THURSDAY, DECEMBER 4, 1890.

THE COUNTY COUNCILS AND TECHNICAL EDUCATION.

A CONFERENCE will be held to-morrow under the auspices of the National Association for the Promotion of Technical and Secondary Education, which may prove of the greatest importance to the future development of education in this country. The Marquis of Hartington will be in the chair, and representatives of County and County Borough Councils and other Local Authorities working the provisions of the Technical Instruction Act have been invited to meet the Executive Committee of the Association and delegates from its Branches throughout the country, to confer on the best means of utilizing the new fund placed at the disposal of County Councils under the Local Taxation Act of this year for the purpose of education.

It will be remembered that, by the accident of the abandonment of the licensing clauses of the Local Taxation Bill, a sum of about £743,000, the residue of the proceeds of the new beer and spirits duty after defraying the charges for police superannuation, has been allotted to County Councils in England and Wales, with permission to apply the fund to technical education, or, in the case of Wales, to intermediate education under the Welsh Intermediate Education Act. The Scotch share, amounting to another £50,000, may be used for the purpose of the Scotch Technical Schools Act.

A number of reflections are suggested by this unexpected appropriation. Wales is ahead of England and Scotland in the matter, because a more complete Act is in force in the Principality. The Welsh may use the fund to organize their secondary education under public control, while in England such organization, though, as has been pointed out,[1] not impossible, has to be carried out on a far more restricted scale and under more hampering conditions. This is not because the English share of the fund is inadequate, but merely because when the critical moment for the appropriation came it found Wales provided with the necessary machinery, in the shape of the Intermediate Education Act which is unfortunately not yet in force in the rest of the Kingdom. It was too late in the session to build up the requisite machinery for England, so we are compelled to wait.

Had it not been for the passage of the Technical Instruction Act of 1889, which, as will be remembered, was fiercely contested at the time by many of the more extreme educational partisans, there would have been no machinery at all in England for the utilization of the new fund for education, and the whole would doubtless have been allotted to other purposes, such as reduction of rates. As it is, the fund can be administered under the provisions of that Act, incomplete as it is, until it is duly amended by the Bill of which Sir Henry Roscoe has already given notice, and by the extension to England, under a somewhat modified form, of the Welsh Intermediate Education Act. Thus, had it not been for the previous passage of a confessedly imperfect measure, the greatest chance yet offered for the organization of secondary education would have been missed altogether. We hope this fact will be remembered when heated advocates of one line or other of educational policy are again tempted to forget that "half a loaf is better than no bread." Get public supervision of higher education recognized in an Act of Parliament, and all the rest will surely follow. The Act may be imperfect; parts of it may be bad; but he is a pedant who refuses it on that ground. The Act will be amended as necessity arises; new powers of control will be acquired when any power of control has been once conceded. The new Act gives increased facilities for the organization of education to various parts of the Kingdom in precise proportion to the powers already possessed. "To him that hath shall be given," in education as in other matters.

So much for the Act. The next question that arises is its administration by the local authorities, who, it is to be observed, have power to devote all or any part of the fund to education, and of course to use the rest for the relief of rates. It must be admitted that it is a great temptation, to some County Councils at least, to take advantage of their undoubted right to use the fund otherwise than Parliament intended; but it is a temptation which should be resisted if only for the reason that the renewal of the grant next year may largely depend on the use which can be shown to have been made of it by local authorities. It would be suicidal policy, even for the strictest friend of the ratepayer, to clutch the money this year for the relief of rates, and thus to lose permanently a grant which will be sorely needed before long in order to meet the new charges which, as Mr. Goschen hinted, are likely to be put upon local authorities with respect to agricultural and intermediate education.

So far as the matter has yet come before County Councils, there is no cause for discouragement. At least nine counties have already determined to use the whole or part of the money for technical education; others have appointed committees to inquire into the whole question. We hope that the result of the Conference which is announced will be to decide many wavering Councils to take the same course.

There will be plenty for the Conference to discuss. The work of organizing technical education is new to County Councillors, and many of them are still quite in the dark on many points. Moreover, practical difficulties have arisen in some districts in the working of the Act, on which it will be useful to compare notes. We have received a copy of the provisional agenda, by which we see that among other things the meeting will discuss the nature of the preliminary proceedings which should be taken by County Councils which are desirous of assisting education out of the new fund. This is an important question. Should the Councils simply pass a resolution inviting applications for assistance, and then judge among the various qualified institutions which send in claims? Or should they institute an inquiry into the state of education in their districts, and on the results of that inquiry formulate a comprehensive scheme? Should the money be devoted chiefly to the assistance and development of existing institutions? Or should the Councils initiate schools of their own under the general powers conferred by the Act? On matters like this many

[1] See "Suggestions to County Councils" (National Association for the Promotion of Technical and Secondary Education, 14 Dean's Yard).

Councils are doubtless waiting for a lead such as they may obtain at the Conference. Meanwhile we may commend to their attention the carefully drawn report just presented to the Shropshire County Council by the Technical Instruction Committee of that Council, as a model in many ways of what such a report should be.

The Conference will also discuss the modes in which the fund may be divided among the non-county boroughs and sanitary districts within the county. This again, is a debatable question. Should the basis of the allotment be rateable value, or population, or existing local effort? How far should the convenience and needs of the county as a whole be exclusively consulted in the scheme, independently of the wishes or claims of particular localities? Clearly the Act gives no legal claim to a non-county borough on any particular proportion of the money, though the County Council may, if it please, act through these minor authorities in the distribution. The question is eminently one on which light will be thrown by a friendly conference.

The third point for discussion is the amendment of the Technical Instruction Act. Certain difficulties have arisen in the administration of the Act, which may be removed without raising controversies. For example, many Councils wish to aid agricultural education by founding or assisting agricultural schools in county boroughs in their district. But this they may not do, for the county boroughs are outside their jurisdiction; while the county boroughs themselves are not likely to do the work, since it will, as a rule, be useless to their own inhabitants. Other points, such as the question of the provision of scholarships, especially to schools outside the counties, have been called in question, and need settlement.

The fourth subject on the agenda is the most important of all; viz. the best mode of assisting intermediate and agricultural education. It is hardly to be expected that the members of the Conference will succeed in evolving a complete plan; but they will do a great service if they demand, with no uncertain voice, the extension to England of a measure based on that which is working so admirably in Wales. Meanwhile, the question of the best mode of assisting intermediate education depends very greatly on the chance of the future renewal of the grant. We look to Lord Hartington to give an assurance that there is no possibility of the diversion of any part of the new fund by the Government to other purposes than technical and secondary education.

DREYER'S LIFE OF TYCHO BRAHE.

Tycho Brahe: a Picture of Scientific Life and Work in the Sixteenth Century. By J. L. E. Dreyer, Ph.D., F.R.A.S. (Edinburgh: A. and C. Black, 1890.)

THE time had evidently come for the publication of an adequate account of the life and work of the great Danish astronomer. Popular presentments of the first, and partial studies relating to the second, there have been, it is true, in more languages than one; but Gassendi's was, until now, the only scientific biography of Tycho Brahe; and numerous documents, inedited and inaccessible in 1654, have since been brought to light, clearing up much that was obscure, supplementing meagre with full information, and thus rendering possible the virtually complete narrative now given to the public by Dr. Dreyer. That unstinted pains were bestowed upon his task is obvious from the manner of its accomplishment; and he was equipped for it with a combination, desirable rather than usual, of linguistic, historical, and scientific attainments. Indeed, at times, superabundant information is afforded; the wealth of details tends to congestion; the essential does not decisively enough maintain its supremacy over the superfluous. The book is none the less a most valuable contribution to the history of mediæval astronomy; it is creditably exempt from slips and inaccuracies of memory, pen, or print; abounds with bibliographical knowledge and indications; and—not its least merit—is furnished with a highly serviceable index. It portrays, moreover, with perfect candour, yet full comprehension and sympathy, a vigorous and picturesque individuality.

Judgment in forming, and persistence in holding to, a large and definite purpose, rather than surpassing intellectual ability, caused Tycho Brahe to tower above his scientific contemporaries. Wittich—to say nothing of Viète and Harriot—was his superior in mathematics; he could lay no claim to the inventive ingenuity of Joost Bürgi; his system of the world was less plausible than that of the uncouth Reymers. But he saw clearly what was indispensable to the progress of astronomy, and by the undaunted energy of his efforts to supply it, raised himself to the high level of his great undertaking.

Until observations were begun at Uraniborg, the astronomy of the ancients, as Dr. Dreyer says with truth, reigned practically undisturbed. A speculative impulse of incalculable importance had indeed been given by the promulgation of the Copernican doctrine; but

"No advance had been made in the knowledge of the positions of the fixed stars, those stations in the sky by means of which the motions of the planets had to be followed; the value of almost every astronomical quantity had to be borrowed from Ptolemy, if we except a few which had been redetermined by the Arabs. No advance had been made in the knowledge of the moon's motion, so important for navigation, nor in the knowledge of the nature of the planetary orbits, the uniform circular motion being still thought not only the most perfect, but also the only possible one for the planets to pursue. Whether people believed the planets to move round the earth or round the sun, the complicated machinery of the ancients had to be employed in computing their motions, and, crude as the instruments in use were, they were more than sufficient to show that the best planetary tables could not foretell the positions of the planets with anything like the desirable accuracy.

"No astronomer had yet made up his mind to take nothing for granted on the authority of the ancients, but to determine everything himself. Nobody had perceived that the answers to the many questions which were perplexing astronomers could only be given by the heavens, but that the answers would be forthcoming only if the heavens were properly interrogated by means of improved instruments capable of determining every astronomical quantity anew by systematic observations. The necessity of doing this was at an early age perceived by Tycho Brahe. . . . By his labours he supplied a sure foundation for modern astronomy, and gave his great successor, Kepler, the means of completing the work commenced by Copernicus" ("Tycho Brahe," p. 9).

On August 21, 1560, the young Scanian noble, then a thirteen-year old student at the University of Copenhagen, watched the moon, punctual to prediction, obscure a few digits of the sun's face, and was impressed with it "as something divine that men could know the motions of the stars so accurately that they could long before foretell their places and relative positions." Thenceforward all other pursuits were with him secondary to that of training himself for an astronomer. "Was ein Haken werden will, krümmt sich bei Zeiten." But the career was regarded as compromising to the dignity of his birth, and his uncle sent him to Leipzig in charge of a tutor—the excellent Anders Vedel—whose chief business it was to interpose between him and the heavens. In vain; while Vedel was asleep, Tycho studied his Almegist, learned the constellations from a little globe the size of his fist, and, on the occasion of an important conjunction of Jupiter and Saturn, made his first recorded observation (August 17, 1564) with no other instrument than an ordinary pair of compasses. In the following year he procured a "radius," or "cross-staff," subdivided by the newly invented method of transversals, and gave, in his table of corrections to the results obtained with it, "the first indication of that eminently practical talent which was, in the course of years, to guide the art of observing into the paths in which modern observers have followed" (Dreyer, *l.c.*, p. 20). By this time, too, the intuition had come to him that the planets themselves, consulted with prolonged and patient vigilance, could alone impart trustworthy intelligence as to the system of their motions; and thus, in the mind of a boy of seventeen, the "restoration" of the prehistoric science *par excellence* was conceived and determined upon.

Other interests, however, supervened; and chemical, or, rather, alchemistic, researches might have continued to absorb the attention of one capable of better things, had not his vocation to astronomy been renewed under sanction of the portent (as it were) of the stellar outburst in Cassiopeia. Tycho hesitated no longer, but waited the propitious moment, and sought the appropriate scene for the commencement of his life's labours. He had fixed upon Basle; but the King of Denmark, Frederick II., anticipated his removal thither by the offer of an island-domain in the Sound, together with funds for the erection upon it of suitable buildings.

Their nature and plan are illustrated, both graphically and descriptively, in the work before us. Suffice it here to say that their cost was commensurate with their splendour, and could only have been defrayed by Royal munificence. Yet, after its withdrawal by Christian IV., private means might have sufficed to maintain what they were inadequate to create; for the "Phœnix of astronomers" (as Kepler designated him) was still a comparatively rich man when, rather in disgust than from necessity, he forsook Uraniborg in April 1597. Nor was he free from blame in the transactions which led to his purely voluntary exile. His government of Hveen was nearly absolute, and somewhat arbitrary; his dealings with his tenants, there and elsewhere, were often harsh and tyrannical; he could scarcely be brought, by repeated Royal admonitions, to comply with the conditions of the various grants made to him; and his arrogant bearing made enemies who did not fail to accentuate his shortcomings. A tradition more honourable to him ascribes, it is true, a share in bringing about his disgrace to the King's physician, Peder Sörensen, whose jealousy is said to have been excited by Tycho's unpaid practice of medicine; but Dr. Dreyer saddles the chief responsibility upon the Chancellor Friis, interested, possibly, both through motives of public economy and of private hostility, in the fall of a costly and unsubmissive subject of the Crown. In short, Sir Roger de Coverley's cautious dictum, that "much might be said on both sides," may serve to convey the equitable judgment of posterity as to affairs at Hveen and their lamentable upshot.

Its worst result was in the waste of valuable time. Four years were virtually curtailed by it from the great astronomer's effective career. Yet one compensatory circumstance attended the disaster. Tycho's presence attracted Kepler to Prague; and their connection, formed just at the right moment for progress, put the younger man in possession of a store of facts, from the co-ordination of which he was enabled to deduce the laws constituting the fit epilogue to the work of Copernicus, the indispensable preface to that of Newton.

The results of Tycho's activity during upwards of twenty years at Uraniborg, were summed up by himself as follows:—An improved solar theory; the detection of the third inequality, or *variation*, in the moon's motion, as well as of fluctuations both in the inclination of the lunar orbit, and in the shifting of the nodes; the accurate ascertainment of the places of one thousand fixed stars; the dismissal from astronomy of the supposed "trepidation" of the equinoxes; the collection of a vast mass of materials for the revision of planetary tables; the supply of ample proof that comets are trans-lunar bodies (Dreyer, *l.c.*, p. 262). He might have added the discovery of the lunar annual equation (almost simultaneously perceived necessary by Kepler, as Dr. Dreyer points out); the employment of systematic corrections for refraction; and observations, continuous and exact, of the "new star" of 1572.

Theory was not Tycho's strong point. He attached much more importance than it deserved to his system of the world, which he vainly hoped would outlive himself; nor was he endowed with a penetrating insight into the significance of the phenomena observed by him with such marvellous skill. This may be estimated from the probable errors of his positions for nine standard stars, amounting, as compared with the best modern results, to $\pm 24''$ in right ascension, $\pm 26''$ in declination (Dreyer, *l.c.*, p. 351). His absolute right ascensions were derived from the longitude of the sun through Venus as an intermediate link; his declinations were directly measured. When it is remembered that he was destitute of any kind of optical aid, that his graduated instruments must have been, judged by modern criteria, intolerably defective, that he knew nothing of the effects of aberration and nutation, and commanded only a rough value for precession, the degree of accuracy thus attained must excite our wondering admiration. It should be added that the utter untrustworthiness of his clocks precluded him from the determination of relative places by the easy method of transits.

The frontispiece of the book under review is taken from a portrait of Tycho Brahe painted at Rostock in 1597, and reproduced some few years since in these pages (NATURE, vol. xv. p. 406). Purchased by the Earl of Crawford in 1881, it formed part of his generous gift to the Royal Observatory, Edinburgh. The illustrations in the body of the work, consisting of plans and elevations of Uraniborg, portrayals of the instruments, later residences, and tomb, of its whilom inmate, are both interesting and well executed. The last represents the figure carved in red marble on the monument to Tycho Brahe in the Teynkirche at Prague. Its martial aspect corresponds with the feudal pomp amid which his remains were borne to their last resting-place. Not recumbent, though sleeping, he stands erect, clad in full armour, a plumed helmet on one side, an emblazoned shield on the other, a sword grasped in his left hand. Nothing recalls the genuine quality of his life except the globe upon which his right hand rests, and the inscription from his demolished observatory, "Nec fasces nec opes, sola artis sceptra perennant." A. M. CLERKE.

TWO NEW PSYCHOLOGIES.

A New Psychology: an Aim at Universal Science. By the Rev. George Jamieson, D.D., &c. (Edinburgh: Andrew Elliott, 1890.)

Hand-book of Psychology: Senses and Intellect. By Prof. J. M. Baldwin, M.A., Ph.D. (London: Macmillan and Co., 1890.)

WE are quite ready to allow Dr. Jamieson that, as he claims in his preface, his work is strictly original. We do not think, however, that he need fear that, because it emanates from a clergyman, it will be any the less attentively considered by any competent persons. The reasonings and speculations, in another branch of science, of Dean Conybeare, of Prof. Sedgwick, of Dr. Bonney, or of Mr. Osmond Fisher, have, so far as we can judge, in no wise suffered from this cause. And if there were any value in the reasonings and speculations of Dr. Jamieson, in the realm of psychology and philosophy, they would be as readily and thankfully accepted as those of Bishop Berkeley or Dr. Mansell. But, in truth, however well grounded in theology, Dr. Jamieson's little book gives abundant evidence that he is not very thoroughly grounded in more mundane branches of science.

It is impossible to summarize briefly this "new psychology." We therefore content ourselves with giving two or three extracts which we consider representative, and must then leave such of our readers as aspire to fuller knowledge to worry through the book for themselves.

"It is of enormous moment," we are told, "to understand what it is that really constitutes the soul, as well as to apprehend under what circumstances the soul is constituted. We know of no other substance, as absolute and eternal, than that of Ether. It is absolute in two important respects, *first*, as positive and conditioned, and *second*, as negative and unconditioned."

"If it holds good, as a fact throughout all nature, that material structures have their conditions represented by an immediate encompassment of their delineation in Ether, and if it holds equally good, that in the animal frame generally, and especially in man as the highest exemplification, there is a nerve system in the body known as the 'sympathetic,' and carrying its affections, *i.e.* conducting the states of the living body to one single pole or axis, as the terminal of concentration,—as the pivot whereon the assembled conditions of animal states are manifested, what follows in the first place, as the necessary result of this concentration? Why, surely this, that precise analogy with all that we know, as express revelations in our experience must, in this particular case, be the actual outcome of an Etherial Image or representation of this complex animal self-hood."

"The very fact of our discovery that the pure Etherial Medium, as spirit-substance, is ever present as the fundamental mainspring of operation, in the linking together of conditions in the material world, opens the door of apprehension for us to comprehend the inter-workings of Brain, in producing the phenomena of Mind, as manifested in the Soul or Ego of each personality."

A very different book is the first instalment of Prof. Baldwin's "Hand-book of Psychology," of which a second and revised edition is before us. Well arranged, carefully thought out, clearly and tersely written, it will be welcomed in this country as it has been welcomed in America. That it views psychology from a standpoint somewhat different from that which Mr. Sully takes up in his "Outlines" will render it none the less acceptable to English students.

