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"SCIENTIFIC WORTHIES"

VII.—SIR CHARLES WHEATSTONE, BORN FEBRUARY 1802, DIED OCTOBER 19, 1875

CHARLES WHEATSTONE was the son of a music-seller at Gloucester, where he was born in February 1802, and was educated at a private school in that city. His father afterwards came to London, where he became a teacher of the flute. He had, we believe, some share in the musical education of the Princess Charlotte, a fact of which he was never tired of boasting. In 1823 young Wheatstone removed to London and commenced business as a musical instrument maker, and in the same year made what was probably his first contribution to scientific literature in a paper entitled "New Experiments on Sound," contributed to Thomson's "Annals of Phiiosophy." In 1827 he contributed to the *Quarterly Journal of Science* an account of "Experiments on Audition," and a description of the beautiful toy known as the Kaleidophone. In 1832 he read an important paper to the Royal Society, "On the Acoustic Figures of Vibrating Surfaces." In this memoir the author gave for the first time the laws of formation of the varied and beautiful figures discovered by Chladni. He continued for several years to devote his attention to Sound, and subsequently to Light. He was appointed Professor of Natural Philosophy in King's College, London, in 1834, and in the same year made his celebrated experiments on the velocity of an electric discharge by the aid of revolving mirrors. Wheatstone does not seem to have lectured regularly at King's College, and many of his discoveries were described to the world by Faraday at the Royal Institution. Indeed he was not well adapted for public lecturing; he was so nervous that even in a very small company he usually sat silent. Though his discoveries have become of such immense practical importance, Wheatstone himself was far from being a practical business man; on this account he often failed to reap the substantial fruit of his discoveries. Wheatstone was married on Feb. 12, 1847.

Wheatstone's name is intimately connected with the early history of Spectrum Analysis. In a paper read in 1835, "On the Prismatic Analysis of Electric Light" at the Dublin meeting of the British Association, he announced the existence of rays of definite refrangibility, emitted in the volatilisation of metals by the electric spark. He showed that the spectrum of the electric spark from different metals presented each a definite series of lines differing in colour and position from each other, and that these appearances afforded the means of distinguishing the smallest fragment of one metal from that of another. "We have here," he wrote, "a mode of discriminating metallic bodies more readily than that of chemical examination, and which may hereafter be employed for useful purposes." These last words furnish the keynote to all Wheatstone's work; however valuable may be the services he has rendered to pure science, his great ultimate aim was the useful and practical. It was at the meeting of the British Association in 1838 that he described and exhibited the newly-invented stereoscope, and at the 1848 meeting he described his "polar clock," an instrument for ascertaining the time by means of the change in the plane of polarisation of the light of the sky in the direction of the pole. One of these instruments, we believe, has been sent out with the Arctic Expedition. Wheatstone's description of the rheostat, and of the well-known "bridge" which bears his name, is printed in the *Transactions of the Royal Society* for 1843. Indeed, it is a popular error to suppose that Wheatstone's scientific fame rests solely on his connection with the electric telegraph; he would have deserved an honourable place in the annals of science had this practical application of electricity been yet undiscovered. In the Catalogue of the Royal Society alone will be found the titles of upwards of thirty papers by him, not to mention many others scattered about in various publications.

The President of the Italian Society of Science, of which he was made an honorary member in 1867, said, in conferring the honour, that the applications of the principle of the rotating mirror are so important and so various that this discovery must be considered as one of those which have most contributed in these latter times to the progress of experimental physics. "The memoir on the measure of electric currents and all questions which relate thereto and to the laws of Ohm has powerfully contributed to spread among physicists the knowledge of these facts and the mode of measuring them with an accuracy and simplicity which before we did not possess. All physicists know how many researches have since been undertaken with your rheostat and with the so-called 'Wheatstone Bridge,' and how usefully these instruments have been applied to the measurement of electric currents, of the resistance of circuits, and of electro-motive forces."

With regard to the scientific value of the revolving mirrors, M. Dumas spoke as follows in the address which he gave at the obsequies of Wheatstone in Paris:—"This admirable method enabled Arago to trace with a certain hand the plan of the fundamental experiment which should decide whether light is a body emanating from the sun and stars or an undulating movement excited by them. Executed by an accomplished experimenter, it proved that the theory of emission was wrong. This method has then furnished to the philosophy of the sciences the certain basis on which rest our ideas of the nature of the forces, and especially of that of light. By means of this or some other analogous artifice, we can even measure the speed of light by experiments purely terrestrial, which, pursued by an able physicist, have guided the measure of the distance between the earth and the sun."

As to Wheatstone's connection with the electric telegraph, it is unnecessary for us here to speak, as this was so fully gone into in the series of articles on "The Progress of the Telegraph," in vols. xi. and xii. of NATURE. De la Rive, in his "Treatise on Electricity," (Part VII., Chap. I.), states so fairly Wheatstone's connection with telegraphy, that we quote here what he says:—

"The philosopher who was the first to contribute by his labours, as ingenious as they were persevering, in giving to electric telegraphy the practical character that it now possesses, is, without any doubt, Mr. Wheatstone. This illustrious philosopher was led to this beautiful result by the researches that he had made in 1834 upon the velocity of electricity—researches in which he had employed insulated wires of several miles in length, and which had demonstrated to him the possibility of making voltaic and magneto-electric currents pass through circuits of this length. It was in 1837, in the month of June, that Mr. Wheatstone took out his first patent. He first employed five conducting-wires, between two distant stations, acting upon five magnetised needles, the movements of which, being combined two and two, were enabled to produce several different signs. Mr. Wheatstone, at this time, entered into partnership with Mr. Cooke, who had likewise devised an ingenious telegraphic apparatus founded upon the same principles. The English philosophers, from the very first, had added to the telegraph—properly so called—an apparatus intended to call the attention of the observers, and designated under the name of *Alarm*. . . . The principle upon which this alarm is founded includes an immense number of applications, for it enables man to put in action—at any distance whatever—all the forces of mechanics, in an instantaneous manner. Indeed, more recently. Mr. Wheatstone applied it to the construction of his dial telegraph; and it is the same principle which serves as the basis of Morse's telegraph, invented at nearly the same period."—*De la Rive*, Part VII. Chap. I.

We may repeat here a fact which does not appear to be generally known or recognised, that Wheatstone was the first suggester and worker at submarine telegraphy. From documents before us we learn that so early as 1837 he was thinking much on and was greatly interested in the subject; and in February, 1840, he stated his opinion before a Select Committee of the House of Commons as to the practicability of establishing electric communication by means of a cable between Dover and Calais.

Wheatstone's applications of electricity were almost innumerable. His electric clocks are well known; various electric registers were invented by him, one especially for recording a variety of meteorological data, and another, which acts as a chronoscope to register the velocity of a bullet. Indeed, his ingenuity was marvellous, and, as we have already hinted, there are doubtless many useful inventions due to him, of which he has reaped neither the profit nor the credit.

Wheatstone received many "honours" during his lifetime. He was made a Fellow of the Royal Society in 1836, in 1868 he was knighted, in 1873 he was elected a foreign member of the Paris Academy, and had altogether upwards of thirty foreign distinctions. The following estimate of Wheatstone and of his contributions to science is from the pen of Signor Paul Volpicelli, the distinguished Italian electrician.

The Academy of Sciences of the Institute of France, at its *séance* of October 18, 1875, was informed by the perpetual secretary, M. Dumas, that the illustrious English physicist, Charles Wheatstone, was seriously ill in Paris at the Hôtel du Louvre. By the 17th of the month, however, fears for the safety of that *savant* were well nigh vanished, and it was believed that he would soon be completely restored to health. The hope unhappily proved fallacious; an aggravation having occurred in the pneumonitis, which was thought to have been overcome, carried off that distinguished physicist on the 19th of the month from his friends and from science.

Our Reale Academia dei Lincei was profoundly afflicted at this irreparable loss, which deprived it for ever of one of the most celebrated of its foreign correspondents, who was and will continue to be one of the proudest and most pure scientific glories, not only of England, but of the whole civilised world, since science does not know nationalities, but belongs to all countries.

The brilliant Wheatstone inevitably became possessed of all those honours which science is wont to confer on her eminent votaries, and in 1875 he obtained from the Academy of Sciences of the Institute of France a title of the highest distinction, that, viz., of one of the eight Foreign Associates. Among the cultivators of physics the name of Wheatstone will never be forgotten; neither the present nor the future can forget his rare penetration, the inventiveness of his genius, his discoveries, and the uncommon ability with which he reproduced in machines the phenomena of nature.

His name will be as that of a star to whose light will turn the minds of those who desire to comprehend the progress of physical doctrine. In doing honour to the memory of this our illustrious foreign correspondent, it will be best to record his scientific labours, which are all of the highest interest.

In the present short obituary notice we shall merely give a rapid sketch of the principal physical researches of the illustrious deceased.

Our countryman, Leonardo da Vinci, in 1500, or thereabouts, conceived and was the first to affirm, that from a picture it was not possible to obtain the effect of relief. But Wheatstone, reflecting profoundly in 1838, on the physiology of vision, invented the catoptric stereoscope, with which he philosophically solved the problem of the optical and virtual production of relief.

This instrument was a converted *dioptric* of the father of modern optics, the distinguished Brewster, and at the same time was more simple, more popular, and more elegant. Continuing to study the physiology of vision, Wheatstone succeeded in constructing the diaphragmatic stereoscope, *i.e.* without mirror and without lenses, but merely with a diaphragm. With this instrument there could be received coexistently on the retina three images, two, viz., one photographic, and one in relief, produced from the first two.

The diaphragmatic stereoscope manifested, better than the two others, the physiology of vision; it was published in the "Atti dell. Academia Pontificia dei Nuovi Lincei," t. vii. p. 219, and in the work entitled "Monographie du Stereoscope," by H. De la Blanchère (Paris, 1861), as also in the *Cosmos*.

The use of the stereoscope, whether catoptric, dioptric, or diaphragmatic, would have remained pretty restricted, if photography had not come in to greatly extend that use, in its application to industries, arts, and sciences.

When one undertakes to study the hyperoptics, it often happens that the vibrations of the molecules of ether, constituting a luminous wave, are found difficult to conceive.

To remove this difficulty, Wheatstone invented a very ingenious apparatus, by which the vibratory motions of the luminiferous ether could be represented with considerable fidelity, and especially the phenomena of polarisation, whether rectilinear, circular, or elliptic.

Many were the achievements realised by Wheatstone in applying himself to optics: we owe to him the invention of a kaleidoscope, in which the persistence of impressions on the retina was utilised in demonstrating the transversal vibrations of an elastic rod fixed at one of its extremities; we owe to him one of the most sensitive photometers, as also the way of estimating the duration of lamps, the movements of the sea, and the ramifications of the retina; the difference between the solar and the electric light, and the lines of the light obtained from combustion of bodies brought to the poles of the voltaic battery. He was, further, the inventor of the polar clock, an instrument designed to indicate the hours through observation of the plane of polarisation of light of the blue sky in the region of the North Pole. That instrument, improved by Soleil, was marvellously adapted for finding the neutral points of Arago, Babinet, and Brewster.

The science of acoustics also profited by the valuable researches of our Lincean correspondent, for he experimented on sound and on hearing, devised his kaleidophone, occupied himself with resonance or reciprocal vibrations produced by a column of air; he also studied the transmission of musical sounds, and the figures obtained with sand on a vibrating surface, or acoustic figures.

Moreover, he cultivated, with great advantage to science, electro-dynamics; and devised two rheostats, one for great, the other for small resistances.

We are indebted to the illustrious deceased for many other scientific contributions, which have realised a notable progress in various branches of modern physics, and especially in telegraphy. Among these contributions, which time would fail fully to enumerate, we must not omit to speak of the method, so fruitful of valuable consequences, by which Wheatstone determined the velocity of the electric discharge in a metallic wire.

He was likewise skilled and practised in ballistics; and he employed the uniform rotation of two pasteboard discs, fixed on a common horizontal axis passing through their centres, to ascertain the initial velocity of a projectile fired from a gun. The projectile, traversing with uniform velocity these rotating discs, produced within two holes, the different situations of which afforded a means of determining the initial velocity of the ball.

Wheatstone was the first to employ the rapid rotation of a reflecting disc for measuring the velocity of propagation of an imponderable agent, without resorting to great distances, such as the planetary. After having in vain turned the spark-exciting organ round an axis, hoping to be able to increase the extent of sparks, and also to alter their direction, according to the direction of turning, he conceived the idea of communicating to a reflecting disc or plane mirror a very rapid rotatory action, by which the electric spark produced at a certain distance from the disc might be reflected.

The interesting consequences derived from these experiments are—(1) that electricity takes an appreciable time in traversing a distance, whence may be inferred approximately the velocity of the electric current; (2) that this velocity does not depend on the direction of the electric current; (3) that of three sparks, reflected by this means, in the same horizontal direction, the two lateral ones appear contemporaneously, but the middle appears retarded with respect to the first, which fact is not reconcilable with the hypothesis of Franklin on the nature of electricity; (4) that the same method was adopted by the celebrated Arago, whose experiments lead to a decisive judgment which of the two theories on the nature of light, that based on emanation, or that based on ethereal vibration, must prevail.

The fact that we now possess methods of determining the velocity of light so practical, elegant, and speedy as those of the distinguished physicists Fizeau and Foucault, is due to the method of rotating mirrors, which was introduced through this order of researches of the English physicist, of whom we deplore being for ever bereaved; the fame of whose discoveries is everywhere—

> "And with the world itself shall still endure."
>
> Dante, *Inf.*, ii. v. 60.

P. VOLPICELLI

THE PROGRESS OF THE LOAN COLLECTION

VARIOUS are the trains of thought suggested by a visit to those galleries in which the science of the past and the present is being represented by so goodly an array of its working implements. If one has been at all in sympathy with the movement which is now so near its goal (and who that has in the least appreciated the progress and benefits of science can be out of sympathy with it?), it will, first of all, be truly gratifying to him to observe on every hand such manifest tokens of hearty co-operation in the movement. Even those who are engaged in marshalling the various treasures of the departments which have been entrusted to their charge seem to be animated with an unusual zeal (a zeal promising the best results), and, at first sight even, it is evident that the various museums and private collections in this country and on the Continent have been ransacked for some of their choicest contents to be sent to these South Kensington galleries, aiding the completion of an ideal which is true in its comprehensiveness. The nations of the Continent who were appealed to for their support of the scheme, have shown, many of them, by an activity which is beyond all praise, how warmly the proposal has been entertained. This is especially true of Germany. The Berlin Committee appointed a short time ago, and including some of the foremost names in science, while it gained also the useful accession of Imperial influence, promptly made application (the time was short) to the various Universities and Polytechnic Schools throughout the country, and they were met as promptly; so that soon quite a network of subordinate committees came into being, all working harmoniously towards the common end. The German contributions form a very considerable proportion of the whole; and they, in common with contributions from the Continent generally, are indeed surprising in their extent, if we consider the shortness of time allowed and the unique character of the exhibition. Both Germans and French have been doing all that they can up to the latest moment, and this has somewhat retarded the arrangements. The Russian contributions have not yet been received. The collection of instruments from Italy is, in many respects, of a peculiarly interesting character.

A catalogue and a series of handbooks are in course of preparation Some idea of the extent of the collections may be obtained from the fact that in the former there will be somewhere about 6,000 entries. The nature and value of the handbooks for the various departments may be learned from the fact that they are prepared by such men as Huxley, Henry Smith, Clifford, Maskelyne, Carey Foster, Guthrie, Clements Markham, Lockyer, and others. The entire work, extending to some 300 pages, in which the history and functions of the several instruments are dealt with, will be expounded in a clear and succinct manner.

It would be impossible, we imagine, to walk through those corridors and inspect the various objects encased on either hand, without soon beginning to marvel at the multiplicity and complexity of the tools which science has come to construct for herself in the progress of her inquiries; and at the degree of precision and skill to which she has at length attained, after many a tentative and faltering step towards the end in view. Yet it may truly be said, that as the branches and divisions of science multiply in increasing ratio, and therewith also the apparatuses become more and more complicated, the investigator, acquiring a deeper insight and wider range, ere long perceives a unity where he had not previously imagined it, and finds that many things which had seemed to be so many *disjecta membra* are knit together in the closest interrelations. It is perhaps not among the least benefits accruing from an exhibition like the present, that scientific men are enabled to survey, in close juxtaposition with their own line of research, other lines which they may have given little heed to, or but imperfectly comprehended. To an outsider also, who appreciates the keen enjoyment of scientific tastes, and is not hopelessly devoted to a hobby, the comparison cannot fail to be pleasant and instructive.

Again, that progress of science just referred to, from the less perfect to the more perfect, from the rough and clumsy to the finished and refined, in the construction of her instruments, affords a retrospect that is fascinatingly instructive. In the inspection of the collection one comes ever and anon upon some antiquated-looking instrument of plain proportions and great simplicity, which almost seems as though it had stumbled by mistake into the company of its elegant and brightly furbished conquerors—had come among a generation that knew it not. Yet these ancient relics have a deeply interesting history, and they will doubtless attract to themselves no small share of attention from the visitors who will take advantage, we trust, in large numbers, of the unique opportunities this exhibition affords. Nor would any contempt which might momentarily arise for the unpretentious and uncouth figure of these instruments be long in giving place to a sort of veneration and awe, for the story they have to tell that they took shape under the hand or at least under the thoughtful direction of a Galileo or a Herschel.

The general arrangement of the exhibition, which, as already mentioned, is by no means complete yet, may here be briefly indicated. The space occupied is in the West and South Courts. After passing through the South Court, in which stand the South Kensington Museum instruments, the Educational apparatus, and those relating to Applied Mechanics, the visitor enters the West Court, the northern portion of which contains the greater bulk of the present collection. Here in succession are arranged the departments of Magnetism and Electricity, Mathematics, Meteorology, and Astronomy. On proceeding upstairs and returning, the rooms devoted to Geography and Geology, Biology, Chemistry, and Physics, are successively passed through. This will indicate the general arrangement of the whole; but the classification of the various instruments in the catalogue now being prepared is considerably more detailed.

It is not our intention here to attempt anything like an exhaustive account of the various objects of interest which line these courts. In the Astronomical department will be seen several of Galileo's instruments, including two telescopes made by himself, one of which served for his most important discoveries and experiments. The object-glass is shown by which he discovered Jupiter's satellites, and first saw spots on the sun. The Reale Institute of Florence, to which the Exhibition is indebted for these instruments, has sent sundry other relics of the great astronomer, including a natural magnet, which he armed, and an air thermometer and microscope, which were his. Nor should we fail to be interested in such instruments as a quadrant of Tycho Brahé, some object-glasses and eye-pieces, which were mostly polished by Christian and Constantine Huyghens, and a telescope of Huyghens. A venerable wooden object near the wall is Sir W. Herschel's 7-feet telescope, both mirrors of which were finished by Sir William's own hands, and there is also shown a 10-feet reflecting Newtonian telescope also made by him. Several of Gravesande's instruments are shown; also the apparatus used by Baily in repeating Cavendish's experiments, Foucault's pendulum apparatus, Gauss's pendulum for demonstrating the rotation of the earth, &c. The more modern aids of astronomical observation are largely represented, and, among others, there is a beautiful transit instrument, of novel construction, from Germany.

Among the instruments of a mathematical order are Babbage's celebrated calculating machine, also two calculating machines constructed in 1775 and 1777 by James Black for Viscount Mahon. We might also note the integrating machine of Sir W. Thomson. The laws of combination of harmonic motions have been illustrated by some ingenious apparatus of Messrs. Tisley and Spiller, and by a machine invented by Mr. Donkin; but the most important application of these laws is to be found in Sir W. Thomson's tidal clock, and in a more elaborate machine which draws curves predicting the height of the tide at a given part for all times of the day and night, with as much skill as can be obtained by direct observation. Then there are the "Napier bones" of the inventor of logarithms, used for performing multiplication and division. Among measuring instruments the gauges lent by Sir Joseph Whitworth are remarkable for their delicacy. With one of these, for measuring the bore of guns, differences of one-ten-thousandth of an inch can be measured. There is another, moreover, by means of which a quantity so minute as one-millionth can be grasped. The apparatuses which Joule employed in ascertaining the mechanical equivalent of heat, are among the collection.

The field of magnetism and electricity is now a very vast one, and in the exhibition it is represented by a correspondingly large variety of apparatus. A peculiar halo of interest gathers round the instruments, many of them so simple and homely, with which Faraday worked out his fruitful ideas. Among old electric machines is one with two glass cylinders, one of which is covered with sealing-wax, so as to obtain both positive and negative electricity; there is also Nairne's early electric machine with glass globe, Armstrong's hydro-electric machine, &c. Volta's electric lamp is exhibited for lighting gas by means of the electric spark. There is an endless variety of batteries, and the numerous Holz and other electric machines exhibited will afford material for careful study, as also the novel forms of magneto-electric and other machines which have of late years multiplied so fast. Here may be found apparatus for regulating the time and place of an electric discharge; apparatus for accumulating electricity; apparatus for observing the effects of discharge of accumulated electricity; apparatus for producing, and apparatus for observing, effects of continuous electric currents; apparatus for measuring the strength of electric currents, and apparatus for measuring resistance, and so on. In the telegraphic department there is a complete historical collection which must be of great interest not only to electricians and telegraphists, but to the general public, illustrating the progress of the electric telegraph from the time when the first idea of it was crudely embodied, down to the present time. This collection includes, of course, all the classical apparatus which belonged to Wheatstone and Cooke, among which is the original Wheatstone Bridge. Many will be interested in seeing the instruments that were used on board the *Great Eastern* in laying the Atlantic Cable. There are copies of the first German telegraphic apparatus (Goemmering's), and the first needle telegraph (Schelling's); the electro-magnetic telegraph apparatus used by Gauss and Weber in Göttingen, from 1833 to 1838, &c., &c.

We must here desist for the present, though the majority of the sections are still unvisited. It may be understood, even from these hasty and imperfect notes—but in any case, the reader may soon convince himself by personal inspection—how rich and varied is the collection now in course of completion, and how ample is the feast therein provided for those who feel in any measure drawn to the "beautiful and true" in science.

The date of opening of the exhibition is still uncertain. It is hoped that her Majesty will grace the occasion with her presence, and conduct the opening ceremony. Some of the galleries of the exhibition will probably be lighted in the evenings with the electric light, and a considerable portion of the apparatus, it is intended, will be kept in motion.

GREEN'S GEOLOGY

Geology for Students and General Readers. Part I. Physical Geology. By A. H. Green, M.A., F.G.S., Professor of Geology in the Yorkshire College of Science, &c. (London: Daldy, Isbister, and Co., 1876.)

THE progress of geological research in every quarter of the globe is exceedingly rapid, and discoveries of new processes of investigation, leading to the opening up of fresh lines of thought in connection with the science, are constantly taking place. Hence, in spite of the acknowledged excellence of some of the existing manuals of geology, such as those of Lyell and Jukes, we cannot but hail with pleasure the appearance of a new text-book of the science—especially of one which, like the present work, is not a mere epitome of one or other of the standard treatises just referred to, but which aims at some originality in its arrangement and mode of treatment of the subject. Prof. Green may be congratulated on having written a work embodying a vast amount of valuable information, which is presented in a very clear and readable form.

Of the two classes for whom Prof. Green writes, we think the "general readers" are those for whom his work is best adapted. Some of the chapters, such as the ninth, which is entitled, "How the rocks came into the positions in which we now find them," and the tenth, of which the heading is, "How the present surface of the ground has been produced," are models of clear and accurate description, and of logical and forcible reasoning; they are evidently written by a man with a thorough acquaintance with his subject, and no little enthusiasm for it to boot. We may, perhaps, demur to the confident tone and the sometimes off-hand manner with which our author disposes of the objections of those who differ from himself on some of the questions discussed; yet we cannot but feel that the conclusions at which he has arrived and which he so clearly states, are the result of independent observation and personal conviction, and are not merely adopted at second-hand. While reading many parts of this work, it is impossible to avoid the consciousness that we are following the pleadings of an advocate, and not the expositions of a judge; yet the arguments are brought forward with such lucidity and earnestness, that we accept the work as embodying the ablest exposition yet offered to us, of the views of that school of geologists to which Prof. Green belongs. Occasionally, however, the author is so carried away by his enthusiasm in behalf of favourite doctrines, that his confidence becomes something very like arrogance, as in the following passage, with which he concludes his chapter on Denudation:—

> "The reader will do well to compare with the theory of surface-sculpture upheld in the preceding memoirs, chapter xix. of the late Prof. Phillips' 'Geology of the Valley of the Thames.' Elegant and ingenious as is the explanation there put forward, there is about it an unsatisfactory vagueness and want of definition, which contrasts strongly with the sharp precision and logical coherence of the views on the subject of which a sketch has been attempted in the preceding pages, and which are steadily gaining ground among modern geologists."

In keeping apart from the other portion of his work purely speculative questions, and treating of them in two chapters *at the end* of the volume, we think the author has exercised a very wise discretion. In these final chapters the reader will find an excellent summary of the latest contributions to Cosmogony, and a generally fair and impartial discussion of the bearing and value of recent geological theories.

Of the suitability of Prof. Green's work as a text-book for "students," we regret to say that we cannot speak so favourably. The ability and enthusiasm with which our author writes on certain portions of his subject, fail to reconcile us to his inadequate treatment and sometimes total neglect of others of equal importance; and, in perusing the book, it is impossible to avoid the feeling that the space devoted to the several subdivisions of the science must have been determined, not as the result of a judicial consideration of their respective claims, but almost entirely by the author's peculiar predilections. For example, it is rather startling to find in a work on Physical Geology, extending to 540 pages, no discussion whatever of the phenomena and origin of mineral veins, and only, indeed, an incidental mention of their existence!

A still more serious blemish of the work, considered as a student's manual, is the looseness of description and inaccuracy of language in certain parts of it. This defect is most conspicuous in the second chapter, which treats of the characters and classification of rocks, and which offers a painful contrast to those later chapters of which we have already spoken. Among the numerous grave errors which every petrologist will remark in this chapter, we may call attention to the following. In the enumeration of the principal crystallographic forms the author omits such commonly occurring ones as the pentagonal dodecahedron, the icositetrahedron, the scalenohedron, and the tetrahedron, although some of these have to be referred to in subsequent pages of the work; of the *hemihedral* forms, indeed, Prof. Green makes no mention whatever. We are told that when a piece of calc-spar is broken up, "the shape of the fragments will be identically that of the block we started with." Again, we read, in reference to the subject of Polymorphism, "This is spoken of as Dimorphism, when the different *crystalline shapes* are two in number; Trimorphism when they are three in number, and so on." The italics are our own. In speaking of the constituents of a rock, the author places side by side metals and oxides in the loosest manner, and while the formula of soda is written Na_2O, that of potash stands as KO. Olivine is classed as a mineral of the augite group, and we read, "It is said that augite has never yet been found with quartz or orthoclase." "Perlite" is confounded with "sphærulite," certain rocks are spoken of as acid*ic*, trachytes are classified as *quartzless* and *quartzose*, and no mention is made of the fact that nepheline is a usual and highly important, if not an invariable, constituent of phonolites. These and many similar errors convince us that Prof. Green has hardly taken that amount of care in mastering those principles of chemistry, mineralogy, and petrology which are indispensable to the presentation of this part of his subject in a manner that will be of real service to the student. And this conclusion is confirmed when we examine the classification of rocks adopted by the author, and many of his definitions, such as those of rhyolite, hypersthene rock, and leucite-rock; or again, when we mark the want of judgment so frequently shown by him, as, for example, in rejecting the name of "porphyrite," while he retains that of "aphanite." Of the little care that has been taken to bring this part of the work "up to date," we have proofs in the circumstance that the dimorphism of silicic acid is not mentioned, and that leucite is spoken of, without hesitation, as belonging to the *cubic* system.

