THURSDAY, JANUARY 26, 1893.

MODERN ADVANCED ANALYSIS.

Theory of Numbers. By G. B. Mathews, M.A. Part I. (Cambridge: Deighton, Bell and Co., 1892.)

THE book under review is a great contrast in many ways to the "Théorie des Nombres" of M. Edouard Lucas, the first volume of which has recently appeared under the ægis of Messrs. Gauthier-Villars. The latter, reminding the reader much of the same author's "Récréations Mathématiques," exhales human interest from well-nigh every page. The former is on severe philosophical lines, and may be greeted as the first work of the kind in the English language. That this should be a fact is somewhat remarkable. When the late Prof. H. J. S. Smith died prematurely many years ago he left his fellow-countrymen a very valuable legacy. Fortunately he had been commissioned by the British Association to frame a report on the then present state of the Theory of Numbers, a subject with which he was pre-eminently familiar, and in which his own original researches had won for him a great and world-wide renown. The pages of the reports for the years 1864–66 inclusive yield as a consequence a delightful account of modern research in this recondite subject. It is, however, much more than a recital of victories achieved by many able men in many special fields. Prof. Smith's fertile genius enabled him to marshal the leading facts of the theory, and to impress upon them his own personality in a manner that was scarcely within the reach of any other man. He contrived to impart a glamour to those abstract depths of the subject to which few mathematicians have sufficient faith and energy to penetrate. Since that day the scientific world has been yearly expecting his collected papers. There is no doubt that their appearance will greatly stimulate interest and research in Higher Arithmetic. The reports of the British Association are not sufficiently accessible. Doubtless the papers will soon emerge from the hands of those upon whom has devolved the responsibility of their production. In the meantime we welcome Part I. of the present work.

The theory of numbers is the oldest of the mathematical sciences, and may be regarded as their sire. Just as applied mathematics is based on pure, so pure mathematics rests on the theory of numbers. Every investigator finds that sooner or later his researches become a question of pure number. Continuous and discontinuous quantity are indissolubly allied. The theory of series, the theory of invariants, the theory of elliptic functions throw light upon and receive light from higher arithmetic. Algebra in its most general sense is everywhere pervaded by numbers. It may safely be affirmed that there is nothing more beautiful or fascinating in the wide range of mathematics than the interchange of theorem between arithmetic and algebra. A proposition in arithmetic is written out as a theorem in continuous quantity or conversely an algebraic identity is represented by a statement concerning discontinuous quantity. In this country the more recent advances in this attractive method are in large measure due to the labours of Sylvester and J. W. L. Glaisher. In a "Constructive Theory of Partitions," published some half-dozen years ago in the *American Journal of Mathematics*, Sylvester showed some beautiful progressions from arithmetic to algebra, and was followed in the same line by Franklin, Ely, and others, whilst in the pages of the *Quarterly Journal of Mathematics* and *Messenger of Mathematics* Glaisher has applied elliptic function formulas to arithmetical theory. The famous theorem which asserts that every number can be composed by four or fewer square numbers, was due to an application by H. J. S. Smith of elliptic functions to arithmetic. These interesting matters are not alluded to in this first volume.

Chapter I. discusses the divisibility of numbers and the elementary theory of congruencies. Euler's function $\phi(n)$, which denotes the number of positive integers, unity included, which are prime to and not greater than n, is not treated as fully as might be desired. Gauss's theorem

$$\phi(d) + \phi(d') + \phi(d'') + \dots = n$$

where $d, d', d'', \dots$ are all the divisions of n (unity and n included) is given, but not some interesting theorems connected with permutations, of which this is a particular case. Sylvester has written much about the same function, which he calls the "totient" of n. M. Ed. Lucas employs the term "indicateur" in the same sense, and believing that there is a great convenience in having a special name for the function, we regret that Mr. Mathews has not taken a course which would have familiarized students with Sylvester's nomenclature, and have enabled them to feel at home with much that has been written by him and others in this part of the theory of numbers.

The author states that this chapter is substantially a paraphrase of the first three sections of the "Disquisitiones Arithmeticæ," the classical work of Gauss; we are inclined to think that advantage would have been gained if the paraphrase had not been quite so close. The next succeeding chapters are occupied with "Quadratic Congruencies" and the theory of "Binary Quadratic Forms."

The account given is fairly complete. There are so many proofs of Legendre's celebrated "Law of Quadratic Reciprocity" that it must have been difficult to make a selection. A wise choice has, we think, been made of Gauss's third proof as modified by Dirichlet and Eisenstein; the latter's geometrical contribution to the proof taken from the twenty-seventh volume of Crelle is, in particular, of great elegance. Gauss's first proof is also given, as well as references to several others. In the difficult subject of Binary Quadratic Forms, the author keeps well in view the close analogy with the algebraic theory of forms; so many additional restrictions present themselves that a large number of definitions are requisite at the outset, and this circumstance is apt to repel a student who approaches the theory for the first time. The definitions, in fact, constitute the alphabet of the science which must be mastered before progress can be expected in the appreciation of the wonderful beauties which are inherent in it. In this subject, more almost than in any other, the initial drudgery must not be shirked, and it may be said in favour of the present work that clearness of definition and conciseness of statement help the learner much to get quickly over the wearisome preliminaries.

We are glad to see the prominence given to the geometrical methods of Klein and Poincaré; that of the former is based on the theory of substitutions, reminding the reader much of the "Icosaeder"; that of the latter s the "Method of Nets," a most ingenious geometrical application throwing light on the theory of "Reduced Forms."

The "Composition of Forms" given in Chapter VI. is logically and judiciously developed, by means of the bilinear substitution, up to the point of showing the method of tabulating the primitive classes of regular and irregular determinants. The chapter on cyclotomy is one of the best written in the book. The discussion of the section of the periods of the roots of unity has engaged the attentions of mathematicians of the first rank since the time of Gauss, so that of necessity much has been written, and while the author states that he has given but an outline of an extensive theory which has not yet been completed, it may be said that the theory as given, with the references to authorities at the end of the chapter, will be quite sufficient to conduct the student bent upon research to the frontiers of the unknown country.

The determination of the number of properly primitive classes for a given determinant, applications of the theory of quadratic forms, and the distribution of primes complete the volume. Mr. Mathews may be congratulated on his resolve to include Sylvester's masterly contraction of Tchébicheff's limits with reference to the distribution of primes; the reader is taken from the "sieve" of Eratosthenes to the work of Legendre, Meissel, Rogel, Riemann, and to the latest researches of Sylvester and Poincaré, of which the ink is scarcely dry. English mathematicians will turn with delight to the account given on page 302 of Riemann's great memoir of 1859, which contains the only satisfactory attempt to obtain an analytical formula for the number of primes not exceeding a given numerical quantity.

In conclusion, though the sequence of the subject matter may be open to criticism, we regard the book as a most valuable contribution to the small library of higher mathematical treatises that, owing chiefly to the energy and enthusiasm of the rising generation of mathematicians, is being brought together. How woefully deficient that library was but a few years since those engaged in research know only too well, and greatly do they rejoice as they see the yawning gaps one by one efficiently filled up. Part II. of the task Mr. Mathews has set himself to accomplish will, we hope, soon appear, and we trust he will be as successful with it as with the present Part I.

P. A. M.

THE DARWINIAN THEORY.

Darwin and After Darwin; an Examination of the Darwinian Theory and a Discussion of Post-Darwinian Questions. By George John Romanes, M.A., LL.D., F.R.S. I. *The Darwinian Theory.* (London: Longmans, 1892.)

WE had hoped ere now to have received the second instalment of this work, and to have dealt with the two volumes in a single critical notice. Unforeseen causes, one of them deeply to be regretted, have presumably prevented the appearance of the discussion of Post-Darwinian questions so early as had been anticipated. We therefore propose to give a short expository notice of the present volume, reserving such criticism as we have to offer for a future occasion, when the second volume shall have come to hand.

The first section consists of an exposition of the scientific evidences of evolution as a fact independent of the Darwinian theory of the method by which this evolution has been brought about. It may be regarded as an expansion of the author's little volume in the "Nature Series," on "The Scientific Evidences of Organic Evolution," published ten years ago. Mr. Romanes has spared no pains in the collection and marshalling of his evidence. His object is to convince, by the abundance of facts and by logical inferences based thereon, those who still hold by the tenets of Special Creation. Whether those who still hold by these tenets are likely to be influenced by the facts or the inferences is a question we do not propose to discuss. The author evidently supposes that they are, and has written for them a good many pages in a strain of which we give a couple of examples:—"It would seem most capricious on the part of the Deity to have made the eyes of an innumerable number of fish on exactly the same ideal type, and then to have made the eye of the octopus so exactly like these other eyes, in superficial appearance, as to deceive so accomplished a naturalist as Mr. Mivart, and yet to have taken scrupulous care that in no one ideal particular should the one type resemble the other." Again, "Although in nearly all the numerous species of snakes there are no vestiges of limbs, in the Python we find very tiny rudiments of hind-limbs. Now, is it a worthy conception of Deity that, while neglecting to maintain his unity of ideal in the case of nearly all the numerous species of snakes, he should have added a tiny rudiment in the case of the Python—and even in that case should have maintained his ideal very inefficiently, inasmuch as only two limbs, instead of four, are represented?"