An interesting and important section is devoted to "consciousness and the unconscious," and Prof. Baldwin brings out very clearly the difficulties that attend the view that mental phenomena may be unconscious. He notes that it is often the case that an association between states of mind is accomplished without conscious links of connection, and that consequently the links of connection must be unconscious. He thinks it unsatisfactory, however, to say that the unconscious links are states of mind. "The missing links in broken chains of associations," he contends, "may be supplied from dynamic connections in the cerebral substance." But there are many people who think that it is as inconvenient as it is unphilosophical to dodge backwards and forwards from the brain to the mind. Either mental states are of a different phenomenal order from physical states or they are not. If they are not, let us at once say that just as the lachrymal gland produces tears so does the brain produce consciousness. If they are, then let us, so far as is possible, avoid hopping backwards and forwards from one to the other. The fact is, this question cannot be discussed without touching on the philosophical basis of psychology. Our own opinion is that we should be aided here by that most reprehensible procedure—the coinage of a new term. Believing, as we do, that mind never has been, and never could be, evolved from matter, the question arises, From what, then, have mental states (psychoses) been evolved? If a similar question be asked with regard to molecular vibrations in the brain (neuroses), the answer is not difficult. Complex neuroses have been evolved from less complex neuroses; these from simple neuroses; these again from organic modes of motion which can no longer be called neuroses at all; and these perchance, once more, from modes of motion which can no longer be called organic. But in the case of psychoses, from what have they been evolved? Complex psychoses have been evolved from less complex psychoses; these from simple psychoses; and these, again, from—what? From other and simpler modes of—something which answers on the subjective side to the organic modes of motion. For this we might use some such term as *metakinesis*. Neuroses are kinetic manifestations; states of consciousness are metakinetic manifestations. But, as neuroses are evolved from kinetic states (kineses) of a lower order, so have psychoses been evolved from metakinetic states (metakineses) of a lower order. According to this view, every mode of organic or neural kinesis has its concomitant mode of metakinesis, and, when the kinetic manifestations assume the form and intensity of neuroses in certain parts of the human brain, the metakinetic manifestations assume the form of human consciousness. Instead, therefore, of jumping across, with Prof. Baldwin, from psychical to physiological states, we would suggest that, from the point of view of psychology, the intermediate states are metakineses, which do not reach a sufficient intensity or complexity to appear in consciousness as states of mind. They are not unconscious states of consciousness, but they belong to the same phenomenal order as consciousness.

In his interesting discussion of the perception of space, Prof. Baldwin says:—

"Two general theories of space perception are held by psychologists, and under them may be classified the various attempts made to explain the origin of this idea. These are the *nativist* and the *empirical* theories. Empiricists hold that presentation of extension (and time) are derived through experience from elements which have not the spacial (and temporal) form. On the other hand, nativists maintain that space and time presentations cannot take their origin in data of consciousness which are simply intensive; consequently that these presentations are primitive data of knowledge, native, innate. In other words, the empiricist asserts the reducibility of presentations of space and time, and the nativist asserts their irreducibility."

Now here it seems to us that, if the empiricist claims to reduce the perception of space to sense-impressions, he is manifestly in error; and that, if the nativist bases his contention on the fact that there are over and above sense-impressions, impressions of relation, he is on firm ground. But it is questionable whether he can carry his claim much further.

Prof. Baldwin has a short but important chapter on illusions. We are unable entirely to agree with him in his treatment of the subject. Speaking of illusion proper, he says:—

"At the outset we find ourselves face to face with a whole class of experiences, in which one mental state is *taken for another*. There are, really, two images involved —one, the rightful image, the presentation, as ordinarily aroused by the stimulus experienced, say the sound of the clock; the other, the image of something different, formed within the domain of the same sense-quality, and usually prominent in consciousness before the time of the illusion, as the alarm of fire, into which the striking of the clock is interpreted."

This does not seem to us altogether satisfactory. Prof. Baldwin, at an earlier stage of his thesis, well brings out the fact that a percept is a mental construction formed at the bidding of a stimulus or sense-impression. Now, an illusion is simply a false and erroneous mental construction. If, to take another of Prof. Baldwin's examples, a man in the half light takes a tree for an Indian, or if a nervous old lady takes a grey donkey for a ghost, at the time of presentation the tree image and the donkey image have no existence. There is one image in each case—that involved in the false and erroneous mental construction. The difference between a true percept and an illusive percept is that the one is confirmable by the process of verification on further examination, and the other is not.

We have left ourselves no space to consider Mr. Baldwin's treatment of reason.

"The rational function," he says, "is contrasted with the apperceptive function in the absence of the element of process, which constitutes the essential nature of the latter. Apperception is a process, through which the material of acquisition passes in preparation for the higher uses of mind. Reason, on the contrary, is not a process, as the more special term, intuition, given to it implies. It conditions and underlies all mental processes. It is the nature of mind itself as it reveals itself in consciousness."

On this somewhat might be said. But we have already said enough to show that we regard the work as a valuable and stimulating hand-book of psychology, so far as the subject is treated in this instalment. C. LL. M.

THE MYOLOGY OF THE RAVEN.

The Myology of the Raven (*Corvus corax sinuatus*): *A Guide to the Study of the Muscular System in Birds.* By R. W. Shufeldt. (London: Macmillan and Co., 1890.)

WITH the exception of the brief descriptions in several of our zootomical text-books of certain of the muscles in birds, there has been up to the present time no special manual treating of this subject in detail. The student of avian myology has therefore had some difficulty in obtaining the requisite directions for his work, more especially if the advantages of a scientific library were not within his reach. Such works as those of Gadow in Bronn's "Thierreich," and of Fürbringer in his monumental "Untersuchungen zur Morphologie und Systematik der Vögel," besides being written in German, are not easily obtainable by the greater number of workers. The present book, from the fertile pen of Dr. Shufeldt, fills up this important gap in a highly satisfactory manner.

At first sight, it might appear an objection that Dr. Shufeldt has chosen such a rare bird as the raven as the subject of his work. But as other species of *Corvidæ* are so easily obtainable almost everywhere, it is perhaps even an advantage that the student in using the book cannot follow out every detail slavishly: by making use of some allied form, which agrees in the main, but differs in detail, from the Raven, he will learn more, and at the same time gain more self-reliance.

Though following, as regards nomenclature, the authority of Owen, Carus, A. Milne-Edwards, Huxley, Garrod, Forbes, Selenka, Coues, Fürbringer, Gadow, and others, the author has in doubtful cases introduced names of his own making, and has quoted very freely from Gadow (Bronn's "Thierreich"), whose synonymy is given fully in footnotes throughout the work. These notes indicate very plainly that we have as yet by no means arrived at a satisfactory knowledge of the muscular system in the Vertebrata, and that our existing nomenclature is far from perfect. It is to be hoped that the present work will aid in laying the foundations of a more satisfactory system, and will help to point out the gaps in our knowledge as regards the muscles in many important types of birds. The author does not entirely confine himself to a description of the raven: other types are referred to in special cases when certain muscles are not represented in the latter; and comparisons are given in many cases with other forms—mammals as well as birds.[1]

Though acknowledging the importance of the question of nerve-supply in helping to determine the homologies of muscles, the author by no means considers this to be an infallible guide; for the representative of the same muscle in various vertebrates is not always supplied by the same nerve. He also quotes some remarks of his own from a previous paper (referred to in the text as No. 124 in the Bibliography, instead of No. 121), with regard to the value of muscles in classification, which are of importance in helping to relegate the muscular system of birds to its proper subordinate position in taxonomy. While fully recognizing the value of Garrod's work in this field, it must be agreed that his myological formulæ only represent one set of characters, which must be taken in conjunction with all others if they are to be of any value in classification.

The various groups of muscles are treated in order, beginning with the dermal system, and a chapter is devoted to each main set. In each group, directions are given how to proceed with the dissection. The figures, of which there are 76, are on the whole excellent, and with few exceptions are original: they represent the different parts of the skeleton, showing the origins and insertions of the muscles, as well as dissections of the muscles themselves. A copious bibliography and index are given at the end of the book.

It is almost impossible to comment on the details of the work without having first made use of it practically. But we can congratulate Dr. Shufeldt on the production of an original and well-arranged text-book, the result of much patient labour in collecting and dissecting, and careful thought in arrangement. The volume will appeal especially to ornithologists, as well as to students of comparative anatomy.

OUR BOOK SHELF.

Chemical Arithmetic. Part I. By W. Dittmar, LL.D., F.R.SS. Lond. and Edin. (Glasgow: William Hodge and Co., 1890.)

THIS volume is not a mere collection of tables extracted from the numerous books which deal with this subject, but is a good and trustworthy piece of work, with no lack of originality about it, and contains the mathematical auxiliaries and both chemical and physical constants which a chemist needs "in the ordinary routine of his laboratory work." Some of the tables contain the results of the author's work, and others have received slight alterations, been put in more convenient forms, and brought up to date.

Indexed tables of three-, four-, and five-place logarithms are given, and following these will be found formula values, F, of a number of substances and radicals and their logarithms, analytical factors and their logarithms, gas volumetric determinations, metric and British systems of units, &c. Near the end there is represented in diagrammatic form a double thermometer scale, with Fahrenheit degrees on one side and Centigrade on the other, ranging from $-148°$ F. to $+392°$ F., by which a result in degrees Fahrenheit can be directly stated in terms of the Centigrade, or *vice versâ*. Throughout the book the author seems to have taken the utmost pains to eliminate all errors that are likely to be made in a work of this sort, and by this means he has placed before the scientific as well as before the technical chemist a useful and trustworthy reference book.

Dictionary of the Language of the Micmac Indians. By Rev. Silas Tertius Rand, D.D., LL.D. (Halifax, N.S.: Nova Scotia Printing Company, 1888.)

THE Micmac Indians are an aboriginal tribe of the Algonquin family, residing in Nova Scotia, New Brunswick, Prince Edward Island, Cape Breton, and Newfoundland. The late Dr. Rand, the compiler of the present work, laboured among them as a missionary for more than forty years. He was a man of great learning and ability, and soon not only mastered the language of the Micmacs, but devoted himself to the task of reducing it to writing. He also translated into it the New Testament and portions of the Old Testament, and claimed to have collected and arranged in alphabetical order no fewer than 40,000 Micmac words. His dictionary consists of a Micmac-English and an English-Micmac part. Greatly to their credit, the Dominion Government paid for the manuscript of both parts, and now they have issued the English-Micmac section of the work. The other section must necessarily be of more scientific interest, but this volume may be of greater practical value. Even from the scientific standpoint, it cannot fail to be of service to philologists. Most students will probably be surprised to find how highly developed a language the Micmac is. It was strongly admired by Dr. Rand, who went so far as to say that in various important particulars it would not suffer from "a comparison with any of the most learned and polished languages of the world."

Elementary Manual of Magnetism and Electricity. By Andrew Jamieson, M.Inst.C.E. (London: Charles Griffin and Co., 1890.)

THIS work is arranged in the form of a series of lectures, and covers the ground laid down in the syllabus for the elementary stage of the Science and Art Department examinations. The subject is divided into three distinct parts. Part I. treats of magnetism; Part II. of electromagnetism and current electricity, under the general heading voltaic electricity; while Part III. deals with frictional electricity. At the conclusion of each lecture a number of examples is given, and in many cases a test question, with the answer. By no means less important are the appendices to each part, in which students will find all the necessary information for making experimental apparatus and for conducting experiments with them. Throughout the book the author has made many references to the practical applications of these experimental facts, such as telegraphy, telephony, electric lighting, &c., and has given some very good illustrations and diagrams, in which the various points for which they were intended have been brought out effectively.

In the present manual, only an elementary knowledge of arithmetic is required in order to follow the expositions; but in the advanced text-book, which the author informs us is now in preparation, and to which the present volume will form an introduction, an elementary knowledge of mathematics will be assumed.

[1] Frequent references are made to Owen's description of the muscles of Apteryx. Quite recently T. J. Parker has shown that some eleven or twelve muscles are present in the vestigial wing of this form in addition to those described by Owen.

The Stereoscopic Manual. By W. I. Chadwick. (London: John Heywood, 1890.)

IN this little manual the author takes up the case of the once popular instrument the stereoscope, and shows how by means of recent improvements very good results may be obtained. The first two chapters are devoted to the subject of binocular vision and the theory of the stereoscope; then follow some practical points concerning the production of stereoscopic photographs. To enable the stereoscopic camera to be used as an ordinary camera, the author has devised a spring roller and curtain division that can be removed in an instant when single views are required, and replaced as quickly for stereoscopic work again. In the remaining part of the book are short chapters on the best lenses to use, transposing and mounting of slides, transparencies on glass, and combination printing. Throughout the manual the author has given enough information as regards the mode of procedure to enable one to obtain a finished picture, so that for amateurs and others who are thinking of taking up this branch of photography the book should prove a most useful and efficient guide.

Astronomy: Sun, Moon, and Stars, &c. By William Durham, F.R.S.E. (Edinburgh: A. and C. Black, 1890.)

IN this little work we have a clear and interesting account of the more important facts connected with the science of astronomy. It treats of the sun and moon, the earth, stars, nebulæ, planets, astronomical speculations—such as those relating to the formation of the heavenly bodies—concluding with chapters on the tides, light and spectrum analysis. Perhaps an improvement might be made in the order in which the chapters are arranged. The first two deal with some bodies in the solar system—namely, the sun, moon, and earth; the next one treats of the stars, nebulæ, &c.; while in the fourth we are suddenly brought back to the solar system in connection with the planets.

The information is well up to date, and is presented in simple and plain language. The author gives a very good idea of the present general knowledge on these subjects, without entering into details which would tend to puzzle the reader for whom works of this kind are written.

The Life Story of Our Earth. By N. D'Anvers. (London: George Philip and Son, 1890.)

The Story of Early Man. By N. D'Anvers. (Same Publishers.)

THESE volumes belong to the "Science Ladders" series, which consists of simple reading-books in elementary science for the young. The author has selected his facts with care, and presents them in a plain straightforward style, well adapted for the purpose for which the books have been prepared. There are many illustrations.

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 intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Great Mogul's Diamond and the Koh-i-nur.

IN the review of my edition of "Tavernier's Travels" which was published in NATURE (vol. xli., p. 313), the writer having commented, with approval, on the arguments advanced by me in favour of the hypothesis that the Koh-i-nur can be identified with the Mogul's diamond, proceeds to say:—

"But as regards the identity of the Koh-i-nur and the Mogul diamond, there remains one objection which, as it appears to us, Mr. Ball has hardly adequately disposed of. If Tavernier's figure, as reproduced by Mr. Ball, represents at all faithfully the general form and especially the height of the Mogul diamond, it is difficult to see how a comparatively flat stone like the Koh-i-nur could have been obtained from it without a much greater reduction of its weight than the 82 carats which are all that his data admit of. The lateral dimensions of the two stones accord fairly enough, so that any reduction of Tavernier's figured stone, to bring it down to the required size, could be effected only by diminishing its height; in which case it would hardly correspond to his description of its form as that of an egg cut in two. The question can only be fairly tested by the weighment of a model constructed, as nearly as possible, in accordance with Tavernier's figure, and of such lateral dimensions as to be capable of including the Koh-i-nur. It may be found that such a model, of the specific gravity of the diamond, would be found much to exceed Tavernier's reported weight of the stone, in which case the importance of his figure as an item of evidence would be invalidated."

This very point suggested itself to me also for investigation long ago; but hitherto from want of a suitable model I postponed undertaking it. Recently I have obtained a glass model, which coincides, exactly, with Tavernier's figure; another model, of which an example has been examined by me, is too small in diameter. These models are included in the well-known collections of glass models of famous diamonds; from them the often faulty figures given in works on precious stones have generally been drawn.

On submitting the first-mentioned model to weighment, it proves to represent a diamond not of 279·56 Florentine carats, *i.e.* 268·38 English carats, as it should; but it represents a diamond of 437·9 carats—in other words, it includes an excess of 169·52 carats. It is therefore far too large, and if we consider in what respect it is most likely to be defective it is surely in the height and general mass of the stone. Tavernier was not an artist—he admits as much himself; but he probably measured the diameter of the stone accurately, though his sketch or diagram was otherwise exaggerated. Out of a flatter stone with the same diameter, the Koh-i-nur might easily have been cleaved, with a loss of about 82 carats (268·38–186·6). And the original might, to some extent, have resembled, as he describes it, an egg cut in two, not across, perhaps, but longitudinally.

It may be asked, Can the true form of the Mogul's diamond be approximately reconstructed? I think it can. By taking an authentic model of the Koh-i-nur, as it was when brought to England, and by replacing symmetrically on the cleaved surfaces and upon the upper portion of the stone an amount of material equivalent altogether to 82 carats, the result would give, I venture to believe, a very close approximation to the form of the Mogul's diamond as it was when seen and described by Tavernier.

Many collateral facts which have come to my knowledge since I wrote on this subject, but which I cannot trespass so far on your space as to detail here, tend to confirm the hypothesis that the Koh-i-nur is identical with the Mogul's diamond—less by certain portions which were demonstrably removed by cleavage, after the original cutting.

It may be added that the models of some of the other famous stones in the collections above referred to also prove to be very inaccurate in their dimensions; none of them, however, are so outrageously exaggerated as that which professes to represent the Mogul's diamond.

Little wonder is it, therefore, that some writers should assert that such a stone no longer exists, for it never did exist in the form and of the size represented by these models, and by the figures prepared from them for works on precious stones.

V. BALL.

Science and Art Museum, Dublin, November 29.

Pectination.

IN looking over a collection of skins early in the present year, I was struck by the recurrence of the pectination of the claw of the third digit in many different orders of birds; such a structure occurring among the owls, nightjars, herons, and gannets. The fact is, of course, well known; but the popular explanation of it—that the serration is useful in enabling the bird to hold its prey (fish, &c.) more securely—cannot be correct: first, because the serration is small in extent, and only present in a single claw; and secondly, because it is at the side, and not on the under surface, of that claw. At the same time, it is too widespread and well-marked a characteristic not to be functional.

The pectination seems to attain its greatest development in the nightjars; and it has been said that these birds utilize it for the purpose of cleaning their mouth-bristles. But the bristles and the pectination do not by any means always occur together, even among the nightjars; while many other birds (*e.g.* barbets) have the former without the serration.

In order to investigate the point, I procured a young common heron (*Ardea cinerea* ♂?) in June last, and have had the bird under more or less close observation from that time until the middle of October. It fed freely for the most part, and continued healthy during the whole of this time. Its food, whether living or dead, and whether taken from water or from the ground, was never touched at all by the feet. The only use to which the serrated claw was put, that I could observe, was that of scratching the cheeks and throat. In this action—which occurred most frequently after a meal—the two other front toes were curved down, so as to leave the middle claw free.

It is, however, the case that other birds, in which the pectination is absent, use the middle claw alone in scratching. Nor would the presence of this peculiarity seem to give any great advantage to its possessor in this connection, owing to its lateral position. I fear, therefore, that I can claim to have done no more than disprove experimentally what hardly needed disproving—the popular theory alluded to above.

Inselstrasse 13, Leipzig, Nov. 25. E. B. TITCHENER.

The Common Sole.

THE correction from Mr. Green, H.M. Inspector of Irish Fisheries, which is published in NATURE of November 20 (p. 56), and which announces that the specimens he at first believed to be the young of the common sole proved to be really the young of *Pleuronectes cynoglossus*, is one more instance of the great importance of specific identification in the investigation of fishery problems. This was a mistake not merely as to the species, but as to the genus of the specimens in question. And yet it is by no means difficult, after a little experience, to distinguish the genus *Solea* at any stage of development from all other genera of *Pleuronectidæ*. It is much more difficult to distinguish from one another the various species of Gadus in their early stages. But in order to ascertain the life-history of the various species of sea-fish used as food, we must trace each with absolute certainty from one environment to another through the successive stages of its existence.

