its site, except by transport from the westward,—probably the north-west, as the island of Kerrera is situated to the west and south-west, and would prevent the boulder coming from that direction.

The Convener was accompanied by a gentleman resident in the neighbourhood (Mr Clerk), well acquainted with the Loch Etive granite, who expressed doubts whether the granite of this boulder was of a similar composition.

(2.) The north part of Kerrera Island is strewed with numerous grey granite boulders, all well rounded. Most of them are on the beach, and on the old terraces adjoining the beach. There are some also, on Ballimore Farm, at heights of from 357 to 437 feet above the sea, on short terraces or flats of detritus facing the east and north-east.

In these cases there would be less obstruction to a transport from Loch Etive than in the case of the Dunolly boulder, but the range of hills near Glenlonan, reaching heights of from 500 to 1500 feet, still presents a difficulty.

If the Dunolly boulder came from a northern source, the Kerrera boulders probably came from the same quarter.

(3.) On the hills to the east and north-east of Oban, there are numerous boulders, chiefly of granite, whose position does not suggest one direction more than any other.

The rocks of these hills being clay slate, the boulders on them must have been transported from some distant quarter.

The granite is grey of different varieties, and very like the Loch Etive granite. But there are others, with large crystals of quartz and felspar, which betoken some other source.

One of this kind is on a hill to the south of the old public road between Oban and Loch Etive, at a height of 530 feet above the sea. It is extremely angular, and rests on a bare rock of the hill. This position would most easily have been obtained by floating ice.

Besides these *granite* boulders there are some of *dark porphyry* and of quartzite; which most probably come from the north.

This conclusion is somewhat strengthened by the circumstance that in this district, where the rocks are smoothed and striated, the surface of the rocks slopes down towards and faces the north, and the striæ run north and south. Examples occur on the old public road before-mentioned.

(4.) An angular boulder of grey granite, $11\frac{1}{2} \times 7\frac{1}{2} \times 7$ feet, occurs at Inverlievern, on Loch Etive, above Bonawe Ferry. This boulder rests on three or four smaller granite boulders, and these again on bare granite rock. There is no hill from which it could have fallen. It must have been transported. A sketch was taken.

It rests on the 40 feet old sea-margin, which is visible round the greater part of Loch Etive.

(5.) The Convener was informed of two very large boulders in the district between Loch Etive and Glen Lonan, at places called Auchnacoshen and Duntarnichan. But he was unable to reach them.

10. *Fasnacloich on Loch Creran.*—Captain Bedford, R.N., wrote to the Convener, calling his attention to a large boulder which he had seen when surveying for the Admiralty. He sent a description of it, and mentioned that its average girth was 30 feet.

The Convener discovered the boulder. It had recently been blown up into four or five fragments, with a view to being used for building a bridge. But they were found unsuited for the purpose, being too hard for masons' tools. The rock consisted of a dark porphyry, with which the Convener was unacquainted.

Mr Hall, the tenant of the farm of Fasnacloich, on which Captain Bedford's boulder was situated, conducted the Convener to a spot, about a mile higher up the glen, where there were multitudes of much larger boulders of the same species of rock. The spot proved to be a mass of detritus, consisting of water-borne gravel, forming a sort of terrace abutting against the hill, which forms the north-east side of Glen Creran. This terrace is covered by numerous boulders, some of very large size. A view of one of them is given in fig. 8, Plate I.

This boulder has the Celtic name of "*Fas-na-cloich*," or "*Fas-na-clach*," which means "stone with growth,"—referring to three trees growing on it, two on the top being firs (each about 15 feet high),—one at the side, a stunted oak. The name of the farm occupied by Mr Hall, and of the residence of the proprietor, Captain Stewart, is Fasnacloich, so-called, most probably, after the boulder.

The boulder with the three trees on it is about 25 yards in girth; its length is 23 feet, its width 15, and its height, in so far as visible above ground, is 15 feet.

Another boulder, a few hundred yards to the south, measured (above ground) 18 × 18 × 12 feet.

It deserves notice that the sharpest end of each boulder points in the same direction—viz., about S.W. (magn.)—*i.e.*, towards the mouth of Loch Creran.

The terrace on which these boulders lie, is about 290 feet above the sea.

Hall mantioned that, in a higher part of Glen Creran, the boulders are more numerous, and some of them larger than the two examined.

All the boulders appeared to be of the same species of rock. Hall, who evidently had some practical knowledge of rocks, called it a black granite, and affirmed that there was no granite like it in all that district. The rocks of the mountain on the opposite, or south side, of Loch Creran, rising steeply to a height of above 2000 feet, he knew were a grey granite. The Loch Etive granite, about four miles to the south, and the Durra granite, about eight miles to the east, being of a light grey colour, he had always wondered where these dark coloured boulders could have come from.

The rocks in Loch Creran, and in the hills immediately adjoining, are a blue schistose clay slate, with a rapid dip.

One or two other points may here be noted, communicated by Hall :—

A small river runs into Loch Creran, at its head, flowing out of a small fresh-water lake, which is separated from the sea by a spit of gravel and sand, crossing the valley, and cut through by the river. The sand, Hall stated, is full of sea-shells, and so is the bed of the lake, and even the channel of the river before reaching the lake. In this last-mentioned river, the shells are in a bed of fine clay—whitish in colour, which is used as a manure for arable land. In fact, it is this bed of shell clay which originated the name "*Crer-an*," i.e., "*Clay*," or "*Chalk River*."

These facts indicate, of course, a period when the sea stood at a higher level—to the extent of at least 20 feet, which is about the height of the shelly bed above mentioned. When the sea fell to its present level, a blockage of drift, now between the sea and the lake, caused the lake to be formed, with an overflow by the river, which runs out of the lake into the sea. There are several other places in the West Highlands where there are fresh-water lakes close to the sea, formed in like manner.

With regard to the boulders, it occurred to the Convener, judging from their locality and their position, that they had probably been floated up Loch Creran, and been then stopped in their further progress by the contraction of the valley and the higher level of the land.

But if they were floated up Loch Creran, from what quarter did they come? It was natural to look to places facing the mouth of Loch Creran, if in these places there were mountains composed of rocks similar in composition to the boulders. The island of Mull, situated to the W.S.W. of Loch Creran, seemed therefore to be one locality which might have supplied the boulders, as from Mr Judd's instructive paper on Mull,* describing numerous varieties of granite in the mountains of that island, it appeared likely that rocks of the same character as the Fasnacloich boulders existed there. With the view of testing this idea, the Convener sent specimens of the boulders to Professor Judd, who he heard had, during the past autumn, spent three months among the Mull mountains, and asked him to state whether he recognised the rock composing these boulders as being identical with, or at all events similar to, any of the Mull rocks? Professor Judd was so obliging as to respond to the application.

With the Fasnacloich specimens, there went to Professor Judd specimens of the rock composing two very large boulders on the shore at Appin, which rock the Convener found on examination to be the same as that of the Fasnacloich boulders. These Appin boulders lie on upturned blue clay slate rocks. Their shape indicated that they had undergone great friction, in consequence probably of being rolled over the sea-bottom by icebergs floating through what was then a sea strait, but now the Linnhe Loch, and the chain of lakes forming the Caledonian Canal. Sketches of these Appin boulders were taken. The largest is 15 × 11 × 10 feet. Both boulders are well rounded at the angles.

Professor Judd's Report is in the following terms:—

"*Appin Boulders, No.* 1.—This rock is not a granite, but a rock of basic composition. It appears to be a gabbro with some black mica. It is very similar in character to the gabbro of Skye, Rum, Ardna-

* See "Geolog. Society's Trans."

murchan, and Mull, which are described in my paper. I think there is no room to doubt it was derived from one of these localities —the rock is so peculiar and well characterised.

"*Fasnacloich Boulders,* Nos. 2, 3, 4, are very ordinary gabbros, such as form great mountain masses in Skye, Ardnamurchan, and Mull. These rocks are of a striking character, and differ from any which I know of on the mainland. I think it is certain, they were derived from the Western Isles."

Professor Judd, in his paper on the ancient volcanoes of Mull, Skye, and Ardnamurchan, refers to proofs that these volcanoes reached a greater height above the sea-level than any of the existing Scotch mountains, perhaps even to the height of 14,500 feet,* and that "denudation" had acted to an enormous extent in breaking up the old volcanic rocks and lowering their height. Professor Judd does not particularly specify the nature of the denuding agent which he supposed produced this effect. But if the sea with ice floating in it, at a height of say 2000 or 3000 feet above the present level, be allowed to be a denuding agent, it is easy to see how the boulders of Appin and Fasnacloich, if derived from Mull or Ardnamurchan, might have reached their present positions.

The distance of Appin and Fasnacloich from Mull and Ardnamurchan is about 30 miles. The intervening sea has in some places a depth of 100 fathoms. The island of Lismore, which is in this part of the Linnhe Loch, at one spot only reaches a height of 420 feet. A sea current flowing across Mull and Ardnamurchan, towards and through what is now known as "Glen na Albin," with mountains on each side of the Glen reaching to 2000 feet above the present sea-level, might, by floating ice, have carried boulders and lodged them in lateral valleys, such as Loch Creran.

11. *Crinan Canal.*—At the summit level, about half-way between the two extremes, there is a large accumulation of boulders, chiefly angular in shape. On the west side of the canal at the "*locks,*" a body of rock stands up, whose surfaces facing the north present marks of abrasion as if caused by some body or bodies passing over from the north. On the south side of this rocky knoll, lie a number of boulders which, if they came from the north on floating ice, may have been projected over the knoll by its intercepting the ice

* Quarterly Journal of the Geological Society for August 1874, page 259.

in its farther progress through this kyle or sea channel. One of the largest of the boulders is lying with its longer axis N. and S., or parallel with the general axis of the valley at this point. These conditions would be met by the sea standing at a height of from 140 to 150 feet above the present level. On both sides of the valley here there are horizontal lines traceable at that height, as if made by the sea.

12. *Island of Islay.*—The Convener, in August 1877, paid a visit to this island, for the purpose chiefly of examining the famed raised sea beaches on the adjoining island of Jura, and also of inspecting some boulders of which notice had been sent to him.

(1.) On the farm of Lossit, about three miles south of Port Askaig, there are four or five boulders of large size. Only two were seen.

One of these, 13 × 8 × 8 feet, is a composite rock containing crystals of quartz, augite, and hornblende. The stone is extremely hard; it was with much difficulty that a small specimen was detached. The boulder was resting on a bed of bright yellow clay, apparently a sediment of deep water. The rocks of the district are a slaty schist. On inquiry, it was surmised that rock of a similar kind existed near Kildalton, about 20 miles to the S.E. But doubt exists on this point.

The other boulder, scarcely so large as the foregoing, resembled a compact Silurian rock, containing numerous crystals of a whitish felspar.

There was nothing to indicate how or from what quarter these boulders came. Their height above the sea was about 300 feet.

(2.) On the farm of Arnahoo, about three miles north of Port Askaig, and 228 feet above the sea, a boulder stands conspicuously on the summit of a hill in a position most precarious (fig. 8, Plate III.). The rock composing the boulder is a hard porphyry, quite different from the rocks of the hill on which it rests. Its height above the sea is 228 feet, and the hill itself is about 300 yards from the sea, towards which it slopes very steeply.

The boulder is not absolutely on the highest peak of the hill, but a few feet below the peak, and on the slope which faces north by east (magn.). The only way in which the boulder could have stuck on this slope was by its coming right against it, and being let down on it gently, *i.e.*, without falling from a height. It must have come in a direction from N. by E. If floating ice brought it—and no other way is here conceivable—from the south, the boulder could not have reached its present position. It would have stuck on the south side of the hill. It could not have reached its position by a somersault over the hill top, for the impetus acquired by its fall would have projected it down the hill altogether.

As bearing on the direction from which this boulder may have come, it is proper to add that towards the north-west there is a range of hills, apparently much higher than 300 feet, whilst towards the north and north-east it is open sea, and the island of Mull is in that direction.

(3.) On the farm of Persibus (occupied by Mr Rounsfell), about three miles S.W. of Port Askaig, four or five boulders, well rounded, occur, and were seen. They are all of a hard porphyritic rock, differing from any of the Islay rocks. Their height above the sea was found to be about 228 feet.

With regard to the probable line of transport to their positions, it may be noticed that towards N. by E. there is an opening or depressed part of the country, through which the boulders might have been floated to their sites.

Mr Rounsfell pointed out a very large boulder situated on a hill slope to the north, about two miles distant, which, however, the Convener was unable to visit. But Mr Ballingall, factor on the Islay estate, has had the kindness to examine the boulder, at the request of the Convener, and he reports as follows:—"Girth, $33\frac{1}{2}$ feet; height, 11 feet; length, 12 feet; breadth, 18 feet. It lies on clay slate rocks, and is all exposed to view. Its *thickest* end faces S.W. Its height above the sea is 410 feet." Mr Ballingall has sent with his letter a small chip of the boulder. It proves to be an igneous rock, with much hornblende. It has probably come from some northern region. The weight of the boulder Mr Ballingal estimates at 25 tons.

(4.) On the south side of the high road between Bridgend and Port Helen a boulder rests at the foot of a low hill which faces about due north. The boulder is tolerably well rounded, and about 7 feet in diameter. It is a stranger to this district. Most probably it came from the north like the rest, and was in its farther progress intercepted by the hill at the base of which it lies. Its height above the sea is about 50 feet. (See fig. 10, Plate III.)

(5.) On the west coast of Islay, in the parish of Kilcheran, there are porphyritic boulders lying on the blue slate rocks, and so situated as to make it clear, that they have been brought and lodged there by some agency from the N.W.

Below the old parish church of Kilcheran a small stream joins the sea through a valley in a direction W.N.W. (magn.). The rocks on the south bank of the stream are ground down and striated in such a way as to show that some force has passed obliquely across the valley from N.W.

In regard to these Islay boulders, it is very apparent that they have all come from the north—some of them very probably from Mull. It is also rather remarkable that the largest should occupy sites very nearly on the same level, viz., 228 feet above the sea, a circumstance suggesting the same means of transport. As bearing on this last point, it may be observed, that on various parts of the Scotch coasts there are traces of old sea-beaches, at heights between 250 and 500 feet above the present sea-level.

13. On the *Peninsula situated between the Firth of Clyde* (*on the east side*), *and Loch Striven* (*on the west side*), there are several boulders of some interest.

(1.) At Dunoon and Kirn there are boulders of a micaceous sandstone rock, all well rounded, lying on the edges of the blue slate rocks which form the beach. One has had painted on it the words "*Jim Crow*," being 15 × 8 × 6 feet; another, the words "*John Bull*," 15 × 12 × 6 feet.

It was stated to the Convener by a local correspondent, that rock of the same nature as in these boulders occurs in the Holy Loch, situated about a mile to the north-west.

Two of the boulders on this part of the beach are so fixed as to indicate from what quarter they must have come into their present position, viz., from the North. Sketches of these were taken.

(2.) Along the shore towards Innellan there are numerous boulders differing from the rocks on which they lie. Some of these rocks show surfaces smoothed and striated, the striæ running north-east and south-west—a direction parallel with the general line of coast. Some local agency has, therefore, probably been at work here.

(3.) On the east shore of Loch Striven lies the large, well-rounded boulder, called "*Craig na Calleach*," or "*Stone of the Witch*"—the legend being that in former times, the witches inhabiting both banks of the loch, threw these great stones at one another. It is said that on the west bank of the loch, near Strome Point, and almost immediately opposite to "Craig na Calleach," there is a boulder of about the same size—which, however, the Convener was unable to go in search of. "Craig na Calleach" is a compact schist of a light grey colour, with thick nodules of quartz in it. The rocks of the beach on which the boulder lies are a slaty schist of a greenish blue colour. A sketch was taken.

(4.) On the farm of *Ach-na-foud*, situated about a mile from "Craig na Calleach," there is on the slope of a hill an angular boulder. It is at a height of 222 feet above the sea, and on the edge or verge of a precipitous bank. It rests partly on rock, and is in a very critical position. If the bank be now in the same condition as when the boulder was deposited, it must have been let down very gently or gradually, to avoid receiving an impetus which would have caused it to roll down the bank. A sketch was taken.

BERWICKSHIRE.

