THURSDAY, JANUARY 27, 1887

SCIENTIFIC FEDERATION

IN an article on "Science and the Jubilee" a week or two ago, we referred to the possibility that the Royal Society might feel it desirable to consider whether it was feasible to signalise the present year of Jubilee by any new departure. It so happens that quite independently of the proposed celebration a very appropriate extension of the Society's usefulness to our colonies has been suggested and has already been accepted by one of the Australian colonies. This suggestion, and the action which the Royal Society has already taken upon the question submitted to it, really raises the whole question of the desirability of a scientific confederation of all English-speaking peoples.

The suggestion to which we refer was made in Prof. Huxley's Anniversary Address to the Royal Society little over a year ago, from which we [make the following extract:—

"Since this Society was founded, English-speaking communities have been planted, and are increasing and multiplying, in all quarters of the globe,—to use a naturalist's phrase, their geographical distribution is 'world-wide.' Wherever these communities have had time to develop, the instinct which led our forefathers to come together for the promotion of natural knowledge has worked in them and produced most notable results. The quantity and quality of the scientific work now being done in the United States moves us all to hearty admiration; the Dominion of Canada, and our colonies in South Africa, New Zealand, and Australia, show that they do not mean to be left behind in the race; and the scientific activity of our countrymen in India needs no comment.

"Whatever may be the practicability of political federation for more or fewer of the rapidly-growing English-speaking peoples of the globe, some sort of scientific federation should surely be possible. Nothing is baser than scientific Chauvinism, but still blood is thicker than water; and I have often ventured to dream that the Royal Society might associate itself in some special way with all English-speaking men of science, that it might recognise their work in other ways than by the rare opportunities at present offered by election to our foreign Fellowship, or by the award of those medals which are open to everybody; and without imposing upon them the responsibilities of the ordinary Fellowship, while they must needs be deprived of a large part of its privileges. How far this aspiration of mine may be reciprocated by our scientific brethren in the United States and in our colonies I do not know. I make it public, on my own responsibility, for your and their consideration."

It would appear that the matter was at once considered by the Council of the Royal Society, because the next year (1886) Prof. Stokes, the present President, referred to the subject in the following words:—

"In his Presidential Address last year, Prof. Huxley suggested the idea, I may say expressed the hope, that the Royal Society might associate itself in some special way with all English-speaking men of science; that it might recognise their work in other ways than those afforded by the rare opportunities of election to our foreign membership, or the award of those medals which are open to persons of all nationalities alike. This suggestion has been taken up by one of our colonies. We have received a letter from the Royal Society of Victoria, referring to this passage in the address, and expressing a hope that, in some way, means might be found for establishing some kind of connection between our own oldest scientific Society and those of the colonies. The Council have appointed a Committee to take this letter into consideration, and try if they could devise some suitable plan for carrying out the object sought. The Committee endeavoured at first to frame a scheme which should not be confined to the colonies and dependencies of the British Empire, but should embrace all English-speaking communities. But, closely connected as we are with the United States by blood and language, they are of course, politically, a foreign nation, and this fact threw difficulties in the way of framing at once a more extended scheme, so that the Committee confined themselves to the colonies and dependencies of our own country, leaving the wider object for some future endeavour, should the country concerned seem to desire it. The scheme suggested was laid before the members of the present Council, but there was not an adequate opportunity of discussing it, and it will of course come before the next Council. Should they approve of some such measures as those recommended by the Committee, they will doubtless assure themselves, in some way or other, that those measures are in accordance with the wishes of the Fellows at large before they are incorporated into the statutes."

What the Council of the Society has already done in the matter is of course unknown to us, as it has not yet been made public; but it is unnecessary to point out the extreme fitness of some such action as this being taken this year, if it is to be taken at all.

Undoubtedly the scheme foreshadowed by Prof. Huxley, if carried out in a proper way, may lead to a great many advantages. It is not unimportant that all the scientific organisations of Greater Britain should be welded into a homogeneous whole, so that, if at any time a common action should be necessary on any subject, the work could be done promptly and with the least strain. If any scientific organisation in a colony were affiliated with the Royal Society at home, there can be no doubt that it would be in a stronger position; that its standard of scientific work would be raised; that other kindred institutions would be more likely to be formed, on which a similar status might at some future time be conferred also. Such an organisation, too, would have a *cachet* conferred upon it, so that colonists would consider it a greater honour to belong to it, and would have a greater inducement to work for it, and to aid in all its efforts.

We can imagine some possible criticisms of Prof. Huxley's suggestions. For instance, it may be asked, Why should not Scotch and English and Irish organisations be treated in the same way? We think there is a very good answer to this objection. Any member of any of the British Societies, by taking a little trouble, may obtain any of the privileges which the Royal Society might confer upon colonists. To a great many British

Societies the publications of the Royal Society are sent gratuitously; there is no difficulty in obtaining access either to the libraries or to the reading-rooms when the members are in London, for the reason that all necessary knowledge as to how these privileges are to be obtained is of course possessed by those at home, whereas the member of a colonial Society who finds himself in England is in a very different position. He may know nobody, he may not know even of the existence of the facilities afforded, and he may leave England without having been present at any meetings of the Society, and without the knowledge that almost anyone who chooses can attend them. We are glad then on these and on other grounds that the question has been raised, and we believe that great good may be accomplished by acting on Prof. Huxley's suggestion.

SUPERNORMAL PSYCHOLOGY

Phantasms of the Living. By Edmund Gurney, Frederic W. H. Myers, and Frank Podmore. (London: Trübner and Co., 1886.)

UNDER the title "Phantasms of the Living," three of the leading members of the Society for Psychical Research have presented to the world at large, in two bulky volumes running to upwards of 1400 pages, the evidence they have collected in support of the hypothesis of telergy and telepathy, or the influence of one mind on another, near or at a distance, without the intervention of the ordinary channels of sense. The division of labour, for such we may truly term it, seems to have been as follows: Mr. F. W. H. Myers writes an introduction and a concluding chapter on "A Suggested Mode of Psychical Interaction"; Mr. Edmund Gurney is responsible for the compilation of the body of the work, the presentation and criticism of the evidence; while in the collection of evidence and examination of witnesses Mr. Podmore "has borne so large a share, that his name could not possibly have been omitted from the title-page."

It is a matter of peculiar difficulty to do justice, in the space that NATURE can place at my disposal, to a work of such portentous bulk, one written in such obvious good faith, one on which the authors have bestowed so much time, labour, and thought, and yet one presenting views which no one who has learnt to believe in the parallelism or identity of neuroses and psychoses can accept without abjuring his scientific and philosophic faith. I hold it to be the duty of a reviewer not merely to air his own opinions, but to give his readers a sketch of the contents of the volumes before him. But how can one sketch in two or three columns so vast a mass of evidence, the chief value of which is, we are told, its cumulative nature? And if the reviewer owes it to his readers to present some sort of outline of the picture his author presents, he none the less owes it to himself, his author, and his journal, to endeavour to estimate the value of the original thus roughly outlined. Difficult as the task is, it must be faced.

The evidential part of the work opens with a record of cases which form, it is held, an experimental basis for thought-transference. The following description is given by the Rev. H. M. Creery of experiments with his own daughters:

"Each went out of the room in turn, while I and the others fixed on some object which the absent one was to name on returning to the room. We began by selecting the simplest objects in the room, then chose the names of towns, dates, cards out of a pack, &c. I have seen seventeen cards, chosen by myself, named right in succession, without a mistake."

In similar experiments the investigating committee acted as agents. This excluded, in their opinion, the possibility of trickery. Tabulating the results thus obtained, they submitted them to Mr. F. Y. Edgeworth, who applied to them the calculus of probabilities, obtaining "a row of *thirty-four nines* following a decimal point," or practical certainty in favour of their being due either to collusion or to thought-transference.

Details are given of experiments on the transference of tastes under conditions which, in the opinion of the authors, precluded the possibility of collusion or deception. The following are a few successive results:—

Substances tasted	Answers given
Vinegar	A sharp and nasty taste.
Mustard	Mustard.
Sugar	I still taste the hot taste of the mustard.
Worcestershire sauce	Worcestershire sauce.
Port wine (quality not stated!)	Between eau-de-cologne and beer.
Bitter aloes	Horrible and bitter.

Instances of the localisation of pains are given. "The percipient being seated, blindfolded, and with her back to the rest of the party, all the other persons present inflicted on themselves the same pain in the same part of the body. Those who took part in the collective agency were three or more of the following: Mr. Malcolm Guthrie, Prof. Herdman, Dr. Hicks, Dr. Hyla Greves Mr. R. C. Johnson, F.R.A.S., Mr. Birchall, Miss Redmond, and, on one occasion, another lady. The percipient throughout was Miss Relf. In ten out of twenty cases the percipient localised the pain with great precision; in seven, the localisation was nearly exact; in two, no local impression was perceived; and in one, the last, the answer was wholly wrong."

Facsimiles are given of pictures reproduced by thought-transference. In a continuous series of six—none of which can be said to have been complete failures—two were reproduced by the percipient with great fidelity; even the comparative failures are instructive from their partial success. The position of the agent, we are told, rendered it absolutely impossible that she should obtain a glimpse of the original.

Such is some of the experimental evidence for thought-transference. Readers of NATURE will understand why this section of the authors' work, giving results obtained under conditions within control, is noticed at greater length than can be devoted to other branches of the evidence.

The next chapter deals with cases transitional between experimental thought-transference—in which both agent and percipient are voluntarily taking part with a definite idea of certain results in view—and spontaneous telepathy, where neither has voluntarily or consciously formed an idea of any result whatever. These transitional cases are those in which the agent acts consciously and voluntarily, but the percipient is not consciously or voluntarily a party to the experiment. Of these cases, a single example must suffice. Two fellow-students of naval engineering at Portsmouth had been in the habit of making experiments in mesmerism. One, ere long, acquired mesmeric control over the other, who was able to see, in the mesmeric trance, places in which he was interested, if he resolved to see them before he was hypnotised. One day he expressed a wish to see a young lady living in Wandsworth. He was hypnotised; and when he came round, he said he had seen her in the dining-room. A few days afterwards, the experiment was repeated. He saw, as he lay entranced, the young lady in a room with her little brother; she fell back in her chair in a sort of faint. A letter was subsequently received from the young lady, dated the morning following the last experiment, beginning: "Has anything happened to you?" and stating that "she could have declared she saw him looking at her" on two occasions, on the latter of which she was so frightened that she nearly fainted. "Luckily," she adds, "only my brother was there, or it would have attracted attention." Although there is some discrepancy as to the date of the first appearance, the second (January 18, 1886) is accordant.

After the enumeration of fifteen or sixteen transitional cases, Mr. Gurney devotes a chapter to a general criticism of the evidence (to which is added an appendix on witchcraft), and then gives a chapter of specimens of the various types of spontaneous telepathy. For these types and their various sub-classes, the reader must be referred to the work itself. I must here again content myself with quoting a single case (which is both "reciprocal" and "collective") from among the 700 or so that are given.

"On the evening of, I think, March 23, 1883," writes a Mrs. Bettany, of Dulwich, "I was seized with an unaccountable anxiety about a neighbour. I tried to shake off the feeling, but I could not; and after a sleepless night, in which I constantly thought of her as dying, I decided to send a servant to the house to ask if all were well." (This is confirmed by the servant.) "The answer I received was, 'Mrs. J. died last night.' Her daughter afterwards told me that the mother had startled her by saying, 'Mrs. Bettany knows I shall die.'"

Mrs. Bettany adds:—"My cook, to whom I had not mentioned my presentiment, remarked to me on the same morning: 'I have had such a horrible dream about Mrs. J., I think she must be going to die.' She distinctly remembers that some one (she does not know *who*, and I think never did) told her in her dream that Mrs. J. was dead." (This is also confirmed by the cook.)

Of somewhat analogous cases of phantasms, presentiments, or dreams occurring to one or more percipients at or shortly after the death of the agent, there is a surprising but wearisome abundance.

So much for the evidence. The authors are fully alive to its liability to error. But they note that their "somewhat persistent and probing method of inquiry has usually repelled the sentimental or crazy wonder-mongers who hang about the outskirts of such a subject as this; while it has met with cordial response from an unexpected number of persons who feel with reason that the very mystery which surrounds these incidents makes it additionally important that they should be recounted with sobriety and care."

We turn now to the theory; and though Mr. Gurney tells us that the character of the present work is mainly evidential, there is no lack of theory scattered up and down throughout its multitudinous paragraphs. The authors, it need hardly be said, regard their hypothesis as strictly scientific. "We wish distinctly to say," writes Mr. Myers, "that so far from aiming at any paradoxical reversion of established scientific conclusions, we conceive ourselves to be working (however imperfectly) in the main track of scientific discovery."

We must, however, carefully separate the views of Mr. Myers from those of Mr. Gurney. Both of them, of course, insist on the reality of experimental thought-transference and of spontaneous telepathy—the radical difference between which is well brought out. In the one an object or sensation kept steadily before the mind of the agent or agents is transmitted as such to the mind of the percipient; in the other the case is different: not the death-swoon of the agent, but the image of the agent as dying is transmitted. And here it is that our authors begin to part company. Calling to mind the facts (or supposed facts) (1) that the dying man may have in intervals of consciousness a vivid mental picture of himself and his surroundings; (2) that most of us have in the background of consciousness a tolerably well-developed conception of our own proper selves; (3) that there is some experimental evidence of collective telepathic influence, so that the percipient may be jointly influenced by the dying man as principal agent, and by the bystanders at the death-bed as subsidiary agents—taking these, avowedly or implicitly, into consideration, Mr. Gurney does not feel forced to go beyond the theory of thought-transference. Not so Mr. Myers. He rises boldly into what looks uncommonly like spiritualism, and accepts clairvoyance, where the percipient "seems to visit scenes, or discern objects, without needing that those scenes or objects should form a part of the perception or memory of any known mind." "*Correspondently with clairvoyant perception*," he says (the italics are his own), "*there may be phantasmogenetic efficacy*," which in plain English means that the percipient may visit in spirit scenes he has never visited in the flesh, and that his spirit may be visible as a phantasm to the human occupants of these scenes. And in support of his view he adduces such cases as that of the two students which I have summarised above.

On the question of the physical aspect of the psychical phenomena, again, our authors do not agree. Mr. Gurney holds that "mental facts are indissolubly linked with the very class of material facts that science can least penetrate—with the most complex sort of changes occurring in the most subtly-woven sort of matter—the molecular activities of brain-tissue." And though he subsequently says: "Not only, as with other delicate phenomena of life and thought, is the *subjective* side of the problem the only one that we can yet attempt to analyse: we do not even know where to look for the *objective* side:" he rather advocates the limitation of the question for the present to the psychical aspect, than dismisses the physical as a piece of unwarrantable materialism. But Mr. Myers goes further: "The psychical element, I repeat, must henceforth almost inevitably be conceived as having relations which cannot be expressed in terms of matter." And again: "I claim at least that any presumption which science had established against the possibility of spiritual communion is now rebutted; and that the materialist must admit that it is no longer an unsupported dream, but a serious scientific possibility, that, if any intelligences do in fact exist other than those of living men, influences from those intelligences may be conveyed to our own mind."

And now, in conclusion, what shall we say of these ponderous tomes? Shall we lightly dismiss the whole subject as a "pack of nonsense"? I do not think that this would be a wise or a scientific procedure. Speaking for myself, I must confess that, in my opinion, Mr. Myers's views are not "on the main track" of the science of to-day, whatever relation they may hereafter be shown to bear to the science of the future. Speaking for myself again, I am ready to accept experimental thought-transference as a working hypothesis, that is to say, a guide to future research on the subject. It may be that any physical explanation we can at present offer is no nearer the truth than was the Ptolemaic hypothesis in astronomy, and yet such a working hypothesis may be valuable in the existing state of psychology. With regard to spontaneous telepathy, notwithstanding the large amount of evidence so carefully collected and criticised, notwithstanding that I have first-hand evidence more convincing (to *me*) than anything recorded in these volumes, I prefer to credit the whole to a suspense account. The physical difficulties are enormous. We have to conceive the action of brain on brain across a whole hemisphere. Not that this must be pressed too far. There is much that is provisionally accepted by science (much anent ether, and atoms, and modes of molecular action) that I find it exceedingly hard to conceive. And perhaps the distant action of brain on brain is not harder for us to conceive than would be the transmission of luminiferous waves to beings in whom the visual sense was not as yet recognised, and who, hitherto only acquainted with auditory vibrations transmitted by the air, were called upon to believe that waves could be transmitted by the ether from distant stars, and could pass almost unchecked through thick masses of solid material. Still, though the mass of evidence is considerable, and though the physical difficulties must not be pressed too far, I am not prepared fully to accept the doctrine of spontaneous telepathy. At the same time, I hold that the evidence adduced by earnest workers is not to be met by easy and ignorant ridicule. I do not think that science is best served by those who are ever ready to throw the cold water of impossibility on the light of new ideas struggling into existence.

I am, moreover, strongly of opinion that normal psychology has much to learn from experiments on supernormal and abnormal "subjects." Beneath the threshold of consciousness there is a vast amount of sub-conscious and unconscious mental action. Of the multitudinous simultaneous neuroses only the superficial film (so to speak) emerge into the light of consciousness as psychoses. The study from the psychological standpoint of the underlying *hypopsychoses*, as I have elsewhere suggested that they should be termed, is as important as it is difficult. If the result of such work as Messrs. Gurney, Myers, and Podmore have entered upon aids in throwing light upon these hidden mysteries, which are none the less realities, of the human mind, their labour will, in my opinion, not have been in vain.

C. LLOYD MORGAN

ELEMENTARY RESULTS IN PURE MATHEMATICS

A Synopsis of Elementary Results in Pure Mathematics, &c. By G. S. Carr, M.A. Pp. xxxviii. + 936 + 20 folding Plates of Figures. (London: Francis Hodgson, 1886.)