It is natural enough that the young fry of *Pleuronectes cynoglossus* should be found in deep water. The adult of this species has an exceedingly wide bathymetrical range. It is found in abundance on our ordinary trawling grounds in the North Sea at 15 to 30 fathoms, and I have taken it in numbers in the Firth of Clyde. In the same region which Mr. Green investigated, Mr. G. C. Bourne found it at 70 fathoms and at 200 fathoms. The Norwegian North Atlantic Expedition found it at 125 to 150 fathoms off the Lofoten Islands, while on the North American side of the Atlantic it has been taken at 120, 263, 395, 603, and 732 fathoms. On the other hand, *Solea vulgaris* has never been found at a much greater depth than 60 fathoms.

Seeing that I have found young soles immediately after metamorphosis between tide-marks, it would have been extremely surprising if the next stage only occurred beyond 50 fathoms. Most probably the fry of the sole will be found pretty uniformly distributed over the grounds where the adults live. After the stage I have mentioned, they are not found in shallow water. I have some evidence that the above suggestion is true, for specimens from 3 to 6 inches in length have been recorded for us as captured by the large beam-trawl in the North Sea in February and March. These specimens would be a year old: in order to find younger and smaller specimens between June and Christmas a small-meshed trawl would have to be worked on the ordinary trawling grounds. The Marine Biological Association has not at present sufficient funds to give me the means of doing this. I have had to obtain my results by going out in deep-sea trawlers, living with the men, and taking whatever opportunities occurred in the course of the regular fishing operations. J. T. CUNNINGHAM.

Marine Biological Laboratory, Plymouth, Nov. 21.

Weights Proceeding by Powers of 3.

THE following rule gives the allocation of weights of 1, 3, 9, &c., necessary for making up any given weight.

Find the least value of $\frac{1}{2}(3^n - 1)$ which exceeds, or is equal to, the given weight. Add it to the given weight, and express the sum in the ternary scale. The digits will indicate the weights to be used, and the pan in which they are to be put, according to the following scheme:—

Digit 2 indicates the positive pan,
,, 0 ,, ,, negative ,,
,, 1 ,, that the weight is not to be used.

Example.—Let 500 be the given weight to be made up. The number to be added is $\frac{1}{2}(2187 - 1) = 1093$, and the work is as follows:—

		Positive pan.	Negative pan.
500			
1093			
3)1593			
3)531	remainder 0	— ...	1
3)177	,, 0	— ...	3
3)59	,, 0	— ...	9
3)19	,, 2	27 ...	—
3)6	,, 1	— ...	—
3)2	,, 0	— ...	243
0	,, 2	729 ...	—

756 − 256 = 500.

I should be glad to learn whether this rule has been previously published. J. D. EVERETT.

5 Princess Gardens, Belfast, November 18.

Measures of Lunar Radiation.

IN your issue of the 13th inst. (p. 44) you have, under the above heading, a notice of a paper in vol. xxiv. of the Proceedings of the American Academy of Arts and Sciences, by Mr. C. C. Hutchins.

The so-called "new thermograph" is obviously, with very slight differences in constructive details, the apparatus used here for some years past. We have employed the thermo-couple in place of the ordinary thermopile in our determinations of lunar radiant heat from the year 1870, and the condensing mirror of focal length about equal to its aperture from the commencement of our heat-work in 1869 (Proc. Roy. Soc., No. 112, 1869, and Nos. 122 and 123, 1870); and about four years ago we replaced the brittle bismuth and antimony alloys by the more tractable metals iron and German silver, for the same reasons which weighed with Mr. Hutchins.

We cannot give an unqualified assent to Mr. Hutchins's conclusions as to absorption of lunar radiant heat in our atmosphere, but I will defer remarks upon this for the present; I may say, however, now, that I have looked over Mr. Hutchins's paper, and he appears to have entirely overlooked Dr. Copeland's extensive and careful work here on the lunar phase curve and the law of absorption in our atmosphere (Phil. Trans., vol. clxiii. p. 587), or he would not have made the sweeping assertion that, "previous to the invention of the bolometer, no instrument existed capable of dealing accurately with so small an amount of heat as the moon affords," a statement which, from what I know of Prof. Langley, I feel confident he would not fully endorse, notwithstanding his success in working with that instrument.

In conclusion, may I congratulate Mr. Hutchins on having taken up with so much apparent manipulative skill and energy so interesting an investigation, and one in which the workers have been so few, and wish him every success? ROSSE.

Birr Castle, Parsonstown, November 29.

The Distances of the Stars.

IT is quite a familiar illustration to represent the distances of the stars in terms of the *light-year*, but I am not aware that it has been noticed that the same figures which express in years the time light occupies in reaching the earth from a star, will also express in miles the distance of the star upon a scale of the radius vector of the terrestrial orbit to the inch. The illustration appears to me useful, as it gives, perhaps, a more distinct idea of the isolation of the solar system in space than can be otherwise obtained, and does not introduce the question of time into the measurement of distance. Thus if the annual parallax of 61

Cygni is assumed to be $0''.434$, which is probably very nearly correct, it will take $7\frac{1}{2}$ (7·464) years for its light to reach the earth, and $7\frac{1}{2}$ (7·499) miles will represent the distance of this star on a scale that gives 1 inch to the distance of the sun from the earth. JOHN I. PLUMMER.

8 Constitution Hill, Ipswich.

Great Waterfalls.

I SHALL be much obliged to any of your readers who can inform me where I can find descriptions of the waterfalls named below:—

Falls of the Rio Grande near Guadalajara, Mexico, referred to by Miss Kingsley in "South by West."

Falls in the Ala-tau Mountains, Central Asia, stated in the "Universal Geography" as consisting of falls of 600, 350, and 350 feet separated by rapids.

Lattin, in Swedish Lapland, stated in the "Universal Geography" as of the height of 400 feet. It is also mentioned in the "Popular Cyclopædia," but I could hear nothing of any waterfall of that name when travelling in Lapland some few years since.

Aguara-y, in Paraguay, also classed amongst the great waterfalls of the world in the table in the "Universal Geography," as of the height of 409 feet.

Falls of the Pykara, India.

Falls of the Ooma Oya and Badulla Oya, Ceylon.

Any particulars of any of these falls will be most useful.

ARTHUR G. GUILLEMARD.

Eltham, Kent, November 22.

AID TO ASTRONOMICAL RESEARCH.

A CIRCULAR was issued last summer announcing the gift by Miss Bruce of 6000 dollars for aiding astronomical research. No restrictions were made upon its expenditure which seemed likely to limit its usefulness, and astronomers of all countries were invited to make application for portions of it, and suggestions as to the best method of using it. Eighty-four replies have been received, and with the advice of the donor the entire sum has been divided so as to aid the following undertakings:—

(3) Prof. W. W. Payne, Director of the Carleton College Observatory. Illustrations of the *Sidereal Messenger*.

(6) Prof. Simon Newcomb, Superintendent of the *American Nautical Almanac*. Discussion of contact observations of Venus during its transits in 1874 and 1882.

(16) Dr. J. Plassmann, Warendorf. For printing observations of meteors and variable stars.

(23) Prof. H. Bruns, Treasurer of the Astronomische Gesellschaft. To the Astronomische Gesellschaft for the preparation of tables according to Gyldén's method for computing the elements of the asteroids.

(27) Prof. J. J. Astrand, Director of the Observatory, Bergen, Norway. Tables for solving Kepler's problem.

(29) Prof. J. C. Adams, Director of the Cambridge Observatory, England. Spectroscope for the 25-inch telescope of the Cambridge Observatory.

(36) Prof. A. Hirsch, Secretary of the International Geodetic Association. To send an expedition to the Sandwich Islands to study the annual variation, if any, in latitude.

(40) Mr. H. H. Turner, Assistant in Greenwich Observatory. Preparing tables for computing star corrections.

(45) Prof. Edward S. Holden, Director of the Lick Observatory. Reduction of meridian observations of Struve stars.

(46) Prof. Lewis Swift, Director of the Warner Observatory. Photographic apparatus for 15-inch telescope.

(54) Prof. Norman Pogson, Director of Madras Observatory. Publication of old observations of variable stars, planets, and asteroids.

(57) Dr. Ludwig Struve, Astronomer at Dorpat Observatory. Reduction of observations of occultations during the lunar eclipse of January 28, 1888, collected by the Pulkowa Observatory.

(60) Dr. David Gill, Director of the Observatory of the Cape of Good Hope. 1. Reduction of heliometer observations of asteroids. 2. Apparatus for engraving star charts of the Southern *Durchmusterung*.

(78) Prof. A. Safarik, Prague. Photometer for measuring variable stars.

(79) Prof. Henry A. Rowland, Johns Hopkins University. Identification of metals in the solar spectrum.

Of the remaining replies many describe wants no less urgent than those named above. Some relate to meteorology or physics rather than to astronomy, some to work already completed, and others were received too late to be included. Two important cases may be specially mentioned. In each of them an appropriation of a part of the sum required would have been made; but in one, in our own country, an active and honoured friend of the science undertakes the whole; and in the other, in France, the generous M. Bischoffsheim, already known as the founder of the great Observatory at Nice, ignoring political boundaries and the comparative selfishness of patriotism, came forward and gave the entire sum required. The same sky overarches us all. It is to be hoped that the above-named, and other foreign institutions, will obtain more important aid from neighbours when these become aware how highly the work of their men of science is appreciated in this country. The replies not enumerated above are confidential, and cannot be mentioned except by the permission of the writers. But they have placed me in possession of important information regarding the present needs of astronomers. In several cases a skilful astronomer is attached to a college which has no money for astronomical investigation. He has planned for years a research in the hope that some day he may be able to carry it out. A few hundred dollars would enable him to do this, and he offers to give his own time, taken from his hours of rest, if only he can carry out his cherished plans.

Such valuable results could be attained by the expenditure of a few thousand dollars, that no opportunity should be missed to secure this end. Fortunately, the number of persons in the United States able and willing to give liberally to aid astronomy is very large. It is hoped that some of them may be inclined to consider the case here presented. The income derived from a gift of one hundred thousand dollars would provide every year for several cases like those named above. A few thousand dollars would provide immediately for the most important of the cases now requiring aid. The results of such a gift would be very far-reaching, and would be attained without delay. Correspondence is invited with those wishing to aid any department of astronomy, either in large or small sums, by direct gift or by bequest.

EDWARD C. PICKERING.

Harvard College Observatory,
Cambridge, Mass., U.S.A., November 11, 1890.

NOTE ON THE DISAPPEARANCE OF THE MOA.[1]

MAJOR MAIR, in an interesting paper on the disappearance of the Moa in vol. xxii. of the Transactions of the New Zealand Institute, makes, on p. 71, the statement that he is a "supporter of the belief

[1] Read before the Philosophical Institute of Canterbury, October 2, 1890, by H. O. Forbes, Director of the Canterbury Museum, Christchurch, New Zealand.

that the Maoris," from the absence of all mention of the bird from their songs and traditions, "never had any personal knowledge of the Moa." Major Mair so intimately knows the history and literature of the Maoris, and their habits and modes of thought, that one—especially one like myself who has had time as yet to acquire only a small amount of experience of New Zealand things—can scarcely hope to contribute any suggestion on the subject of the history of the Moa which has not occurred to this specialist.

Still, the following observations, made last year during a very complete exploration of a recently discovered cave in the property of Mr. Monck, near Sumner, lead me to think that the Maoris must have been personally acquainted with the *Dinornis*. The exploration was conducted under my own direction by two very trustworthy workmen, and the more important finds in it have been described in a paper read before the Institute last session by the President. The cave, it is certainly known, has been closed since before the advent of Europeans to Canterbury, but for how long before it is impossible to determine. The condition of the cave on entry gave all the appearance of having been untouched since the last dwellers in it left it. Its entrance was covered over by a very extensive landslip, which evidently fell during the absence of its frequenters, as no human bones were discovered in it. Quarrying occupations have been carried on amid the material of this landslip for between twenty and thirty years. These operations, on reaching last year the live rock of the hills, disclosed an aperture through which a lad squeezed himself into the cave. On its floor were found implements in wood and in greenstone, half-burned pieces of timber and fire-making apparatus, so lying as to give the doubtless correct impression that its occupiers intended to return. The greenstone objects were beautifully made, some of them; while the implements of wood, such as the canoe bailer, the paddle, and the fragment of a paddle handle, exhibit ornamentation characteristic of the Maoris. On the floor of the cave were found also numerous largish fragments of Moa bones, partly burned and partly broken, scattered round the last fireplace, or lying on the surface of the ground in the inner caves. In the kitchen-midden in front of the cave were found many fish-hooks and barbed spear tips, made of bone from the same birds. On the surface I picked up several bones of more than one individual of a species of swan, which I described to the Institute last year under the name of *Chenopis sumnerensis*. Just below the surface of an untouched part of the midden, I myself picked out pieces of Moa egg-shell, each with its shell membrane perfectly preserved. The question, therefore, stands thus: The Moa egg-shells, being among the refuse of the feasts of the occupants of the cave, who used the carved bailer, are the remains, it is legitimate to argue, of the eggs they used for food. There is no purpose I can think of for which pieces of shells of rotten eggs could be used; for eggs exposed on the ground, or buried under the soil, with their contents, would soon burst or disintegrate into fragments. It may be inferred, consequently, that these eggs were found in a more or less fresh condition, and were brought into the cave for food purposes. If they were sufficiently fresh for food, it is obvious that the birds that laid them could have been then still living, and probably were so; and that the bones from which the frequenters of this cave made their implements were as likely to be obtained directly from birds which they killed or might have killed. It may be suggested that eggs of Moas might have been found sufficiently whole to be used for utensils. The fragments that I found could not have been so used, as demonstrated by the condition of the membrane lining the interior. In the Sumner caves explored by Sir Julius von Haast, and at many Maori encampments, the remains of Moa eggs have been found abundantly in the kitchen-middens, and in such positions among the *débris* of their meals as to suggest that they had been used for food.

The black swan (*Chenopis atrata*), the only undomesticated swan in the country, was introduced into New Zealand from Australia a number of years after the settlement of Canterbury. The bones of the swans found in the Sumner cave were consequently also brought there by the feasters who ate the Moa eggs, and they, too, must have been, therefore, contemporaries of the Moa.

The figure of a dog carved out in wood was also discovered in the cave. It probably formed the termination of the handle of a paddle. A good figure of it may be found in the President's paper to which I have already referred. The Maori dog must, therefore, also have been contemporaneous with the Moa, and with the now, in New Zealand, non-indigenous (if not altogether extinct) swan *Chenopis sumnerensis*.

The fishing family, or families, who fed on the Moa eggs, and who last occupied the Sumner cave, were, as far as the style of their ornamentation and handiwork can decide for us, as much Maoris as those who carved the woodwork of the typical Maori dwellings of the King Country, or executed the ornamentation of which our museums exhibit specimens labelled "Maori"; and they were Maori in contradistinction to an earlier more primitive people who have been named "Moa-hunters," as is testified by their highly-executed and polished greenstone work.

How long ago it is since the Maori and the Moa were living together no evidence has yet been afforded by the Sumner cave explorations. A very great deal still remains to be done in the determination of the extensive osteological and other material obtained. It is being slowly examined; and when this work has been accomplished, some more light may possibly be thrown on the question of which this note forms the subject.

GLACIAL STRIÆ AND MORAINIC GRAVEL IN NORWEGIAN LAPLAND FAR OLDER THAN "THE ICE AGE."

AROUND the inner part of the Varanger Fjord, the mountains are low, and consist chiefly of sandstone and conglomerate, the strata of which lie in a nearly horizontal position. Between the village of Nesseby and the farm, Mortensnes, a mass of unstratified conglomerate or breccia occurs at least 50m. thick. Its component stones, which have been mostly derived from Archæan rocks (gneiss, &c.) are not properly water-worn pebbles, but have only their edges rounded, while flat faces may often be observed among them. A few of them consist of dolomite. On some of these fragments very distinct striæ occur, while similar markings may occasionally be detected on other kinds of material among the included stones. As in recent moraines, it would seem that here also the depth and distinctness of the engraving have had some relation to the relative hardness of the material. Not far from this conglomerate a smaller layer of a similar rock lies in the sandstone. The conglomerate is here very friable, and by its weathering a part of the upper surface of the hard sandstone under it has been laid bare. On this surface some excellent glacial furrows have been preserved. I had the pleasure of laying before a meeting of my geological colleagues on October 27 my specimens and diagrams. They all agreed with me in believing that in this deposit we have evidence of glacier-action dating back to the time of the sandstone and conglomerate of Lapland. The geological age of these strata is not yet settled, as no fossils have been found in them. Dr. Dahll has referred the formation to the Permian period. I think it not improbable that it belongs to the Cambrian or Silurian series, which in other parts of Scandinavia play so large a part in our mountain system.

Further details will be published in the year-book (*Aarbog*) of the Geological Survey of Norway for 1890.

Kristiania, November 29. HANS REUSCH.

NOTES.