In the parish of Dunse the boulder clay has lately been cut through for some hundred yards to make a new road, and to a depth of 8 or 10 feet below the surface of the boulder clay. Beds of gravel and sand lie over the boulder clay, in some places to the thickness of 12 feet. On an inspection by the Convener (November 1877), in company with Mr Stevenson, Dunse, boulders in the clay were recognised as having all come from the west, chiefly W. by N. The Kyles Hill and Dirrington Hills porphyries were among these; the former is three miles W. by N., the latter six miles W.N.W. There were also sandstones with fossils, which Mr Stevenson knew to have come from a sandstone rock a few hundred yards to the westward, and which he pointed out to the Convener. The fossils were the ordinary plants of the coal formation, and an annelid.

DUMBARTONSHIRE.

1. *Loch Lomond.*—The large Mica schist boulder reported to the Committee by Mr Jack, and mentioned in the Committee's Second Report (Roy. Soc. Proc. for 1872–73, p. 152), was visited by the Convener, in company with Mr Smollett of Cameron House. Its

provincial name is "*Kerstone Galloch,*" it is situated on the farm of Callendoon, and is about 150 feet above the sea. Its length is 28 feet; width 19 feet; depth 12 feet.

It is shown on fig. 11, Plate III., with Mr M'Arthur, tenant of the farm, standing on it.

Originally, the boulder had been in a somewhat higher position. A small stream running past the boulder at its east side had washed away part of the gravel bed on which it had been resting,—so allowing it to sink.

With reference to the quarter from which this boulder was transported, Mr Jack suggested that if it came from the west, it must have come over hills from 1000 to 1200 feet high; and therefore he thought it more probable that it had been floated south down the valley now occupied by Loch Lomond, and then floated west up Glen Fruin.

It appeared to the Convener, that the line of transport was more likely from the westward. The land towards W. by N. (true), is on about the same level as the land to the north-east, as shown by the contour lines on the Ordnance maps of the district. If the boulder came from the westward, there would be no obstruction to its progress in a direct line; whereas, if it came from the north end of Loch Lomond valley, as suggested by Mr Jack, it must have changed its course to reach Callendoon.

2. On a moor, about half a mile to the north east of the above boulder, there are several smaller boulders of mica schist, of the shapes and sizes shown on fig. 12, Plate III.

It will be observed that they all occupy similar positions in respect of their longer axis, and their sharpest end. Their height above the sea is about 250 feet. The rocks of this district are Old Red Sandstone. There is much probability that these boulders had been left here by floating ice, in a current flowing from the westward; and that they acquired their bearings from the action of the current.

3. On the west side of Loch Lomond there is at Arden a low valley, which runs up from the Loch in a westerly direction. The summit level of this valley towards the west is about 150 feet above the sea.

Along the south side of this valley a number of boulders, chiefly of primitive rocks, have been deposited. They are at a height of about 94 feet above the sea. As usual, the most frequent position is here, as elsewhere, N.W. and S.E. for the longer axis, and the sharpest end towards the west.

4. In the policy of Cameron House a boulder of gneiss, $6\frac{1}{2} \times 5 \times 5$ feet, is lying on gravel, and at a height of about 55 feet above the lake, or 80 feet above the sea. Its longer axis is N.W. and S.E.

5. There is a hill called "Caer-man," about 3 miles to the S.W. of the south end of Loch Lomond. Its height above the sea is 720 feet. From its top, a good view is obtained of Helensburgh and Greenock towards the S.W.

The rocks on the top of this hill are a coarse porphyry. Huge fragments have been strewed in great abundance down the side of the hill sloping eastward, and especially S.E. The unmoved rocks present their west surfaces rounded and smooth, their east surfaces angular and rough.

On examining the separate blocks where heaped upon one another, it was apparent that the uppermost blocks, to obtain their positions, must have been projected on the others from the westward.

EAST LOTHIAN.

Linton.—On the farm of Drylaw, a greenstone boulder, $5\frac{1}{2} \times 3\frac{1}{2} \times 3$ feet, was found in cutting a deep trench through the boulder clay. The N.W. end was the most rounded. The longer diameter was N.N.W. (magn). There were no striæ on the top, but there were horizontal striæ on the two sides fronting the N.E. and the S.W. These two sides met in an angle towards the N.W. If a current had flowed from W. by N. the current would divide at the angle; and if ice floated in the current, the striæ on the two sides might have been produced by hard pebbles from the westward pushed against them. The smoothing and striation on the north were greater than on the south side. Close to the boulder there were pebbles of limestone shale, sandstone, and coal, which most probably came from the westward. The nearest greenstone rocks are on the Garlton hills, situated about 6 miles to W. by N. The boulder, therefore, most probably came from these hills.

About half a mile to south, there are rocks (viz., in Linton village, and in a railway cutting to the west), presenting smoothings and striations, made by some agent moving over them from W. by N.

FIFE.

1. *Isle of May.*—There are small Sienitic boulders on west side, at sea-level. On the west side there are also smoothed rocks. Direction of smoothing agent has been from W. ½ N. No boulders or smoothings are on east side.

2. *In Bogward Den* (Mr White Melville's property), 3 miles west of St Andrews, there is a boulder of conglomerate rock. Probably it came from Drum Carro Craig, which is said to be same species of rock, and situated some miles to N.W. The legend is, that the devil threw it from that hill, when the first Protestant church was being erected at St Andrews.

3. *At Kincraig, Fife,* there is on the beach a granite boulder, with girth of 23 feet and height of 4 feet. The lower half is angular, the upper half rounded. Has this boulder been floated from westward, and been stranded on the rocks at Kincraig? Stirling Castle, which is visible, bears W. ½ N. But it probably came from a more north-westerly direction. Fragments of this Kincraig rock (a trap tuff), have been carried eastward, and were found in the cuttings made for the railway 2 miles distant from Kincraig point.

4. *At Elie.*—Whinstone boulder on beach, 8 × 4 × 2½ feet. Its longer axis N.W. Striæ on boulder run N.W.

INVERNESS.

The Convener having been informed by the officers of the Ordnance Survey that some remarkable horizontal terraces had been discovered by them in Glendoe, a valley branching off from Glen Morriston, on the north side of the Caledonian Canal, he took the opportunity, when paying a visit to Mr Ellice of Invergarry, of going to Glendoe.

Under the guidance of two gamekeepers on the property of Mr Grant of Invermorrison, who reside at the foot of Glendoe, the Convener proceeded to the head of Glendoe, the place indicated in his map by the Ordnance Surveyors.

Unfortunately, a heavy fall of snow had (17th October 1877) occurred during the night preceding this visit, and it continued during the expedition.

There was at first some difficulty in making the gamekeepers comprehend the spot wished to be reached; and it was not till the party had gone some miles up the valley of the Doe, that the gamekeepers began to see what was sought for at the head of the glen. This was brought about by the Convener drawing attention, as he proceeded up the valley, to two lines of a terrace or flat noticed by him on a hill on the opposite side of the river. On his asking the keepers, whether there were similar lines at the head of the glen,—still about 6 miles distant, as they alleged,—the answer was, that there were such terraces, and so remarkable, that on one occasion, when accompanying a shooting party, some of the gentlemen remarked, that marks were there of Noah's flood!

The Convener was encouraged by this information, and in spite of snow and wind continued his progress up the glen.

The keepers stated that the marks to which they referred were on "English Hill;" and that though this hill was rocky on some parts, there was a great deal of sand and gravel near the top.

Following up the river Doe, a point was reached where the river divided into two branches, and called by a Celtic word meaning "Tongue of the Burus." The portion of the stream towards the right has the name of "Carriscreuch," or "Middle Corry;" and it was along that stream, flowing through what the keepers called "The Long Glen," that "English Hill" could best be reached. But the snow was here so deep, that no track was visible, and walking became dangerous, at least to a stranger.

At this point a consultation was held. The height above the sea reached was only about 850 feet, whereas the highest terrace marked by the Ordnance Surveyors was 1280 feet, and apparently still about 2 miles further up the glen.

The gamekeepers' advice was to abandon any hope of reaching the terrace, and to be satisfied with a distant view of the place, which could be obtained from a low hill in front.

This low hill accordingly was ascended, and with satisfactory results. The hill itself was found to consist, as shown by numerous scaurs, of fine gravel and sand; and on its flat top, the aneroid showed a height above the sea of 1190 feet.

This gravel knoll was as it were in an amphitheatre of hills, on several of which, towards the west, horizontal terraces were observed, at a somewhat lower level. These appeared to run continuously for about a mile. On the opposite side, the hill bearing about east showed a short line at the same level. Looking towards "English Hill" on the N.E. no terraces were discernible; the snow, owing to the direction of the wind (which was west), was so thick on the slope of the hill facing the knoll, that inequalities, if any existed, were undiscoverable. But one of the keepers pointed in the direction of the part of the hill for the terraces he had before spoken of. The part so pointed out seemed to be about 2 miles distant, and at an elevation of about 100 feet above the knoll on which the party were then standing.

The Ordnance Surveyors had marked on the Convener's map two lines of terrace, one at 1280 and the other at 1140, as existing on a hill on the left side of the Doe water. Though, from the circumtances above stated, it was impossible to make out these terraces, there was enough discovered to show a line at the lowest of these levels on the other hills adjoining—and the existence of detritus quite capable of being formed into a terrace at a much higher level. One of the keepers stated that on a hill towards the N.W. there were beds of gravel and sand to the very top, and without any covering of turf. He pointed in the direction of Ben Doe, which has an elevation of 2000 feet above the sea.

The Convener having on his way up Glen Doe observed several large boulders on the slope of a hill above him on the left hand, resolved to visit them on his way back. So, accompanied by one of the keepers, he ascended the hill, and in looking across the valley, he discovered four horizontal terraces on the opposite hill, and continuous for about half a mile. They were apparently on detritus, for at one spot, where a rock projected, there was an interruption.

The uppermost terrace the aneroid showed to be about 985 feet above the sea, the lowest about 895.

The first of the boulders visited was 919 feet above the sea. Its dimensions, roughly measured, were $14\frac{1}{2} \times 11\frac{1}{2} \times 7$ feet. It was a coarse reddish granite, and very angular. It could not have been rolled or pushed. It seemed to bave been carried from its parent rock, wherever that was, without undergoing any change of form. It was resting on gravel and sand.

The next boulder reached was at a height of 1204 feet above the sea. It also was a coarse red granite. It was about 30 yards in girth, and 14 or 15 feet in height. It is known as "The Glen Morrison Stone," probably because of being the largest boulder in the glen.

This boulder is on a flat, and in looking across the valley, a terrace is seen which corresponds in level with the boulder.

In several parts of the hill the boulders were in clusters or groups, piled over one another.

It deserves notice that all these boulders were resting on gravel and sand; and that the hills on both sides of the valley were thickly covered with detritus.

The gamekeepers spoke of a very large boulder at Clachnaharry, about 16 feet high, on the south side of Loch Clunie, which is two or three miles to the west of Glen Doe. The Convener saw it through his glass.

It may be added here, that Mr Ellice of Invergarry informed the Convener of a large bed of pure white sand, which he could not distinguish from sea sand, existing on a property belonging to him in that district, at a height of about 1000 feet above the sea.

MID-LOTHIAN.

1. In September 1877 the Convener visited excavations on the *north side of Craiglockhart Hill*, about two miles S.W. of Edinburgh. His attention was drawn to them by Mr Hutchison of Carlowrie.

These excavations were in the boulder clay. A number of boulders had been exposed, and were still undisturbed in their original positions.

The largest was angular, the smaller boulders were comparatively round. The greatest number were of blue whinstone rock. Among the smaller boulders, there were some of sandstone. The contractor for a large new building about to be erected being present, had his attention drawn to the sandstone boulders, and was asked if he knew any rock of the same kind which was in sufficient quantity to be quarried? He said that the sandstone of Hailes Quarry and Redhall Quarry was the same rock as that of the boulders. On being asked to point out the direction of these quarries from where the boulder lay, he pointed in a direction which was N.W. (by compass).*

* These quarries are about a mile distant from the site of the boulders.

The height of these boulders above the sea is about 340 feet.

2. At *Granton Harbour* (on west side) a very large blue whinstone boulder lies on the beach at high-water mark, part of which only is visible, the rest being covered and concealed by the sea wall which protects the road. On the upper surface of this boulder, there are innumerable striæ, the direction of which is W. 3° S. (magn).

About 100 yards to the eastward, there is another whinstone boulder having an iron ring in it, by which boats or vessels may be moored. There are striæ on it running in the same direction.

Between these two boulders there are some strata of hard sandstone rock, portions of which have been ground down and show striations running also as above.

3. In the *New Docks, situated to the eastward of Leith*, there is an immense bed of boulder clay, which continues along the coast eastwards for some miles.

This boulder clay at the Docks is covered by a muddy sand in horizontal beds about 8 or 10 feet thick. On the surface of the boulder clay there is a bed of oyster shells, of large size. There is as usual on the surface of the boulder clay a great accumulation of boulders, these having remained when the upper portion of the boulder clay bed was washed away by the sea. Most of the boulders are well rounded. The largest I saw, a light coloured blue whinstone, measured 10 × 8 × 6 feet, and was estimated to weigh 18 tons. About nine-tenths of the boulders are whinstones, but there are also some of quartz, limestone, sandstone, silurian, granite, and black ironstone concretions from beds of shale. These boulders have evidently come from the westward. On a great many, there are ruts or striæ all maintaining the same direction, viz., W. by N. (magn.) Those which are longer than they are broad, have their longer axis in the same direction.

Among the boulders, there were two metallic in composition, which deserve special notice.*

One, nearly spherical, measures $7\frac{1}{2}$ inches in circumference, and

* The Committee have to thank Mr Hugh Campbell, who is professionally engaged in the formation of these new docks, for bringing to them the two remarkable balls here referred to, as well as for affording to the Convener opportunities for seeing the excavations.

weighs 24 oz. It was found about $4\frac{1}{2}$ feet down in the boulder clay, among the large boulders.

The other ball was even more spherical, its least girth being 30 inches, and its greatest 31 inches. Its weight was 54 lbs. It was found 10 feet below the top of the boulder clay.

Professor Crum Brown (Edinburgh University) was so obliging as to examine both of these balls for specific gravity and composition. He reports that the smallest ball is marcasite or white iron pyrites, and that its specific gravity is 4·63. It is entirely of pure ore, being apparently unmixed with any other substance.

With regard to the larger ball, the Professor has sent the following report:—

"The fragment of the large round stone which I took for examination had a specific gravity of 3·36. It consisted of a mixture of silica (not obviously crystalline) and iron pyrites, in the following proportions:—

"Silica, 52·3 } per cent.
"Pyrites, 47·7 }

"Calculating from these numbers and the sp. gr., it is plain that the pyrites must be in the 'marcasite' form, as 'pyrite' would give a considerably higher sp. gr.

"The sp. gr. of the whole stone, *i.e.*, the mean sp. gr., was found to be 3·28. It cannot, therefore, be a uniform mixture."

Mr Murray having kindly offered to examine, microscopically, this large stone ball, has sent the following report:—

"*Challenger Office, Teviot Row*,
May 1878.

"Dear Sir,—The microscopic section of the boulder is made up of crystalline particles of quartz and marcasite. The marcasite fills the interstices between the grains of quartz; and among the quartz there are pieces of mica. (Signed) John Murray."

The Convener paid two visits to the excavations in the boulder clay at Leith to examine the spot where these two remarkable balls were found. He saw the superintendent, who was directing the excavations, and also the "*navvy*" who found the larger ball. The latter pointed to a whinstone boulder, and said the "big bullet" was close to this boulder.

There can be no doubt that both balls had come with the boulders, and had been deposited with them in the great bed of clay which covers the rocks in this district. This bed extends for fully half a mile on each side of the Water of Leith at its mouth, and reaches to a depth in some places of nearly 100 feet.

The black ironstone concretions found in this boulder clay bed show marks of friction. There are strata of shale containing such concretions, two or three miles to the westward. These concretions, as well as the boulders of granite and quartz, clearly indicate transport on a large scale from the westward.

The Convener learns from Mr Robertson, C.E., Albany Street, Edinburgh (who planned both the Albert Docks at Leith, executed some years ago, and the new docks now being constructed), that similar metallic balls were found in the Albert Docks excavations. But he has no specimens of them.*

* Since the foregoing was written, the Convener has received from Mr Charles W. Peach, of 30 Haddington Place, Edinburgh, a letter regarding marcasite nodules, from which letter, with Mr Peach's permission, the following extracts are made :—

"In the Falkirk and Slamannan district a band of these nodules, known as '*Speckled Ball Ironstones*,' occurs. It occupies a horizon a few fathoms above that of the '*Slaty Band Ironstone*,' the base of the Coal measures.

"The direction of the *striæ* on the rocks and the carry of the boulders and boulder clay is towards the east, and varies from E. 10° N. to E. 15° N.