The second section of the volume is devoted to the setting forth of the theory of natural selection as it was held by the master. This, as was to be expected, is a well-ordered and lucid exposition. We could wish that Mr. Romanes had been more careful to avoid all appearance of personifying natural selection. He says, for example, "it is the business of natural selection to secure the highest available degree of adaptation for the time being." Such language is highly metaphorical, if not misleading. If we can talk of business at all we may say that it is the business of various eliminating agencies, in the struggle for existence, to weed out and exclude from any share in perpetuating their race all those individuals who are too weakly to stand the stress of the struggle. The survival of the fit is an incidental result of the stern business of elimination. It is here that the naturalistic hypothesis differs most markedly from the teleological interpretation of nature. In conversation a while since a friend observed to us: Since your school of thought admit that the eye of natural selection is ever on the watch for the slightest improvement in adaptation, why should they hesitate to say with us that it is the eye of Beneficence that is thus ever watchful? The misunderstanding of the naturalistic position here shown is not surprising. It is due to the too free use of metaphorical language on the part of expounders of the Darwinian hypothesis.

In the chapter entitled "Criticisms of the Theory of Natural Selection," an interesting digest is given of the work of Prof. Ewart and others on the electric organ of the skate, concerning which Mr. Romanes says, "I freely confess that the difficulty presented by this case appears to me of a magnitude and importance altogether unequalled by that of any other single case—or any series of cases—which have hitherto been encountered by the theory of natural selection." And he adds, "So that, if there wêre many other cases of the like kind to be met with in nature, I should myself at once allow that the theory of natural selection would have to be discarded," by which he means, we presume, that the theory would have to be discarded as offering a solution of such cases.

The book contains many excellent illustrations, the series which show the variations due to artificial selection being a noteworthy feature. They, and the volume which contains them, will prove of service to those general readers for whom, as the author tells us in his preface, this exposition of the Darwinian theory has been mainly prepared.

FERNS OF SOUTH AFRICA.

The Ferns of South Africa, containing Descriptions and Figures of the Ferns and Fern-allies of South Africa. By Thomas R. Sim. 275 pp., 159 plates. (Cape Town and Johannesberg: J. C. Juta and Co. London: Wm. Wesley and Son, 1892.)

THE present work will be a useful and acceptable addition to our stock of fern-books. It contains descriptions and plates of all the ferns and fern-allies known to exist in Africa south of the tropic of Capricorn, the same area which is included by Harvey and Sonder in their "Flora Capensis," three volumes of which, including the orders from Ranunculaceæ to Campanulaceæ, have been published. The author won the Jubilee gold medal given by the North of Scotland Horticultural Association, and for many years has filled the post of curator of the Botanic Gardens at King William's Town. Several years ago Mr. Sim published an illustrated handbook of the ferns of Kaffraria, and now he has extended his area so as to include the whole of South Temperate Africa.

The fern flora of the Cape does not show the same richness and remarkable individuality which characterises its phanerogamic flora. It is probable that the flowering plants of this area are not less than ten thousand, and the number of large endemic genera and of species is very considerable. In ferns we get in South Africa 179 species, out of which 42 species, or something under 25 per cent., are endemic. There is no genus that is peculiar to the Cape; of Mohria, which comes nearest, the Cape species, *M. caffrorum* extends to Madagascar and Tropical Africa, and two new species have lately been found in the high regions north of the colony. The section Rhizoglossum of the genus Ophioglossum, which differs from the true adder's tongues by having the fertile spike separate from the barren frond, the single species, *O. bergianum*, is peculiar to the Cape. Hymenophyllum is represented by 8 species, Trichomanes by 5, Adiantum by 6, Cheilanthes by 8, Pellæa by 14, Pteris by 7, Lomaria by 5, Asplenium by 25, Nephrodium by 12, Polypodium by 12, Acrostichum by 8; and Lycopodium by 8 species. Some of the species, *e.g. Vittaria lineatia, Marattia* and the two tree-ferns, are tropical types; some, such as *Cystopteris fragilis* and *Lycopodium clavatum*, are common to Britain and the Cape. *Todea barbara* is confined to the Cape and Australia, and abundant in both areas. *Lomaria alpina* is a plant of all the three south-temperate areas. *Blechnum australe* of the Cape is not, I think, really distinct specifically from *B. hastatum*, and is widely spread in South Temperate America.

Lomaria punctulata is remarkable for its polymorphic fructification, which is sometimes like that of a Scolopendrium. *Asplenium lunulatum* is remarkable for its variability in outline and cutting.

Mr. Sim gives introductory chapters on the parts of ferns and their nomenclature, on their reproduction and propagation, on their cultivation and the preparation of herbarium specimens, and on the history of the discovery of the Cape species and the books and papers that have been written about them. His statistic table on page 34 needs much revision in some of its items. He gives the ferns of Madagascar at 144. The number now known in the island is 326 true ferns and 40 fern allies, a total of 366. There are nothing like 683 species and 458 endemic types in Africa and its islands. When I counted them up in 1868 I made the two figures 346 and 127. Since that date probably 100 species have been added. Madagascar, Bourbon, and Mauritius are very rich in ferns, but Continental Africa is very poor both in number of species and in peculiar types as compared with Asia and America.

The descriptions are carefully drawn up from the study of actual specimens, and by the aid of these and the plates there can be no difficulty for any one, even without any previous botanical knowledge, in making out the name of any reasonably complete specimen of any of the Cape species.

Therefore, no doubt, the existence of such a book will give a great impulse to the study of ferns by ladies and others who reside in or visit the Colony.

J. G. BAKER.

OUR BOOK SHELF.

Newcomb-Engelmann's Populäre Astronomie, Zweite vermehrte Auflage. Herausgegeben von Dr. H. C. Vogel. (Leipzig: Wilhelm Engelmann, 1892.)

THE well-known Popular Astronomy of Prof. Newcomb was translated into German by Rudolf Engelmann, and published in 1881 with considerable additions and alterations, most of which were improvements. It was very favourably received on its first appearance in German, probably because it is not only comprehensive, exact, and scientific, but has a fresh and vigorous style, in pleasing contrast to the ponderous German standard works. The original translator being dead, the publishers entrusted the work of preparing a new edition to Dr. H. C. Vogel, Director of the Astrophysical Observatory at Potsdam, a task for which he was specially fitted, because astronomical progress during the decade since the appearance of the first edition of the book has been mainly in his special department. Dr. Vogel's chief difficulty has been to keep the book within reasonable limits while bringing it up to date, but he has not been wholly successful in this. By a slight further enlargement of the book he might without difficulty have very much increased its value. A description of the diffraction spectroscope should have been given in the section on spectroscopes; Prof. Hale's work in photographing prominences and faculæ should have been introduced; the chapter on Mars is very much behind the times; and some details should certainly have been given of the international scheme for photographically charting the stars.

Dr. Vogel has considerably altered the arrangement of the chapter on comets and meteors, and this alteration has led to the curious result that the same woodcut appears as Figs. 152 and 165. The chapter on stellar astronomy is also recast, the editor's own latest classification of star spectra being given to the exclusion of all others. The section on variable stars has also been entirely rewritten. These chapters would have been much improved by an account of recent discoveries as to the resemblances between comets, nebulæ, and stars, and of the theory that variable stars are formed of revolving swarms of meteorites. The classification of star spectra which recognises an ascending and descending temperature should have been given, and recent work and theories on temporary stars certainly deserved attention. The bibliography given in the first edition has been omitted in the second, as being too much for the general reader, and insufficient for the student of science. The excellent series of biographical notices in the appendix has been carefully extended to 1891, and completely rearranged. Dr. Vogel has adopted the admirable plan of arranging these notices chronologically in order of death, instead of birth, probably on the grounds that all work is largely the result of previous discoveries, and that the later years of a man's life are usually his best and most productive. A series of excellent tables and a full index complete the volume.

The general appearance of the book has been much improved by the use of new woodcuts for the illustrations, and by the substitution of two excellent photographs of nebulæ (those of Orion and Andromeda) for the very unsatisfactory star charts of the earlier edition.

A. T.

The Hemiptera Heteroptera of the British Islands. By Edward Saunders, F.L.S. (London: L. Reeve and Co., 1892.)