THE anniversary meeting of the Royal Society was held on Monday at Burlington House. Sir G. Stokes resigned the presidency in favour of Sir William Thomson. The new Council was elected, and Sir G. Stokes presented the medals, a list of which we have already published. The official report of the proceedings is not yet ready. The annual dinner was held at the Hôtel Métropole in the evening, and, as usual, it was very numerously attended. Many of the speeches were so long that it is impossible to reproduce them here, but it is fitting that Dr. Hopkinson's reply for the medallists should be put on record. He said:—"You have done me great honour in asking me to respond to this toast, but the honour carries its burden with it. I could have wished for your sakes that the duty had been in hands abler for its performance. In matters of science, so far as nationality exists at all, it must be held to depend not upon the accidents of government, but upon community of descent and community of language. If you were to ask Prof. Newcomb, I have no doubt he would tell you that he was of the same nation as Bacon and Newton. We are undoubtedly of that nation, it therefore follows that Prof. Newcomb is our fellow-countryman. But intellectually Newcomb is a descendant of Newton in a peculiar sense. Newton not only originated that greatest of all scientific generalizations, the law of universal gravitation, but followed out in some measure its effects in the perturbation of the bodies of the solar system. Newcomb has attained to the proud position he now holds by advancing the theory of gravitation in its application to the details of the motion of the moon and some other of the planetary bodies. When Prof. Newcomb visited Birmingham some 16 years ago, he did me the honour of being my guest. He had then attained a high reputation. I little thought that on the day on which he should receive the highest honour the Royal Society has to bestow, I should myself receive a Royal Medal. Of Prof. Hertz and his work, I could say much of its connection with our own Maxwell, and of its immediate and enthusiastic appreciation in this country; but Prof. Hertz is here to-day, and can answer for himself far better than I could have answered for him. It is one of the boasts of modern science that she can accomplish by inanimate means many results which it was supposed that Nature had reserved for the laboratory of the living body. The work of Prof. Fischer belongs in part to this class; amongst other work he has produced by synthesis from inorganic sources many definite sugars, and in so doing has greatly extended the territory of chemistry into the domain of physiology. Rather than speak of Mr. Wallace's work, I would set him forth as an example of that chivalrous feeling which one would wish men of science should always exhibit. You all know the story of how Wallace had worked out the theory of natural selection and was ready to publish; of how he learnt that Darwin had worked it out further and worked at it earlier, and how he postponed his own publication till Darwin had time to take the first place. When we read of the noble work which is being done in France and in Germany by Pasteur and by Koch, and others, we feel with much bitterness that we do not live in a free country. The work which Dr. Ferrier has done on the brain proves that we have men here as capable of physiological discovery by experiment as anywhere abroad, and that, if there is fear that we may fall behind, it is because our investigators are unnecessarily hampered in their work. My own case in physics is very analogous to Dr. Ferrier's in physiology—we are both of us professional men, we both of us desire to further the pure science of our subjects on lines suggested by our professional work. I say deliberately that if I had been obliged to obtain the sanction of a Government Department to make experiments or to make them in a licensed place it is very little experimental work I could have done; I should not have been in the proud position of one of your medallists to-day. Neither physics nor physiology can be efficiently advanced by mere observation: the more powerful method of experiment is necessary. If we must have laws restraining our best men, let us at least admit what it costs us. Speaking for myself, looking back, I have been fortunate in my surroundings. My father cultivated in me a taste for science from a time before I can remember, my mother gave me the first systematic instruction of which I have any recollections. If my father gave me my first taste for science, you may be sure that taste was encouraged at Owens College. Mathematics is the most essential weapon of the physicist, and nowhere can mathematics be learned as at Cambridge. I owe to Sir William Thomson the first impulse to experimental work in electricity and magnetism. He has been to me for many years the kindest of friends, always ready to encourage and to help. Looking at the present, I admit that your Council, in awarding me a Royal Medal, have raised me not a little in my own estimation. There is one point of view from which this cannot be regretted, if a man's past work obtains for him honour from the highest and most competent tribunal: surely that should make him feel that it is incumbent upon him to endeavour to do more and better work of the same kind. This, gentlemen, speaking for the other medallists and for myself, is the best thanks we can return you—to endeavour to justify you in the future in giving us these honours to-day."

LETTERS have been received from Mr. J. Graham Kerr, Naturalist to the Pilcomayo Expedition, dated from the s.s. *Bolivia* on that river, in lat. 24° 58′ S., long. 58° 40′ W., on October 4 last. The vessel had got so far up the Pilcomayo with great difficulty, owing to the extreme shallowness of the water, and had stuck exactly in the same spot since June 14. They had almost given up all hope of ever getting out, when on October 4 a relief party of twenty soldiers reached them and brought assistance. Some of the soldiers and Captain Page's son (Captain Page himself having died on August 2, whilst proceeding to Corrientes for medical advice) came back immediately by land, while Mr. Graham Kerr and the remainder of the party proposed to return down stream in the *Bolivia*. If that turned out to be impossible, it had been determined to burn the boat, and return to the La Plata overland. The Pilcomayo Expedition must therefore be considered to be at an end.

THE Scientific Committee of the Royal Horticultural Society have undertaken, under the direction of Dr. D. H. Scott and Dr. Francis Oliver, to investigate the effects of London fogs on cultivated plants. The Royal Society has granted £100 in aid of the experiments.

MANY horticulturists feel that there should be some permanent memorial of the services rendered by the late Mr. Hibberd to horticulture. A meeting, therefore, will be held to take the matter into consideration.

THE authorities of the Natural History Museum have placed in the Central Hall of that institution a small temporary exhibit, consisting of a set of highly magnified daawings of Bacteria. It includes such prominent forms as *Bacillus tuberculosis*, Koch, and the *Bacillus* of fowl cholera, and is the work of Dr. W. Migula.

THE Board of Agriculture proposes the appointment of a widely representative body under whose direction all examinations in dairy work shall in future be held.

The Photographic Society has now secured premises which are likely to be suitable for its work. At the meeting of the Society on November 11, Mr. W. S. Bird gave all necessary information on the subject. The new quarters are "in a fine building of considerable architectural pretension," exactly opposite the centre of the British Museum. They consist of one large room, capable of holding about sixty persons; a smaller room, suitable for a library; a good dark-room; and a smaller one for a similar purpose.

The workshop accommodation of the Physical Laboratory in the University College of Wales, Aberystwith, has recently been considerably extended, and arrangements are being made for a course of instruction in electrical engineering. The recent additions include a Crossley D. high-speed electric lighting engine, Crompton dynamo (shunt and compound-wound), and a Crompton D.D. arc lamp, &c.

The following lectures on scientific subjects will be delivered at the Royal Institution before Easter:—Prof. Dewar, six Christmas lectures to juveniles, on frost and fire; Prof. Victor Horsley, nine lectures on the structure and functions of the nervous system (part i. the spinal cord, and ganglia); Prof. C. Meymott Tidy, three lectures on modern chemistry in relation to sanitation; the Right Hon. Lord Rayleigh, six lectures on the forces of cohesion. The Friday evening meetings will begin on January 23, when a discourse will be given by the Right Hon. Lord Rayleigh on some applications of photography; succeeding discourses will probably be given by Lord Justice Fry, Prof. J. W. Judd, Prof. A. Schuster, Dr. E. E. Klein, Mr. Percy Fitzgerald, Dr. J. A. Fleming, Dr. Felix Semon, Prof. W. E. Ayrton, and other gentlemen.

The second series of lectures given by the Sunday Lecture Society begins on Sunday afternoon, December 7, in St. George's Hall, Langham Place, at 4 p.m., when Sir James Crichton Browne, F.R.S., will lecture on "Brain Stress." Lectures will subsequently be given by Mr. Whitworth Wallis, Mr. Edmund Gosse, Mr. Eric S. Bruce, Dr. Henry Hoole, Sir R. S. Ball, and Prof. G. S. Boulger.

Early on Monday morning the inhabitants of the valley of the Ness, in Inverness-shire, were alarmed by another shock of earthquake. The *Times* says that a slight tremor was felt shortly before midnight, and this was followed about ten minutes past 12 o'clock by a sharp shock, accompanied by a low rumbling noise. People were awakened by the shaking of their houses, but the shock was of short duration, and was not so serious as that experienced a fortnight ago.

The United States National Electric Light Association will hold its thirteenth convention at Providence on February 17, 18, and 19, 1891. The following subjects will be dealt with:—How can the National Electric Light Association best serve central station interests? by C. R. Huntley; distribution of steam from a central station, by F. H. Prentiss; distribution and care of alternating currents, by T. Carpenter Smith; municipal control of electric railroads, by M. W. Mead; the Ferranti system, by C. B. Haskins. *Science* says that the committee has not only secured the promise of these papers, but has named a person to open the discussion on each paper—a plan which can hardly fail to increase the value and interest of the proceedings.

In his message to Congress, presented on Monday, President Harrison said Congress would be asked to provide means for the acceptance of the invitation of Italy to take part in a Conference to consider the adoption of a universal prime meridian. He further suggested the passing of a general law giving the Executive discretionary authority to accept foreign invitations to Conferences having for their objects international reforms in science, sanitation, &c.

There is much complaint among American men of science with respect to the effect of the McKinley Bill on the cost of scientific instruments. The tax on microscopes has been raised to 60 per cent., so that a microscope which can be bought in Germany for 94 dollars costs in the United States 150.40 dollars. "Not content," says the New York *Nation*, "with the attack on the microscope, McKinley increased the duty on mathematical glass instruments from 45 to 60 per cent. *ad valorem*. Everybody knows the importance of glass tubes in laboratory work; well, the brave McKinley increased the tax on them from 40 to 60 per cent. Everyone knows the value of telescopes in astronomy and navigation. McKinley has increased the duty on them from 45 to 60 per cent. His keen protective eye also fell on 'raddle,' a substance used in polishing lenses for microscopes and telescopes, so he put the duty on that at 1.50 dollars per ton. What rate per cent. this is we are unable at this moment to say, but nobody but an ignorant barbarian would have taxed it at all. Everyone knows the value of spy-glasses on shipboard. McKinley has raised the tax on them from 45 to 60 per cent., and to prevent cheap navigation he has raised the tax on compasses from 35 to 45 per cent. Glass disks for optical instruments he has also raised from 45 to 60 per cent. He has done the same thing by glass tubes for thermometers."

The Fishery Board for Scotland has issued a memorandum on the present state of the shore and sea fisheries of Scotland, by Dr. T. Weymss Fulton, secretary for scientific investigations. His review of the subject brings out the following facts:—(1) That the shore fisheries, especially those for lobsters, crabs, oysters, and mussels are steadily declining. (2) That the number of fishermen, the number of fishing-boats, and the value of the boats and gear, are decreasing. (3) That the number of Scottish beam-trawlers is increasing. (4) That the number of English beam-trawlers which have forsaken the less productive fishing-grounds off the English coast and have resorted to those lying off the Scotch coast is increasing year by year.

The Pilot Chart of the North Atlantic Ocean, issued by the Hydrographer of the United States, shows that the month of October was very stormy north of a line from Bermuda to the British Isles. The month opened with a severe storm about 500 miles north-east from Newfoundland, and pressures below 28·61 inches. There was also at this time a storm in the North Sea, which appears to have originated off Cape Sable on September 27. A marked feature of the month was the existence of a great anticyclone extending from the Azores to near the British Isles, which caused the storms originating to the west of it to take a north-east instead of an east-north-east direction. Considerable fog was reported off the Banks and west of the Irish Channel, but the amount for the month was probably not greater than the average. More ice than usual for the time of year was reported off the Banks as far south as the 45th parallel.

Symons's Monthly Meteorological Magazine for November contains a climatological table for the British Empire for 1889. These interesting results of a certain number of well distributed stations, prepared from the summaries published in the *Magazine* each month, have been continued for many years, and, as the author points out, the extremes are monopolized year after year by the same stations. The highest temperature in the shade was 109°·0 at Adelaide on January 13; for the last five years Adelaide has recorded the highest temperature in the shade, reaching 112°·4 in 1886. It had also the highest temperature in the sun, 170°·7, and was the driest station during the year, having a mean humidity of 63 per cent. The lowest shade temperature was recorded at Winnipeg, on February 23, −42°·6; only once does any other station come within 20° of it. It had also the greatest range in the year, the greatest mean daily range, 24°·5, the lowest mean temperature, and the least rainfall, 14·95 inches. The highest mean temperature was 80°·5, at Bombay, and the greatest rainfall 73·79 inches, at Trinidad. London was the most cloudy and the dampest station, the mean humidity being 81 per cent. The brightest station was Malta, which had little more than half the cloud of London.

SPEAKING a few days ago at Agra College, the Viceroy of India expressed his approval of the proposal to add scientific and technical training to the curriculum at Indian Colleges, and hoped that it would tend to stimulate and resuscitate some of those arts and industries for which India was famous in by-gone days. If this is really the result, it will be a striking instance of the importance of scientific and technical instruction.

A NOVEL whaling expedition is about to be undertaken by three American whalers—*Hume*, *Grampus*, and *Nicoline*—which have gone to the Arctic regions to winter at the mouth of the Mackenzie River. In order to be well supplied with food, they have taken what will last for two years, and they expect also to get food from the whalers in the summer. This is the highest point any whaler has reached, being 1000 miles from the North Pole. Directly the ice breaks after the winter, the whales come to the mouth of the river in great numbers to feed, and it is expected that a large number of them will be secured. If the experiment proves successful, it will cause a revolution in the whaling industry, as vessels at present have to spend several months on the voyage to good whaling-ground, and then have only a very short time for hunting.

THE new number of the Journal of the Marine Biological Association of the United Kingdom (new series, vol. i., No. 4) opens with a list of governors, founders, and members. In addition to the report of the Council for the year 1889–90, and the director's report, it contains the following papers:—Notes on recent experiments relating to the growth and rearing of food-fish at the Laboratory; report on the surface collections made by Mr. W. T. Grenfell in the North Sea and West of Scotland, by G. C. Bourne (with plate); notes on the herring, long-line, and pilchard fisheries of Plymouth during the winter 1889–90, by W. Roach; notes on the hydroids of Plymouth, by G. C. Bourne (with plate); a complete list of the Opisthobranchiate Mollusca found at Plymouth, by W. Garstang (with plates); notes and memoranda—(1) colour-changes in *Cottus bubalis*, by J. T. Cunningham, (2) note on the British Palæmonetes (*P. varians*), by W. F. R. Weldon, F.R.S.

HAZELL'S Annual for 1891 has been published. This is the sixth year of issue, and we need scarcely say that the volume contains a vast mass of useful information, clearly arranged. In the additions included in the present issue the editor has been careful to keep the work well up to date.

TO the new number of the Natural History Transactions of Northumberland, Durham, and Newcastle-upon-Tyne (vol. x., Part 2), Mr. R. Howse contributes a note on the South Durham salt borings, with remarks on the fossils found in the magnesian limestone cores, and the geological position of the salt; a catalogue of the local fossils in the Museum of the Natural History Society; and a catalogue of the fishes of the rivers and coast of Northumberland and Durham and the adjacent sea. The number also contains the report of the Committee for 1888–89.

THE Geological Survey of New Zealand has issued its reports of exploration during 1888–89, with maps and sections. The Director, Sir James Hector, notes that the Department was very fully represented at the Melbourne Exhibition by collections and maps illustrating the geological structure and mineral wealth of the colony. He also mentions that, as a contribution to the work of the Australasian Association for the Advancement of Science, a complete census of the mineral species which have been found in New Zealand has been compiled, together with the localities where found, the names of the first discoverers, and the dates. From this census it appears that 237 species are now recorded.

MESSRS. DULAU AND CO. have published a catalogue of the botanical works which they offer for sale. It includes a large number of valuable books relating to Phanerogamia.

IN 1865, Mr. J. G. Lockhart, while serving as an officer of the Hudson Bay Company, wrote some interesting notes on the moose in the far north of British America. These notes have been preserved in the archives of the Smithsonian Institution, and now appear in the Proceedings of the National Museum. Referring to the hunting of the moose, Mr. Lockhart says the most usual way is to approach the animals on snow-shoes or on foot, as only a hunter knows how, and shoot them. The old men who are not able to walk much in deep snow make a kind of fence of three poles tied equidistant from each other, a little taller than a man, stretching, perhaps, for two days' march between lakes, or a lake and a river, or between two mountains, or in any particular place where the moose are accustomed to pass. Spaces are left vacant here and there in this fence, and in these snares are set. In autumn, during the rutting season, the hunter carries with him the clean, dried shoulder-blade of a moose, and when he hears the call of the male moose, which is audible at a distance of several miles, he rubs the shoulder-blade against a small, dry tree and imitates the call of the male. The moose, as soon as he hears the sound, imagines, no doubt, that it is another moose, and runs in the direction, till met by a shot. The male is very dangerous at that season, especially when wounded. Many years ago, before guns and ammunition found their way into the country, the Indians used to build snow embankments near favourite feeding places, and lie hid there for days until a moose should chance to pass near, when they would kill him with arrows.

THE U.S. Department of Agriculture has issued the third of a series of papers entitled "Contributions from the U.S. National Herbarium." It consists of a list of plants collected by Dr. Edward Palmer in 1890, in Lower California and Western Mexico. Great interest was felt in Dr. Palmer's trip to La Paz and vicinity, and his collection is said to have added much to what was previously known regarding the flora of that region.

AN interesting paper is communicated by Prof. von Hofmann to the current number of the *Berichte* upon the dissociation of carbon dioxide gas into carbon monoxide and oxygen by means of the electric spark. Dalton and Henry long ago showed that carbon dioxide, although formed by exploding a mixture of two volumes of carbon monoxide with one volume of oxygen by the passage of an electric spark, is again partially decomposed into carbon monoxide and oxygen by the continued passage of the spark. This dissociation, however, is very slow, and usually incomplete. Hofmann and Buff, in the course of their well-known work upon gaseous reactions, further showed that "the electric spark passes through carbon dioxide with a violet glow, producing at first a rapid increase in the volume, which, however, becomes less and less marked until at the expiration of about half an hour the separated carbon monoxide and oxygen recombine with a sudden explosion, the re-formed carbon dioxide at once commencing to be again dissociated." Deville and Berthelot afterwards investigated the same phenomena, and also found that the reaction was never complete, proceeding only until about 28 or 29 per cent. of the carbon dioxide was decomposed, but they never observed any explosive recombination as described by Hofmann and Buff. Prof. Hofmann has therefore determined the exact conditions under which the explosive recombination occurs. It certainly appears somewhat remarkable that the same spark can effect both dissociation and recombination, yet such, within the limits described in the memoir, is an actual fact. The first essential point to be observed is the length of path of the spark; the most suitable distance apart of the platinum terminals appears to be between two and a half and three millimetres, and Prof. Hofmann advises the use of adjustable terminals rather than the ordinary platinum wires fused into the side of the eudiometer. A Leyden jar in the circuit renders the occurrence of periodical explosions more certain. The spark should also pass at about a quarter the height of the gas column instead of, as usual, near the top. The current itself, moreover, should not be too strong; that from two Bunsen cells and only a moderate sized Ruhmkorff coil is quite sufficient, and yields the best results. It is also preferable to use a volume of carbon dioxide, previously dried over oil of vitriol, not exceeding ten cubic centimetres at a pressure of 650–700 mm.; eight c.c. give excellent results. Under these conditions the first explosion usually occurs in 15–20 minutes, and sometimes earlier. The flame commences in the neighbourhood of the spark, and then perceptibly spreads through the whole length of the gas column. It is coloured blue in the first explosion, and green in the succeeding ones owing to the volatilization of a little mercury vapour. The second and succeeding explosions occur after shorter intervals than the first. This experiment is certainly one of the most interesting in all the range of dissociation phenomena, and full details, with drawings of the apparatus, are given by Prof. Hofmann in his memoir.

The additions to the Zoological Society's Gardens during the past week include an Ocelot (*Felis pardalis*) from South America, presented by Mr. J. H. Bennett; a Common Fox (*Canis vulpes* ♀) from North Wales, presented by Mr. B. Myddelton Biddulph; two Cape Zorillas (*Ictonyx zorilla* ♀ ♀) from South Africa, presented by Mr. R. Southey; two Ring-necked Parakeets (*Palæornis torquatus*) from India, presented by Miss S. L. Hands; ten Thunder-fish (*Misgurnus fossilis*) from the Baltic Sea, five Golden Orfe (*Leuciscus orfus*), European fresh waters, purchased.

OUR ASTRONOMICAL COLUMN.