"Near Kilsyth, and about 2 miles to the west of that place, the tributaries of the Corrie burn cross an area of blue shales, with several courses of ironstone nodules. Some of these are of iron pyrites (marcasite), and are known among the mining population as '*brassy balls*.' They occupy a horizon between the Hosie and Hurlet limestones, near the base of the carboniferous limestone series.

"The direction of the *striæ* and carry of the boulders in this district is E. or E. 5° N.

"Either of these sources could supply *balls* at Leith, as they are right in the direction of the ice-flow.

"As to *concretionary balls in sandstone*,—there is on the coast of East Lothian near Cockburnspath, to the north of Cove, a cliff of calciferous sandstone full of spheroidal concretions, which weather out on the wasting of the cliff by the sea, and being harder than the matrix, they lie piled up in great numbers at the base of the cliff. Many of them are of very large size.

"Similar concretionary balls occur in sandstone rocks at Grange Quarry near Burntisland, from whence, no doubt, the ball found lately at Leith was carried. (Signed) "C. W. PEACH."

This information in regard to marcasite brassy balls, the Committee deems highly interesting. If the marcasite ball found in the boulder clay at Leith, was transported from any part of the district situated to the north

4. At *Alnwick Hill, near Liberton Church*, at an elevation of about 350 feet above the sea, extensive excavations have been made in the boulder clay for the new Edinburgh water-works. The boulders consist chiefly of fragments of rocks, which are known to be *in situ* situated in districts of the country to the west and north-west. The great majority of the boulders are of hard red sandstone rock, such as occurs at Grange and Merchiston, to the west of Edinburgh, though these places are at a lower level. There are boulders of marine limestone, similar to rocks of that description in Linlithgowshire. There is an immense quantity of blue-coloured greenstones and dark-coloured basalts, and also buff-coloured felspathic rocks. There are some small boulders of pure quartz, which probably hail from the Silurian rocks to the north-west of Callendar and Doune.

Many of the boulders occupy positions, present shapes, and bear marks of some interest.

The largest seen by the Convener were about 7 feet long by 4 feet wide, and 2½ thick or deep.

The boulders were all well rounded and smooth, but more particularly so on what had been the upper and the under sides.

Mr Black, the superintendent of the excavations, being aware of the interest attaching to the position of the boulders and the *striæ* on them, had, with a compass, ascertained that the long-shaped boulders, before being moved, generally were lying in directions varying between W.N.W. and N.W.; that the striæ, when such existed, were almost always parallel with the longer axis of the boulder; and that there were striæ, sometimes only on the upper side, sometimes only on the lower side, sometimes on both sides. In one of the boulders, well rutted on the under side, he had remarked that the ruts were deepest at the east end of the boulder, and that they gradually diminished in depth and numbers towards the west. This feature might be accounted for on the supposition that the boulder, whilst being pushed forward, encountered hard obstacles which produced deep ruts on the boulder when the first

of Glasgow, as suggested by Mr Peach, what was the transporting agent to suit those localities? A glacier moving from west to east by the action of gravity would be hardly conceivable. The levels preclude that agent. A sea current, loaded with floating ice, seems a more probable conjecture.

contact took place, afterwards the boulder would rise over these obstacles, and in consequence the striæ produced by them would diminish in depth.

5. *Tynecastle, near Edinburgh.*—A basalt boulder, $4\frac{1}{2} \times 4 \times 2$ feet, was discovered, striated on both upper and under side, but the ruts were much deeper on the under side. The under side ruts had begun to be formed at the east end of boulder,—the striæ on the upper side begun at the west end. This might be accounted for by supposing that the boulder had been pushed towards the east over hard rocks, and that floating ice from the westward had pushed stones over the upper surface. The smallest end of boulder pointed towards west. The sides of the boulder were well rounded.

This boulder lay in a hill of muddy sand containing many pebbles of all kinds, hard and soft, such as quartz, shale, and coal. Height above sea, 200 feet.—*Ed. Geol. Soc. Tr.*, vol. ii. p. 347.

PEEBLESSHIRE.

At the east end of the town of Peebles there is a boulder of white quartz, about 3 feet long, $2\frac{1}{2}$ feet broad, and with a girth of about 7 feet. It is now built into a wall. Previously to its being thus disposed of, the stone stood from time immemorial in an adjoining low hill, which in consequence had obtained the popular name of the "White Stone Knowe." It is alluded to as a boundary stone in a title deed dated in 1436. Mr Richardson, the Secretary of the Edinburgh Geological Society, who was the first to take public notice of this boulder, says that "the nearest beds of quartz are about 80 miles to the N.W." The boulder on its surface is smoothed and polished. It is, like many other boulders, rudely pointed at one end, whilst the other extremity is more broad and heavy. The height above the sea is 550 feet.—*Ed. Geol. Soc. Trans.*, vol. ii. p. 397.

PERTHSHIRE.

1. *Loch Tay.*—On the farm of Morenish, situated on the north bank of the lake, and about 2 miles from the village of Killin, there are several boulders worthy of notice.

Figs. 13, 14, 15, Plate II., are intended to show the positions and specialties of these boulders. They were at a height of about 1400 feet above the sea, assuming Loch Tay to be 300 feet.

These boulders had all come from the westward, viz., down the valley, as shown by the way in which they were fixed.

If the question be, whether they were brought by glacier or by floating ice, the answer is, that there is not much evidence either way. It may however be remarked, that if they were pushed forward by a heavy glacier, it is odd that the boulders should not have been carried further down the valley, and that the obstructions on their east side, against which they have stuck, should not have yielded under the pressure of a ponderous glacier. The boulders in figs. 13 and 14 were resting on detritus, and pressing against detritus only on their east sides. The boulders in fig. 15 was pressing against a hard rocky stratum of clay slate on its east side.

In several parts of the hill, smoothed rocks of mica schist occur, with knobs of quartz standing up above the general surface. Being harder than the mass of rock, they had resisted the friction better; these knobs were smoothed, the smooth parts being always on the west sides.

Fig. 16, Plate III., shows a rock with joints. The projecting angles facing the west have been smoothed by some abrading and grinding force.

2. *Glen Dochart.*—There are many boulders of considerable size, resting on detritus, and chiefly on the south side of the valley.

One near an old toll-bar measured, in so far as above the ground, $13 \times 12 \times 8$ feet, at a height of 630 feet above the sea.

At the small farm-house of Wester Lix, at a height of 660 feet above the sea, there is a flat or terrace, partly rock, partly detritus, on which there are several large well-rounded boulders, two of them a coarse granite, probably from Ben Cruachan.

On ascending the hill towards the south, a boulder, $12 \times 9 \times 5$ feet, was met with, at a height of 1116 feet above the sea. Its longer axis bore E. $\frac{1}{2}$ S., which is also the direction of the axis of the valley in this place. There being no rocky hill near, from which this boulder could have come, it has certainly been brought to the spot where it now lies, by some transporting agent.

At the height of 1250 feet there is a mass of rock on the same side of the valley, and nearer the top of the ridge, which has on it some noteworthy marks. The rock stands out prominently, and forms a nearly vertical cliff, as shown in fig. 17, Plate III. On the side facing the west, there are horizontal groovings, apparently formed by some force, which, acting on the whole mass, has worn down certain portions more than others, these portions being less compact, and so more capable of abrasion.

Such abrasion might have been effected by a body of water passing from the westward; and more readily, than by the solid ice of a great glacier.

On the top of the ridge forming the south bank of this valley (Glen Dochart), a cairn stands at a height of 1500 feet above the sea. A boulder of considerable size lies on the top of this ridge, on the east side of a projecting knoll. Has the boulder been stranded on what was the lee side of the knoll?

ROSS-SHIRE.

At *Auchnasheen* (Dingwall and Strome Ferry Railway,) there is a boulder about 15 feet in girth, which stands on a flat of detritus about 610 feet above the sea.

In this district, there are several other detrital flats, in sight of this one, all nearly on the same level. There can be no doubt that these flats have been originally one continuous plateau, which formed a sea-bottom. It has been cut through by several streams, the banks of which, about 18 feet high, show an enormous accumulation of gravel and sand;—sand, *below* (deposited probably when the water was deep); gravel, *above* (deposited when the water was shallower and more subject to currents).

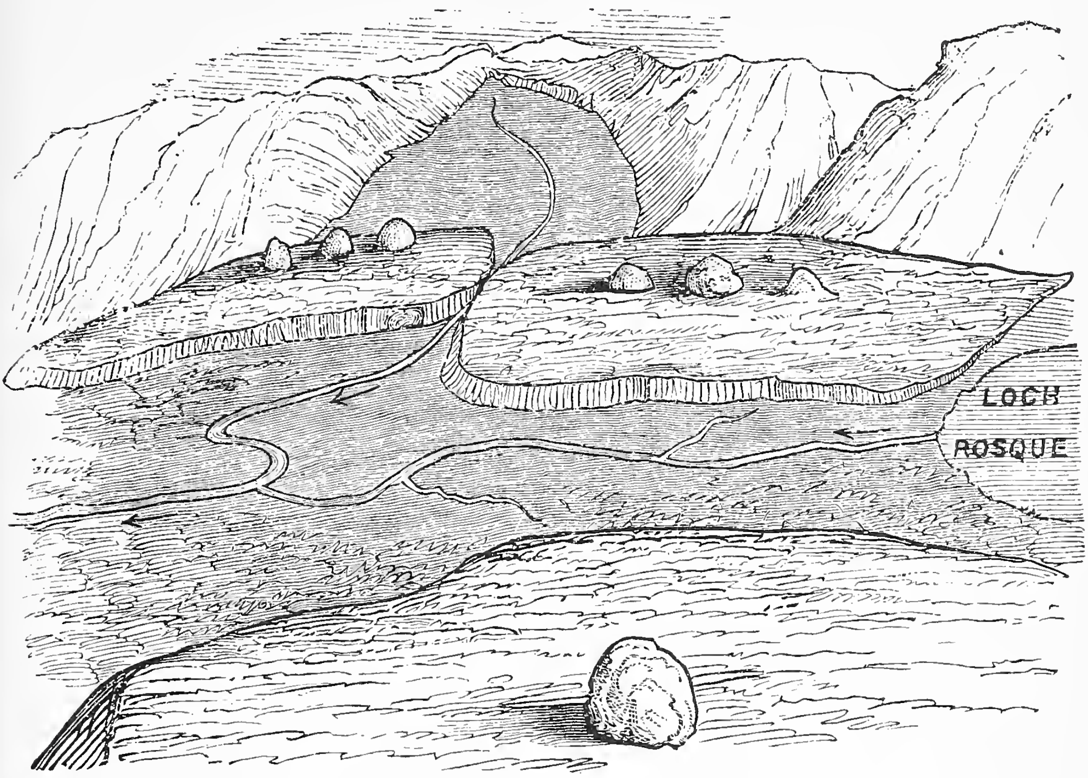
The annexed diagram represents a portion of these remarkable flats, —cut through by several streams, the principal of which flows from Loch Rosque,—situated to the north of the boulder. The knobs on the woodcut are intended to represent knolls of gravel or sand—remnants of a greater mass of these materials. The boulder is well rounded, and it has evidently come from a distant quarter.

Professor Nicol of Aberdeen has expressed an opinion * that the formation of these Auchnasheen terraces is due to the action of a great river flowing from the west. I regret to differ on this point from a geological friend; but I can see no grounds for that opinion.

To the east of Auchnasheen, close to the railway, there are several spots of rock evidently rounded by friction—whether by ice or by

* Nicol's Geology of Scotland, p. 69.

water, or by both, it is difficult to say. Their height above the sea is about 780 feet. On the hills, on each side of the railway, there are traces of horizontal lines on the detritus, which deserve better observation than could be given from the railway carriage.



STIRLINGSHIRE.

1. *On Sheriffmuir*, 3 miles from Bridge of Allan, near Blackford, there is said to be a large boulder, called Wallace's Putting Stone.

NORTHUMBERLAND.

It was intended that only Scotch boulders should be inquired after by the Committee; but it is not irrelevant to notice a boulder which, though now in England, was probably transported from a Scotch mountain.

In Chillingham Park, the seat of the Earl of Tankerville, near Alnwick, there are several small boulders of granite. The rocks of the immediate neighbourhood are carboniferous sandstones and limestones. The nearest point for granite is the "Big Cheviot," eight miles to the W.N.W., and reaching a height of about 1800 feet above the sea. The largest boulder is 3 feet 2 in length, 2 feet

4 in width, and 2 feet high. It is round in shape, and about 400 feet above the sea.

Several valleys and ridges of hills lie between Chillingham and the Big Cheviot, across which the boulder must have been transported to reach its present site.

Remarks by David Milne Home, LL.D., Convener of the Society's Boulder Committee, on presenting the Committee's Fourth Report at a Meeting of the Society, 20th May 1878.

1. In presenting a Fourth Report from the Society's Committee on Boulders, I may be allowed, first, to refer to the main object for which the Committee was appointed.

It was to collect data which might help towards a solution of the problem, by what agency boulders in Scotland had been transported from the parent rocks to the positions they now occupy.

The Transactions of the Society contain numerous papers by eminent geologists on this question.

At a very early period, Sir James Hall, when he drew attention to many large boulders, and also to the remarkable appearances called "crag and tail" in the midland districts of Scotland, ascribed both sets of phenomena to the agency of great bodies of water, which had passed over the country from west to east.

At a later period (about the year 1842), Agassiz and Dr Buckland started the idea, that as in Switzerland, glaciers had been the means of carrying masses of rock from the Alps across the valley of Geneva to the Jura mountains, so there might in former days have been glaciers in Scotland producing similar effects.

More recently a third theory was started,—that if the sea stood several hundred feet above its present level, floating ice might have been the means of transporting the boulders, and carrying them great distances.

2. There being thus three different theories of transport, each supported by eminent geologists, the Committee has endeavoured to gather facts to ascertain which theory is the most probable, or whether any better can be suggested.

I do not presume to say that the information contained in this and the previous Reports will yet allow the problem to be solved. But at all events it may be conceded that some new facts have been ascertained, which throw considerable light on the question.

I venture to indicate what appear to me to be several conclusions warranted, though in doing so I offer only my own opinion. Perhaps the Committeé, after more information has been obtained, may be induced to consider whether they will pronounce on the various questions of interest which the subject presents.

I confine myself this evening to only a few points, and chiefly to illustrate what occurs in our last Report.

3. The boulders referred to in the Report may be divided into two classes.

First, There are boulders which, from the nature of the rock composing them, are so soft and friable, that they could have been transported only short distances—such as sandstone, coal, and shale. In the Report, examples are given of such boulders, from Berwickshire and Mid-Lothian.

The *second* class of boulders, namely, such as are ascertained to have come from remote quarters, are composed of rocks, hard, compact, and homogeneous in composition; such as basalt, greenstone, granite, felspar, quartz, greywacke, and old conglomerate.

Boulders of these rocks have been found even as far as 80 or 100 miles from the parent rocks; and, generally speaking, they are well rounded, presenting evidence of enormous friction undergone whilst *in transitu;* and even in some cases acquiring almost a spherical shape.

Specimens of small spherical boulders are now on the Society's table.

There are, however, exceptions to the rule that boulders of hard compact rocks are generally well rounded. Cases of boulders of these hard rocks occur extremely angular in shape. Examples are shown in this Report, by the lithographs appended to it, and in previous Reports. These *angular* boulders are almost invariably at *high* levels, on the sides of mountains or near their tops. The *well rounded boulders* are generally at *low* levels, and most frequently imbedded in boulder clay.

4. It will be asked, whether the Committee has in any case ascertained the parent rock from which a boulder has come.

The answer is, that the Committee can in no case point out the particular rock from which a boulder had originally been broken off. All they can affirm is, that in several cases they have ascertained the *district* or *quarter* from which the boulder must have come.

(1.) For example, in Berwickshire, as will be seen from this last Report and the second Report, particular hills are specified from which boulders must have come. The direction in which they came, and the number of miles traversed, are therefore in these cases matter of certainty. In every case over the whole county of Berwick, from its lowest to its highest level, the direction of transport is from points between W. and N.W. (magn.)

The same is the case in Mid-Lothian. The sandstone boulders at Craiglockhart are shown to have most probably come from rocks situated a few miles to the N.W. The quartz and other hard rock boulders at the same place, as also at Liberton and at Leith, in like manner probably came from points between W. and N.W.