IN our last notice of this work (vol. xxxi. p. 100) we gave an account of Sections X., XI., and XII. The complete volume contains two additional sections. The first of these treats of plane co-ordinate geometry, under which heading we have systems of co-ordinates, analytical conics in Cartesian and trilinear co-ordinates (we miss the *m* equations for the parabola and the corresponding equations for chords, &c.). In the latter division we have, amongst the particular conics considered, the triplicate-ratio and seven-point circles (or, as they are more usually styled, the Lemoine and Brocard circles). The account is carefully drawn up from original authorities, and will help to bring this latest development of the geometry of the circle and triangle more into notice. At present this and Dr. Casey's books are the only source readily accessible to students. We are promised another presentment of these circles shortly, but of this more anon. The concluding portion of this section is devoted to the theory of plane curves. Here we have, *inter alia*, inverse and pedal curves, roulettes, and the various forms of transcendental curves. Considerable space is taken up with linkages and link-works: here we have accounts of Kempe's five-bar linkage, the six-bar inversor, the eight-bar double inversor, the quadruplane, the isoklinostat, the planimeter, and the pantograph (this Mr. Carr generally calls pentograph—evidently he has not consulted the "English Cyclopædia"—and in one place only, pantograph). The concluding section is mainly taken up with solid co-ordinate geometry, the final articles being devoted to Guldin's rules, moments and products of inertia, perimeters, areas, volumes, &c. Here we have the theorems which go by the names of Fagnani, Lambert, and Griffiths (not Griffith, as the "Contents" and "Index" print the name; the text, § 6096, is right).

We have in our former notices sufficiently indicated our opinion of the utility of such a book as this if thoroughly trustworthy, and have suggested that a student should have this synopsis by his side when he is carefully going through his subject, that so he may be able to spot any slight inaccuracy in the text. We believe the book is singularly free from errors, but it would be absurd to suppose that there are not several which have escaped even the notice of the author, who has imposed upon himself numerous guards for the prevention of such slips. For it must be remembered that this is no hastily-prepared work: it has occupied much of the writer's time since 1866, when the *magnum opus* was commenced. The author is to be heartily congratulated on the successful termination of labours which must have occupied most if not all of his leisure time from other more regular work.

But Mr. Carr has not confined his work to the limits of his previous title-pages: he has "supplemented" it "by an index to the papers on pure mathematics which are to be found in the principal Journals and Transactions of learned Societies, both English and foreign, of the present century." Such indices have, in these busy times, great value for students in all branches of knowledge, and this one is, we think, very accurate for a first edition. There are, however, some defects. On p. 720 occurs an historical note on the cycloid; no reference is made to the exhaustive treatise, on the cycloid and all forms of cycloidal curves, by Mr. Proctor (Longmans, 1878). In the like case of Cartesian ovals there is no account taken of Prof. Williamson's paper on these curves in Hermathena (No. iv. p. 509; subsequently given in the author's "Differential Calculus"). For the length of an arc of an oval expressed by elliptic functions, Mr. Carr (p. 731) cites a paper by Mr. S. Roberts, F.R.S., in the London Mathematical Society's Proceedings (vol. v. p. 6), but does not mention that Mr. Roberts (vol. vi. p. 200) subsequently found that he had been anticipated by Prof. Genocchi (1855, see "Il Cimento," Turin). The name of Desargues, rightly given p. 917, is twice printed Desarques on p. 858. On pp. 913 and 935 we have "polyzonal," "zonal," and "tetrazonal": the author of the paper in question has "m" in place of "n" throughout. "Nicomaque" (p. 853), "implexe" (p. 859), "pseudosfera" (p. 915), are easily traceable; "coplanation" and "complanation" are also to be traced to the titles of the original papers. Cases of wrong spelling occur in the names of Hesse (p. 890), Kronecker (p. 890), Plücker (p. 916), Rodrigues (p. 921), Lissajous (p. 900), and there are various forms of MacCullagh; but "Tetratops" (p. 931) eludes us. These are trifling matters, and in the text in such parts of XIII. and XIV. as we have read we have not observed any errata of consequence.

It would be a boon to students if Mr. Carr would issue this supplement in a separate form, and add to his extensive list of thirty-two periodicals references to the papers on pure mathematics which occur in the *Philosophical Magazine*, *Lady's and Gentleman's Diary*, the *Mathematician*, "Reprint from the *Educational Times*" (a limited selection here of general results and the occasional papers), and *Mathesis*. For instance, on the "15-girl" problem there is a good article in the *Diary*, on the "chessmen" in the *Philosophical Magazine*.

Mr. Carr will understand that our remarks are made in no captious spirit: we are very grateful for the trouble he has taken, and desire only that a second edition may be made even more valuable from its increased accuracy and stores of information than the present one is.

He is to be congratulated on the arrangement of his text, the several different kinds of types which have been put at his disposal by the printers, and the excellent diagrams. It only remains to express the wish that what the author has done for one side of mathematics he may be encouraged to do for the other, *i.e.* for applied mathematics.

COMMERCIAL ORGANIC ANALYSIS

Commercial Organic Analysis. By Alfred H. Allen, F.I.C., F.C.S., &c. Second Edition, Revised and Enlarged. Vol. II. Fixed Oils and Fats, Hydrocarbons, Phenols, &c. (London: J. and A. Churchill, 1886.)

THIS work has been so much enlarged and so thoroughly revised that it has become almost a new book, and certainly its value has been greatly enhanced to all analysts and others interested in the special points discussed. These copious additions have rendered it necessary to divide the present edition into three volumes, the first of which appeared some little while ago, whilst the third (on Aromatic Acids, Tannins, Colouring Matters, Cyanogen Compounds, Organic Bases, Albumenoids, &c.), is now in course of preparation, and is promised shortly. Of the sections treated in the present volume it may be said generally that the author has collected together and systematically digested almost all the available information extant scattered about in text-books and numerous papers read before scientific Societies, and that he has largely contributed personally to the mass of information by means of experimental and observational work carried out in his own laboratory at the cost of much time and labour. Moreover, he has sought and obtained the aid of several well-known chemists possessing special knowledge and skill in certain kinds of work, by whose assistance many of the more important articles have acquired an almost exhaustive character. As a result, the treatise has become a most valuable hand-book and book of reference with respect to the class of matter coming within its scope; and it may now be fairly said to be an essential item in the list of works requisite in the library of an analytical chemist.

Amongst the numerous special points of research to which the author has devoted much personal attention, a notable one is the examination of various of the physical properties of fatty and oily matters with a view to their discrimination and identification, and more especially the methods in use in the determination of their specific gravity. In order to avoid complications arising from differences in physical state—some being solid, others pasty, and others fluid at the ordinary temperature—he recommends, as previously proposed by Estcourt, that comparisons should always be made at the temperature of boiling water, the melted fat or fluid oil being placed in a test-tube heated in a water-bath (of such construction that no steam escapes in the vicinity of the operator), and the indications of a Westphal's hydrostatic balance noted when the plummet is immersed in the hot oil. The figures thus obtained on repetition of experiments show very little divergence; and characteristic values are thus obtainable for certain kinds of fats, *e.g.* butter-fat when genuine. Owing to the fact that the Westphal balance as sold is constructed to give specific gravity indications *at the ordinary temperature* (*i.e.* that the plummet is adjusted so as to lose a particular weight, conveniently 5 or 10 grammes, when immersed in water at, say, 15° C.), it is obvious that the numerical values obtained on immersion in hot material at close upon 100° C. represent neither the true densities at 100° of the substances (weights per cubic centimetre) nor the ratios between these densities and that of water at 100°, *i.e.* their specific gravities at 100°. But for any particular instrument, the different values obtained with different fatty matters exhibit the same differences as those obtained with any other instrument; whilst the indications of any two instruments are obviously comparable, provided that the mode of graduation and the coefficient of expansion of the plummet are the same; which practically is the case if a *glass* plummet be always used, as recommended. In a recent communication to the *Analyst* (January 1887, p. 18), the author suggests that the term "*indicated plummet-gravity*" should be employed to represent the apparent values obtained at such and such a temperature by means of the plummet-balance; which is clearly preferable to the use of either of the terms "specific gravity" or "density" in such cases. It may be noticed in passing that the "indicated plummet-gravities" of fats and oils at 100° or thereabouts by no means necessarily follow the same order as the so-called specific gravities obtained at lower temperatures, not only on account of difference of physical state, but also through the different rates of expansion possessed by the various substances.

Another point on which the author has worked with results of some interest is the determination of the amount of glycerol yielded by fats and oils on saponification. He concludes that there is no experimental basis for the suggestion put forth some time ago by Wanklyn and Fox that *isoglycerides* are present in such substances, these bodies yielding on saponification propionic (or other homologous) acid and water, instead of glycerol. Such a view is opposed not only to the author's laboratory experience, but also to that of manufacturers, who frequently recover 7·5 to 8·0 parts of glycerol per 100 of fatty matter, instead of less than 5 as stated by Wanklyn and Fox. Making allowance therefore for deficient saponification and loss of glycerol by evaporation during recovery, the theoretical amount of glycerol obtainable is satisfactorily accounted for, instead of being largely in excess of that actually produced. It is noticeable that, whilst the author obtained results reasonably concordant with the permanganate process for the determination of the glycerol produced during saponification as compared with the other ordinary methods in many instances, this was not always the case, the former process sometimes yielding figures far in excess, indicating the presence of other substances besides glycerol capable of forming oxalic acid by treatment with permanganate. An extreme case was afforded by linseed oil dried up to elastic skin, which gave 4·9 per cent. of impure glycerol directly isolated, and 15·5 per cent. by the permanganate process.

The author considers that the usually received molecular weight of linoleic acid, generally represented as indicated by the formula $C_{16}H_{28}O_2$, is incorrect, as the mean equivalent of the acids obtainable from linseed oil on saponification has been found by him to be considerably higher than that thus indicated. The formula $C_{18}H_{32}O_2$ agrees better with his results, and moreover is not at all incompatible with the analytical data obtained by previous investigators. The analogous determination of the mean molecular weight of the acids produced on saponification (by means of alcoholic potash and phenolphthalein) of fatty, waxy, and oily matters, and of the fatty and resinous acids contained in soaps, is justly regarded by the author as a valuable criterion in judging of the nature of such substances, especially when taken in conjunction with other data (*e.g.* in the case of butter-fat, the amount of volatile acids capable of being subsequently distilled off along with water, working under particular constant conditions as recommended by Reichert; and the proportion between acids soluble and insoluble in water, &c.); and a large amount of experimental work has been done by him in connection with such valuations. He has also made an excellent digest of recent researches in connection with fats, oils, waxes, soaps, and analogous bodies. In connection with toilet-soaps, he regards the addition of sugar to produce transparency as "simply diluting the true soap-material as so much water would do, without communicating any compensating property of value." This is a very mild way of putting the case, the fact being that soaps containing sugar are liable to produce, in sensitive subjects, a great amount of irritation of the skin (in fact, a mild kind of "grocer's eczema," traceable to the same cause), even though free from causticity and otherwise unobjectionable: and numerous persons are, to the reviewer's knowledge, unable to use certain widely-advertised soaps, in consequence of the large admixture of sugar therein contained, although the composition would otherwise be quite uninjurious.

The sections devoted respectively to hydrocarbons and phenols are equally comprehensive, including descriptions, necessarily in some cases somewhat brief, of the leading points in the petroleum, coal-tar, and shale-oil industries, and of the technical examination of the various products obtained on fractional distillation and subsequent further treatment of these and allied raw materials, *e.g.* wood-tar; and the commercial examination of turpentines and resins, essential oils, camphors, and various miscellaneous substances, such as cantharidin and cholesterin. In these, as in the previous section, the value of the various *précis* given of other observers' work is frequently further enhanced by the comparative experiments and examinations made in connection with different analytical methods, &c., by the author himself. Bone-tar, obtained as a by-product of the animal charcoal manufacture, is not described, probably on account of the limited uses to which, hitherto, it has been put; whilst, for similar reasons, but little is said of blast-furnace and coke-oven tars.

C. R. ALDER WRIGHT

OUR BOOK SHELF

Practical Dynamo-Building for Amateurs. By Fred. W. Walker, M.E. (London: Iliffe and Son, 1886.)

WOULD that all books for amateur guidance were written with as full a knowledge of proper principles as this unassuming little work. We are not saying that the machine which the author recommends amateurs to construct is the equal of the commercial dynamos of the current date. His field magnet cores are of cast iron, and not of the best form; his armature might be improved by getting greater cross-section of iron into it. But there is nothing wrongly done. All instructions about the essential details of proper insulation and testing of the work in progress are accurate and full; and an appendix on alternative constructions of field-magnets supplies in some degree the deficiencies of the earlier text.

Hand-book of Zoology, with Examples from Canadian Species, Recent and Fossil. By Sir J. William Dawson, LL.D., F.R.S., &c. Third Edition, revised and enlarged. (Montreal: Dawson Brothers, 1886.)

IN this little work, the President of the British Association for the Advancement of Science has concentrated into some 300 pages a very fair account of the principal divisions of the animal kingdom. It is specially adapted for Canadian students, inasmuch as the examples of every group are selected, as far as possible, from species found within the limits of the Dominion. The fact of the volume having reached a third edition shows that Sir William Dawson's plan and method have been appreciated. That the arrangement adopted is altogether unexceptionable, and that all the most recent discoveries in zoological science are taken advantage of, we could not fairly say. For example, *Eozoon* is still treated of as if it were without doubt an organic structure; the unquestionable affinity of the larval Ascidian to the Vertebrate embryo is but faintly alluded to; and the much-talked-about *Peripatus*—one of the most singular types of Arthropodal life—seems to have been altogether omitted from the list. Yet there is, in the main, an absence of the serious errors which are too often found in such manuals. The volume is well illustrated and well printed, and will, we have no doubt, be of much service as a text-book in Canadian schools of science.

Theory of Magnetic Measurements. By Francis E. Nipher, A.M., Professor of Physics in Washington University, President of the St. Louis Academy of Science. (New York: D. Van Nostrand; London: Trübner and Co., 1886.)

DURING the last twenty years there has been considerable activity amongst observers on land and at sea in adding to our knowledge of the magnetism of the earth, and it is certainly desirable, if not necessary, that those busy workers, who are only acquainted with the practical use of the instruments employed, should know something of the theory of the magnetic measurements upon which they may be engaged.

To those conducting magnetic surveys under English auspices, the article on Terrestrial Magnetism in the "Admiralty Manual of Scientific Inquiry" has proved a valuable aid in showing both what was required and the practical means of obtaining observations on land and sea, with the methods of calculating the results. The theory of the subject, however, is beyond the intended scope of the Manual.

The magnetic survey of the United States has, during the period under consideration, been continuously carried on under the Government and private enterprise, and Prof. Nipher has been one of the diligent workers, as shown by his survey of Missouri, published in NATURE, (vol. xxiii. p. 583). In the book before us he combines some excellent practical information for those undertaking the observation of the three magnetic elements on land, with the theory of the several magnetic measurements thus made.

A large portion of the book is necessarily occupied by the theory of the horizontal force magnetometer and its several parts. Here the reader will find some differences from the English notation. For instance, H is substituted for X when denoting the horizontal force, and I for K, as the moment of inertia of the deflecting magnet. A more important departure from the usual method of calculation will be noticed in the omission of the coefficient of induction μ, which has been so entirely rejected as not even to be discussed. The retention of this coefficient has already been challenged elsewhere, but the general support of European practice seems to forbid any change until its place in our formulæ has been proved unnecessary. In the concluding pages there is an article on the systems of units adopted in magnetic measurements, and plates of the unifilar magnetometer and dip-circle generally used.

The appendix is devoted to a discussion of the method of least squares in the reduction of observations, an article on graphic methods, with the aid of which so much may be done to shorten the labour of computation, and some tables giving the times of the elongation of Polaris with its corresponding azimuth for the years 1886-95, from which the true meridian may be readily deduced for declination-observations. These tables will probably be found convenient in latitudes between the northern tropic and the Arctic circle.

The large domain of magnetic observations at sea is not touched upon, but intending observers on land should be pleased to possess a book of this kind, which might be included in their travelling equipment without fear of adding much to the weights to be carried.

The Coming Deluge of Russian Petroleum. By C. Marvin. (London: R. Anderson and Co.)

THIS pamphlet does not pretend to add to our limited knowledge of the origin of petroleum or of its connection with the Tertiary deposits and volcanic activity of the Caucasus. It is in reality an appeal to English enterprise to direct its attention to at least the carrying trade of a district so rapidly growing in industrial importance. The enormous figures given (*e.g.* more than a million gallons a day from a single well) are enough, however, to stimulate scientific as well as Stock-Exchange inquiries, and the development of communications between Baku and the West is sure to be, sooner or later, fruitful in geological results. G. C.

LETTERS TO THE EDITOR

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

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Cambridge Cholera Fungus

I HAVE read with great surprise Mr. Gardiner's letter in your issue of January 20 (p. 271). We are there told that on reconsideration Mr. Gardiner has now come to the conclusion that the organism which he saw in Prof. Roy's preparations of the intestinal mucous membrane—which Prof. Roy took to be the more usual and typical form, and which Mr. Gardiner then thought to belong to the Chitridiaceæ—is probably the particular phase in the life-history of Bacterium known as an involution form, *i.e.* "a thin and somewhat moniliform filament which at one end exhibited a distinct nodular swelling." If Mr. Gardiner has studied the filaments of a growth of mould in animal tissues, he must have come across numbers of such forms. But granting for the sake of argument that what Mr. Gardiner saw in Prof. Roy's specimens bears a resemblance to and is in reality an involution form of Bacterium, how about the branched threads figured in the Report by Messrs. Roy, Brown, and Sherrington in No. 247 of the Proceedings of the Royal Society, on p. 179?

Each of these two figures introduced here, no doubt as typical representations of the organisms in the mucous membrane, sho s unmistakably BRANCHED mycelial threads of a true fungus. If what Mr. Gardiner has seen in Prof. Roy's preparations is an involution form of some Bacterium, then the branched threads figured in Messrs. Roy, Brown, and Sherrington's Report are something else, unless somebody started the novel and extraordinary view that a Bacterium possesses branched mycelial threads like a true fungus.

I should be glad to hear Mr. Gardiner's opinion as to these branched mycelial threads figured on p. 179 of the Proceedings of the Royal Society, No. 247, in the Report by Messrs. Roy, Brown, and Sherrington. E. KLEIN

The Coal-Dust Theory

THOSE readers of NATURE who have followed the various phases of the coal-dust question will be interested to know that the following resolution was carried at the Conference of Miners, which concluded its sittings in Birmingham yesterday morning:—

"This Conference, believing that recent explosions have demonstrated that coal-dust is sufficient, without the presence of gas, to cause a serious explosion, is of opinion that a clause should be inserted in the new Mines Act, making it illegal to use blasting-powder, or other inflammable substance, in any part of the tram or trolly-way, unless the dust on the top, the bottom, and the sides of such tram or trolly-way has been properly damped or removed, for a distance of fifteen yards on each side of the hole in which the shot is to be fired."

This practical method of dealing with part of the difficulty will doubtless lead to the happiest results, if properly carried out.