The Variations in Latitude.—The most important question discussed at the International Conference on Degree Measurement held at Freiburg on September 15 was that of the variability of terrestrial latitudes. The Central Bureau, represented by Drs. Helmert and Albrecht, communicated two notes, one by Dr. Albrecht, "Resultate der Beobachtungsreihen betreffend die Veränderlichkeit der Polhöhen," the other by Dr. Marcuse, "Resultate der fortgesetzten Berliner Beobachtungsreihen betreffend die Veränderlichkeit der Polhöhen." These papers, and a general account of the whole question of variation in latitude, first raised by Prof. Fergola at the Conference of Rome (1883), is given by M. Tisserand in the *Bulletin Astronomique* for September. The method adopted in the observations made at the different stations is that of Horrebow. It consists in forming nine groups, each containing eight or nine couples of stars, two groups at least being observed each evening. The two stars of each couple were of almost equal magnitude; their difference in right ascension was comprised between 3m. + 15m., and that of their meridian zenith distances between ± 12′, whilst the meridians were never separated more than 27°. By choosing such stars, and taking the arithmetical mean of the differences of zenith distances of the two components, the small error due to an imperfect knowledge of the movement of the micrometer wire is eliminated. At each station a number of couples of stars ranging from 1400 to 1700 have been observed during the whole of 1889 and the first three months of 1890. It is evident that if the same couple of stars could be observed throughout the year, during the day as well as the night, the variations in latitude could be obtained independently of the errors of the absolute declinations of the two stars, provided that we knew exactly the variations of these declinations during the whole interval of observation. These variations depend on the motion of the terrestrial axis (precession and nutation), on aberration, annual parallax, and proper motion. It is impossible to work in this way for two reasons, (1) stars cannot be observed in the day-time by means of the instruments employed; (2) only a very small number of observations could be made. The groups of stars referred to above have been formed in order to avoid these inconveniences, for the differences, from the mean declinations, of each of the couples may be used as if only a single couple had been observed. When the necessary corrections are applied to the calculations it is found that the latitudes of Berlin, Potsdam, and Prague, indicate clearly a diminution of about 0″·5 from August 1889 to February 1890. The observations made at Berlin from April 15 to August 30 in this year show an increase of 0″·4 in the latitude of this place during the interval considered, and point to a period of about a year. The fact that the latitude of a place is subject to an annual periodic variation is not entirely new, for Gaillot discussed 1077 observations of latitude made between 1856 and 1861. On arranging the observations according to the month in which they were made, the following differences from the mean value were obtained (*Annales de l'Observatoire de Paris*, p. 319):—

	Paris. ″	Potsdam. ″		Paris. ″	Potsdam. ″
January ...	−0·23 ...	−0·11	July	+0·25 ...	+0·19
February...	−0·06 ...	−0·07	August ...	+0·16 ...	+0·17
March ...	−0·03 ...	−0·04	September	+0·13 ...	+0·10
April... ...	−0·03 ...	0·00	October ...	−0·07 ...	−0·03
May	+0·10 ...	+0·05	November	−0·11 ...	−0·14
June	+0·16 ...	+0·14	December	−0·27 ...	−0·26

These observations, therefore, indicate a difference of almost half a second between the observed latitudes in January and July. M. Nobile has discussed, from the same point of view, the observations made at Greenwich, Milan, Oxford, Pulkova, and Washington (*Bulletin Astronomique*, vol. v. p. 544). The Greenwich observations show a maximum of latitude in July and August, and a minimum in December and January, the amplitude of the variation being nearly 1″. At Washington a minimum also occurs at the end of the year, but at Milan it occurs in May. The Oxford observations show a maximum in the autumn, whilst those made at Pulkova do not appear to present any periodic variation with the seasons. But although these results are somewhat contradictory, those communicated at Freiburg clearly demonstrate the existence of a variation of 0″·5. It remains to be seen how the amplitude and the phase of the variation is affected by passing from the northern to the southern hemisphere, or when observations are made at two stations having appreciably the same latitude, but widely separated in longitude.

The cause of the variation has yet to be found. M. Tisserand has shown ("Traité de Mécanique céleste," vol. ii. p. 489) that the transportation of a mass of water, 0·10m. thick, from latitude + 45° to − 45°, and covering a tenth of the earth's surface, may cause the principal axis to move 0″·16. The weight of a column of water 0·10m. thick, is equal to that of a column of mercury 0·007m. thick, hence considerable changes in the barometric pressure may have an appreciable effect on the direction of the earth's axis. That the variations are connected with the succession of the seasons is manifest. This seems to indicate that temperature plays an important part, and that the variation is a result of the varying amount of heat received throughout the year by the atmosphere and the instruments. The theories of refraction require the atmosphere to have a regular constitution and a statical condition that is probably never realized. If the constitution varies with the season, the refractional effect must also change, and this may cause the difference in the observations of latitude.

Another probable cause was suggested by M. D. Lamey at the meeting of the Paris Academy on November 17. He showed that if the atmospheric tidal effects be calculated for each month in the year, the values obtained followed a curve very similar to that derived from the observed differences in latitude. That the sun and moon produce atmospheric tides must be incontestable. The physical consequence is that refraction phenomena must also vary with the tidal effect, and therefore the positions of stars may experience an annual periodic fluctuation comparable with that observed. The cause wherein lies the solution of the problem can only be definitely determined, however, after a more extended study than has yet been made.

NEW ASTEROIDS.—M. Charlois discovered the asteroid (298) on September 9. On October 3, what was supposed to be the same body was again observed by Charlois. Dr. Palisa also observed an asteroid near the computed position of (298) on October 11. It has since been found that the observations of October 3 and 11 must refer to another asteroid, and, as Dr. Palisa discovered (299) on October 11, the one thought to be identical with (298) will be reckoned as (300). M. Charlois rediscovered (298) on November 14. Dr. Palisa added (301) the list on November 16 (*Astronomische Nachrichten*, 3006).

The large number of asteroids now known renders it impossible for the Berlin Computing Bureau to furnish all the data for their observation. The editors of the *Berliner Jahrbuch* have therefore decided only to furnish predictions for the following asteroids :—

(1) Those that approach near the earth, and are therefore well adapted to the determinations of parallax.

(2) Those whose near approach to Jupiter renders them useful for determining his mass.

(3) Those remarkable for a period having a closely commensurable ratio to that of Jupiter ; such orbits being of the highest importance in the theory of absolute perturbations.

(4) Those that are of value for photometric work on account of their considerable brightness.

ZONA'S COMET (*e* 1890).—Dr. Bidschof gives the following elements and ephemeris of this comet in *Astronomische Nachrichten*, 3006 :—

T = 1890 July 25·051 Berlin mean time.

$$\left.\begin{aligned} \omega &= 321^\circ\ 58' \\ \Omega &= 84\ \ 45 \\ \iota &= 153\ \ 28 \end{aligned}\right\}\ \text{Mean Eq. 1890.}$$

Perihelion distance = 1·8996 (earth's mean distance = 1).

Ephemeris for Berlin Midnight.

1890.	R.A. h. m.	Decl. ° ′	Distance in terms of the earth's mean distance.
December 4	3 47·0	+ 34 50	1·53
,, 8	3 26·8	+ 34 19	1·59
,, 12	3 8·7	+ 33 36	1·65

The comet is, therefore, in the constellation Perseus, which is in the south about 10 p.m. It passed perihelion about the end of July, and is increasing its distance from the earth. M. Bigourdan, of Paris Observatory, describes the comet as "très faible (grandeur 12·5–13), et se présentait sous l'aspect d'une petite tache blanche, ronde, de 1′ environ de diamètre, avec condensation central assez diffuse et d'aspect un peu grande."

THE SCIENTIFIC WORK OF JOULE.[1]

PROF. DEWAR commenced by remarking that the Royal Institution had been so closely identified with the great workers in physical science that it was impossible to allow the work of Joule, whose researches had produced as marked a revolution in physical science as Darwin's in biology, to pass without recognition in the present series of Friday evening discourses. Sir William Thomson, as Joule's friend and fellow-worker to the last, had been invited to undertake the duty, and had agreed to do so ; but at the last moment had been compelled to decline by reason of important official duties in Scotland, and the task had consequently devolved upon him.

Having given a brief account of Joule's parentage, early life, and education, Prof. Dewar reviewed, as fully as time would permit, his scientific work, which extended over about forty years, and was represented by 115 original memoirs. The first period (1838–43) was distinguished as that in which Joule educated himself in experimental methods, chiefly in connection with electricity and electro-magnetic engines. This work led him in 1840 to his first great discovery, the true law governing the relation between electric energy and thermal evolution, which enabled him later on to account for the whole distribution of the current, not only in the battery in which it is produced, but in conductors exterior to it. Joule was thus led to take up the study of electrolysis. Faraday had already made the discovery that electrolytic bodies could be split up into equivalent proportions by the passage of the same electric current ; Joule saw that there would be great difficulty in finding out the distribution of the current energy, and accounting for the whole of it. After a laborious research he succeeded in showing that during electrolytic action there was an absorption of heat equivalent to the heat evolved during the original combination of the constituents of the compound body. The prosecution of his electrical researches rapidly brought Joule on the road to his great discovery of the mechanical equivalent of heat, it being clear from a footnote to a paper dated February 18, 1843, that he already had well in hand the study of the strict relations between chemical, electrical, and mechanical effects.

[1] Abstract of the Friday evening discourse delivered at the Royal Institution of Great Britain on January 24, 1890, by Prof. Dewar, F.R.S.

In working out these laws, it was to be remarked that Joule—in common with most inventors and seekers after new scientific truths—chose perhaps the most difficult means that could have been selected ; and in looking back at his work in the light of present knowledge, it seemed simply astounding that he should have succeeded so completely as he did. The original coil used by Joule for the mechanical determination of heat (kindly lent for the occasion by Prof. Rücker) was shown, and the course of the experiment explained. The vast difficulties which Joule had to overcome in order to prove that there was a definite, permanent, and persistent relation between the amount of mechanical energy expended and the heat produced were commented on ; the thermal effects being produced not directly but through the medium of an electric current varying in intensity, and calculations having to be made not only for these fluctuations, but for the effects of radiation, the movement of the air, and other indirect complications. The very small increment of heat to be measured obliged Joule to use thermometers of great delicacy, and these he had to devise and construct himself. One of the thermometers so used was exhibited.

Working in this way, Joule was able, by the end of July 1843, to state definitely that the amount of heat capable of increasing the temperature of a pound of water by 1° F. was equal to, and might be converted into, a mechanical force capable of raising 838 lbs. to the height of 1 foot. Soon afterwards he attained almost identical results by a more direct method—the friction of water passing through small tubes—which gave him 770 foot-pounds per unit of heat.

It was impossible, said the lecturer, to thoroughly appreciate Joule's work without glancing at the early history of the subject ; and when one did so it was amazing to find how near men of the stamp of Rumford, Davy, and Young had been to Joule's great discovery, and yet missed it. Count Rumford was the first to clearly define the relation between the constant production of heat and loss of movement by frictional motion. He proved that the amount of heat produced by friction was continuous, and apparently unlimited ; but he did not think of measuring the relation between the mechanical energy expended and the amount of heat produced. Alluding to the results obtained from this apparatus, the lecturer said that Count Rumford might have shown that in his experiments the heat produced was proportional to the time of working, and so obtained a result capable of being expressed in horse-power. The value so deduced from Rumford's experiments is not far removed from Joule's first number.

The experiments commenced by Count Rumford were carried on by Davy, at that time working with Beddoes at Bristol ; and led to one of the most remarkable essays on heat of that period, which disposed for ever of the theory of the separate existence of caloric. Taking two pieces of ice on a cold day, Davy mounted them so that they could be rotated against each other with frictional pressure, the effect being that the pieces of ice were melted, the water so produced having a much higher specific heat than the original ice. To guard against the possibility of heat being conveyed to the frictional apparatus by the surrounding air, Davy made an experiment *in vacuo*, isolating the apparatus by means of ice ; and found that under such conditions sufficient heat could be produced to melt wax placed in the receiver. The lecturer here showed an experiment illustrating the production of water by the friction of two pieces of ice *in vacuo*, under conditions of temperature much much severe than those of Davy's experiment.

Following Davy, Young devoted a great deal of attention to the subject, and by 1812 he and Davy had quite changed their opinions, and had adopted the view that heat and motion were convertible effects.

Having by July 1843 assured himself of the principle of his discovery, Joule now devoted his attention to the elaboration of methods of working, modifying, and repeating experiments in various ways, but always approaching nearer and nearer to exactness, as shown by the following table of results:—

Joule's Values of the Mechanical Equivalent of Heat.

		Kilogramme metres.
Magneto-electric currents	1843	460
Friction of water in tubes	,,	424·6
Diminution of heat produced in a battery current when the current produces work	,,	499
Compression of air	1845	443·8
Expansion of air	,,	437·8
Friction of water	,,	488·3
,, ,, ,, ...	1847	428·9
,, ,, ,, ...	1850	423·9
,, ,, mercury	,,	424·7
,, ,, iron	,,	425·2
Heat developed in Daniell's cell ...	,,	419·5
,, ,, in wire of known absolute resistance	1867	429·5
Friction of water in calorimeter ...	1878	423·9

Prof. Dewar here exhibited and explained the action of the original calorimeter used by Joule. It was seen to consist of a set of vanes which were made to revolve in water by the falling of known weights through a definite and known height, the heat produced being due (after making the necessary deduction for the friction due to the momentum of the weights) entirely to the friction of the fluid. It was found that, whatever fluid was employed, the same definite results were obtained—a production of heat in the liquid bearing a constant relation to the unit of mechanical energy expended. The extreme delicacy of Joule's apparatus, and the marvellous accuracy of his observations, were shown by the fact that working with weights of 29 pounds each, and repeating each observation 20 times, the total increase of temperature did not exceed half a degree Fahrenheit. In contrast to this the lecturer showed, by means of apparatus kindly lent by Prof. Ayrton, the method now employed for repeating Joule's work and arriving at substantially the same results by much simpler means.

While continuing to work intermittently at his great discovery, Joule employed himself in the following years in elaborate investigations bearing upon the point of maximum density of water, specific gravity, and atomic volumes. An illustration of his method of determining maximum density was given by means of two large cylinders filled with water and connected by a narrow channel in which was placed a floating indicator. It was shown that the slightest variation in density of the water of either cylinder—variations far beyond the scope of the most delicate thermometer—set up currents which were immediately detected by the movement of the indicator, and that by this means it was quite possible to ascertain the exact temperature at which water attained its maximum density.

Joule's determinations of atomic volumes were marvellous at the time they were made, and were still interesting. Illustrations of his work in this direction were given by means of a solution of sugar, which was seen to occupy practically the same space as was occupied by an amount of water exactly equivalent to that combined in the carbohydrate, the carbon hypothetically combined with the water to form the sugar appearing to make no sensible difference to the volume; and in contrast to this was seen the enormous difference in volume brought about by dissolving two equal portions of soda carbonate, one portion being ordinary hydrated crystals, and the other portion being anhydrous, in equal volumes of water.

Joule's last great research was carried out conjointly with Sir William Thomson, and occupied nearly ten years of laborious inquiry. Its chief object was to prove that in compressing a gas the amount of heat produced is equivalent to the work done, and independent of the mere fact of the approach of the particles. But Joule was desirous of amplifying the inquiry, and in fact the work might be divided into three sections: (1) the study of gases passing through narrow apertures; (2) the velocity attained by bodies passing through the air; and (3) the temperature ultimately attained by such moving bodies. With respect to (2) and (3), it was shown that a body rotating in the air at the rate of about 150 to 180 feet per second increased in temperature by nearly 1° F., and that this increase of temperature was definite for a given velocity, and independent of the size of the moving mass and the density of the gaseous medium. With regard to (1), the relation of gaseous pressure and volume to temperature, the researches of Regnault had already shown that the simple law of Marriotte and Boyle could not stand by itself; and Joule sought to modify it by the study of gases passing through very small tubes or porous bodies. The investigations were carried out at Manchester on a large scale, and were assisted by a Government grant. Steam engines were employed to maintain a current of gas at a constant temperature and pressure through long coils of pipe placed in water tanks. They proved that any difference of temperature in the gas brought about in its passage through the porous body must be due to work done by it, and that this difference of temperature varied for different gases, according to their constitution. Working under the same conditions, hydrogen was shown to be reduced a small amount in temperature, air somewhat more (about 0°·3), and carbonic acid a much greater amount. A repetition of Joule and Thomson's experiment was shown by means of a 100-feet coil of lead pipe, compressed hydrogen, air, and carbonic acid gas being employed, and the original results verified in each case. The effect of this research was to enable Joule and Thomson to formulate a great improvement on the gaseous laws; for, instead of the product of the volume and pressure being strictly proportional to the absolute temperature, as it had been hitherto believed, they found that a new term was involved, which is equivalent to a constant divided by the absolute temperature and multiplied by the volume.

In conclusion, Prof. Dewar read the following letter, which he had received from Sir Lyon Playfair in response to his request for some reminiscences of Joule:—

January 20, 1890.

Dear Dewar,—You ask for some of my memories of Joule from 1842 to 1845, when I was Professor of Chemistry at the Royal Institution in Manchester. The great Dalton died in the autumn of 1844, and had long been President of the Manchester Philosophical Society. He naturally gave impulse to the study of science in that town, where there was an active band of young workers in research.

Joule was, even then, foremost among these; and the names of Binney, Williamson, Schunck, Angus Smith, Young, and others show that the spirit of scientific inquiry was active. We were also stimulated by the fact that Baron Liebig and Bunsen came to pay me visits during that time; they were men to excite research.

Joule was a man of singular simplicity and earnestness. We used to meet at each other's houses at supper, to help the progress of our work by discussion. Joule was an earnest worker, and was then engaged on his experiments on the mechanical equivalent of heat. He took me to his small laboratory to show me his experiments, and I, of course, quickly recognized that my young friend the brewer was a great philosopher. We jointly worked upon questions of far less importance than his great central discovery, but he was equally interested. I was very anxious that he should devote his life to science, and persuaded him to become candidate for the Professorship of Natural Philosophy at St. Andrews. He was on the point of securing this, but his slight personal deformity was an objection in the eyes of one of the electors; and St. Andrews lost the glory of having one of the greatest discoverers of our age.

When Joule first sent an account of his experiments to the Royal Society, the paper was referred, among others, to Sir Charles Wheatstone, who was my intimate personal friend. Wheatstone was an eminently fair man and a good judge, but the discovery did not then recommend itself to his mind. For a whole Sunday afternoon we walked on Barnes Common, discussing the experiments and their consequences, if true, to science. But all my arguments were insufficient to convince my friend; and I fear that then the Royal Society did not appreciate and publish the researches. I write from memory only, for I know that, later, no Society or institution honoured Joule more than the Royal Society and its members.

Not for one moment, however, did Joule hesitate in the accuracy of his experiments or his conclusions. He once suggested to me that we might take a trip together to the Falls of Niagara, not to look at its beauties, but to ascertain the difference of temperature of the water at the top and bottom of the fall. Of course the change of motion into heat was a necessary consequence of his views.

No more pleasant memory of my life remains than the fact that, side by side, at my lectures in the Royal Institution, used to sit the illustrious Dalton, with his beautiful face, so like that of Newton, and the keenly intelligent Joule. I can give no other explanation than the fact of organic chemistry being then a new science that two philosophers of such eminence should come to the lectures of a mere tyro in science. I used to look upon them as two types of the highest progress in science. Newton had introduced law, order, and number into the movements of masses of matter in the universe; Dalton introduced the same into the minute masses which we call atoms; and Joule, with a keen insight into the operations and correlation of forces, connected them together, and showed their mutual equivalence.

I do not know whether these memories are of any use to you, but, such as they are, they are at your disposal for your lecture on the friend of my youth. Yours sincerely,

LYON PLAYFAIR.

WEIGHING BY A SERIES OF WEIGHTS.