It is only fair, however, to point out that this very great want of accuracy is far more conspicuous in the earlier portions of the book than in its later chapters. This is a most unfortunate circumstance, inasmuch as we fear that many teachers who examine the work with a view to determine its fitness for the wants of students will be tempted to lay it down with feelings of disappointment and despair before they arrive at its really valuable portions.

We cannot but remark that Prof. Green's partial failure in a work which in many respects presents so much of promise, seems to us to have arisen from the attempt, which he boldly avows in his preface, that of making the teaching of physical geology take precedence of that of petrology and palæontology. While the petrological division of his work is treated so much more feebly, as we have already seen, than the other portions of the subject, the palæontological is omitted altogether. We do not think this can be safely done in any work on physical geology, and the danger of attempting it is well illustrated in Prof. Green's endeavour to explain the manner in which the conditions under which different deposits must have been formed is determined by geologists; in this explanation he almost entirely ignores the important evidence afforded by the characters of the animals or plants embedded in the sediments.

Rocks and fossils are the objects with which the geologist is called upon to deal at every step of his inquiries, and an accurate, if not an extensive, knowledge of them, is indispensable to the student, before he can hope to grapple successfully with those "broader questions" for which our author shows so much partiality. We cannot but regard the attempt to teach physical geology without the aid of petrology and palæontology, as bearing a suspicious resemblance to the specious promises which are made by those who profess to be able to impart a knowledge of a language without instruction in its grammar. We are sure that if such an experiment could have been successful it would have been so in the hands of so experienced a field geologist, so earnest a teacher, and so lucid a writer as Prof. Green; and, if he has to some extent failed, it is only because a more complete success, under the conditions accepted by him, was impossible. Had our author shown more deference towards the results attained by petrologists, we feel convinced that he would have written with far less boldness on such open questions as that of the "metamorphic origin of granite;" and had he admitted the importance of palæontological evidence, he would have recognised the difficulties which stand in the way of the acceptance of the theory of alternations of universal hot and cold climates during the earth's past history dependent on astronomical causes.

The views of scenery in the book are admirably selected, but unfortunately their value as illustrations of the text is greatly detracted from by the inferior style in which, in too many cases, the wood-engraving has been executed.

DITTMAR'S CHEMICAL ANALYSIS

A Manual of Qualitative Chemical Analysis. By William Dittmar, Professor of Chemistry in Anderson's University, Glasgow. (Edinburgh: Edmonston and Douglas, 1876.)

WITH the numerous works on chemical analysis already in existence we are justified in asking what special advantages Prof. Dittmar has to offer to chemical students in bringing out another book on the same subject. It will be found, in answer to this question, that the present work contrasts favourably with many of our standard books on the subject both as regards completeness, originality of treatment, and the introduction of a large amount of important matter which has not hitherto found its way into our manuals of analysis.

In giving our readers a brief sketch of Prof. Dittmar's mode of treatment, we shall point out what appear to us to be the special features in the new analysis deserving commendation.

To quote from the "Introduction":—"The book is intended for the use of students who, after they have mastered the first rudiments of chemistry, enter a laboratory to work *under the direction of a teacher*, while, at the same time, they *continue their study of theoretical chemistry*."

Following the Introduction, we have a series of exercises calculated to make the student practically acquainted with most of the operations and processes employed in analysis. Among the readable matter interspersed between the exercises, we notice with satisfaction a clear elucidation of the meaning of the term *equivalent*—a term which appears to have dropped out of most of the modern text-books, leading students to believe (we speak from actual experience) either that the idea is altogether obsolete or is covered in some mysterious way by the word "atomicity."

We are of opinion that the meaning of "equivalent," or "equivalent weight" of an element, should be laid before all students of the science and the relationship of these numbers to the "atomic weights" clearly pointed out. In this same portion of the book will be found also an excellent exposition of the general theory of double decomposition.

The next division treats of the metals, these being divided into six groups, viz., the copper group, comprising silver, mercury, lead, copper, bismuth, cadmium (and palladium); the arsenic group, comprising this metal, antimony, tin (molybdenum, tungsten, gold, platinum, and the platinum metals other than palladium); the iron group comprising chromium, aluminium, iron (uranium), cobalt, nickel, manganese, zinc (thallium), &c.; the barium group comprising this metal, strontium, and calcium; the magnesium group consisting of this metal and lithium; and the potassium group comprising sodium, potassium (rubidium and cæsium). The method here adopted does not much differ from that in general use, excepting that the groups are considered in the same order as that followed in the systematic course of analysis instead of in the inverse order. After the reactions of each of the metals in the group have been considered, their separation from each other and from the other groups is entered upon. We are glad to see that in many cases the author does not limit himself to one particular method of separation, but gives the most effective methods known, and points out under what particular conditions each process is applicable. The reactions of the rarer metals are given in appendices to the main groups. We observe also that Bunsen's flame reactions are sometimes resorted to, this being, so far as we know, the first work since the last edition of the English translation of Fresenius' "Qualitative Analysis," in which these valuable film-tests are introduced to the notice of students in this country. This portion of the book concludes with a general scheme to be followed in performing a systematic analytical search for metals.

The third division of the work treats of the non-metallic elements. In each case the properties of the free element are first considered, then its reactions and the reactions of its acid compounds, and finally the discrimination of the element and its acid compounds in complex mixtures. The order in which the various groups are treated of is as follows:—The halogens, sulphur, selenium, and tellurium, nitrogen, phosphorus, boron, silicon, fluorine, carbon, hydrogen, and oxygen. This list of course gives no idea of the complete manner in which the author has treated the subject. That our readers may form a more just estimate of the contents of the work, we propose to point out a few selected details. Thus the chapter on the halogens includes a description of the oxygen acids of these elements and the organic halogenides; under the sulphur group we have, in addition to the reactions of the sulphides, sulphites, and sulphates, a discussion of the characters of the dithionates, polythionates, and organic sulphur compounds; under nitrogen we find ammonia, the oxides and acids, and organic nitrogen compounds (ammonium compounds are treated of as an appendix to the potassium group of metals); the acids of phosphorus are considered in great detail, and a section devoted to organic phosphorus compounds. The chapter on carbon includes the analytical characters of a large number of organic bodies, *e.g.*, cyanogen and its compounds, the fatty acids, the acids of the lactic and oxalic series; also a section on the ultimate analysis of carbon compounds. Under hydrogen the author treats of water, and under oxygen we find remarks on ozone, hydrogen peroxide, and a very complete section on the detection of this element in a combined state. This division concludes with a "Summary of operations available for the detection of the *non-metallic* constituents of substance in general, and of the *inorganic acids* in a mixture of metallic salts in particular."

Having mastered the analytical reactions of the metals and non-metals the student is, in the concluding division of the work, introduced to the analysis of substances of unknown composition. The preliminary chemical examination is conducted in the usual manner—some of the substance is first heated *per se*, then in a current of air, with "bisulphate of potash," with soda-lime, a mixture of caustic soda, nitre, &c. Then follows an account of the well-known flame and blowpipe reactions and of Bunsen's "film tests." The preliminary examination in the wet way is next undertaken, and this is followed by a section on "methods of disintegration for some of the more frequently occurring classes of substances." With regard to the exhaustive analysis of complex mixtures, the author contents himself with a few general remarks, leaving it to the student to apply the methods acquired in working through the foregoing portions of the book, instead of guiding him, or, we should rather say, binding him down to the usual "tables."

It would be invidious on our part to institute comparisons between the present and any existing work on the same subject; but, considering the volume as the expression of the method taught by Prof. Dittmar, we are of opinion that it stands on a decidedly higher level than the generality of such works.

Although the author's accuracy is throughout unimpeachable, there are some few questionable, or at least debateable points, which demand a passing notice.

In the first place, we regret to see the occasional appearance of what we must consider badly-constructed phrases such as the following (p. 44):—"To students who have not yet got far enough advanced to invent their own methods." Then, again, we hardly know whether to admire or to condemn the frequent inconsistencies of formulation. To give a few examples:—Phosphoric acid is written in different parts of the book in no less than four different ways: thus, at p. 11, $HHHPO_4$; p. 253, $PO(OH)_3$; p. 244, $P_2O_5 . 3H_2O = 2PH_3O_4$; at pp. 244-245, the metallic phosphates are written $PO_4HR'_2$, PO_4H_2R', and $PO_4R'_3$, and on the same page metaphosphoric acid is written HPO_3. Then on the same page (41) we find two nitrates thus formulated: $HgON_2O_5$ and $Bi(NO_3)_3$. Now, although we admire the spirit which leads a writer to adopt these different modes of formulation as being a spirit of independence, which in the *original worker* shows that he is not the slave of any hypothesis, we think that the case is entirely altered when we have to deal with *students* of the science, nothing shaking the faith of a learner so much as an apparent want of consistency.

We cannot conceive why the author has gone back to the old nomenclature—"nitrate of potash," "bisulphate of potash," "phosphate of soda," &c. Although consistency is displayed throughout the book in this matter we cannot sanction the use of a system of nomenclature which, if not entirely obsolete, is rapidly becoming so among the scientific chemists in this country. Be it understood that our protest is here again entered solely from a student's point of view. It must perplex the learner to find out that the substances he has been employing in Prof. Dittmar's laboratory under the names of "bisulphate of potash," "phosphate of soda," &c., are known elsewhere as "potassic disulphate," "hydric potassic sulphate," or "hydrogen potassium sulphate," "hydric disodic phosphate," or "hydrogen disodium phosphate," &c.

With these remarks we may conclude our notice of what we venture to look upon as a valuable addition to our literature of the important subject of chemical analysis. We are confident that Prof. Dittmar's work will stand, by virtue of its own merits, as a scheme for instructing in this branch of the science.

We have in these columns (vol. xi. p. 107) formerly expressed the opinion that the systems of analysis in general use in our schools of chemistry need reforming in certain particulars. Thus in the article referred to we found occasion to complain of the want of chemical science displayed by the majority of students practising analysis according to certain cut and dried systems of "tables." It must be conceded that the student who is thoroughly well grounded in the *scientific principles* involved in chemical analysis must take a higher position than he who works blindly from a given set of rules. That some such idea is entertained by the author of the present work is shown by the fact that the three first divisions of the book are considered enough to furnish a sufficient groundwork of scientific principles to enable the use of tables to be dispensed with altogether when the analysis of complex mixtures presents itself so that both teachers and students may now congratulate themselves on possessing a work in which a step has been taken in the right direction—a system which brings into exercise the thought, knowledge, and judgment of the analyst, instead of leaving him a mere helpless machine forced to proceed in the fixed direction laid down in this or that set of "tables." R. M.

RICHARDSON'S "DISEASES OF MODERN LIFE"

Induced Diseases of Modern Life. By B. W. Richardson, M.D., M.A., F.R.S. 8vo. Pp. 520. (London: Macmillan and Co., 1876.)

HEALTH is proverbially one of the greatest blessings man can enjoy, and yet in this hardworking, hurrying, struggling age, many a one deliberately sacrifices it in the endeavour to succeed in his pursuits, commercial, literary, or scientific. Success is the object of their desires, and they are quite willing to pay for it the price of broken health and shortened life. This is even more the case with literary and scientific than with commercial men, for the latter generally look forward to several years of retirement and ease as a reward for their labours, while the former are rather anxious that their work itself shall be such as to secure them a certain place among the world's great ones, than concerned whether their fame be posthumous or not. In struggling to accomplish it they too often forget that "the race is not to the swift," but rather to the long enduring, and that if Cuvier or Darwin had died before reaching middle age, not only would their names have remained comparatively unknown, but science would have sustained an irreparable loss. Sometimes the worn-out body reminds them only too forcibly of the dependence of the mind upon it, work becomes impossible, every occupation must be renounced for a time, and the vantage ground which has been gained by unremitting toil is entirely lost. Nay more, the exhausted energies require a long time to recover; when work is resumed it can rarely be carried on with the same vigour as before, and meanwhile some slower but steadier competitor steps in front and wins the longed-for prize, or makes the eagerly-desired discovery. Several years ago we began to ascend the long flights of steps which lead to the higher part of the island of Capri, at the same time with another party. They ran briskly up while we went slowly on, and they reached the top of the first flight while we were only half way up. But here they were out of breath and stopped to rest. We, on the contrary, never stopped; if breath began to fail we went more slowly, but we never stood still. The consequence was that we passed the other party about the middle of the second flight, and by the time they had reached its top, we had mounted the third. In such a competition as this the increasing difficulty of respiration soon warns a man to stop, but in the life-long struggle for existence it is not so easy for one to know when he is getting out of breath and to relax his exertions in time. As a help to do this Dr. Richardson's work is most valuable, for he paints in vivid colours the symptoms of disease from worry and mental strain, beginning with the slighter ones of restlessness, irritability, and "an overweening desire to do more and yet more work," and ending with dementia, diabetes, &c. He gives a most salutary warning to those who strive to counteract the effects of mental overwork by adding to it hard bodily exercise, and his remarks on physical strain should be carefully perused by all young athletes. If his cautions were constantly attended to, we would have fewer instances of break-down either mental or physical. The effects of the passions on the body are next taken up, and then the action of alcohol and tobacco discussed at length.

Dr. Richardson seems to regard alcohol as an unmitigated evil, and although he acknowledges that sometimes tobacco may be useful in soothing the excited brain, he omits this beneficial action from the summary which he gives of the effects of smoking, and includes only the baneful effects which follow the abuse of the weed. This part of his book recalls to our mind a lecture in which the late Prof. Hughes Bennett denounced pastry as one of the chief causes of consumption. No one can doubt that pastry, alcohol, and tobacco are all capable of abuse, but whether their use is to be entirely prohibited on that account is an entirely different question.

The chapters on disease from the use of narcotics, and from late hours and broken sleep are especially interesting and instructive; and that on disease from food contains some most useful remarks on the injurious effects of too much tea, coffee, soda-water, seltzer, and sweets, as well as on the consequences of over-eating.

In treating of diseases from impurity of air the author mentions the bad effects of stoves, but he might also with advantage have drawn attention to the languor and inability to work which may be induced by burning much gas in the room where one is thinking or writing. He might have mentioned the Italian proverb, that when you have built a house you should make your enemy live in it for the first year, your friend for the second, and should inhabit it yourself in the third; but his observation of the occurrence of eight cases of consumption and fourteen of rheumatic fever in one row of pretty houses during the first two years after they were built may perhaps convince people of the danger of inhabiting damp dwellings, without any additional testimony.

Other chapters deal with diseases incident to some occupations, disease from sloth and idleness, from errors of dress, from imitation and moral contagion, automatic disease and hypochondriasis, and intermarriage of disease. The book concludes with a summary of practical applications or short directions how to avoid or counteract the sources of disease already discussed.

The work is of great value as a practical guide to enable the readers to detect and avoid various sources of disease, and it contains in addition several introductory chapters on natural life and natural death, the phenomena of disease, disease antecedent to birth, and on the effects of the seasons, of atmospheric temperature, of atmospheric pressure, of moisture, winds, and atmospheric chemical changes, which are of great general interest. In several points we do not agree with Dr. Richardson; we would like him sometimes to give fuller reasons for his dogmatic statements; we think he has perhaps pictured the effects of overwork in too glaring colours, and we think he has been somewhat unfair to alcohol and tobacco. But his book is most suggestive; it is written in a most attractive style, and it may assist the work and prolong the days of some who are unwittingly destroying their health, if they will only learn and attend to its warnings and counsels.

OUR BOOK SHELF

Over the Sea and Far Away, being a Narrative of Wanderings Round the World. By Thomas Woodbine Hinchliff, M.A., F.R.G.S., President of the Alpine Club. With Fourteen Illustrations. (London: Longmans and Co., 1876.)

Mr. HINCHLIFF, who is already known as the author of one or two pleasant narratives of travel, managed, in one year, to do 36,000 miles of ocean, besides spending a considerable time in exploring various regions of America and Asia. His reasons for writing this considerable book on his tour of the world are to induce other tourists to follow his example, not in writing a book, but in leaving the beaten paths and learning something about and enjoying the many beauties of South America especially, and also because he believes there is abundant room for a further and more detailed account of the natural aspect of many of the countries visited, "especially with regard to their scenery, their flowers, ferns, and fruits." We are bound to say that Mr. Hinchliff, from these points of view, has fairly justified the publication of the present work. He writes in excellent spirits, tells clearly what he saw, keeps up the interest from beginning to end, and the general reader, at all events, will find many things in the book quite new to him. Mr. Hinchliff spent most of his time in Western North America, in California, and the Yosemité Valley especially, in Brazil, Peru, and Japan. He is a good and enthusiastic botanist, a shrewd observer, and a clear narrator. He managed to see a great deal that was well worth seeing of the countries visited, their products, and their inhabitants, and although he opened up no new ground, he has been able to suggest aspects and describe phases that, we daresay, even those familiar with the literature of travel will recognise as original. The illustrations are good and appropriate, and altogether we can recommend the work as a really interesting and instructive record of a long tour.

Une Réforme Géométrique. Introduction à la Géométrie descriptive des Cristalloïdes. Par le C$^{\text{te}}$ Léopold Hugo. (Paris, 1874.)

Géométrie Hugodomoïdale, anhellénique, mais Philosophique et Architectonique.

La Question de l'Équidomoïde et des Cristalloïdes Géométriques. Par le C$^{\text{te}}$ Léopold Hugo. (Paris, 1875.)

ÉQUIDOMOÏDE : Sphère : : Prisme : Cylindre. "Équidomoïde, c'est en effet le nom que j'ai proposé pour la figure polygonale qui se place *avant* la sphère, comme le prisme et la pyramide se placent avant le cylindre et le cône en vraie philosophie. Il y a donc des équidomoïdes trigonaux, tétragonaux, pentagonaux, et ainsi de suite jusqu'à ce qu'on arrive à la sphère, leur sœur cadette . . . mon nouveau système, envisageant toutes les figures polygonales qui sont les aînées de famille de tes (the extract is taken from a hypothetical address to Archimedes), sphéroïdes et conoïdes, leur donne le nom générique de *domoides;* puis j'y fais adjonction, comme préfixe, des syllabes caractérisant les trois sections coniques; d'où regulièrement ellidomoïde, paradomoïde et hyperdomoïde. D'autres figures, moins chargées, au contraire, que la pyramide, sont dites *trémoides*. Ce seront toujours, domoïdes et trémoïdes, des corps ou solides polygonaux, ou du moins considérés comme tels, et les rapports caractéristiques $\frac{2}{3}$, $\frac{1}{2}$, se constitueront le lien commun dans chacune des diverses familles. Ce sont choses que les curieux peuvent étudier dans mon ouvrage: *Théorie des Cristalloides*."

"Geometria renovata. Création d'une géométrie nouvelle, d'une morphologie architectonique. Geometria philosophica. Doctrine préexcellente; de même que le polygone engendre le cercle, de même les cristallodomoïdes engendrent les sphéroïdes; geometria Hugodomoidica sive Hugodomoidalis! geometria asphéristica! de même l'équidomoïde engendre la sphère!"

"Circulaire à messieurs les mathématiciens (on est très-poli dans cette géométrique-là):—'L'équidomoïde pré-archimédien a l'honneur d'informer votre seigneurie que par arrêté de S. E. le Commandeur Léopold Hugo, Président de la Géométrique Architectoni-primordiale, il a été nommé au poste occupé précédemment par la sphère, et qu'il s'y maintiendra envers et contre tous. L'équidomoïde espère que votre seigneurie voudra bien, ainsi que LL. AA. les Académies scientifiques, accueillir favorablement sa nomination et lui donner aide et appui contre les retours offensifs de la titulaire dépossédée. Il saisit cette occasion pour exprimer à votre seigneurie toutes les assurances de sa très-respectueuse consideration.

"'Equidopolis, le .'"

The motto is "Devise anti-archimédienne. L'equidomoïde va bien: le rebelle gagne du terrain! . . . . suppression de la sphère!"

We have, in a recent number of NATURE, given a sketch of a work by the same author. Now we let him speak for himself. When we say that there are "Placards singuliers," "Placards plus ou moins singuliers destinés à MM. les Elèves de Mathématiques (Pamphlet fantasia)," "Objurgation Hugodomoidale," "Inauguration Transatlantique,

"Yankee doodle went to town
Upon th' equidomody,
Cocked a feather to his hat
And called it cristallody!"

&c., &c., in many languages, we have furnished our readers with some idea of the two works before us. *Spectatum admissi, risum teneatis?*

Count Hugo is the author of at least six pamphlets; two more are in the press, and more in preparation, "and still they come."

Our latest acquisition is a sheet on "the Pan-imaginary theory (not the frying-pan)." "Here the space with $\frac{l}{m}$ dimensions gives birth, by its successive phases, to the *real space*, with *n* dimensions, and specially to the *natural space* with three dimensions, and to the sub-natural space with two dimensions, &c."

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

"The Recent Origin of Man"

THE letter of the author of the above work in NATURE, vol. xiii. p. 484, presents two points which demand an answer. 1. The reviewer is asked for his authority for the statement that palæolithic implements have been met with in Asia Minor. It is to be found in Evans' "Ancient Stone Implements," p. 571, and in Dawkins' "Cave-Hunting," p. 429. The discovery was made by the Abbé Richard between Mount Tabor and the Sea of Tiberias. 2. My opinion, which is also shared by some of the leading archæologists of Britain, that the interments at Solutré have not been proved to be palæolithic, has unfortunately evoked a charge of "ignorance and treacherous memory" from the author. I would merely remark that I am not ignorant of the account of Solutré in the "Matériaux," and in the "Archives du Muséum de Lyon," the latter of which is apparently unknown to the author, nor has my memory failed me concerning the debate on Solutré at the French Association, and the human skulls and implements which I then saw. Mr. Southall's argument as to the modern date of some of the reindeer, based on the percentage of gelatine in their bones, may be left to the tender mercies of Mr. Evans, and the comparison of the finely-chipped implements, with the Danish Neolithic finds, to those of M. de Mortillet, who takes them to be typical of one of the stages of the palæolithic period.

The discussion of the other questions raised in the letter, such as the Neolithic age of the *Rhinoceros hemitœchus* of the Gibraltar caves, or the reiterated assertion that the Irish Elk lived in Europe in the middle ages, is unnecessary in the present state of scientific inquiry. How an appeal to the mound at Hissarlik, to the discoveries at Alise, to the pile dwellings, to the food in the stomachs of fossil elephants and Mastodons, or to the recent elevation of Uddevalla can prove the "recent origin of man," may safely be reserved for decision to the judgment of the reader, without any comment from THE REVIEWER

On the Formation of Coral Sand

IN the best books on geology one finds that the formation of coral sand is attributed to trituration by the force of the surf, the waste of shells and minute globigerinæ, and even to the droppings of those fishes which are said to browse upon the living coral.

While residing at Santa Cruz in the West Indies about this time last year, my friend Mr. Quin, inspector of schools there, first pointed out to me the great importance of a certain seaweed in the formation of coral sand, and I had ample opportunity for verifying his observation while I stayed there.

A Coralline limestone is formed of coral blocks, consolidated coral-sand, and mud, shells, and myriad calcareous cases of minute organisms. Of these, next perhaps to the coral itself (of which I have seen great masses whose features were not quite effaced by percolation, &c., in the upheaved limestone of Santa Cruz), the bulkiest ingredient is the coral sand and mud, especially the sand, the shells and cases being of minor importance.

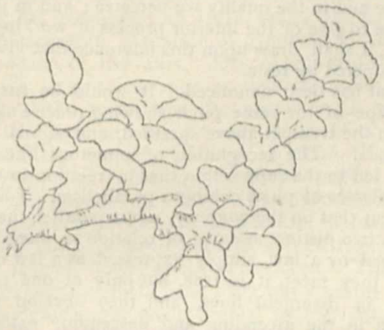
We are invariably taught, as far as I have seen, that coral sand is mainly formed of the trituration of the coral skeletons among each other, but it is difficult to see how this can be when one has seen both the sand and the skeletons, and the action of the surf which is mostly among the coral yet alive and cushioned with a vegetable matter. The coral skeleton is extremely hard and crystalline, and when two pieces of dead coral are rasped together by the action of the wave breaking over the reef they will triturate themselves into very fine grains. One can understand how the coral mud can be formed in this manner; but not so easily how the coral sand is formed. A glance at coral sand as it is seen forming the curving beaches in the pretty coves of the West Indian Islands shows that it is formed of *coarse* calcareous grains smoothed and rounded by the water, and of rather a soft friable nature, more like water-washed fragments of stucco or shell than crisp coral. On examining it more closely one sees that it is mainly composed of fragments and scales of soft calcareous matter of a mellow whiteness, and easily broken between the fingers. The larger of these scales have a peculiar shape, roughly like a half moon, whilst others are plainly only broken pieces of the larger.

Nor is the source of these far to seek. One finds everywhere strewed over the surface of the sand white bunches of a dead sea-weed, or rather of its calcareous skeleton, bleaching in the sunshine, every perfect leaf of which is one of these half-moon shaped scales, and all connected together by flax-like fibres. They have been cast ashore by high tides from the fringing reef. (See fig.)

In the reef itself, while sailing over it, one sees among the dark coral masses white sheets of coral sand, and when these are scrutinised more closely they prove to be almost entirely formed of these broken scales or leaf-skeletons.

One day I went with Mr. Quin to the outer edge of the reef at low water, and landed on its shoaling crest. Mr. Quin was provided with a very useful lens, wherewith to view the "wonders of the deep," in the shape of a square wooden box with a glass bottom, which on being set on the water and looked through, obviated the surface ripple, and permitted a clear view into the coral caverns, some of which, by the way, were of great beauty, natural aquarium tanks, hoary with mosses and sea-blooms, floored with coral sand and shells, and tenanted by curious fishes of the most brilliant and varied hues.

The huge rounded bosses of green growing coral among which the surf-breaks resembled much the moss-covered granite-boulders of a boggy Scotch glen. Here we found banks and beds of the coral sand where it is formed at our very hands.



The scales and half scales were here in a most perfect state, and seemed to make up almost the entire mass of sand. It was easy now to see the principal source of coral sand—at least at Santa Cruz—and that what is seen on the beach is merely what is found out here in a more finely divided state.

Over all the reef about us, growing plentifully, was the living weed which supplies these scales—the vegetable tissue covering the calcareous interior being of a dull-green like the living coral itself. I procured a specimen of the growing weed, and also of the sand from these beds where it is first formed and from the beach; but unfortunately I have lost these. I can only send you some of the dried skeleton, and append a rough sketch of it for the benefit of readers. JOHN MUNRO

West Croydon

Floating Radiometers

IN Mr. Crookes' paper reported in NATURE, vol. xiii. p. 489, occur the following words: "The envelope turned very slowly a few degrees in one direction, then stopped and turned a few degrees the opposite way." Assuming that this is rightly reported, it is inexplicable to me how Mr. Crookes could have written it. For, as the lawyers say, it is "void from ambiguity." The whole question between Mr. Crookes and Dr. Schuster appears to me to turn on the one point ignored by the former. When the rotation of the envelope began, in which direction was the first oscillation? To say that the envelope first turned in one direction and then in the other is simply to say that it oscillated, which, while it is a shorter mode of expressing the same thing, is an equally useless expression. The very nerve of the problem lies in the point omitted. If the first oscillation of the envelope was in the direction opposite to that of the mill, it is surely incontestable that the kick, which caused it, could not be the effect of any external force acting on the discs only.