The use of *dust-tight* tubs, or mine-waggons, as a means of preventing the deposition of coal-dust in the first place, and thus avoiding the necessity for so many precautions in dealing with it afterwards, suggested itself to me while I was examining the scene of the explosion in the silkstone-pits at Altofts Colliery. In order to fulfil the condition of being dust-tight, the mine-waggons would require to have no crevices of any kind through which dust could be shaken, and they would require to be so filled that no pieces of coal would roll over their sides on to the floor.

This alternative method, which I propose to discuss at greater length elsewhere, showing practical results as far as they extend, might perhaps meet the case of those mines in which the use of water is objectionable, on account of its disintegrating effect upon the roof or floor. W. GALLOWAY

Cardiff, January 15

Barnard's Comet at Perihelion

IN spite of the bad weather and the glare of twilight and moonlight, I have made a good number of observations and drawings of this comet; especially on December 16, 1886, the day of its passage at perihelion, and consequently of its shortest distance from the sun (nearly two-thirds of the distance of the earth from the sun).

The head of the comet was a large, brilliant, star-like nucleus, surrounded with a splendid, globular *chevelure*. From this sprang two tails: the larger, directed towards the North Pole, was straight, or but a little convex to the east (on which side it was also a little thicker), ending in an extremely faint nebulosity nearly 10° from the head. The smaller tail was much shorter, and directed 50° to the west of the other. The colour of the comet was a beautiful light blue.

The spectrum of the comet resulted from the ordinary three bands of the hydrocarbons; the green being the strongest, then the yellow, whilst the blue was the faintest. The bands were crossed by the linear spectrum of the nucleus, continuous, but strongly reinforced on the bands, and extended very little beyond the limits of the bands themselves.

Before the passage at perihelion the comet had the same form, but less developed; the spectrum also was the same.

On the morning of December 7 I followed the comet with the 10-inch refractor till twenty-five minutes before sunrise: this proves the great brilliancy of the comet. A. RICCÒ

Palermo Observatory, January 9

Magnetic Theory

MAY I trespass upon your space to ask a questiun which I have never seen proposed, but which is so obvious that it must have occurred to many others interested in magnetic theory?

In a current field—with closed currents—we have the familiar equations—

$$F = \iiint \frac{udxdydz}{r},\ G = \&c.,\ H = \&c.,$$

$$\frac{du}{dv} + \frac{dv}{dy} + \frac{dw}{dz} = 0 = \frac{dF}{dx} + \frac{dG}{dy} + \frac{dH}{dz},$$

$$\nabla^2 F + 4\pi u = 0,\ \&c.$$

And if a, b, c are the components of magnetic force—

$$a = \frac{dG}{dz} - \frac{dH}{dy}\ \&c., \qquad 4\pi u = \frac{dc}{dy} - \frac{db}{dz}\ \&c.$$

In a magnetised mass, A, B, C being components of magnetisation, if we take

$$F = \iiint \left\{ B\frac{d}{dz} - C\frac{d}{dy} \right\} \frac{dxdydz}{r}, \qquad G = \&c., \quad H = \&c.$$

We have at all *external* points a magnetic force of the components a, b, c, where $a = \frac{dG}{dz} - \frac{dH}{dy}$, &c.

If we introduce the vector whose components per unit volume, u, v, w, are $\frac{dB}{dz} - \frac{dC}{dy}$, &c., where A, B, C are continuous and per unit surface are $nB - mC$ over surfaces where A, B, C pass discontinuously to zero, we get $F = \iiint \frac{udxdydz}{r}$, &c.

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0 = \frac{dF}{dx} + \frac{dG}{dy} + \frac{dH}{dz},$$

and all the equations of the current field are reproduced.

Only the components α, β, γ, of the magnetic force at *internal* points, being derivable from the potential

$$V = \iiint \frac{1}{r}\left(\frac{dA}{dx} + \frac{dB}{dy} + \frac{dC}{dz}\right)dxdydz,$$

are not identical with a, b, c, as they do not satisfy the equations $\frac{dc}{dy} - \frac{db}{dz} = 4\pi u$, &c.

My question is, what is the physical evidence in favour of the existence of A, B, C and α, β, γ?

All we know, and can know, about a magnetised mass is derivable from observations of the *external* field.

Everything, therefore, that we can know is satisfied by expressing the state of the mass in terms of u, v, w, and regarding these quantities as the ordinary electric current components. So that a, b, c are components of force everywhere.

My meaning is that if the order of our investigations had been reversed, commencing with current phenomena and so passing on to magnetic, it seems almost certain that we should have attempted to explain the latter in terms of the former.

Doubtless many of the observed facts of induced magnetism would present grave difficulties, but I do not think these difficulties would have driven us to the hypothesis of permanently polarised molecules, or that we should have derived any additional help from such hypothesis. H. W. WATSON

Berkeswell Rectory, Coventry

Sounding a Crater, Fusion-Points, Pyrometers, and Seismometers

I HAD expected to see some confirmation of the remarks that form the chief part of the letter on this subject by Dr. H. J. Johnston-Lavis in NATURE of the 30th ult. (p. 197), but as no one has taken up the matter yet, perhaps you will allow me, as for years the chief assistant of the late Robert Mallet, to say that it is quite true that elaborate apparatus was devised by him and made by different instrument-makers, with a view to obtaining experimental imformation on the whole of the questions and more than those referred to by Dr. H. J. Johnston-Lavis. A preliminary report was presented by him to the British Association in 1863, in which the scope of his inquiry and nature of apparatus were mentioned, and other reports were written by him which I have not by me now. I am also able to say that the whole of the apparatus remained for years, through Prof. Guiscardi, in the University of Naples, and that Mallet wrote to him as to its disposition for the use of others, should occasion permit, just before his death. W. WORBY BEAUMONT

Norwood Road, S.E.

Folkestone Gault

MR. JOHN GRIFFITHS, of Folkestone, the well-known collector of Gault fossils, is without resources, and is permanently disabled by rheumatism, brought on by exposure in his daily labours, which have not only enriched the museums of Europe and the United States, but have formed the groundwork of the investigations into the zones and fossils of the Gault made by myself and fellow-workers—the Rev. Prof. Wiltshire, F.G.S., before my own endeavours, and those of Messrs. F. G. H. Price, F.G.S., and Starkie Gardner, F.G.S., since. Mr. F. G. H. Price, of Messrs. Child's Bank, Temple Bar, W.C., has kindly undertaken to receive subscriptions.

H.M. Geological Survey C. E. DE RANCE

Wolves, Mares, and Foals

WHEN in The Asturias in 1885, I was told of a very curious case of animal instinct, which may be worth recording. Wolves are by no means unfrequent in The Asturias, and often attack the young foals which are sent up to pasturage with the mares in the mountains. The experienced danger seems to have begotten a precautionary instinct of a very intelligent kind. It is said that, on an alarm of wolves, the mares and foals congregate for mutual protection and common defence. The mares form themselves into a sort of cordon, heads outwards, surrounding a space inclosing the young foals, and are ready for attacking with their fore-feet the wolves on their approach.

My informant gave me a graphic account of such an attack, of which he was an eye-witness for nearly an hour, and described to me how the wolves circled round and round the defenders, first at some distance, then gradually approaching nearer and nearer, seeking an opening into the inclosure, till at last they came within striking distance, and he saw one wolf rolled over dead by a blow from the fore-foot of one of the mares.

The fore-foot is not commonly used for defence by any equine species; but it is obvious that the more powerful hind-leg blow would be of little service against the spring of a wolf from behind, without the directing eye to guide the stroke. What a long experience must this mutual protection have been the result of! We can scarcely understand it, without councils of war having been held, the dangers discussed, and signals for concerted action arranged; but now all this instinct may merely be the inheritance of the experience of former generations.

Benthall, Kenley, Surrey, January 6 GEORGE MAW

THE SUN'S HEAT[1]

FROM human history we know that for several thousand years the Sun has been giving heat and light to the earth as at present; possibly with some considerable fluctuations, and possibly with some not very small progressive variation. The records of agriculture, and the natural history of plants and animals within the time of human history, abound with evidence that there has been no exceedingly great change in the intensity of the Sun's heat and light within the last 3000 years; but for all that, there may have been variations of quite as much as 5 or 10 per cent., as we may judge from considering that the intensity of the solar radiation to the earth is $6\frac{1}{2}$ per cent. greater in January than in July; and neither at the equator nor in the northern or southern hemispheres has this difference been discovered by experience or general observation of any kind. But as for the mere age of the Sun, irrespective of the question of uniformity, we have proof of something vastly more than 3000 years in geological history, with its irrefragable evidence of continuity of life on the earth in time past for tens of thousands, and probably for millions of years.

Here, then, we have a splendid subject for contemplation and research in natural philosophy, or physics, the science of dead matter. The sun, a mere piece of matter of the moderate dimensions which we know it to have, bounded all round by cold ether, has been doing work at the rate of four hundred and seventy-six thousand million million million horse-power for 3000 years, and at possibly more, and certainly not much less, than that for a few million years. How is this to be explained? Natural philosophy cannot evade the question, and no physicist who is not engaged in trying to answer it can have any other justification than that his whole working time is occupied with work on some other subject or subjects of his province by which he has more hope of being able to advance science.

I suppose I may assume that every person present knows as an established result of scientific inquiry that the sun is not a burning fire, and is merely a fluid mass cooling, with some little accession of fresh energy by meteors occasionally falling in, of very small account in comparison with the whole energy of heat which he gives out from year to year. You are also perfectly familiar with Helmholtz's form of the meteoric theory, and accept it as having the highest degree of scientific probability that can be assigned to any assumption regarding actions of prehistoric times. You understand, then, that the essential principle of the explanation is this: at some period of time, long past, the sun's initial heat was generated by the collision of pieces of matter gravitationally attracted together from distant space to build up his present mass; and shrinkage due to cooling gives, through the work done by the mutual gravitation of all parts of the shrinking mass, the vast thermal capacity in virtue of which the cooling has been, and continues to be, so slow. I assume that you have not been misled by any of your teachers who may have told you, or by any of your books in which you may have read, that the sun is becoming hotter because a gaseous mass, shrinking because it is becoming colder, becomes hotter because it shrinks.

An essential detail of Helmholtz's theory of solar heat is that the sun must be fluid, because even though given at any moment hot enough from the surface to any depth, however great, inwards, to be brilliantly incandescent, the conduction of heat from within through solid matter of even the highest conducting quality known to us would not suffice to maintain the incandescence of the surface for more than a few hours, after which all would be darkness. Observation confirms this conclusion so far as the outward appearance of the sun is concerned, but does not suffice to disprove the idea which prevailed till thirty or forty years ago that the sun is a solid nucleus inclosed in a sheet of violently agitated flame. In reality, the matter of the outer shell of the sun, from which the heat is radiated outwards, must in cooling become denser, and so becoming unstable in its high position, must fall down, and hotter fluid from within must rush up to take its place. The tremendous currents thus continually produced in this great mass of flaming fluid constitute the province of the newly-developed science of solar physics, which, with its marvellous instrument of research—the spectroscope—is yearly and daily giving us more and more knowledge of the actual motions of the different ingredients, and of the splendid and all-important resulting phenomena.

Now, to form some idea of the amount of the heat which is being continually carried up to the sun's surface and radiated out into space, and of the dynamical relations between it and the solar gravitation, let us first divide that prodigious number (476×10^{21}) of horse-power by the number ($6 \cdot 1 \times 10^{18}$) of square metres in the sun's surface, and we find 78,000 horse-power as the mechanical value of the radiation per square metre. Imagine, then, the engines of eight ironclads applied to do all their available work of, say, 10,000 horse-power each, in perpetuity driving one small paddle in a fluid contained in a square metre vat. The same heat will be given out from the square metre surface of the fluid as is given out from every square metre of the sun's surface.

But now to pass from a practically impossible combination of engines and a physically impossible paddle and fluid and containing vessel, towards a more practical combination of matter for producing the same effect: still keep the ideal vat and paddle and fluid, but place the vat on the surface of a cool, solid, homogeneous globe of the same size ($\cdot 697 \times 10^9$ metres radius) as the sun, and of density ($1 \cdot 4$) equal to the sun's density. Instead of using steam-power, let the paddle be driven by a weight descending in a pit excavated below the vat. As the simplest possible mechanism, take a long vertical shaft, with the paddle mounted on the top of it so as to turn horizontally Let the weight be a nut working on a screw-thread on the vertical shaft, with guides to prevent the nut from turning—the screw and the guides being all absolutely

[1] Lecture on "The Probable Origin, the Total Amount, and the Possible Duration, of the Sun's Heat," delivered by Sir William Thomson, F.R.S., at the Royal Institution, on Friday, the 21st inst.

frictionless. Let the pit be a metre square at its upper end, and let it be excavated quite down to the sun's centre, everywhere of square horizontal section, and tapering uniformly to a point at the centre. Let the weight be simply the excavated matter of the sun's mass, with merely a little clearance space between it and the four sides of the pit, and a kilometre or so cut off the lower pointed end to allow space for its descent. The mass of this weight is 326×10^6 tons. Its heaviness, three-quarters of the heaviness of an equal mass at the sun's surface, is 244×10^6 tons solar surface-heaviness. Now a horse-power is 270 metre-tons, terrestrial surface-heaviness, per hour; or 10 metre-tons, solar surface-heaviness, per hour. To do 78,000 horse-power, or 780,000 metre-tons, solar surface-heaviness, per hour, our weight must therefore descend at the rate of 1 metre in 313 hours, or about 28 metres per year.

To advance another step, still through impracticable mechanism, towards the practical method by which the sun's heat is produced, let the thread of the screw be of uniformly decreasing steepness from the surface downwards, so that the velocity of the weight, as it is allowed to descend by the turning of the screw, shall be in simple proportion to distance from the sun's centre. This will involve a uniform condensation of the material of the weight; but a condensation so exceedingly small in the course even of tens of thousands of years, that, whatever be the supposed character, metal or stone, of the weight, the elastic reaction against the condensation will be utterly imperceptible in comparison with the gravitational forces with which we are concerned. The work done per metre of descent of the top end of the weight will be just four-fifths of what it was when the thread of the screw was uniform. Thus, to do the 78,000 horse-power of work, the top end of the weight must descend at the rate of 35 metres per year: or 70 kilometres, which is one one-hundredth per cent. (1/10,000) of the sun's radius, per 2000 years.

Now let the whole surface of our cool solid sun be divided into squares, for example as nearly as may be of 1 square metre area each, and let the whole mass of the sun be divided into long inverted pyramids or pointed rods, each 700,000 kilometres long, with their points meeting at the centre. Let each be mounted on a screw, as already described for the long tapering weight which we first considered; and let the paddle at the top end of each screw-shaft revolve in a fluid, not now confined to a vat, but covering the whole surface of the sun to a depth of a few metres or kilometres. Arrange the viscosity of the fluid and the size of each paddle so as to let the paddle turn just so fast as to allow the top end of each pointed rod to descend at the rate of 35 metres per year. The whole fluid will, by the work which the paddles do in it, be made incandescent, and it will give out heat and light to just about the same amount as is actually done by the sun. If the fluid be a few thousand kilometres deep over the paddles, it would be impossible, by any of the appliances of solar physics, to see the difference between our model mechanical sun and the true sun.

Now, to do away with the last vestige of impracticable mechanism, in which the heavinesses of all parts of each long rod are supported on the thread of an ideal screw cut on a vertical shaft of ideal matter, absolutely hard and absolutely frictionless: first, go back a step to our supposition of just one such rod and screw working in a single pit excavated down to the centre of the sun, and let us suppose all the rest of the sun's mass to be rigid and absolutely impervious to heat. Warm up the matter of the pyramidal rod to such a temperature that its material melts and experiences enough of Sir Humphrey Davy's "repulsive motion" to keep it balanced as a fluid, without either sinking or rising from the position in which it was held by the thread of the screw. When the matter is thus held up without the screw, take away the screw or let it melt in its place. We should thus have a pit from the sun's surface to his centre, of a square metre area at the surface, full of incandescent fluid, which we may suppose to be of the actual ingredients of the solar substance. This fluid, having at the first instant the temperature with which the paddle left it, would at the first instant continue radiating heat just as it did when the paddle was kept moving; but it would quickly become much cooler at its surface, and to a distance of a few metres down. Convection-currents, with their irregular whirls, would carry the cooled fluid down from the surface, and bring up hotter fluid from below, but this mixing could not go on through a depth of very many metres to a sufficient degree to keep up anything approaching to the high temperature maintained by the paddle; and after a few hours or days, solidification would commence at the surface. If the solidified matter floats on the fluid at the same temperature below it, the crust would simply thicken as ice on a lake thickens in frosty weather; but if, as is more probable, solid matter, of such ingredients as the sun is composed of, sinks in the liquid when both are at the melting temperature of the substance, thin films of the upper crust would fall in, and continue falling in, until, for several metres downwards, the whole mass of mixed solid and fluid becomes stiff enough (like the stiffness of paste or of mortar) to prevent the frozen film from falling down from the surface. The surface film would then quickly thicken, and in the course of a few hours or days become less than red-hot on its upper surface. The whole pit full of fluid would go on cooling with extreme slowness until, after possibly about a million million million years or so, it would be all at the same temperature as the space to which its upper end radiates.

Now, let precisely what we have been considering be done for every one of our pyramidal rods, with, however, in the first place, thin partitions of matter impervious to heat separating every pit from its four surrounding neighbours. Precisely the same series of events as we have been considering will take place in every one of the pits.

Suppose the whole complex mass to be rotating at the rate of once round in 25 days.

Now at the instant when the paddle stops let all the partitions be annulled, so that there shall be perfect freedom for convection-currents to flow unresisted in any direction, except so far as resisted by the viscosity of the fluid, and leave the piece of matter, which we may now call the Sun, to himself. He will immediately begin showing all the phenomena known in solar physics. Of course the observer might have to wait a few years for sunspots, and a few quarter-centuries to discover periods of sunspots, but they would, I think I may say probably, all be there just as they are; because I think we may feel that it is most probable that all these actions are due to the sun's own mass and not to external influences of any kind. It is, however, quite possible, and indeed many who know most of the subject think it probable, that some of the chief phenomena due to sunspots arise from influxes of meteoric matter circling round the sun. The energy of chemical combination is as nothing compared with the gravitational energy of shrinkage, to which the sun's activity is almost wholly due, but chemical combinations and dissociations may, as urged by Lockyer, be thoroughly potent determining influences on some of the features of non-uniformity of the brightness in the grand phenomena of sunspots, hydrogen flames, and corona, which make the province of solar physics. But these are questions belonging to a very splendid branch of solar science with which we are not occupied this evening.