THIS subject, now under discussion in NATURE, is by no means new. The following remarks on the general theory of such questions may prove interesting. The problems are divisible into two classes:—

(1) To assign a series of weights so as to be able to weigh any weight of an integral number of pounds from 1 to n inclusive, the weights being placed in only one scale-pan.

(2) The same problem when the weights may be placed in both scale-pans.

Two other conditions may be imposed, viz.:—

(*a*) That no other weighings are to be possible.

(*b*) That each weighing is to be possible in only one way, *i.e.* to be unique.

The question considered in NATURE is of the second class, and is further subject to the two conditions above stated. Moreover, the problem for the number of pounds 1, 4, 13, 40, &c...., as solved by the series of weights 1; 1, 3; 1, 3, 9; 1, 3, 9, 27; &c...., respectively gives in each case the series containing the least number of weights for which the solution is possible.

For a number of pounds equal to 40, the solution may be given by means of a single algebraic formula, constituting an identity, viz.

$$(x^{-1} + 1 + x)(x^{-3} + 1 + x^3)(x^{-9} + 1 + x^9)(x^{-27} + 1 + x^{27})$$
$$= x^{-40} + x^{-39} + \ldots\ldots + x^{-1} + 1 + x + \ldots\ldots + x^{39} + x^{40}.$$

This is easily verified. When the left-hand side is multiplied out, the resulting powers of x are obtained by addition of certain indices, some positive and some negative. The indices in question are 1, 3, 9, 27, and, from the form of the factors, may in any single term be taken *either* positively or negatively. Thus one term on the left is $x^{-3} . x^9 . x^{27} = x^{-3+9+27}$, giving the term x^{33} on the right-hand side. The identity shows that every number from 1 to 40 inclusive can be thus composed by the numbers 1, 3, 9, 27, taken positively or negatively. It shows, moreover, since the coefficients of the several powers on the right are each unity, that the composition is unique. The identity includes also negative integers from -1 to -40. This is necessitated by the very nature of the question.

The above is one out of eight distinct solutions of the problem before enunciated. Before giving the other seven, it will be as well to show the principles of solution in general.

Consider, first, the problem of the first class in which weights can only be placed in one scale-pan. The number of pounds to be weighed being n, we must discover the ways in which it is possible to break up the expression

$$1 + x + x^2 + x^3 + \ldots\ldots + x^n$$

into similar expressions, for, as will be evident, each such factorization corresponds to a definite solution of the problem. By similar expression is to be understood one in which the coefficients are unity and the indices are in arithmetical progression.

At the outset may be remarked the trivial solution consisting of n ones corresponding to the unfactorized expression. There is one solution of this kind in respect of every number n.

Putting the expression in the form

$$\frac{1 - x^{n+1}}{1 - x},$$

it will be clear that if $n + 1$ be a prime number, the expression cannot be factorized in the required form. This is a consequence of the properties of prime roots of unity.

Hence, when $n + 1$ is prime, the only solution is obtained by taking n one-pound weights.

If, however, $n + 1$ be not prime, the number of solutions depends upon the composite character of $n + 1$. If $n + 1 = st$ where s and t are primes, we may write

$$\frac{1 - x^{n+1}}{1 - x} = \frac{1 - x^{st}}{1 - x} = \frac{1 - x^s}{1 - x} \cdot \frac{1 - x^{st}}{1 - x}$$
$$= (1 + x + x^2 + \ldots + x^{s-1})(1 + x^s + x^{2s} + \ldots + x^{st-s}),$$

a factorization of the required form.

Multiplying x^p in the first factor into x^{qs} in the second, we find that the number $p + qs$ is composed by p numbers equal to 1, and q numbers equal to s. In general, every number from 1 to n inclusive can be composed by means of $s - 1$ ones, and $t - 1$ s's. That is to say, that all numbers of pounds from 1 to n (where n is of the form $st - 1$) can be weighed, and in a unique manner, by means of $s - 1$ weights of 1 pound each, and $t - 1$ weights of s pounds each.

Another solution is obtained from the identity

$$\frac{1 - x^{n+1}}{1 - x} = \frac{1 - x^{st}}{1 - x} = \frac{1 - x^t}{1 - x} \cdot \frac{1 - x^{st}}{1 - x_t}$$

obtained from the former by interchange of s and t. These two solutions, together with the trivial one, constitute the complete set of three solutions. For brevity, these may be represented in the form—

$$1^{st-1},$$
$$1^{s-1}\, s^{t-1},$$
$$1^{t-1}\, t^{s-1},$$

where s^{t-1} means $t - 1$ s's, and so on.

From the above simple example it will be sufficiently clear that the number of solutions in the case of a given number n is entirely dependent upon the form of $n + 1$, when regarded as a product of prime factors. The number of solutions is not in all cases easy to determine, but some of the simpler results are interesting. Suppose the number $n + 1$ to be a product of k unrepeated prime numbers, and, under these circumstances, represent the number of solutions in regard to the number n by

$$[1^k].$$

Then it is easy to prove the relation—

$$[1^k] = 1 + k[1^1] + \frac{k(k-1)}{2!}[1^2] + \frac{k(k-1)(k-2)}{3!}[1^3] + \ldots$$
to k terms,

and thence to calculate the values of $[1^1]$, $[1^2]$, $[1^3]$,... in succession. The series of numbers thus obtained commences 1, 3, 13, 75, 541,... where it will be noticed that the second number, 3, corresponds with the result obtained above for the case of $n + 1$, being of the form st, or the product of two unrepeated primes. It may be observed that this series of numbers is of great interest, and presents itself frequently in mathematics, notably in Prof. Cayley's "Theory of the Analytical Forms called Trees" (see vol. xiii. and subsequent volumes of the *Philosophical Magazine*, and "Collected Papers," No. 203).

A much simpler case to consider is that for which $n + 1$ is merely a power of a prime number. If $n + 1$ be the kth power of a prime, represent the number of solutions in regard to the number n by

$$[k].$$

A small amount of reflection shows the truth of the relation

$$[k] = 2[k - 1];$$

whence

$$[k] = 2^{k-1}.$$

Turning now to the second class of problems, in which it is permissible to place weights in either or both scale-pans, it can be shown that the theory is not essentially different from that belonging to those of the second class.

In order to weigh any number of pounds from 1 to n inclusive, we have to factorize the expression

$$x^{-n} + x^{(-n-1)} + x^{-(n-2)} + \ldots + n^{-1} + 1 + x + \ldots + x^{n-2} + x^{n-1} + x^n,$$

which may be thrown into the form

$$\frac{1 - x^{2n+1}}{x^n(1 - x)}.$$

The solutions depend upon the composite character of the number $2n + 1$. There always exists the trivial solution consisting of n ones, and when $2n + 1$ is prime, this constitutes the only solution.

Supposing $2n + 1$ the product of two primes, viz.

$$2n + 1 = st,$$

we may write

$$\frac{1 - x^{2n+1}}{x^n(1 - x)} = \frac{1 - x^{st}}{x^n(1 - x)} = \frac{1}{x^n} \cdot \frac{1 - x^s}{1 - x} \cdot \frac{1 - x^{st}}{1 - x^s}$$

$$= \frac{1 - x^s}{x^{\frac{s-1}{2}}(1 - x)} \cdot \frac{1 - x^{st}}{x^{\frac{s(t-1)}{2}}(1 - x^s)}$$

$$= \left(x^{-\frac{s-1}{2}} + x^{-\frac{s-3}{2}} + \ldots + x^{-1} + 1 + x + \ldots + x^{\frac{s-3}{2}} + x^{\frac{s-1}{2}}\right)$$
$$\times \left(x^{-\frac{st-s}{2}} + x^{-\frac{st-3s}{2}} + \ldots + x^{-s} + 1 + x^s + \ldots + x^{\frac{st-3s}{2}} + x^{\frac{st-s}{2}}\right).$$

The factors appearing in this last expression are of the required form, and the factorization indicates that any number from 1 to n inclusive may be composed by means of $\frac{1}{2}(s - 1)$ ones, and $\frac{1}{2}(t - 1)$ s's, if all the ones that are taken be taken *either* positively or negatively, and all the s's also *either* positively or negatively.

The solution may be represented, according to the method before explained, by

$$1^{\frac{1}{2}(s-1)}\ s^{\frac{1}{2}(t-1)},$$

and the complete system of solutions will in the present case be denoted by

$$\begin{cases} 1^{\frac{1}{2}(st-1)}, \\ 1^{\frac{1}{2}(s-1)}\ s^{\frac{1}{2}(t-1)}, \\ 1^{\frac{1}{2}(t-1)}\ t^{\frac{1}{2}(s-1)}. \end{cases}$$

From the above, it is clear that there is a one-to-one correspondence between the solutions of the second class problem in regard to the number n and the solutions of the first class problem in regard to the number $2n$. The theory of the second class problems is thus included in that of the first class problems. If we take the case considered in this journal of $n = 40$ and both scale-pans, the number of solutions will be the same as that for $n = 80$ and one scale-pan; the number depends upon the composite character of the integer 81, which is 3^4; hence the number of solutions (see *ante*) is [4], which is 2^3 or 8.

Corresponding to the several identities—

$$\frac{1 - x^{81}}{x^{40}(1 - x)}$$
$$= x^{-40} + x^{-39} + \ldots + x^{-1} + 1 + x + \ldots + x^{39} + x^{40};$$

$$\frac{1 - x^{81}}{x^{39}(1 - x^3)} \cdot \frac{1 - x^3}{x(1 - x)}$$
$$= (x^{-39} + x^{-36} + \ldots + x^{-3} + 1 + x^3 + \ldots + x^{36} + x^{39})(x^{-1} + 1 + x);$$

$$\frac{1 - x^{81}}{x^{36}(1 - x^9)} \cdot \frac{1 - x^9}{x^4(1 - x)}$$
$$= (x^{-36} + x^{-27} + \ldots + 1 + \ldots + x^{27} + x^{36})$$
$$(x^{-4} + x^{-3} + \ldots + 1 + \ldots + x^3 + x^4);$$

$$\frac{1 - x^{81}}{x^{36}(1 - x^9)} \cdot \frac{1 - x^9}{x^3(1 - x^3)} \cdot \frac{1 - x^3}{x(1 - x)}$$
$$= (x^{-36} + x^{-27} + \ldots + x^{36})(x^{-3} + 1 + x^3)(x^{-1} + 1 + x);$$

$$\frac{1 - x^{81}}{x^{27}(1 - x^{27})} \cdot \frac{1 - x^{27}}{x^{13}(1 - x)}$$
$$= (x^{-27} + 1 + x^{27})(x^{-13} + x^{-12} + \ldots + x^{13});$$

$$\frac{1 - x^{81}}{x^{27}(1 - x^{27})} \cdot \frac{1 - x^{27}}{x^{12}(1 - x^3)} \cdot \frac{1 - x^3}{x(1 - x)}$$
$$= (x^{-27} + 1 + x^{27})(x^{-12} + x^{-9} + \ldots + x^{12})(x^{-1} + 1 + x);$$

$$\frac{1 - x^{81}}{x^{27}(1 - x^{27})} \cdot \frac{1 - x^{27}}{x^9(1 - x^9)} \cdot \frac{1 - x^9}{x^4(1 - x)}$$
$$= (x^{-27} + 1 + x^{27})(x^{-9} + 1 + x^9)(x^{-4} + x^{-3} + \ldots + x^4);$$

$$\frac{1 - x^{81}}{x^{27}(1 - x^{27})} \cdot \frac{1 - x^{27}}{x^9(1 - x^9)} \cdot \frac{1 - x^9}{x^3(1 - x^3)} \cdot \frac{1 - x^3}{x(1 - x)}$$
$$= (x^{-27} + 1 + x^{27})(x^{-9} + 1 + x^9)(x^{-3} + 1 + x^3)(x^{-1} + 1 + x).$$

These are the eight solutions represented by

$$\begin{cases} 1^{40} \\ 1 \cdot 3^{13} \\ 1^4 \cdot 9^4 \\ 1 \cdot 3 \cdot 9^4 \\ 1^{13} \cdot 27 \\ 1 \cdot 3^4 \cdot 27 \\ 1^4 \cdot 9 \cdot 27 \\ 1 \cdot 3 \cdot 9 \cdot 27. \end{cases}$$

The subject is more fully entered into in a paper by myself in the *Quarterly Journal of Pure and Applied Mathematics* for 1886.

P. A. MACMAHON.

THE SCIENTIFIC RESULTS OF THE OCCUPATION OF BRITISH NEW GUINEA.

A VOLUMINOUS and extremely interesting report on the first year of the administration of British New Guinea by Sir William MacGregor was issued some time ago by the Colonial Office. It deals with the period ending June 30, 1889. One of the sections of the report deals with "scientific results," which we are glad to notice have a place like "finance," "legislation," "trade and shipping," and the other usual divisions of these colonial reports. In sending the report home, Sir Henry Norman, the Governor of Queensland, observes that it is fortunate that the administrator is most anxious to obtain the best scientific results on his visits and tours, and that he is well able to judge for himself in such matters. The scientific collections, therefore, are made with judgment, and the various reports on collections are of interest and value. Sir William MacGregor himself, in summing up the scientific results, says that during the year some addition was made to our knowledge of the natural history of the country. Unfortunately, it is not possible to set out fully the progress made, as the report on specimens sent to England had not reached him at the moment of writing. It is his hope, however, that in future all specimens collected may be examined in Australia, so that the information gained can be kept together and be summarized in each annual Report.

Geology.—Thirty-one small bags of specimens were examined by Mr. Jack, Government Geologist of Queensland. All except two were from the Louisiade and D'Entrecasteaux groups. Mr. Jack's report, which will be found to be interesting and valuable, is given in an appendix. A set of specimens, covering the route from Manu-Manu to the summit of the Owen Stanley Range, was submitted to careful examination by Mr. Rands, Assistant Government Geologist of Queensland, who at the same time classified the specimens collected in the Rigo district. Mr. Rand's report will also be found in an appendix. Although not forming any part of the work of the year, there is added to the same appendix a report on certain geological specimens collected by Mr. C. S. Wilkinson. These three reports practically contain all that is really known of the geology of the country.

Ornithology.—The greater portion of the birds obtained were classified by Mr. de Vis. His report, prepared after much careful labour, is added as an appendix. From it may be inferred that the probability is that no great addition will be made now to the more beautiful and gorgeous birds of British New Guinea.

Reptiles.—The reptiles collected during the latter part of the year were classified by Mr. de Vis, Director of the Queensland Museum. They include ten species of snakes. Mr. de Vis has reported that of these ten species only two, a death-adder and a whip-snake, are to be dreaded. A small batrachian, mentioned Mr. de Vis, is interesting as having been brought from the top of Mount Victoria. A note furnished by Mr. W. H. Miskin, giving the results of his examination of a collection of Lepidoptera, and a description by Mr. C. Hedley of a new Rhylida from the highest summit of the Owen Stanley Range, are also given in an appendix.

Botany.—All botanical specimens were sent to Baron Mueller. He has found time to classify the specimens forwarded to him, and his report is also included in the "White-Book." In this branch much yet remains to be done, the collecting of plants being attended with great difficulties in New Guinea. It thus appears that the scientific work is contained in a series of appendices by specialists, in the shape of reports made on the various collections of Sir William MacGregor and his officers. Appendix B is a report by Prof. Liversidge on the hot springs of Ferguson Island in the D'Entrecasteau group; while D is a very long report from Sir William MacGregor himself on a tour made by him from Manu-Manu on the coast to the Owen Stanley Range in the interior. It gives *inter alia* a fascinating description of the mountain scenery in that great range. Appendix F contains the reports of Messrs. Jack and Rands on the geological collections. Mr. Jack observes that the specimens tell no connected tale such as would enable one to construct even a theory regarding the geology of the islands. They show, however, that palæozoic rocks, such as are the matrices of gold and other metallic deposits in Australia and elsewhere, are abundant, and that basaltic lavas are of common occurrence. The limestones may yield fossils which would be of great service in unravelling the structure of the islands. Mr. Rands, to whom were submitted the specimens collected by Sir William MacGregor on his expedition up the Vanapa river to the top of Mount Victoria, says that they enable one to form an idea of the geological character of the country drained by the Vanapa. The whole region consists almost entirely of schists, which become more highly metamorphosed as the loftier heights of the Musgrave Range and Mount Victoria are reached. On Mount Victoria the schists are very numerous, highly crystalline, and closely approaching to gneiss; on passing down the river, the country consists of clay, schists, and slates; while from near the mouth there are specimens of a but slightly altered sandstone. The report of Mr. de Vis, of the Brisbane Museum, on the birds is contained in Appendix G. The collection contained 161 specimens, representing 82 species, of which 13 appear to be hitherto unrecorded; "and of the apparent novelties one at least lays claim to generic rank." This is a very distinct kind of bower bird, obtained on Mount Knutsford, at an elevation of 11,000 feet, and rivalling the Regent Bird in beauty. "The name *Cnemophilus* (Mountain-slope Lover) has been appropriated to it, and the species I propose, with permission, to dedicate to yourself [Sir William MacGregor]. A second new bower bird, constituting a third species of the genus *Amblyornis*, and distinguished by a very ornate crest, will, if allowed, be honoured with the name of Lady MacGregor. It is well to note that the diversity in the structure of the bowers of this and of the other crested species of *Amblyornis* is far greater than the differences in their personal attributes. At your request the name of Mr. Belford, one of your party, has been associated with a capture in which he was concerned, a new honeyeater, of the genus *Melirrhophetes*. A similar compliment has been paid to another member of your collecting staff, Mr. C. Kowald, in connection with the beautiful genus of flycatchers, *Todopsis*. The number of species procured during the expedition to the Owen Stanley Range was 61, eight of them being apparently new to science. The expectations of ornithologists who have for some time been awaiting the exploration of that region will thus be in some measure fulfilled, notwithstanding that no new Birds of Paradise have been discovered. Perhaps, however, the greatest interest attaching to the ornithological results obtained arise from the fact that the decided change of climate observed at the altitude attained, over 13,000 feet, is not attended by a corresponding change in the types of bird life; it would seem that there is even here no infusion of forms characterizing temperate or cold latitudes. It is true that no birds were brought down from the highest points reached, but at 13,000 feet a flycatcher was procured which is essentially Australian in type. The presence of a blackbird, now first discovered in New Guinea, is not in this connection contradictory, since the genus *Merula* is represented in other of the Pacific Islands. Some interesting additions to our knowledge of the birds of the Louisiade Archipelago result from your visits to the islands within your jurisdiction. 21 species from East, Sudest, Ferguson, Rossel, and other isles have been determined; of these several cannot be identified with species previously known, as far as I am able to judge. As these birds were procured hurriedly, they doubtless represent but a very small proportion of the several faunas. If it were possible to station a collector on one of the larger islands, Sudest, for example, so that a fairly complete knowledge of its zoology could be obtained, science would be greatly benefited." The descriptive list of the birds which follow is very full and interesting. It is succeeded by a report by the same gentleman on the reptiles collected during the expedition; they consisted of two species of lizards, ten of snakes, and one frog. The snake-like lizard (*Lialis*) is common to Australia and New Guinea; the sleeping lizard (*Cyclodus*) is in Australia represented by other species; both are perfectly harmless. Of the snakes also, the greater majority are innocuous; the death-adder (*Acanthophis*) and the whip-snake (*Diemansia*) are, indeed, the only kinds to be dreaded. With the exception of the tree-snake (*Dendrophis*) the rest are the constrictors *Liasis*, *Chondropython*, *Aspidopython*, and *Enygrus*, and the colubine snakes *Lycodon*, *Mainophis*, and *Pappohis*. It is clear that deadly snakes are not to be added to the imaginary terrors of the Papuan climate. Four of the snakes represented are Australian species. The sole exampler of the batrachians is a little frog, which Mr. de Vis regards as a new species of its genus, *Michrohyla*. This is followed by Mr. Miskin's note on the collection of Lepidoptera. He says that a glance at the specimens, confirmed upon closer inspection, conveyed the impression that they represented only the fauna of the lower altitudes, although as a fact they were collected at some distance from the coast, proving, with but two or three exceptions, to be species already known to science. Sir William was unfortunately unable to give attention to this branch of zoology during his explorations of the higher altitudes, where new forms of the greatest interest might be anticipated to occur. He observes that the collection is interesting as showing the similarity of the New Guinea fauna with that of North East Australia, there being no less than 23 of the 53 species common to both regions; while of the 31 genera represented 25 are found in both countries.