IT is now nearly thirty years since Douglas and Scott first made the study of the British Hemiptera Heteroptera possible to ordinary students by the publication of a description of these insects in a volume issued by the Ray Society. The difficulties were then very great, for purely insular ideas in entomology were prevalent, and our hemipterous insects had not been sufficiently compared with continental species. Douglas and Scott did all that was possible at that time and produced a good work that has held the ground as the best published authority on the subject. Very much, however, has been done since that period, and restricted specialists in entomology, as in most other branches of natural science, have exercised unlimited time and patience in studying the classificatory problems of a single family or even of a large genus. Hence in a monograph of to-day the standard of advanced classification and descriptive facility is considerably raised from that which dominated the writings of the earlier authors. Mr. Saunders has not only aimed at this perfection, but has sought to place in the hands of the British student and collector a thoroughly trustworthy handbook by which he may understand and identify his collection, and in this we think the author has altogether succeeded. We must not look for bibliographical references or synonymical notes, the names of the describers of families, genera, and species being only indicated, while the habitats of the species are confined to such localities in the British Islands as are recorded by collectors; and this is perhaps all that can be expected in a local monograph. It is therefore in no spirit of criticism we express a regret that in all faunistic writings the complete recorded distribution of the species is not given. Thus even the purely British collector would not be the worse for learning that *Zicrona cærulea*, to be found in the suburbs of London, is not only widely distributed throughout the Palæarctic region, but is also found in Continental India and in the Malay Peninsula and Archipelago; or that *Ischnorhynchus resedæ*, to be taken even at Hampstead, is common throughout Europe and Siberia, and is also neither scarce in North nor in Central America.

We welcome Mr. Saunders's book as a distinct and valuable addition to our insular entomological literature. We also notice that an illustrated edition is advertised, but on the quality of the plates we are compelled to be silent, as the publishers have only forwarded us a plain copy.

W. L. D.

Physical Education. By Frederick Treves, F.R.C.S. (London: J. and A. Churchill, 1892.)

THIS essay is reprinted from the "Treatise on Hygiene" by various authors, edited by Stevenson and Murphy, the first volume of which we recently reviewed (NATURE, vol. xlvi. p. 609). It well deserves to be issued separately, for the author has mastered his subject thoroughly, and sets forth his ideas in a plain, straightforward style which will be cordially appreciated by readers who are especially interested in athletics. Mr. Treves is quite as strongly conscious of the evils which may spring from excessive or unsuitable physical exercise as of those which may result from physical exercise being neglected or underrated, so that there is a welcome tone of perfect impartiality in all he has to say about the various ways in which efforts are made to promote health by the use of the muscles. The volume may be confidently recommended to all who desire to understand the conditions under which physical exercise is most likely to be of service.

LETTERS TO THE EDITOR.

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

The Geology of the North-west Highlands.

IN the kindly review of my work by Prof. de Lapparent, which appeared in NATURE of 5th inst., there are one or two inaccuracies which I would at once have corrected had I not shrunk from drawing attention, even for purposes of rectification, to an article which I felt to be too eulogistic. Lest, however, my silence be misinterpreted, there is one point on which I wish to say a few words. Prof. de Lapparent, when alluding to the solution of the problem of the geological structure of the North-west Highlands, makes no reference to the distinguished part taken in that subject by Prof. Lapworth. But every one who has followed the progress of geology in recent years is familiar with his work. For myself, I have had no personal share in the discovery. Like most geologists I had accepted the views of Sir Roderick Murchison, and I held to them, until, after the Geological Survey was for the first time extended to Sutherland in 1883–84, I was finally convinced that they were untenable by the brilliant mapping of my colleagues, Messrs. Peach and Horne, who, following Prof. Lapworth's lead, share with him in the glory of one of the greatest achievements of field geology in recent times. My recantation was published in NATURE of November 13, 1884, and the whole history of the investigation of the North-west Highlands up to Prof. Lapworth's latest paper was sketched in a detailed Report communicated by me to the Geological Society on April 25, 1888. My friend Prof. Lapworth has no scientific comrade who has more frankly and practically acknowledged his great geological achievements than I have done. ARCH. GEIKIE.

January 23, 1893.

The Identity of Energy.

I AM glad to see that in the introduction to his severely-difficult memoir, published in the *Philosophical Transactions* for 1892, "On the Forces, Stresses, and Fluxes of Energy in the Electromagnetic Field" (p. 427), Mr. Oliver Heaviside notices and criticizes some ideas of mine, published in the *Philosophical Magazine* for June 1885 and other places, concerning energy.

The statements I then made, and to which I still rigidly hold, are (1) that energy has identity like matter, and not merely conservation; (2) that whenever energy is transferred from one body to another, it is also transformed from potential to kinetic, or *vice versâ*.

The basis of the first assertion is the fact that energy is always passed on continuously through space, *i.e.* that its transfer occurs along a definite path, instead of merely appearing in one place and disappearing in another.

The law of conservation would be satisfied by disappearance and equal reappearance; the law of identity requires a continuous act of transfer. The latter is true for matter, and I assert that by thinking of a number of instances, it will be perceived true for energy. In all mechanical instances, as of belts and shafting, the transfer of energy is obvious; it was not so obvious in electromagnetic actions, between dynamo and motor for instance, until Prof. Poynting clearly demonstrated that it was in accordance with Maxwell's principles.

Mr. Heaviside objects that we are not able to assert it for gravitational energy. Well, that depends on what view we take of gravitation; but I submit that until something more is certainly known about it, the safest plan is not to assert, but to assume, that in this case also what is known in every other case likewise occurs, and to trace the consequences of the hypothesis in the hope that it may lead to some conclusion verifiable or falsifiable by experiment. The reason I attach importance to this doctrine of the identity or continuity of transfer of energy is because it greatly simplifies the fundamental mechanical laws, and emphasizes without risk of vagueness the denial of action at a distance.

If action at a distance (no matter how minute) can ever occur, then indeed the continuous transfer of energy breaks down. But observe that there is no necessity for the transfer to occur at a finite velocity in order to avoid action at a distance, *i.e.* action without a medium. By the thrust of an incompressible pole, energy is transferred from butt to tip, just as really as if the compressed and recoiling layers could be demonstrated and its velocity measured. So likewise the pull of gravitation may be (and *pro tem.* I believe is) transmitted by an incompressible (or nearly incompressible) ether, so that the force is felt instantaneously (or nearly instantaneously) at all distances where matter exists; but that by no means militates against a genuine act of transfer. The conservation of matter makes experiments on gravitation difficult; if we could suddenly create or destroy a piece of matter there might be some remote chance of determining the rate at which its gravitative influence was felt. Especially if by alternately generating and destroying it we could set up a series of waves of perhaps measurable length.

And although this is as yet impossible, many known facts lead us to conclude that if gravitation has any velocity at all short of infinite, it is at least immensely greater than the speed of light. And seeing that the one phenomenon is concerned with the transverse (electric) elasticity of ether, and the other with its longitudinal elasticity, there is nothing surprising in that.

By all means, however, as Mr. Heaviside urges, let gravitation be included in general etherial equations whenever possible; and it may perhaps be wise to assume some unknown finite rate of propagation and trace its consequences with the object of verifying or disproving them.

So far as I understand, however, this is not unlike what Helmholtz did, by his generalization of Maxwell's electromagnetic theory; with the result that the course of experiment so far has been to justify the simple Maxwellian theory, and to make the longitudinal ether thrust velocity practically infinite.

And now for the second assertion, that whenever energy is transferred from one body to another, it is also transformed, and *vice versâ*. This is to me not an opinion, but a demonstrated theorem (as has been shown in the paper referred to); but it must be understood in what sense I consistently use the word body in this connection. I do not necessarily mean a visible lump of matter. The molecules of a lump are to be regarded as a different "body" to the whole mass; and again, the ether everywhere embathing them is another distinct "body."

But so long as a piece of matter is merely moving through space with all the energy it may happen to contain, I do not consider that a transfer at all. There is a transfer of energy in one sense, viz. that of locomotion, but there is no transfer from one body to another except when work is done at their point of contact, and energy gained by one and lost by the other, being transferred across their common boundary surface. In all such cases of "activity" the energy transferred is necessarily in the first instance transformed; though by means of another transfer it may very speedily be transformed back again; and so speedily sometimes is the re-transformation effected that the intermediate condition has a tendency to get overlooked. In wave-motion a transfer and transformation occurs during every quarter period.

Mr. Heaviside seems to think that the mere convection of energy should be included as one kind of transfer; but surely that is scarcely convenient? So long as energy retains its form and adherence to one body, so long there is no true activity; no work is being done—the energy is simply stored. It may be stored in a bent spring, or in a flying bullet, or in a revolving fly-wheel. It is impossible to have kinetic energy at all without convection, and a distinction must be drawn between the mere existence of energy and the active and useful flux or transfer of the same.