Valentines, Ilford C. M. INGLEBY

Preece and Sivewright's "Telegraphy"

IT is neither usual nor becoming for authors to question the judgment of a reviewer in dealing with their works, and although I think that in your number (vol. xiii. p. 441) you have treated the little work by Mr. Preece and myself with some severity, I do not propose to depart from this wholesome rule. Nevertheless, I think it but right to point out that the reason why the scientific part of the subject was so far left out was because this had been already dealt with in another work of the same series. Prof. Fleeming Jenkin's text-book on "Electricity and Magnetism" had appeared before that on "Telegraphy" was undertaken. In the former "the part of science" had been so ably treated that it became unnecessary and would have been out of place to go over the same ground in a practical text-book which was to appear in the same series. J. SIVEWRIGHT

On the Nature of Musical Pipes having a Propulsive Mode of Action

IN the concluding paragraph of my last paper (NATURE, vol. xii., p. 146), I brought under notice the remarkable difference in the effect of increased diameter upon the two great classes of pipes, regarded by me as referable to the fact of the mass of air in the pipe being in the one class (that of pipes with reeds of wood or metal) under the influence of a propulsive current, and in the other class (that of pipes with reeds of air, or flue-pipes), under the influence of an abstracting current; the distinction thus manifested on the mode of action will, if clearly apprehended, enable us to reconcile many apparent anomalies in the behaviour of pipes perplexing to inquirers.

Considering a current simply as flowing, that is to say without the energy which the word propulsive implies, the nature of a tube or conduit is to cause friction between the walls of the tube and the particles of the substance flowing through the tube whether of air or of water. The friction of air upon air is also a calculable effect. In organ-pipes of the class now in question we have to recognise that we are dealing primarily with a current, with a true transport of air through a tube, a current propelled, abruptly arrested, and in a secondary stage converted into vibration; therefore all the conclusions arrived at concerning the propagation of waves of vibration in tubes are suggestively applicable here, and in practice we find these conclusions verified.

As regards ordinary tubes or conduits, Seebeck, following Regnault and Kundt, has shown (NATURE, vol. i. p. 456) that the effect of friction in retarding the velocity of a wave *in propagation* is not so insignificant as might be supposed; it is greatest upon those of tones of highest pitch, and it increases according as the diameters of the tubes are less. In musical pipes of the propulsive class exactly the same relations are preserved, and if two pipes of different diameters give the same pitch-note, then the pipe of larger diameter will prove to be of greater length, in fact the opposite of the law obtaining in pipes of the abstracting class. In a narrow pipe the friction is in excess, with an increased diameter the current gains greater freedom, and coincidently, that inner motion vibration is less impeded. Pipes of this class, for brevity here called propulsive in action, the trumpets, posaunes, bassoons, oboes, have this characteristic that the whole of the wind used passes through the body of the pipe and makes its exit at the upper orifice. In flue-pipes on the contrary the amount of wind actually passing up the interior of the pipe is scarcely noticeable. The form of trumpets and the like is conical, but the oboe has a special feature, its tube is very slender, slightly enlarging upward, until at the top it suddenly expands into a terminal shape called a bell. An actual comparison will afford the clearest illustration of the effect of form.

Two pipes of the standard pitch 256 vibrations per second:—

TRUMPET.
Sounding Mid C.

Diameter at root of reed	$\frac{3}{8}$	in.
Diameter at upper orifice...	3	,,
Length from tip of reed	23$\frac{3}{8}$	,,

OBOE.
Sounding Mid C.

Diameter at root of reed	$\frac{3}{8}$	in.
Diameter at upper orifice...	$\frac{3}{4}$	,,
Length from tip of reed	21$\frac{1}{4}$	,, (including bell)

The oboe bell is not ordinarily reckoned in the effective length, yet it is not altogether to be disregarded; from its juncture at the tube and up to the rim 3 inches, with diameter expanding from $\frac{3}{4}$ to 1$\frac{1}{2}$ inches. The influence of narrow bore will be best exhibited by comparing with these the orchestral oboe where the bore commences at $\frac{5}{16}$ and the note C is given by that portion without the bell, and which will measure from the finger hole to the tip of the reed only 19$\frac{1}{2}$ inches. In the Chinese organ, or "Little Shang," which, when in proper condition is most perfect in relation of tube to pitch, the pipe sounding C octave above oboe measures 9$\frac{3}{8}$ in length, including the beak, and the bore of the tube is barely $\frac{1}{4}$ inch, being cylindrical, not conical. The reed tongue is so very small that a larger bore would be disproportionate, the column of air seems well suited to the strength of the reed, the pitch does not quite accord with our organ or our concert pitch, but that will not affect the argument. What I am anxious to point out is that these varying relations of pipes result from a natural standard, which underlies all empirical changes. The true standard for all instruments of the propulsive class is *three-fourths of the half-wave length*, and then in accordance with varying diameters above stated all larger diameters demand length to be increased in ratio, and more allowance to be made for the diverging than for the uniform bore; greater or less *amount* of wind, and greater or less *degree* of pressure also enter into calculation, and practically are convertible in effect, the one doing duty for the other.

In the distinctive mode of action of this class we may find reasons for the varying relations of the pipes to each other, and for the contrasts shown in comparison with the other class acting under its own peculiar mode. In these propulsive pipes in both the wide and narrow scales, the wind-current, after entering the foot of the pipe or boot, passes into the body of the pipe by a very contracted inlet formed by a hollow plug usually of metal called "the beak," or more commonly "the reed," to the confusion of inquirers; properly named, as we see it in old authors, it is "the shallot," from its resemblance in shape to the once favourite esculent the eschallot; ordinarily we speak of "the tongue" the elastic strip of metal covering it, as "the reed," for in the clarionet this part is really a reed. The main impulse of the current passes into the cone of the pipe through the mass of air in a central direction, and thus in a wide pipe, as compared with its course in a narrow pipe, the current has exchanged the friction upon the sides for the lesser friction of air upon air, still restricted, but less so in degree as the cone expands, as of a swift river escaping the confinement of banks, flooding the quiet expanding delta, agitating its waters with gradually-decreasing strength, and then becoming diffused in surrounding ocean.

Utmost exactness in length is quite as important for pitch and tone in these as in flue-pipes. Although the reed tongue has a determinate pitch of itself, yet a proper length of tube to reciprocate its action is indispensable, any inaccuracy only "upsets the tone," as the technical phrase says, and gives rise to curious freaks of behaviour. The slim tapering oboe is so sensitive that if we make it a quarter of an inch too long, or if we merely pat the top of its bell whilst sounding, the tone will immediately leap to its third above—not to a harmonic—a problem as puzzling as that of some echoes falling successively by thirds.

The action of the air-reed as causing suction by the velocity of passage of wind over the mouth was illustrated by me in a previous paper by reference to the abstracting power of a current of air, as shown in the spray-diffuser where globules of liquid are lifted and withdrawn by its agency. The action in these beating reeds is also susceptible of as simple an illustration. Take six or eight feet of india-rubber tubing of $\frac{1}{8}$-inch bore, for this length defines action more clearly—coil the length round your hand, and placing one end in the mouth blow through the tube sharply, at the same time allow the tip of your tongue freedom of play near the orifice, and you will find it drawn suddenly to the tube by the suction of the current passing down it, and released only on the exit of the current into the atmosphere. Lightly pressing the coil in your hand you may likewise feel the throb of the passing air-pulse. The trombone-player is conscious of his lip being forcibly drawn into the cup by a like cause. A stream of air suddenly cut off cannot pass down a tube without leaving a vacuum behind it. Organ and orchestral trumpets and oboes, and all of like propulsive action, are subject to this power, and only through it are able to generate tones. Suction is thus seen to be the final cause of vibration, the vacuum exists until the initial pulse of the vibration has made exit at the outer orifice, or in the second and succeeding courses until the pulses reach the colliding point or place of the prime node. Always thus in every musical pipe the current is essential to the suction, but with the striking difference that in the flue-pipe there is continuity of stream, and the continuity of flow is made available by conversion in reciprocating motion, but in the propulsive class the action is effective through discontinuity, by abrupt cessations and sequences of stream.

Here also in the beating-reed pipes we come upon distinct evidence of the interval of rest lengthening the period of vibration. The pitch of beating-reeds is regulated to consort with the pipe by means of a tuning-wire altering the vibrating length of the tongue; thus regulated, the pitch may, however, within limited degree be altered by changing the force of wind, or by cutting off rim of pipe, or by adding thereto. Let it be observed that whether the tongue is pressed to the beak slowly or quickly, it will spring back in recoil in just the same time. By additional weight of wind, pitch may be raised, and in this case the tongue flies to more rapidly, but possibly any gain of speed in the advance may be counteracted by the recoil being impeded in the more compressed medium in which the tongue moves; the only remaining effect otherwise is that of an increased swiftness of the current of air more vigorously propelled in its course, and this in itself would account for the acceleration of pitch. On the other hand, leaving the force of wind constant, we may by temporary addition to the rim of upper orifice sensibly flatten the pitch, for the current takes longer time to pass this extra boundary, hence the tongue is in consequence held longer upon the beak by the suction, its recoil delayed, or in other words recognising the physical result, its interval of rest is lengthened.

Many indications that come before me in my experiments lead directly to the inference that in all wind instruments this interval of rest is an important influence both on the pitch we regulate and in the quality we perceive; and in the estimate I shall have to give of the interior process of working in the flue organ-pipe, I shall draw upon this inference that vibration is an activity tempered by rests.

One point has been unnoticed. It would be easy to find a diapason-pipe of the same pitch showing precise agreement in length with the trumpet above specified, and similarly for other various kinds. The recognition of numerous like correspondences has led to the supposition that in relation to wave-length these two classes of pipes exhibit a parallelism. I hope to have made it clear that on the contrary, never parallel, the two classes proceed on two distinct systems of relation to wave-length, and are governed by a law, simply expressed as a law of divergent variation; they meet, it is true, but only at one point, where they cross in divergent lines, and they develop in opposite phases both in the ascending and descending extents of their range, the pitch of the one rising under an enlarging, and the other under a diminishing diameter.

Beyond the particular effects of friction already stated, the agency of the friction of air in the sound of wind instruments appears to me inadmissible. Reasons for this conclusion will occupy another paper in connection with details of my experiments bearing thereon under a simple device somewhat on the principle of the siren, and which may be named a "displacement siren." HERMANN SMITH

Solar Halo

ON Saturday last at Penruddock station, between Penrith and Keswick, about one o'clock, I observed a solar halo which at first was not perfect, but showed a reddish tint in the arc below the sun. Afterwards the circle became complete and continued so with small intervals until about half-past four, when I went indoors. At five o'clock the halo had disappeared in the haze. The day was thick, so hazy indeed that I could hardly distinguish the outline of Saddleback from Penruddock. The colour disappeared when the circle was complete, but occasionally I thought I could distinguish a reddish tinge on the inner side of the arc. I had no means of accurately measuring the radius, but with two pieces of stick which I picked up I estimated the tangent at $\frac{4}{9}$, which would give nearly 24°. This is more than your correspondent Mr. Gledhill found in his observations, but my measurement is confessedly rough. JAMES HEELIS

April 21

Safety Matches

MR. TOMLINSON'S remarks on safety-matches in NATURE, vol. xiii., p. 469, reminded me that, not long ago I accidentally kindled one of those matches by rubbing it on the edge of a Wedgwood-ware mortar. This material appears even better adapted than those mentioned by Mr. Tomlinson for igniting such matches, and I found that a common earthenware dish (glazed inside) answered the same purpose admirably. I tried to ascertain the degree of certainty with which a safety-match could be kindled by friction against these two materials, and was surprised to find that they are little inferior in this respect to amorphous phosphorus itself. After a little practice in the manner of striking, it is easy to kindle nearly every match. Thus I have lighted forty matches out of fourty-four (most of them at the first or second stroke), using the glazed portion of the basin referred to. I should add that the surface becomes improved by use, which can hardly be said of the composition on the sides of the safety-match boxes.

Manchester, April 18 FRANCIS JONES

"The Ash Seed Screw"

THE delicate twist in the samara of the ash is clearly not that best calculated to retard descent. The more decided the twist, the greater number of revolutions will the samara perform ere reaching the ground, and the longer consequently will be the path through which its friction is exercised.

In seeking a model for a screw-propeller, we must remember that the pitch should *vary* with the velocity of propulsion.

London, April 15 WM MCLAURIN

OUR ASTRONOMICAL COLUMN

THE ROTATION OF VENUS.—It was Jean Dominique Cassini (Cassini I. as he has frequently been designated) who during his residence in Italy, made the first serious attempt to ascertain the time of rotation of this planet and the position of the axis. His observations with one of Campani's long telescopes appear to have been commenced about the middle of the seventeenth century, as related in the *Journal des Savans*, 1667, Dec. 12, but it was not until the evening of Oct. 14, 1666, that he perceived any spot of sufficiently definite aspect to be of service for the purpose in view. It is described as "Une partie claire située proche de la section, et fort éloignée du centre de cette planète vers le septentrion." At the same time several dusky spots were noted. These observations were continued till June 1667, but Cassini expressed himself very cautiously with regard to the inferences to be drawn from them. They appeared to indicate a return of the bright spots to the same position upon the disc at intervals of about twenty-three hours, but from the short time that the spots could be followed Cassini was unable to decide whether the appearances were to be attributed to an axial rotation or to a libration. "De dire maintenant," he says, "supposé que ce soit toujours la même partie luisante, si ce mouvement se fait par une libration, c'est ce que je n'oserais encore assurer, parce que je n'ai pas pu voir la continuité de ce mouvement dans une grande partie de l'arc, comme dans les autres planètes, et par cette même raison cela sera toujours très-difficile à déterminer."

In 1726 Bianchini, domestic prelate of the Pope, observing at Rome, with glasses, also by Campani, of 70 to 100 Roman palms in focus, remarked on Feb. 9 several spots which he continued to observe with the view to determine the time of rotation. His observations were published in "Hesperi et Phosphori nova Phenomena," 1728, and he considered them to show a period of rotation of 24 days 8 hours, the North pole of Venus being directed to longitude 320°, with an inclination of 15° only to the plane of the ecliptic. Bianchini's observations appear to have been made under very unfavourable conditions, whereby he was prevented from following the spots in a continuous manner. They were discussed at length by Jacques Cassini, the son of Jean Dominique, in "Elemens d'Astronomie" (1740), who arrived at the conclusion that a rotation of 23 hours 20 minutes would represent equally well his father's observations and those of Bianchini, while if the rotation assigned by the latter was admitted, it would be necessary to reject entirely the observations of the elder Cassini, "comme n'etant qu'une apparence trompeuse."

Jacques Cassini mentions that after Bianchini had communicated to him the observations at Rome, he made attempts to discern the spots upon Venus at Paris. He examined the planet on a great number of occasions with a glass of 114 feet focus, one of the best produced by Hartsoëker, and also with one of Campani's, of 120 Roman palms' focus, which had been tried by Bianchini and considered excellent, but with all the precautions taken neither he nor Maraldi could perceive any distinct spot upon the planet's disc.

Schroeter, in 1789, examining Venus with a 7-feet reflector, discerned a bright spot in the dark hemisphere, and by following the appearance of this object, inferred that the planet rotated in 23h. 21m. 19s., thus supporting the result obtained by Jacques Cassini from his father's observations. Schroeter's observations appear in D. J. H. Schroeter, "Cythereographische Fragmente, oder Beobachtungen über sehr Betrachtlichen geberge und rotation der Venus," Erfurt, 1792; and in "Aphroditographische Fragmente, &c.," Helmstadt, 1796. In an appendix to the latter work, noticed by Zach in "Monatliche Correspondenz," xxv. p. 366, it is stated that observations of "atmospheric spots," and of the horns, with eight determinations of "a definite point upon the surface," give for the final value of the rotation-period of Venus, 23h. 21m. 7·98s.

De Vico's observations and investigations bearing upon the time of rotation and the position of the axis are published in "Memoria della Specola . . . in Collegio Romano," 1840–41, p. 32, and in the succeeding part of the same for 1842, p. 29. The period of rotation assigned from these observations, which were made with the Cauchoix refractor of the Roman College, is 23h. 21m. 21·93s. (sidereal time). The longitude of the ascending node of the equator of Venus upon her orbit is fixed to 56° 30′, and the inclination thereto 53° 11′, while for the same elements referred to the ecliptic we have 57° 19′ and 49° 57′. There is some error of the press or of calculation here which it is not easy to rectify. In a note to Secchi's Life of De Vico, "Memorie dell'Osservatorio . . . in Collegio Romano," Anno 1850, p. 140, the inclination of the equator of Venus to the ecliptic is given, 53° 11′ 26″, and the longitude of the ascending node of the equator upon the ecliptic for 1841, 57° 19′ 18″.

Notwithstanding the near agreement of De Vico's period of rotation with that assigned by Schroeter, it must be admitted that further investigation is very essential before we can consider the period established. There are so many negative observations upon record and these made under circumstances at least as favourable as those upon which the rotation of the planet has been supposed to be fixed, that there is ample justification for doubt in the matter.

We hear from more than one correspondent that dusky spots have been suspected upon the disc of Venus, within the last few weeks; if there be no illusion, the present may prove a favourable opportunity for attempting a new determination of the rotation-period, and this consideration has suggested the above outline of the actual state of our knowledge upon the subject.

MINOR PLANETS.—By a note from Herr Palisa it would appear that the small planet observed at Pola on November 22 and 23 was not, as supposed at the time, identical with the one he had detected on November 8. No. 155 is therefore lost or in similar predicament to the planet observed by Watson, 1873, July 29.

M. Leverrier's *Bulletin International*, of April 22, announces the discovery of another small planet at Paris, by M. Prosper Henry, during the previous night, in R.A. 14h. 9m. 58s., N.P.D., 102° 18′.

PROF. FLOWER'S HUNTERIAN LECTURES ON THE RELATION OF EXTINCT TO EXISTING MAMMALIA[1]

VIII.

THE existing families of the Carnivora, spoken of in the last lecture, do not appear to have been distinctly differentiated in the Eocene period, at all events not till towards its close, but the order was represented by other and very singular forms, the systematic position of which is not easy to determine. The earliest in point of time is *Arctocyon primævus*, from the lowest Eocene of La Fère, Aisne, France, an animal nearly as large as a wolf, with a long tail, and heavy, strong limb bones, and

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[1] Abstract of a course of lectures delivered at the Royal College of Surgeons "On the Relation of Extinct to Existing Mammalia, with Special Reference to the Derivative Hypothesis," in conclusion of the course of 1873. (See Reports in NATURE for that year.) Continued from p. 488.

remarkable for the exceedingly small size of the brain cavity, compared with the arches and ridges of the skull developed for muscular attachments. This character has been supposed to indicate marsupial affinities, but the rest of the osteology, as far as known, does not favour this view. The lower jaw has not been found, but the cranium shows the full complement of teeth so frequent in Eocene mammals. There are three broad tubercular molars behind the trihedral sectorial, which indicate that the animal was rather omnivorous than truly carnivorous in its habits. Another genus which includes many species of various size, and having a wide geographical range, being found in late Eocene and early Miocene deposits in France, Germany, England, and North America, is called *Hyænodon*. It also has been by many naturalists placed among the marsupials on account of the peculiarities of its dentition, which is certainly without parallel among placental Carnivores. It possesses the primitive or typical dental formula of the Eocene mammals, and the incisors, canines, and premolars, are not unlike those of a dog, but the three true molars, both above and below, are shaped like the sectorial teeth of a cat or hyæna, and increase in size from the first to the last, and thus there are no teeth formed like the "tuberculars" of ordinary Carnivores. This repetition of the sectorial character in the true molars occurs in the carnivorous marsupials, though the general structure of the skull, and limb bones as far as they are known, including the position of the lachrymal canal within the orbit, will not permit our placing *Hyænodon* in that group. Many of the lately discovered American Eocene carnivores presented the same peculiarity of several successive molars having sectorial characters. One of these from Wyoming, apparently allied to Hyænodon, has been described by Cope, under the name of *Mesonyx*, and another still more aberrant form, as *Synoplotherium*. The inferior canines project forwards, and are closely approximated, the incisors (at all events in the aged specimen on which the genus was founded) being absent. The molar teeth were so much worn that their characters cannot be satisfactorily made out. The most interesting features of these animals are in the structure of the feet, the ungual phalanges being flatter and broader than in any existing Carnivora, and grooved above, and the scaphoid and lunar bones of the carpus not being united as in all existing Carnivores.

These naturally lead to the consideration of some animals, the remains of which have been discovered in the same locality and formation, of such anomalous construction, that they cannot be placed in any of the known groups, and for which Prof. Marsh has constituted the order *Tillodontia*. The type of the order *Tillotherium*, Marsh, is described as having a skull with the same general shape as in the bears, but in its structure resembling that of Ungulates. The molar teeth are of the Ungulate type, the canines are small, and in each jaw there is a pair of large scalpriform incisors faced with enamel, and growing from persistent pulps, as in Rodents. The adult dentition is $i\,\frac{2}{2}\;c\,\frac{1}{1}\;p\,\frac{3}{2}\;m\,\frac{3}{3}$. The articulation of the lower jaw with the skull corresponds to that in Ungulates. The brain was small and somewhat convoluted. The skeleton most resembles that of Carnivores, especially the *Ursidæ*, but the scaphoid and lunar bones are not united, and there is a third trochanter on the femur. The radius and ulna, and the tibia and fibula, are distinct. The feet are plantigrade, and each had five digits, all terminated by long, compressed and pointed ungual phalanges, somewhat similar to those in the bears. Judging from the figures and description, this animal is the same as that of which a lower jaw was previously described by Leidy as *Trogosus castoridens*, and which is perhaps identical with *Anchippodus riparius*, described by the same naturalist at a still earlier date, from a single tooth from New Jersey. If this identity can be satisfactorily established, the latter name must be adopted, but as the lower molars of so many very different animals bear a close resemblance to each other, it is not very easy to do so, and the whole history is a good illustration of the inconvenience that often arises from the practice of giving names to minute and isolated fragments.

In some of its dental and osteological characters, *Tillotherium* or *Anchippodus* bears some resemblance to the *Rodentia*, but the definition of that order would have to be widened considerably before it could be admitted within its bounds. *Mesotherium*, spoken of in the third lecture, has better claims to be considered a Rodent, though certainly a very aberrant one. Leaving this animal aside, palæontology tells us nothing of connecting, or even of more greatly generalised forms of Rodents, or affords any better indications of the affinities of the order than can be derived from the study of its living members. Nearly all the existing families have been well represented throughout the Pliocene and Miocene epochs, and the earliest known Rodents, those of the Upper Eocene, do not appear to have been more generalised than the existing species.

Numerous species of extinct *Insectivora* have been described from various formations from the Upper Eocene to the present time, both in Europe and America, but their characters and affinities have not been thoroughly worked. The European species mostly belong or are allied to genera now existing. It has been suggested that some of the generalised American Eocene Carnivores may possibly be gigantic *Insectivora*, though in the actual fauna of the world there are no connecting links between these orders. It is also not certain whether some of the mammals of the Mesozoic strata may not be placental *Insectivores*.

The *Chiroptera*, or bats, differ strikingly from all other mammals in the adaptation of the fore-limbs as organs of true flight. Their origin is an extremely interesting question to the evolutionist. No existing forms throw any light upon it, and what little is known of the past history of the order shows that its general characters and geographical distribution have not changed materially during the Tertiary period. All the bats found fossil in the Brazilian caves resemble those now inhabiting the same country, though it is true these only go back as far as Pleistocene ages. In France, however, remains of bats have been found in the Miocene and Upper Eocene (Paris gypsum), but all belonging to the *Vespertilionidæ* and *Rhinolophidæ*, families now existing in Europe, and in the earliest known forms no signs of generalisation have been detected, nor have any of the intermediate stages between the ordinary mammal and the bat, if they ever existed, yet been discovered. No fossil remains of the large fruit-eating bats, or *Pteropi* have been found.

(*To be continued.*)

PROF. HUXLEY'S LECTURES ON THE EVIDENCE AS TO THE ORIGIN OF EXISTING VERTEBRATE ANIMALS[1]

V.

WE saw in the last lecture that the differences between birds and reptiles were very great; nevertheless, many of them tend to disappear on a closer examination. For instance, the extremely avian character of the absence of teeth, and the presence of a horny beak, is found in turtles and tortoises; that of the penetration of the bones by air cavities exists in the skull of crocodiles; and, although no existing reptile possesses the power of flight, or a fore-limb in any way approaching in structure to a bird's wing, yet, in the crocodiles, the fourth and fifth digits—those we found to be wholly absent in the bird—are much smaller than the others, and have no claws.

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[1] A course of six lectures to working men, delivered in the theatre of the Royal School of Mines. Lecture V., March 27. Continued from p. 469.

On passing to the internal organs, and the mode of development, we find far greater points of resemblance; as to the latter, in fact, the correspondence is wonderful, the account given of the development of a reptile (NATURE, vol. xiii., p. 429), applying in every respect to that of a bird.

On the whole it is certain, from anatomical characters alone, that birds are modificatons of the same type as that on which reptiles are formed, and if this similarity of structure is the result of community of descent, we should expect to find, in the older formations, birds more like reptiles than any existing bird, and reptiles more like birds than any existing reptile. If the Geological record were sufficiently extensive, and the conditions of preservation favourable, we ought to find an exact series of links, but this, of course, is hardly to be expected, and it will be a great step if we can show that certain forms tend to bridge over the gulf between the two groups.

Let us see, then, what the facts of Palæontology tell us in this matter: and first, as to birds.

It is a curious fact that, just as in the case of Crocodiles, all the birds found in the Tertiary deposits differ in no essential respects from those of the present day. Great numbers of remains have been found in beds of Miocene age—beds found at the bottoms of great lakes—and the very perfectly preserved specimens show, beyond any doubt, that the Miocene birds are referable to precisely the same groups as those of our own time. Our knowledge of the Eocene forms is less perfect, but enough is known to show that the same fact held good at the commencement of the Tertiary epoch.

Throughout the secondary period remains of birds are very rare; until lately, in fact, there were none at all. But within the last ten or fifteen years some remarkable discoveries have been made—one or two in Europe, and a whole series in America, which give us some very precise information as to the nature of the Mesozoic birds.

Two of the most interesting of these—the genera *Hesperornis* and *Ichthyornis*—occur in certain beds in the United States, corresponding in age to our later Cretaceous. Hesperornis is stated, by its describer, to have had nearly the organisation of our Northern Diver (*Colymbus*); it was five or six feet in length, of swimming habits, had small wings, like those of the Penguin or Auk, and a long beak like the Diver. But—and this is the interesting feature in its organisation—both jaws were beset with teeth: not mere serrations of the jaw, such as many existing birds have, but true teeth like those of a reptile. Here then we have the appearance of a true reptilian character.

Ichthyornis was, in some respects, even more curious. It was about as large as a good-sized pigeon, had large wings adapted for powerful flight, and teeth in both jaws, like Hesperornis. In another character it showed a still greater approximation to the lower reptilian type: the bodies of its vertebræ, instead of having the cylindroidal or saddle-shaped form so characteristic of nearly all birds,[1] were bi-concave. Thus, in tracing birds back in time, we find a parallel series of modifications to those described in the Crocodilia.