(2.) The two remarkable spherical balls of marcasite, found in the boulder clay at Leith and mentioned in this Report, must in like manner have come from the westward. A presumption to that effect arises, from the mere fact that they are in the same bed of clay which contains granite and other Highland rocks. But there is more than presumption. Mr Peach having indicated where pyrites balls might be found *in situ*, viz., at Campsie and Kilsyth, I went to Campsie last week, and on inquiry was shown some thin strata of coal, abounding in nodules of pyrites, several of the nodules so large as to weigh half a cwt. The coal is worked for burning limestone. It is too full of sulphur for domestic use. Specimens of this coal, with the pyrites nodules which I obtained on the spot, are now on the table of the Society.

Kilsyth I did not visit, because the overseer at Campsie told me that he had worked at Kilsyth, and that there were pyrites nodules in the coal strata there, similar to those at Campsie, but of rather smaller size.

Some of the nodules which I obtained at Campsie I submitted to Professor Crum Brown, that he might examine them to see whether they contained "marcasite." He has reported to me as follows:—
"These nodules have a specific gravity of 4·12, and consist of iron, sulphur, and coaly matter in the following proportions :—

"Iron, . . .	44·56 per cent.
"Sulphur, . . .	52·14 „
"Coaly matter, . .	3·30 „

Deducting the coaly matter, the iron and sulphur would be in the proportions in which they are generally found in 'marcasite,' viz.,

"Iron, 45·61; and Sulphur, 54·29."

As regards chemical compositions, therefore, the small metallic boulder may be considered as exactly agreeing with the nodules found in the Campsie coal strata. This agreement in composition affords a strong ground for inferring that the boulder had been transported from Campsie, or from Kilsyth, as suggested by Mr Peach.

With regard to the larger spherical ball found in the same bed of boulder clay at Leith, I am now able also to indicate the part of the country from which it was probably transported. Mr Hutchison of Carlowrie, happening to see this stone ball, informed me of two quarries in Linlithgowshire where concretions resembling it were in abundance. These quarries are near Humbie and Dalmeny, situated from nine to ten miles due west from Edinburgh. Mr Hutchison having sent to me several of these concretions, I was induced to visit Dalmeny Quarry. I found in the sandstone rock there, numerous concretions of all sizes up to nearly 4 feet in diameter. Humbie Quarry I did not visit, as the working of it had been given up, and it was full of water. A concretion from this last mentioned quarry, sent to me by Mr Hutchison, Professor Crum Brown has examined, with the following result:—"It weighs 17¼ lbs. It consists externally of a thin shell of sandstone, and internally of a mixture of quartz and marcasite, closely resembling the substance of the large ball from Leith. The mean specific gravity of the ball was 3·49."

There is thus a sufficient similarity of composition in regard to the stone ball and the Humbie concretions, to make it exceedingly probable that these Humbie sandstone rocks supplied the stone ball. I do not say that Humbie Quarry was the exact spot from which the stone ball found at Leith actually came. The sandstone strata which occur at Humbie and Dalmeny of course crop out elsewhere in the district near South Queensferry; all that can be said is, that the stone ball may have come, and most probably came, from some part of that district. Mr Peach mentions in his letters, quoted in the Report of the Committee, that similar concretionary balls occur in sandstone rocks near Burntisland, and suggests that the ball in question came from that quarter. In that case, the direction of transport would be from about due N. If the stone came from near South Queensferry, the direction would be from W.N.W., which last would be more in accordance with the evidence of direction indicated by many other data.

Assuming, then, as most probable, that the large stone ball, as well as the small metallic ball found in the Leith boulder clay, came from parent rocks, situated to the westward, the next question will be, by what agency were they transported?

Mr Peach, in his letter, apparently assumes, as matter of course, that these balls were transported by the agency of *ice*. But "ice" in what form?—land-ice, or sea-ice?

If the metallic boulder came from Campsie, the distance over which it travelled to Leith could not have been less than 30 miles; and as the Campsie coal strata are only about 150 feet above the present sea-level, there would not be gradient sufficient for a *glacier* either to carry on its surface, or to push before it, debris of rocks from Campsie to Leith. Moreover, Leith is not at or near the mouth of any valley which could create or guide a glacier from the west of Scotland.

But there are in the Campsie and Kilsyth districts marks of various kinds, indicating the action of a deep-sea current. These marks it is proper to notice, as having an important bearing on the general question of boulder transport.

Mr John Young of Glasgow, in the year 1868, wrote an instructive paper in the "Transactions of the Glasgow Geological Society," on the geology of Campsie. He says (page 14)—"There are few localities in the central district of Scotland, where such an extent of polished and striated rock surface is to be seen, as along the flat summits of the south hill of Campsie. The striæ vary in their direction from a few points north of west to south of west, according to the deflection of the ground;—many tracts of the sandstone rock, still showing the channelled markings in great perfection," at about 600 feet above the sea.

Mr Young then refers to the Strathblane Valley, which lies between the north and south hills of Campsie, and to the appearances indicating that it had been "*swept by powerful currents of water*, which have helped to produce those inequalities of surface seen along the outer margin of the tracts now occupied by the rivers Kelvin and Glazert. It was during the period when Scotland sat several hundred feet lower in the sea than it does at present, and when *the valley of the Kelvin existed as a deep sound connecting the German and Atlantic Oceans*, that those great beds of stratified sand and gravel were deposited which we now see filling up the Strath (as near the village of Torrance) to more than 100 feet above the level of the river. At other points along its course, similar deposits exist to more than 100 feet *below* the present sea-level. This shows that a *very deep sound or valley must have originally extended across Scotland, previous to the glacial period, in this particular direction.* A depression of the land to the extent of 350 feet would produce the following results :—The German and Atlantic Oceans would be united by the valley of the Kelvin, also by the valley of the Leven, Loch Lomond, and onwards by the low ground near Kippen to the Forth at Stirling. *A narrow sound through the Campsie valley* would connect the two seas, as the water-shed at Ballagan Bridge is only 330 feet. *The Campsie and Kilpatrick hills would then form two islands*, and the valleys of the Carron and the Endrick would be estuaries or arms of the sea. It is only by assuming conditions such as these, that we can hope to explain the superficial sedimentary deposits" (page 16).

In the year 1871, in company with Mr Young, I had an opportunity of visiting the Campsie district, and from my note-book I make the following extracts :—

a. On Craigend moor, at about 450 feet above the sea, situated two miles west of Strathblane, I found the sandstone rock presenting extensive sheets of smoothed horizontal surface, evidently ground down by friction, and presenting occasional striæ, running in a direction S.E. by S. The rock had in some places imbedded in it quartz pebbles, standing up above the general surface. Being harder than the sandstone rock, these pebbles had been able to withstand the friction; but some of them showed marks of rubbing on their north-west sides.

b. At this place, looking towards the N.W.—viz., in the direction of Loch Lomond—an opening between the hills, which are apparently about 1000 feet high, was discernible; this opening being about 1½ mile wide.

c. At four other places on Craigend moor, from 500 to 600 feet above the sea, two to three miles apart, there were striations on the rocks, pointing repectively S.E. by S., S.E. ½ S., S.E. by S., and S.S.E.

At all these places the direction was seen to pass through the opening between the hills above referred to, indicating that the agent, whatever it was, which produced the striations might have come, and probably came, by that opening.

d. On this same moor (forming an extensive plateau of about 6 miles long by about 3 miles wide) I had pointed out to me by Mr Young several boulders in different places.

Two were of trap, from the Kilpatrick hills, situated some miles to the W.N.W., and at a height of 570 feet above the sea. In circumference, each boulder measured 27 feet, and, so far as not buried in the drift on which they were lying, the height of one was 4½ feet, of the other 6 feet.

Another boulder, well rounded, 500 feet above the sea, was of grey granite, weighing about 2 cwt., which Mr Young considered, from the size of its felspar crystals, to have come from Ben Awe, a mountain situated to the N.W., and distant about 50 miles.

There were several smaller boulders of old conglomerate—transported, no doubt, from the well-known band of that rock which, running from Dumbarton, crosses Loch Lomond in a N.E. direction towards Aberfoyle.

e. In the valley of the Blane there are deep beds of sand formed, most probably, whilst the sea occupied the valley, and numerous well-rounded boulders of all descriptions. At Strathblane Railway Station there was a deep cutting of a sandbank, with several boulders in the sand, and one in such a position as to indicate that it had fallen from some raft which had been conveying it, as it was sticking with its narrowest point downmost.*

f. It was remarked to me by Mr Young, that whilst boulders,

* See a diagram of this sandbank and boulder in a little book, published by Edmonston & Douglas in 1871, called "Estuary of the Forth."

gravel, and beds of sand are abundant in the valleys of Strathblane and Campsie, he had never found any marks of grinding or striation on the rocks in these valleys. These effects seemed to have been produced at levels higher than 400 feet above the sea.

On another occasion, when geologising on the Campsie hills, above Glorat, situated 3 miles to the east of Campsie, and at a height of 800 feet above the sea, I found the sandstone rock striated, in a direction due E. and W. On the Kilsyth hills, a few miles still farther east, and at a height of 1200 feet above the sea, the striations on the rocks were seen to be E. and W.

g. One other fact observed was the immense accumulation of boulders of all kinds at Croyhill, a knoll of trap, at the summit level betweeen the firths of Clyde and Forth—viz., about 160 feet above sea-level. As some of these boulders were of "*old conglomerate*," they afford additional evidence of an agency which brought them from the westward.*

h. In addition to these facts, notice may be taken of two boulders reported to the Committee by Mr Jack of the Geological Survey. One is of mica slate, weighing about 6 tons, on the Kilsyth hills, at 1260 feet above the sea, the parent rock of which Mr Jack supposes to be situated about 15 miles to the north. The other is of conglomerate, weighing about 7 tons, on the north hill of Campsie, at 1803 feet above the sea, with its longer axis W. 20° N. Its parent rock is supposed by Mr Jack to be to N.W. (First Report of Committee, p. 51.)

Now what do all these facts prove? They prove that an agent of some kind or other moved over this district, having a depth of at least 1800 feet, and covering a great breadth of country; and that, whilst this agent was moving, the rocks over which it passed were ground down and rutted and striated; large boulders, at a high level, were carried forward, and boulders at a low level were pushed in a similar direction.

There is an additional fact deserving notice. The valley at Lennoxtown, where the pyrites coal strata are worked, seems to have at one time been filled up by these strata. These strata now, however, exist only on each side of the valley. Some agent has scooped them away, whereby the present valley was excavated; and it is

* "Estuary of the Forth," p. 95.

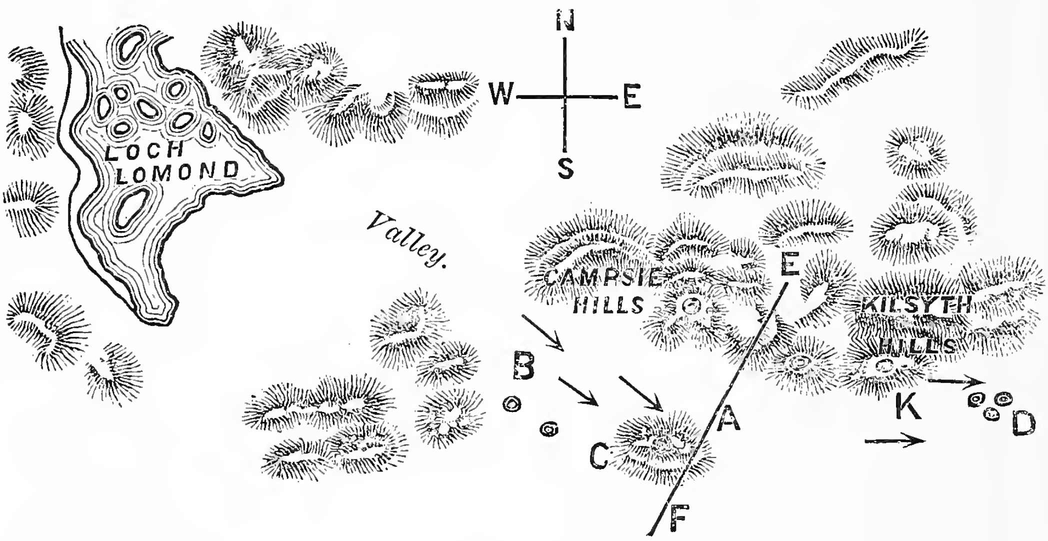
possible that the balls found in the Leith boulder clay form a portion of the debris of these pyrites strata so broken up.

What agent can fit into all these conditions so well, as a sea current loaded with ice?

On this theory, it is intelligible why the rocks along the moors of Craigend and Craigmaddie, stretching for 5 or 6 miles in a direction S.E. and S.S.E., at a level of from 500 to 700 feet above the sea, should show more effects of grinding and striation than the rocks at a lower level. Had a glacier been the agent, the grinding would have been chiefly at the lowest, not at the highest levels.

The subjoined plan and section of Campsie hills and valley will make the foregoing explanations more intelligible. The plan is copied from a published map by Johnston. The section has been

Ground Plan of Campsie Valley.

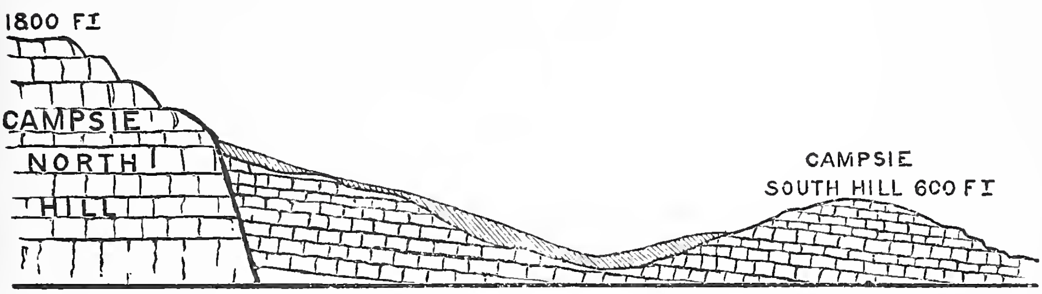


A, Pyrites coal strata, out-crop of.
B, Craigend Moor, 450 feet.
C, Craigmaddie Moor, 700 feet.
D, Boulder and striated rocks at Croyhill.
K, Kilsyth coal strata.

Boulders shown by black dotts.
Striæ on rocks by arrows.
FAE, Line of section across Campsie Valley.

kindly drawn for me by Mr John Young of Glasgow, who is thoroughly well acquainted with the geology of the district. In his letter sending the section, Mr Young says—" The Campsie coal and limestone is at present worked on the flank of the north hill, as well as in the mine which you saw in the south hill. The valley between these hills is one of denudation. Several hundred feet of strata, belonging to the Lower Carboniferous Limestone series, have been removed, or scooped out by currents of the ocean.

"If you examine Sheet 8 of the horizontal section of the Geological Survey (by Professor Geikie) you will find on the south hill of Campsie the outcrop of the coal and limestone. This sheet shows, quite as distinctly as my sketch, the valley running between the south and north hills, and the great denudation of the coal strata containing the marcasite balls."



Section across Campsie Valley ; coal and limestone strata overlaid by gravel and earth.

These explanations go far to show how the small marcasite ball found in the Leith boulder clay probably came from Campsie. A geological study of that district indicates the agency of deep-sea currents loaded with ice, which flowed upon the Campsie hills from the W.N.W., scooping out the valley which now occurs there, and breaking up to a large extent the coal strata in that valley. The debris of these strata would be swept along to the eastward; and some of the nodules forming part of these strata would be buried in the boulder clay now existing at Leith.

4. The cases which I have just been describing are of boulders, large and small, which have come from remote places, now separated by an intervening tract of *dry land* from the present sites of these boulders.

(1.) But there are cases of boulders which to reach their present sites must have crossed *arms of the sea*, even now of considerable depth and extent. In such cases, the theory of local glaciers is, of course, scarcely conceivable.

Thus on the Island of Islay, the Committee's last Report refers to several large boulders of rock, differing from any rock known in the island. At least, such was the opinion I formed after a week's ramble, and after inquiring among intelligent persons well acquainted with the rocks of the island.

So also in the Island of Kerrera, opposite to Oban, there are numerous blocks of grey granite, though no rocks of any kind of granite occur in the island.

On the small island of Staffa, consisting entirely of basalt and greenstone, I found boulders of red granite and gneiss, which probably came from the Mull mountains, situated to the N.E. (Committee's 2d Rep., p. 157).

(2.) In Nairnshire there are many conglomerate boulders of huge size, and angular in form, which must have been transported across what is now the Cromarty Firth from Ross-shire. They are at a height of from 400 to 600 feet above the sea. (First Rep. of Committee, p. 42.)