Mr. Heaviside further seems to consider circuital fluxes of energy as strange and useless phenomena. But I see no reason in this at all. The circulation of matter—for instance in the inner circle of the Metropolitan railway—is, I suppose, considered useful. The circulation of commodities is the essence of commerce. So does the circulation of energy constitute the activity of the material universe. It is the act of transfer that is beneficial (or the reverse); what becomes of a conservative quantity is a minor matter. It must go somewhere, and may very well, after a series of transfers, ultimately return to its starting point. [Parenthetically I should like to preach here against what I hold to be the pernicious doctrine of (at least amateur) political economists, that because money locally spent is not destroyed, but remains in the community, it does not much matter how much transferring power is permitted or granted to one individual,—as if the money itself were the useful commodity, and not the power of determining its direction of transfer or non-transfer. The control of every transfer should be jealously watched, for that is the greedily-desired power.]

So long as circuital convection of energy goes on *without* transfer—as, for instance, in the rim of a non-working fly-wheel—so long the energy is merely *stored*; but directly a belt is fitted on with different tensions in its two halves, a portion of the energy is tangentially tapped off, and transfer and activity begin. The kinetic energy of the wheel is converted into strain or stress energy of the belt, which then by simple locomotion passes it on to something else. I perceive, however, that there is a slight difficulty about this simple case of locomotive conveyance of stress energy by a really inelastic substance; but only because the details of any infinitely rapid process are difficult to follow. I perceive moreover that in many cases it is not worth while to attend to the alternate compressions and motions which constitute a longitudinal pulse, and that the idea of simple locomotion may be conveniently introduced to cover the case of a stressed body moving; but the convenience is I think only attained by shutting our eyes to the essential processes which in all actual matter must be occurring.

I trust that Mr. Heaviside may find time to notice this letter, and attack anything he disagrees with, in order that the whole matter may become thoroughly clear. OLIVER LODGE.

A Proposed Handbook of the British Marine Fauna.

I AM obliged to Prof. Thompson for his criticism of my scheme, although only one of the points he raises is new to me—as I think it will be to most zoologists—viz. that "there are *no* nematophores on the stem" in *Antennularia*. I thought *A.*

ramosa had nematophores on the stem, *and I think so still.* Some of his other remarks are so very obvious as to have scarcely required mention, at any rate to biological readers; a few, however, are just such debateable points as I was anxious to have opinions upon from as many naturalists as possible, and I am glad to know Prof. Thompson's. I am glad to say a number of biologists have written to me, since the scheme appeared in NATURE, expressing general approval, and criticising various points of detail, and some of them kindly making offers of assistance in special groups—and without that kind of assistance from specialists I need scarcely say it would be impossible to carry out the work satisfactorily. The proposal was first brought before the Biological Society of Liverpool on November 11, and it was only after some weeks of intermittent discussion with some of my friends in that Society (such as Dr. Hanitsch, Mr. Isaac Thompson, and Mr. A. O. Walker) who are specialists in certain groups of marine invertebrata, and after correspondence with Canon Norman and other biologists, that I sent the scheme to NATURE, with the view of getting further opinions. Consequently some of the debateable matters alluded to by Prof. Thompson (limits of British area, introduction of certain non-British forms, specific nomenclature, how to treat records of size and distribution, best terms to use for zones of depth, and, I may add, for relative abundance) have already been considerably discussed. The other points raised by Prof. Thompson in connection with *Antennularia* only require a few words. I said *A. ramosa* was *usually* branched. Prof. Thompson says it "may sometimes" be unbranched. The difference between these statements is slight. As to dimensions, a zoophyte which grows to 12, or occasionally to 24, inches in height, will, of course, be also frequently found of smaller sizes; and it might be the best plan to give the extreme range, say, 1 to 24 inches. What I gave was the fair average size of most of the specimens dredged or seen in collections, which I still consider to be 6 to 9 inches.

The rest of Prof. Thompson's contention is practically that there are great difficulties in the way of drawing up such a book of the known British marine invertebrate animals, and that if it is ever done it will be more or less incomplete, because Canon Norman and others (I hope including both Prof. Thompson and myself) will continue to find new British animals. That is perfectly true—in fact obvious—but the same objection applies more or less to every work on systematic zoology that has ever been published; and I do not consider that because our British Pycnogonids, and some other small groups, are still very imperfectly known, that is any sufficient reason for delaying indefinitely an attempt to deal with the rest of the invertebrata. On the contrary my opinion is rather that an approximation is better than nothing, and that every group, or every family, reduced to "Handbook" form with specific diagnoses and figures must be a distinct gain. I hope Prof. Thompson will not think that I am trying to dispute all his criticisms, or that I am ungrateful for the trouble he has taken. I have no doubt that he could correct me in many details, and give me great assistance in records, &c., of zoophytes, pycnogonids, and other groups, and I hope he will do so. W. A. HERDMAN.

University College, Liverpool, January 20.

PROF. D'ARCY THOMPSON'S letter raises a question which is, I think, well worthy of Prof. Herdman's consideration. That a handbook of our marine fauna is needed cannot for a moment be doubted, and the only matter that calls for discussion is one of scope and method, of ways and means. Prior to the appearance of Prof. Herdman's circular and article I had intended, if possible, to bring this very matter before the British Association at its next meeting, believing that a select Committee of the Association would best be able to further the interests of marine zoology in this respect. But, as the matter now stands, I leave any such action very willingly to Prof. Herdman's initiative.

Put broadly (although I well know that such a work in Prof. Herdman's hands would by no means have the character of a mere compilation), the question at issue is whether the handbook should be mainly a compilation from existing material, or should express the work of various specialists and be based upon a series of special investigations. For myself I agree with Prof. Thompson, and for the same reasons, that the adoption of the latter alternative would be likely to meet our needs most fully and satisfactorily. It would ensure, as far as possible, the equal treatment of the various groups, and would thus give to the book (which is important) a more permanent and authoritative value than could be attained by a book depending upon the personal labours of one zoologist. I feel confident that, should Prof. Herdman admit the force of this consideration and be willing to edit a handbook in which the diagnoses were drawn up for the various groups by specialists or specially-chosen investigators, he would find no difficulty whatever in meeting with willing co-operation.

But I hardly see the point of extending the scope of the work to the extent which Prof. Thompson would seem to desire. We need a handbook for use around the coasts of our own islands. To include the fauna of the whole North Atlantic would needlessly add to the size of the work, delay the time of its appearance, and even in the end be incomplete; while it is doubtful whether the advantages would at all outweigh these defects. W. GARSTANG

Marine Biological Association, Plymouth, January 20.

Fossil Plants as Tests of Climate.

MR. J. STARKIE GARDNER, in his interesting review of Mr. Seward's valuable essay, makes a statement which I fancy may be misinterpreted at page 268 of NATURE, where he speaks of the fragmentary character of the Arctic tertiary plants, and the inexperience of the collectors. He doubtless is referring to the remains of certain supposed "palms and cycads in the Greenland Eocene," but those who have not followed this branch of Arctic research would hardly gather from the review that Prof. Heer has determined a magnificent flora of more than 350 species from these northern tertiaries, and that he at once pointed out the absence of tropical and subtropical forms, and the fact that large leaves are not only perfectly preserved up to their edges, but that upright trees associated with their fruits and seeds prove them to have grown on the spot. "Thus of *Sequoia Langsdorffi*," he writes, "we see not only the twigs covered with leaves, but also cones and seeds, and even a male catkin."[1]

In April 1875 I endeavoured to give an abstract of all that was then known of Arctic geology, in a series of articles that appeared in your columns (NATURE, vol. xi. pp. 447, 467, 492, and 508), and added some general conclusions of my own, which are further accentuated in the joint communications of Colonel Feilden and myself to the Geological Society in 1878, and in the "Geology Appendix" to Sir George Nares' "Voyage to the Polar Sea," in which expedition Colonel Feilden played a most valuable part. I have ever since carefully followed the progress of Arctic research, and am now of opinion that looking to the identity of a large number of species (often extending to the varieties of the same) occurring in the Silurian, Carboniferous, Lias, Oolite, Cretaceous, and Tertiary strata of the Arctic regions, with those occurring in similar strata in Europe and other parts of the world, they point to a common temperature over these areas and probably over the whole world, from Silurian to early Cretaceous times, and that this was the case does not appear to me to be affected by the question as to whether or not these deposits were homotaxeous.

In late Cretaceous times commenced *horizontal* variation of cold, or what we now term "climate," though previously *vertical* variation had evidently been present, for the later investigations of Messrs. Blanford appear to place beyond doubt the existence of glaciers in geological times, as was suggested in 1855 by my lamented chief, Sir Andrew Ramsay; but I equally fail to see that the slightest evidence has been anywhere adduced to support the theory of "recurrence of ice-ages," originated by my talented colleague the late Dr. Croll, and now supported with a "modification" by Sir Robert Ball.

The facts, whether we look to the history of plant life, or animal life, or the character of the rocks themselves, appear to me to be all the other way, as they disclose nothing resembling the refrigeration that, gradually increasing in the Tertiary epoch, culminated in the Glacial episode, which choked up the North and Irish Seas with an ice-sheet since man has been an occupant of our islands. CHAS. E. DE RANCE.

H.M. Geological Survey, Alderley Edge, Manchester.

Racial Dwarfs in the Pyrenees.