Beyond this point, the history of birds is almost a blank, the only other remains being—curiously enough—one or two feathers, and the *Archæopteryx* of the Solenhofen slates, a formation which has been of great service in the preservation of organic remains, the same qualities which make it so useful for purposes of lithography having fitted it for the preservation of even such perishable structures as jelly-fish.

Archæopteryx, known only by a single specimen now in the British Museum, was a bird about the size of a crow. Its head is unfortunately wanting; its tail is quite unlike that of any existing bird, being long, composed of a great number of vertebræ, and having two rows of feathers attached, one to each side of it. The leg is quite like that of any ordinary perching bird. Unluckily, the bones of the wing are detached, so that the exact structure of the manus is not known, but it is quite certain that the metacarpal bones were not united together, but were separate and terminated by distinct claws; there was thus an approximation in structure to a true fore-paw. Long quills were attached to the wings, and both they and the tail-feathers are in an exquisite state of preservation.

With Archæopteryx we come to the end of all precise information as to the history of birds, and the only possible trace of the group in earlier formations are certain footprints found in the Trias of Connecticut, and referred to the genus *Brontozoum*. These were prints of some gigantic three-toed animal, which certainly walked on its hind legs, and was always supposed to be an ostrich-like bird until some recent discoveries, presently to be mentioned, have shown that Brontozoum may have been a reptile.

It would at first seem easy to show an equally striking approximation of reptiles to birds, for we have, throughout the greater part of the secondary rocks, and notably in the Solenhofen slates, remains of a group of reptiles known as *Pterodactyles*. These remarkable creatures had teeth set in distinct sockets, sometimes extending to the end of the long snout, sometimes stopping short, and having their place taken by a horny beak. The neck was long; the sacrum consisted of from three to six vertebræ; the tail was short in some, long in other genera. The breast-bone had a great keel, like that of a bird, the shoulder-girdle was also quite birdlike, as also were the humerus and the bones of the fore-arm. The manus, on the other hand, was quite different to anything found in birds; the first, second, and third digits were of the usual reptilian character and bore claws, but the fourth was immensely prolonged, produced downwards, and clawless. The pelvis was, in some respects, birdlike, in others quite peculiar: the hind-limb was reptilian.

It is certain that the Pterodactyles were animals of flight, and that there was a membrane, like a bat's wing, stretched between the fourth finger and the sides of the body; it is also certain that it was unable to walk, though it may have used its hind-limbs, as bats do, for hanging itself head downwards from branches.

Although these creatures are, in many respects, very birdlike, yet it can hardly be said that they give us any direct help, or that they connect reptiles and birds any more than bats connect birds and mammals. Their avian characters seem to have been purely adaptive, or produced in relation to their peculiar mode of life, and we must therefore try some other line of reptiles for the origin of birds.

In the rocks from the Trias to the later Cretaceous there are, in many places, abundant remains of a group of wholly extinct terrestrial reptiles known as *Dinosauria*. Most of these are of great size, the genus *Iguanodon*, for instance, must have attained a length of fully thirty feet. Our knowledge of most of them is imperfect, but many points of the greatest possible interest are perfectly well known.

Some genera have the snout turned downwards like a turtle's beak, and both it and the large lower jaw were ensheathed in horn. In some the vertebræ are slightly excavated on both faces, and are penetrated with air cavities. The shoulder-girdle consists of a long blade-bone and a short coracoid like that of many lizards. Of the fore-limb nothing is known for certain in the larger species. The sacrum is composed of as many as six vertebræ, which often take on a remarkably birdlike character. More curious still, the ilium has a great forward process,

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[1] In this peculiarly avian form of vertebra, the front face of the centrum is convex from above downwards, and concave from side to side, the hinder face being concave from above downwards, and convex from side to side. The Penguins have the dorsal vertebræ opisthocœlous, *i.e.*, with a ball in front and a cup behind.

and the ischium and pubis are both turned backwards, parallel with one another, so as to have almost exactly the same position as in birds. There can be no doubt about this most remarkable point now, as the parts have been found in place in the genus *Hypsilophodon*. The femur was evidently brought parallel to the long axis of the body, and it has the characteristic ridge between the places of articulation of the tibia and fibula. The tibia has a great crest on its front surface, the fibula is quite small, and the flattened end of the tibia fits on to a pulley-shaped bone exactly like the ankylosed astragalus of a bird. The middle or third toe is the largest, and the outer and inner toes small; the metatarsals, although separate from one another, have their faces so modelled that they must have been quite incapable of movement. Substitute ankylosis for ligamentous union, and a bird's metatarsus is produced; in fact the whole structure of the Dinosaurian hind-limb is exactly that of an embryonic bird.

In the very remarkable genus *Compsognathus* of the Solenhofen slates, which is nearly allied to the Dinosauria, and included, with them, in the order *Ornithoscelida*, the head is small, the neck extremely long, and the peculiarities of the hind-limb are entirely bird-like; it also seems that the tibia and astragalus were actually united. The fore-limb, moreover, was very small, and it is certain that Compsognathus must have walked on its hind-legs.

The question, then, naturally arises, did the gigantic Dinosauria, such as Iguandon and Megalosaurus, have the same mode of progression? This seems, at first sight, hard to believe, but there is considerable reason for thinking that it may have been the case, for, in the case mentioned above of the great three-toed footprints of the Connecticut valley and others found in the Wealden formation, no impression of a fore-foot has ever been found; so that, even if we suppose that the impressions of the fore-feet were entirely obliterated, as the animal walked, by those of the hind-feet, the former must, at any rate, have been very small.

When we consider what a very strong piece of evidence this is, we are forced to the conclusion that the evolution of birds from reptiles, by some such process as these facts indicate, is by no means such a wild speculation as it might, from *à priori* considerations, have been supposed to be.

(*To be continued.*)

THE UNEQUAL DISTRIBUTION OF RARE PLANTS IN THE ALPS[1]

M. DE CANDOLLE has recently distributed copies of a paper communicated by him to the Botanical Congress held at Florence in 1874, in which he explains in a very convincing manner a fact which all botanists have noticed in Switzerland, but the causes of which have not hitherto been properly understood. No one is better acquainted with the plants of the Alps than Mr. Ball, and M. De Candolle prints as a text to his paper a remark made by the well-known author of the Alpine Guide, that it is matter of curious inquiry to ascertain why the vegetation of certain districts of the Alps is more varied than that of others.

Two instances to illustrate this will be sufficient. The mountain chain situated between Italy and the Valais is rich in rare and local plants, while that between the Valais and Canton Bern is very poor; again, after tabulating the species found in Switzerland in single cantons only, while sixty-three are peculiar to the Valais, the Canton Bern has but one.

The explanations which have been given hitherto have mainly rested on existing physical causes. Wahlenberg, at the beginning of the century, insisted upon the action of soil and climate. Perrier and Songeon have endeavoured to correlate the distribution of plants with that of different geological formations. Grisebach, more recently, cuts the knot by supposing that the Alps have been a centre of vegetation, and that their present distribution is an ultimate fact.

De Candolle has sought the true reason in the circumstances which accompanied the retirement of the glaciers at the close of the glacial period. "The valleys and groups of mountains which have at present a maximum of rare species and the most varied flora, belong to districts in which the glaciers disappeared soonest. On the other hand, where the duration of snows and glaciers has been most prolonged, the existing flora is poor."

The objection which may be made that a cause so remote can hardly influence the present distribution, is met by pointing out the extreme slowness with which a vegetation establishes itself, and the persistence with which it maintains its *status quo* when so established. Thus the rare plants for which the botanists of the sixteenth century were accustomed to visit particular localities may still be gathered there. Again, the Rhone valley is intersected by numerous moraines; the lower and more ancient are covered with chestnuts, while the higher are more and more barren and still covered only with pines.

From a variety of causes which De Candolle enumerates, it seems probable that the southern and eastern glaciers of the Alps were of smaller extent than the northern, and would consequently be the soonest to retreat. They also probably furnished a refuge amongst their ramifications on smaller mountains which even in the Glacial period would be without snow in the summer, to some of the ancient Alpine and sub-alpine plants which were driven southwards as the glaciers increased.

We have therefore the curious fact that some of the most ancient fragments of the Alpine flora are now only to be found on the *southern* slopes of the Alps. This is the case with species of *Primula*, *Pedicularis*, and *Oxytropis*, which exist neither in the interior of Switzerland nor in the north of Europe. But it is easy to see that, like the other members of this flora, they were driven south during the Glacial period, returning as the mountains reappeared from underneath their snowy covering; while on the northern side they were in great measure exterminated. De Candolle points out as a fact in further confirmation that the Alpine species of *Campanula*, peculiar to Mont Cenis and the Simplon and neighbouring valleys, are not related to the Arctic species, but find their nearest allies in Greece, Asia Minor, and the Himalaya.

The Valais was freed from glaciers while the Mont Blanc district and the interior of Switzerland was still in the condition of Greenland. It was gradually stocked by means of species which arrived from France. The first plants to arrive must have been those which are found at the present time on the Jura and the mountains between Geneva and Chamouni. Established at first in the lower part of the valley, they would ascend as the snow diminished. The remarkable plants of the Grande Chartreuse and of Mounts Vergy and Brezon in Savoy, of the higher parts of the Western Jura, and even of the neighbourhood of Bex in Switzerland, probably belong to this period. When the perpetual snow and glaciers had disappeared from these mountains, the neighbourhood of the Lake of Geneva, the base of the Jura, and even the commencement of the Valais were more favourably circumstanced. Plants of still more southern origin could then arrive from France. This is probably the date at which the box and many *Cistineæ* and *Labiatæ*, characteristic of dry southern districts, established themselves at the foot of the Jura. Seeds carried from Italy by winds or birds introduced some of the rare

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[1] Sur les Causes de l'Inégale Distribution des Plantes rares dans la Chaîne des Alpes, par Alphonse De Candolle. (Florence, 1875

species into the Lower Valais, while others of later origin were principally introduced by human agency.

During these changes the Mont Blanc district and the country between the Alps and the Jura were still ice-bound, and seeds carried by the wind from the south and west would fall on snow or sterile moraines. And when in their turn these districts were released, their opportunity of being stocked by the flora fast disappearing from the lower levels had gone. The asylums which were earliest opened were most richly supplied and have remained so.

M. De Candolle considers that a potent cause of the extermination of this flora has been the destruction of the forests which has rendered the climate south of the Alps hotter and drier in summer, and colder in winter.

The rare plants of the Italian Alps are the remains therefore of an ancient flora like that of St. Helena on its last legs. The climate of Europe tends to become drier, and M. De Candolle thinks it probable that in the course of centuries the centre of Switzerland may in turn become relatively rich in rare species, while the southern slopes of the Alps become poor. In the Lebanon and the Pyrenees this reversal of conditions has actually taken place, and their southern face—once rich probably in species re-migrating northwards—is now actually poorer than the northern. The Caucasus and the Himalaya are, however, at present comparable with the Alps. T. D.

DEEP-SEA TELEGRAPH CABLES: HOW THEY ARE TESTED

THE "testing" of a telegraph cable, whether long or short, proceeds upon the principle that the materials offer to the electrical current a certain resistance: the testing of a cable is the measurement of this resistance. In any cable there are two kinds of these resistance measurements; one of the resistance which opposes the current in its progress along the conducting wire, the other of that which opposes its lateral dispersion. The conductor-resistance is technically termed the copper-resistance, and is extremely small compared with the other resistance. The lateral resistance to the escape of the current opposed by the insulating substance which surrounds the copper-conductor is technically termed the insulation-resistance. Where the resistance to the direct propagation of the electric current through a conducting wire is represented in units, the resistance to lateral dispersion through the insulator will be represented by hundreds, or even thousands of millions, of these units. A third property is that known as the electro-static, or inductive capacity, or simply "charge"[1] of the cable; in other words, that measured quantity of electricity which the given cable will take up in a given time. So much for the necessary explanation of technical terms.

The copper-resistance (1), the insulation-resistance (2), and the "capacity" (3) are the three points to be ascertained in the testing of a cable; and it is useful to inquire why these are the points to be ascertained.

The chief commercial requisite in any cable, and upon which depends its value to its owners, is the speed with which signals can be transmitted. Speed depends directly upon two of the foregoing points (that is upon the copper resistance and "capacity"), and indirectly upon the insulation-resistance. Popular assumption is very much given to the idea that the electrical worth of a cable increases with its insulation-resistance; as usual with popular notions this is only half-truth. That the cost of a cable follows the ratio may or may not be, but it is certain that above a definite limit the thickness of the insulating coating has no effect upon the practical working condition of the cable. It may be that minor indirect benefits arise, but with these, under the present consideration of the practical testing of a cable, we have nothing to do. A certain standard of insulation-resistance attained, there remain the two points, first, of the resistance offered by the copper wire; secondly, of the charge. Now it is collaterally to be understood that, as there can be obtained through a pipe a greater flow of liquid when the pipe offers little resistance to the flow, so through the conductor of a cable can a greater flow be obtained when the conductor has low resistance. With most of the Atlantic cables each nautical mile of the conductor has a resistance equal to that of three to four of the arbitrary units selected by the profession for comparison. There are in use two units of electrical resistance, namely, that determined by a committee of the British Association and the Siemens unit. These units are very nearly of the same value, one Siemens' mercury unit (the resistance offered by a column of pure mercury of one metre length and one square millimetre section at 0° C.) being equal to 0·9536 of an Ohm, the technical term for a British Association unit. There is, then, to be considered an electrical length as well as an absolute (or ordinary) length; the proportion that one bears to the other being known, the measures are convertible. Vague as may appear to the reader this idea of electrical resistance, when he knows that of a copper wire of given diameter or weight two lengths offer twice the resistance of one, he is as learned as the most skilled electrician who virtually knows no more.

The consideration of the electrical capacity of a cable is more difficult. While the two other points relate to mass, the question of capacity involves that of surface, and of a property of the insulating material of the cable known as its "specific inductive capacity." The material with which long telegraph cables are insulated is gutta-percha. Two different cables may be insulated with this material to precisely the same dimensions, both as regards the thickness of the insulator and the thickness of the copper wire, but the "charge" taken by these cables may be very different, and the difference will be due to difference in the specific facilities offered by the two gutta-perchas to induction. This difference between various kinds of gutta-percha is as inherent as is the difference between resistances to conduction offered by different metallic alloys, and is probably very often due to want of homogeneity of the substance. It is by judicious selection and careful manipulation that the cable manufacturer is enabled to maintain a certain standard for any particular cable in question. Capacity, however, not only varies with the insulating material, but it also varies with the amount of surface of the conductor. It is different with different thicknesses of insulating material, but in this respect, after a certain limit has been passed, the decrease in capacity is very small for very large increase in the thickness of the insulating material.

High charge is incompatible with high speed. That cable will, other conditions the same, have the greatest speed in which the charge, or the fraction of the charge to be altered at each signal, is least. Professional necessity has given rise to a unit of quantity of electricity termed a "farad," of which the "micro-farad" is the millionth part. The capacity of a telegraph-cable generally ranges from three to four-tenths of a micro-farad per nautical mile.

The object of testing a cable is, then, to ascertain whether the insulation reaches the amount specified, and whether the conductor-resistance and the charge are of the required minimum. As these tests are each applied separately to the cable, their consideration will fall under the several heads. It would clearly be impossible within the limits of this paper to describe the many methods which have from time to time been proposed and in use for the testing of telegraph cables. The first methods of testing submarine lines are undoubtedly due to Dr. Werner

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[1] "Capacity" and "charge" are not equivalent terms, although they are so considered in this article to prevent confusion, by the general reader, with the ordinary meaning of the word "capacity." The capacity of a cable remains constant, while the charge varies with the battery power employed.

Siemens and Dr. C. William Siemens, who early in the history of submarine telegraphy communicated their researches on the subject to the British Association at the Oxford meeting of 1860. The principle of these early methods still remains the principle of the methods employed by Sir W. Thomson in his testing of the Direct United States Cable at Ballinskelligs Bay Station in September, 1875, and upon which he has reported to the manufacturers of the cable, Messrs. Siemens Brothers. It is the purport of this paper to describe these tests and the results obtained.

To those who may be unacquainted with the route of the Direct United States cable, it will be necessary to explain that the course taken is from Ballinskelligs Bay, on the west coast of Ireland, to Torbay, in Nova Scotia, whence it again passes to Rye Beach, in New Hampshire, America.

The construction of the cable, which was decided upon by the company acting under the advice of Dr. William Siemens, their scientific consultant, is as follows:—The cable from Ireland to Nova Scotia consists of a conductor formed of a strand of twelve copper wires weighing 400 lbs. per nautical mile. This conductor is surrounded with four coatings of gutta-percha and gutta-percha-compound weighing 360 lbs. per nautical mile, so that the total weight of the "core," as it is technically termed, is 760 lbs. per knot. It was specified that the core should have an insulation resistance per nautical mile equal to 160 millions of mercury units; tests, however, checked and taken under the direction of Mr. von Chauvin, the manager and electrician to the Company, show that no length of core was passed that did not insulate to nearly double this extent, or to 300 million units per knot, the tests being taken after twenty-four hours' immersion of the core in water at 75° F. The "core" is "served" or enveloped in jute yarn, and is then sheathed or covered with iron wires of a diameter best suited to the position of the cable. Thus for the deep-sea, 1,630 knots of the cable are sheathed with ten strands of wire and hemp, each strand consisting of a homogeneous iron wire surrounded with five strands of Manilla hemp, each strand being passed through a compound of pitch, tar, and india-rubber. Each of the iron wires has an average breaking strain of 53 tons per square inch, and is of 0·099 inch diameter. The cable termed medium cable is sheathed with fifteen wires of 0·148 inch diameter with proper sewings of yarn, while for the shore ends, where there is considered to be more friction or wear, this medium cable is again surrounded with iron sheathings of twelve strands of iron wires, each strand consisting of three iron wires of 0·230 of an inch diameter.

The cable from Nova Scotia to New Hampshire consists of a strand conductor of seven copper wires weighing 107 lbs. per knot, covered with three coatings of gutta-percha and compound weighing 150 lbs. per knot, and is also sheathed with iron wires.

The non-electrical reader who may choose to wade through detail that must be somewhat technical will perhaps find help in considering the conducting wire as representing a line of flow or force, such that if two of these lines be directed into a galvanometer or current-measurer in opposite directions, that having the greatest head or greatest force will preponderate, while no indication will be found on the instrument when the forces are equal; also that from a known force giving through a known resistance a certain instrumental measure, any unknown resistance may be reduced when its instrumental measure is ascertained.

Testing the Resistance of the Copper Conductor.—Electrical measurements upon a long submerged cable differ from measurements made in the laboratory as described in text-books in one very important particular—that of earth-currents. Earth-currents are the *bête-noir* of the electrician, who not infrequently finds them so far masters of the field that his chance of obtaining accurate measures is a poor one. Fortunately, earth-currents do not have so much influence upon the working of a cable as they have upon the testing, and more fortunately still these currents do not always exist, so it is possible to obtain measures during a tranquil period. On the Direct United States' Cable, Sir William Thomson found these currents to be equal in value at a period of greatest strength to that from about eighteen cells of the testing-battery—the Irish end being positive generally to the Nova Scotian end. Under such conditions, Sir W. Thomson employed the simple deflection-method of measuring the conductor-resistance, which he takes to be "the only proper method for measuring copper-resistance in a submerged cable." In the following description of the method and its results, it will be seen that the method consists in applying together with a measuring instrument an electric force which yields a certain measure through the unknown resistance of the cable; a known resistance (7,300 units) is then substituted for the resistance of the cable, and the latter determined by proportion. The principle of this method is applicable not only to the measurement of the copper-resistance, but is that also of the ordinary method of measuring insulation-resistance, a higher known resistance being used in order more readily to effect comparison with the unknown and much greater insulation-resistance. The actual operation during the period of testing is thus described:—

"The insulation-galvanometer quickened three- or four-fold by a magnetic adjustment, and, with a shunt of twenty Siemens' units on its coil, was put in circuit between line, battery, and earth, and the deflection was observed and recorded every ten seconds. As was to be expected, large and rapid variations of the deflections were continually taking place on account of earth-currents. The direction of the earth-current was from east to west the whole time, as was shown by the 'copper' current being always greater, and the 'zinc' current less, than the true mean concluded from the observations. It increased gradually (but with some slight backward pulsation) from the beginning—when its amount was that due to a difference of potentials between the Ballinskelligs and Torbay earths equal to 1·7 of a cell—till the end, when it was more than five times as strong, and corresponded to nine cells; the Irish earth positive relatively to the Nova Scotian earth the whole time. To measure the copper-resistance a time of comparative tranquillity was chosen, a reading taken, and then as quickly as possible the galvanometer short-circuited, the battery reversed, the galvanometer circuit reopened, and a fresh reading taken. Half the space travelled by the spot of light from the first reading to the second is taken, as being the deflection which would be produced by the battery applied in either direction were there no earth-currents. This was done seven times, and the half ranges were as follows:—235, 231, 229½, 234½, 231, 235, 230—mean 232·3. I found that the same battery applied in the two directions through the galvanometer, and 7,300 Siemens' units gave 232 divisions on one side of zero, and 233 on the other—mean 232·5. Hence the copper-resistance to be inferred from the observations is—

$$7300 \times \frac{232\cdot5}{232\cdot3}, \text{ or 7306 Siemens' units."}$$

As the cable in question is 2,420 nautical miles in length, we have $\frac{7306}{2420} = 3\cdot02$ Siemens' units per knot.

Insulation Test.—The ordinary method of testing the insulation-resistance of a cable consists, as has been said, in obtaining upon the galvanometer or measuring instrument a certain measure with a known resistance, and a measure with the unknown resistance, the electric force being constant during the two measurements. From these two measures the unknown resistance is determined.

If, for instance, it is known that with a certain battery power and a resistance of 100,000 units we have a deflection-measure of 100, it is deduced, when with an unknown resistance and the same battery power the deflection of 50 is obtained, that the resistance must be twice as great (namely, 200,000), since the observed effect is halved. This system is that generally pursued, but, like the other measurements upon submerged cables, comes under the effect of earth-currents; and to meet this contingency Sir William Thomson has arranged a new method, bearing upon the principle that the insulation of a cable may be determined from the proportion of loss (during a given time) of electric power that has been imparted to it. In the following description it will be seen that this loss is measured by the deflection due to the current entering the cable to make up the loss, and this deflection is compared with another deflection obtained by altering suddenly by a small quantity the battery power employed. The latter deflection being a measure of a known force or potential, the other measure for lost potential is determined, and consequently the loss of potential known.

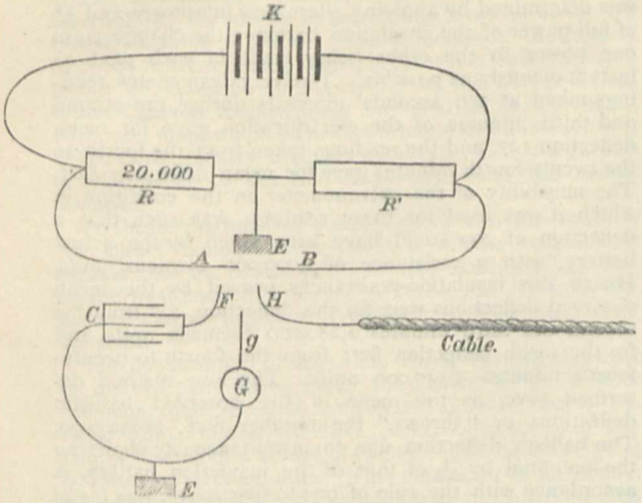
"The cable being offered to me again from midnight till 2 A.M. on the 17th, I made," says Sir W. Thomson, "another series of tests at that time for the main object of measuring the insulation-resistance. I found the line in a much less disturbed state, and was able to make a perfectly satisfactory insulation test by the ordinary galvanometer method. I applied, however, also a new method which (no electrometer being available) I had planned to meet the contingency of the line being disturbed by earth-currents so much as to render the ordinary test unsatisfactory, but not so much as to vitiate an electrometer-test. This method, which I think may be found generally useful for testing submerged cables when an electrometer is not available, is as follows:—1. Apply the ordinary test by battery and galvanometer for a certain time. 2. Insulate the cable for a certain time and then shunt the galvanometer to prepare for No. 3 (unless you have conveniently available a second galvanometer suitable for discharges). 3. Instantaneously reapply the battery, through the insulation galvanometer properly shunted (or a special discharge galvanometer), to the cable, and observe the maximum of the sudden deflection produced. 4. Go on repeating Nos. 1, 2, and 3 as long as you think proper, according to circumstances. 5. To determine the proper ballistic constant of the galvanometer for utilising the observed result of No. 3, find the maximum of the sudden deflection which takes place when a sudden change of electrification is produced by instantaneously changing by a small measured difference the potential of one electrode of the galvanometer, the other electrode being in connection with the cable. 6. The change of potential which, in the operation of No. 5, would give the same deflection as that observed in No. 3, is equal to the change of potential which the conductor of the cable has experienced during the time when it was left insulated according to No. 2. Hence calculate the insulation-resistance in ohms or megohms as in the ordinary electrometer method when the electrostatic capacity of the cable is known."

In carrying out this test, the 20-cell insulation battery (with its poles joined through 20,000 Siemens' units) was applied, zinc to cable, through the insulation galvanometer with a shunt of 5,000 Siemens' units on it. Then, the galvanometer indication was read and recorded every ten seconds for three and a half minutes, when the cable was insulated during a minute according to No. 2 of the directions above, and a shunt of 30 substituted for the 5,000. At the end of the minute the battery was instantaneously reapplied, the throw of the galvanometer observed according to No. 3 and the shunt of 30 removed, and 5,000 reapplied. The cable was again insulated for a minute, the galvanometer shunted with 50 (instead of 30 used the first time), and the operation of No. 3 repeated. The proper ballistic constant of the galvanometer was determined by applying alternately full power and $\frac{49}{50}$ of full power of the insulation battery; the change from one power to the other being made in each case as instantaneously as possible. Twelve galvanometer readings taken at ten seconds' intervals during the second and third minutes of the electrification gave for mean deflection 127, and the readings taken from the fourth to the twenty-fourth minutes gave for mean deflection 82·1. The sensibility of the galvanometer in the condition in which it was used for these readings was such that a deflection of 290 would have been given by the actual battery, with a resistance of 1,000,000 Siemens' units. Hence the insulation-resistances proved by the mean observed deflections were for the deflection, 127 from the second and third minutes 2,280,000 Siemens units, and for the mean deflection 82·1 from the fourth to twenty-fourth minutes 3,540,000 units. The new method described gave, as the mean of the observed ballistic deflections or "throws," the number 89·8, or say 90. The ballistic deflection due to instantaneously changing the potential by $\frac{1}{40}$ of that of the insulation battery, in accordance with the rule of one to five above, was found to be 112 divisions. This is $1\frac{1}{4}$ time the preceding mean throw (90), which therefore showed a change of potential equal to $\frac{1}{50}$ of that of the battery. Hence the mean fall of potential was $\frac{1}{40}$ during the minute, or at the rate of $\frac{1}{3000}$ per second. The capacity of the cable (measured in the way presently described) had been found to be 991 microfarads. Hence the insulation-resistance is $\frac{3000}{991}$, or 3·027 megohms, or 3,170,000 Siemens' units, corresponding to the 3,540,000 units given by the ordinary method. With copper to line, a fresh series of tests gave 3,520,000 megohms, or 3,690,000 Siemens' units.

In the reduction of the insulation-resistance of the whole cable to its insulation-resistance per knot, it has to be observed that as the insulation of the cable is inversely as its length, one knot of the cable will give an insulation resistance equal to that of the whole cable multiplied by the number of knots' length in the whole cable.