Other examples are afforded by the black granite boulders at Appin and in Loch Creran. Specimens of these are now on the table. As the present Report gives a full explanation regarding these boulders, I do not require to repeat how, when these specimens were submitted to Professor Judd of London, who has made the igneous rocks of the West Highlands a special study, he gave his opinion that there was no rock of the same description on the mainland, and that it was to be found only in Mull. From that island, therefore, these boulders must have been transported, and across a sea, which even now has at one place a depth of 100 fathoms, but which transportation probably took place at a period when the sea stood hundreds or even thousands of feet above its present level, or when the land sat that much lower in the ocean.

(3.) If Professor Judd's opinion of the Loch Creran and Appin boulders be correct, it goes far beyond an explanation of the boulders in these localities. For example, the Island of Lismore, whose rocks are entirely limestone, has on it many boulders of granite, which probably also came from Mull, inasmuch as Lismore lies between Mull and Appin (Com. 2d Rep., p. 157). In Lochaber there is the hill called Craig Dhu, about 2000 feet in height, so called, I believe, from the great number of black granite boulders resting on and near its top. These boulders, on account of their peculiar colour as well as position, attracted the notice of Professor Nicol and Mr Jamieson; and they are mentioned in both of my recent papers "On the Parallel Roads." (See also Committee's First Report, p. 39.)

In these papers I had occasion to point out how the position of the boulders both in Glen Roy and Glen Spean indicated that they had come—not *down* these glens, but *up* the glens. If the boulders at Loch Creran were rafted on ice from Mull by a sea current flowing eastward, the position of the boulders in Glen Roy and Glen Spean could be explained in the same way.

(4.) There is another fact connected with the position of boulders in the West Highlands, and indeed over Scotland generally, which receives explanation from Professor Judd's paper "On the Ancient Volcanoes of the Hebrides," I mean the high position of many large boulders.

In the Committee's Second Report notice is taken of a remark by the Ordnance Surveyors (p. 157), that in the Stratherrick district, where the highest hills are about 2900 feet above the sea, the boulders on the sides of these hills extend down to a level of about 2250 feet, *but not lower*.

In Fortingall parish (Perthshire) a gneiss boulder, weighing above 400 tons, is lying on clay slate rocks at a height of 2500 feet, being very near the ridge of clay slate hills. The gneiss hills form a range about 20 miles to the north and north-west. (Committee's First Report, p. 49.)

On the Fannoch Mountains (Ross-shire) a gneiss boulder of about 130 tons weight lies on a water-shed at a height of 2000 feet above the sea. (Committee's First Report, p. 49.)

On Schehallion (Perthshire) blocks of grey granite are seen at a height of 3000 feet. (Committee's Second Report, p. 173.)

On the top of a hill in Lochaber, exceeding 3000 feet above the sea, there are granite boulders. (Paper on Parallel Roads, Tr. of Soc. vol. xxvii. p. 740.)

Now where are there at present in Scotland ranges of mountains from which fragments could have been transported to such heights as those above named? There are now none such. Isolated peaks there are, but none exceeding 4300 feet; and of these there is but one, in the West Highlands (Ben Nevis), though it is from the westward that the great bulk of the boulders which overspread Scotland have come. Professor Judd's paper, giving reasons for believing that there were in Pliocene times mountains in Skye, Mull, Ardnamurchan, and even in Rum, some of which reached to a height of at least 14,000 feet, solves the difficulty, and explains many other curious facts besides.

For example, there is a series of granite boulders containing unusually large crystals of quartz, felspar, and mica, which occupy the straths between Fort-William and Kingussie. A boulder near Fort-William is 1500 feet above the sea, and from its position appears to have necessarily alighted on the hill from the westward (Committee's 2d Report, p. 161–2). If the sea stood at 2000 feet or more above the present level, the valleys of Lochaber and the Spey would be occupied by sea, and through them a current could flow from the ocean on the west to the ocean on the east. The summit level now between Lochaber and Strathspey is 850 feet above the sea, so that if the climate at that time was such as to allow of glaciers among the mountains and of floating ice on the sea, there would be means of transporting boulders from Mull to Lochaber and Strathspey.

5. There are several other instructive features connected with boulders brought out in this as well as in previous Reports.

(1.) The different shapes of boulders.

The Appin boulders are round shaped, whilst the Loch Creran boulders are angular, though the rock composing them is the same. The former are known in the district as "the round stones of Appin."

These Appin boulders are on the shore of the Linnhe Loch, through which in former times there must always have been a rapid current flowing, between the high mountains, forming the Glen-na-Albin or Great Glen of Scotland.

If icebergs then floated on the sea, these boulders must have undergone much pushing and rolling; whereas the Loch Creran boulders, being in what would then be only an arm or inlet from the main channel, would be exposed to no such friction.

In reference to the Kyle or sea strait, in what is now the line of the Caledonian Canal, the grinding to which the rocks on the sides of the valley have been subjected, is well seen at Cullochy on the north side, and at Inverfarrignig on the south side of the canal.

(2.) Another common feature presented by boulders in Scotland is, that when they are longer than they are broad, the longer axis is parallel with the direction in which the boulder had been transported. Very frequently also, when one end is sharp and the other end broad, the former points towards the direction from which the boulder has come. On the theory of icebergs and floating ice this feature is intelligible; on any glacier or ice sheet theory it is not.

(3.) The existence of striæ on boulders, and the circumstance that these striæ are sometimes deeper at one edge than on the rest of the surface, is a new fact brought out in this last Report (page 688).

6. In several parts of the Report allusion is made to the evidence which boulders seem to afford, of the enormous denudation which there must have been in the district where these boulders are situated (pp. 662–667).

7. Notice is also taken in two districts of the West Highlands of horizontal terraces on the sides of hills, up to a height of 1800 feet above the sea.

If these are to be ascribed to sea action, as suggested in the Report, they would only show that Scotland possesses the same features in this respect as Norway, Sweden, and America, where there are horizontal terraces to even greater heights. It is only reasonable to expect that in the north of Scotland such records of the ocean should be discernible, considering the enormous beds of sand and gravel found at great heights in many of the mountains. On Schehallion (Com. 2d Rep., p. 173) there is gravel up to a height of at least 3000 feet.

In reference to the suggestion, that these terraces on the sides of mountains in the Highlands are marine, it is not unimportant to observe, that similar horizontal terraces at high levels occur also in lowland districts. Mr James Geikie, in his "Great Ice Age," refers to a series of "high level terraces of gravel and sand at Eaglesham," about 12 miles S.W. of Glasgow, the highest being 800 feet above the sea. "I have also traced them," he adds (page 248), "on the Moorfoots, up to 1050 or 1100 feet; and these, like the Eaglesham beds, seem equally to require the agency of the sea. Still farther south, high level shelves of gravel and sand have been detected by my colleague, Mr Skae, in Nithsdale, at a height of 1250 feet above the sea."

8. Lastly, may I be permitted, as there is still a wide field for farther investigation, to express a hope that the Boulder Committee may be re-appointed, and with additional labourers to carry on the work. I will be happy to be allowed to remain on the Committee, but I wish to resign the honour of being Convener. I begin to find that I am now not able for the hill-climbing and trudging across Highland moors and morasses, which boulder-hunting requires.

Edinburgh, 24*th May* 1878.

At a Meeting held this day, the Council re-appointed the Boulder Committee, with the addition of Dr Andrew Fleming, M.D.; William Jolly, Inspector of Schools, Inverness; and Ralph Richardson, Secretary of the Edinburgh Geological Society; and agreed to express a hope, that Mr Milne Home would continue Convener of the Committee.

The Council further agreed, that the "Remarks" by Mr Milne Home, at the Society's meeting on the 20th inst., when he presented the Committee's Fourth Report, appear in the Society's Proceedings, along with the Report.

J. H. Balfour, *Secretary*.

The Boulder Committee now consists of the following Fellows:—

Sir Robert Christison, Bart.
Sir Charles Wyville Thomson.
Rev. Thomas Brown, Edinburgh.
Dr Andrew Fleming, M.D., Edinburgh.
Professor Archibald Geikie, Edinburgh.
William Jolly, Inverness.
Dr Arthur Mitchell, M.D., Edinburgh.
Professor Nicol, Aberdeen.
Ralph Richardson, Edinburgh.
Thomas Stevenson, C.E., Edinburgh.
David Milne Home, LL.D. (*Convener*).

Monday, 3d June 1878.

Sir C. WYVILLE THOMSON, Vice-President, in the Chair.

The following Communications were read:—

1. On the Splitting up of Electric Currents, as detected by the Telephone, and the founding thereon of a Sounder to call attention from one Telephone to another. By R. H. Bow, C.E.

The telephone is of no use as a "far-speaker," without some means of calling the attention of the attendant at the distant station. Nothing could well be better than the "electric-bell call," and the sounder which I am about to describe makes no pretensions of competing with the bell, except on the points of simplicity, cheapness, and facility of use; and although its employment is limited to short length lines, it may be assumed that it is upon short length lines that the telephone will be most frequently used. I have had this sounder in experimental use for more than three months, and have shown it to many persons as a very obvious expedient. However, as it does not appear to have been referred to in any publication, I venture to bring it as a Note before the Society; it is of too trifling a nature to be made the subject of a formal paper.

In any of the sounders I have seen described, the battery, or other source of electrical excitement, has been placed in simple circuit with the pair of telephones, or put into circuit with the distant one. In the proposed method of sounding, by means of a galvanic battery, the battery is kept separate, and, when used, the short wire from the one electrode is rested against one of the wires of the telephone, while the wire from the other electrode is slid with a very gentle vibrating touch upon the other wire leading from the telephone. On the well-known principle of *derived currents*, we know that the greater portion of the electricity will pass through the shorter or less resisting circuit of the nearer telephone, and yet that there will be a not inconsiderable portion diverted to travel round by the distant one; and this, if the distance be not very great, and the telephones be of suitable construction, will suffice to elicit sufficient noise at the distant station to call the attention of any one in the same or even an adjoining apartment; the noise given out by the distant telephone will not be altogether due to this diverted current, but will owe part of its volume of sound to telephonic sympathy with the great agitation produced in the one near the operator.

Before entering into any details as to the construction of telephones best suited for this mode of sounding a "call," and for short distances, permit me to describe two experiments upon the detection of derived currents by means of the telephone.

Fig. 1.

Fig. 1 represents a wire of about 320 feet in length, bent so as to bring the middle of its length near to the telephone T, to which its extremities are attached; G is a battery of one cell, and A and B the wires from its electrodes; one of these, A, is fixed to the long wire at *a*; the other, B, is movable at *b*, the point *b* being taken at different distances from *a*. When the length of *a–b* is taken equal to from 3 to 4 feet, the noise emitted by the telephone might be sufficient to act as a call, although only about 1 per cent. of the current from the battery can then pass through the telephone; when the length of *a–b* is reduced to 1 foot, the sound from the telephone may yet be heard several feet away, and as the distance of *a–b* is decreased, the telephone must be brought nearer and nearer to the ear in order to hear the crepitating sound; with *a–b* equal to 3 inches, it may be heard 3 inches off; and when the telephone is held against the ear the distance of *a–b* may usually be reduced to less than a quarter of an inch before the sound is lost. Now, when the length of *a–b* is one quarter of an inch, the strength of the diverted current passing through the telephone used would only amount to the one twenty-thousandth ($\frac{1}{20000}$) of that circulating through the battery. But I have now to note a very puzzling circumstance. Sometimes, under conditions I have not been able to determine, the crepitating sound was found not to cease even when *b* and *a* were brought together, but continued to be heard when the one wire, B, was moved upon the other A, at *c*. Here there can be no dynamic current produced in the long wire. I shall not detain you with my speculations on the subject; but I should mention that the wire was very imperfectly insulated.

The second experiment gives an interesting practical example of the interference of one pair of telephones with another pair, due to the splitting up of currents. The rough diagram, fig. 2, will serve to explain the arrangement. A and A′ constitute one pair of tele-

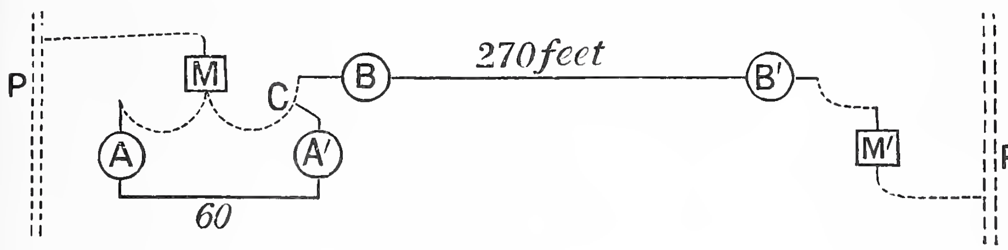


Fig. 2.

phones, the single connecting wire measuring 60 feet; B and B′ are another pair of telephones—these are connected by a copper wire 270 feet long; for the return currents the gas pipes and earth are employed. The gas pipes are indicated by dotted lines; M and M′ are gas metres belonging to different houses; and P and P′ are the gas mains in different streets, and at a direct distance apart of about 400 feet. It will be observed that the gas pipe from C to M is common to the two circuits. Now, it was soon noticed that when the battery-sounder was made use of at A, it caused a sounding not only in A′, as intended, but also in B, sufficiently loud to be heard several yards off if attentively listened for. This noise was, of course, due to a derived current splitting off at C, and making the long circuit round by B, B′, M′, possibly to the gas main P′, and thence to P and back by M, where it rejoins the principal current on its way to A. And using the battery at any one of the telephones causes more or less noise in all the four. But the small amount of sound produced by such straying currents is not likely to cause confusion.

The Telephones used with the Battery-Sounder.—If it were attempted to use the sounder in combination with telephones constructed in accordance with Professor Bell's instructions—that is, having coils so long as 180 feet of wire so thin as No. 36—comparative failure would result from the very great resistance offered to the current. We must try to do with coils offering an insignificant amount of resistance.

I find that the mode of arranging the parts of the telephone has a great influence on the efficiency, and the construction I have found up to the present time to be the best is as follows:—Taking a 3-inch horse-shoe magnet, I break off about half an inch from one leg; the nick made to indicate the north end is usually at about this distance from the extremity, and the breaking of it there is an easy operation. I then round the unbroken end on a grindstone, and next gum a strip of thin paper round that leg, and upon the paper coil the desired length of silk-covered wire. Taking next a piece of wood 4×4 inches or 4×5 inches, and half an inch thick, a hole 2 inches in diameter is cut cleanly through it, and crossing the hole is fixed, in letter T fashion, another piece of wood 4×3 inches; on this latter piece the magnet is laid with its south or coil-covered end projecting centrally into the hole, and it is fixed down by one screw passing through a small cross piece of wood placed above it. This admits of after-adjustments being very readily made. The ferrotype plate, $2\frac{1}{4}$ inches in diameter, is laid as a cover over the outer aspect of the 2-inch hole, and secured there by the usual ring of wood which constitutes the ear or mouth piece.

With such an instrument placed at A, fig. 2, and another telephone at A′, I have experimented on the length of coil needed. Four or five feet of wire gave very satisfactory results, speaking being heard distinctly, and with sufficient loudness. I then tried shorter and shorter lengths, and found the volume of sound to become reduced certainly, but in a surprisingly small degree; and at last I carried the shortening so far, that only $4\frac{1}{2}$ inches remained as a coil, and though the sound with this fragment was much reduced, giving the voice a far-away character, conversation could still be carried on. I did not think it necessary to push the reduction further, having become convinced that no very great length of wire was needed for the coil to secure distinct hearing, at least for short lines, say of quarter of a mile or less. And for the present I have adopted a length of 9 feet of No. 28 wire (weighing 6 grains per foot length) as a satisfactory coil, presenting a resistance of only one-third of an ohm, in place of the twenty or thirty ohms when the coil is made up of 180 feet of the much finer wire.

I have had no opportunity of satisfactorily testing the limits to which the sounder is restricted, but it would probably work well enough when the resistance did not exceed ten or fifteen ohms per Daniell cell. A marked advantage is usually found to attend the sending of the currrent in that direction, which increased the power of the magnets of the telephones. And in using the sounder a little skill will elicit a considerably augmented effect.

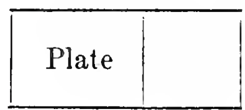
The principle of split currents may be applied to other purposes; it might offer a ready method of communicating with two adjoining stations on a railway from any intermediate point—that is to say, at any point between the two extreme telephones we could attach a battery or a telephone for sounding to, or communicating with, the attendants at the extreme telephones; or, if a battery be permanently included anywhere in the circuit, a wire alone, at any point on the line, offers the means of sounding the telephones at both stations, the wire being so used as to form an intermittent connection between the earth and the telephone line; and this might perhaps be practicable even from a train in motion, the engine and rails taking part in the earth connection.