What concerns us at present may be summarised in two propositions:—

(1) Gigantic convection-currents throughout the sun's liquid mass are continually maintained by fluid, slightly

cooled by radiation, falling down from the surface, and hotter fluid rushing up to take its place.

(2) The work done in any time by the mutual gravitation of all the parts of the fluid, as it shrinks in virtue of the lowering of its temperature, is but little less than (so little less than, that we may regard it as practically equal to[1]) the dynamical equivalent of the heat that is radiated from the sun in the same time.

The rate of shrinkage corresponding to the present rate of solar radiation has been proved to us, by the consideration of our dynamical model, to be 35 metres on the radius per year, or one ten-thousandth of its own length on the radius per two thousand years. Hence, if the solar radiation has been about the same as at present for two hundred thousand years, his radius must have been greater by 1 per cent. two hundred thousand years ago than at present. If we wish to carry our calculations much farther back or forward than two hundred thousand years, we must reckon by differences of the reciprocal of the sun's radius, and not by differences simply of the radius, to take into account the change of density (which, for example, would be 3 per cent. for 1 per cent. change of the radius). Thus the rule, easily worked out according to the principles illustrated by our mechanical model, is this :—

Equal differences of the reciprocal of the radius correspond to equal quantities of heat radiated away from million of years to million of years.

Take two examples :—

(1) If in past time there has been as much as fifteen million times the heat radiated from the sun as is at present radiated out in one year, the solar radius must have been four times as great as at present.

(2) If the sun's effective thermal capacity can be maintained by shrinkage till twenty million times the present year's amount of heat is radiated away, the sun's radius must be half what it is now. But it is to be remarked that the density which this would imply, being 11·2 times the density of water, or just about the density of lead, is probably too great to allow the free shrinkage as of a cooling gas to be still continued without obstruction through overcrowding of the molecules. It seems, therefore, most probable that we cannot for the future reckon on more of solar radiation than, if so much as, twenty million times the amount at present radiated out in a year. It is also to be remarked that the greatly diminished radiating surface, at a much lower temperature, would give out annually much less heat than the sun in his present condition gives. The same considerations led Newcomb to the conclusion "that it is hardly likely that the sun can continue to give sufficient heat to support life on the earth (such life as we now are acquainted with, at least) for ten million years from the present time."

In all our calculations hitherto we have for simplicity taken the density as uniform throughout, and equal to the true mean density of the sun, being about 1·4 times the density of water, or about a fourth of the earth's mean density. In reality the density in the upper parts of the sun's mass must be something less than this, and something considerably more than this in the central parts, because of the pressure in the interior increasing to something enormously great at the centre. If we knew the distribution of interior density we could easily modify our calculations accordingly, but it does not seem probable that the correction could, with any probable assumption as to the greatness of the density throughout a considerable proportion of the sun's interior, add more than a few million years to the past of solar heat, and what could be added to the past must be taken from the future.

In our calculations we have taken Pouillet's number for the total activity of solar radiation, which practically agrees with Herschel's. Forbes[1] showed the necessity for correcting the mode of allowing for atmospheric absorption used by his two predecessors in estimating the total amount of solar radiation, and he was thus led to a number 1·6 times theirs. Forty years later Langley,[2] in an excellently worked out consideration of the whole question of absorption by our atmosphere, of radiant heat of all wave-lengths, accepts and confirms Forbes's reasoning, and by fresh observations in very favourable circumstances on Mount Whitney, 15,000 feet above the sea-level, finds a number a little greater still than Forbes (1·7, instead of Forbes's 1·6, times Pouillet's number). Thus Langley's number expressing the quantity of heat radiated per second of time from each square centimetre of the sun's surface corresponds to 133,000 horse-power per square metre, instead of the 78,000 horse-power which we have taken, and diminishes each of our times in the ratio of 1 to 1·7. Thus, instead of Helmholtz's twenty million years, which was founded on Pouillet's estimate, we have only twelve millons, and similarly with all our other time reckonings based on Pouillet's results. In the circumstances, and taking fully into account all possibilities of greater density in the sun's interior, and of greater or less activity of radiation in past ages, it would, I think, be exceedingly rash to assume as probable anything more than twenty million years of the sun's light in the past history of the earth, or to reckon on more than five or six million years of sunlight for time to come.

But now we come to the most interesting part of our subject—the early history of the sun. Five or ten million years ago he may have been about double his present diameter and an eighth of his present mean density, or ·175 of the density of water; but we cannot, with any probability of argument or speculation, go on continuously much beyond that. We cannot, however, help asking the question, What was the condition of the sun's matter before it came together and became hot? It may have been two cool solid masses, which collided with the velocity due to their mutual gravitation; or, but with enormously less of probability, it may have been two masses colliding with velocities considerably greater than the velocities due to mutual gravitation. This last supposition implies that, calling the two bodies A and B for brevity, the motion of the centre of inertia of B relatively to A, must, when the distances between them was great, have been directed with great exactness to pass through the centre of inertia of A; such great exactness that the rotational momentum after collision was of proper amount to let the sun have his present rotational period when shrunk to his present dimensions. This exceedingly exact aiming of the one body at the other, so to speak, is, on the dry theory of probability, exceedingly improbable. On the other hand, there is certainty that the two bodies A and B at rest in space if left to themselves, undisturbed by other bodies and only influenced by their mutual gravitation, shall collide with direct impact, and therefore with no motion of their centre of inertia, and no rotational momentum of the compound body after the collision. Thus we see that the dry probability of collision between two of a vast number of mutually attracting bodies widely scattered through space is much greater if the bodies be all given at rest, than if they be given moving in any random directions and with any velocities considerable in comparison with the velocities which they would acquire in falling from rest into collision. In this connection it is most interesting to know from stellar astronomy, aided so splendidly as it has recently been by the spectroscope, that the relative motions of the visible stars and our sun are generally very small in comparison with the velocity (612 kilometres per second) a body would acquire

[1] "On the Age of the Sun's Heat," by Sir William Thomson (*Macmillan's Magazine*, March 1862); and Thomson and Tait's "Natural Philosophy," 2nd edition, vol. i. part ii., Appendix E.

[1] *Edin. New Phil. Journal*, xxxvi. 1844.
[2] "On the Selective Absorption of Solar Energy," *American Journal of Science*, vol. xxv., March 1883.

in falling into the sun, and are comparable with the moderate little velocity (29·5 kilometres per second) of the earth in her orbit round the sun.

To fix the ideas, think of two cool solid globes, each of the same mean density as the earth, and of half the sun's diameter, given at rest, or nearly at rest, at a distance asunder equal to twice the earth's distance from the sun. They will fall together and collide in half a year. The collision will last for a few hours, in the course of which they will be transformed into a violently agitated incandescent fluid mass, with about eighteen million (according to the Pouillet-Helmholtz reckoning, of twenty million) years' heat ready made in it, and swelled out by this heat to possibly one and a half times, or two, or three, or four times, the sun's present diameter. If instead of being at rest initially they had had a transverse relative velocity of 1·42 kilometres per second, they would just escape collision, and would revolve in equal ellipses in a period of one year round the centre of inertia, just grazing one another's surfaces every time they come round to the nearest points of their orbits.

If the initial transverse component of relative velocity be less than, but not much less than, 1·42 kilometres per second, there will be a violent grazing collision, and two bright suns, solid globes bathed in flaming fluid, will come into existence in the course of a few hours, and will commence revolving round their common centre of inertia in long elliptic orbits in a period of a little less than a year. The *quasi*-tidal interaction will diminish the eccentricities of their orbits; and if continued long enough will cause the two to revolve in circular orbits round their centre of inertia with a distance between their surfaces equal to ·644 of the diameter of each.

If the initial transverse component relative velocity of the two bodies were just 68 metres per second, the moment of momentum, the same before and after collision, would be just equal to that of the solar system, of which seventeen-eighteenths is Jupiter's and one-eighteenth the sun's: the other bodies of the system being not worth considering in the account. Fragments of superficially-melted solid, or splashes of fluid, sent flying away from the main compound mass could not possibly by tidal action or other resistance get into the actual orbits of the planets, whose evolution requires some finer if more complex fore-ordination than merely the existence of two masses undisturbed by any other matter in space.

I shall only say in conclusion:—Assuming the sun's mass to be composed of portions which were far asunder before it was hot, the immediate antecedent to its incandescence must have been either two bodies with details differing only in proportion and densities from the cases we have been now considering as examples; or it must have been some number more than two—some finite number—at the most the number of atoms in the sun's present mass, which is a finite number as easily understood and imagined as number 3 or number 123. The immediate antecedent to incandescence may have been the whole constituents in the extreme condition of subdivision—that is to say, in the condition of separate atoms; or it may have been any smaller number of groups of atoms making up minute crystals or groups of crystals—snowflakes of matter, as it were; or it may have been lumps of matter like this macadamising stone; or like this stone, which you might mistake for a macadamising stone, and which was actually travelling through space till it fell on the earth at Possil, in the neighbourhood of Glasgow, on April 5, 1804; or like this—which was found in the Desert of Atacama in South America, and is believed to have fallen there from the sky—a fragment made up of iron and stone, which looks as if it has solidified from a mixture of gravel and melted iron in a place where there was very little of heaviness; or this splendidly crystallised piece of iron, a slab cut out of the celebrated aërolite of Lenarto, in Hungary;[1] or this wonderfully shaped specimen, a model of the Middlesburgh meteorite, kindly given me by Prof. A. S. Herschel, with corrugations showing how its melted matter has been scoured off from the front part of its surface in its final rush through the earth's atmosphere when it was seen to fall on March 14, 1881, at 3.35 p.m.

For the theory of the sun it is indifferent which of these varieties of configurations of matter may have been the immediate antecedent of his incandescence, but I can never think of these material antecedents without remembering a question put to me thirty years ago by the late Bishop Ewing, Bishop of Argyll and the Isles: "Do you imagine that piece of matter to have been as it is from the beginning; to have been created as it is, or to have been as it is through all time till it fell on the earth?" I had told him that I believed the sun to be built up of stones, but he would not be satisfied till he knew or could imagine, what kind of stones. I could not but agree with him in feeling it impossible to imagine that any one of these meteorites before you has been as it is through all time, or that the materials of the sun were like this for all time before they came together and became hot. Surely this stone has an eventful history, but I shall not tax your patience longer to-night by trying to trace it conjecturally. I shall only say that we cannot but agree with the common opinion which regards meteorites as fragments broken from larger masses, but we cannot be satisfied without trying to imagine what were the antecedents of those masses.

PROTOPLASM[2]

It is a natural and beneficial result of the present energetic pursuit of biological science that every now and again some thinker comes forward to show us where we stand, and to what our thoughts are impelling us. Subordinate to the universal eminence and influence of a Linnæus or a Darwin, the critics of a decade exert no small effect on contemporary investigation by suggesting new modes of viewing or expressing things; and even though the originality is not always happy, and the generalisations are sometimes unfortunate, it is nevertheless a healthy sign that specialists of reputation, led to view matters with a severely critical eye as their work progresses, occasionally turn round and warn us that it would be as well to take stock of the facts, and see what are the chances of solving some large problem. Moreover, it has to be borne in mind that as various branches reach a certain stage their results need overhauling by specialists in other departments, and it becomes a question who is to prepare the problems of biology, for instance, so that the mathematician or the physicist may criticise them.

As much on this account as for his own contributions to the store of facts, we must welcome Dr. Berthold's clever "Studies" as an earnest and important attempt to contribute to a knowledge of the mechanics of life. Of course it is always a difficulty to decide how far a specialist may be expected to take an accurate view of a large problem to the direct solution of which his own researches can contribute but little; but experience has shown that more is to be looked for from the deep insight obtained by close investigation than from the few brilliant suggestions scattered through volumes of merely clever thinking. In the present case, the moderate tone of the book, and the easy earnestness of the writer, should at least insure careful reading of the 324 pages of text in which Dr. Berthold expresses his bold ideas; and whether the conclusions stand or fall, the reader will be amply repaid by the observations collected and the criticisms on several questions now agitating the minds of botanists.

[1] The three aërolites now exhibited belong to the Hunterian Museum of the University of Glasgow, and have been kindly lent me for this evening by the Curator, Dr. Young.

[2] "Studien über Protoplasmamechanik." By Dr. G. Berthold, Professor of Botany in the University of Göttingen. (Leipzig: Arthur Felix, 1886.)

The title of the book must be taken in a comprehensive sense, for the whole "cell-theory" (if we possess one) is under review in the nine chapters into which the text is divided. This extensive aim is justified by the author's treatment of the subject, which affords an admirable survey of current botanical speculation.

Before proceeding to a closer examination of the work, we may state that there are seven well-executed plates, with descriptions which are too short. The index might have been more ample—it includes the names of plants and authors only—but the table of contents is very full and good.

An introduction of eleven pages leads us at once to the chief position assumed by the author. So long as we do not understand the *Amœba*, we must be in the dark as regards the mechanism of life in higher organisms. Nothing has been gained by regarding protoplasm as "living proteid," or as containing "living" as opposed to "dead" proteids, and so forth: moreover, no clearness, but rather the contrary, has so far resulted from hypotheses as to the "structure" of protoplasm, or from distinctions between "idioplasm" and other constituents. The author therefore inquired whether we are not perhaps treading an aimless path, and whether we should not go back and examine earlier views, and proceed anew. The consequence to his mind was the resumption of the old analogy between a drop of protoplasm and a drop of fluid, and he was led to inquire into the analogy more deeply, especially on finding that a detailed analysis of the problem had not before been seriously undertaken. As the general result of investigations begun in 1882, the author decides that protoplasm is to be regarded as a highly complex emulsion, differing in consistence in the different cases. There is nothing in the chemistry and metabolism known which need clash with this view of the fluid nature of the "physical basis of life," and the author decides that the forces upon which the changes of form, internal movements, and so forth, depend, are the same as those which determine whether a fluid shall assume the form of a drop, or drops, or spread out and wet another body, and so on—in fact, the forces concerned in surface-tensions.

The author frankly admits the difficulty, and even seeming impossibility, of imitating some of the conditions, or even of deciding whether the actions of protoplasm accord with the theory. This must naturally be the case; and of course no one expects him to imitate all the conditions experimentally. The method employed is essentially deductive and analytic throughout, and for this reason the greatest possible care must be employed in taking any step forward. Partly on this account, and partly owing to other circumstances, the book needs cautious reading, and great difficulties will be felt in regard to many points. This is apart from an undoubted (though perhaps unavoidable) blemish in the book, which consists in the author so often putting off for some pages the consideration of a subject commenced.

The key-note, as it were, of the work having been indicated, a few words must be said regarding some leading features in the various chapters. The first subject dealt with is the layered or stratified nature of the typical cell. A spore of *Equisetum*, for instance, may be regarded as a system of concentric layers. First there is a central nucleus; then various layers of protoplasm, of which the innermost is colourless and contains certain minute granules, the second is thicker and carries the chlorophyll-corpuscles, the third is hyaline and contains lenticular refractive bodies of peculiar nature; then follows the cell-wall, if nothing further. The cell-wall is usually composed of three or more layers.

If we consider the cells of a tissue, Berthold points out that a given partition membrane must be regarded as dividing and belonging to two symmetrical plasmatic systems, and as being their middle and innermost layer.

But all cells are not systems of concentric layers. Not only are excentric layers found, but a complexity is introduced as soon as the sap-vacuoles appear, in many cases making the cell not monocentric but polycentric. The *normal* order of the layers, as exemplified by the spore of *Equisetum*, or any simple cell with one large vacuole, &c., may be distinguished from the *inverse* order exhibited, for instance, by the cords in a *Caulerpa*, or the central mass in a cell containing raphides, or anywhere where the sap bathes the system of layers referred to.

It is then shown that in many cases where oil-drops, &c., have usually been regarded as lying free in a cell, they are inclosed in an ingrowth from the cell-wall, reminding us of cystoliths. An examination of intercellular spaces follows: the most interesting question is as to the existence of protoplasm in lacunæ between cells. Berthold quotes *Aconitum Napellus* as affording conclusive evidence, and confutes the contention of Gardiner and Schenck against Russow's statements. Berthold goes much further, however. He finds a thin layer of protoplasm overlying the cuticle of the epidermis and of spores, and, to put it shortly, concludes that the cell-wall is formed and embedded in protoplasm, and not excreted on its surface—the cell-wall is a supporting apparatus, not a protective one. Again, a cell forming part of a tissue cannot be forthwith compared with a unicellular Alga, for this reason: the latter may be regarded as consisting of two parts, (1) the inner protoplasmic system with its contiguous share of cell-wall, (2) the outer strata of cell-wall *plus* the hypothetical covering of protoplasm. Only the first of these two parts of the algal cell can be compared with a tissue-cell.

The relation of these ideas to Sachs's view, that we are to regard a plant as a whole cut up into cell-chambers, and not as a whole built up of single cells, is obvious to all who have followed recent speculations in botany.

It is, of course, impossible to go at any length into the contents of all the chapters. The second is concerned with the finer structure of the cell—nucleus, chlorophyll-corpuscles, and other cell-contents. Incidentally we may note the emphatic statement that starch is *not* formed in the *Melanophyceæ* (p. 57); that the word "microsome" has no definite meaning, and had better be discarded (p. 61). Later on the author expressly states his inability to confirm Strasburger's and Schmitz's conclusions that microsomes are employed in building up the cell-wall (p. 208), and even hints at confusion between crystalline particles and microsomes in the case of *Spirogyra!*

If protoplasm is an emulsion, it follows that the various processes of separation of sap-vacuoles, oil-drops, crystalline and other particles, have to be explained as according with similar separations in lifeless mixtures. Berthold finds no difficulties insuperable here, and even discusses the probable origin and disappearance of chlorophyll-corpuscles and nuclei on the assumption that they are part of the protoplasm. Although they now always arise by the division of those previously existing, they must have been formed from protoplasm in the first instance. The action of external stimuli offers a fertile subject for discussion. As regards geotropism, the author regards "the primary effect of gravitation" as consisting in the different rates of movement of substances of different specific gravity.