Measurement of "Capacity."—Just as the chemist has his vessels for measuring out quantities of liquid, so has the electrician his special arrangements for measuring out quantities of electricity; but there the analogy ends, for while the measure of the liquid is direct and visible, the electrician infers his measured quantity generally by the mechanical work effected on the index of the measuring instrument, or by the absence of such work. The apparatus used in practice for measuring quantities of electricity is termed a "condenser." "Condensers" are constructed having any required capacity, and if such a condenser of which the capacity is known is charged from a battery, then discharged through the measuring instrument, and the deflection produced noted, it is only required to charge from the same battery the cable or any other condenser of which the capacity is to be measured, then to note this discharge deflection, and by proportion to deduce the unknown capacity. On short lengths of cable this procedure is actually adopted, but on long lengths it becomes liable to error, chiefly from the fact that as with long lengths some perceptible time is required to discharge the cable, the ballistic throw or sudden deflection produced upon the measuring instrument by the rush of electricity from the cable does not measure all that passes out. It is consequently necessary to devise some method like the following used by Sir W. Thomson, in which the charge from the cable (communicated thereto by a different battery power to that charging the condenser, but the relative powers being known) is neutralised by a charge of opposite electricity from the condenser, and the neutralisation declared by the non-production of movement in the measuring instrument.

The following diagram, which is not, however, taken from the report, will explain the method :—



K, battery of 80 cells, well insulated ; R resistance of 20,000 units ; R', variable resistance ; C, condenser of 80 microfarads' capacity ; G, shunted galvanometer ; E, earth.

The condenser is electrified by bringing F and A into contact, and the cable by making contact between H and B, for sufficiently long time to fully charge the cable. These contacts are then broken, and instantly after contact made between F and H. This contact is maintained for five to ten seconds, when the additional contact with *g* is made. The variable resistance is adjusted till this last contact produces no movement on the measuring instrument.

It was found that when the cable and condenser were charged to opposite potentials in the proportion of 1,615 to 20,000 no throw occurred, whence the deduction that the capacity of the cable was

$$\frac{20000}{1615} \times 80 \text{ microfarads, or}$$

991 microfarads, and the length of the cable being 2,420 knots, this was equal to 0·409 of a microfarad per knot.

In concluding the report upon the electrical conditions of the Direct United States Cable, Sir William Thomson remarks : "I am glad to be able to say that my tests proved the cable to be in perfect condition as to insulation, and showed its electrostatic capacity and copper resistance to be so small as to give it a power of transmitting messages, which, for a transatlantic cable of so great a length, is a very remarkable as well as valuable achievement." This article would be exceeding its purpose if it were to include inquiry into the present position of Atlantic Telegraphy ; but it is a mark of great progress in electrical engineering and cable manufacture that a cable of such length as 2,420 miles can be delivered up to the company working it in a *perfect* electrical condition. This has not been the case in earlier transatlantic attempts ; and some idea may be formed by the general reader of the care required to bring about this end, when it is known that a small hole, smaller in size than the finest pin-hole, in any portion of the two thousand miles length of gutta-percha covering would render the electrical conditions of the cable imperfect.

THE CLIMATIC CHARACTERISTICS OF WINDS AS DEPENDENT ON THEIR ORIGIN[1]

OF the climatic characteristics of winds the most important are, primarily, their temperature, and, secondarily, their moisture. The general occurrence of certain characteristics, especially when more strongly marked, with particular directions of the wind, experience soon forces on our attention, and much labour has been bestowed, particularly by Dove, in grouping the winds simply according to their directions, and calculating the mean atmospheric pressure, temperature, humidity, cloud, and rainfall, for each of the directions. Interesting and to some extent valuable results have been obtained from these inquiries ; results, it must, however, be added, far from being commensurate with the enormous labour expended in arriving at them. But in extending this line of research into such regions as Western Norway, Farö, Iceland, Newfoundland, and the Azores, its unsatisfactoriness soon becomes evident ; and the further consideration that the climatic qualities of a particular wind repeatedly differ widely from its general character, makes it evident that a climatic inquiry which groups the winds merely according to their direction does not proceed from a scientific basis.

A striking case, showing a great deviation from the general qualities of a wind, occurred during the great Edinburgh hurricane of the 24th of January, 1868, on which occasion the wind remained persistently in the south for several hours, and possessed a coldness and a dryness which were truly polar. The qualities of this south wind are readily explained by the limited area of high pressure, which lay immediately to the south-eastward of Scotland at the time, out of which this wind blew. As the barometer continued to fall, the wind ultimately veered to S.W. and W., and the temperature presented the unusual phenomenon of rapidly rising with a change of the wind into westward. The point to be noted here is, that as long as the wind was connected immediately with the circumscribed area of high pressure it was cold and dry, but when it was involved in the area of low pressure it became mild and moist. This relation between the climatic qualities of a wind and the state of the pressure is a vital point in atmospheric physics, and to Dr. Köppen belongs the merit of applying the principle in inaugurating a new method of inquiring into the climatic characteristics of the different winds by referring each wind-observation to the system of atmospheric pressure with which it is at the moment immediately connected.

If we examine weather-charts representing a large portion of the earth's surface, such as those published in the *Journal of the Scottish Meteorological Society*, vol. ii. p. 198, two distinct systems of pressure are seen, which change their position and form from day to day, one indicated by isobars inclosing spaces of low pressure, into which the winds all round blow vorticosely in the northern hemisphere in a direction contrary to that of the hands of a watch, and the other by isobars inclosing areas of high pressure, out of which the winds blow on all sides, but in opposite directions to those assumed in blowing inwards upon a space of low pressure. The former are usually called cyclones, and the latter anticyclones. Not only do the direction of the winds within the areas of cyclones and anticyclones respectively differ from each other, but the temperature and humidity of the winds connected with each have certain well-marked characteristics. A south-east wind at St. Petersburg, for instance, blowing in immediate connection with a cyclone, comes from the south and south-west, that is, from the south-west of Russia or from Germany ; whereas a south-east wind in immediate connection with an anticyclone comes from the east, that is, either from the east of Russia or from the White Sea, and consequently these two south-east winds are markedly different in their climatic qualities from each other.

Dr. Köppen has compared the temperature, humidity, and other weather conditions at St. Petersburg each day for 1872 and 1873 with daily weather-charts constructed for the whole of Europe, and separated each of the eight winds (N., N.E., E., &c.) and calms into the following

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[1] Ueber die Abhänigkeit des Klimatischen Charakters der Winde von ihrem Ursprunge. Von Dr. W. Köppen. (St. Petersburg, 1874.)

four classes:—1. Those which occur when the isobar, passing through St. Petersburg, bounds a space of low pressure, or when St. Petersburg is included within the area of a cyclone; 2. When the isobar bounds the space of high pressure, or is within the area of an anticyclone; 3. When it is in the calm centre of an anticyclone; and 4. When the isobar does not, at least on the map of Europe, inclose a space, but stretches away in a line which is either straight or irregularly waved. This division is carried out as regards the two great divisions of the year, viz., the cold half, extending from October to March, and the warm half from April to September.

The following will indicate the importance of the results arrived at:—1. *During the cold half of the year*, northerly winds (N.E. and N.) when connected with a cyclone have the pressure 0·370 inch below the average, the temperature 2°·3 above the average, the relative humidity 90, and the sky all but completely covered with cloud; with an anticyclone, pressure is 0·271 inch above the average, temperature 8°·6 below the average, humidity 84, and sky only three-fourths covered; and with a straight indeterminate isobar, pressure is 0·201 inch above the average, temperature 5°·8 below the average, humidity 89, and sky less than three-fourths covered. 2. *During the warm half of the year*, northerly winds connected with a cyclone have pressure 0·192 inch, and temperature 5°·4 below the average, humidity 87, and cloud 8; with an anticylone pressure is 0·206 inch, and temperature 0°·5 above the average, humidity 75, and cloud 4; and with straight indeterminate isobars, pressure is 0·104 inch above, and temperature 3°·4 below the average, humidity 76, and cloud 5.

As regards the S.E. wind, with a cyclone the temperature is 7°·2 above the average in winter, but only 1°·8 in summer; and with an anticyclone, 4°·0 below the average in winter, but 2°·5 above it in summer. Again, with straight isobars, S.E., S., and S.W. winds have in winter a temperature 7°·7 above the average, humidity 93, and cloud 9; but in summer the figures are 5°·2, 82, and 6 respectively. One of the most suggestive results is that obtained from the examination of the anticyclone in summer, particularly as regards its calm central space. In the periphery of the anticyclone where winds prevail, the cloud accompanying the different winds varies from 3 with S.W. to 5 with E. winds, but in the calm central space the amount is only 2; in other words, the space covered by the anticyclone is remarkable for the clearness of its sky, and the central portion is the clearest. Owing to the strong insolation which takes place under these circumstances, the temperature of the whole space covered by the anticyclone is raised 2°·1 above the average; with northerly winds (N.E., N., and N.W.) the excess is, as might be expected, small, being only 0°·5, but with S. and S.W. winds the average excess is 4°·1. The excess in the calm centre is only 1°·3, which is *smaller* than the excess which accompanies winds from the E., S.E., S., S.W., and W. points of the compass.

In a review of the weather of Europe during 1868,[*] Mr. Buchan drew attention to the anticyclone which overspread a considerable portion of Europe from the 2nd to 4th August, as the immediate cause of the hot weather experienced in Great Britain at the time, and which he regarded as the simple result of the widespread high pressure, the comparatively calm atmosphere, the clear sky, the dry air, and the strong insolation which accompanied these conditions. At the same time, to the west, north, and south-east, pressures were low, the sky clouded, and much rain fell. Since the wind blew out from the anticyclone in all directions,—E. and S.E. winds in Great Britain and France, W. in Austria, and S.W. in Sweden and W. in Russia,—without diminishing the high pressure of the anticyclone, it was suggested that the high pressure was maintained by air-currents ascending from the regions of low pressure to the west, north, and south-east, and thence flowing as upper currents towards and then down upon the region of the anticyclone. In connection with this point Clement Ley has made some valuable observations on the upper currents of the atmosphere, showing that they flow outwards from the centre of the cyclone, and inwards towards the anticyclone. If this view be correct, the centre of the anticyclone must necessarily be filled with a slowly descending current.

Now, it will be observed from Dr. Köppen's inquiry that the centre of the anticyclone is the clearest, on leaving which and entering the regions in which winds blow, the sky becomes more clouded—a result strictly in accordance with a descending current over the calm region of the centre. Again, in the calm centre the temperature is lower than round the periphery (except where N.E., N., and N.W. winds prevail, bringing air currents from colder regions, and therefore of a lower temperature), a result admitting of explanation only on the supposition of a descending current within the central space, since, were there no descending current, the temperature would be hottest in the centre where the atmosphere is clearest and the air stillest.

We are now, thanks to Dr. Köppen, put in possession of a truly scientific method of discussing wind-observations in their climatic relations, the value of which will be the more apparent when the observations of places in different parts of Europe have been discussed in accordance with it. What is now wanted, as regards the difficult but vital question of the observation of the wind, is that truly comparable anemometers be procurable, and that they be placed in situations and positions so that they may fairly record the direction, velocity, and pressure of the air-currents which pass over the district where they are placed.

SCIENCE IN GERMANY

(*From a German Correspondent*)

M. VON JOLLY, in Munich, has recently constructed an apparatus for gas-determination by absorption. Its arrangement is, in general, similar to that of Frankland's apparatus, but it is distinguished from this, as from all other apparatus hitherto used for gas analysis, by the principle of measurement that is peculiar to it. Measurement is made, not of the changes of volume, which, for example, the air undergoes through absorption of carbonic acid and of oxygen, but of *changes of pressure, the volume remaining the same.* All the air-quantities to be measured are brought to the same volume, through corresponding change of pressure, in a measuring vessel, the contents of which must not be known; while a temperature of 0° is produced by surrounding the measuring vessel with snow or ice. Thus, each time the tension is measured under which the air-quantity to be considered assumes that constant volume, before and after absorption of a portion of the mixture. This process has various advantages over those hitherto in use. The calculation of the gas-volumes at normal pressure and temperature is rendered superfluous; the number of separate observations required is diminished.

M. Jolly's apparatus is represented in the annexed figure. A is the absorption bell-jar; it is of nearly 100 c.c. capacity. It is open below, and stands in a pneumatic trough. B is the cylindrical measuring vessel of equal capacity with the absorption jar. By means of a ⊤-shaped perforated glass cock *h*, the vessels A and B can be connected; they can also be put in communication with the atmosphere by the capillary tube *g*. B is connected by means of a capillary tube, with a wide glass tube E. At *f* there is, within the curve, where the capillary tube opens, a small tongue of dark glass, 1 to 2 mm. long, with

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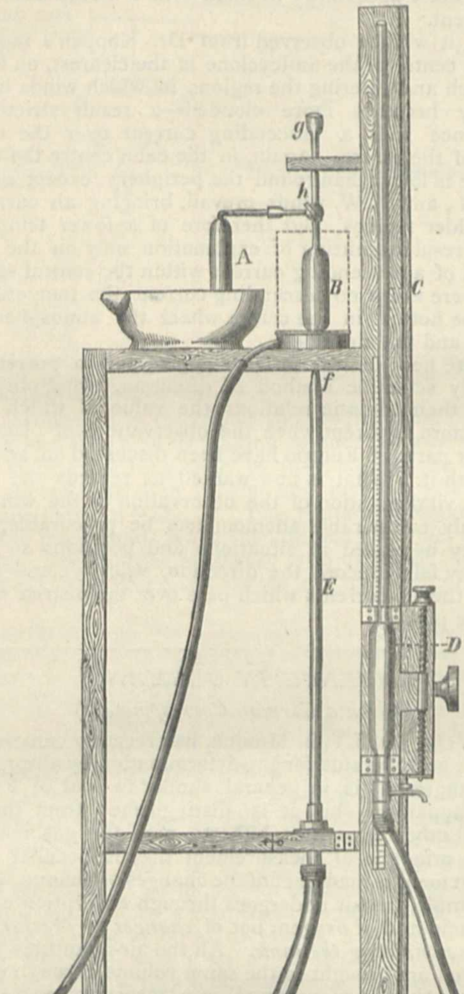
[*] Atlas Météorologique de l'Observatoire Impérial, Année 1868, D. 39, Paris, 1869.

the point directed downwards. (This point serves the purpose of fixing with certainty the surface of the mercury contained in E, for the point is reflected in the surface of the meniscus.) E is connected by means of caoutchouc tubing with a wide vessel D, which is fixed to a moveable carrier, and can be easily moved up and down. The tube C, which forms the vertical prolongation of the vessel D upwards, moves before a scale with millimetre divisions.



When the apparatus is to be used, a portion of D and E is first filled with mercury. Then the cock *h* above is opened and D pushed up to the height of the cock; whereupon the rising mercury fills the measuring vessel B, while the vessel D gets emptied. When the rising mercury comes into view in the small funnel *g*, you shut the cock and bring the vessel D down again, whereupon the mercury runs out of the measuring vessel and leaves there an empty space. If B be now connected with A the air is sucked over from the absorption jar into the measuring vessel, while the mercury of the pneumatic trough rises in the absorption jar. This air is also removed from the apparatus in the way above described. The air-specimen to be examined is now brought into the absorption jar, and allowed to pass over into the measuring vessel, which is surrounded with snow or pounded ice.[1] When the gas is cooled, the mercury in E is so placed that the tongue point touches the meniscus. Since the scale numbers begin at the height of the tongue point, by reading off the position of the mercury in D, the height of the mercury column is obtained, which taken together with the tension of the included air-specimen, makes equilibrium with the external air-pressure. On deducting from the immediately observed barometer height the observed column of mercury, the pressure of the enclosed air at 0° is obtained. Next, by suitable position of the vessel D and turning of the cock, the measured air-specimen is allowed to flow over again into the absortion jar. Afterwards, some small pieces of fused pyrogallic acid are introduced with a pincette under the mercury into the absorption jar. When the resulting absorption of the oxygen is finished, you measure, in the same way, in the measuring vessel, the tension of the remaining nitrogen. (The tension of the saturated water vapour at 0° = 4·6 mm. is deducted in each calculation from the tension obtained in the measuring vessel.)

Let V be the volume of the measuring vessel, P the pressure of the air-quantity contained in it, then, according to Mariotte's law—

$$V P = 760 y,$$

if y denotes the volume of this air-quantity at 0° C and 760 mm. Let P' be the pressure of the remaining nitrogen, then—

$$V P' = 760 x,$$

where x denotes the volume of this quantity of nitrogen at 0° C and 760 mm. Consequently—

$$x = y \frac{P'}{P} = 100 \frac{P'}{P}$$

if $y = 100$.

When it is wished to analyse breathed air with this apparatus, the carbonic acid is first removed with potash lye, and the oxygen with pyrogallic acid. The oxygen may also be removed with a piece of phosphorus, by inflaming it with an induction-spark in the absorption jar.

It is known that the experiments which have been made by Maxwell, O. E. Meyer, Obermeyer, and Puluj, on the dependence of friction of gases on temperature, have not led to concordant results. M. Puluj has therefore lately made a large number of experiments with air, hydrogen, and carbonic acid, in order definitely to determine the relation between friction of gases and the temperature. This investigation was carried out in the laboratory of Prof. Kundt, in Strassburg. The apparatus used for the purpose was the friction apparatus of Kundt and Warburg, which is essentially not very different from Maxwell's, and consists of a glass disc oscillating between two fixed discs.

According to the dynamical theory of gases, the constant of friction should be proportional to the square root of the absolute temperature, *i.e.*—

$$n = n_0 (1 + a \theta)^{\frac{1}{2}}$$

where n_0 denotes the constant of friction of the gas at temperature $\theta = 0°$ C., and a the coefficient of expansion of the gas. If we make, generally,

$$n = n_0 (1 + a \theta)^n$$

we have from the experiments of Puluj—

For air, $n = 0·72196 \pm ·01825$ between $-3°$ C. and $+25°·6$ C.
For H, $n = 0·69312 \pm 0·01088$ „ $-1°·5$ C. and $+30°$ C.
For CO_2, $n = 0·91654 \pm 0·01394$ „ $+1°$ C. and $+29°$ C.

The exponent for air is smaller than Meyer's ($\frac{3}{4}$),

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[1] To apply the ice a vertical sheet-iron cylinder is used, separable into halves; it surrounds the measuring vessel at an interval of about 3 cm. In the figure the outline of the cylinder is shown by dotted lines.

and larger than that formerly obtained by Puluj from transpiration-experiments with capillary tubes ($\frac{2}{3}$). The exponent for hydrogen is somewhat smaller than that for air. Not a little interesting is the remarkably large exponent for carbonic acid, in which the friction appears nearly to follow the law of temperature, to which Maxwell's new theory of gases leads (which, as is known, proceeds from the supposition of a repulsive action at a distance, inversely proportional to the fifth power of the distance of the molecules).

From these experiments it clearly appears that *the friction does not, in all gases, vary with the temperature in the same way*. The theory of gases must still undergo modification, in order to afford us a satisfactory explanation of this molecular process. S. W.

THE EARLY HISTORY OF MAGNETISM

THE earliest references to the properties of the magnet occur in the annals of the Chinese nation, who used it as a means of guiding the wayfarer over the vast and trackless plains of Eastern Asia, long before it was applied to maritime purposes. To the Emperor Hoang-Ti, who lived 2,000 years before our era, is attributed the invention of a chariot, upon which stood an elevated figure pointing to the south, independently of any position of the chariot. Nearly ten centuries later, we find the learned Tchéou-Koung presenting and teaching the use of the tchi-nân-kiu, or chariot indicating the south, to some envoys from Youé-tchâng, a southern maritime province. The compass, or, as it is even now called in Chinese, *tchi-nân*, appears to have been first used at sea by this remarkable nation about the third century of our era, during the Tsin dynasty.

When the compass became known in Europe is disputed; Gilbert refers its introduction to Marco Polo about 1260,* but it is probable that earlier accounts of it were brought from the East by the Crusaders, an accurate description of it occurring in a poem entitled "La Bible," written by the minstrel Guiot de Provence about the year 1190. A Latin letter ascribed to Peter Adsiger, 1269, preserved among the manuscripts of the university of Leyden, contains the following remark on the declination of the needle:—"Take notice that the magnet, as well as the needle that has been touched by it, does not point exactly to the poles, but that part of it which is reckoned to point to the south declines a little to the west; and that part which looks towards the north inclines as much to the east. The exact quantity of this declination I have found, after numerous experiments, to be five degrees."

The discovery of the dip of the needle is due to Robert Norman, a nautical instrument maker at Wapping, near London, who is described by Gilbert as "a skilful sailor and ingenious artificer." He found that after being touched by a magnet the needle always appeared heavier at its northern end, and making an instrument to determine the greatest angle formed with the horizon, he observed the inclination in 1576 to be 71° 50′.

In the early part of the following century, the variation of the declination was clearly ascertained, and was attributed by Bond, a teacher of navigation in London, to the motion of two magnetic poles.

In the year 1600 was published the celebrated treatise "De Magnete," by Gilbert of Colchester, who was pronounced by his great contemporary Galileo, to be "great to a degree that might be envied." Gilbert regarded our globe as a great magnet, and its centre as the centre of the magnetic motions of the earth. Variation he defines to be the arc intersected between the point where the meridian of the place cuts the horizon, and that point to which the magnetic needle looks; the length of this arc varying with the place of observation. He states that from the coast of Guinea to the Canary Islands, and thence throughout Spain, Gaul, England, Germany, and Norway, the magnetic needle turns towards the east, that on the opposite shores of North America it turns to the west, whilst near to the Azores it points exactly north and south; nor does he fail to observe that from the north of Brazil, along the coast of South America to the Straits of Magellan, the southern end of the needle points west of the true meridian. He rejects the vulgar opinions of variation depending upon magnetic mountains or magnetic rocks, upon the poles of the zodiac, or the positions of certain fixed stars, but ascribes it in some measure to the configuration of sea and land on the surface of the earth; chiefly, however, to irregularities in what he terms the magnetic globe and true earth, which he conceives to be more considerable under the continents than below the depths of the ocean. He devotes the fifth book of his work to a full account of the dip of the needle, termed by him declination, with a minute description of the instruments used in its measurement.

Descartes attributes variation to the irregularities of the earth's surface, considering magnetic attraction strongest wherever iron and loadstone are most abundant. To account for the variation of the compass, he asserts that the amount of iron in certain localities constantly changes, partly because man draws it from one place to transport it to another, and partly because new iron is formed in some districts where there was none before, whilst in others old iron becomes corrupted and disappears entirely. To explain his theory of magnetism it is necessary to state briefly the hypothesis he formed respecting matter in general, an hypothesis for which he does not claim absolute truth, but one from which deductions may be made in conformity with experience. The universe he supposed was formed originally of one uniform material, divided into equal parts having equal movements. These movements he considered to be twofold, each part revolving on its own axis, and several together revolving round fixed centres, and thus forming distinct vortices. As he deemed no void possible, it followed that these parts being equal, could not at first have been round, but might eventually become so, their angles as they met together being rubbed off and the intervening spaces filled with the dust or *débris*.

Descartes considered these two forms of matter as two elements of the universe, the first consisting of the *débris* and the second of the little spheres. The less agitated parts of the first move chiefly in straight lines from the poles to the centre of each vortex, and in passing through the triangular spaces often left between contiguous balls of the second element, they assume the form of fluted, spiral columns. On the disposition of their channels the force of the magnet principally depends. His third element is formed by the union of the less subtile matter of the first, including the fluted columns. From the centrifugal force of the round parts a central space is left within each vortex, composed purely of matter of the first element; this Descartes supposed to form an extremely subtile body, such as he conceived the fixed stars to be, and even considered that the earth formerly occupied such a centre till the less subtile matter collecting on its surface changed into that of the third element, and thus formed clouds and other obscure bodies. As each new layer was added, the force of the containing vortex diminished, and more matter escaped into the surrounding vortices than returned to occupy its place; finally, the earth, enveloped in its atmosphere, descended to the position it now occupies in the powerful vortex around the sun. He divides it into three regions, the lowest consisting entirely of matter of the first element; the middle, of an opaque solid body containing passages sufficiently large to admit the fluted columns of the first, but not the spheres of the second element,

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* "Scientia Neuticæ pyxidulæ traducta videtur in Italiam per Paulum Venetum, qui circa annum M.CCLX. apud Chinas artem pyxidis didicet. De Magnete, p. 4.

whilst the upper is formed of a confused mass of matter which belongs chiefly to the third element, but is interspersed with the round balls of the second. The passages in the intermediate region he conceives to be so grooved that the fluted columns entering from one side cannot return again by the same passages, but when opposed in their straight course are forced back through the air or upper portions of the earth to those openings by which they entered, whilst those from the other side make similar circuits. He considers that magnets contain passages the same as those first mentioned, and such is the inclination of the fluted columns to enter these passages, that even if the poles are not turned to receive them they will push aside all opposing particles, till, if not restrained by still stronger bodies, the magnets are forced to assume those positions in which their poles point oppositely to those of the earth.* Such is the hypothesis of Descartes, ingenious rather than plausible, and interesting chiefly as exhibiting the speculative mind of its author.

In 1683 the celebrated Halley presented a paper of great importance to the Royal Society of London, entitled "A Theory of the Variation of the Magnetical Compass." In this communication he states that the "deflection of the magnetical needle from the true meridian is of that great concernment in the art of navigation, that the neglect thereof does little less than render useless one of the noblest inventions mankind ever yet attained to," and gives as the result of "many close thoughts" the following explanation of the variation of the compass. "The whole globe of the earth is one great magnet, having four magnetical poles or points of attraction, near each pole of the equator two; and in those parts of the world which lie near adjacent to any one of those magnetical poles, the needle is governed thereby, the nearest pole being always predominant over the more remote." He remarks that the positions of these poles cannot as yet be exactly determined from want of sufficient data, but conjectures that the magnetic pole which principally governs the variations in Europe, Tartary, and the North Sea is about 7° from the north pole of the earth, and in the meridian of the Land's End, whilst the magnetic pole which influences the needle in North America, and in the Atlantic and Pacific Oceans, from the Azores westward to Japan, is 15° from the north pole, and in a meridian passing through the middle of California. The variation in the south of Africa, in Arabia, Persia, India, and from the Cape of Good Hope, over the Indian Ocean to the middle of the South Pacific, is ruled by the most powerful of all these magnetic poles, which is situated 20° from the south pole of the earth, and in a meridian passing through the island of Celebes; in the remainder of the South Pacific Ocean, in South America and the greater part of the South Atlantic Ocean, it is governed by a magnetic pole 16° from the south pole, in a meridian 20° west of the Straits of Magellan. On this hypothesis Halley explains the variation observed in different places, and among others cites the two following instances. On the coast of America, about Virginia, New England, and Newfoundland, the variation was found to be west, being above 20° in Newfoundland, 30° in Hudson Strait, and 57° in Baffin's Bay. On the coast of Brazil, on the contrary, it was found to be east, being 12° at Cape Frio, and increasing to 20½° at the Rio de la Plata, thence decreasing towards the Straits of Magellan. Thus, almost in the same geographical meridian, we find the needle at one place pointing nearly 30° west, at another 20½° east; this is explained by the north end of the needle in Hudson Strait being chiefly attracted by the North American magnetic pole, whilst at the mouth of the Rio de la Plata the south end is attracted by the south magnetic pole, situated west of the Straits of Magellan. Sailing north-west from St. Helena to the equator, the variation is always in the same direction, and slightly east. Here the South American is the chief governing pole, but its power is opposed by the attraction of the North American and Asian south poles; the balance as you recede from the latter being maintained by approach to the former.