IN consequence of evidence that I had obtained as to the existence of a dwarf race in Spain, I wrote to Mr. McPherson,

[1] "On the Miocene Flora of North Greenland," by Prof. Oswald Heer. Translated by R. H. Scott, F.R.S., Brit. Assoc., 1867, pp. 53.

our Consul at Barcelona, and enclose his reply. There have long been rumours of survivals of a dwarf or a prehistoric race existing in parts of Spain, but careful inquiries at Madrid failed to supply any definite information on the subject. Last summer on reading over an old number of *Kosmos* (Paris, 1887), I found a brief paragraph referring to a pigmy race having been found in the province of Gerona, Spain, who had slightly Mongolian eyes, yellow, broad, square faces, height from 1 m. 10 to 1 m. 15, and red hair.

An Austrian gentleman recently told me he had seen, in the market-place at Salamanca, some very under-sized peasants, with broad faces and mahogany-coloured woolly hair.

You will see that these accounts all agree substantially, and that these dwarfs and those of Africa are precisely similar.

I have got a deal of information from an old Spanish woman who belongs to a half-breed nano family, and who says that there are in such families frequently nanos (or "enanos") who have red tufts of wool, and are as small as ordinary small boys. But these tufts of wool are peculiarly characteristic of dwarf races nearly everywhere.

I shall write more fully as to my inquiries among half-breed nanos; but they are of very secondary interest now that we can find pure racial nanos within easy reach.

It is most fortunate that they live in the Valley of Ribas and the Col de Tosas, within a little more than a half-day's journey from Toulouse. Some health-seekers or tourists in the South of France may perhaps feel inclined to pay a visit to these little people.

Should the suggestion be acted on, and prove satisfactory, a line to myself on the subject, addressed to 28, Pall Mall, would be highly valued. R. G. HALIBURTON.

Tangier, January 9.

[COPY.]

"*British Consulate, Barcelona, December* 10, 1892.

"DEAR SIR,—Since I received your letter of November 18 and its enclosures I have endeavoured to ascertain what truth there is in the statement that pigmies, or 'enanos' (not 'nanos') exist in the Valley of Ribas. From conversations I had with various individuals who have visited that district it appears certain that a race of men, of about from one metre to one metre and twenty centimetres high, of a darkish complexion (copper-coloured), dark hair and woolly, and flat, broad nose, live in that district, particularly in the 'Collado de Tosas.' They are active, and are generally employed as shepherds. It is also asserted that they are not very intelligent, and that they appear to understand and to make themselves understood with difficulty. It would be an easy journey to go to that place from this town. I had no little difficulty in finding out that such a race lived in that place, for many of the persons with whom I have spoken on the subject were evidently confused and confused me, as besides these, evidently racial pigmies, there are in that neighbourhood many 'cretins,' which were at times described to me as if these were the 'enanos' I spoke about. I am now certain that there are cretins and pigmies in the Valley of Ribas. It is stated that the 'enanos' are rapidly disappearing, and that latterly many have died of smallpox. The men you speak of, who were seen at Salamanca, are, I should say, natives of the Batuecas, or rather of Los Hurdes. These men were discovered in the sixteenth century, and they were then and are even now, in an almost absolute state of savagery." [The remainder as to this race is omitted, as it does not appear that they are nanos.—R. G. H.]

"Yours very truly,
(Signed) "WM. MCPHERSON.

"R. G. Haliburton, Esq."

British Earthworms.

I WRITE to suggest—in connection with the recent letters in NATURE upon this subject—that some one give a thoroughly trustworthy list of British earthworms, with the memoirs in which the species were originally described, and the chief characteristics of each. Dr. Benham would be doing very useful and acceptable work if he were to accomplish this. From what I understand everybody has been making mistakes, and the whole matter is in the utmost confusion. It is very necessary that such a classification should exist, if only for the benefit of those who are working on the earthworm more from a comparative anatomist's than from a specialist's point of view.

FRANK J. COLE.

Zoological Department, Edinburgh, January 12.

DANTE'S "QUÆSTIO DE AQUA ET TERRA."

"Quæstio Aurea ac perutilis edita per *Dantem Alagherium*, poetam florentinum clarissimum, de natura duorum elementorum Aquæ et Terræ disserentem."

"Lo, the past is hurled
In twain: up thrust, out staggering on the world,
Subsiding into shape, a darkness rears
Its outline, kindles at the core, appears
Verona."—R. BROWNING, "Sordello," Book i.

"TO all and each who shall see this document, *Dante Alighieri* of Florence, the least amongst true philosophers, wishes health in Him who is the Beginning of truth and the Light.

"Be it known unto ye all that whilst I was at Mantua there arose a certain question, the which after having been many times dilated upon rather for vain show than for Truth's sake, still remained undecided. Wherefore I, since from boyhood I have been nurtured continually in love of Truth, could not bear to leave the question undiscussed; but I thought fit to show the truth concerning it and to dissolve the arguments adduced to the contrary, both for love of Truth and hatred of Falsehood. And lest the malice of many who are wont to fabricate envious lies against the absent should behind my back alter what was well said, I have moreover thought fit to leave written down on paper what I proved, and to set forth the form of the whole disputation."

These are the words with which Dante commences this "golden and most useful" inquiry concerning the nature of the two elements, earth and water. The treatise is little known in comparison with the other writings of the poet;[1] but although rejected by Ugo Foscolo and others as "impostura indegna d'esame," its genuineness and importance are now almost universally admitted; and without yielding unreservedly to the enthusiastic opinion of an Italian geologist (Stoppani) that there are more truths relating to cosmology to be found prognosticated, affirmed, and even demonstrated in these few pages of the supreme poet than in all the writings of the middle ages taken together, we may nevertheless acknowledge it to be a work of the greatest interest and importance, and by no means unworthy of the singer of the "Divina Commedia."

It seems to be the last work of the poet's life, written at that period which he himself describes in his sonnet to Giovanni Quirino:—

"Lo Re, che merta i suoi servi a ristoro
Con abbondanza, e vince ogni misura,
Mi fa lasciare la fiera rancura,
E drizzar gli occhi al sommo consistoro
E qui pensando al glorïoso coro
De' cittadin della cittade pura,
Laudando il Creatore, io creatura
Di più laudarlo sempre m'innamoro."
—Sonetto xliv. ed. Fraticelli.[2]

Dante was at this time the guest of Guido Novello di Polenta at Ravenna. About the commencement of the

[1] It is, I believe, the only one of Dante's writings that has not yet been translated into English.

[2] "The King by whose rich grace His servants be
With plenty beyond measure set to dwell,
Ordains that I my bitter wrath dispel
And lift mine eyes to the great consistory;
Till, noting how in glorious quires agree
The citizens of that fair citadel,
To the Creator I, His creature, swell
Their song, and all their love possesses me."
—Rossetti's translation in "Dante and his Circle."

year 1320, he seems to have gone for some unknown reason to Mantua, and there to have entered upon this discussion, which he then completed at Verona. The disputation took place at this latter city on January 20, 1320, as Dante himself tells us, in the church of St. Helena (where in recent years the metropolitan chapter have put up a monument in commemoration of the event). All the clergy of Verona were present, except some few who, in the words of Rossetti—

> "Grudged ghostly greeting to the man
> By whom, though not of ghostly guild,
> With Heaven and Hell men's hearts were fill'd."
> —"Dante at Verona."

From a passage which occurs in the course of the treatise, one might almost think that ladies also were present, but let not the reader therefore conclude that the assemblage which listened to Dante's eloquence in that little Veronese temple resembled so many modern philanthropical and other associations in being chiefly composed of ladies and clergymen, for doubtless Can Grande della Scala himself was present to do honour to his former guest, and his poetic fame, which we know to have already spread far and wide, would certainly have brought together as many as the church could hold.

The question to be solved is whether, on any place on the earth's surface, *water* is higher than the *earth*. This question, Dante tells us, was generally answered in the affirmative, and he gives us the five chief reasonings adduced in support of it, of which perhaps the most striking is this one :—

"If the earth were not lower than the water, the earth would be entirely without waters, at least in the uncovered part, and so there would be no fountains, nor rivers, nor lakes. So water must be higher than the earth. For water naturally flows downwards, and the sea is the source of all waters, and if the sea were not higher than the earth, the water would not flow to the earth, since in every natural motion the source of the water must be higher."

Another is this :—"Water seems chiefly to follow the motion of the moon, as is evident in the flow and ebb of the sea, and therefore since the moon's orbit is eccentric, it seems reasonable that water in its sphere should be eccentric too ; and another argument shows that this cannot be unless it be also higher than the earth."

Such be their arguments, but sense and reason alike are against them, and Dante proceeds to explain how he will treat the question. First, he will prove that it is impossible that water in any part of its circumference be higher than this emergent or uncovered earth on which we dwell. Secondly, he will prove that this emergent earth is everywhere higher than the surface of the sea. Thirdly, he will urge arguments against his own demonstrations, and then demolish these objections. Fourthly, the final and efficient cause of the elevation and emergence of the earth will be shown. Fifthly, he will demolish the five chief arguments of the other side which he has already stated.