2. An Account of some Experiments on the Telephone and Microphone. By James Blyth, M.A.

Dr M'Kendrick stated, that by applying the microphone or carbon-interrupter of Hughes to the membrane of a phonograph, he had succeeded in using the latter as a transmitting instrument. With such an arrangement, speech could be heard in the distant telephone even after it had become inaudible near the phonograph. He also mentioned that a tambour of Marey, used in physiological experiments, spoke distinctly when the fine point at the end of the lever was applied to the marks on the tinfoil of the phonograph. When a tube was carried from the tambour to the ear, distinct speech could be obtained from phonographic tracings on copper foil, which were scarcely perceptible to the eye. This method also got rid of the difficulty of having the tinfoil impressions quickly rubbed out, as happened when the stillette of the phonographic membrane was employed. He also introduced to the Society a phonograph made by Messrs Macgillivray and Scobie of Glasgow, which, for loudness, was superior to any one he had yet heard.

3. Note on a Variation of the Microphone. By R. M. Morrison, D.Sc.

Following out a suggestion made by Mr Seabrook in *Nature* of May 30th, I mounted the three carbon blocks of Professor Hughes' microphone on, not as Mr Seabrook recommends, a plate of 3 inches diameter, but on a ferrotype plate about 6 inches by 4. This plate formed part of the top of a box, thus—



This form I found to be extremely sensitive, as a piece of cotton wool $\frac{1}{2}$ inch in diameter falling through 1 inch made a loud sound in the telephone. When the plate was lightly brushed by a camel's hair brush the sound produced in the telephone could be heard a yard away. A small clock placed on any part of the table on which the microphone stood could be heard distinctly ; also a tap on the table, or even walking on the floor, each step producing a clang in the telephone. On speaking into the open part of the box the words spoken were distinctly and loudly heard in the telephone, notwithstanding the difficulty of hearing words spoken by one's self at the same time.

4. On the Action of Heat on some Salts of Trimethyl-Sulphine. Part II. By Prof. Crum Brown and J. Adrian Blaikie, B.Sc.

(*Abstract.*)

In the former paper on this subject, the authors stated that when hyposulphite (thiosulphate) of trimethyl-sulphine is heated to about 135° C., it loses sulphide of methyl to the extent of 23·58 per cent., the salt at the same time fusing to a clear colourless liquid. On cooling, this solidifies to a hard, very hygroscopic crystalline mass.

Analysis agrees with the formula

$$\left.\begin{matrix}(CH_3)_3S \\ CH_3\end{matrix}\right\} S_2O_3.$$

The solution of the substance does not decolourise iodine solution. These results point to

$$\left.\begin{matrix}(CH_3)_3S—O \\ CH_3—S\end{matrix}\right\} SO_2,$$

as the probable rational formula of the substance.

Sulphite of Trimethyl-Sulphine.—This salt was obtained by the action of sulphurous acid on the hydrate. It crystallises well, but there is some difficulty in preparing a perfectly normal salt. The salt, as nearly normal as possible, does not, like the hyposulphite, give up its water of crystallisation in the cold over anhydrous phosphoric acid; at 140° C., however, it becomes anhydrous. Heated to 175° C. it gives off sulphide of methyl—8·3 grammes lost 2·32 grammes, or 27·95 per cent. On cooling, the clear liquid residue solidifies, forming a hard, very hygroscopic crystalline mass. This substance was so deliquescent that no analysis of it was made. The mode of formation leads to

$$\left.\begin{matrix}(CH_3)_3S \\ CH_3\end{matrix}\right\} SO_3$$

as its most probable formula.

Note received on July 24, 1872.—In order to ascertain the nature of the crystalline substance obtained by the action of heat on the sulphite of trimethyl-sulphine, the authors converted it, by double decomposition with iodide of potassium, into the corresponding potash salt, which was purified from the iodide of trimethyl-sulphine by crystallisation. This potash salt was found to agree in properties and composition with the "sulpho-metholate," or "methyl-sulphonate" of potash—

$$\begin{matrix} & & O & \\ & & \| & \\ K—O— & & S & —CH_3 \\ & & \| & \\ & & O & \end{matrix}$$

The bearing of this fact on the constitution of the sulphites is obvious.

5. On a Class of Determinants. By Mr J. D. H. Dickson, Tutor of St Peter's College, Cambridge.

6. On the Wave-Forms of Articulate Sounds. By Professor Fleeming Jenkin, F.R.S., and J. A. Ewing, B.Sc.

(*Abstract.*)

By the help of the phonograph we have continued the investigation described in a previous Communication (Proc. R.S.E., p. 582), and have now obtained about two hundred magnified traces of the phonographic records of vowel sounds spoken and sung by various voices, and of these sixty-five have been already subjected to harmonic analysis, extending as far as the sixth partial tone. In each case the results have been accepted as satisfactory only when, after the magnified trace had been obtained, the record on the tinfoil of the phonograph still gave the vowel sound satisfactorily. Our attention has hitherto been almost exclusively directed to the vowels *u* (the vowel sound in "food,") and *o* (as in "oh,") both of which are well spoken by the phonograph. The results, which are still incomplete, are briefly as follow:—

When a vowel sound is continuously sung without change of pitch or quality, the wave-form produced is of remarkable constancy, showing that the compound sound does not contain any, or at least any important, constituent which is inharmonic to the prime tone.

A naturally high man's voice, with the comparatively small range *f* to *f′* saying *u* at any pitch throughout its range, produced a wave-form which was substantially a simple harmonic curve of length corresponding to the pitch. The upper partials, when present at all, were present only very feebly. Thus *u* sung on *b*♭ gave a prime whose amplitude was 25 (in the unit of measurement used), the second partial was only 0·8, the third 1·1, and the others inappreciably small.

When the same voice spoke *o* anywhere throughout the same range, it produced a trace which in every case consisted of a strong prime and a strong second partial (that is to say, the octave of the prime), the higher partials being feeble or absent. Experiments on this part of the scale with various voices proved that the proportion which the amplitude of the prime bore to the amplitude of the second partial might vary greatly at any one pitch, although the sounds were all sung or spoken as *o*, and received by the ear as (generically) that vowel. For example, on *b* one voice gave the ratio of prime to second as 1 to 0·87, while another voice on the same note gave the ratio 1 to 1·8. In any one voice there is not very much change in the ratio in passing from note to note. When the pitch is as high as *d'* or higher, the ratio of prime to second is decidedly greater than on the lower notes of this range. It is probable that the ratio is a minimum for any one voice about the pitch *b♭*, but this is a point requiring further investigation. We have not yet got any satisfactory *o*'s above *f'*.

When, however, the investigation was carried lower in the scale by help of voices of a wider range, several much less simple phenomena presented themselves. Voices capable of singing bass, when singing *u* down the scale gave the usual simple harmonic from above *a*; but, at or near that note a remarkable change suddenly took place in the wave-form given by the vowel sound *u*. At that point it became a duplex wave, with a very small prime, which corresponded to the pitch, and an immensely strong second partial, the ratio of amplitudes being somewhere about 1 to 4. This form continued as the voice went down the scale; but in addition to the very strong second partial a weak third appeared, which became pretty strong on *c*. We cannot say that we have got true and articulate *u*'s at any lower pitch.

The voice of small range mentioned at the beginning of this paper continued to give the single simple harmonic form for *u* down to *f*, below which it could not go. Two other voices experimented with agreed in making the change at or near *a*.

The excessive weakness of the prime in the lower, or what we call the duplex, form of *u* shows how weak a prime tone may be as compared with its upper partials, and yet fix the musical pitch. It also shows how small even the prime may be when not reinforced by oral resonance. The primes of the duplex *u*'s, even when loudly uttered, were absolutely as well as relatively much weaker than those of the *o*'s already described.

The experiments with *u* seem to point to the conclusion that so long as the simple form is given the mouth cavity is adjusted so as to reinforce the prime exclusively, whatever be the pitch. When the duplex form is reached the cavity must be suddenly changed so as to be approximately if not exactly in unison with the second partial. A voice singing *u* up the scale may sometimes carry the duplex form as high as *b*♭, but the vowel quality of an *u* so spoken is apt to approach *o*. When attempts were made to *slur* either up or down, past the place at which the change occurred, one or other of two things took place—either the sound died away almost completely while the critical point was being passed, or the vowel quality changed form *u* to *o* for an instant. This has been observed both directly and by examination of the magnified phonographic record, in which either an almost blank space or the well known wave-form of *o* might be seen.

An examination of the sound *o* through the lower regions of the scale, and by the help of several bass voices, did not show any such sudden change as took place in *u*, but the number of partials conspicuously present became more and more numerous as the pitch fell. The third partial began to appear about *f*, and on *d* and *e* the prime second and third were all strongly present, the prime being the weakest of the three, and the second the strongest. On *c* the fourth partial was moderately strong, the third stronger, the second stronger still, and the prime weaker than any. The first four partials were all conspicuous on B. On B♭ the fourth was much stronger than any of the others, and the prime was the weakest of the lower ones. The same description applies to A. On G the fourth was still much the strongest, and the fifth appeared weakly. The lower ones were weak. On F the fifth was much the strongest, but the prime, second, third, and fourth were all distinctly present. We have not got any trace good enough for analysis below F. We reserve the numerical result for more detailed publication.

The bearing of our experiments on the theory of vowel sounds is very important. For a considerable range it will be seen that the constituents of *o* and *u* might be simply described, with no reference whatever to absolute pitch—the *u* always being a simple tone, and the *o* a compound of two simple tones an octave apart.

When, however, certain limits are passed, phenomena appear which are evidently connected in some way with an absolute pitch. It seems, however, that this connection is rather due to the necessities of the mouth than to the requirements of the ear; for, as stated above, one voice will give a simple harmonic curve for *u*, while another gives (at the same pitch) a double curve for a sound which is intended for the same letter in the same language, and which is at least generically the same vowel. This fact suggests that the mouth, being unable to shape itself so as to continue the simple form of the letter, adapts itself in some way so as to produce what may perhaps be termed an imitation. It is by no means impossible that this imitation may in some cases be produced by a recurrence to the form used for the same letter at a higher pitch. If this be so, then the hypothesis of a constant cavity for a given vowel sound would be true for the letter *u* when pronounced on notes an octave or a twelfth apart, although not for intermediate notes.

When we examine the sound *o* we find less necessity for insisting on recurrence or any tuning of the mouth cavity. A fair approach to the phenomena observed might be obtained by assuming a constant mouth cavity, having a pitch of maximum resonance near *b*′♭, as stated by Helmholtz, and reinforcing tones over a large range of nearly two octaves—from *f*″ to *f*, or thereabouts. There are, however, some peculiarities in the constituents which suggest that the *o* cavity may also be adjustable. Our results are not inconsistent with the assumption that the cavity is slightly altered or tuned so as to bring the maximum resonance approximately into unison with that upper partial of the prime sung which lies nearest to *b*′♭. This hypothesis would allow us to diminish the very large range of reinforcement which the constant cavity theory requires, although even an adjustable cavity must still reinforce tones over a considerable range. Further experiments are required to determine how the mouth actually produces the results obtained, but is clear that the idea of relation between the constituents must be combined with that of absolute pitch in any complete vowel theory. The relation may, however, depend for some vowels, though hardly for *u*, on the simple range over which the reinforcement acts. The pitch of maximum resonance in the *o* cavity given by Helmholtz is *b*′♭, and this, on either hypothesis, agrees well with our results. It is either the pitch of maximum resonance of the constant cavity, or the pitch near which the upper and strongest resonance is to be found, where the cavity is tuned. It may be observed that, in the case of this letter, possibly the idea of a tuned cavity may be true for singing, and that of a constant cavity for speech.

7. On the Electric Conductivity of the Bars employed in his Measurements of Thermal Conductivity. By Prof. Tait.

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

Dr J. J. Kirk Duncanson, 8 Torphichen Street.

Monday, 17th June 1878.

Sir WYVILLE THOMSON, Vice-President, in the Chair.

The following Communications were read:—

1. On the Biliary Secretion, with Reference to the Action of Cholagogues. Part II. By Professor Rutherford, F.R.S.S. L. and E., and M. Vignal.

(*Abstract.*)

The method of experiment by which the following results have been obtained has been described in the abstract of Part I., published in the "Proceedings" February 1877. All the experiments were performed on dogs. In the previous abstract the effects of 29 different substances were briefly stated:—

30. Dilute nitro-hydrochloric acid is a hepatic stimulant of considerable power.

31. Jaborandi is a very feeble hepatic stimulant.

32. Calabar bean stimulates the liver, but not powerfully, unless it be given in very large doses.

33. Atropia sulphate antagonises the effect of Calabar bean on the liver, and thus reduces the hypersecretion of bile produced by that substance. It does not, however, arrest the biliary secretion; and when given alone, does not notably affect it.

34. "Menispermin," a resinous matter obtained from the Yellow

Parilla, does not stimulate the liver. It slightly stimulates the intestinal glands.

35. "Baptisin," a resinous matter derived from the *Baptisia tinctoria,* is a hepatic and also an intestinal stimulant of considerable power.

36. "Phytolaccin," a resinous matter prepared from the *Phytolacca decandra,* is a hepatic stimulant of considerable power. It also slightly stimulates the intestinal glands.

37. Sodium Benzoate is a powerful stimulant of the liver. It does not stimulate the intestinal glands.

38. Ammonium Benzoate stimulates the liver, but not quite so powerfully as the sodium salt of benzoic acid. It does not stimulate the intestinal glands.

39. Benzoic acid stimulates the liver, but owing to its insolubility, its action is less rapid and much less powerful than that of its salts.

40. Sodium Salicylate is a powerful hepatic stimulant. It does not notably stimulate the intestinal glands.

41. Ammonium Phosphate is a moderately powerful stimulant of the liver. It is not an intestinal stimulant.

42. Tannic acid does not affect the biliary secretion.

43. Hyosciamus does not notably affect the biliary secretion, and does not prevent such a stimulant as sodium salicylate from augmenting it.

44. Morphia does not affect the biliary secretion, and does not prevent the stimulating effect of such a substance as sodium salicylate.

45. Pure diluted alcohol does not affect the biliary secretion.

46. Potassium iodide does not affect the biliary secretion.

47. Veratrum viride has no notable effect on the biliary secretion.

48. Manganesium sulphate is not a hepatic stimulant. It powerfully stimulates the intestinal glands, and like other purely purgative agents, such as magnesium sulphate and gamboge, it indirectly lowers the biliary secretion.

49. Ailanthus glandulosus is an intestinal but not an hepatic stimulant.

50. Acetate of lead somewhat diminishes the biliary secretion, probably by a direct action on the liver.

51. "Hydrastin," a resinous matter, prepared from the root of the *Hydrastis canadensis*, is a hepatic stimulant of considerable power. It also slightly stimulates the intestinal glands.

52. "Juglandin," a resinous matter, prepared from the root of the *Juglans cinerea*, is a hepatic stimulant of considerable power. It also slightly stimulates the intestinal glands.

Thus, by means of a new and precise experimental method, the physiological pharmacology of one of the most important organs of the body has been in this research worked out as far as at present it appears desirable to proceed, and knowledge that is definite and reliable, because obtained by a method of accurate measurement, after the elimination of disturbing factors, has by this research been substituted for the vague traditions of the past.

The effects of fifty-two medicinal agents upon the liver have been investigated, and the vast majority of the conclusions are in complete harmony with the results of clinical observation. Very many new facts are, however, given to the physician, and even as regards well-known substances, our knowledge of their effects on the liver is now of a character very different from that which previously obtained.

All the experiments relate to the *bile-secreting* and not to the *bile-expelling* mechanism. The authors do not intend to investigate the effects of medicinal agents on the latter, as this point appears to them one of very minor importance compared with the subject of the above research.

The following remarks indicate the position in pharmacology of the above results. Of necessity, the influence of a drug upon a diseased state is the ultimatum of pharmacology, and every experiment upon a healthy bodily system, whether of man or animal, is merely ancillary to experiments with the drug in disease. Having discovered that this or that drug stimulates the healthy liver of a dog, we do not infer that it *must* also stimulate the human liver in health, and still less do we conclude that it must also have this action in disease. The experiments on the healthy liver of the dog, on the normal and on the abnormal human liver, are three sets of experiments closely related, but still distinct. The facts derived from any one of the three sets cannot be substituted for those of the other two. Each set of facts has its own proper place. The above research, therefore, leaves it to the clinical observer to experiment on man with such substances as sodium benzoate, sodium salicylate, baptisin, euonymin, sanguinarin, &c., and thereby to ascertain whether or not these substances also stimulate the human liver; and of necessity it is also left to him to ascertain in what diseased state the employment of this or of that substance is most advantageous.