Nine years later Halley made another communication to the Royal Soceity, in which he endeavoured to meet two difficulties he had always felt in his former explanation; one, that no magnet he had ever seen or heard of had more than two opposite poles; the other, that these poles were not, at least all of them, fixed in the earth, but slowly changed their positions. The following observations are cited by Halley in proof of the motion of the magnetic system. At London, in 1580, the variation was 11° 15′, east; in 1622 it was 6° east, in 1634 it was 4° 5′ east, and in 1657 there was no variation; whilst in 1672, it was 2° 30′ west; and in 1692, 6° west. At Paris the variation was 8° or 9° east in 1550, 3° east in 1640, 0° in 1666, and 2° 30′ west in 1681. At Cape Comorin it was 14° 20′ west in 1620, 8° 48′ west in 1680, and 7° 30′ west in 1688. Halley considered the external parts of our earth as a shell, separated by a fluid medium from a nucleus or inner globe, which had its centre of gravity fixed and immovable in the common centre of the earth, but which rotated round its axis a little slower than the superficial portions of the earth. The nucleus and exterior shell he regarded as two distinct magnets, having magnetic poles not coincident with the geographical poles of the earth. The change observed in Hudson's Bay being much less than that observed in Europe, Halley concluded that the North American pole was fixed, while the European one was movable; and, from a similar observation on the coast of Java, he considered the Asian south pole as fixed, and the pole west of the Straits of Magellan to be in motion. The fixed poles he regarded as those of the external shell, and the movable those of the inner nucleus. Of these latter, the one placed by him in the meridian of the Land's End was ascertained, in the present century, to have moved to Siberia, in 120° east long., and that placed by him 20° from the Straits of Magellan to have moved between 30° and 40° west of this position; while those poles regarded by Halley as fixed were found but slightly altered in position since his time. It is extremely interesting to find that not only modern observations of declination, but also those of dip and magnetic intensity, have received their best explanation on the assumption of four magnetic poles. Much, however, that is mysterious remains unsolved, and Halley's remarkable words may even now with truth be quoted: "Whether these magnetical poles move altogether with one motion or with several; whether equally or unequally; whether circular or libratory; if circular about what centre, if libratory after what manner, are secrets as yet utterly unknown to mankind, and are reserved for the industry of future ages." K.

THE POTATO DISEASE

IN the *Journal of the Royal Agricultural Society of England*, Second Series, vol. xii., Part I., No. xxiii., 1876, Prof. A. De Bary of the University of Strasburg has published a paper entitled "Researches into the Nature of the Potato Fungus."

De Bary's essay treats of the Peronosporeæ, Artotrogus (in its plain and echinulate forms) and Pythium. These fungi are described by De Bary as four distinct plants, whilst I, in common with several other observers, believe the first three (if indeed not all four) to be mere conditions of one and the same fungus, viz., the *Peronospora infestans* of Dr. Montagne. In replying to De Bary's remarks it will be convenient (especially as the potato-fungus appears to be somewhat imperfectly understood),

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* Descartes designates the south pole of the magnet that which turns to the north pole of the earth, and the north pole of the magnet that which turns to the south pole of the earth.

to refer to each of the above forms separately, and to illustrate each to an uniform scale. Therefore in the present paper I will confine myself to the description of the two forms of Artotrogus, and leave the consideration of Peronospora and Pythium with De Bary's criticisms of my observations for another paper.

I.—ARTOTROGUS, *Mont.*

To make my description of Artotrogus quite plain, it is necessary to briefly recapitulate the early history of the potato-fungus. Mdlle. Libert was one of the first to describe this in 1844. In the same year Mr. Berkeley writes he first saw diseased potatoes.

In 1845 Dr. Montagne described and illustrated the potato fungus. In his illustrations he included certain spherical bodies found in spent potatoes by Dr. Rayer; these bodies were attached to threads, some of the bodies being termimal (Fig. 1, A), and others within the threads (Fig. 1, BC), just as true oogonia are now known to occur in Cystopus and other of the Peronosporeæ. A third form (Fig. 1, D), was more or less echinulate, and this was assumed to be the mature spore. Dr. Montagne, in 1845, did not thoroughly comprehend the meaning of these bodies, so he named them provisionally Artotrogus.

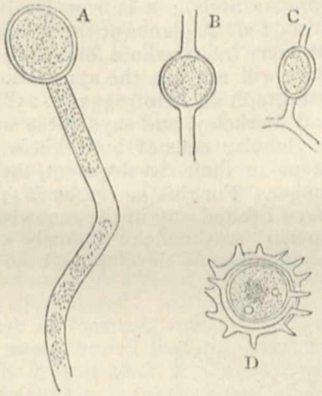


FIG. 1.—*Artotrogus hydnosporus*, Mont. A, B, C. Oogonia from Montagne's original camera-lucida drawing. D. Echinulate oogonium from De Bary ("Researches," p. 256, Fig. 3), × 400 dia.

From 1845 till 1875, when I rediscovered the entire series of these bodies in direct connection with *Peronospora infestans*, in the Chiswick tubers, no record of their rediscovery had ever been published. Botanists had sought for Artotrogus in vain.

In 1846, in the first vol. of the *Journal of the Royal Horticultural Society*, the Rev. M. J. Berkeley published his famous paper on the potato-murrain (the essay is dated Nov. 22, 1845), and in this paper Mr. Berkeley reproduces the description and illustration of Montagne's Artotrogus. Mr. Berkeley's published belief has for many years been that the spherical and echinulate forms of Artotrogus belong to no other than the secondary condition of the potato fungus.

In 1849, Mr. C. Edmund Broome, of Batheaston, discovered a second species of Artotrogus; this was found in decayed turnip. A copy of the original camera-lucida drawing made by Montagne is here reproduced. It shows the mycelial threads and the mature resting-spore at E.

Fourteen years afterwards (1863), Prof. A. De Bary published a paper on the development of parasitic fungi in the *Annales des Sciences Naturelles*, vol. xx. In this paper the author illustrated, amongst other things, the resting-spore of *Peronospora parasitica*, Corda, a plant common upon turnips, &c. Part of De Bary's illustration is here reproduced to show the probability of the second described species of Artotrogus being identical with the Peronospora. Some allowance must of course be made for the sketch of the turnip Artotrogus (Fig. 2), as it was made in 1849 when resting-spores were little understood, but it is clear that Mr. Broome detected not only the resting-spore with its collapsed oogonium, but probably also a group of antheridia (see the detached antheridium in De Bary's illustration, Fig. 3).

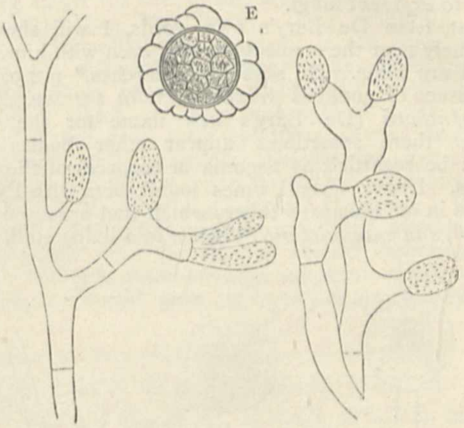


FIG. 2.—Artotrogus on decayed turnip = *Peronospora parasitica*, Corda. × 400 dia.

This probable identity of the Artotrogus and Peronospora of the turnip, points in the direction of the probable correctness of Mr. Berkeley's views as to the Artotrogus of the potato. The oogonia of the turnip parasite are similar in size and form with the oogonia found in potatoes, and to these latter I shall now return.

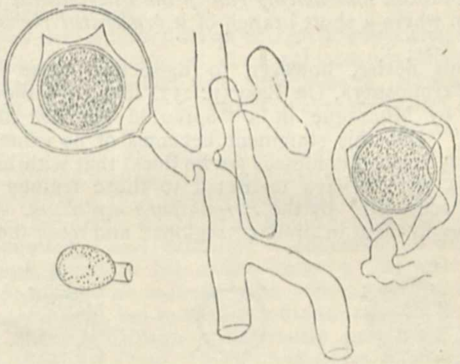


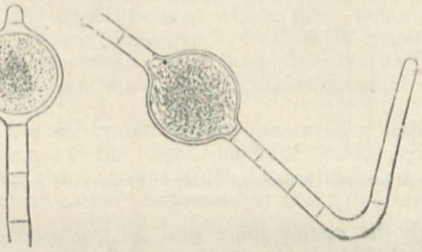
FIG. 3.—*Peronospora parasitica*, Corda. Resting-spores and detached antheridium (De Bary, "Ann. des Sc. Nat.," 4th series, vol. xx., Pl. X., Fig. 5–7), × 400 dia.

The bodies found by me in the Chiswick tubers I have from the first identified with Montagne's Artotrogus, though De Bary in his criticisms has found it convenient to omit all mention of this fact. But De Bary himself now illustrates the same bodies "from Montagne's original specimen;" this illustration is here reproduced (Fig. 4), and I can certify as to its general correctness with the reservations (1) that the oogonia shown are larger than any I have seen in the "original specimens;" (2) larger than any Montagne has figured; and (3) that De Bary has omitted all the terminal oogonia. In Montagne's original camera lucida drawing there are three terminal oogonia, one is reproduced at A (Fig. 1), with two similar bodies intercalated at B C.

De Bary (as well as Montagne, Berkeley, and myself) has also met with the more or less echinulate bodies, and one of De Bary's illustrations is reproduced, at D (Fig. 1). I agree as to its general accuracy.

Now whilst De Bary contends that all these forms are distinct species of fungi, and not belonging to the potato fungus, I maintain with Berkeley, and other competent observers, that they are *one, including Peronospora*. I have seen them all growing from the same threads, and I will now review the grounds on which De Bary refers them to *different* fungi.

First, from De Bary's own words, I will show how extremely near the connection was, even with him. The italics are mine. He says ("Researches," p. 256), "In the tissues of potatoes *penetrated with the mycelium of Phytophthora* (De Bary's new name for the potato fungus) there sometimes appear other bodies which might be regarded as oogonia or oospores of the potato fungus. I have several times found them with Pythium vexans in old collapsed tubers which had sprouted in the ground, *and once without Pythium* in a living stalk which had been on the ground. But they were *always restricted to those regions which were occupied by the Phytophthora mycelium*." And again, "It was certainly remarkable that they were *often* situated *close* to the inner surface of the cell-walls in places where externally the mycelium of Phytophthora *undoubtedly ran in the intercellular spaces*, or even where a short branch of it *penetrated the interior of the cell*."



G. 4.—*Artotrogus hydnosporus*, Mont. De Bary's illustration ("Researches," p. 258, Fig. 8), × 400 dia.

In his desire, however, to dissociate these bodies from Peronospora, De Bary (p. 257) first says they were found by Montagne in a "sprouted but *not diseased potato*." But this statement becomes of no value when De Bary himself confesses (as he does) that with him the oogonia were always restricted to those regions which "were occupied" by the *Peronospora mycelium*. I, too, have found them in similar "regions" and *upon* the *Peronospora* mycelium. De Bary says (p. 256), "In most cases I found these bodies complete, mature, and without any distinct indication of their being attached to mycelium," and he says the same of Montagne's material and afterwards of mine. But this statement (like the former one) becomes of no value when we find the author writing on the very next page that after "long searching in vain he found them to grow on the extremities of the branches of a mycelium which is very like that of *Pythium vexans*." And now what is the mycelium in *P. vexans* like? De Bary tells us, on page 253, that it is "scarcely possible to draw a positive distinction" between it and the mycelium of *Peronospora infestans!* The conclusion is obvious.

FIG. 5.—Oogonia of *Peronospora infestans*, Mont. A, B, C. Different forms from Chiswick potatoes. D. From De Bary's slide, No. IX. (common in Montagne's preparations). E. From De Bary's slide, No. XI. F. From De Bary's slide, No. X., as sent to the Royal Agricultural Society.

It is, perhaps, difficult to explain why a smooth oospore should become a rough or echinulate one, but the fact remains that the phenomenon is perfectly well known in fungi, notably in the spores of some of the Gasteromycetes. Plain and echinulate oospores are also produced on the same plant in some Saprolegnieæ. Max Cornu also maintains that *Saprolegnia asterophora* of De Bary is the same as the warted form of *Achlya racemosa* of Hildebrand. Dictyuchus also occurs with warted oogons. The three upper figures on the accompanying illustration (Fig. 5) are exact reproductions from different forms of oogonia found by me growing on the same threads with *Peronospora infestans*. The lower three are from De Bary's own specimens sent to the Royal Agricultural Society; D is from slide ix. (this is also very common on Montagne's preparations); E is from De Bary's slide xi., and F is from slide x. A glance at the actual preparations will show that every intermediate form is to be found.

But De Bary will not see the association, and in his concluding paragraph on Artotrogus (p. 258), he criticises Montagne and Berkeley, and says these authors "have explained the globular cells of both kinds as exhibiting progressive steps in their development, the smooth ones being the younger. For this *no reason is given*," says De Bary, "nor have I found any in the renewed examination of the specimens. Anyone can scarcely conceive, from the known phenomena of development how the smooth thick walled cells (the cell-walls are exactly the same in both) could become the smaller star-shaped ones."

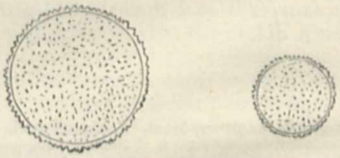


FIG. 6.—Resting-spores of *Peronospora Holostei*, Casparry. × 400 dia. From De Bary's preparation sent to the Royal Agricultural Society.

Now, the accompanying illustrations (especially Fig. 1) show De Bary to be quite wrong. The three upper oogonia on Fig. 1 are from Montagne's own camera-lucida tracing, the left-hand figure, A, is the *largest* oogonium he has shown, and the right hand oogonium, B, is the smallest. I can testify as to their correctness from an examination of the original material. The starry figure at D is one of the (presumedly) mature bodies, and is reproduced from *De Bary's own figure* ("Researches," p. 256, Fig. 3). I reproduce this figure because De Bary calls it the "common form." Now, instead of being smaller, a glance will show this echinulate body to be *larger than the largest plain oogonium found by Montagne*. On turning now to Fig. 5 it will be seen there is no "getting smaller" in the case, for all the bodies are as a rule very uniform in size, and if they vary at all they get somewhat larger instead of smaller at maturity. This is the rule, but all botanists know well that the oogonia of the Peronosporeæ are liable to vary. A good illustration of this is afforded by De Bary's own preparation of *P. Holostei*, as furnished by him to the Royal Agricultural Society. The accompanying illustration (Fig. 6) is a camera-lucida reproduction of two oogonia belonging to this species from his own slide, and it shows well how oogonia may vary in size.

Thus De Bary's notes and criticisms (taken in connection with the observations of other competent observers) on this, *the first point* (Artotrogus), completely fall to the ground. De Bary confesses to having found both forms of Artotrogus in those restricted regions *only* of diseased potatoes where *Peronospora mycelium* was *undoubtedly* and *always* present. He moreover found the bodies attached to a mycelium so like that of Peronospora that it was almost impossible to distinguish it from Peronospora.

As regards the unfortunate criticism of Montagne and Berkeley as to the size of the oospores De Bary effectually refutes himself. WORTHINGTON G. SMITH

SCIENCE IN LEEDS

THE Annual Meeting of donors and subscribers to the Yorkshire College of Science was held in Leeds last Friday. Financially the College seems to be fairly prosperous. The subscriptions promised prior to the inauguration amounted to 28,000*l.*, and of the special fund of 10,000*l.* started by Sir Andrew Fairbairn's conditional offer of a second donation of 1,000*l.*, about 8,000*l.* have been raised. The Council are most anxious that this should be completed without delay. The College will also participate yearly in the proceeds of William Akroyd's Foundation. Of the present condition of the Yorkshire College as regards efficiency, the following communication from Mr. G. T. Bettany will afford a fair idea:—

A recent visit to Leeds enabled me to inquire into the working of the Yorkshire College of Science, which has now been in existence for nearly two years. In the building which has been temporarily adapted to the purposes of the College, I found abundant evidence of labour and study on the part of both professors and students. The attendance of seventy-five day and more than two hundred afternoon and evening students during the present session shows that the advantages offered by the College are becoming widely appreciated. Although youths are admitted at the age of fourteen, most of the students are much older; I was informed that the average age was a year and a half greater than at Owens College. The chemical department has the lion's share of accommodation; the lecture-room is large and good, and the laboratory would allow of forty students working at the same time. There have been few vacant benches during the past term. Prof. Thorpe has a room fitted up as a museum and reference library, and has also a private laboratory. A considerable amount of work is done in the department of mathematics and physics, but physical teaching suffers from want of space. Practical work can only be carried on in Prof. Rücker's private room. Geology has been fairly attended, though the day class is but small at present. Prof. Green is forming most instructive series of rock-specimens, illustrating stratigraphical geology, volcanic phenomena, and transitions in metamorphism. Mr. Miall's biological lectures have resulted in some very good work. He has prepared a large number of dissections for demonstration, including a series illustrating Prof. Rolleston's "Forms of Animal Life;" practical work has been undertaken by several students, including ladies, one of whom gained the highest place in an examination at the end of last term. Finally, the instruction on textile industries, under Mr. Beaumont, has been made scientific in many respects, especially in relation to the theory of colouring.

It can hardly be considered a misfortune that the College has been started in temporary buildings; for by means of its present effort science will become more widely appreciated, and much larger donations will come to hand than those already received; and the construction of the permanent college buildings cannot fail to be advantaged by the experience now being gained by the professors. It is to be hoped that many wealthy Yorkshire manufacturers who have at present given little or nothing to the College will be induced to follow the example of men in other localities, and liberally support a system of teaching which will be of great intellectual and material benefit to Yorkshire. I was struck with the large amount of work undertaken by the professors. When more prosperous times come, it will be for the good of the College not to exact so much work from them as their zeal is now leading them to perform.

The Leeds Philosophical Museum is becoming yet more interesting under the care of Mr. Miall, who has worthily succeeded the late Mr. Denny. The whole of the Museum is gradually being arranged in the most educative manner, and very great progress has been made. The casual visitor cannot fail to be instructed as well as interested, which can hardly be said of many more pretentious museums. Brief and clear printed descriptions or explanations abound, showing the particular interest of a specimen, or giving the general characters of a class of animals or of a geological formation. If an additional skilled curator could be appointed, who should relieve Mr. Miall from the care of several departments, the Leeds Museum would advance still more rapidly than at present, and would soon be worthy of any provincial college.

THE LATE SIR WILLIAM WILDE

SIR WILLIAM ROBERT WILLS WILDE, M.D., &c., was born in Castlerea, county of Roscommon in Ireland, in the year 1814, and he died at his residence in Dublin on the 19th instant. He was educated at the Royal School at Banagher and at the Diocesan School at Elphin; when scarcely eighteen years of age he was bound apprentice, according to the practice of those days, to the well-known surgeon, Abraham Colles, and he acquired his professional knowledge from such men as the Cramptons, Marsh, Wilmot, and Cusack. Early in 1837 he became a Licentiate of the Royal College of Surgeons in Ireland, and shortly after he resolved to devote himself to ophthalmic surgery, in which he attained a position of the highest eminence.

However distinguished as an oculist, however renowned as a writer on statistics, in these columns we lament in his decease the departure from among us of one who, as an earnest, devoted, and painstaking student of the early history of the Irish races, has left in his writings on this subject a great and an enduring monument.

Sir W. Wilde was elected a member of the Royal Irish Academy in June, 1839, having previously read two papers before the Academy, which were published in abstract in their *Proceedings*, and exhibited a collection of ancient spear-heads found in his native country. At this time the Academy had no museum (the Underwood purchase was not arranged), but in the same month that Wilde was elected, Prof. M'Cullagh munificently presented them with the Cross of Cong, "in order that he might contribute to the formation of a national collection, the want of which was regarded by Sir Walter Scott as a disgrace to a country which, like Ireland, so abounded in valuable remains." This noble gift bore speedy fruit, and meeting after meeting witnessed the presentation of donations, many of which were from time to time described by Wilde.

In 1855 Wilde was elected member of the Council of the Academy, and Secretary of Foreign Correspondence in 1857. In 1852 the Academy had moved to the premises that they at present occupy, and the Council took steps to have a catalogue of their museum made. The task was entrusted to Dr. Petrie. The resolution of the Council would seem not to have been carried into effect, and after some years of anxiety the Council and the Academy were but too happy, in March, 1857, to accept Wilde's liberal proposal to arrange and catalogue their museum. The energy that he brought to bear on this task may be judged from the fact that Part I. was ready in the month of August in the same year, when the British Association met for a second time in Dublin. Part II. was published in 1860. Part III., concluding Vol. I., in 1863. Part I. of Vol. II. had been published in the previous year. Part II. of this volume, although in great part ready, was never printed; let us add that the best tribute of respect that the Academy could pay to Wilde's memory would be to complete this work. Few know the hours that were stolen from professional work, from the enjoyment of social life, and from much-needed rest, during the years that were engaged in this work. Despite the criticisms of some who knew little of what they criticised, this catalogue will always remain as a testimony to the author's energy and ability; already has it proclaimed far and wide what a storehouse of treasure exists under the Academy's roof. Sir William Wilde's many good qualities will keep his memory alive in the hearts of those who knew him, and when these are dead and gone it will still and for ever hover around the collection of the antiquities of the Royal Irish Academy.

MIDDLE-CLASS EDUCATION IN HOLLAND

THE following article on this subject, "from a Correspondent," appeared in the *Times* of Tuesday:—

It is not unfrequently the case that great nations search laboriously for the solution of problems which smaller peoples have completely solved, as one may say, without effort. We old-fashioned English are at present devoting much pains to discover a good system of education for our middle classes, and yet we have only to cross the Channel in order to see in actual work one altogether satisfactory in a country whose manners, traditions, and laws are almost those of our own.

According to the constitution of Holland there are three degrees of education—Primary, Middle, and Superior. As the Primary Education comprehends all schools intended for children from six to twelve years of age, and as the Universities, the Gymnasia, and other establishments where the study of the ancient languages occupies the first place, are considered as belonging to the Superior class, it follows that all educational establishments not included in one or other of these categories are regarded as establishments for middle-class education.

It appears that until the year 1862 the Dutch were no further advanced in respect of this kind of education than we are now. Wishing to put an end to this state of things, the Minister of the Interior (the Home Secretary) of the time, M. Thorbecke, formerly Professor in Leyden University, presented to Parliament a bill, which was passed into law at the beginning of the following year. From the discussions which preceded the adoption of this law, we learn that its object is to insure a suitable education to young people who are not obliged to learn a business before the age of from fourteen to seventeen years, and for whom, although they are not intended to take up University studies, a deeper and wider instruction is necessary than that which can be obtained at the primary school.

Setting out from the principle that youths who quit the primary schools may be divided into two classes—those who are able to devote only two years, and those who can afford to give five years to further study, it was decided that there should be two kinds of middle-class schools, the one to have a two years' course, and the other a course of five years.

The programme of study in the establishments in which the course is one of two years, and which are called Lower Middle-Class Schools, includes, in the first place, the elements of Mathematics, Mechanics, Physics, Chemistry, Natural History, Geography, History, and the Dutch Language, and in addition, Drawing, Gymnastics, and some idea of Political Economy and of Technology for towns, and of Agriculture for the country. The teachers in these establishments are moreover required to devote the evenings to courses for young artisans or agriculturalists who are prevented from taking the courses which are given during the day.

As to the number of these schools, the law requires that each commune whose population exceeds 10,000 shall establish at its own expense at least one Lower Middle-Class School.

The programme for those schools in which the course is one of five years, and which we may designate Upper Middle Class Schools is of course more extensive. It embraces first the branches included in the Lower Schools, but, as might be expected, this education in the Upper Schools goes much deeper. Then come three foreign languages—French, English, and German. The law requires, moreover, that the pupils should receive some notion of the political institutions of the country and of its statistics, including those of the Colonies. Needless to add, that in a country like Holland the tenure of land must form an integral part of education.

The Higher Schools are naturally those from which the most important results are to be expected, and which, from the English point of view, are best worth careful study. It is simply the truth to say that I have been amazed at what I have seen. It is a very remarkable thing that although no commune is obliged to establish a Higher School—only the State is obliged to maintain five—yet at the present time there is no town having a population of above 15,000 which has not its Higher School in full work. A still more remarkable thing is, that nowhere do the school fees exceed 5*l.* a year. As an Englishman, I was very curious to learn how they were able to give at the rate of 5*l.* a year an education which, in our happier England, can scarcely be obtained at all. This is what I learned. The expenses of a Higher School (not including the maintenance of the building) amount to about 1,750*l.* per annum. Supposing the school to be attended by 100 pupils (a medium estimate), the receipts, under the head of school fees, do not exceed 500*l.* There thus remains a deficit of 1,250*l.*; but the State generally provides a subsidy of 7,000 florins (about 583*l.*), and the town has therefore only to make up the difference by contributing 667*l.* We have supposed the school to be attended by 100 pupils, it is evident that when this number is exceeded, the receipts rise in proportion. This, however, is not always to the advantage of the Communal budget, for it should be known that in Holland a class is not allowed to contain more than thirty pupils, the result being that a greater number necessitates the creation of a double class, and this may require an increase in the number of teachers. Let us note, also, in passing, that the communes which are not able to bear the expense of a complete Higher School are authorised to establish schools of three classes corresponding to the three lower classes of a complete school.

The Communal Councils (town councils) may appoint such teachers as appear to them efficient. It is only necessary that these present certificates of competency and character, and that they have consequently passed the examinations required by the law. There are exempted from these examinations the bearers of certain academic degrees; thus for the mathematical and physical sciences the greater part of the candidates are former students of the Universities. These are generally young doctors of science who have taken a high place. Holland is not slow in showing her gratitude to them.

I have said that in the Higher Schools the school fees, although the law has not fixed a maximum, do not exceed 5*l.* For the Lower Schools the maximum is 1*l.* per annum, but this figure is rarely reached.

It is evident from the above that when a boy of twelve years of age leaves the Primary School and is not immediately obliged to earn money, his father, called in to decide whether or not he shall be sent to a Middle School, has no obstacle to face in the matter of school fees. A foreman or superior workman in a position to keep his son till he is fourteen years of age, can easily pay a shilling a month for school fees; 5*l.* would be an almost insuperable obstacle, though it is none to a father who is able to provide for the other wants of his son until the latter reaches the age of seventeen or eighteen years.

A Dutchman who boasted greatly of the system which his country has adopted, and to whom I remarked that it might be objected that in virtue of the system it was not himself but the taxpayers who paid for the education of his children, replied eagerly: "But am I not myself a taxpayer? Does not the system which we have adopted come simply to this, that instead of my being compelled to pay for the education of my children in a few years under the form of very heavy school-fees, the law allows me thirty or forty years in which to pay it under the form of a tax? As for myself personally, it matters very little, but look at my neighbour, whose three sons are being educated at the Higher School. Change the system; his taxes would perhaps be lessened by twenty florins, but, on the other hand, the school fees would reach so high a figure that he could not meet them. The case of my neighbour is not an exceptional one; it is the case of at least one-half of the parents who send their children to the Higher Schools. Of 100 pupils who are now attending these establishments there would remain scarcely one-half, and it would consequently be necessary to raise to 35*l.* the fees to be paid by each of them; this figure speaks more than all the arguments put together."