1. It is impossible that water in any part of its circumference be higher than the earth.

There are only two ways whereby water can thus be higher than the earth: either the water must be *eccentric*, or, if it be *concentric* with the earth, it must be *gibbous* in some part. By water being *eccentric*, Dante means the centre of its natural sphere to be out of and different from the centre of the earth ; by being *gibbous*, Dante means some part of its sphere to be raised up so as to form a protuberance or hump, just as he considers the earth on which we live to be a protuberance or gibbosity of the spherical surface of the earth.

He now shows by means of diagrams that neither of these things are possible, but first makes these two statements—(1) Water naturally flows downwards ; (2) Water is by nature a labile body and has not a boundary of its own, but takes the boundary of the thing in which it is contained.[1]

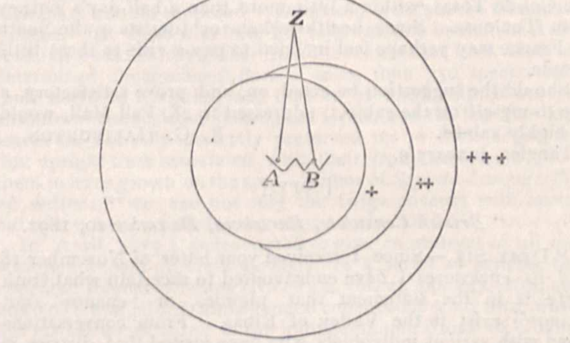
We may compare with this a modern definition of a fluid :—

"A perfect fluid is a body whose form can be changed to any extent, provided its volume remain constant, by the application of a stress, however small, if we allow it sufficient time."—Garnett, "Treatise on Heat."

In the first place, *water cannot be eccentric.*

For if it were so, then three impossibilities would follow —(1) Water would naturally flow both upwards and downwards ; (2) water would not be moved downwards by the same line as the earth ; (3) an equivocation would arise in speaking of the *gravity* of water and of earth ; all which things are seen to be not only false but impossible.

The demonstration *ab absurdo* follows thus :—Let the heavens be the circumference on which are placed three crosses ; water the circumference on which are two ; earth the circumference on which is one cross.



Let the centre of heaven and earth be at point A, the centre of water at point B. Thus A, being the centre of the universe, is the lowest spot of all, and everything which has in the world a position alien from A must be higher. Now if there be any water at A and the way be open to it, it will naturally flow to its own centre, B, since it is the property of every heavy body to move to the centre of its own sphere. But the motion from A to B is a motion upwards ; therefore water will flow *upwards*, which is impossible.

Again, let there be at Z a lump of earth and some water, and let there be nothing to hinder. Then, since it is the property of every heavy body to move to the centre of its own sphere or circumference, the *earth* will move in a straight line to A, and the *water* in a straight line to B, and this, from the figure, must needs be along different lines. This, says Dante, is not only impossible, but would make Aristotle laugh if he were to hear it.

The third impossibility follows thus :—*Gravity* and *levity* are "passions" of simple bodies which are moved with linear motion, and *light* bodies tend upwards and *heavy* tend downwards, by "heavy" and "light" being meant that which has the power of being moved. If now water moved to B and earth to A, since these are simple bodies and heavy, they will be moved down to different centres. If this were so, the word *gravity* would have an *absolute* signification with respect to earth and *relative* with respect to water. This is what the argument amounts to, and so there would be an equivocation of meaning in the word "gravity."

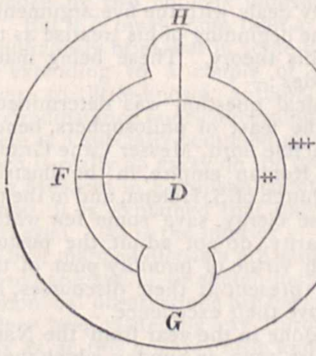
Therefore, *ab absurdo*, water in its natural circumference is not eccentric or out of the centre common to the circumference of the earth.

In the second place, *water cannot be gibbous.*

[1] "Aqua est labile corpus naturaliter, et non terminabile termino proprio." —§ xi.

Let the heavens be where are four crosses, the water where three, the earth where there are two.

Let D be the centre of *earth* and concentric *water* and *heaven*. Water cannot be concentric with the earth unless the earth be in some part "gibbous" above the central circumference. Let the protuberance of the earth be at G, and at some part of the circumference of water let there be a protuberance of water at H. Then let a line be drawn from D to H and another from D to F; it is manifest



that D H is longer than D F, and therefore the summit of one is higher than the summit of the other. Since both touch at their summit the surface of the water without passing beyond, it is clear that the water of the protuberance will be higher, with regard to the surface where F is. Since, therefore, there is no obstacle, the water of the protuberance will flow down until it become level at D with the central or regular circumference. And thus it will be impossible for a protuberance of water to last or even to exist.—Q.E.D.

Dante now brings forward a subsidiary argument to show that probably water has no protuberance out of the regular circumference. The protuberance of the earth is sufficient to prove and explain everything, and "Quod potest fieri per unum, melius est fieri per unum quam per plura." So there is no protuberance on the surface of the water, because God and Nature always do what is best and do not work in vain.

Since water cannot be eccentric, as was shown by the first figure, nor concentric with a protuberance, as was shown by the second figure, it is necessary that water be concentric and coequal, *i.e.* equally distant in every part of its circumference from the centre of the world.

Thus it has been proved impossible for water in any part of its circumference to be higher than the surface of the earth; and so the first point is completed.

We now pass on to the second.

2. This emergent earth is everywhere higher than the surface of the sea. This is shown in this way:—

All the shores of the ocean, as well as of the Mediterranean seas, rise above the surface of the sea which bounds them, as is clear to the eye. Therefore all the shores are further from the centre of the world, since the centre of the world is also the centre of the sea and the shoreward surfaces are parts of the whole surface of the sea; and since everything that is more remote from the centre of the world is also more high, it follows that the shores everywhere rise above the surface of the sea. And if the shores are higher than the sea, much more must be the other regions of the earth, since the shores are the lower parts of the earth, as we see by the rivers flowing down to them.

3. In accordance with the order of the question as at first stated by Dante, he now brings forward various arguments which seem to contradict his demonstrations, and these arguments he then proceeds to demolish. They need not detain us here. In the course of the operation there occurs a most interesting distinction between *homogeneous* and *simple* bodies, in which I seem to see a distinct foreshadowing of our modern view of the chemical elements in contradistinction to the ordinary four or five elements of Aristotle and his followers. "Corpora enim homogenea et simplicia sunt; *homogenea ut aurum depuratum;* et corpora simplicia, ut ignis et terra." § xviii.

But perhaps it might not be out of place to quote here the following passage from G. H. Lewes's "Aristotle":—"One of the great difficulties in interpreting ancient opinions is to guard against the tendency of reading our fulness of knowledge into their vague expressions. We often find in ancient works the precious metal we have ourselves brought with us; as the alchemist often unconsciously put into his crucible the gold, which he afterwards discovered there with surprised delight."—G. H. Lewes, "Aristotle," x. § 170.

He thus sets forth the *final* cause of the elevation or emergence of the earth: There must needs be a part in the universe where all *miscibilia*—to wit, elements—can come together; this cannot be unless the earth be in some part emergent. Thus, although earth, according to its own nature, tends always downwards, it has in it another nature by which it obeys the intention of Universal Nature, and allows itself to be here and there raised up, in order that mixture of the elements may be possible, and thence all things that are subject to generation and corruption may be formed.

He further shows that the emergent earth on which we dwell has the form of a semilune, by arguments which he graciously tells us that even ladies can follow—"Manifestum esse potest etiam mulieribus."

4. What now is the *efficient* cause of the elevation or emergence of the earth above the surface of the water?

Dante first shows that neither the *earth* itself, nor *water*, nor *air*, nor *fire* can be the efficient cause. Therefore it must be referred to the *heavens*. But there are many heavens, and to which are we to refer it? Dante shows that it cannot be referred to the *moon*, nor to the heavens of any of the planets (Mercury, Venus, the Sun, Mars, Jupiter, Saturn), nor yet to the Crystalline Heaven or *Primum Mobilè*, the 9th sphere. Now, since the only mobile bodies which remain are the Heaven of the Stars, *Cœlum Stellatum*, or 8th circle, we must refer the cause of the elevation of the earth of our hemisphere to that. This Heaven of the Stars has at once unity in substance and multiplicity in its virtues or influences, as the poet himself sings:—

"Il ciel, cui tanti lumi fanno bello,
Dalla mente profonda che lui volve
Prende l'image, e fassene suggello.
E come l'alma dentro a vostra polve,
Per differenti membra, e conformate
A diverse potenzie, si risolve;
Così l'intelligenzia sua bontate
Multiplicata per le stelle spiega,
Girando se sovra sua unitate."
—"Paradiso," ii. 130 138.[1]

Dante further refers the elevation of our earth to that region of the Cœlum Stellatum which roofs over this uncovered earth; that is, that this elevating virtue or influence is in those stars which are in the region of the heaven, which is bounded by the equator and the circle which the pole of the zodiac describes around the pole of the world; "whether it elevate by way of attraction as

[1] "The heaven, which lights so manifold make fair,
From the Intelligence profound, which turns it,
The image takes, and makes of it a seal.
And even as the soul within your dust
Through members different and accommodated
To faculties diverse expands itself,
So likewise this Intelligence diffuses
Its virtue *multiplied* among the stars,
Itself revolving on its *unity*."
"Paradiso," ii. 130-138, Longfellow's trans.

the magnet draws the iron, or by way of impulsion, generating impelling vapours, as in certain mountains." A truly scientific and most suggestive remark!