Mr. Hedley in his note on the new Rhytida says that from the highest summit (13,000 feet) of the Owen Stanley Range two land shells were brought down by Sir W. MacGregor and committed for examination to the Queensland Museum. As these are the first traces of molluscan life collected in the New Guinea mountains, and as no form at all resembling them is described by Sig. Tapparone-Canefri, he feels some confidence in deciding them to be a hitherto unknown species. More globose than other of the genus, the glossy exterior, nacreous interior, characteristic colour and sculpture, stamp it as a Rhytida; and thus another genus is added to those common to Australia and New Guinea. Though well preserved both the specimens that furnish the following description are "dead shells"; one is slightly darker and less globose than its fellow:—*Rhytida Globosa*, n.s. Shell, depressed-globose, thin, translucent, perforate, very glossy; whorls 4½, the earlier flattened, the latter rounded, rather rapidly increasing, the last a little expanded, not descending at the aperture; colour, reddish-chestnut above, lighter beneath, first three whorls bleached nearly white; sculpture almost effaced on the body whorl, where nearly obsolete spiral impressed lines cross the faint irregular growth lines; the earlier whorls exhibit fine close oblique striae cut by fine spiral grooves, a pitted (not striated) surface is offered by the first whorl and a half, which seem embryonic; suture impressed, slightly crenulated, bordered beneath by a narrow white band, which is in turn edged by a black line; aperture ovate, oblique; peristone simple above, slightly reflected below; interior bluish-white, probably iridescent when fresh; columella wall overlaid by a thin deposit; umbilicus narrow, partially hidden by the reflected peristone at its junction with the base; base a little inflated. Greater diam. 17 mm., lesser 14 mm., height 10 mm. A report from Baron von Mueller on the Papuan Highland plants collected brings the scientific part of the "White-Book" to a conclusion. This is somewhat lengthy to reproduce in full now, and it is not easy to summarize it adequately. Readers must therefore be referred to the volume itself, where the report appears at pp. 125–129.

THE DETERMINATION OF THE WORK DONE UPON THE CORES OF IRON IN ELECTRICAL APPARATUS SUBJECT TO ALTERNATING CURRENTS.[1]

When the case is one of a transformer, the problem may be solved by the employment of three dynamometers in the way I have already pointed out; but in that of an electro-magnet, where we have only one coil to deal with, the problem still admits of solution if we further employ a condenser of determined capacity, and possess a knowledge of the period by means of some speed indicator. The plan is as follows:—

Arrangement.—Having the machine and magnet in series, insert the three dynamometers in series immediately at one terminal of the electro-magnet, placing one pole of the condenser to the other terminal, and the second pole to that point of the middle dynamometer where its two coils join.

Observations.—Obtain good simultaneous readings of the three dynamometers, and if necessary of the speed indicator.

Elements of Calculation.—Let α_1, α_2, α_3 be the angles read upon the instrument (1) in the generating section, (2) in the electro-magnet section, (3) which has its coils divided.

Let the reducing formula for the three instruments be respectively,

$$(\text{Current})^2 = k_1\theta,$$
$$\quad ,, \quad = k_2\theta,$$
$$\quad ,, \quad = k_3\theta.$$

Let C be the capacity of the condenser.
R ,, ,, resistance of the electro-magnet.
T ,, ,, semi-period.

Then the entire power at work beyond the terminals, *i.e.* the heating of the wire, the heating of the core by induced currents, and the heating of the core due to hysteresis is expressed by the simple formula—

$$\frac{T}{\pi C}\sqrt{k_1k_2\alpha_1\alpha_2 - k_3^2\alpha_3^2}.$$

The expression itself is independent of the resistance, but if we desire to know the power heating the core, we must deduct from the above the power heating the wire, viz.:

$$k_2\alpha_2 R.$$

The difference between these two quantities also happens to be proportional to the tangent of the magnetic lag, another proof of the universal concurrency of lag and loss of power.

Royal Naval College, Greenwich, October.

[1] By T. H. Blakesley, M.A., M.Inst.C.E., Hon. Secretary of the Physical Society.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Cambridge.—Dr. A. Macalister has been elected Chairman of Examiners for the Natural Sciences Tripos. Dr. C. H. Ralfe has been appointed an additional examiner in Medicine. Prof. Hughes has been elected a member of the General Board of Studies, Mr. J. Prior and Mr. C. Geldard members of the Botanic Garden Syndicate, Dr. Cayley a member of the Library Syndicate, Prof. J. J. Thomson and Mr. H. F. Newall members of the Observatory Syndicate, Dr. Bradbury and Dr. Ingle members of the State Medicine Syndicate, Mr. L. Humphry a member of the Special Board for Medicine, Mr. W. N. Shaw of the Fire Prevention Syndicate, Dr. Besant of the Mathematical Board, Mr. Newall of the Physics and Chemistry Board, and Dr. Gaskell of the Biology Board. Mr. E. H. Griffiths, Lecturer at Sidney, has been approved as a Teacher of Physics with reference to the regulations for medical degrees.

SOCIETIES AND ACADEMIES.

London.

Royal Society, November 20.—"On the Specific Heats of Gases at Constant Volume. Part I. Air, Carbon Dioxide, and Hydrogen." By J. Joly, M.A., B.E., Assistant to the Professor of Civil Engineering, Trinity College, Dublin. Communicated by Prof. Fitzgerald, M.A., F.R.S., F.T.C.D.

In this first notice the specific heats, at constant volumes, of air, carbon dioxide, and hydrogen are treated over pressures ranging from 7 to 25 atmospheres. The range of temperature is not sensibly varied. It is found that the specific heats of these gases are not constant, but are variable with the density. In the case of air the departure from constancy is small and positive; that is, the specific heat increases with increase of the density. The experiments afford directly the mean value 0·1721 for the specific heat of air at the absolute density of 0·0205, corresponding to the pressure of 19·51 atmospheres. A formula based on the variation of the specific heat with density observed in the experiments ascribes the value 0·1715 for the specific heat at the pressure of one atmosphere. The formula assumes the specific heat to be a linear function of the density, which must as yet be regarded only as an approximation, the exact nature of the relation being concealed by variations among the experiments.

These results appear to be in harmony with the experiments of Wiedemann on the specific heat at constant pressure, and of Rowland on the mechanical equivalent of heat, from which the value 0·1712 is deduced for C_v at 760 mm.

The experiments on carbon dioxide reveal a more rapid variation of the specific heat with density, the variation in this case being again positive in sign. The formula

$$C_v = \rho \times 0{\cdot}2064 + 0{\cdot}16577$$

appears with considerable reliability to express the relation between specific heat and density.

The relation between specific heat and density in the case of hydrogen is of a negative character; that is, the specific heat diminishes with increase of density. The experiments are chiefly directed to elucidate this point, for, owing to the difficulty of preparing pure hydrogen, it was found that variations in the quantitative results of experiments on different samples of the gas were unavoidable. Accordingly the experiments were directed to a comparison of the specific heats of like samples of the gas at different densities. The variation with density is small, but (with one exception) all experiments on the purer hydrogen ascribe a negative character to it.

The nature of these variations of specific heat with change of density is, in the case of the three gases, in accord with their behaviour as regards Boyle's law, within the range of pressure.

The experiments were effected in the steam calorimeter, a differential method being used in which an empty or idle vessel is thermally compared with the vessel holding the gas at high pressure. The vessels possessing approximately the same calorific capacity, the result, theoretically, is as if the gas was dealt with isolated from any containing vessel. Although practically this is not attained, many sources of error are eliminated by the procedure adopted.

November 27.—"On the Homology between Genital Ducts and Nephridia in the Oligochæta." By Frank E. Beddard, M.A., Prosector of the Zoological Society. Communicated by Prof. E. Ray Lankester, M.A., LL.D., F.R.S.

It is usually stated in text-books that the genital ducts of the Oligochæta are homologous with nephridia; but nevertheless the question is one which has not yet been satisfactorily settled, for the total independence of the two structures in *Lumbricus* and those aquatic Oligochæta of which the development is known is a difficulty in the way of accepting this view. Claparède, who first clearly formulated the arguments in favour of regarding the genital ducts as slightly modified nephridia, made a mistake in stating that the genital segments of the aquatic Oligochæta contain no nephridia; this error was pointed out by Vejdovsky, who discovered that the genital segments are originally furnished with nephridia, which atrophy on the ripening of the sexual products and the appearance of their ducts. Prof. Lankester pointed out that in *Lumbricus* the genital ducts and the nephridia have a close relation to one or other of the two pairs of setæ with which each segment is provided. He suggested that the genital ducts might represent the only portion left of a ventrally opening series of nephridia. M. Perrier's memorable investigations into the structure of exotic earthworms tended at first to confirm this theory. He discovered that in one earthworm (*Plutellus*) the nephridia alternated in position from segment to segment, thus suggesting that the supposed original two sets of nephridia had both partly persisted and partly disappeared. In other forms the nephridia were found to be related to the ventral setæ, and the genital apertures to the dorsal setæ, the exact converse of the condition which occurs in *Lumbricus*. Later investigations, however, which resulted in the discovery that the genital apertures and nephridiopores may coincide at the same setæ, led M. Perrier to abandon the hypothesis. My own discovery, first published in the Proceedings of this Society, that in *Acanthodrilus multiporus* there are more than a single pair of nephridiopores to each segment, removed the difficulties urged by Perrier. And as this discovery has been extended by myself and by others to many species and genera of earthworms, there can be no longer any intrinsic improbability] in the hypothesis. The whole subject has been lately reviewed by Eisig in his treatise upon the anatomy and physiology of the Capitellidæ, which forms one of the series of monographs issued by the Zoological Station at Naples. Dr. Eisig decides that the genital ducts are probably modified nephridia in the Oligochæta; in the Capitellidæ they certainly are; but, as the Capitellidæ do not appear to me to be so nearly related to the Oligochæta as Dr. Eisig considers, I should regard this argument as only having the force that an argument from analogy can have. Since the appearance of Dr. Eisig's work, an important paper by Dr. Stolc, dealing with the generative organs of *Æolosoma*, has come into my hands; it appears that in this Annelid there are no special sperm ducts, but that the function of such ducts is performed by several pairs of nephridia. This fact, however, interesting though it is, is not a proof of the homology between sperm ducts and nephridia in other types.

I have lately had the opportunity of studying the development of the New Zealand species *Acanthodrilus multiporus*. The sum of money which the Government Grant Committee of the Royal Society were good enough to place at my disposal has enabled me to defray the expenses of this investigation.

In the young embryos of this worm each segment is furnished with a pair of nephridia, each opening by a ciliated funnel into the segment in front of that which carries the dorsally placed external pore. In later stages the funnels degenerate, and that portion of the tube which immediately follows the funnel becomes solid, losing its lumen; at the same time the nephridium branches, and communicates with the exterior by numerous pores. At a comparatively early stage, four pairs of gonads are developed in segments X.–XIII.; each of these is situated on the posterior wall of its segment, as in *Acanthodrilus annectens*, and not on the anterior wall, as in the majority of earthworms. When the gonads first appear, the nephridial funnels, with which they are in close contact, are still ciliated, and their lumen is prolonged into the nephridium for a short distance. Later the cilia are lost, and the funnels increase greatly in size, while those of neighbouring segments—in fact, all the remaining funnels—remain stationary for a time, and then become more and more degenerate. The large funnels of the genital segments become the funnels of the vasa differentia and oviducts; it will be observed that the number of ovaries and oviducal funnels (*two* pairs) at first corresponds to that of the testes and sperm duct funnels; subsequently the gonads and commencing oviducts of segment XII. atrophy. Each of these large funnels is continued into a solid rod which passes back through the septum, and then becomes continuous with a coiled tuft of tubules, in which there is an evident lumen, and which is a part of the nephridium of its segment. In the segments in front of and behind the genital segments, the rudimentary funnels communicate in the same way with a solid rod of cells which runs straight for a short distance and then becomes coiled and twisted upon itself and provided with a distinct lumen. In fact, apart from the relative size of the funnels and the presence of the gonads, it would be impossible to state from which segment a given section through the terminal portion of a nephridium had been taken. In a later stage the large funnels of the genital segments become ciliated; but this ciliation takes place before there is any marked change in the tube which is connected with the funnel.

In the young worm which has just escaped from the cocoon the funnels are ciliated, and they are each of them connected by a short tube, in which a lumen has been developed, but which ends blindly in close proximity to a coil of nephridia. No trace of any nephridial tube other than the sperm duct or oviduct could be observed, whereas in the preceding and succeeding segments the rudimentary nephridial funnel, and a straight tube leading from it direct to the body wall, was perfectly plain. Dr. Bergh has figured, in his account of the development of the generative organs of *Lumbricus*, a nephridial funnel in close contact with the funnel of the genital duct. It may be suggested that a corresponding funnel has been overlooked in the embryo *Acanthodrilus*; the continuity of a structure, identical (at first) with the nephridia of the segments in front and behind, with the genital funnels, seems to show that a search for a small nephridial funnel would be fruitless.

I can only explain these facts by the supposition that *in Acanthodrilus multiporus the genital funnels and a portion at least of the ducts are formed out of nephridia*. This mode of development is a confirmation, to me unexpected, of Balfour's suggestion that in the Oligochæta the nephridium is broken up into a genital and an excretory portion.

In the comparison of the facts, briefly described here, with the apparently independent origin of the generative ducts in other Oligochæta, it must be borne in mind that in *Acanthodrilus* the segregation of the nephridium into several almost detached tracts communicating with the exterior by their own ducts precedes the formation of the genital ducts.

"The Patterns in Thumb and Finger Marks: on their Arrangement into naturally distinct Classes, the Permanence of the Papillary Ridges that make them, and the Resemblance of their Classes to ordinary Genera." By Francis Galton, F.R.S.

The memoir describes the results of a recent inquiry into the patterns formed by the papillary ridges upon the bulbs of the thumbs and fingers of different persons. The points especially dwelt upon in it are the natural classification of the patterns, their permanence throughout life, and the apt confirmation they afford of the opinion that the genera of plants and animals may be isolated from one another otherwise than through the influence of natural selection.

The origin of the patterns was shown to be due to the existence of the nail, which interfered with the horizontal course of the papillary ridges, and caused those near the tip to run in arches, leaving an interspace between them and the horizontal ridges below. This interspace was filled with various scrolls which formed the patterns. The points or point at which the ridges diverged to inclose the interspace were cardinal points in the classification. It was shown that there were in all only nine possible ways in which the main features of the inclosure of the interspace could be effected. In addition to the nine classes there was a primary form, occurring in about 3 per cent. of all the cases, in which the interspace was not clearly marked, and from this primary form all the other patterns were evolved. The forms of the patterns were easily traced in individual cases by following the two pair of divergent ridges, or the one pair if there was only one pair, to their terminations, pursuing the innermost branch whenever the ridge bifurcated, and continuing in an adjacent ridge whenever the one that was being followed happened to come to an end. Twenty-five of the principal patterns were submitted, and a few varieties of some of them, making a total of forty. They are by no means equally frequent.

The data as to the permanence of the patterns and of the ridges that compose them were supplied to the author by Sir W. J. Herschel, who, when in the Indian Civil Service, introduced in his district the practice of impressing finger marks as a check against personation. Impressions made by one or two fingers of four adults about thirty years ago, and of a boy nine years ago, are compared with their present impressions. There are eight pairs of impressions altogether, and it is shown that out of a total of 296 definite points of comparison which they afford, namely the places where ridges cease, not one failed to exist in both impressions of the same set. In making this comparison no regard was paid to the manner in which the several ridges appear to come to an end, whether abruptly or by junction with another ridge. The reason was partly because the neck where junction takes place is often low, and may fail to leave a mark in one of the impressions.

Lastly, the various patterns were shown to be central typical forms from which individual varieties departed to various degrees with a diminishing frequency in each more distant degree, whose rate was in fair accordance with the theoretical law of frequency of error. Consequently, wide departures were extremely rare, and the several patterns corresponded to the centres of isolated groups, whose isolation was not absolutely complete, nor was it due to any rounding off by defined boundaries, but to the great rarity of transitional cases. This condition was brought about by internal causes only, without the least help from natural selection, whether sexual or other. The distribution of individual varieties of the same patterns about their respective typical centres was precisely analogous in its form, say, to that of the shrimps about theirs, as described in a recent memoir by Mr. Weldon (Roy. Soc. Proc., No. 291, p. 445). It was argued from this, that natural selection has no monopoly of influence either in creating genera or in maintaining their purity.

"The Conditions of Chemical Change between Nitric Acid and certain Metals." By V. H. Veley, M.A.

This paper is in continuation of a preliminary communication on the same subject. The main points contained in it are as follows:—

(1) The metals copper, mercury, and bismuth do not dissolve in nitric acid of about 30 per cent. concentration (the acid commonly employed for the preparation of nitric oxide gas) and heated to a temperature of 30° C., provided that nitrous acid is neither present initially nor formed subsequently. To prevent this, it is necessary in the cases of copper and bismuth to add a small quantity of some oxidizing substance, such as hydrogen peroxide or potassium chlorate, or, as less efficacious, potassium permanganate, or to pass a current of air, or, lastly, such a substance as urea, which destroys the nitrous acid by its interaction.

(2) If the conditions are such that these metals dissolve, then the amount of metal dissolved and the amount of nitrous acid present are concomitant variables, provided that the nitric acid is in considerable excess. Change of conditions, such as concentration of acid and variation of temperature, which increase the former increase also the latter.

(3) If the conditions are such that these metals dissolve, it would appear that the metallic nitrite is at first formed, together with nitric oxide; the former is decomposed by the excess of nitric acid to liberate nitrous acid, while the latter reduces the nitric acid to form a further quantity of nitrous acid.

Eventually the net result is the product of two reverse chemical changes represented by the equations:—

(i.) $2NO + HNO_3 + H_2O = 3HNO_2$,
(ii.) $3HNO_2 = 2NO + HNO_3 + H_2O$.

The nitrous acid is thus destroyed as fast as it is generated.