If in defence of a new order of things it is only necessary to urge the argument of success, it must be confessed that the advocates of the Higher Middle Schools of Holland do not require to urge any others. By universal consent the success has surpassed all expectation; it has been complete. Yet whoever knows human nature will not be astonished to find that these schools, simply because of their success, are still the object of much criticism particularly among the Clergymen and Scholars of the country. I should have wished to learn from M. Thorbecke, himself a very distinguished scholar, what he thought of these criticisms. That statesman, however, being dead, I applied to one of his former colleagues in Leyden University, whose advice M. Thorbecke to a large extent followed at the time when he was occupied in drawing up his scheme of superior education. I will give you a summary of our conversation. Having asked if it was not a mistake to found a system of education which had not Greek and Latin as its basis, he replied as follows:—

"Allow me to observe to you that our Middle Schools are not intended to produce scholars, orators, statesmen. For these there are the Gymnasium and the University. Has it moreover been thoroughly proved that the profound study of a modern foreign language cannot, as mental gymnastics, take the place of the study of a dead language? I could name to you members of our parliament who have never given any attention to Greek and Latin, and yet who, as orators, are on a par with the most eloquent of their colleagues. The Greeks are represented as having left to us in literature and in philosophy monuments of a perfection such as modern writers can never equal. Yet the Greeks studied no dead language that I know of. Besides what would it serve, in the matter of education, to make a theoretically perfect law, when the mass of the public would condemn it? If there [is one idea strongly rooted in the mind of our middle classes, it is the conviction that Greek and Latin are perfectly useless to anyone who has not to pass through the University. It was daring enough to give so large a place in our new schools to the mathematical and physical sciences, to which our *bourgeoisie* had hitherto given so little attention. To go further and compel this class of people to study in addition Greek and Latin, would have been wantonly to court an inevitable defeat."

I next ventured to point out that the programme is overloaded.

"Overloaded?" replied he. "From whom have you got this accusation? From men who pass their time in their study? Speak a little with our manufacturers and our merchants, and they will give you quite another version of the matter."

"It is not said that useless subjects are taught," I went on to add; "it is urged only that too many things are taught at once, that the mind of the pupil cannot take them in, and that in the end his intellect will be enervated."

"I understand how this objection could have been urged in 1862 and 1863, during the discussion of the law, when experience had not yet pronounced; but now!—at the present time our merchants, who formerly maintained that a man of business has nothing to do with science, that it was rather an embarrassment than otherwise, now receive with open arms any young man having no other recommendation than that of having studied in one of our schools; they will tell you, moreover, that at the end of five or six weeks the new-comer is more useful to them than the majority of their old *employés*, grown gray in harness. There is more to come; it happens that some pupils of the Middle Schools, having acquired a taste for the mathematical and physical sciences, wish to complete their education at the University. Well, they almost always surpass those of their companions who come from the Gymnasia. Confess that all this is very difficult to explain if it be true that in the new schools the mind of the pupil is enervated and atrophied."

Our conversation then went on as follows:—

"You maintain then, that in your new schools, everything is for the best?"

"Pardon! I believe, on the contrary, that there is room for reform. It cannot be denied that the mediocre pupils have great difficulty in learning all that is taught them in the first three forms. Instead of three years, they would require four. The entire course ought to be six years."

"But why at the first did you not fix the course at six years?"

"Because we old-fashioned Dutch, like all the rest of the world, have our characteristic faults. We are a people essentially economical, but unfortunately we are too anxious that our children should begin early to earn money. It was a great point gained, even, to fix the course at five years. What an outcry would there have been had we taken a year more. Besides we had not then the experience that we have now."

"It will then be necessary to modify the law?"

"Yes, but gradually. There are some members of our Chambers who think it will suffice to cut out from the programme the subjects which are called superfluous. I believe it will be well not to oppose this opinion. Let us commence by setting these members to work. That which will be superfluous in the eyes of some will be quite indispensable in the estimation of others. Moreover, they cannot touch either the mathematical and physical sciences or language, and if they end by cutting out anything, a thing which appears to me very problematical, it will be of so little importance as to make scarcely any difference. It will only be when the insufficiency of all these palliatives has been well established that the time will have arrived to apply the remedy that I have indicated to you."

"You believe, then, that if we should decide in England to establish schools of a kind similar to your Higher Middle Schools, it would be necessary to have a course of six years?"

"I do not venture to assert this. You are under better conditions than we are. Our children must, beside their mother tongue, learn three foreign languages—English, French, and German; yours have only to learn French and German. This is a very important point."

"Allow me to ask you one more question. It is urged that your Lower Middle Schools have not succeeded. To what is this ascribed?"

"It would be more correct to say that they have not succeeded throughout. Moreover, M. Thorbecke was never under any delusion on this point. He considered the Lower Middle Schools as placed for the future. The proof is that he got inserted in the law a clause which enacts that the Government may for a certain number of years exempt a communal council from the obligation of erecting a Lower Middle School if it is probable that a sufficient number of pupils could not be obtained to attend it. It is necessary first that the economical condition of the country should be improved. Remember that in Holland wages are in general lower than in all the surrounding countries. We cannot blame our poor artisans for requiring their children to earn some money at the age when these would enter the Middle School."

Such is a *résumé* of what I have seen and heard in Holland.

NOTES

AT the meeting last week of the delegates of the French Learned Societies at the Sorbonne, the Science Section was divided into three committees—Mathematical, Physico-chemical, and Natural History. The general meetings of the three sections were presided over by M. Leverrier, who developed at full length the organisation of agricultural warnings which have been established in Puy de Dome, Vienne, and Haute Vienne, and will be in operation from May 1 to October 15, when agriculturists have practically nothing to lose in the fields. About thirty stations have been established in each of these departments and connected by telegraph with the chief towns of the district. Each local observatory will receive telegraphic warnings through the préfet of the department, to whom will be sent daily the telegrams of the International Service. All these warnings will be posted at the stations and special warnings for the vicinity deduced by local meteorologists. All the observations taken on these stations will be sent to the observatory and tabulated under the supervision of M. Leverrier. The system will very likely be extended to other departments. The distribution of prizes was held on the 22nd in the large hall of the Sorbonne. The Minister of Public Instruction, M. Waddington, gave an address, in which he promised to create new libraries, new faculties, and to group new faculties in order to establish Universites. It is inferred thence that M. Waddington, who, as is well known, is a Cambridge man belonging to Trinity College, will try to remodel the French high schools according to the English method. The old Université de France is, perhaps, to be divided into the Universities of Paris, Lyons, Lille, Marseilles, and Toulouse. M. Waddington's address has created quite a sensation amongst French University men. Five gold medals were awarded—to MM. Abria (Bordeaux), for physics; Clos (Toulouse), for botany; Dumartier (Lyons), palæontology; Filhol (Toulouse), geology; Lortet (Lyons), zoology and palæontology. Ten silver medals were also awarded in botany, zoology, and natural philosophy. In connection with this meeting, M. Lecoq de Boisbandran has been made a Chevalier de la Legion d'Honneur. Several other scientific men have been appointed *officiers* of the University and *officiers* of the Paris Academy, which are special honorary degrees in acknowledgment of some special services either in the prosecution of scientific researches, or in carrying out the results of the scientific investigations of other people.

A TREASURY COMMISSION has just been appointed by Government for the purpose of inquiring into and reporting on the Queen's Colleges in Ireland. The Commissioners are the Rev. Osborne Gordon, of Christchurch, Oxford; Prof. Allman, F.R.S., M.R.I.A.; and Mr. Herbert Murray, Treasury-Remembrancer in Ireland; with Mr. B. Leech as Secretary.

SIR ROBERT CHRISTISON has resigned the position of President-elect of the forthcoming Glasgow meeting of the British Association. Dr. Andrews, of Queen's College, Belfast, has been nominated by the Council in his stead.

THE French Geographical Society are to invite Lieut. Cameron to Paris to a special meeting of the society, to be held for the purpose of marking the appreciation of his merits felt in France.

THE freedom and livery of the Turner's Company were presented at the Guildhall, on Saturday, to Lieut. Cameron and Dr. Atherstone, to whose labours as a geologist the discovery of the value of the South African diamond fields is principally due.

ADMIRAL LA RONCIÈRE LE NOURRY has been reappointed by a large majority the President of the French Geographical Society.

THE French Minister of Public Instruction has given instructions for a series of observations to be made on all the streams of oceanic France, in order to determine the formation of the bar. Stations will also be established on the French coasts for observations of the tides. The previous French observations were made at Brest as far back as 1770, and on them the calculations in Laplace's "Mécanique Celeste" were based.

THE conversazione given last Friday evening at King's College by Mr. H. C. Sorby, F.R.S., to the Fellows of the Microscopical Society (of which Mr. Sorby is president) and their friends, was a brilliant and successful one. One of the greatest novelties exhibited was a new binocular spectroscope, illustrating Mr. Sorby's important discovery of a new method of measuring the position of the bands in spectra.

WE would remind our readers that the afternoon lectures at the Zoological Gardens commence to-day at 5 P.M., the first being by Mr. Sclater, F.R.S., "On the Society's Gardens and its Inhabitants." They will be continued on Thursdays for the next nine weeks.

THE first annual meeting of the Cumberland Association for the Advancement of Literature and Science, will be held at Whitehaven on May 1st and 2nd.

IN Guido Cora's *Cosmos* for April the valuable information on recent expeditions to New Guinea is continued. There is a paper by Major Wood on the Oxus in the time of Alexander, an account of Cameron's work, the continuation of G. Bove's narrative of his visit to Borneo, besides other matters of geographical interest.

PETERMANN'S *Mittheilungen* for April contains an account of the results obtained by Lieut. Cameron to accompany the excellent map of the country explored, which we have already referred to. There is an interesting account of the ascent of the two Norwegian summits, Galdhöpig and Sneehätta, by Hauptmann M. Riuth. Along with a map of New Zealand there is a long article by J. I. Kettler, showing the recent progress of that colony. Drs. Radde and Sievers furnish an interesting preliminary account of their recent travels in Caucasia and the Armenian highlands.

IN the *Bulletin* of the French Geographical Society for March, Dr. Nachtigal's account of his journey in Central Africa (1869-74) is concluded, as is also the account of Abbé David's travels in Western China in 1868-70, and M. J. Codine's paper on early Portuguese discoveries on the West African Coast. There is an itinerary from Tangier to Mogador, by M. Auguste Beaumier.

THE Physical Society of Paris held its Anniversary Meeting last Thursday.

ON Saturday, the *Times* states, there was exhibited in Wolverhampton a meteorolite weighing 8 lbs. It is believed to have fallen on Thursday afternoon in a turf field in a meadow near the Wellington and Market Drayton Railway, about a mile north of the Gradgington Station. It is stated that about ten minutes to four, within a seven miles' radius of the Wrekin, the villagers were alarmed by an unusual rumbling noise in the atmosphere, followed immediately by an explosion resembling the discharge of heavy artillery. Rain was falling heavily throughout the afternoon, but there was neither lightning nor thunder. About an hour after the report what proved to be a mass of meteoric iron was found in the meadow referred to at a depth of 18 inches, having passed through 4 inches of soil and 14 inches of clay. It rested upon the gravel underneath these. The hole is almost perpendicular, and the meteorolite is assumed to have fallen in a south-easterly direction. It is stated that the meteoric stone when found was quite hot, although nearly an hour had elapsed from the time of the explosion being heard.

THE more important articles in the *Quarterly Journal of the Meteorological Society* (London), April, 1876, are the President's Address, the Report of the Council for 1875, a paper by the Hon. Ralph Abercrombie on an improvement in aneroid barometers, and another by Colonel Puckle on Meteorology in India in relation to Cholera. The work of establishing stations begun by the Society in 1874 proceeds satisfactorily, and the Council deserve all praise for the ample details given regarding the twenty-two stations they have now established, and particularly for the valuable addition to this part of the Report, consisting in the lithographed ground-plans of the individual stations, which show the positions and surroundings of the different instruments. We regret, however, to see that the mistake in science regarding the height of the thermometers above the ground, pointed out by us (vol. xi. p. 446) still remains to be rectified, for while exact and minute directions are given respecting the various instruments, each observer is apparently left to his own discretion as to the height at which he places his thermometers. The president's remarks on lightning-rods will be read with interest; and Mr. Abercrombie's improvement on aneroids may be noticed as likely to lead to a more satisfactory observation of small barometrical fluctuations, on a correct knowledge of which so much depends.

WE have received the Programme for Easter, 1876, of the Realschule of Lippstadt. Besides a carefully arranged prospectus of the studies of the school, from the lowest to the highest class, which, in the number and nature of the subjects taught, would make most English teachers stare with wonder, there is an admirable paper by our contributor, Dr. Hermann Müller, who is a master in this school, on the system of teaching Natural History at Lippstadt.

IN a four-page monthly journal published at Laurence, Kansas, U.S., called *The Observer of Nature*, the first five numbers of vol. iii. of which have been sent us, Prof. F. H. Snow gives a catalogue of the Lepidoptera of Eastern Kansas. It is published by the Natural History Society of the Kansas State University, the Vice-President, Secretary, and Treasurer of which are ladies, and which has on its list an officer known as "Critic."

WHAT seems to have been an interesting and successful conversazione was given by the University School Naturalists' Field Club, Hastings, on Saturday week. A number of papers on subjects connected with natural history were read by the boys.

PROFESSORS JORDAN and Copeland, of Indianapolis, Indiana, U.S., are organising a scientific excursion for next summer for the benefit of those wishing to study practically the botany and zoology of the United States. They think that possibly some young Englishmen visiting the American continent during the Centennial, might wish to join such an expedition.

THE degree of LL.D. has been conferred upon Prof. Stanley Jevons, F.R.S., by the University of Edinburgh.

TO-MORROW evening, at the Royal Institution, the discourse will be given by Mr. G. J. Romanes (on the Physiology of the Nervous System of the Medusæ) instead of Prof. Gladstone, who will give the discourse on May 5th.

THE time-honoured New York Lyceum of Natural History has lately changed its title to that of the New York Academy of Sciences, and has published a circular explanatory of its object in so doing, referring to the fact of having published eleven volumes of its Annals. The circular proceeds to state that the limitation of the Society to the subject of natural history is at present unwise, and that if it desires to take a position among the first institutions of the kind at home and abroad, its scope should be greatly enlarged and its title altered to correspond. Although for many years past attention has been paid to chemistry and physics, this is not indicated in its name, and misapprehensions are likely to arise in consequence. Under the new constitution, the direction of the affairs of the Academy is placed in the hands of a body of Fellows chosen for their attainments in science, and four sections have been established, into one or the other of which all members are to be placed. These are first, zoology, botany, and microscopy; second, chemistry and technology; third, geology and mineralogy; fourth, physics, astronomy, and mathematics. Each of these has its own special administration, and is charged with the scientific work of its department. The Annals will be continued under the title of Annals of the New York Academy of Sciences, four numbers being issued every year.

A NEW ZEALAND correspondent states that he met with a curious instance of tenacity of life in the eel lately in Tasmania. Seven years ago a man placed an eel which had been slightly injured along with others in a tank from which he was in the habit of removing them for use constantly as required. The box in which they were kept was perfectly tight and fitted with finely perforated zinc at each end, through which nothing but the most minute organisms could pass. A few days afterwards, when the others were all taken out, the injured eel was left, and so again, when the next supply was put in and removed. It had got very thin, and so he left it, he said, "just to see how long it would live on nothing." It is still in the tank, perfectly transparent and quite white, and is to all appearances healthy and lively enough.

PARTS 3 and 4 of Mr. J. Clifton Ward's paper on the Granitic, Granitoid, and Associated Metamorphic Rocks of the Lake District, has been reprinted separately for the *Quarterly Journal of the Geological Society*.

A FOSSIL cockroach and earwig (*Labidura*) from South Park, Colorado, is described by Mr. S. H. Scudder in the Sixth Bulletin of the United States Geological Survey of the Territories.

A "SCHOOL of Science Philosophical Society" has been for some time established at Gloucester. The members seem bent on real work, and have already formed the nucleus of a good scientific library.

THE Fifteenth Annual Report of the Manchester Scientific Students' Association for 1875 is a very satisfactory one. It contains an account of the excursions made during the year, and also reports of several very good papers read by the members.

THE additions to the Zoological Society's Gardens during the past week include a Weeper Capuchin (*Cebus capucinus*) from Brazil, presented by Major F. J. Ricarde Seaver; a Common Otter (*Lutra vulgaris*), European, presented by Mr. J. Herbert; a Grey Squirrel (*Sciurus cinereus*) from North America, presented by Mrs. M. E. Symons; a Yarrell's Curassow (*Crax carunculata*) from South-east Brazil, presented by Mr. Aug. Ceiyoto; a Scaup Duck (*Fuligula marila*), European, presented by Mr. H. Colliver; two Common Thicknees (*Œdicnemus crepitans*), European, presented by Mr. J. E. Harting; a Coati (*Nasua nasica*) from South America, two Silky Marmosets (*Midas rosalia*) from Rio Janeiro, Brazil; five Graceful Pigeons (*Columba speciosa*) from South America, purchased; a Molucca Deer (*Cervus moluccensis*), born in the Gardens.

SCIENTIFIC SERIALS

THE current number of the *Journal of Anatomy and Physiology* commences with a paper by Dr. A. Ransome on the relative powers of fresh and previously used pepsine in the digestion of albumen, in which it is demonstrated that pepsine has greater activity after it has been used than when fresh, in which respect it is shown to agree with ptyalin, as shown by Dr. Foster, and with pancreatin according to Thiersch.—Following is a contribution on the anatomy of the cutis of the dog, by Dr. Stirling, with two plates, republished from the *Berichte d. Math. Phys. Classe der König. Sächs. Gesel. d. Wiss.*, 1875.—Mr. R. H. A. Schofield makes observations on taste-goblets in the epiglottis of the dog and cat, closely resembling the same structure in the tongue.—Dr. J. Blake, of San Francisco, describes the physiological action of the salts of beryllium, aluminium, ytrium, and cerium, by injecting them into the blood.—Dr. Brunton shows that Condurango is physiologically inert.—Mr. J. C. Ewart has a note on the abdominal pores and urogenital sinus of the lamprey, in which he demonstrates that the ureters and internal abdominal pores open into a urogenital sinus which opens behind the rectum on a papilla.—Mr. E. Thurston determines the length of the systole of the heart, as estimated from sphygmograph tracings, in which, from a series of measurements, he verifies Mr. A. H. Garrod's law that in health the systole, as indicated in the radial artery, is constant for any pulse-rate, and varies as the cube root of the rapidity.—Mr. A. M. Marshall explains the mode of oviposition of Amphioxus, verifying Kowalevsky's observation that the ova escape by the mouth.—Mr. F. Darwin describes the structure of the snail's heart histologically. No nervous mechanism was found. The contractile tissue is striated, and the fibres of the auricle and ventricle are continuous.—Dr. Stirling notes the effects of division of the sympathetic nerve in the neck of young animals.—Prof. Turner describes the structure of the non-gravid uterine mucous membrane in the kangaroo, and makes a note on the dentition of the narwhal.—Mr. F. M. Balfour continues his valuable account of the development of the Elasmobranch fishes, with five excellent plates and many woodcuts.—Mr. P. H. Carpenter makes remarks on the anatomy of the arms of Crinoids, his results being arrived at from sections of decalcified specimens.—Dr. Foster describes some effects of Upas Antiar on the frog's heart, demonstrating that the resulting tetanus is brought about by an extraordinary prolongation of the diastole, and not by a too rapid sequence of beats. The arguments for and against the existence of both accelerator and inhibitory fibres in the heart are discussed, in relation with the influence of antiar; and the assumption of the existence of specific accelerator fibres is shown to be unnecessary.—Dr. Curnow notes variations in the arrangement of the extensor muscles of the fore-arm.—Dr. Brunton explains a simple method of demonstrating the effect of heat and poisons upon the heart of the frog.—Mr. G. A. Berry and Prof. Rutherford note with reference to Pflüger's law of contraction, that the excitability and length of the portion of nerve traversed by the voltaic stream must be taken into account in studying the changes of the electrotonic state.—Prof. Rutherford notes with regard to the action of the internal intercostal muscles, their elevating action, as rendered evident by binding similarly situated elastic bands to the ribs themselves.—Mr. Reoch has a paper on the oxidation of urea.—Mr. R. Hughes describes an improved freezing microtome, in which ether spray is the cold-producer.—Dr. S. Coupland records an example of Meckel's diverticulum in man.—The Report on Physiology, by Dr. Stirling, concludes the number.

Poggendorff's Annalen der Physik und Chemie. Ergänzung, Band vii. Stück 2.—In this number is concluded M. Voigt's paper on determination of the constants of elasticity of rock salt; the case of torsion being here dealt with. Comparing his general results with Navier and Poisson's theory, he finds they contradict it in some points, *e.g.* the crystals of the regular system do not behave, in reference to elasticity, like uncrystalline media; for the bending and torsion coefficients are *not* independent of the direction; the constants also have different relative values.—M. Obach describes some interesting experiments on the behaviour of amalgams and metallic alloys under the galvanic current. He finds (1) that the current does not produce in either electrolytic separation of the constituents; (2) that sodium amalgam, after being traversed by the current, decomposes water at both poles as before; (3) that action of the current alters neither the hardness nor malleability of tin-lead alloys, nor the liquid state of potassium-sodium alloy. It works in the chemical composition of the alloy near the electrodes no changes exceeding errors of experiment and analysis. In all these points the author opposes M. Gerardin, who, a short time ago, published experiments on the subject. As to the electric currents occurring in amalgamation of metals, M. Obach regards them as thermo-electric currents due to the temperature changes produced by amalgamation.—M. Clausius contributes a lengthy memoir on the proposition of the mean ergal and its application to the mechanical motions of gases; and a paper by M. Weinberg treats of the application of the mechanical equivalent of heat to molecular forces, size, and distance.

Zeitschrift der Oesterreichischen Gesellschaft für Meteorologie, Jan. 1.—An article extracted from the Proceedings of the Vienna Academy, containing the results of Prof. Kerner's studies and observations, in the neighbourhood of Innsbrück, on the abnormal rise of temperature with increasing elevation in the valleys of the Alps in late autumn and in winter, occupies nearly the whole of this number. The phenomenon occurs every year without fail in this district, and has been observed in Carinthia, Upper Austria, the Tyrol, and Switzerland. The number of farmhouses upon the mountain sides at an inconvenient distance from the pastures below shows that the inhabitants are well aware of the milder climate to be found at moderate altitudes. While frost reigns in the valleys and trees are leafless, the grass and trees upon the heights frequently keep beautifully green, and flowers that bloom elsewhere in autumn and even in spring bloom in the genial air. The valley folk say at such times that the south wind blows aloft and will soon descend to them. Prof. Kerner acknowledges the plausibility of this notion, but gives good reasons for believing it to be ill-founded. It is true that the equatorial current does descend upon the valleys, gradually displacing the polar, but in the first half, at least, of the period of reversed temperature, none of the signs of a south wind appear in the atmosphere, barometric pressure keeps very high, and the sky clear. The latest uncommonly long spell of seventeen days with reversed temperatures, from Oct. 25 to Nov. 10, 1874, enabled Herr Kerner to ascertain the real cause of the strange and hitherto perplexing phenomenon. From an ascent of the Unnutz (2,111 metres), he learnt that the warm region in every valley lies between two cold regions, whose borders differ in position in every valley. The situation of the nether border of the warm region certainly depends on the height of the bottom of the valley. He reached this border at about 200 metres above the level of the Inn, or 700 metres above the sea, and passed out of the warm region into an atmosphere colder than that of the valley at 1,890 metres above the sea. In crossing the Achnenthal at 950 metres, there was a fall of the thermometer, which was soon succeeded by a rise as he continued to climb. In descending the favoured slopes of the mountain he observed several kinds of flowers, some of which generally come out in spring; but a little lower all vegetation was sprinkled with hoar frost. In making his ascent on the sunny side an upward current accompanied him. On the summit a very slight air came from N.E. as long as the sun kept high. Late in the afternooon it had risen to a fresh breeze, and after sunset the N.E. wind was violent. He then made a short descent on the N.E. slope to about 30 or 40 metres from the top. Here he found a calm, and a little lower a breeze blowing down towards the valley. It appeared accordingly that the polar wind divided itself near the top into two streams, one of which turned down into the valley, while the other flowed over the top and then down into the other valley at the foot of the southern slope. This distribution of currents seemed to him to point to a reason for the existence of a warm region, like that which Herr Hann found for the high temperature of the Föhn wind, namely, that in descending the cold air becomes condensed, and by condensation raised in temperature. From 4 P.M. on the 4th to 5 P.M. on the 5th of November, 1874, readings of the temperature were taken by four observers at Innsbrück (575 metres), Rumer Alpe (southern slope, 1,227 metres), Heiligwasser (northern slope, 1,239 metres), and at the summit of the Blazer (2,240 metres). The mean temperatures for the twenty-four hours at these stations were respectively, 2·16, 7·06, 4·26, and – 64. The lowest night temperature at Innsbrück was – 2·8; on the Rumer Alpe, + 2·4. The minimum was reached at Innsbrück, just before sunrise, but on the Rumer Alpe at 3.30 A.M.; at sunrise at this elevation the thermometer marked 4·4. At Heiligwasser the same kind of relation was noted, and temperature rose after 4 A.M.; but the maximum by day was much lower than at Innsbrück. The high temperature at this station was not due to heating of the ground by sunshine, for a thermometer fixed on the surface of the soil never rose above 1° C. The wind blew uninterruptedly towards the valley, down the mountain side. There remains but one explanation, namely, that the increasing pressure raises the temperature of the air as it descends. Prof. Kerner proceeds to a more detailed analysis of the distribution of currents over hill and valley both by day and by night, illustrating his theory by diagrams. After sunset the ground of the valley and the air above it cool rapidly by radiation. The air thus made specifically heavy cannot flow off, but rests like a lake at the bottom of the valley. The current which has flowed down the mountain sides being raised in temperature, glides over this stratum, and rises about the middle of the valley, to rejoin the polar wind aloft. By day the air ascends from the valley up the southern slope, and is replaced by a current descending the opposite mountain face. Obviously, the phenomenon of increasing temperature with increasing height must be most striking where the ridges and valleys stretch from west to east, and during periods of polar wind, when the sky is clear and radiation strong.

Der Naturforscher, January.—This number contains an account of observations by M. von Schleinitz, on board the *Gazelle*, when on the transit expedition to Kerguelen's Land, of changes of temperature and specific gravity of water in the southern Indian Ocean. His conclusions are briefly these:—1. Ocean currents, with the exception of the currents caused by regular winds, are due to differences in absolute specific gravity of different parts of oceans, and a small difference produces a strong current. 2. The differences in saltness of tropical and cold seas (in relation to absolute specific gravity), acting oppositely to the temperature differences, moderates ocean currents, which would otherwise be so strong in meridional directions that navigation would be impossible. 3. There is probably a zone where the differences in saltness compensate the differences in temperature, so that waters of different temperature and different saltness may be near each other in equilibrium, *i.e.*, without perceptible current. In the western part of the Indian Ocean this zone is between 40° and 45° S. lat.—There is a notice of two recent series of researches by M. Voigt and M. Groth (conducted by quite different methods), on the elasticity of rock salt; it is shown that in regular crystals the co-efficient of elasticity, and therewith the velocity of sound, is a function of the direction; and that both vary, in accordance with Neumann's theory, symmetrically with reference to the planes of symmetry of the crystal.—M. Frank calls attention to the action of light on the opening of some catkin-like blossoms.—From experiments by M. Luchsinger, it appears that glycerine injected under the skin of animals has an arresting action on the fermentative formation of sugar from the glycogen of the liver.—The remaining papers do not call for notice here.