We may compare the last clause with those well-known lines of the "Inferno," in which is described how the earth, and likewise the mountain of Purgatory were formed when Lucifer fell from Heaven:—

"Da questa parte cadde giù dal cielo;
E la terra, che pria di qua si sporse,
Per paura di lui fe del mar velo,
E venne all'emisperio nostro: e forse
Per fuggir lui, lasciò qui il luogo voto
Quella ch'appar di qua, e su ricorse."
—"Inferno," xxxiv. 121–126.[1]

But now it may be asked, Since that region of the heaven moveth circlewise, why did not this elevation happen circlewise? Because, Dante answers, the matter was not sufficient for so great an elevation. Then why was the elevation of the earth produced in our hemisphere rather than in the other? To this, says Dante, we must answer as Aristotle does (in "De Cœlo," book ii.) in answer to the question why the heavens move from east to west and not contrariwise, that such questions proceed either from much folly or from much presumption, because they are above our intellect. God made all things for the best, and when He said, "Congregentur aquæ in locum unum et appareat arida," then were the heavens virtuated to act and the earth potentiated to be passive.

"Let therefore men cease," cries Dante, "yea, cease from inquiring into those things which are above their intellect, and let them strive to the utmost of their power to raise themselves to things immortal and divine, and so leave those things which exceed their understanding. Let them listen to Job:—'Numquid vestigia Dei comprehendes, et omnipotentem usque ad perfectionem reperies.'—(Job xi. 7.) Let them hearken to the words of the Psalmist: 'Mirabilis facta est scientia tua; et me confortatus est, et non potero ad eam.'—(Ps. cxxxviii.) Let them hear Isaiah speaking in the person of God to man: 'Quam distant cœli a terra, tantum distant viæ meæ a viis vestris.'—(Is. lv. 9.) Let them hear the voice of the Apostle to the Romans: 'O altitudo divitiarum scientiæ et sapientiæ Dei! quam incomprehensibilia judicia ejus, et investigabiles viae ejus!'—(Rom. xi. 33.) Lastly, let them hearken to the very voice of the Creator, saying: 'Quo Ego vado, vos non potestis venire.'—(S. John vii. 34). And let these things suffice for the inquiry of the truth before us."

We may most fittingly compare this Dantesque passage with the close of Galileo Galilei's famous "Dialogo intorno ai due massimi sistemi del mondo, tolemaico e copernicano," which I here venture to translate:—

"*Simplicio.* If either of you were asked, If God in His infinite power and wisdom could confer upon the element of water the reciprocal movement which we perceive in it, in another way than by the moving of the vessel containing it, I know that you would answer, that He could have done so in many ways, even unimaginable by our intellect; whence I immediately conclude that, this being so, it would be excessive daring for any one to wish to limit and restrict the Divine power and wisdom to a particular phantasy of his own.

"*Salviati.* An admirable and truly angelical doctrine, to which very conformably answers that other divine doctrine, which, whilst it allows us to dispute about the constitution of the world, adds (perhaps in order that the exercise of human minds be not suppressed nor grow lazy) that we are not to find out the work of His hands. Let therefore the exercise permitted and ordained to us by God make us recognize and so much the more wonder at His greatness, as we find ourselves the less competent to penetrate into the profound abysses of His infinite wisdom.

"*Sagredo.* And this will serve for the last conclusion of our four days' argument."—Galileo Galilei, "Dialogo dei Massimi Sistemi, Giornata quarta."

Dante now briefly deals with the five arguments which he mentioned at the beginning of his treatise as the most important against his theory. These being made short work of, he concludes:—

"This philosophical question was determined by me, Dante Alighieri, the least of philosophers, beneath the sway of that invincible lord, Messer Cane Grande della Scala, for the holy Roman empire, in the illustrious city of Verona, in the church of S. Helena, and in the presence of all the Veronese clergy, save some few who, aflame with too much charity, do not admit the postulates of others, and through virtue of humility poor of the Holy Spirit, shun being present at their discourses, lest they may seem to approve their excellence.

"Now this was done in the year from the Nativity of our Lord Jesus Christ, 1320, on Sunday, which the Saviour injoined on us to venerate for His glorious Nativity and His wondrous Resurrection. The which day was the 7th from the ides of January and the 13th before the calends of February" (*i.e.* January 20).

I have dealt merely with the chief parts of this Dantesque dissertation. According to Signor A. Stoppani ("La questione dell' Acqua e della Terra di Dante Alighieri," in "opp. Lat. di Dante," ed. Giuliani, vol. ii.) there are nine truths relating to cosmology, presaged, affirmed, and in part demonstrated. These nine he makes out thus:—

(1) The moon the principal cause of tides.
(2) Equality of the sea's level.
(3) Centripetal force.
(4) Sphericity of the earth.
(5) Dry land simply protuberance of the earth's surface.
(6) Northern grouping together of the continents.
(7) Universal attraction.
(8) Elasticity of vapours a motive power.
(9) Heaving up of the continents.

Let me now add a tenth: A vague foreshadowing of our modern idea of chemical elements as distinct from those of Aristotle, or at least of homogeneous chemical bodies; "Corpora enim homogenea et simplicia sunt; homogenea, ut aurum depuratum; et corpora simplicia, ut ignis et terra."

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MOROCCO.[1]

MOROCCO has a paradoxical place in the history of exploration; although the only part of Africa fully in sight from the shores of Europe, and dotted with one or two half European coast towns, its interior is more firmly closed to the traveller, sportsman, and missionary than the dense forests of the Congo, or even the shores of Lake Chad. The difficulties in the way are not physical, nor are they wholly political. They arise mainly from the deeply-rooted antagonism in race and creed between the inhabitants of Morocco and all Christendom—this quaint and semi-fossil phrase is still here a necessary and sufficient term. At this moment public atten-

[1] "Upon this side he fell down out of heaven;
And all the land, that whilom here emerged,
For fear of him made of the sea a veil,
And came to our hemisphere; and peradventure
To flee from him, what on this side appears
Left the place vacant here, and back recoiled."
—"Inferno," xxxiv. 121–126, Longfellow's trans.

[1] "Bibliography of the Barbary States." Part IV. A Bibliography of Morocco from the earliest times to the end of 1891. By Lieut.-Col. Sir R. Lambert Playfair and Dr. Robert Brown.

tion is turned somewhat intently on the political conditions of the Oriental despotism which has so anomalously maintained itself to the west of our prime meridian. Hence the politician has a temporary interest in what would otherwise have appealed mainly to the geographer and man of science, the publication by the Royal Geographical Society of a "Supplementary Paper," the "Bibliography of Morocco." This is a work of splendid thoroughness, almost, if not quite, exhaustive in its list of 2243 titles, and made convenient for reference by two copious indexes of subjects and authors. But it is much more than a catalogue. Comments, judiciously brief, but in some cases of exceptional interest extending to a couple of pages, give information as to little-known authors, or record some striking circumstance in or concerning the books referred to. There is a specially-compiled map, and an introduction which is really an essay on the growth of knowledge regarding Morocco in European countries. With regard to the map, it is explained that only the coast-line has been surveyed. As to the interior :—

"The best mapped districts are laid down solely from running *reconnaisances* or sketch-maps. Positions fixed by astronomical observations are few. Many wide areas have never been visited by any Europeans, and most of the Atlas is at this hour as little known as it was in the days of Leo Africanus. There are cities within a few hours' ride of Tangier, which no person capable of giving a correct account of his observations has visited; and there are others not much farther away, to attempt to enter which—Zarhoun, for example—would, were the intruder detected, be certain death. There is scarcely a river laid down with even approximate accuracy, and, not to enumerate more distant provinces, the entire Riff country, that bold *massif* which is familiar to the thousands who every year sail up and down the Mediterranean, is less explored than many regions in the centre of the continent."