Other general conclusions have been already stated at the close of Part I.

2. On a New General Method of Preparing the Primary Monamines. By R. Milner Morrison, D.Sc.

3. On the Preparation and Properties of Pure Graphitoid and Adamantine Boron. By R. M. Morrison, D.Sc., and R. Sydney Marsden, B.Sc.

4. On Colour in Practical Astronomy, spectroscopically examined. By Professor Piazzi Smyth.

Monday, 1st July 1878.

Sir WYVILLE THOMSON, Vice-President, in the Chair.

The following Communications were read:—

1. On the Disruptive Discharge of Electricity. By Alexander Macfarlane, D.Sc., and P. M. Playfair, M.A.

(*Abstract.*)

During the months of May and June of this session, we have endeavoured to investigate certain questions suggested by our experience of the discharge of electricity through the gases and through oil of turpentine.

Ordinary paraffin-oil, when used as a dielectric, exhibits the same phenomena as oil of turpentine. Gas is liberated by the passage of the spark, and at the same time carbon is deposited. Once produced, the gas bubbles make the passage of the spark more easy through bringing the electrified surfaces nearer to one another; hence,

in taking a series of observations, it is necessary to get rid of the bubbles after the passage of each spark. They were attracted generally to the positive surface, but sometimes to the negative. The attraction was more marked when no jars were attached to the Holtz; it was not so powerful as in oil of turpentine, and was generally in the opposite direction.

The electrostatic force required to pass a spark through a layer of paraffin oil or of turpentine is constant, whereas it is variable in the case of air and other gases. For the observed differences of potential plotted with respect to the thickness of layer give a straight line through the origin, while in the case of the gases the curve is concave.

The electric strength of the paraffin oil used was found to be 4, of the turpentine 3·7; air being unity.

To investigate the effect upon the electric spark of heating the electrodes, we constructed electrodes of thick platinum wires placed at right angles to one another—a suggestion we owe to Professor Clerk Maxwell. When one of the wires was heated by a current from a battery of four Bunsen elements, the electrometer deflection was diminished by about one-fourth of its amount, and that whether the wire heated was positive or negative. A similar diminution was observed when the deflection for continued sparks was taken. This diminution of the difference of potential must be due to change at the surface of the wire; for the air between the wires (the shortest distance between the wires being 4 millimetres) cannot be so much rarefied by the heating of the wire as to produce the effect.

We have also investigated the effect upon the electric spark of heating the air round the discs, the pressure being kept constant. We have observed the deflections of the electrometer for a constant spark for temperatures from 20° C. to 280° C., and find that they indicate a curve, which slopes down gradually as the temperature is increased, while the deflections during cooling give a curve which is somewhat lower at the lower temperatures.

It appeared an important matter to ascertain whether the electrometer used in all these observations gives deflections strictly proportional to the inducing charge. To calibrate it by means of cells would have required a very large number; hence the following method suggested by Professor Tait was adopted. A charge was put upon the inducing ball of the pair on the stand, and the deflection read; the charge was then divided by bringing an equal ball into contact with the inducing ball, the deflection read, and so on. The deflections were so nearly halved each time, that we may infer that they are strictly proportional to the charge on the inducing ball. We had arranged to verify our former observations in this matter last Thursday forenoon (27th June); but as the deflection on the scale always fell in the negative direction, and went to a great distance beyond the proper zero when the dividing ball was brought into contact, we gave it up. This effect was, doubtless, due to a strong negative electrification of the air; for the thunderstorm came on immediately.

2. On the Wave Forms of the Vowel Sounds produced by the Apparatus exhibited by Professor Crum Brown. By Professor Fleeming Jenkin, F.R.S., and J. A. Ewing, B.Sc.

At a recent meeting of the Society, Dr Crum Brown exhibited a gutta percha bottle of irregular form, which, when applied as a resonance cavity to reeds of various pitches, gave very good imitations of certain vowel sounds. By closing certain apertures in the side of the bottle it could be made to say A ("f*a*ther"), A ("*a*we"), O ("*o*h"), and I ("mach*i*ne"). When the cavity was kept constant, and the pitch of the reed was altered, the same vowel continued to be given. Dr Crum Brown was good enough to lend us the apparatus, in order that we might investigate the sounds given by the bottle in the same way as we have been investigating certain human vowel sounds, by obtaining and magnifying phonographic traces, and then subjecting them to harmonic analysis as far as the sixth partial tone. Of the vowels which the bottle speaks, O is the only one which we have fully examined in this way when spoken by the human voice, and we have confined our attention to it among the artificial vowels also.

By using reeds of various pitches we have obtained curves or traces of the artificial O's sufficiently good for harmonic analysis on the following pitches :—*e*, *f*♯, *g*, *b*, *c*′, *e*′♭, and *e*′. The pitch was in each case determined by measuring the length of the traces. The vowel quality of the sounds, as repeated by the phonograph, was exceedingly good, even better that the original sound, as the jarring noise of the reed was lost. The sounds were thoroughly recognisable as O, of perhaps a somewhat bright species. The table below shows the amplitude of the successive partial tones, along with their absolute pitch to the nearest semitone.

Partial.	1 On *e*.		2 On *f*♯.		3 On *g*.		4 On *b*.		5 On *c*′.		6 On *e*♭′.		7 On *e*′.	
	Amplitude.	Pitch.	Amplitude.	Pitch.	Amplitude.	Pitch.	Amplitude.	Pitch.	Amplitude.	Pitch.	Amplitude.	Pitch.	Amplitude.	Pitch.
I.	3·8	*e*	4·3	*f*♯	9·8	*g*	9·1	*b*	4·2	*c*′	2·9	*e*♭′	2·1	*e*′
II.	7·3	*e*′	6·8	*f*♯′	9·0	*g*′	5·2	*b*′	4·5	*c*″	2·3	*e*♭″	3·1	*e*″
III.	3·9	*b*′	2·8	*c*♯″	4·8	*d*″	0·8	*f*♯″	0·5	*g*″	0·2	*b*♭″	0·1	*b*″
IV.	2·1	*e*″	1·0	*f*♯″	1·6	*g*″	0·3	*b*″	0·4	*c*‴	0·3	*e*♭‴	0·1	*e*‴
V.	0·6	*g*♯″	0·3	*b*♭″	0·8	*b*″	0·3	*e*♭‴	0·3	*e*‴	0·3	*g*‴	0·1	*g*♯‴
VI.	0·2	*b*″	0·2	*c*♯‴	0·1	*d*‴	0	*f*♯‴	0·2	*g*‴	0·1	*b*♭‴	0·2	*b*‴

It cannot be said that these figures show any specially strong resonance on or close to *b*♭′, which Helmholtz gives as the proper tone of O, but they do show a wide range of resonance, extending a long way above and below that pitch. There is distinct reinforcement as high as *g*″, or even *g*♯″, and as low as *e*′, if not lower, and partial tones falling anywhere between these limits are more or less reinforced.

The above analysis appears to show that a strong resonance on or near *b*♭′ is not essential to O, and that this vowel effect may be satisfactorily produced by other joint resonances above and below that pitch.

In a previous communication we pointed out that if the view be adopted that the constituents of the O's, sung at various pitches by a human voice, are due to the reinforcement caused by a constant oral cavity, the results of our analyses showed that this cavity not only has the property of strongly reinforcing tones close to *b*♭′, but must also be capable of strengthening, more or less, tones widely distant from that pitch, and extending over a large range. The analysis of the artificial O's now shows that a constant cavity may possess the latter property in quite a sufficient degree.

In order, however, to test this still further, we made the following experiment. A tube consisting of a piece of cane of the same size as one of the reeds was put into the neck of the bottle in place of the reed, and the side apertures of the bottle were closed so as to arrange the cavity for the vowel sound O. The end of the tube was then inserted in the ear, so that the whole apparatus acted as a resonator to sounds from outside. Then, striking the keys of a piano in succession, we observed what notes gave the peculiar humming effect due to reinforcement by the resonator. On working down the scale the resonance first became appreciable on $g\sharp''$. It then got stronger and stronger down to f'' and e'', which were both intensely and nearly equally strong. $e\flat''$ and d'' were a little weaker, but still very strong, and on $c\sharp''$ the resonance again became very intense. c'' was a little weaker, but also very strong. The resonance continued as the pitch fell, being sometimes stronger and sometimes weaker. g' and $f\sharp'$ were both strong,—decidedly stronger than could be accounted for by the reinforcement of their second partials g'' and $f\sharp''$. f' was weaker,—so much so that the resonance observed on it might be due to the second partial.

The presence of the upper partials in the notes struck made this method of testing the resonant qualities of the cavity inapplicable to pitches below those named. But the above cases, in which the reinforcement was distinctly of the prime, sufficed to show that the cavity would strengthen any tones between $g\sharp''$ and $f\sharp'$, at least, some more and some less strongly, while they left it an open question whether there was not resonance down to a lower limit of absolute pitch. Of course, it is to be observed that the bottle, when applied to the ear in the manner described, might differ in its resonant peculiarities from the same bottle applied to a reed, but its range is probably as great in the former case.

When the bottle was arranged for the vowel sound A, and tested in the same way, the resonance was perceptible as high as e''', and the highest maximum occurred on c'''. In this case also there was reinforcement over a range of at least an octave.

Traces of the wave forms of the artificial O's spoken of in this paper will be given along with those of other O's when the full account of our work is printed.

3. Notes on the Fungus Disease affecting Salmon. By A. B. Stirling, Assistant Conservator of the Anatomical Museum in the University of Edinburgh. Communicated by Professor Turner.

It is widely known that a destructive epidemic has this spring appeared among the salmon of the rivers Eden, Esk, and Nith. The mortality among the fish has been so great as to cause considerable alarm among proprietors, salmon commissioners, taxmen, anglers, and the general public.

The newspapers inform us that within three days the watchers have taken out of the Esk as many as 350 dead salmon. All who have examined the fish carefully, agree in referring the disease to the presence of a fungoid growth.

The other fish in those rivers, as the smolts, trout, eels, lampreys, minnows, pike, and flounders, are also said to be attacked in a similar way to the salmon, and fears are entertained that the disease may become thoroughly established in the district.

In these circumstances, I have thought it might be interesting to describe the condition of some of the fish which have come under my observation. In March last my friend Dr Philip Hair of Carlisle sent me the fin of a salmon which had been affected by the disease, and requested me to state if possible its nature. Unfortunately, the fin was in a putrid condition when it reached me, and as a result of the examination, I could only state to Dr Hair that the disease was probably a fungoid one. A few days later I received from Dr Hair a fine specimen of a trout, but it was not stated whether the fish was taken alive or picked up dead. It was, however, quite fresh, and the effects of the disease were painfully exhibited on the carcase. A hurried examination of this specimen enabled me to inform Dr Hair that the disease was due to what I had previously suspected, namely, a fungoid growth.

While examining this specimen, I observed entangled among the fronds of the fungus foreign matter of various kinds, namely, torulæ or yeast fungus, triple phosphates, fecula, human hairs, hairs of the cat and the mouse, also desmids, diatoms, shreds of dyed wool and cotton, with other fragments of matter unknown to me. Respecting the torulæ, I, in my letter to Dr Hair, asked if their presence could be accounted for by bakeries or breweries in Carlisle, whose refuse might have got into the river.

My letter was published by Dr Hair in the *Carlisle Journal* of March 29th, and in the *Field* newspaper of March 30th, and as worded it might have been inferred that I regarded the presence of bakeries and breweries as the cause of the disease. This was of course not intended. On 12th April I received two salmon and a trout from J. Dunne, Esq., chief constable of Cumberland and Westmoreland, all of them in a diseased condition. Mr Dunne requested me to make an examination of those fish, and hoped, on public grounds, that I might be able to discover the true nature and cause of the disease.

As a result of my examination of those fish I sent a preliminary report to Mr Dunne. This report was forwarded to the Fishery Inspectors, and was considered of so much importance that it was published in the *Times* and many of the provincial and local newspapers. Sir Robert Christison had also very kindly supplied me with a number of specimens from the river Nith, all of them affected with this disease. An examination of these has confirmed me in the opinion expressed in the report above referred to. All these fish had the disease in an advanced stage, being more or less affected about the head, chin, branchiostegal rays, and fins in every instance. One salmon had rubbed the chin till the lower jaw had nearly separated at the symphysis, the skin was rubbed off the branchiostegal rays, and the rays broken; a trout had the upper left jaw bare of skin, the bone worn and hanging loosely attached to the cheek, the pectoral fin of the left side in rags, and the rays worn to stumps.

Another salmon had the skin rubbed off the nose and crown, and the matted fungus covered the bare parts; the dorsal fin was quite destroyed, the strong anterior rays being reduced to stumps of half an inch in length, and the remains of the fin bare, bleached, and without membrane. Beneath the dorsal fin on each side were spaces extending 3 inches forward towards the head, and $2\frac{1}{2}$ inches backwards toward the tail, thickly covered with the fungus. Besides these there were other spaces on the sides of the fish from 1 inch to 2 inches in diameter, all covered by the fungus, which gave the fish a spotted appearance.

This fish appears to have been alive when taken, as the skull and brain had been punctured by the fisherman. The greater part of this fish was cooked; it was very firm and fat, and the three persons who made a meal of it pronounced it capital. I tasted a portion of the flesh from a part where the fungus covered the skin, and could not detect anything different in the flavour from an ordinary fishmonger's salmon.

The fungus appears, in the first instance, to attack those parts of the fish that are not covered with scales, as the crown, nose, sides of the head, chin, throat, and the membranous parts of the fins. From those parts the fungus extends by vegetative growth (which seems very vigorous), to those portions of the surface of the body which are covered with scales. On the sides of the fish where small patches of the fungus were situated on the scales (and no rubbing had taken place) no sore could be detected, and the fungus was easily wiped off with the finger.

I may also mention that all the fish which I received from the Eden river, both trout and salmon, were infested with tape-worms of a large size, the worms being about 2 yards in length and $\frac{3}{16}$ths of an inch in breadth. One of the salmon had from 60 to 80 yards of those worms in the pyloric portion of the gut. Another salmon had three varieties of worms in various parts of its alimentary canal. 1*st.* In the stomach were many round worms, about 4 inches in length, tapering to each end, and as thick as ordinary whip-cord in the thickest part of the body; many of those worms were entangled among the gill rays, it being their habit to crawl there when the fish dies, and from their presence in this situation they are called gill worms by the fishermen. 2*d.* A small spiral worm, which attaches itself, by burrowing in the outer walls of the intestine, in the fat and pyloric appendages. 3*d.* Tape-worms seated within the pylorus and intestine.

On 30th May, I received from Sir Robert Christison a large salmon from the Nith. This fish was believed to have been to the sea, after being attacked with fungus, and was captured on its return. The specimen was a female, and had the roe about one-fourth grown; the viscera were very healthy, and no entozoa were found in it. The head of this female is peculiar in having a kip on the under jaw, and a cavity in the upper jaw to receive it, as in the male fish of the species. The right side of the head, including the eye and nose, was very deeply rubbed and the bones injured, but no fungus adhered to the injured part. The pectoral fin on the same side had no membrane, the rays being bare, broken, and separate from the muscles at their roots. There were several patches on both sides of the fish, from which the scales were rubbed off, but no fungus adhered to the rubbed parts. In several of those rubbed parts, although the skin was unbroken, a portion of the muscle, corresponding in breadth to the external injury, and half an inch in depth, was in a pulpy condition; beneath other rubbed spots the muscle was quite sound. The dorsal, ventral, caudal, and anal fins were all more or less injured by rubbing. No fungus adhered to any of the fins except the anal, the rays here being reduced to stumps of an inch or half an inch in length, on which a thickly matted covering of fungus is seated. The branchiostegal rays are very slightly rubbed, and are the only other part of the fish on which the fungus remains. In my report to the Fishery Commissioners in April last, I stated that the fish did not die of the fungus, but of the injuries they inflict by rubbing in trying to rid themselves of the pest. As some objection was taken in regard to this statement, I quote, in corroboration of my views, from a letter published in the *Field* of 25th May last. The letter was written by Commander Duncan Stewart, R.N. He says:—"In regard to the disease from which salmon are suffering in some of our rivers, it may be of advantage that I should mention what I observed in a small river at the head of Castrie's Bay in Siberia. I found the river rather low, but with plenty of clear running water. But what astonished me was to see thousands of salmon in all stages of disease and death, some darting away, but soon stopping to rub the side on the bottom or on a rock; others were constantly rubbing, others unable to rub. In those last cases large sores, from the size of a shilling to that of a half crown, of a most filthy appearance, were always present. Fish in which the scales had been rubbed off would try to get out of my way, but I could kill them with a stick; those with the skin gone would rub themselves against my trousers."