Jahrbücher für Wissenschaftliche Botanik. Herausgegeben von Dr. N. Pringsheim. Zehnter Band, Drittes Heft, Mit. 11, Tafeln (Leipzig: Verlag von Wilh. Engelmann, 1876).—The present number of Pringsheim's well-known "Year-book" contains three papers, all of great value. The first is by Dr. George Winter, on the genus Sphæromphale and its allies (with three plates). Kœrber in criticising the Schwendener-Bornet theory of lichens, stated that Sphæromphale had only greenish-brown microgonidia, and that the spores did not produce hyphæ. Both these statements are shown to be erroneous, and after a careful anatomical and morphological examination of numerous original specimens, dried and recent, of Sphæromphale and its allies, he groups them together under a single species, *Polyblastia umbrina* (Whlnbg.), Winter, and adds nearly three pages of synonyms!—an eloquent tribute to the species-making capabilities of modern Lichenographers.—The second paper is by Dr. A. Engler, Contributions to the knowledge of the formation of the anther in Metasperms. This paper, which is illustrated with five plates, describes the following subjects: (1) the anthers and pollen of the Mimoseæ; (2) the anthers of Orchidaceæ; (3) the anthers of Asclepiadaceæ; (4) on the so-called introrse and extrorse anthers; (5) on certain apparent departures from the type in the formation of stamens; and (6) on the homologies between stamen and carpel.—The third paper is by Dr. J. Reinke, Contributions to the knowledge of Fucaceæ and Laminariæ (with three plates). The anatomy and external construction of several genera and species are detailed, the most interesting portion of the paper being the paragraphs devoted to secondary circumferential growth in Fucaceæ, and to the formation of adventitious buds.—The illustrations are excellent as usual, and the high character of the *Jahrbücher* well sustained.

Bulletin de l'Académie Royal des Sciences, Nos. 9 and 10, contains an article by Van Beneden on the Pachyacanthus in the Museum at Vienna. The description of other marine mamifers in other museums is to follow, and the whole are to form an introduction to the descriptions of the allied fossil forms discovered in excavations near Antwerp.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, April 6.—"Experiments on the Friction between Water and Air." By Dr. Ritter von Lang. Communicated by N. Story Maskelyne, F.R.S., Keeper of the Mineral Department, British Museum.

The method adopted for estimating the mutual friction of water and air consisted in connecting a glass tube of 8 centims. in length and 0·72 internal diameter with the pipes which supply Vienna with water at a pressure of four atmospheres. Arrangements for securing a vertical position for the tube ensure a perfectly continuous jet, devoid of any broken surface; and a tube surrounding this jet, with its axis coinciding with that of the jet, acts as an aspirator into and along which air is drawn through a lateral feeding-tube. The amount of this in-drawn air corresponding to the fall of a given amount of water was determined by observing the rate at which a film of soap was borne along the feeding-tube; and the velocity of the water causing the in-draught was calculated from the diameter of the water-column and the quantity of water discharged along it in a given time; but after having once determined the form of the slightly conical water column, the amount of water discharged was the only datum required for the calculation.

The influence of a greater or less section of the air feeding-tube on the volume of the aspirated air was carefully determined, while also the absence of any appreciable retardation due to the soap-film was established.

Neglecting the slightly conical character of the surface of the water-column, and assuming (as the result of experiments in which the motion of a smoke-cloud was observed) that the movement of the air was throughout in lines parallel to the axis of the tube along which it flowed, and showing that the pressure does not vary along the length of the tube, the author proceeds to discuss the hydrodynamic equations expressing the conditions of the problem (the motion of the air being uniform and independent of time), and represents the volume of air A passing through the tube in a second as

$$A = W\left[\frac{R^2 - r^2}{2r^2(\log R - \log r)} - 1\right],$$

W being the weight of water, in grammes, discharged in a second, r the radius of the jet in turns of the micrometer-screw (6·8 turns of which correspond to 1 centim.), R being the radius of the aspirating tube.

The results obtained by observation accorded well with those given by this equation, so long as the value of R did not exceed the limit within which the suppositions regarding the motion of the air hold good.

The question whether the results might not be brought into even closer accord with theory by the assumption that a slipping action takes place between the air and the water-jet on the one hand, and between the air and the tube on the other, instead of the assumption previously made that the air adhered alike to the water and to the tube in its passage. The result of the calculation, however, led to no nearer approximation; and finally, experiments with other materials for the tube and other gases (namely, coal-gas and carbonic anhydride) were made without resulting in any marked difference from the results obtained with air and glass.

Mathematical Society, April 13.—Prof. H. J. S. Smith, F.R.S., president, in the chair.—Prof. Henrici, F.R.S., having taken the chair, the President gave an account of a note—"Sur une théorème d'Eisenstein"—by M. Charles Hermite. This is the celebrated theorem, considered by M. Heine, on the development in a series of the roots of an algebraic equation $f(y, x) = o$. M. Heine has added the very important remark that we can make all the coefficients of such a development, supposed commensurable, integers, with the exception of the first by changing x into $k\,x$ (Crelle, Band 48, p. 267). M. Hermite's communication gives a simplified proof of this.—Prof. Smith then spoke on the aspects of circles on a plane or on a sphere. He pointed out the connection between his results and those obtained by Prof. Cayley in his researches on trees. He next made some remarks on a problem in crystallography.—Mr. Tucker read part of an abstract (drawn up by Dr. Hirst, F.R.S.) of a paper on correlation in space, by Prof. Rudolf Sturm, of Darmstadt. The paper is connected on the one hand with Sturm's previous one on projectivity in space (*Math. Ann.*, vol. vi.), and on the other with Dr. Hirst's papers on the correlation of two planes, and two space. (*Proc. of Math. Soc.*, vols. v. and vi.)

Chemical Society, Prof. Andrews, F.R.S., in the chair.—A paper on the manufacture of sulphuric anhydride, by Dr. R. Messel and Dr. W. Squire was read by the latter. The authors prepare the anhydride by decomposing ordinary sulphuric acid at a white heat into water, oxygen, and sulphurous anhydride, removing the water by suitable means and then passing the mixed gases over platinised pumice heated to low redness; the oxygen and sulphurous anhydride then reunite to form sulphuric anhydride.—After this paper there was an adjourned discussion on Dr. H. E. Armstrong's paper on systematic nomenclature read at the last meeting, in which Prof. Odling replied at length to the criticisms on the article recently published by him on the same subject in the *Philosophical Magazine*.

Royal Astronomical Society, April 12.—Mr. Wm. Huggins, D.C.L., president, in the chair.—J. Bagnold Smith, Sir David Solomons, W. T. Smedley, Wm. Durrad, Wm. Allsup, and the Rev. Joseph Ferguson were elected Fellows of the society.—Mr. Penrose described an instrument for calculating the sides and angles of spherical triangles. It consisted of two wooden semicircles which could be fixed at any angle, and a graduated arm moving on a universal joint which slid along one of the semicircular arcs. The graduated arm was made use of to measure the cord of the third side of the triangle. Mr. Penrose showed how the instrument might be made use of for roughly checking calculations in spherical trigonometry. He thought that it would also be of use in expeditiously reducing observations in which no great degree of accuracy was required. The instrument was very portable and might be made still more so if the graduated semicircles were divided on brass instead of on wood, as in the instrument he showed. A paper by the Rev. T. W. Webb was read describing some observations of the two exterior satellites of Uranus which had been made by Mr. Isaac Ward of Belfast. Mr. Ward's instrument is a refractor of only 4·3 inches aperture, but he had apparently succeeded on some dozen evenings during the months of January, February, and March, in picking up both the outer satellites Titania and Oberon. A table was given comparing the position angles and distances as estimated by Mr. Ward with those taken from Mr. Marth's ephemeris of the satellites. It was stated that the estimates of Mr. Ward had been made without any previous reference to the ephemeris, the coincidences were such that there seemed little room left for doubt that Mr. Ward had in each instance been successful in picking up the satellites. Mr. Lassell said that he had not seen the satellites with his own nine-inch. It was quite possible that the extraordinary sharpness of Mr. Ward's eye might have enabled him to pick up the satellites; there were records which could not be doubted of persons who had observed the satellites of Jupiter with the naked eye. He thought that if any one else made use of the same telescope they would certainly not be able to detect the satellites.—Mr. Green drew the attention of observers to the visibility of the dark limb of Venus during the coming quadrature; he had on many occasions thought that he perceived the dark limb on a brighter background, but on placing the bright limb of the planet behind a dark bar in his eye-piece, he had entirely lost sight of the dark limb. He wished that other observers would try the same experiment during the coming quadrature. The meeting adjourned till May 12.

Geological Society, April 5.—Prof. P. Martin Duncan, F.R.S., president, in the chair.—James Mansergh, M. Inst. C.E., was elected a Fellow of the Society.—On the bone-caves of Creswell Crags (second paper), by the Rev. J. Magens Mello. In this paper the author gives an account of the continuation of his researches upon the contents of the caves in Cresswell Crags, Derbyshire. The further exploration of the Pin Hole cave described in his former paper,[1] furnish a few bones of Reindeer, *Rhinoceros tichorhinus*, and other animals, but no more remains of the Arctic Fox, which were particularly sought for. Operations in this cave were stopped because the red sand in which the bones were found towards the entrance became filled with limestone fragments, and almost barren of organic remains. The author then commenced the examination of a chambered cave called Robin Hood's cave, situated a little lower down the ravine on the same side. The section of the contents of this cave showed a small thickness of dark surface-soil, containing fragments of Roman and mediæval pottery, a human incisor, and bones of sheep and other recent animals; over a considerable portion a hard limestone breccia, varying in thickness from a few inches to about 3 feet; beneath this a deposit of light-coloured cave-earth, varying in thickness inversely to the breccia, overlying a dark-red sand about 3 feet thick, like that of the Pin Hole, but with patches of laminated red clay near the base, and containing scattered nodules of black oxide of manganese, and some quartzite and other pebbles, which rested upon a bed of lighter-coloured sands containing blocks of limestone, probably forming part of the original floor of the cavern. The hard stalagmitic breccia contained a great many bones, chiefly of small animals, but with some of reindeer, and teeth of *Rhinoceros tichorhinus*, hyæna, horse, water vole, and numerous flint-flakes and chips, and a few cores. Some of the flakes were of superior workmanship. A few quartzite implements were also found in the breccia. The cave-earth contained a few flint implements, but most of the human relics found in it were of quartzite, and of decidedly palæolithic aspect. There was also an implement of clay-ironstone. The animal remains chiefly found in the cave-earth were teeth of horse, *Rhinoceros tichorhinus*, and hyæna, and fragments of both jaws of the last-mentioned animal. Bones and teeth of reindeer and teeth of cave-lion and bear also occurred. The red sand underlying the cave-earth contained but few bones, except in one place, where antlers and bones of reindeer and bones of bison and hyæna occurred. At another part a small molar of *Elephas primigenius* was found. A large proportion of the bones had been gnawed by hyænas, to whose agency the author ascribed the presence of most of the animal remains found; but he remarked that no coprolites of hyænas had been met with. The following is a list of the animals whose remains occurred in this cavern:—*Felis leo* (var. *spelæa*), *Hyæna crocuta* (var. *spelæa*), *Ursus arctos*, *U. ferox*, *Canis familiaris*, *C. lupus*, *C. vulpes*, *Elephas primigenius*, *Equus caballus*, *Rhinoceros tichorhinus*, *Bos bison*, var. *priscus*, *Bos longifrons*, *Capra hircus*, *Sus scrofa*, *domesticus*, and *ferox*, *Cervus megaceros*, *C. tarandus*, *Arvicola amphibius*, and *Lepus timidus*.—On the mammalia and traces of man found in the Robin Hood Cave, by Prof. W. Boyd Dawkins, F.R.S. The author noticed the various species of animals discovered by Mr. Mello during the researches, the results of which are given in the preceding paper, and drew certain conclusions from their mode of occurrence as to the history of Robin Hood's Cave. He considered that the cave was occupied by hyænas during the formation of the lowest and middle deposits, and that the great majority of the other animals whose remains occur in the cave were dragged into it by the hyænas. That they served as food for the latter is shown by the condition of many of the bones. During this period the red sand and clay of the lowest stratum was deposited by occasional floods. The red loam or cave-earth forming the middle stratum was probably introduced during heavy rains. The occupation of the cave by hyænas still continued, but it was disturbed by the visits of palæolithic hunters. The remains found in the breccia indicate that the cave was inhabited by man, and less frequently visited by hyænas than

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[1] See Quart. Journ. Geol. Soc., vol. xxxi. p. 679.

before. The presence of vertebræ of the hare in the breccia would imply that the hunters who occupied the cave had not the dog as a domestic animal. After a discussion of the relations of the animals forming the fauna of the cave, the author proceeded to describe the traces of man found in it, which consist of fragments of charcoal, and implements made of antler and mammoth tooth, quartzite, ironstone, greenstone, and flint. The distribution of these implements in the cave represents three distinct stages. In the cave-earth the existence of man is indicated by the quartzite implements, which are far ruder than those generally formed of the more easily fashioned flint. Out of 94 worked quartzite pebbles only three occurred in the breccia, while of 267 worked flints only 8 were met with in the cave-earth. The ruder implements were thus evidently the older, corresponding in general form with those assigned by De Mortillet to "the age of Moustier and St. Acheul," represented in England by the ruder implements of the lower breccia in Kent's Hole. The newer or flint series includes some highly-finished implements, such as are referred by De Mortillet to "the age of Solutré," and are found in England in the cave-earth of Kent's Hole and Wookey Hole. The discovery of these implements considerably extends the range of the palæolithic hunters to the north and west, and at the same time establishes a direct relation in point of time between the ruder types of implements below and the more highly-finished ones above.—Notes on the gravels, sands, and other superficial deposits in the neighbourhood of Newton-Abbot, by Horace B. Woodward, F.G.S. The writer pointed out that most of the deposits termed Upper Greensand in the immediate neighbourhood of Newton-Abbot, were in reality intercalated with coarse gravel-beds, containing, among others, fragments of greensand, chert, and chalk-flint. He considered that the only traces of greensand *in situ* were probably on the summit of Milber Down and east of Combe Farm, deposits which were identified by Mr. Godwin-Austen. But he could not agree in the identification of greensand at other localities in the Bovey Valley, considering the few fossils found to have been derived from, and with much other material to have been evidently due to ,the denudation of chalk and greensand. He pointed out the geographical distribution of these beds of sand and gravel, which extend from the hill-tops bordering the Bovey Valley to near the bottom of the valley, but do not descend into any outlying valleys. He likewise alluded to the peculiar dip into the valley which affects these beds in several places, and observed that sometimes they rested on the Bovey clays and lignites. He thought some connection in their method of formation might be traced with somewhat similar deposits on the Haldon and Black Down Hills. He pointed out the "Head" at the bottom of the valley was sometimes not to be distinguished from the older gravels, from which, however, it was largely derived. He alluded to the discovery of bones, a bronze spear-head, and a wooden doll or idol in this deposit; observing that they indicated the rapid accumulation of gravel, and that this indication was one out of many that might be given, that our modern river-gravels are to a great extent made up of older gravels. In conclusion the writer alluded to some of the deposits now forming on the margin of the Teign estuary, and which are identical in character with the Triassic breccia.—On certain alluvial deposits associated with the Plymouth limestone, by R. N. Worth, F.G.S. The author adduced certain deposits found in fissures and caverns of the Plymouth limestone, as furnishing evidence in opposition to the views advocated by Mr. Belt in his paper on the drifts of Devon and Cornwall.[1] The best examples occur at Plymouth Hoe, where the chief deposit fills a large "pocket" in the limestone, and consists (beneath the turf) of a bed of clayey soil, containing pebbles and small boulders, beneath which are patches of white and red clay, containing a few pebbles, and overlying a large quantity of siliceous sand. Similar, but slightly varying deposits, not unfrequently occur in association with the limestone; and these are regarded by the author as the remains of considerable deposits which once occupied large areas in the valleys of South Devon; and if they are not the lowland gravels of Mr. Belt, the latter are not represented in the district. The author states that there is evidence of the contemporaneity of these deposits with those of the Oreston caves; and he adds that they furnish no proof of cataclysmal action, but of orderly deposition, the bulk of the pebbles and gravels being inland nearer the source of the *débris*, and further off the sands and clays in fairly regular succession. The author further explains the presence in Cornwall of stanniferous gravels only in valleys opening to the south, by reference to the position of the watershed in that county, which has only two rivers running to the north, whilst on the south-east rivers abound.

Physical Society, April 8.—Mr. W. Spottiswoode, vice-president, in the chair.—Mr. H. M. Klaassen was elected a member of the Society.—Prof. Foster exhibited and described an instrument for illustrating the law of refraction. It is founded on the well-known method of determining the direction of the ray after refraction by means of two circles described from the point of incidence as centre, the ratio of whose radii is the index of refraction. If the incident ray be projected to meet the inner circle, and through the point of intersection a vertical line be drawn, the line drawn from the point of incidence to the point where this meets the outer circle is the direction after refraction. This principle is applied in making a self-adjusting apparatus as follows:—A rod representing the incident ray is pivoted at the point of incidence, and projects to a point about 4 inches beyond. To this extremity is attached a vertical rod which slides through a nut in another rod also pivoted at the point of incidence. The lower extremity of the vertical rod is attached to a link, so fixed as to constrain it to remain vertical. By this means the two rods always represent respectively the incident and refracted rays, and the index of refraction can be varied by altering the position of the nut, through which the vertical rod passes, on the rod to which it is attached.—Prof. Foster then exhibited a simple arrangement for showing the interference of waves. It consists of two glass plates placed one in front of the other, on each of which is drawn the ordinary sine wave. They are supported in a frame, and behind them is a paper screen bearing lines to indicate the points of maximum and minimum displacement. The plates can be made to slide in opposite directions, and all the phenomena of wave motion generally, and the state of the air in open and closed tubes can be shown. Lastly he exhibited a method, which has been suggested by Prof. Kundt, for showing in a simple manner that the air in an organ pipe is in a constant state of alternate condensation and rarefaction. At the upper end of a closed pipe are placed two valves opening inwards and outwards respectively, and the chambers behind these are connected by india-rubber tubes with small water-guages which, for the sake of exhibition, were projected on the screen. The guages were to the eye permanently set, showing at the same time condensation and rarefaction, an appearance which was of course due to the rapidity of change. It was shown that beats cause the air to approximate to its normal density.—Prof. Guthrie exhibited and described an arrangement which he thought might be useful for determining the rate at which machinery is revolving. The instrument is analogous to one which he devised some years ago for rendering a galvanic current constant. The chamber of a manometer is connected with a small force-pump, which makes one complete stroke for every revolution of the engine. A capillary glass tube affords a means of escape for the air introduced by the pump into the manometer. If now the pump be worked uniformly, that is if the engine rotates uniformly, the pressure in the manometer will shortly attain a position of equilibrium, so that the mercury will remain stationary. But if the velocity of the engine increase, the mercury will immediately ascend, and so indicate this increase of speed. The main objection to the instrument, as exhibited, was the oscillation of the mercury, but this might be avoided in several ways which were pointed out.—Mr. Coffin referred to some works in America where he had seen a similar principle applied. The engine was connected with an air chamber, to which was applied a Bourdon's gauge, the indications of which gave an approximate measure of the revolutions of the engine.—Prof. Unwin thought there would be some difficulty in keeping the capillary orifice perfect for any length of time. He referred to a proposal made by Prof. Thomson in about 1852 to use a centrifugal pump for a similar purpose.

Anthropological Institute, April 11.—Col. A. Lane Fox, president, in the chair.—Five new members were announced.—A note on some suggested archæological symbols for maps, by Mr. Joass, was read by Capt. Dillon.—Dr. Comrie, R.N., read a paper entitled "Anthropological Notes on the Natives of New Guinea," being the result of his observations while attached to H.M.S. *Basilisk*, engaged surveying there. The physical, social, and religious character of the Papuans were discussed, and the probable Polynesian intermixture and origin of the natives of New Guinea considered, the author inclining to the opinion that the Papuan was a pure type of race, the most characteristic fea-

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[1] See "Quart. Journ. Geol. Soc.," vol. xxxii., p. 80.

ture next to language being the tape-like flattening of their hair noticeable in an ordinary lens. The paper was accompanied with a comprehensive exhibition of Papuan weapons, works of art, utensils, and articles of dress, which will remain at the Institute till their next meeting, April 25, when the discussion, in which Col. Fox, Lieut. Armit, R.N., Mr. Franks, Mr. Hyde Clarke, the Rev. A. H. Sayce, and others took part, will be continued.—Mr. Brabrook, the Director, then read a paper by Mr. B. Walker entitled "Religion, Politics, and Commerce of Old Calabar," which contained an account of the singular institution of Egbo, the principal object of which is to secure mutual protection amongst the freemen. Admission to the various grades, nine in number, is by purchase. As regards religion each district has a separate but subordinate divinity. Their commerce consists of palm oil, ebony, ivory, and barwood. The inhabitants appear to be advancing in civilisation.

Entomological Society, April 5.—Prof. Westwood, president, in the chair.—Messrs. J. W. Douglas, E. C. Rye, G. Lewis, C. Fenn, J. Dunning Kay, and W. C. Copperthwaite were elected Ordinary Members; and Mr. B. A. Bower, jun., was elected a Subscriber.—Mr. F. Bond exhibited a specimen of *Xylina lambda* taken near Erith in September last by Mr. W. Marshall, being the fifth instance of its having been taken in Britain. He also exhibited *Ebulea stachydalis*, taken by himself at Kingsbury, Middlesex, in June 1862.—Mr. Champion exhibited specimens of *Ægialia rufa*, taken by Mr. Sidebotham near Southport, and he brought examples of *Psammodius sulcicollis* for distribution.—The President made some observations respecting the habits of the common gnat, in continuation of his remarks at the meeting of Nov. 4, 1872. Large numbers of females had again appeared in his house at Oxford, not a single male having been observed, and he believed they had hibernated in the house, appearing the first warm days of spring. He also remarked that Dr. Leconte's valuable collection of *Coleoptera* had been presented to the University of Cambridge, Mass.—Sir Sidney S. Saunders exhibited living specimens of *Stylops Kirbii* taken by himself at Hampstead; altogether he had found eighteen males. Mr. Enock also exhibited a row of eleven males taken on the wing at the same place and about the same time.—The Rev. A. E. Eaton stated that he was preparing a supplement to his monograph of the *Ephemeridæ* (*Trans. Ent. Soc.*, 1871), chiefly from materials in the collections of Mr. McLachlan and Mr. Albarda, and that he would be glad of any assistance that could be given him by entomologists possessing insects belonging to that group. It appeared that the deficiency in legs in *Campsurus* and some of its allies was due to their being shed with the pupa skin when the insect obtains well-developed wings, and that in some forms all the legs were thus cast off by the female.—Mr. Smith made remarks on the distribution of some genera of Hymenopterous insects from New Zealand, of which a collection had been placed in his hands by Mr. C. M. Wakefield; and was followed by Mr. McLachlan, who remarked on the gradual extinction of the endemic fauna of New Zealand, although introduced forms throve wonderfully.—The Rev. R. P. Murray stated that he was preparing a list of the species of Japanese butterflies, and that he would be grateful to any entomologist who could assist him with the loan of specimens.—Mr. McLachlan exhibited a series of *Anomalopteryx Chauviniana*, Stein., from Silesia, given to him by the discoverer of the species, Fräulein Marie von Chauvin, of Freiburg. This singular Trichopterous insect pertained to the family *Limnophilidæ*, and was remarkable for the lanceolate anterior, and abbreviated posterior wings of the male; those of the female being normal, excepting that the posterior wings were smaller than usual. Also apterous females of *Acentropus niveus* received from Mr. Ritsema, of Leyden; and a microscopic slide with a full-grown female example of *Phylloxera vastatrix* of the root form. This he had recently obtained, with many others, from a vinery near London, which was terribly infested with the insect.

Meteorological Society, April 19.—Mr. H. S. Eaton, president, in the chair.—T. H. G. Berrey, Assoc. Inst. C.E., H. G. Bolan, J. Bravender, J. Holden, G. A. Hutchins, F. Jackson, J. L. Johnson, B. Latham, A. G. McBeath, W. R. Maguire, A. S. Moss, C. Pink, J. R. Rogers, E. Toller, S. Tomlinson, W. A. Mc I. Valon, H. Walker, W. E. Woolley, were elected Fellows of the Society. The following papers were then read:—Velocity of the wind at Liverpool, tabulation of anemometric records, by W. W. Rundell; on the aspiration of the dry and wet bulb thermometers, by Samuel H. Miller, F.R.A.S.; on the storm which passed over the south of England on March 12, 1876, by Robert H. Scott, F.R.S.—The members of the Permanent Committee of the Vienna Meteorological Congress were present and took part in the discussion.

Victoria (Philosophical) Institute, April 3.—Mr. Howard, F.R.S., read a paper on the history of Egypt in connection with the Bible.

ROME

R. Academia dei Lincei, Dec. 5, 1875.—M. Moriggia communicated the results of experiments on the natural poisons of the body, bile and amygdaline.—M. Volpicelli gave a short necrological memoir of Wheatstone.

Jan. 2.—M. Volpicelli described the construction, properties, and applications of a constant inductor.—M. Sella gave the composition of various salt springs in Italy.—M. Capellini presented a paper on Tuscan fossil whales.—M. Castaldi communicated a note on fossils from the dolomitic limestone of Monte Chaberton, studied by M. Michelotti.—M. Cossa described the periclasiferous predazzite of Monte Somma.—M. Canizzaro reported on a memoir by MM. de Negri on the purples of the ancients; also on a memoir by M. Paterno, on usnic acid and on two new principles accompanying it in *zeora sordida*; also on one by M. Selmi, on toxicological chemical studies relating to atropine and its detection.—M. Struver communicated a memoir on the minerals of Lazio.

BOOKS RECEIVED

COLONIAL AND FOREIGN.—Verhandlungen der Naturhistorischen Gesellschaft für Natur-und Heilkunde, 2 parts (Bonn, Cohen and Sohn).—La Transfusione del Sangue, pel Dott Malachia de-Christoforis (Milan).—The Fungus Disease of India: T. R. Lewis, M.B., and D. D. Cunningham, M.B. (Calcutta).—The Soil and its Relation to Disease: Same Authors (Calcutta).—Kurze's Chemisches Handwörterbuch: Dr. O Dammer (Berlin, R. Oppenheim).—Check List of North American Ferns: J. Robinson (Salem, Mass.).—Freshwater Shell Mounds of the St. John's River (Florida, Jeffries Wyman, Peabody Academy).—Nephrit und Jadeet: Heinrich Fischer (E. Koch, Stuttgart).—Adolf Stieler's Hand-Atlas (Gotha, Justus Perthes).—List of Hemiptera of the Mississippi: P. R. Uhler.—Algebra for Beginners: Prof. James Loudon (Toronto, Copp, Clark and Co.)—Le Positivisme: André P. Gernier Baillière.—Annual Report of the Smithsonian Institution (Washington, U.S.A.)—The Vertebrata of the Cretaceous Formations of the West: E. D. Cope (Washington, U.S.A.—Daily Bulletins of Weather Reports for March 1873 (Washington, U.S.A.)

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