The supposition that anything is explained by regarding protoplasm as essentially "living proteid," is severely criticised on pp. 74 and 75, and the author agrees with Baumann that the arguments which exalt proteids into the position of being the most essential constituent of protoplasm would apply equally well to water. The "living substance of organisms" is always an extremely complex mixture. At the same time, it would seem that the author here raises some gratuitous difficulties, since no biologist really regards protoplasm as a simple substance, proteid or otherwise. One consequence of the discussion might almost be foreseen: Berthold proposes to recast the definition of protoplasm, and to subordinate to it—the fluid mixture absent from no living cell—cytoplasm, nucleus, chlorophyll-bodies, vacuoles, tannin and oil-drops, &c., as so many parts, of the protoplasm as a whole. He urges that the introduction of the ideas cytoplasm, ectoplasm, and so on, have driven the time-honoured word protoplasm out of the field, whereas its usefulness as a comprehensive word—though with a somewhat different meaning from the current one—for the whole is undoubted. Moreover, it is to be insisted upon that the protoplasm is active as a whole.

The discussion as to the meaning of the term "organised" must be here passed over, with many other points of interest.

In the third chapter we have a long analysis of the movements of naked masses of protoplasm. All turns upon the tendency of a mass of protoplasm to assume the form of a spherical drop; this can only be due to the same causes which impel a drop of any accepted liquid to assume the drop condition. Justice could not be done by summarising this analysis, and the demonstration that cylinders of protoplasm, like cylinders of liquids, tend to break up in a definite way. The end result of a long argument is, that the amœboid condition depends upon the degree of wetting of the environment by the fluid protoplasm and *vice versâ*.

If three fluids which do not mix are in contact with one another, the tensions at their surfaces can be mathematically investigated, and Berthold maintains that the principles here concerned govern the behaviour of a drop of protoplasm as they do that of an ordinary liquid under the given conditions. The phenomena of spreading out, putting forth and withdrawing pseudopodia, rounding off, &c., are due to the same causes and ruled by the same laws as the flowing of one liquid over another, or its withdrawal from it (glycerine and alcohol *e.g.*), or its assumption of the drop form, and so on. Of course amœboid movements are complex, because the liquid amœba is not a simple fluid, but is undergoing rapid changes due to its metabolism and exchanges with the environment, processes which are acting with different energy at different places. It must be clearly understood that a rapid survey of Berthold's position cannot do justice to his argument: whether his position is accepted or rejected, there is no doubt that he clearly sees and provides for many important difficulties, some of which seem to have been overlooked. It will be regarded as a startling idea by some (though the idea is not altogether new) that fine pseudopodia are not the results of activity on the part of the amœba: such pseudopodia must be looked upon as *drawn out by* the surrounding medium, not *put out into* it. Here, again, exceptions occur where blunt processes are driven forth by local contractions and other causes, but the sum total of all the argument is (as expressly stated again on p. 109) that the amœboid condition is the symptom that the organism wets the substratum and displaces the surrounding medium, indicating that the intensity of the tension between the medium and the protoplasm is but small. The discussion as to the causes and effects of the internal movements in protoplasm must here be passed over, with the simple remark that the author sees no difficulty which cannot be explained from our knowledge of the mechanics of liquids. On p. 106 is proposed an explanation of the remarkable filaments observed by F. Darwin on *Dipsacus sylvestris*.

Chapter IV. deals with what is practically a continuation of the second chapter—the symmetry or arrangement of the cell-contents. The stratified or shell arrangement is again expressly referred to, and an attempt made to explain it on the main assumption of the book. The arrangement referred to is a consequence of exchanges (diffusion, absorption, &c.) with the environment: passive particles suspended in the cell would have to assume positions which are definite; active particles (*i.e.* particles which themselves exchange with the layer in which they are embedded) might interfere with the simple shell arrangement, and we have systems within a system. After examining what occurs in the case of a spherical system or cell, the author extends the analysis to an ellipsoid and other anisodiametric systems, and finds the results accord with what is found in Nature. The question of the "Hautschicht" is then attacked, and De Vries' late statements as to the existence of a pellicle or "wall" around the vacuole are criticised. Berthold, to put it shortly, condemns this pellicle as an artificial product—a "precipitation-membrane"—in many if not in most cases. On p. 154 it is still more emphatically stated that the cell-wall inside the cell is formed "always in the interior of the protoplasm, never on its surface," and it is probable "that the same is the case even when free masses of protoplasm surround themselves with a membrane." The membrane stated to exist around the nucleus is condemned, with a certain reserve, as a probable precipitation-membrane. Other interesting points must be passed over.

The fifth chapter is practically concerned with showing that in spite of the great variety of forms exhibited by the chlorophyll-bodies of different plants, especially Algæ, their position, consistence, changes in form, division, &c., can be explained in accordance with the view that they are parts of an emulsion. Other cell-contents are considered also—oil-drops, tannin, nucleus, vacuoles, &c.—but at less length. The chlorophyll-corpuscles of higher plants are compared to drops resting on a substratum which they do not wet, their shape being in part due to radial pressures. When they are more extended and amœboid, their actions are explained according to the principles (contact of three surfaces, &c.) employed before. *Spirogyra* and other Conjugatæ present difficulties. While the "chlorophyll-apparatus" displays a relatively large surface, the converse is the case with nuclei and other cell-contents, and the form of the spherical drop (maximum cubic contents with minimum superficial area) is usual; though exceptions exist and are investigated.

The division of chlorophyll-corpuscles is then examined, and this leads naturally to the division of the nucleus and cell, which is treated separately. A spherical mass of fluid must increase its surface if it divides: this implies a diminution of tension at the common surface (as with the formation of pseudopodia), and concentric shells in the medium or in the mass of fluid in question. All the conditions fulfilled, pseudopodia can be formed either from the medium into the mass, or from the mass into the medium. An annular pseudopodium would divide the spherical (or spheroidal) mass into two. This is, shortly put, the position as Berthold views it. He then again applies the analysis of dividing cylinders, and proceeds to apply the results to what is observed in a cell. The radial pressure, and growth in one direction of the cell, may be important factors. But the real difficulty is met with when considering the division of spherical bodies in the cells of the growing-points, for instance; and the same applies to cell-division. Why should a sphere—a stable form—pass over into an elongated body, which then divides? It must be assumed that "under the influence of its own metabolism, and that of its environment a bi-polar symmetry arises in the chlorophyll-corpuscle, in consequence of which the division takes place equatorially."

This leads to the sixth chapter, which is in many respects the most important, as it is the most interesting. After reviewing the process of cell-division generally, the author separates the essential from the unessential processes, and agrees with Strasburger that the division of the nucleus must be regarded as an accompanying phenomenon. The division of the ovum of *Echinus* and *Ciona* is described: soon after the male and female nuclei have fused, two centres appear in the egg, each with radii—the required bi-polarity is established. The exchanges and movements in the protoplasm are then followed; the result is that certain constituents accumulate to excess in the equator between the two radiating centres, or "suns." The chief points are illustrated by diagrams. The two "suns" are the centres of the future daughter-cells; the still single nucleus lies between them in a bridge of the same protoplasm as the "suns" (these "suns," by the bye, are the *Attractions-Kugeln* of Van Beneden, and the *Pol-Kugeln* of others) are embedded in: the more peripheral protoplasm of the cell (ovum) has accumulated chiefly around the nucleus—*i.e.* in the equatorial plane. This equatorial protoplasm then begins to cut in two the nucleus, which has assumed the "karyokinetic" condition. Passing over many details, we may sum up the explanation shortly. The superficial shells of protoplasm are assumed to put forth pseudopodia between the "suns"—*i.e.* the author regards it as fundamentally a wetting process, due to changes at the surfaces. The processes are essentially of the same nature in vegetable cells, though it is impossible in a short space to summarise Berthold's discussion as to the relative importance of the numerous details which occur in different cases. Obviously the stumbling-block which is best worth further attack is the origin of bi-polarity in a spherical mass: that Berthold's suggestions do not satisfy the requirements will probably occur to everyone. The explanation offered to account for the complex karyokinesis cannot be regarded as fully satisfactory. At the same time some advantage may accrue from the new lights in which he puts the central figures of cell-division. We are here only half through the book however, and must proceed, confining our remarks still more closely.

Chapter VII. treats of the cell-network of plants, and the directions of cell-divisions, &c. It is in great measure a criticism of Sachs's celebrated view of the structure of the higher plants, and deals at some length with several of his positions. Of course, Berthold assumes primarily that the plant is to be regarded as chambered—cut up into cells, not built up of them. Two main principles are then employed. (1) The cell-divisions are, as a rule (at least in growing-points, &c.), halvings—*i.e.* each daughter-cell has the same cubic contents. This leads to a discussion of very many cases. Of course the shape of a segment does not forthwith enable us to judge of its relative contents, and difficulty occurs sometimes on this account: it is impossible to summarise the remarks, and especially since reference to the figures is necessary. (2) The second fundamental principle is that which regulates the position of fluid lamellæ elsewhere—the principle of least areas. The rule is that the new cell-wall takes such a direction that its area is the smallest possible. There are exceptions, *e.g.* cambium cells; but at least one feature appears to indicate a tendency to follow the principle—cell-walls never abut in the angles of cells. Sachs's law of rectangular division is comprehended as a particular case of Berthold's more general law: it fails where simultaneous divisions result in the formation of polygonal cells—*e.g.* in the embryo-sac—with walls inclined at angles greater than the right-angle.

The eighth chapter deals with the sculpturing on the interior of cell-walls, and allied phenomena; while Chapter IX. (the last) is devoted to "free cell-formation."

Enough has been said to show the wide scope of the book, though full criticism of it will only be possible after some of Berthold's test-cases have been worked over. Of course, from the nature of the work, it is open to the charge of being transcendental; but at the same time it must be allowed that we are getting into serious difficulties with protoplasm, and good, bold, shaking criticism is beneficial. In any case, several investigators will, no doubt, have something to say to Berthold's statements, for there is no lack of observations, old and new, as well as hypothesis, in the book we dismiss with this short review. H. MARSHALL WARD

ON THE EXPLOSION OF METEORITES

WE have received from M. Hirn a *tirage à part* of a communication to *L'Astronomie*, in which he discusses the various phenomena accompanying the explosion of meteorites, with a view to explaining their causes.

M. Daubrée, a long time ago, pointed out how very striking and difficult of explanation the noises are which are often heard in connection with the passage of meteorites, and called in question the explanation which had been given of their being really due to a veritable explosion.

M. Hirn, in his paper, begins by considering the causes which are at work in the production of the thunder which accompanies electric discharges, and of this he writes as follows:—"The sound, which we call thunder, is due, as everybody knows, to the fact that the air traversed by an electric spark, that is, a flash of lightning, is suddenly raised to a very high temperature, and has its volume moreover considerably increased. The column of gas thus suddenly heated and expanded is sometimes several miles long; as the duration of the flash is not even a millionth of a second, it follows that the noise bursts forth at once from the whole column; but for an observer in any one place it commences where the lightning is at the least distance. In precise terms, the beginning of the thunder-clap gives us the minimum distance of the lightning; and the length of the thunder-clap gives us the length of the column. It must be remarked that when a flash of lightning strikes the ground, it is not necessarily from the place struck that the first noise is heard." M. Hirn then gives an interesting case which proves the truth of this remark. He next points out that a bullet whistles in traversing the air, so that we can to a certain extent follow its flight; the same thing happens with a falling meteorite just before striking the earth. The noise actually heard has been compared to the flight of wild geese or the sound produced when one tears linen: it is due to the fact that the air rapidly pushed on one side in front of the projectile, whether bullet or meteorite, quickly rushes back to fill the gap left in the rear.

The most rapid cannon-shots scarcely attain a velocity of 600 metres a second, while meteorites penetrate the air with a velocity of 40,000 or even 60,000 metres per second; and this increased velocity gives rise to phenomena, which, although insignificant where cannon-shots are in question, become very intense and important when we consider the case of the meteorite. With that velocity the air is at once raised to a temperature of from 4000° to 6000° C. The matter on the surface of the meteorite will be torn away by the violence of the gaseous friction produced, and will be vaporised at the same time by the heat. This is undoubtedly the origin of the smoke which meteorites leave trailing behind them.

We have, then, precisely as in the case of lightning, a long narrow column of air, which is expanded, not so instantaneously certainly as by lightning, but at all events in an extremely short time and through a great length. Under these circumstances we should have an explosion in one case as in the other: a clap of thunder followed by a rolling noise more or less prolonged. If a cannon-ball could have imparted to it a velocity of 100,000 metres per second, it would no longer whistle, it would thunder, and at the same time it would produce a flash, as of lightning, and would be instantly burnt up. M. Hirn depends upon this line of reasoning to show that meteoric thunder need not necessarily have anything to do with an actual explosion. He then points out that the intensity of the noise produced in every point of its trajectory depends, (1) on the height; (2) on the velocity of the meteorite; (3) on its size; and (4) on the configuration of the country over which it passes. He refers to the observation of Saussure that a pistol fired at a height of 5000 metres makes very little noise: he then points out that at a height of 100,000 metres the density of the air is reduced to the small value of 0·000,000,004 krg.; the temperature being supposed to be − 200° C. In such a medium as this a meteorite could produce no sound, although it might give out a very brilliant light, because its temperature and light depend not on the absolute value, but on the rapid change of density.

SIR JOSEPH WHITWORTH

ON Saturday night last, Sir Joseph Whitworth died at the English Hotel, Monte Carlo. In the department of mechanical engineering there is, perhaps, no greater name, and his career was one upon which his countrymen may well look back with pride and pleasure. He was born on December 21, 1803, at Stockport, where his father was a schoolmaster. At the age of twelve he was sent from his father's school to Mr. Vint's academy at Idle, near Leeds, where he remained until he was fourteen, when he was placed with his uncle, a cotton-spinner in Derbyshire. Here he made himself familiar with the construction and working of all the machines then used in cotton-spinning. If he had chosen, he might perhaps have inherited his uncle's property, but he was already conscious of the true bent of his genius, and after six years' service, being unable to emancipate himself in a more regular manner, he ran away to Manchester. At Manchester he remained for four years, working in the shops of Messrs. Crighton and other employers, and obtaining a thorough mastery of the methods of manufacturing cotton-machinery. Recognising the necessity of wide experience, he went to London when he had secured all the practical knowledge that could be obtained in his special line at Manchester, and he was fortunate enough to be employed by Maudslay, who soon learned to appreciate his exceptional gifts, and took him into his own private workroom, and placed him next to Hampson, the best workman in the establishment. From Maudslay's, Mr. Whitworth went to Holtzapfel's, and afterwards to Clements's, where Babbage's calculating-machine was being constructed. During his residence in London, Mr. Whitworth began the splendid series of inventions which were to secure for him the foremost place among the mechanical engineers of his period. His first important self-imposed task was to construct the true plane, by which tool-makers might be enabled to produce, for all kinds of sliding tools, surfaces on which the resistance arising from friction would be reduced to a minimum. The work to be achieved was one of immense difficulty, and his fellow-workman, Hampson, used to laugh at him for having undertaken an impossible job. Mr. Whitworth, however, was a man of extraordinary tenacity of purpose, and did not allow himself to be discouraged. At last he succeeded, and showed his friend the perfect plane he had produced. "You've done it," said Hampson, who was astounded by a result which he had always thought to be beyond the reach of human effort.

In 1833, at the age of thirty, Mr. Whitworth, feeling that he might now safely trust to his own energies, returned to Manchester and opened a shop for the manufacture of engineers' tools. He was far from thinking that his first triumph had given the full measure of his powers. Already he had been working at another very complicated problem—how to do away with the inconveniences caused by variations in the pitch and thread of the screws used in the construction of machinery. In this enterprise he was as successful as in his first great undertaking. Obtaining specimens of the screws made by leading manufacturers, he constructed one which, without being exactly like any one of those before him, was the average of them all. It was everywhere accepted, and its introduction marked an era in the history of the manufacture of machinery. The advantage derived from the invention is that every screw of the same diameter has now a thread of the same pitch and of the same number of turns to the inch, and that all screws of the same size are interchangeable. His next achievement was the construction of an instrument capable of measuring the one-millionth part of an inch. This instrument was so delicate that when a steel bar 3 feet in length was warmed by momentary contact with a finger-nail, it at once indicated the expansion due to this slight cause.

As a maker of engineers' tools Mr. Whitworth of course soon became famous, and in 1853 he was sent to America as one of the Royal Commissioners to the New York Exhibition. Afterwards he drew up a remarkable report on American manufacturing industry. On his return to England it was suggested by the late Lord Hardinge that the great mechanician, whose fame was now firmly established, should be asked by the Government to design and produce machinery for the manufacture of rifles for the army. The rifles at that time issued to the army were carefully examined by him, and he decided that if his services were to be of any avail it would be necessary for him to determine the form and dimensions which would produce the best results. With an alacrity very unusual in such matters, the Government consented to erect in his private grounds at Rusholme, near Manchester, a shooting-gallery 500 yards long. Here Mr. Whitworth laboured assiduously, trying many kinds of experiment, and at every stage of his progress making absolutely sure of his ground before advancing a step towards fresh conclusions. The result of his investigations was to revolutionise the manufacture of rifles. As the *Times* has said, "he determined, by absolute and precise experiment, the effects of every conceivable pitch and kind of rifling, and of every length of projectile, from the sphere to one of twenty diameters in length; and he settled once for all the conditions of trajectory and of accuracy of flight." The significance of his efforts began to be understood by every one when, at the first Wimbledon meeting, Her Majesty fired the first shot from a Whitworth rifle, placed on a mechanical rest sliding upon true planes. At 400 yards' range the bullet struck the target on its vertical diameter, one inch and a quarter above the intersection of the horizontal. What he established with regard to rifles he found to be in the main true with regard to weapons of a larger calibre, and he proved the importance of this fact by constructing a series of magnificent cannon.

In the course of his inquiries as to the principles which ought to be observed in the manufacture of rifled small arms and ordnance, Mr. Whitworth became penetrated by the conviction that a new material must be provided, since mild steel was apt to be rendered unsound by the imprisonment of escaping gases during the process of cooling from the molten state. He solved the problem by using great hydraulic presses for the squeezing of the molten metal in the act of cooling, so that the particles might be brought into closer contact and the gases liberated. The steel produced by this method is remarkable for its lightness, strength, and tenacity, and is largely used in the construction of boilers, screw-propeller shafts, and for other purposes.