Supposing this salmon from the Nith had been to the sea, and had while there got rid of the greater part of the fungus with which it was affected, it had returned to the river in such a mutilated condition, and with unhealed sores of such a nature, as in all likelihood would have ultimately proved fatal. Besides, the fact that the fungus was not killed by the salt water, but was found in a highly vigorous condition on the parts to which it still adhered, gives but small hope of any permanent benefit to diseased fish from a visit to the sea.

The fungus belongs to Saprolegnieæ, a natural order of doubtful affinity—said to have the habits of moulds and fructification algæ. This order consists of the genera saprolegnia and achlya, which are great enemies of fish and other animals preserved in aquaria.

The filaments of the fungus arise free from the outer surface of the epidermic layers of the fish, having neither branches nor articulations. They are tubes, the walls of which are perfectly translucent, and in their interior at irregular intervals are small groups of fine granular matter.

The majority of the filaments are spear-shaped at their upper terminations, and appear to be barren.

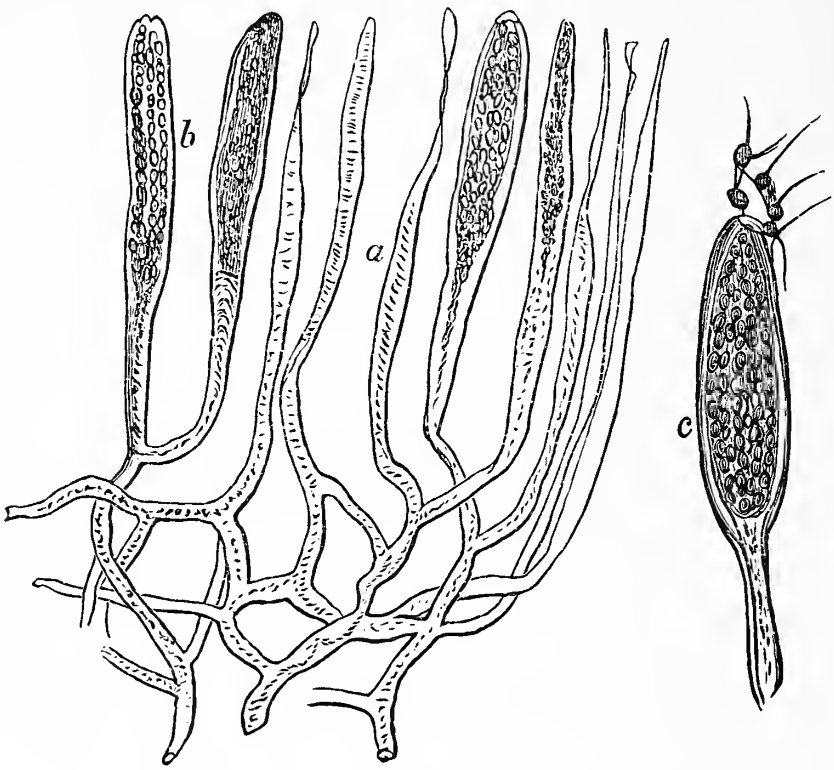
The prolific filaments, on the contrary, enlarge at their upper extremities, and form elongated club-shaped chambers, in which granular matter gathers. In the midst of this granular matter small round bodies appear, and those enlarging, gradually develop into spores. The prolific filaments apparently contain more granular matter, and are of greater calibre than the other filaments. They are evidently destined from the first to be the propagating media.

The spores escape by an opening in the summit of the chamber. This aperture is not an original opening. It is produced in a somewhat remarkable manner—so long as the spores are unripe and unfit for expulsion, a slender continuation of the filament projects from the apex of the chamber in manner similar to the neck of a bottle. At the point at which this joins the spore-sac, there is a slight contraction, which goes on gradually increasing in depth. Ultimately, when the spores are fully matured it drops off, and the aperture is formed. The filaments forming the mycelium of the plant are tortuous and branched; they ramify in the mucous and epidermic layers of the fish; they do not penetrate the corium where there are no scales. In other situations they never reach a greater depth than the outer surface of the scales; they are tubular, the whole plant being without septa forms a single individual of apparently indefinite extent. The spores are variously shaped at different stages, ovate and kidney being the commonest forms. They are very minute, and require a power of 450 to observe them well. The cilia are two in number, a longer and a shorter one, and are situated at the long axis of the spore. They are difficult to observe, and always disappear in permanently mounted preparations, although the spores themselves remain unaltered in all other respects. When the fungus is stained with logwood or picric acid, excellent permanent preparations can be got. It has been stated that the fungus dies with the fish. I have not found this to be the case; on the contrary, all my observations have been made from dead fish. Some of the specimens sent me from Carlisle by Mr Dunne were mis-sent to Aberdeen, and returned to me on the seventh day after the death of the fish, and yet I have scores of permanent preparations from these specimens, which show distinctly the characteristic form of saprolegnia ferax.

I have also found the fungus perfectly identical in all the specimens I have examined, which consist of salmon, sea trout, and river trout from the Eden, and salmon and grayling from the Nith.

It has also been said that a salt solution destroys the fungus, "*which melts in the solution like sugar in water.*" On the contrary, salt and water is an excellent preservative of saprolegnia; masses of it before me as I write have been in a salt solution for two months, and it remains unaltered. Further, the salmon captured in the Nith, which is believed to have gone to the sea in order to get rid of the fungus, had the fungus growing vigorously on several parts of its body. The fungus must either have instantly attacked the fish on its return to the river, or not have been destroyed during its stay in the salt water.

Regarding the cause of the disease, I can offer no opinion further than that some functional condition of the fish seems necessary for the propagation of the fungus. The germs of saprolegnia ferax must exist at all times, and in many places; and if so, there must be a reason why fish are not constantly affected with the fungus and in every river. I am persuaded that the condition of the fish is in some way either suitable or unsuitable for the propagation and growth of the fungus. Whether this arises from too high or too low condition, I am quite unable to say; but I may remark that while some of the fish examined were in the kelt stage, others were in a condition perfectly fit for food.



Saprolegnia ferax parasitic on the Salmon.
a. Barren filament. *b*. Prolific filament. *c*. Prolific filament, more highly magnified.

4. On some New Bases of the Leucoline Series, Part I. By G. Carr Robinson.

5. On the Crystallisation of Isomorphous Salts. By G. Carr Robinson.

It is generally stated that isomorphous salts are capable of crystallizing together in any proportions, or that the isomorphous elements which enter into them are capable of replacing one another in any proportion; *e.g.*, potash alumina alum and potash iron alum can crystallise together in all proportions.

Hauer * states that mixed crystals of alumina and chrome alum grow in a solution of ammonia iron alum; and, again, he states † that the more soluble isomorphous salt completely hinders the solution of the less soluble, so that if solutions of common alum, chrome alum, and iron alum be mixed, precipitation of the less soluble alums will occur.

The present paper is the result of some experiments made with the four alums—potash alumina, ammonia alumina, potash chrome, and ammonia iron.

The solutions of the two first, potash alumina and ammonia alumina, were obtained by saturating water at 100° C. with the salts, and pouring off the mother-liquor from the crystals that deposited on cooling.

The chrome alum solution was obtained by saturating water with the alum at a temperature carefully kept below that at which the green modification of chrome alum is produced.

Whilst the solution of iron alum was made by saturating water, acidulated with sulphuric acid, with the alum, at about 50° C.

The following experiments were then made and observed :—

a. When a crystal of potash chrome alum is placed in a solution of potash alumina alum exposed to the air, the chromium is turned out by the alumina, the interior of the crystal becomes granular, whilst a clear shell of alumina alum grows over it, the faces of which are finely striated.

b. When potash chrome alum meal is added to a solution of potash alumina alum in a well-closed bottle, and kept at very nearly a constant temperature, the chrome alum dissolves, but no replacement takes place.

c. When a crystal of potash chrome alum is placed in a solution of ammonia alumina alum exposed to the air, the ammonia alumina alum grows on it, there being only very slight replacement.

d. Potash chrome alum meal added to solution of ammonia alumina alum in well-closed bottle, and kept at very nearly a constant temperature, no change takes place.

e. Ammonia iron alum grows on crystals of potash alumina alum.

f. Ammonia iron alum grows on crystals of potash chrome alum.

* "Sitzungsberichte," Imperial Academy of Sciences, Vienna, 1860, xxxix.
† "Sitzungsberichte," Imperial Academy of Sciences, Vienna, 1866, liii.

g. When a crystal of ammonia iron alum is placed in a solution of ammonia alumina alum, replacement goes on very slowly, rust at the same time being thrown down.

h. Potash chrome alum grows on crystals of potash alumina alum.

i. If a crystal of ammonia iron alum be placed in a solution of potash chrome alum, the iron alum is turned out, whilst a skeleton in chrome alum of the original crystal is left; if this skeleton be placed in a solution of ammonia alumina alum, the latter alum grows on it, completing the form of the original crystal of ammonia iron alum.

6. On a New Method for the Separation of Yttrium and Erbium from Cerium, Lanthanum, and Didymium. Part I. By J. Gibson, Ph.D., and R. M. Morrison, D.Sc.

The method for the separation of these two groups hitherto in use was first proposed by Berzelius, and has been followed by almost all chemists who have investigated these earths. For the details of this method we must refer to his "Handbuch;" but we may briefly state that it depends on the relative solubilities of the double sulphates of these metals with potassium in a saturated solution of potassic sulphate. The yttrium and erbium double sulphates are said to be easily and completely soluble, while the double sulphates of cerium, lanthanum, and didymium are said to be perfectly insoluble. Wishing to prepare pure salts of yttrium and erbium, in order if possible to obtain the metals and to determine their specific heats, we tried this method, and found that, although it is a good rough method, the separation is by no means complete. We repeated the separation six times, but never obtained the earths pure, the spectroscope always showing the characteristic absorption-spectrum of didymium, provided we examined a sufficiently thick layer of a saturated nitric acid solution. The incompleteness of this method is indeed acknowledged by Bahr and Bunsen in their well-known paper on these metals.* The test given for the presence or absence of these two groups by these chemists was the presence or absence of the absorption-spectra of didymium and erbium respectively. We found, however, that

* Ann. d. Chem. u. Phar. cxxxvii.

not only are the double sulphates of the didymium group somewhat soluble in a saturated solution of potassic sulphate, but that some erbium, if not also some yttrium, is precipitated. Repetition of this process fails to remove the last traces of the didymium group.

After trying various modifications of this method, some of which gave better results, notably boiling the double sulphates with the saturated potassic-sulphate solution and filtering hot, we determined to look for another method, none of these variations being sufficiently good.

We have obtained better results by the following method. After having extracted as much as possible of the didymium group by the old method, the earths are dissolved in nitric acid, and to this solution a large excess of a solution of carbonate of ammonia is added, and the whole allowed to digest for a day in a closed flask, the precipitate being frequently shaken up. The liquid is then filtered and the undissolved residue washed. On acidifying the filtrate with hydrochloric acid, and adding oxalic acid, a precipitate is obtained which, on ignition, yields the oxides of yttrium and erbium, free from, or only containing the merest trace of, lanthanum or didymium, which may be removed by a repetition of the process. Any cerium originally present goes into solution, and must be removed by boiling a solution of the sulphates with carbonate of magnesia. The undissolved residue of the carbonate of ammonia solution still contains yttrium and erbium carbonates, but is much richer in didymium, and must be treated with a fresh portion of carbonate of ammonia solution.

It is essential that the ammonium carbonate solution be neither too concentrated or too dilute, as in the first case some lanthanum and didymium dissolves, and in the latter the carbonates of yttrium and erbium, after dissolving, crystallize out as double salts, leaving almost nothing in solution. The strength we found most suitable was about half saturated.

7. On certain Effects of Periodic Variation of Intensity of a Musical Note. By Professor Crum Brown and Professor Tait.

Recent discussions as to the nature of vowel-sounds have led us to make experiments (partly with an apparatus constructed some years ago for the same purpose) upon the effect of a periodic variation of intensity of a simple tone.

It is obviously impossible to secure a simple harmonic variation, so we endeavoured to produce a displacement varying as

$$1 - \cos mt.$$

An organ-pipe, giving a tone very free from harmonics, was sounded on one side of a partition in which were cut a series of large holes. These were opened and shut periodically by a revolving disc cut into separate sectors. The form of the holes was calculated on the rough assumption that the intensity of the sound passing through them was at each instant proportional to the uncovered area of the openings. This may be approximated to in many ways, most simply by making the holes approximately square or of rhombus form, with one diagonal radial, and the corners at the ends of that diagonal somewhat rounded off.

Supposing the adjustment perfect, the result should have been the disturbance

$$(1 - \cos mt) \cos nt,$$

or

$$\cos nt - \tfrac{1}{2} \cos (m+n)t - \tfrac{1}{2} \cos(m-n)t.$$

Thus, in addition to the tone given by the pipe, there should be two others of the order of summation and difference tones. The result was tested very easily by the help of resonators. Standing in front of the openings in the partition, the observer applied a resonator to each ear, and the pipe giving the tone whose number of vibrations was the arithmetical mean of those of the tones of the resonators was sounded on a small organ, and the disc made to rotate with gradually increasing velocity. The resonators were found to be affected simultaneously.

The experiments, as we made them, succeed much better with low notes than with high—because, though the rotating apparatus always produces a siren effect, the intensity of this effect is very small at low speeds. A very curious case occurs when $m = n$ (*i.e.*, the siren giving the same note as the pipe), for then the first harmonic comes in very marked.

8. Note on a Mode of Producing Sounds of very great intensity. By Professor Tait.

Two years ago I had an opportunity of making from the deck of the steamer "Pharos" some observations on the performance of the fog-siren at Sanda, off the Mull of Cantire. The instrument is worked by air at about $1\frac{1}{4}$ atmospheres pressure; and, though driven by a powerful air-engine, sounds for 7 seconds only per minute. One obvious defect of such an arrangement I saw to be the waste of energy in producing a current of air through the trumpet of the siren along with the oscillations. It then occurred to me that a regular alternation of puffing and sucking—exactly analogous to the air-disturbance produced by a drum—must be a much less costly source of sound. I have since constructed a siren on this double action principle, the air in the trumpet, which acts as a resonator, being put alternately in connection with reservoirs of compressed and rarefied air. The small model has given very good results, and a larger one is in progress. The only defect which my model showed was a waste of energy in the form of pulsations in the tubes leading to the exhausted receiver and to that containing compressed air. This can be very greatly reduced, but I do not yet see how to get rid of it entirely, unless it be possible to make both receivers so exactly as to act as additional resonators to the siren. If this can be carried out in practice there will be no energy spent except in sound. It is obvious that the principle just described is approximated to in practice whenever steam is employed in a siren:—the vacuum being produced by the condensation of the steam.

Another device of a somewhat different character was suggested to me by the experiments described in the preceding paper. After trying, without much success, to reduce the intensity of the siren notes by filing the edges of the apertures, it occurred to me that I might usefully *intensify* them. I therefore had copper plates soldered perpendicularly to the revolving disc, so as to increase instead of diminishing the virtual thickness of the edges of the apertures. The result was very striking. Such a siren gives a sound whose intensity is not sensibly increased by a powerful blast from an organ bellows. It produces strong currents of air through the holes in the fixed disc, whose direction in general depends upon the direction in which the rotating disc is made to revolve; and especially does so when the copper plates are inclined to the surface of that disc. When the discs are both furnished with these plates, turned in opposite directions, the result is still more striking. Various other modifications have occurred to me, and are now under trial, especially one for producing currents alternately in opposite directions through the holes.

By bringing up a flat plate towards the instrument, the quality of the sound is altered in a remarkable manner, and to such an extent that it seems well adapted for rapid Morse-signalling. As this instrument requires no work to be spent except in turning it, a very large number may be kept continuously at work at once by the same expenditure of power as is required for the intermittent roaring of a single fog-siren.

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

James R. Stewart, M.A. Oxon., 10 Minto Street.
John Archibald Campbell, M.D., Garland's Asylum, Carlisle.

Donations to the Library of the Royal Society during Session 1877–78.

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