(4) If the conditions are such that metals dissolve in nitric acid, then nitrous acid is invariably the initial product of reduction.

(5) The metals copper, mercury, and bismuth dissolve very readily in a 1 per cent. solution of nitrous acid; under these conditions nitric acid present in slight excess interferes with, rather than promotes, the chemical change. This result is probably due to the greater stability of nitrous acid in the presence of nitric acid.

(6) Hydrogen gas reduces nitric to nitrous acid in presence of cupric or lead nitrate; it also converts mercuric into mercurous nitrate, but does not produce any change in solutions of bismuth and zinc nitrates dissolved in nitric acid.

Physical Society, November 14.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—The following communications were made:—On certain relations existing among the refractive indices of the chemical elements, by the Rev. T. Pelham Dale. The first part of the paper corroborates the results announced in a communication made in May 1889 on the same subject, and says that as far as experimental data are forthcoming the refraction $(\mu - 1)$ divided by the vapour density (d) is equal to a constant multiplied by some integer. Several metals whose refractions have since been determined conform to this law. On examining the relation between molecular weight (M) and refraction, similar conclusions are arrived at; for to a fair degree of approximation the ratio $M/(\mu - 1)$ is a constant, or a simple multiple of this constant. The question as to how far the relation $(\mu - 1)/d = c$ holds good for the same element in the three states of vapour, liquid, and solid, has been examined as far as data exist for this purpose. The resulting numbers are not identical, but some of the data themselves are doubtful. Another relation is between the molecular distances (h) (see Proc. Phys. Soc., vol. ix. p. 167) and the atomic weight (a) of the elements, h being nearly proportional to $\sqrt{a}$. In the case of selenium, sulphur, and phosphorus, the agreement is close, but for bromine, chlorine, and carbon, not so good. A fifth relation appears to exist between the upper limit of refraction and the line spectra of elements. For example, the upper limit of refraction for selenium occurs at wave-length 5295·7, whilst its spectrum exhibits a remarkable series of strong lines about this wave-length. A similar relation apparently holds with sulphur, phosphorus, and bromine. Gold also shows a series of strong lines about c, in the vicinity of which the metal has the greatest reflective power. The author finds that selenium polarizes and reflects nearly all the light that falls on it at a large angle, and suggests that it may be used in polariscopes. He has also endeavoured to connect together the phenomena of a limit of refraction and anomalous dispersion. In the case of fuchsin the dark space coincides with the limit of refraction, and the same is probably true of cyanin. If one of the anomalous indices be given the other can be found. He also believes that bodies of high molecular weight give anomalous dispersion, and thinks solutions of iodine will exhibit the phenomenon. The mathematical investigation of the whole subject involved difficulties arising from the want of trustworthy data, and the author hopes that some member will take up the necessary experimental determinations. Dr. Gladstone thought the author under-estimated the amount of work done and in progress on the subject, for the question whether $(\mu - 1)/d$ is constant or not is being investigated by many. The French physicists, he said, had found the quantity nearly constant, but Lorenz's expression $(\mu^2 - 1)/(\mu^2 + 2)$ is slightly better when applied to compounds in the liquid and gaseous states. Metals were difficult to deal with, especially as, according to the recent paper of Du Bois and Reubens, their refractions do not follow the law of sines. Mr. Dale here suggested that they might be related to hyperbolic sines. Dr. Gladstone, continuing, said that, by taking solutions of metals, it was found that their specific refractive energies were nearly inversely as the square roots of their combining weights, but at present the known cases were not sufficient to establish a law. Prof. Rücker said that of the two expressions, $(\mu - 1)/d$ and $(\mu^2 - 1)/(\mu^2 + 2)$, the latter seemed preferable, for it could be converted into electrical quantities by writing K for μ^2. The expression then becomes $(K - 1)/(K + 2)$, and if this can be shown to be constant by electrical work, this would be an argument in its favour. On the subject of anomalous dispersion he directed Mr. Dale's attention to Mr. Glazebrook's Report on Optical Theory made to the British Association. Mr. Dale, in reply, pointed out that, from the nature of the two formulæ, any inaccuracy or variation in μ would affect theirs more than Lorenz's. He also thought that $(\mu - 1)/d$ was a limit towards which the numbers tend.—Tables of spherical harmonics, with examples of their practical use, by Prof. J. Perry, F.R.S. The author defined a spherical harmonic as a homogeneous function of x, y, z, satisfying the equation—

$$\frac{d^2V}{dx^2} + \frac{d^2V}{dy^2} + \frac{d^2V}{dz^2} = 0,$$

stated the fundamental properties of such functions, and pointed out their importance in problems on heat, electricity, and hydrodynamics. Referring to zonal harmonics (homogeneous functions of $(x^2 + y^2)^{\frac{1}{2}}$ and z), he showed that these harmonics are symmetrical with respect to the axis of z, and might be expressed as functions of the angle (θ) which the line joining the point (x, y, z) to the origin makes with the axis of z, multiplied by r^i; where r is the radius-vector and i the degree of the homogeneous function. These functions of θ are called zonal surface harmonics, and are designated by $P_0, P_1, P_2, \ldots P_i$, according to the degree of the function, and it was the values of these quantities which form the tables brought before the Society. The tables comprise the values of P_1 to P_8, and are calculated to 4 places of decimals and for every 1° between 0° and 90°. As an example of the use of such tables, the case of a spherical surface covered with attracting matter whose density varied as the square of its distance from a diametral plane was taken. It was required to find the potential both outside and inside the sphere, and to determine the equipotential surfaces and lines of force. The potentials inside (A) and outside (B) were shown to be given by

$$\frac{A}{\pi} = 8 + \frac{16}{5} r^2 P_2 \quad \text{and} \quad \frac{B}{\pi} = \frac{8}{r} + \frac{16}{5} \frac{1}{r^3} P_2$$

respectively. By giving A and B definite values, and choosing values of r, the corresponding P_2's can be calculated, and the value of θ determined from the tables. Hence any equipotential surface can be easily determined, and lines drawn to cut these surfaces orthogonally are lines of force. Another problem which had been tried consisted in finding the directions of the lines of force near a circular coil of rectangular cross-section when an electric current circulates in the coil. This was treated approximately by first calculating the potential at 6 points along the axis in the neighbourhood of the coil, and then finding by Gauss's method the coefficients A_0, A_1, A_2, &c., of an expression in ascending powers of z which agreed with the calculated potentials at the points chosen. The formula

$$V = A_0 + A_1 r P_1 + A_2 r^2 P_2 + \&c.,$$

or its corresponding expression in inverse powers of r, was then assumed to give the potential at any point in the space considered. By giving V definite values, a series of equipotential surfaces were determined, and the lines of force drawn. On putting the calculations to the test of experiment, the approximate solution of this very difficult problem was found to be very nearly correct.

Linnean Society, November 20.—Prof. Stewart, President, in the chair.—Mr. G. Murray exhibited specimens of a fresh-water *Delesseria* previously unknown.—On behalf of Mr. Henry Hutton, of Cape Town, Mr. B. D. Jackson exhibited some follicles and seeds of a somewhat rare Asclepiad (*Dregia floribunda*); and showed also, on behalf of Mr. W. Matchwick, some ripened seeds of *Ailanthus glandulosa* from a tree at Reigate said to be a hundred years old.—Prof. Bower exhibited several drawings from microscopic sections of Carboniferous nodules belonging to Prof. Williamson, and pointed out the peculiarities of structure. Microscopic details of such sporangia being very rare, he remarked that a comparison of the slides showed a peculiar uniformity of type. For comparison with these sporangia from the coal, he exhibited sections of the sporangia of *Todea barbara*, and while not going so far as to refer these Carboniferous sporangia (which are not attached to the plants which bore them) to any distinct genus, he thought the Osmundaceous affinity was unmistakable.—Mr. J. E. Harting exhibited some original MSS. and water-colour sketches of birds, fishes, and plants, found in Sussex by William Markwick, the friend and correspondent of Gilbert White, of Selborne, which had been presented by him to the Society in his lifetime, and had been lost sight of for many years. The drawings are sufficiently well executed to enable the correct determination of several species which the author had failed to identify.—A paper was read by Prof. T. Johnson, of Dublin, on *Punctaria*, a genus of brown seaweeds (*Phæophyceæ*). He described in detail the formation of the plantlets by trichothallic germination from the tufts of primary hairs and from the secondary hairs formed on epicortical filaments on the old thallus of *P. plantaginea* and *P. latifolia*. He pointed out that neither the nature of the dots nor the position of the sporangia is of specific value, and that sporangiferous and plant-forming hairs are to be found on the filamentous root-disk of *P. tenuissima*, Grev., which, he maintained, is the young state of one species, of which *plantaginea* and *latifolia* are the more mature growths.—Mr. Vaughan Jennings gave an abstract of a paper on a variety of sponge, *Alectona millari*, Carter, boring in the shell of *Lima excavata*, from the Norwegian coast. The sponge had endeavoured to grow inwards, dissolving the nacreous layer and encroaching on the mollusk, instead of restricting its wanderings to the thickness of the shell. The mollusk had retaliated by depositing fresh layers on the intruder, and the struggle had gone on until the chambers were several times the normal thickness of the shell, and were roofed over by a thin curved layer of secondary shell substance, while the points at which branches had been pushed further in were represented by thick conical papillæ.

CAMBRIDGE.

Philosophical Society, November 10.—Prof. Darwin, President, in the chair.—The following commuications were made:—Note on the principle upon which Fahrenheit constructed his thermometrical scale, by Arthur Gamgee, F.R.S., Emeritus Professor of Physiology in the Owens College (Victoria University). The author commenced by drawing attention to the fact that, although the Fahrenheit thermometer has been so generally used in England, no accurate information was to be found in our text-books concerning the principles upon which its scale had originally been constructed. He referred, however, to a view advanced by Prof. P. G. Tait in his elementary treatise on "Heat," and which had been accepted by many teachers, according to which Fahrenheit divided his scale between 32° and 212° into 180 degrees, in imitation of the division of a semicircle into 180 degrees of arc. This theory rested on the incorrect supposition that, before Fahrenheit's time, Newton had suggested, as a basis for a thermometric scale, the fixing of the freezing and boiling points of water, the space between these being divided into a number of equal degrees. The author pointed out that in his "Scala graduum caloris," Newton made no such suggestion as that attributed to him by Prof. Tait, and prior to him by Prof. Clerk Maxwell; and, indeed, that Fahrenheit had settled the basis of his scale and had constructed a large number of thermometers, which were used by scientific men throughout Europe, many years before the discovery by Amanton (which Fahrenheit confirmed and gave precision to) of the fact that under a constant pressure the boiling point of water is constant. The author stated that the thermometers which were first constructed by Fahrenheit were sealed alcoholic thermometers, provided with a scale in which two points had been fixed. The zero of the scale, representing the lowest attainable temperature, was found by plunging the bulb of the thermometer in a mixture of ice and salt, whilst the higher of the two points was fixed by placing the thermometers under the armpit or inside the mouth of a healthy man. The interval between these two points was, in the first instance, divided into 24 divisions, each of which corresponded to supposed well-characterized differences in temperature, and each being subdivided into four. In his later alcoholic and mercurial thermometers, the 24 principal divisions were suppressed in favour of a scale in which 96 degrees intervened between zero and the temperature of man; in these later thermometers the 32nd degree was fixed by plunging the bulb of the thermometer in melting ice. The author then pointed out that Fahrenheit was led to construct mercurial thermometers in order to be able to ascertain the boiling point of water; with this object the scale, constructed as has been stated, was continued upwards, in some cases so as to include 600 degrees. It was *as the result of experiment alone*, that the number 212 was obtained as the temperature at which water boils, at the mean, atmospheric pressure. The author in conclusion argued that Fahrenheit took as the basis of his thermometric scale the duodecimal scale, which he was constantly in the habit of employing.—On variations in the floral symmetry of certain plants with irregular corollas, by Mr. W. Bateson and Miss A. Bateson.—On the nature of the relation between the size of certain animals and the size and number of their sense-organs, by H. H. Brindley. In speculation as to the evolution of various forms, it is generally held as a principle that the conditions of the struggle for existence are such that variations in the direction of atrophy or diminution in bulk of a useless organ must necessarily be beneficial by reason of the saving of tissue and effort which is effected by this reduction. It has been assumed by many that this benefit must be so marked as to lead to the natural selection of the individuals thus varying. This principle has been invoked especially in the case of sense-organs, and, for example, it has been suggested that the blindness of cave-fauna may have come about by its operation. With the object of testing the truth of this assumption, it seemed desirable to obtain a knowledge of the normal variations in size and number of sense-organs occurring within the limits of a single species. The cases chosen were (1) the olfactory organ of fishes (eel, loach, *Pleuronectidæ*, &c.), and (2) the eyes of *Pecten opercularis*. In the first case tables were given showing that large individual fluctuations occur, but that on the whole the number of olfactory plates increases with the size of the body; and it was pointed out that a similar relation holds with regard to the eyes of fishes. In the case of *Pecten*, however, though the *size* of the eyes increases with the diameter of the animal, yet in specimens having a diameter of 3 cm. to 6 cm. the *number* of the eyes is not thus related (cp. Patten), but varies in a most surprising and, as it were, uncontrolled manner. Statistics were given showing that, in individuals of the same size, the number of eyes may vary between 70 and 100, and that no uniformity is to be found. It was pointed out that these eyes are large and complicated organs, having lens, retina, tapetum, &c., involving great cost in their production. These facts suggest that the "economy of growth" cannot be a principle of such precise and rigid character as to warrant its employment as a basis for speculation as to the mode of evolution of a species. The diverse results in the case of the two sets of organs examined further indicate that the problem is one of far greater complexity, and shows clearly that argument from analogy is inadmissible in these cases.—On the oviposition of *Agelena labyrinthica*, by C. Warburton. The oviposition and cocooning of *Agelena labyrinthica* is a striking case of the performance of a series of complicated operations in obedience to a blind instinct. The eggs are always laid at night, but the presence of artificial light is quite disregarded by the animal. For about 24 hours before laying, the spider is engaged in preparing a chamber for the purpose. Near its roof a small sheet is then formed, and the eggs are laid upwards against it and are covered with silk. A box is then constructed with this sheet as its roof, and is firmly attached by its angles to the roof and floor of the chamber. This box is constructed and jealously guarded even if the eggs are removed immediately on oviposition. The whole operation involves about 36 hours of almost incessant industry.—Supplementary list of spiders taken in the neighbourhood of Cambridge, by C. Warburton.

Paris.

Academy of Sciences, November 24.—M. Hermite in the chair.—Experiments on the mechanical actions exerted on rocks by gas at a high pressure and in rapid motion, by M. Daubrée. The working of diamond mines in South Africa has revealed the existence of vertical canals or chimneys in the earth's crust. All these chimneys are circular or elliptical in section. Their diameter varies from 20 metres to 450 metres, and is generally comprised between 150 metres and 300 metres. Their depth is considerably greater. M. Daubrée finds that he can produce precisely similar formations by means of the gas evolved when dynamite or gun-cotton is exploded, and therefore believes that the chimneys referred to are produced by the action of gases at high pressures and velocities. That the effect may be considerable is evident from a consideration of the action of gases at high pressure upon the meteorites that traverse our atmosphere.—On some facts relative to the history of carbon, by MM. Paul and Léon Schutzenberger.—On the relation of the circumference to the diameter, by Prof. Sylvester. Lambert has shown that π cannot be the square root of a whole number. The author proves that π cannot be the root of a rational equation.—Observations of Zona's comet (November 15, 1890) made at Paris Observatory with the West Tower equatorial, by M. G. Bigourdan. Three observations for position were made on November 21.—Observation of the same comet made with the East Tower equatorial, by Mdlle. D. Klumpke. An observation for position was made on November 21.—Generalization of one of Abel's theorems, by M. Albert La Maestra.—Variations of conductivity under different electrical influences, by M. Ed. Branly. If a circuit be formed with a Daniell's cell, a high resistance galvanometer, and a very thin layer of copper deposited on a ground glass or ebonite plate, 7 cm. long and 2 cm. wide, only an insignificant current passes. An abrupt diminution of resistance is experienced, however, when one or more electric discharges, from a Wimshurst machine or a Ruhmkorff's coil, are produced in the neighbourhood of the circuit. The action diminishes as the distance of the sparking apparatus increases, but it may be very easily observed, without special precautions, at several metres. The author has examined the conditions necessary to produce the observed phenomena.—Periodic visibility of interference fringes, by M. Charles Fabry.—On the artificial production of a chromium blue, by M. Jules Garnier. By heating in a brasqued crucible a mixture of K_2CrO_4 48·62 grammes, fluor spar 65 grammes, and silica 157 grammes, the author obtained a blue glass, the colour of which he attributes to reducing action taking place at a high temperature in the chromium salt.—Researches on the application of the measure of rotatory power to the determination of combinations formed by aqueous solutions of malic acid with the double molybdate of potassium and sodium, and the acid molybdate of sodium, by M. D. Gernez.—On the employment of the potato in the agricultural distillery in France, by M. Aimé Girard. The French potato crop in 1890 was remarkable for quantity and quality. The author urges the application of these crops to the production of alcohol, and experimentally proves that such a use may be commercially successful.—On the spermatozoa of Locustides, by M. Armand Sabatier.—On *Cyclatella annelidicola* (van Beneden and Hesse), by M. Henri Prouho.—On the destruction of *Heterodera schachtii*, by M. Willot.—On an eruptive rock from Ariège, and on the transformation of felspar into wernerite, by M. A. Lacroix.—On a tornado observed at Fourchambault (Nièvre), by M. Doumet-Adanson.

Göttingen.

Royal Society of Sciences.—The *Nachrichten* from January to June 1890, contain the following papers of scientific interest:—

January.—B. Galitzine, on Dalton's law. The author reviews the different estimates taken of Dalton's law, and concludes from his experiments that it is not accurately true either as regards the resultant pressure of a mixture of gases, or as regards the pressure of vapours.

February.—Nernst, on a new principle in the determination of molecular weights. The author gives the results of experiments conducted to verify the familiar formula $a - a'/a' = n/100$.

March.—Felix Klein, on the theory of Lamé's functions. This is an account of lectures in continuation of previous courses, and is concerned with the extension of Lamé's equation, the replacing of his functions by algebraic forms, and the conformable representation of a quotient of two solutions. Hertz, on the fundamental equations of electrodynamics. This is an attempt to explain the method of proof and the significance of Maxwell's equations.

May.—Voigt, on the coincidence of two simple tones—being a reconciliation of the formulæ obtained by Helmholtz and R. König. Hartlaub, contribution to the knowledge of the *Comatulidæ* of the Indian Archipelago. Riecke, the pyro-electricity of tourmaline. The object is twofold—to examine the correctness of the formula $\epsilon = E(1 - e^{-at})$ for the electric charge during the cooling (where t denotes the time), and to find the dependence upon the difference of the initial and final temperatures of the electricity developed during the cooling.

June.—Riecke, on W. Gibb's theory of the changes of state of a system; geometrical development of the same. Fr. Brioschi, on a transformation of the differential equation of the even sigma-function of two variables; deduction of the development. Schoenflies, the mutual relations of the various theories of crystalline structure; a general account.

CONTENTS.