In 1869 Mr. Whitworth was created a baronet, and he had already been for some years a Fellow of the Royal Society and a D.C.L. of Oxford. He had amassed wealth, and thoroughly appreciated all the advantages it secured for him. He was, however, a man of enlightened ideas and generous impulses, and early in 1869 he did splendid service to mechanical and engineering industry by founding the Whitworth Scholarships, which he endowed to the extent of 100,000*l*. He was twice married—first, in 1825, to Fanny, youngest daughter of Mr. Richard Ankers; then, in 1871, to Mary Louisa, widow of Mr. Alfred Orrell. Notwithstanding his unwearied attention to business, he contrived to have some leisure time, and he spent it very agreeably at his estate of Stancliffe, in Derbyshire, where he devoted himself to landscape gardening. He also derived a great deal of pleasure from his horses and his herd of short-horns. For some time his health had been failing, and until lately he went every winter to the Riviera. Two years ago he made for himself at Stancliffe a winter garden, hoping that he might thus be able to spend the winter at home. Last year he went abroad again, and now, at the age of eighty-three, his long and great career has come to an end. The whole civilised world may be said to be familiar with his name, and he will always be remembered as the most illustrious English mechanician of the present age. Few men of his time have done more for the nation than Whitworth. His "Scholarships" have had the most important influence upon the knowledge and training of the rising generation of engineers. There are now nearly 200 Whitworth Scholars throughout the land, and they will doubtless be largely represented at his funeral.

NOTES

SINCE our last week's number was issued, Prof. Huxley has sent an important letter to the *Times* on the subject of the true functions of the Imperial Institute. From this letter we make the following extract:—"That with which I did intend to express my strong sympathy was the intention which I thought I discerned, to establish something which should play the same part in regard to the advancement of industrial knowledge which has been played in regard to science and learning in general, in these realms, by the Royal Society and the Universities. I pictured the Imperial Institute to myself as a house of call for all those who are concerned in the advancement of industry; as a place in which the home-keeping industrial could find out all he wants to know about colonial industry and the colonist about home industry; as a sort of neutral ground on which the capitalist and the artisan would be equally welcome; as a centre of inter-communication in which they might enter into friendly discussion of the problems at issue between them, and, perchance, arrive at a friendly solution of them. I imagined it a place in which the fullest stores of industrial knowledge would be made accessible to the public; in which the higher questions of commerce and industry would be systematically studied and elucidated; and where, as in an industrial University, the whole technical education of the country might find its centre and crown. If I earnestly desire to see such an institution created, it is not because I think that or anything else will put an end to pauperism and want—as somebody has absurdly suggested—but because I believe it will supply a foundation for that scientific organisation of our industries which the changed conditions of the times render indispensable to their prosperity. I do not think I am far wrong in assuming that we are entering, indeed have already entered, upon the most serious struggle for existence to which this country has ever been committed. The latter years of the century promise to see us embarked in an industrial war of far more serious import than the military wars of its opening years. On the east, the most systematically instructed and best informed people in Europe are our competitors; on the west, an energetic offshoot of our own stock, grown bigger than its parent, enters upon the struggle possessed of natural resources to which we can make no pretension, and with every prospect of soon possessing that cheap labour by which they may be effectually utilised. Many circumstances tend to justify the hope that we may hold our own if we are careful to 'organise victory.' But, to those who reflect seriously on the prospects of the population of Lancashire and Yorkshire—should the time ever arrive when the goods which are produced by their labour and their skill are to be had cheaper elsewhere—to those who remember the cotton famine and reflect how much worse a customer famine would be, the situation appears very grave. I thought—I still think—that it was the intention of the Prince of Wales and his advisers, recognising the existence of these dangers ahead, to make a serious effort to meet them, and it was in that belief that I supported the proposed Institute." We are glad to see that in the pamphlet which is now being circulated by the organisers of the Imperial Institute it is acknowledged that in this communication Prof. Huxley "has clearly defined the functions of the Imperial Institute as recognised by the propounders of the scheme."

THE Royal Society of New South Wales offers its medal and a prize of 25*l*. for the best communication (provided it be of sufficient merit) containing the results of original research or observation upon any one of a list of subjects which it has published. Communications on the following subjects must be sent in not later than May 1 next:—On the silver ore deposits of New South Wales; origin and mode of occurrence of gold-bearing veins and of the associated minerals; influence of the Australian climate in producing modifications of diseases; and on the Infusoria peculiar to Australia. A year later the Society will receive papers on the anatomy and life-history of the Echidna and Platypus; the anatomy and life-history of Mollusca peculiar to Australia; and the chemical composition of the products from the so-called kerosene shale of New South Wales. The subjects on which communications must be sent in not later than May 1, 1889, are:—On the chemistry of the Australian gums and resins; on the aborigines of Australia; on the iron ore deposits of New South Wales; list of the marine fauna of Port Jackson, with descriptive notes as to habits, distribution, &c. The competition is open to all without any restriction whatever, excepting that a prize will not be awarded to a Member of the Council for the time being; neither will an award be made for a mere compilation, however meritorious in its way. The communication to be successful must be either wholly or in part the result of original observation or research on the part of the contributor.

THE Compagnie du Congo pour le Commerce et l'Industrie is organising an expedition, composed of geologists and others, for the exploration of the Upper Congo and its tributaries.

WE regret to announce the death of Mr. Edward Livingstone Youmans, a well-known American writer on science. Mr. Youmans was born in New York in 1821, and though suffering much from defective vision, prosecuted from his early youth the study of science. He became well known as a public lecturer. He planned the "International Scientific Series" in 1871, in connection with which he made several visits to Europe. In 1872 he established the *Popular Science Monthly* in New York. Mr. Youmans died on Thursday last, January 20.

IT is sometimes said that intellectually Scotland does not stand on so high a level as in former times. This may be true so far as literature is concerned, but it is certainly not true with regard to science. At a recent meeting of the Royal Society of Edinburgh Mr. John Murray, of the *Challenger*, one of the Vice-Presidents, declared that he questioned whether any country in the world, taking its size into consideration, could show a better record of scientific work or a greater mass of scientific literature than Scotland during the past ten or twenty years. In making this statement Mr. Murray's object was not to glorify his own country but to show that its scientific establishments have a solid claim to better treatment than they have hitherto received at the hands of the Government. Money grants, he stated, of considerable annual value are devoted to the maintenance of learned Societies in London and Dublin. In Scotland, according to Mr.

Murray, the only grant of the kind is 300*l.* annually to the Royal Society of Edinburgh, and this is repaid to a Government Department in the form of rent. With regard to London, Mr. Murray, we think, should verify his references, as we know of no Society which receives "a money grant of considerable value."

THE death is announced of M. Feil, the well-known glass-founder, who prepared so many disks for the large telescopes in use in several Observatories; and of M. Mercadier, Professor of Physics to the Polytechnic School of Paris, and author of the only French book on electrical measures. M. Feil was seventy-four years of age; M. Mercadier ten years younger.

THE French are making use of their occupation of Madagascar to gain a thorough knowledge of the natural history of the island. There have already issued from the national press several fascicules of a magnificent "Histoire physique, naturelle, et politique de Madagascar," edited by M. Alfred Grandidier, to be completed in thirty volumes quarto. The subjects to be comprised in this work are: (1) physical and astronomical geography; (2) meteorology and magnetism; (3) ethnology, anthropology, and linguistics; (4) political, colonial, and commercial history; (5) natural history of mammals; (6) natural history of birds; (7) natural history of fishes; (8) natural history of reptiles; (9) natural history of Crustacea; (10) natural history of terrestrial and freshwater mollusks; (11) natural history of plants; (12) geology and palæontology. The various sections are intrusted to competent authorities; and the geological portion is to be illustrated by about 500 chromolithographs or coloured plates, the anatomical details being represented in lithography and photography. The total number of plates will not be less than 1200.

PREPARATIONS for the first general meeting, at Rome, of the International Statistical Institute are being made by the Executive Committee, consisting of Sir Rawson W. Rawson, K.C.M.G., C.B. (the President), M. E. Levasseur and Prof. von Neumann-Spallart (Vice-Presidents), Signor Luigi Bodio (General Secretary), and Mr. John Biddulph Martin (Treasurer). The arrangements will be announced by the Committee in due course.

ON Friday evening last an important lecture on "Modern War-Ships" was delivered at the Mansion House by Mr. W. H. White, Director of Naval Construction and Assistant Controller of the Navy. The lecture, which was illustrated by diagrams and models, was one of a series given by members of the Company of Shipwrights. Mr. White's object was to place before the meeting facts and figures illustrating the progress of war-ship building in recent years, and he confined his attention almost exclusively to the period between 1859, when the ironclad reconstruction of the Royal Navy began, and the present year. He presented a very lucid and interesting account of the extraordinary changes which have taken place during this time in the methods of war-ship building.

DR. HOPKINSON'S account of the electric lighthouses of Macquarie and of Tino, which was read before the Institution of Civil Engineers last month, has just been issued in pamphlet form, with a report of the oral and written discussion to which it gave rise.

IT is stated that the Lake District in New Zealand is showing signs of fresh disturbances. Tremors have been felt at Rotorua, and Tarawera has emitted dense volumes of steam. The Wahanga Peak appeared most active. No fire was visible, and after this outburst everything quieted down again.

WE have before us the first number of the "Bulletin of Miscellaneous Information," issued from the Royal Gardens, Kew. The "Bulletin" will appear from time to time as an occasional publication, and will contain notes, too detailed for the Annual Report, on economic products and plants to which the attention of the staff of the Royal Gardens has been drawn in the course of ordinary correspondence, or which have been made the subject of particular study at Kew. These notes will serve the purpose of an expeditious mode of communication to the numerous correspondents of Kew in distant parts of the Empire, and they will be useful to members of the general public interested in planting or agricultural business in India and the colonies. The present number contains much valuable information about Teff, one of the cereals indigenous to Abyssinia, and about Oil of Ben.

MESSRS. GINN AND CO., publishers, Boston, U.S.A., are about to issue a *Journal of Morphology*, which will be devoted principally to embryological, anatomical, and histological subjects. Mr. C. O. Whitman, Milwaukee, Wis., will be the editor. For the present only two numbers a year will be issued. The agent for Great Britain is Mr. W. P. Collins.

MESSRS. DE LA RUE AND CO. have in the press the second volume of "A Treatise on Electricity and Magnetism (Methods of Measurement and Applications)," by E. Mascart, Professor in the Collège de France, and Director of the Central Meteorological Bureau, and J. Joubert, Professor in the Collège Rollin. The work is translated by Dr. E. Atkinson, Professor of Experimental Science in the Staff College.

MESSRS. CASSELL AND CO. have just issued the first part of "Our Earth and its Story," a serial which will be completed in thirty six parts, and they are about to publish "Practical Electricity," by Prof. Ayrton.

M. BÉCLARD has presented some interesting statistics to the Academical Council of Paris on the number of female students in the Faculty of Medicine in the University there. He reports that since Germany closed the doors of its Universities to women, the number in Paris has been constantly increasing. At present the numbers of the various nationalities are: Russians 83, English 11, French 7, Americans 3, Austrians 2, Roumanian 1, and Turk 1. The greater number of these do not pursue their studies as far as the doctor's degree. The large proportion of Russian ladies is due to the closing of the female medical school recently founded at St. Petersburg. M. Béclard thinks that the number of students has now reached the maximum, and will probably decline, since the preliminary studies of the Faculty for both sexes have been made alike.

ALTHOUGH the competition which takes place annually for the vacancies in the assistantships in the Paris hospitals is not over, it is known that among the *internes* whose names will be published in some days, there will be one woman. Miss Klumpke is the first woman who has successfully competed for the *concours de l'internat*. In the written examination she ranked second (*ex æquo* with one or two others). She obtained 27 marks, the maximum being 30, and the highest number secured being 28. A good deal of grumbling is going on among the students. The idea of being distanced by a woman is not agreeable to them. Miss Klumpke has done very good work in neuro-pathology, and her name is known to all who study this branch of medicine.

THERE are at present nine female students at the Upsala University, three of whom study medicine, five philosophy, and one jurisprudence.

THE Aquarium constructed by the Executive of the Fisheries Exhibition in 1883 has just been sold by the Royal Commissioners by public auction, the property realising 100*l.* in the aggregate. Until recently it was expected that this Aquarium would be maintained as a part of the Buckland Museum. Many of the fish with which it made the British public familiar were hatched from ova of foreign fish. There were various Trans-atlantic forms; and fishes indigenous to India, China, Brazil, Austria, and many other countries were exhibited. Considering the fact that this Aquarium was the only one in London worthy of note, naturalists and the public have good reason to regret that it has been abolished.

MR. Z. NUTTALL, of the Peabody Museum, Cambridge, Mass., has been led to some interesting results by the study of the Mexican codices. Familiarity with certain phonetic symbols of frequent recurrence in these picture-writings enabled him to perceive that identical symbols are reproduced on the so-called Calendar Stone, the Sacrificial Stone, and other equally well-known Mexican monoliths. The Calendar Stone was, he maintains, the Market Stone of the city of Mexico, and he thinks that from the fixed market days recorded on it the Mexican calendar system may have sprung. The so-called Sacrificial Stone seems to him to have been a Law Stone, recording the periodical collection of certain tributes paid by subjugated tribes, and by others whose obligation it was to contribute to the common wealth of Mexico. Mr. Nuttall expresses his belief that many of the large stone receptacles which are generally called "vessels for containing the hearts and blood of human victims," were in reality standard measures kept for reference in the market place.

WE regret to hear of the death of Dr. Julius Lüttich, the well-known astronomer, who died in Rome on January 3; also of Prof. Jean Louis Trasenster, who died on the same day. M. Trasenster was Professor of Engineering and Mining at the Liége University.

THE Report of the Kew Committee for 1886, lately published, shows that the well-known work of the Kew Observatory has been actively carried on during the year. To particularise in certain subjects, it may be mentioned that in the magnetic observations four notable magnetic disturbances were recorded, occurring severally in the months of January, March, July, and October, and that the diurnal range of the declination for the summer and winter seasons, as well as the whole year, is given in a table in the Appendix. In solar observations the results of sketches of sunspots in continuation of Schwabe's enumeration are also recorded in the Appendix. The adoption of a new graphic process for determining cloud heights and motions, devised by Prof. Stokes, has been very satisfactory in saving computation when reducing the photographic pictures. Whilst thus adding its valuable yearly contributions to science, the Observatory is becoming more and more useful in results of immediate utility to the general public. In this respect the rating of watches is a matter of growing convenience to those who require a good time-keeper accompanied with a trustworthy certificate as to the performance of the watch they are about to purchase. Chronometers are also now rated here, and from the 35 days' period of trial in a range of 30° of temperature to which these instruments are subjected by the staff of the Observatory, there is every reason to believe in the ascertained rates. It is encouraging to note that increasing good work points to the necessity for enlarging the existing accommodation afforded by the buildings.

WE have received the third volume of the Proceedings and Transactions of the Royal Society of Canada. It relates to the year 1885. Among the scientific articles may be mentioned "The Artistic Faculty in Aboriginal Races" and "Palæolithic Dexterity," by Dr. Daniel Wilson; "A Natural System in Mineralogy, with a Classification of Native Silicates," by Dr. T. Sterry Hunt; "The Mesozoic Floras of the Rocky Mountain Region," by Sir W. Dawson; "Illustrations of the Fauna of the St. John Group, continued," by Mr. G. F. Matthew; "Catalogue of Canadian Butterflies, with Notes on their Distribution," by Mr. W. Saunders; and "The Skull and Auditory Organ of the Siluroid Hypophthalmus," by Mr. R. Ramsay Wright.

AN elaborate paper on "The Right Hand and Left-Handedness" was lately read before the Royal Society of Canada by Dr. Daniel Wilson, President of University College, Toronto. His final conclusion on this difficult subject, which he has repeatedly discussed from various points of view, is, that left-handedness is due to an exceptional development of the right hemisphere of the brain. Dr. Wilson, who is himself left-handed, concludes his paper with the expression of a hope that after his death his own brain may be "turned to account for the little further service of settling this physiological puzzle." "If my ideas are correct," he says, "I anticipate as the result of its examination that the right hemisphere will not only be found to be heavier than the left, but that it will probably be marked by a noticeable difference in the number and arrangement of the convolutions."

THE additions to the Zoological Society's Gardens during the past week include a White-whiskered Swine (*Sus leucomystox* ♀) from Loochoo Islands, presented by Mr. H. Pryer, C.M.Z.S; two Blackiston's Eagle Owls (*Bubo blackistoni*) from Yesso, Japan, presented by Mr. J. H. Leech, F.Z.S.; two Schlegel's Doves (*Chalcopeliæ puella*) from West Africa, presented by Mr. H. C. Donovan; a Macaque Monkey (*Macacus cynomolgus*) from India, a Suricate (*Suricata tetradactyla*) from South Africa, deposited; a Red Kangaroo (*Macropus rufus* ♀), a Yellow-footed Kangaroo (*Petrogale xanthopus* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THREE NEW COMETS.—The discovery of a great comet is telegraphed from several southern Observatories. So far as is yet known it was discovered by Mr. Thome at Cordoba on January 18. It was then situated in the constellation Grus; apparently not far from γ Gruis. On the following evening the tail only was seen at Melbourne, projecting some 30° above the south-western horizon. On January 20 it was remarked at Adelaide; here again the tail only was seen. In its physical appearance the comet strongly recalls the great southern comet of 1880, being long, narrow, and straight. It is not brilliant, though readily visible to the naked eye in the twilight. The tail was traced as far as α Toucani. It is expected that the comet will become very brilliant. The nucleus was observed at Adelaide and Melbourne on January 23. The Melbourne observation is as follows:—January 23d. 8h. 0m., R.A. 21h. 20m. 28s.; daily motion + 7m. 44s., Decl. 44° 17′ S., daily motion + 51′.

Another comet was discovered on January 22 by Mr. W. H. Brooks, of the Red House Observatory, Phelps, New York. Its place on that day at 6h. 54m. was R.A. 18h. 0m., Decl. 71° N. It was faint, and was moving slowly in an easterly direction. A third comet has been discovered by Mr. E. E. Barnard, Nashville, Tennessee; and observed at Harvard College as follows:—January 24d. 17h. 55·7m., R.A. 19h. 10m. 17·4s., daily motion + 2m. 36s., Decl. 25° 57′ 45″ N., daily motion − 0° 35′. The comet is faint.

NEW VARIABLES.—Mr. S. C. Chandler, Jun., writes in Gould's *Astronomical Journal*, No. 149, to state that the period of the new variable of the Algol type, D.M. + 34° No. 4181, the discovery of which we announced last week (p. 282), is not yet precisely known. It is either 5·997d. or some aliquot part thereof, but not either the third or fifth part. The approximate elements supplied by Mr. Chandler are as follows:—

$$1886 \text{ December } 9\cdot458\text{d. G.M.T.} + \left(\frac{5\cdot997\text{d.}}{n}\,E\right),$$

where n can be neither 3 nor 5. The period may therefore be about three days, one day and a half, or a shorter period still. An examination of the relation which the duration of the oscillation in the light of the other stars of the type bears to the whole period leads Mr. Chandler to conclude that the most probable period is one of 1d. 11h. 59m., or if not that, 20h. 34m., or possibly 18h. 6m. The following table shows that the shorter the period of the variable, the higher is the ratio which the period of oscillation bears to it. In the present star the oscillation probably occupies about six hours; a period so great as three days or much shorter than one day would make it, therefore, an exception to the rule followed by the other seven stars of the same order.

Star	Period h.	Oscillation h.	Ratio
U Ophiuchi	20·13	5·0	0·248
δ Libræ	55·85	12·0	0·214
U Cephei	59·82	10·0	0·167
Algol	68·81	9·15	0·134
U Coronæ	82·85	9·75	0·118
λ Tauri	94·87	10·0	0·105
S Cancri	227·63	21·5	0·094

The variable was discovered by Mr. Chandler and not by Dr. Gould as at first reported.

Mr. Espin, in Circular No. 12 of the Liverpool Astronomical Society, notes the variability of a star 0m. 35s. *p* and 0° 8′ *n* of θ^1 Tauri. It is probably a variable of long period ranging from 9 m. ± to below 12 m. Its place for 1885·0 is R.A. 4h. 21m. 25s., Decl. 15° 50′·7 N.

THE WASHINGTON OBSERVATORY.—The Annual Report of the U.S. Naval Observatory, dated October 30, 1886, has recently been issued. Commodore G. E. Belknap, who was Superintendent of the Observatory at the date of the last Report, retired from that post on June 7, and was succeeded by Commander Allan D. Brown, who therefore is the writer of the Report now before us. In connection with the Chronometer and Time-Service Department, under Lieut. S. C. Paine, it is remarked that the time-service continues to increase in popularity, and its usefulness is daily becoming more apparent to the public. The time-balls that have been established have been much appreciated, and are of great value to the shipping and commercial interests. Much attention appears also to have been given to the chronometer trials, it evidently being the desire of the Observatory to afford makers every assistance in its power in obtaining data that will tend to the improvement of chronometers. The 26-inch refractor, in charge of Prof. Asaph Hall, has been used in observations of satellites, of double stars, and of Saturn. Observations of stellar parallax have also been made. The reduction of the observations of Iapetus and of the six inner satellites of Saturn, as well as those for stellar parallax, have been completed, and the results published. The transit-circle has been employed in observations of stars of the American ephemeris, of the sun, moon, and planets, and such miscellaneous stars as were necessary to complete the data for the proposed transit-circle Catalogue. The whole number of observations since the last Report has been 5180. The reductions have also been proceeded with as rapidly as possible. The instrument remains in charge of Prof. J. R. Eastman. Photographs of the sun have been taken with the photo-heliographic apparatus lately belonging to the Transit of Venus Commission, whenever practicable. The work was commenced on January 11, 1886; and up to and including September 30, 1886, there have been obtained ninety-eight negatives showing spots on the sun's surface. Hitherto no photographs have been taken, except when the sun showed spots on his disk, and then one only near noon. This work has been intrusted to Ensign A. G. Winterhalter, who hopes that in the future the number of photographs in a given period will be considerably increased, better arrangements having been made for securing them between 10 a.m. and 2 p.m.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 *JANUARY* 30—*FEBRUARY* 5

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on January 30

Sun rises, 7h. 44m.; souths, 12h. 13m. 31·8s.; sets, 16h. 43m.; decl. on meridian, 17° 39′ S.: Sidereal Time at Sunset, 1h. 21m.

Moon (at First Quarter on February 1) rises, 10h. 23m.; souths, 16h. 50m.; sets, 23h. 27m.; decl. on meridian, 4° 40′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	7 46	11 55	16 4	20 56 S.
Venus	8 25	13 11	17 57	14 51 S.
Mars	8 31	13 29	18 27	12 40 S.
Jupiter	0 35	5 37	10 39	12 3 S.
Saturn	14 29	22 36	6 43*	22 11 N.

* Indicates that the setting is that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich)

Jan.	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image ° °
30	ν Piscium	4½	21 24	22 15	185 289
Feb.					
3	48 Tauri	6	1 15	2 9	150 300
3	B.A.C. 1526	6	18 26	19 24	122 233

Variable Stars

Star	R.A. h. m.	Decl. ° ′		h. m.
R Andromedæ	0 18·1	37 57 N.	Jan. 31,	0 0 *m*
U Cephei	0 52·3	81 16 N.	,, 31,	22 0 *m*
			Feb. 5,	21 39 *m*
Algol	3 0·8	40 31 N.	,, 5,	4 23 *m*
ζ Geminorum	6 57·4	20 44 N.	,, 3,	0 0 *M*
δ Libræ	14 54·9	8 4 S.	,, 3,	1 49 *m*
S Serpentis	15 16·4	14 43 N.	,, 4,	*M*
U Ophiuchi	17 10·8	1 20 N.	Jan. 30,	4 16 *m*
			and at intervals of	20 8
β Lyræ	18 45·9	33 14 N.	Feb. 4,	19 0 m_2
δ Cephei	22 25·0	57 50 N.	,, 4,	1 0 *M*

M signifies maximum; *m* minimum; m_2 secondary minimum.

GEOGRAPHICAL NOTES

IN connection with Major Macgregor's paper on his journey from Upper Assam to the Irrawadi, read at a recent meeting of the Royal Geographical Society, and printed in the new number of the Proceedings, Dr. G. Watt made some valuable remarks on his own observations in the Manipur district. Manipur is a small valley surrounded by mountain-ranges, and in this valley the rainfall was found to be only about 39 inches, but seventeen miles off, in the mountains which formed the north-east ranges, the rainfall was as much as 120 inches, and towards the Naga country to the north it became greater and greater in certain limited tracts. In the Khasia Hills 600 inches might fall in one place, and twenty miles off only 50 inches. Nothing in Manipur struck Dr. Watt so much, as a botanist, as the remarkable transitions of vegetation in that small region. Dr. Watt gathered twelve or more species of oaks, many of which were new to science, and ten or twelve species of rhododendrons, in Manipur alone. The *Rhododendron Falconeri*, found in the Naga Hills by Sir Joseph Hooker, is nowhere met with in the immense tract between the Naga Hills and Sikkim. This and the epiphytic *R. Dalhousiæ*, which grows on a hill thirty miles north of Darjeeling, Dr. Watt found in the Naga Hills at an altitude of 6000 to 8000 feet, and these rhododendrons never occur in Sikkim below 10,000 to 13,000 feet. There were many instances of plants falling in their altitude as the traveller passed to the east and south-east from Sikkim, until at Moulmein a rhododendron was found growing near the sea, a circumstance which was not met with in any other part of Asia. There is something in that region which, apart from pure geography, is of vital interest. Sarameti, which is under 13,000 feet high, the natives said, had snow all the year round, whereas on the Himalayas the lowest point at which snow occurs is 17,000 feet. In Manipur, the whole valley, 3000 feet high, was covered with hoar-frost in December. Dr. Watt thought this was a point that should be thoroughly investigated: what is the cause of this falling in altitude in the vegetation? General Strachey, who was in the chair, considered that the peculiarities of the vegetation of Manipur compared with Assam were connected with the evident lowering of temperature indicated by the low snow-line. There could be no doubt that the warm currents of air coming up the valleys of the Irrawadi and the Salween and meeting the snowy mountains to the north produced an enormous precipitation of rain, which during winter fell as snow. The consequence seemed to be that there was snow there at a very much lower level than in the mountains further to the north. That an immense quantity of rain fell in the upper portions of the valley of the

Irrawadi there could be no question. Such a rainfall seemed in itself quite sufficient to account for the large volume of water that was drained off by the lower portions of the Irrawadi; and anybody who knew what Tibet was, General Strachey stated, must be aware that, even with a course of several hundred miles, the river would pick up but a small quantity of water in comparison with the enormous volumes which were collected from the rain which fell in Upper Burmah. General Strachey had roughly calculated that a monthly fall of rain of 18 inches over a square degree would mean 65,000 cubic feet per second for the whole month.

THE latest news from Dr. Bunge, chief of the Russian Polar Station at the mouth of the Lena, is encouraging. Telegraphing from Orkinga, a telegraph-station on the road to Yakutsk, Dr. Bunge informs the Academy of Sciences at St. Petersburg that his expedition has had a successful issue. They passed the summer in two islands of the New Siberia Archipelago; Bunge on Great Liakovsky, and Toll on Kotelnoy Island. During spring all the five islands of the group were explored, New Siberia especially by Toll. The mainland was reached at the end of October. The scientific results are very considerable, and, as we know so little about these islands, are likely to be novel.

MM. POTANIN, SKASSY, AND BÉRÉSOFSKY have lately returned from their expedition to China and Mongolia, bringing numerous collections in anthropology, zoology, and botany, besides maps of the country which they have traversed during their three years' journey (1884-86). The Russian Geographical Society has nominated a committee, consisting of MM. Stebnitsky, Tillo, Mushketoff, and Schmidt, to make inquiries as to the desiccation of Siberian lakes. It is expected that an expedition will be despatched to investigate the subject on the spot.

WE learn that the geographical results achieved by the Survey officers on the Afghan Frontier Commission extend over 100,000 square miles of country. The Indian Survey officers have been very busy in Upper Burmah. Captain Hobson's map, prepared from all available sources, in 14 sheets, is all published already. A reduction therefrom, on the scale of 16 miles to an inch, has been prepared in the Surveyer-General's Office, Calcutta, and published also. The Survey party, which has lately completed the Andaman Islands survey, left Calcutta on November 19, under the charge of Major G. Strahan, R.E., to undertake the survey of the Nicobar Islands.

THE ESKIMO

SPECIAL interest attaches to a paper on "The East Greenlanders in their relations to the other Eskimo Tribes," contributed by Dr. H. Rink to the current number of the *Deutsche Geographische Blätter* (Bremen, 1886). Hitherto these hyperboreans have been studied by independent observers, chiefly in Alaska at the eastern, and in Greenland and Labrador at the western extremity of their domain, while through lack of sufficient materials the intermediate branches thinly scattered round the Arctic shores from the Mackenzie to Baffin Bay have been mostly neglected. Here, however, we have for the first time a comprehensive ethnological survey of the whole field by perhaps the greatest living authority on the subject, based on the rich collections recently brought to Europe by Capt. Holm from East Greenland, by the brothers Krause and A. Jakobsen from Alaska, and by F. Boas from the central region of Baffin Land.

With these materials before him, and keeping in view the facts already determined by previous students, Dr. Rink is able to throw much light, if not on the origin, at least on the general line of dispersion, and still more on the social evolution and art history, of the Eskimo race. He makes it sufficiently evident that their primæval home must be placed in the extreme northwest, on the Alaskan shores of the Bering Sea, where they probably acquired a knowledge of some of the useful industries connected with navigation, fishing, and hunting from the neighbouring Indian tribes of Athabascan stock. From this point the migratory movement appears to have been partly across the neck of the Alaskan Peninsula to the Copper River, where their further progress in this direction was arrested by the Thlinkit Indians on the coast and by the Athabascans in the interior. But their wanderings were chiefly directed towards the north and east, that is, along "the line of least resistance" around the unoccupied Arctic seaboard down to Baffin Bay, which seems to have formed a fresh point of dispersion, southwards to Labrador and eastwards to East and West Greenland. Dr. Rink is inclined to accept the view of Capt. Holm, that the Angmagsaliks, or East Greenlanders, found their way round the unexplored north coast of Greenland to their present homes, and that the West Greenlanders passed from Baffin Bay directly southwards, while a mixed race, most probably including old Norse elements, was developed at the southern extremity of the peninsula. In the extreme west there has also been a slight intermingling, with Thlinkits about the Copper River, and with Athabascans, back of Kotzebue Sound; but elsewhere the Innuit and Karalik (Western and Eastern Eskimo) have kept entirely aloof, nowhere amalgamating with the Red Man, and keeping mainly to the seaboard throughout the whole extent of their domain, which, between the Copper River and Cape Farewell, Greenland, cannot be estimated at less than 7000 miles in extent, although scarcely anywhere exceeding 150 miles inland from the coast. This explains the curious fact that the social organisation of the Indian tribes in families, gentes, phratries, confederacies, and nations can nowhere be detected amongst the Eskimo, unless to it is to be attributed a certain restriction in the choice of a wife, and an obligation to lend each other mutual aid, universally recognised amongst all branches of the race. Even the general distribution into tribes, assumed by most writers, appears to be quite groundless, and the final syllable, *mût*, *miût*, of the so-called tribal names, meaning "dweller," "inhabitant of," shows that they are purely *topographical* terms without any ethnical significance whatsover. Thus, Angmagsalingmiût, Mahlemiût, Aglemiût = inhabitants of the *Angmagsalik*, *Mahle*, *Agle* districts, and so on; so much so, that a family migrating from one of these districts to another changes its name accordingly. Hence Dr. Rink considers it sufficient for all practical purposes to class the whole race into the following seven *geographical* groups:—(1) South Alaskan; (2) North Alaskan; (3) Mackenzie; (4) Central (Baffin Land, &c.); (5) Labrador; (6 and 7) West and East Greenland. Between these various groups there certainly exist differences, by which they may often be readily distinguished; but these are mainly of a social and linguistic, and to a less extent of a physical character; and such is the great uniformity even in the structure of the Eskimo tongue, that an East Greenlander and an Alaskan, if fortuitously thrown together, would soon begin to understand one another. It is noteworthy that in Greenland, where the language has been most carefully studied, greater differences are observed between the eastern and western than between the northern and southern dialects—a circumstance doubtless due to the different routes followed by the two streams of immigration from the central region. Compared with the West Greenland dialect, taken as the written standard, the Labrador is found to contain 15, the Central 20, the Mackenzie 31, and the Alaskan 53 per cent. of different root-words—relations which correspond remarkably well with the conclusions arrived at, on other grounds, regarding the general migratory movement from Alaska, the assumed cradle of the race.

But here an important exception is formed by the Aleutian Islanders, who are treated by Dr. Rink as a branch of the Eskimo family, but whose language diverges profoundly from, or rather shows no perceptible affinity at all to, the Eskimo. The old question respecting the ethnical affinities of the Aleutians is thus again raised, but not further discussed by our author. To say that they must be regarded as "ein abnormer Seitenzweig," merely avoids the difficulty, while perhaps obscuring or misstating the true relations altogether. For these islanders should possibly be regarded, not as "an abnormal offshoot," but as the original stock from which the Eskimos themselves have diverged. It is remarkable that in his new work on "Alaska and the Seal Islands" Henry W. Elliott discovers a striking resemblance between the Aleutians and the Japanese. They constantly remind him of "Japanese faces and forms in another costume," so much so that in his opinion they form "a perfect link of gradation," not between the Eskimo and Red Man, nor between the Eskimo and Asiatic hyperboreans, but "between the Japanese and Eskimo" (p. 173). Mr. Elliott may have here unconsciously hit upon the solution of a very interesting ethnological problem, for in his "Classification of the Varieties of the Human Species" (*Journal* of the Anthropological Institute, May 1885), Prof. Flower also connects the Eskimo with the Japanese:—"Every special characteristic which distinguishes a Japanese from the average of mankind is seen in the Eskimo in an exaggerated degree, so that there can be no doubt about their being derived from the same stock. It has also been shown that these special characteristics gradually increase from west to east, and are seen in their greatest perfection in the inhabitants of Greenland, at all events in those where no crossings with the Danes have taken place."

The Aleutians would thus help to bridge over the somewhat abrupt gap still undoubtedly separating the Eskimo and Japanese groups. At the same time this view suggests a primæval line of migration from Japan through the Kurile Islands and Kamchatka to the Aleutian chain and Alaska, which again presents other difficulties of a somewhat formidable character. In the first place, the Japanese appear to be themselves only comparatively recent intruders in Niphon, whose primitive inhabitants were the Ainos, a people of totally different physical type. Hence it is not easy to understand how they could have thrown off an easterly branch, which has had time to develop into the Eskimo, probably the most specialised of all existing races. In the second place, in his "Tales and Traditions of the Eskimo," Dr. Rink himself advances some solid reasons for bringing the Eskimo, not from Asia at all, or at least not in the first instance, but from the interior of the North American continent. He holds in fact, with some other ethnologists, that they were originally inlanders, who, under pressure from the American Indians, gradually advanced along the course of the Yukon, Mackenzie, and other great rivers, to their present homes on the Bering Sea and Frozen Ocean. But a discussion of these contradictory theories, for which a solution may yet be found, must be deferred to another occasion. Meantime enough has probably been said to show the highly suggestive character of the paper under review.

A. H. KEANE

SCIENTIFIC SERIALS

L'Astronomie: Revue mensuelle d'Astronomie populaire, de Météorologie, et de Physique du Globe, January 1887.—We have received the January number of the above periodical, edited by Camille Flammarion. M. Flammarion has done a great work in popularising astronomy in France, and the success which has attended this review—for it is entering on its sixth year—proves how widespread an interest is now taken in the science in that country. The present number contains an "Annuaire astronomique pour 1887," by the editor, a series of descriptive notes of a general character on the principal objects of astronomical observation for the current year, the sun, moon, eclipses, occultations, and the planets. M. Daubrée follows with a paper on some recent meteorites. M. Flammarion gives an account of the storms of October 16 and December 8, and of the general principles of weather prophecy. The notes chiefly relate to the two comets of the season, those of Barnard and Finlay, three diagrams being given of the first, showing the position and character of the two tails, and one of the second. A sort of general observing ephemeris for the month January 15 to February 15, of a popular rather than of a scientific character, concludes the number. M. Flammarion and his co-workers frequently affect a somewhat magniloquent and sensational style, and deal principally with the more popular, easy, and interesting aspects of astronomy; the wonders of our own globe, earthquakes, volcanoes, &c., receive much attention, so that the field embraced is not confined to pure astronomy alone. But after every allowance is made and every drawback admitted, *L'Astronomie* has done much good in circulating astronomical information and in arousing and fostering scientific tastes, and it must be confessed that for an astronomical journal containing forty well-printed imperial octavo pages and, as in this case, more than thirty illustrations, to command a remunerative circulation at the price of a franc a number is highly creditable alike to editor, to publishers, and to the public which supports it. It may well be doubted whether such an enterprise would meet with the same success either here or in America.

Bulletin de l'Académie des Sciences de St. Pétersbourg, tome xxx., No. 4.—The appearance of Encke's comet in 1885 compared with its previous appearances, by O. Backlund. The paper is the first of a series, and contains, besides the numerical data of the observations made in 1885, an inquiry into the disturbances due to the attraction of the earth. The summer parallax of the earth is taken to be 8″·80, and the elements of the comet are determined accordingly.—On the formation of buds among the Phanerogams, by A. Famintzin.—The period of the rotation of the sun, according to the magnetic disturbances, as observed at Pawlowsk, by P. A. Müller. The average value of 25·66 is deduced from observations made from August 1, 1882, to August 31, 1883.—Photography applied to astronomy; abstract of a lecture by Otto Struve.—On several new Trilobites and kindred forms from East Siberia, by Fr. Schmidt. The following species (nearly all new) are described, with plates:—From the Cambrian, on the Vilui River, *Anomocare pawlowskii* and *Liostracus* (?) *maydeli*; from the Cambrian on the Olenek, *Agnostus czekanowskii*; from the Lower Silurian of the Middle Tunguska, *Phacops lopatini* and *P. sibiricus*; from the Devonian limestone at Krasnoyarsk, *Proëtus slatkowskii*, *Cyphaspis sibirica*; *Eurypterus* (?) *czekanowskii*, and *E. punctatus* from the Devonian on the Angara at Padun.—A new form of *Opalina* (*spiculata*), by Warpachowsky.—On a new *Otomela* (*bogdanowi*), by V. Bianchi.—Remarkable hail at Bobruisk, by H. Wild (with plates). On November 28, 1885, with an absolutely clear sky, not a cloud being visible, hail fell for five minutes. The fall was quite local, and did not extend farther than five miles from Bobruisk. Many pieces were like broken pieces of ice, others apple-shaped, with conical depressions at the poles.—On the electromotory difference and the polarisation of electrodes on telegraphic lines, by P. Müller.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 13.—"On the Crimson Line of Phosphorescent Alumina." By William Crookes, F.R.S., V.P.C.S.

In a paper which I had the honour of communicating to the Royal Society in March 1879 (Phil. Trans., Part 2, 1879, pp. 660, 661), I described the phosphorescence of alumina and its various forms when under the influence of the electrical discharge *in vacuo*, in the following words:—"Next to the diamond, alumina in the form of ruby is perhaps the most strikingly phosphorescent stone I have examined. It glows with a rich full red; and a remarkable feature is that it is of little consequence what degree of colour the earth or stone possesses naturally, the colour of the phosphorescence is nearly the same in all cases; chemically precipitated amorphous alumina, rubies of a pale reddish yellow, and gems of the prized 'pigeon's blood' colour, glowing alike in the vacuum, thus corroborating E. Becquerel's (*Annales de Chimie et de Physique*, vol. lvii. 1859, p. 50) results on the action of light on alumina and its compounds in the phosphoroscope. . . . The appearance of the alumina glow in the spectroscope is remarkable. There is a faint continuous spectrum ending in the red somewhere near the line B; then a black space, and next an intensely brilliant and sharp red line to which nearly the whole of the intensity of the coloured glow is due. . . . This line coincides with the one described by E. Becquerel as being the most brilliant of the lines in the spectrum of the light of alumina, in its various forms, when glowing in the phosphoroscope."

In the *Comptes rendus* for December 6 last (vol. cii. p. 1107) appears a brief note by M. de Boisbaudran, in which he announces, "to that date, that alumina, calcined and submitted to the electrical discharge in a vacuum, has not given him a trace of red fluorescence. This fluorescence, as well as its special spectrum, shows itself brilliantly when the alumina contains 1/100 and even 1/1100 of Cr_2O_3. With the 1/10,000 part of Cr_2O_3 we still obtain very visible rose colour. . . . From these observations the presence of chromium appears to be indispensable to the production of the red fluorescence of alumina."

This statement being opposed to all my experience, I immediately instituted experiments with a view, if possible, to clear up the mystery. I started with aluminium sulphate which I knew to be tolerably pure, and in which ordinary tests failed to detect chromium. On ignition and testing in the usual manner in a radiant-matter tube the alumina line was brightly visible in the spectrum of the emitted light. Different portions of this aluminium sulphate were now purified by various processes for the separation of chromium. All gave as a result the absence of this impurity. The most trustworthy process being that devised by Wöhler ("Select Methods in Chemical Analysis," second edition, p. 124), I used it to purify the bulk. The salt was dissolved in water, and excess of caustic potash added till the precipitate first formed re-dissolved. Chlorine was now passed through till no more precipitate fell down and the liquid retained a strong odour of chlorine. The whole of the chromium would now be in solution, whilst the alumina would be in the precipitate. The alumina was filtered off, well washed, and a portion tested in the radiant-matter tube. It gave as good an alumina spectrum as did the original sulphate; the crimson line being very prominent.

The alumina thus purified was a second time dissolved in caustic potash and submitted to the chlorine purification. Again in the radiant-matter tube the alumina gave its characteristic crimson line spectrum.

Many other experiments are given, and the paper concludes as follows:—

These experiments are perhaps too few to permit any important inference being drawn from them. There seems, however, to be four possible explanations of the phenomena observed:—

(1) The crimson line is due to alumina, but it is capable of being suppressed by an accompanying earth which concentrates towards one end of the fractionations.

(2) The crimson line is not due to alumina, but is due to the presence of an accom ..nying earth concentrating towards the other end of the frac .nations.

(3) The crimson line belongs to alumina, but its full development requires certain precautions to be observed in the time and intensity of ignition, degree of exhaustion, or its absolute freedom from alkaline and other bodies carried down by precipitated alumina, and difficult to remove by washing; experience not having yet shown which of these precautions are essential to the full development of the crimson line and which are unessential.

(4) The earth alumina is a compound molecule, one of its constituent molecules giving the crimson line. According to this hypothesis alumina would be analogous to yttria.

Zoological Society, January 18.—Prof. W. H. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of December 1886, and called attention to a young male of the true Zebra (*Equus zebra*), purchased December 11; and to a young male Indian Rhinoceros, presented by H.H. the Maharajah of Cooch Behar, through the kind intervention of Dr. B. Simpson, and received December 25.—Mr. F. W. Styan exhibited and made remarks on a series of Chinese birds' eggs which he had collected at Kiukiang and Shanghai.—Mr. Howard Saunders exhibited and read some notes on a skin of the Mediterranean Black-headed Gull (*Larus melanocephalus*), killed on Breydon Water, near Great Yarmouth, and sent for exhibition by Mr. G. Smith, of that town. This was stated to be the first absolutely authentic occurrence of this southern species on the British coasts.—Mr. Sclater exhibited and made some remarks on an example of a rare Amazon Parrot (*Chrysotis bodini*) from British Guiana.—Various other specimens were exhibited, and papers read.

EDINBURGH

Royal Society, January 17.—Sheriff Irvine, Vice-President, in the chair.—Mr. John Murray read a paper on the total rainfall of the globe, and its relation to the discharge of rivers. 2243 cubic miles of rain fall annually on areas with inland drainage. Such areas extend to 11,486,350 square miles. The land draining directly to the ocean has an area of 44,211,000 square miles. If from this quantity we subtract all areas having less than 10 inches of annual rainfall, we get 38,829,750 square miles. The mean discharge from this area into the ocean is 6569 cubic miles annually. The total weight of substances carried by this means to the ocean is rather more than 5,000,000,000 tons each year.—Mr. W. Durham read a paper on chemical affinity and solution.—The fourth part of a paper on thermometer-screens was communicated by Mr. John Aitken.—Prof. Armstrong read a paper by Mr. A. C. Elliot, containing an extension and improvement of Rankine's formula for the pressure of earth on a retaining wall.—Prof. Tait communicated the third part of his paper on the foundations of the kinetic theory of gases. In the first division of this part the author discusses the modifications which are introduced into his previous formulæ by the consideration of the effects of molecular attraction of small range, but great intensity, on the behaviour of a group of hard, smooth, impinging spheres. In the second division he makes the assumption that the spheres are not perfectly hard, but possess a definite coefficient of restitution. He then endeavours to make an approximation to the conditions of the liquid state by considering the action of spheres whose relative speed of approach is such that, after impact, they are unable to pass out of the range of molecular attraction in consequence of the loss of translational energy by impact.

DUBLIN

Royal Society, December 15, 1886.—The physical properties of manganese steel, by Prof. W. F. Barrett. The author pointed out that Mr. J. T. Bottomley had sent a brief note on the feebly-magnetic character of manganese steel to the Aberdeen meeting of the British Association, and had kindly furnished him with a specimen of this steel, and the name of the makers and patentees, Messrs. Hadfield and Co., of Sheffield. The steel contains 12 to 14 per cent. of manganese. Through Messrs. Hadfield, the author had obtained wire drawn from manganese steel, a process that first presented great difficulties, but was ultimately accomplished with ease by heating the steel to whiteness, and quenching in cold water after a reduction through every two sizes had been drawn. Sudden cooling softens this steel; slow cooling hardens it. A No. 19 S.W.G. wire (diameter 0·98 millimetre) was thus obtained of two kinds—hard and soft; the density was 7·808. The *electric conductivity* was found by Prof. Barrett to be very low. The No. 19 wire had a resistance of about an ohm per metre, the exact specific resistance in C.G.S. units being 77,000 for 1 cubic centimetre; ordinary iron is only 9800, and German silver 21,170 in the same units; so that some use might be made of manganese steel wire for resistance-coils in electric lighting. The variation of resistance with temperature is now being examined. The *magnetic character* of this steel was then carefully tested by the author. Mr. Bottomley found the intensity of magnetisation of this steel, after submitting it to the most powerful magnetising force, was 2·55 in C.G.S. units, or the magnetisation per gramme was 0·013 in C.G.S. Ordinary steel gives a number varying from 40 to 90, and even 100, C.G.S. units per gramme. So that, if ordinary steel of average quality be 100,000, manganese steel is 20. This represents the permanent magnetism. Prof. Barrett, by different methods, has determined the susceptibility—that is, the induced magnetisation—in a uniform field. Compared with iron as 100,000, manganese steel was found to be 300. In fact, it is very wonderful, judging by muscular sense, to find no sensible force required to move this steel, even in the most powerful magnetic field that could be obtained. Hence, as the author suggests, the use of manganese steel for the bed plates of dynamos and the plating of iron vessels is obvious. Ships built of such steel would have no sensible deviation of the compass. As excellent castings can be obtained from this steel, it ought to have many applications from its extreme hardness, enormous tenacity, and feebly-magnetic character. Dr. Hopkinson's important memoir on the magnetisation of iron contains a measurement of the magnetic susceptibility of manganese steel, of which Prof. Barrett was unaware until his paper had been written. Though Dr. Hopkinson's method of determination was wholly different, the ratio of the susceptibility of iron to manganese steel which he obtained is fairly accordant with the number obtained by the author, the composition of the specimen being the same in both cases. As regards the *tenacity* of manganese steel, the author had found the hard wire had the extraordinary tenacity of 110 tons per square inch, or 173·5 kilogrammes per square millimetre—a number confirmed by independent tests which the chief engineer of the Irish Great Southern and Western Railway Works had kindly made for Prof. Barrett. The tenacity of ordinary steel wire is from 80 to 100 kilogrammes per square millimetre, the best pianoforte steel wire alone showing a higher tenacity than the manganese steel wire. The soft manganese steel wire had a tenacity of only 48 tons per square inch, with an elongation of nearly 20 per cent. The *modulus of elasticity* was also determined by the author by direct stretching. It was found to be lower than wrought iron, the mean number for the hard manganese wire being 16,800 kilogrammes per square millimetre, the soft manganese wire having a still lower modulus. The modulus for ordinary steel wire is 18,810, and for iron wire 18,610 kilogrammes per square millimetre; so that, though hard manganese steel has an enormous tenacity, it "gives" more than steel under sudden stress, recovering itself, of course, if the limits of elasticity are not passed. Obviously this is a most useful property for many purposes to which the steel may be applied. Further experiments on this interesting material are in progress in the Physical Laboratory of the Royal College of Science.

PARIS

Academy of Sciences, January 17.—M. Gosselin, President, in the chair.—Obituary notices of M. Paul Bert on the occasion of his obsequies at Auxerre, by M. Janssen in the name of the Academy of Sciences, and by M. A. Chauveau on behalf of the Biological Society.—Observations of the minor planets made with the large meridian instrument of the Paris Observatory during the third quarter of the year 1886, communicated by M. Mouchez. Comparative observations are here tabulated for Electra, Aletheia, Olympia, Juno, Pallas, Ceres, and several other minor planets. Those for the three last mentioned are referred to the ephemerides of the "Nautical Almanac," all the others to those of the "Berliner Jahrbuch." The observations were taken by MM. F. Boquet, O. Callandreau, and P. Puiseux.—Study of the horizontal flexion of the telescope of the Bischoffsheim meridian-circle of the Paris Observatory, by MM. Lœwy, Leveau, and Henri Renan.—On the solar statistics of the year 1886, by M. R. Wolf.—Letter addressed to the Academy by M. Em. Barbier, thanking it for the Francœur Prize recently awarded to him, and submitting a means by which he has succeeded in converting an ordinary watch into a repeater. A process is also explained by which a person both deaf and blind may tell the time by this repeater.—On the accelerations of the points of an invariable system in motion, by M. Ph. Gilbert. Two cases are considered: (1) that of a solid revolving round a fixed point, O; (2) that of a free solid body.—On the laws determining the absorption of light in crystals, and on a new method enabling the observer to distinguish in a crystal certain absorption-bands belonging to different bodies, by M. Henri Becquerel. His researches in this branch of physics have led the author to several important conclusions here specified on the absorption of light in crystals. He finds generally that in different crystals the characters of the phenomena of absorption differ considerably from those that one might expect to observe, regard being had to the optical properties of the crystal.—Heat of formation of some alcoholates of soda, by M. de Forcrand. Having already determined the heat of formation of the methylate and ethylate of soda, the author passes here to the study of the alcoholates of soda formed by the propylic, isobutylic, and amylic alcohols.—On some combinations of the bioxide of tin, by M. A. Ditte. Sulphuric acid is known to readily dissolve the hydrates of the bioxide of tin derived from various sources, yielding a liquor soluble in water and alcohol. Here the author studies the products of this reaction, which have not yet attracted the attention of chemists.—Action of some metalloids on the nitrates of silver and copper in solution, by M. J. B. Senderens. The author deals here with powdered selenium, tellurium, sulphur, arsenic, phosphorus, and bromium.—Note on the composition of the grain of starch, by M. Em. Bourquelot. From his researches the author concludes that the grain of starch is formed neither of one nor of two chemical species (granulose and amylose) as has been hitherto supposed, but of a larger number of hydrates of carbon.—On the plastidogene body, or pretended heart of the Echinoderms, by M. Edmond Perrier.—On some new parasites of the Daphnidæ, by M. R. Moniez.—On some Crustaceæ, parasites of the Phallusiæ, by M. Paul Gourret.—On the removal of Lamarck's Herbarium to the Museum of Natural History, by M. Ed. Bureau. After remaining for some fifty years in the University of Rostock, this famous historical collection, containing over 10,000 specimens in good condition, has just been purchased and transferred to the Paris Natural History Museum. The tickets, descriptions, and other accompanying documents are all in the handwriting of the illustrious naturalist.—On the genus *Plesiadapis*, a fossil mammal of the Lower Eocene from the neighbourhood of Rheims, by M. Lemoine. Various remains are described by means of which the author determines two sub-genera of the genus *Plesiadapis* (Gervais), presenting lemurian characters with a marsupial facies.—Note on giovannite, a new cosmic rock, by M. Stanislas Meunier.—On the deterioration of vaccine, by M. P. Pourquier. An experiment is described showing the deterioration of this virus, with suggestions on a means of preventing its attenuation.—Note on the copper detected in wines from vineyards treated with the sulphate of copper against mildew, by M. A. Andouard. An analysis of several samples shows that the quantity of copper detected in such wines is infinitesimal, and in no way injurious to health.

STOCKHOLM

Royal Academy of Sciences, January 12.—Prof. S. Lovén gave an account of the researches effected at the zoological station of the Academy at Christineberg, in the province of Bohus, during last summer.—Prof. Rubenson gave an account of a posthumous memoir by the late Col.-Lieut. Klercker on the so-called anomalous dispersion.—Determination of some physical constants of germanium and titanium, by Profs. Nilsson and Petterson.—On experiments on the electrical conducting power of the air, by Prof. Edlund.—On bryological researches in the province of Småland, by Herr R. Tolf.—Annotations on the vegetation in the west of Herjeådalen, particularly as to the occurrence of the Hymenomycetæ within different formations of the vegetation, by Dr. E. Henning.—A comparative research on the monosulphon-combinations of benzol and toluol, by Dr. Weibull.—Construction of the curves of the fourth order and second kind by means of rules and compasses, by Prof. Björling.—On the pleochroism and light-absorption in epidote from Sulzbachthal, by Herr W. Ramsay.—On the amido-naphthaline-sulphon acid, by Herr S. Forsling.—On the sponges of the province of Bohus, by Dr. Fristedt.

BOOKS AND PAMPHLETS RECEIVED

Travaux et Mémoires du Bureau International des Poids et Mesures, tome v. (Gauthier-Villars, Paris).—Lease and Release, by Sea Verdure (Chiswick Press).—Folk-Lore Journal, vol. v. part 1 (E. Stock).—Notes from the Leyden Museum, vol. ix. No. 1 (Brill, Leyden).—The Auk, vol. iv. No. 1 (New York).—Palæolithic Man in North-West Middlesex: J. A. Brown (Macmillan).—Zeitschrift für Wissenschaftliche Zoologie, 44 Band. 1 Heft (Engelmann, Leipzig).—Proceedings of the Biological Society of Washington, vol. iii. (Washington).—Spolia Atlantica, 1885-86 (Dreyer, Copenhagen).—Botanische Jahrbücher für Systematik, Pflanzengeschichte, und Pflanzengeographie, Achter Band, ii. Heft (Engelmann, Leipzig).—Revue d'Anthropologie, 1887, No. 1.—Supplementary Catalogue to the Newcastle-on-Tyne Public Libraries: W. J. Haggerston.—Report of the Superintendent of the U.S. Naval Observatory for the Year ending June 30, 1886 (Washington).—Sulla Velocitá del Suona nei Liquidi: Prof. T. Martini (Venezia).—Systematic Catalogue of Species of Vertebrata: E. D. Cope.—The Phylogeny of the Camelidæ: E. D. Cope.—Vertebrata of the Swift Current Creek Region of the Cypress Hills: E. D. Cope.—Monthly Weather Report, July and August 1886.—Economic Problem of the Unemployed: W. Westgarth (Mathieson).

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