The present population of Morocco is a puzzle almost as difficult, although on a smaller scale, as that of China. The authors of the Bibliography give 4,000,000 as an estimate, but the guesses of various authorities vary between 1½ and 15 millions. The roads shown on the map are mere mule and camel tracks made by the feet of the pack-animals, unaided by any engineer. Ferries are rare, and, of course, bridges are unknown in the interior. The distribution of towns and villages is often at variance with the rules holding for civilised countries. The villages are built out of the way of the main tracks, because people never travel in Morocco for the good of the inhabitants, and it is safer to live off the path of the tax-collector and the Government official, who demands free food and quarters. The great number of place-names on the map of so thinly-peopled a country is due to the fact that the tombs of saints are such important landmarks that they must be indicated, even if only a few persons live beside them. "All the places beginning widh 'Sidi' (Lord, master) are either actually tombs or the tomb has formed, as in so many of our cathedral cities, the nucleus of the town or village." "Sok," another affix of frequent occurrence, means market-place, and many of the established sites for periodical fairs are uninhabited between the gatherings of people from far and near. Many of the place-names on the coast exist in two forms at least—the native word and its Portuguese or Spanish translation; Casablanca and Dar-el-beida (both meaning White house) for example. We regret that the authors did not see their way to lay down precise rules for the spelling of Moorish place-names, either by giving a standard transliteration of the Arabic, or a uniform phonetic system. Indeed, even in the introduction a few anomalous spellings are found, *e.g.* *Zarhoun* and *Zerhun*, *Moulai* and *Mowlai*.

The physical geography of Morocco appears to be changing, and the natural conditions of the country are less favourable for agriculture than they were a few centuries ago. The forests have been destroyed with such recklessness that the soil has been dried up and swept away in many places; there is evidence that the rainfall has diminished, lakes have dried, and rivers formerly navigable have become silted up, or alternate as dry tracts of stone and raging torrents.

In one respect alone—the enthusiastic Moslemism of its people—does Morocco show no sign of degeneration. Although the Moors can no longer seize and hold the Christian slaves, whose stories bulk so largely in the bibliography, their hatred and contempt towards "unbelievers" is in no sense abated. Into such a land no Europeans could penetrate far, except in the past as slaves, or now as official messengers of European Powers under special protection, jealously watched and prevented from studying places or people. The last serious attempt at scientific exploration—that of Mr. Joseph Thomson—was again and again almost stopped by the fanatical Kaids, and only his remarkable persistence and daring stratagems carried him as far as he reached. Such stratagems would hardly serve again, and for the present the exploration of the Atlas Mountains, with their half-guessed topography, imperfectly-known flora, and unsurveyed mineral wealth is at an end. The futility of disguise as an aid to exploration is fully proved in the records before us, where the ghastly fate of many who tried to pass as Moslems, and the unsatisfactory results obtained by others who escaped alive, are briefly told.

It seems to us that an attempt might well be made to open communications with fanatical Mohammedan countries either by explorers or diplomatic agents of the same faith, and there must be many amongst the educated Mohammedans of India who are well suited for such work. The religious beliefs of a people with whom belief and conduct are so closely related, must be taken into account in dealing with them, just as much as the physical features of a country. And as Arctic sailors have been proved to be the natural explorers in the Antarctic seas, Swiss mountaineers the safest pioneers on New Zealand glaciers, and Canadian boatmen the most expert in shooting the Nile cataracts, so Mohammedan envoys might be expected to make the most favourable impression on the people of Morocco or of the Mohammedan Sudan.

Sir Lambert Playfair and Dr. Brown deserve the heartiest thanks for completing their Bibliography of the Barbary States in such an admirable way, and we do not doubt that the work will be very widely consulted in the immediate future.

THE RATE OF EXPLOSION IN GASES.

THE following is an abstract of the Bakerian Lecture on "The Rate of Explosion in Gases," delivered before the Royal Society by Prof. Harold B. Dixon, on January 19 :—

1. Berthelot's measurements of the rates of explosion of a number of gaseous mixtures have been confirmed. The rate of the explosion wave for each mixture is constant. It is independent of the diameter of the tube above a certain limit.

2. The rate is not absolutely independent of the initial temperature and pressure of the gases. With rise of temperature the rate falls; with rise of pressure the rate increases; but above a certain *crucial pressure* variations in pressure appear to have no effect.

3. In the explosion of carbonic oxide and oxygen in a long tube, the presence of steam has a marked effect on the rate. From measurements of the rate of explosion with different quantities of steam, the conclusion is drawn that at the high temperature of the explosion wave, as

well as in ordinary combustion, the oxidation of the carbonic oxide is effected by the interaction of the steam.

4. Inert gases are found to retard the explosion wave according to their volume and density. Within wide limits an excess of one of the combustible gases has the same retarding effect as an inert gas (of the same volume and density), which can take no part in the reaction.

5. Measurements of the rate of explosion can be employed for determining the course of some chemical changes.

In the explosion of a volatile carbon compound with oxygen, the gaseous carbon appears to burn first to carbonic oxide, and afterwards, if oxygen is present in excess, the carbonic oxide first formed burns to carbonic acid.

6. The theory proposed by Berthelot—that in the explosion wave the flame travels at the mean velocity of the products of combustion—although in agreement with the rates observed in a certain number of cases, does not account for the velocities found in other gaseous mixtures.

7. It seems probable that in the explosion wave—

(1) The gases are heated at *constant volume*, and not at *constant pressure*;

(2) Each layer of gas is raised in temperature *before* being burnt;

(3) The wave is propagated not only by the movements of the burnt molecules, but also by those of the heated but yet unburnt molecules;

(4) When the permanent volume of the gases is changed in the chemical reaction, an alteration of temperature is thereby caused which affects the velocity of the wave.

8. In a gas, of the mean density and temperature calculated on these assumptions, a sound wave would travel at a velocity which nearly agrees with the observed rate of explosion in those cases where the products of combustion are perfect gases.

9. With mixtures in which steam is formed, the rate of explosion falls below the calculated rate of the sound wave. But when such mixtures are largely diluted with an inert gas, the calculated and found velocities coincide. It seems reasonable to suppose that at the higher temperatures the lowering of the rate of explosion is brought about by the dissociation of the steam, or by an increase in its specific heat, or by both these causes.

10. The propagation of the explosion wave in gases must be accompanied by a very high pressure lasting for a very short time. The experiments of MM. Mallard and Le Chatelier, as well as the author's, show the presence of these fugitive pressures. It is possible that data for calculating the pressures produced may be derived from a knowledge of the densities of the unburnt gases and of their rates of explosion.

NOTES.

THE forty-sixth annual general meeting of the Institution of Mechanical Engineers will be held on Thursday evening and Friday evening, February 2 and 3, at 25, Great George Street, Westminster. The chair will be taken by the president, Dr. William Anderson, F.R.S., at half-past seven on each evening. The annual report of the council will be presented to the meeting on Thursday, and the annual election of the president, vice-presidents, and members of council, and the ordinary election of new members will take place on the same evening. The following papers will be read and discussed, as far as time permits:—Description of the Experimental Apparatus and Shaping Machine for Ship Models at the Admiralty Experiment Works, Haslar, by Mr. R. Edmund Froude, of Haslar (Thursday); description of the Pumping Engines and Water-Softening Machinery at the Southampton Water Works, by Mr. William Matthews, Waterworks Engineer (Friday).

PROF. CAYLEY, we are glad to learn, is now convalescent.

WE greatly regret to have to announce the death of Mr. H. F. Blanford, F.R.S. He died on Monday at the age of fifty-eight.

PROF. MICHAEL FOSTER, Sec.R.S., has been appointed Rede Lecturer at Cambridge for the present term. His Rede lecture will be delivered early in June.

THE Bill for the introduction of a standard time (mean solar time of the fifteenth meridian) was read a second time in the German Imperial Parliament on Monday. The measure was accepted without much discussion.

AN excellent report on technical education in London has been submitted to the London County Council by a special committee appointed to investigate the subject. The report was prepared by Mr. Llewellyn Smith, the committee's secretary, and displays a thorough grasp of the essential conditions of the problem. It is proposed that a Technical Instruction Board shall be appointed, and that it shall consist of some members of the Council, and of representatives of the School Board, the City and Guilds of London Institute, the City Parochial Charities, the Head Masters' Association, the National Union of Elementary Teachers, and the London Trades Council. The committee think that one-third of the amount derived from the beer and spirits duties should be handed over to this body for the provision of adequate technical instruction in all parts of London.

THE French Minister of the Interior has established at Marseilles, in connection with the university, an institute for botanical and geological research, and a museum. The director is Prof. Heckel, who, as well as a curator and a librarian, gives his services gratuitously.

IN the year 1793 was published Christian Konrad Sprengel's "Das entdeckte Geheimniss der Natur, im Bau und in der Befruchtung der Blumen," the work which first directed the attention of naturalists to the contrivances which, in many flowers, render self-pollination difficult, and promote the visits of insects to assist cross-pollination. The copper-plate illustrations of this work still maintain their character as among the best that have been published in this branch of science. Sprengel was in many respects a forerunner of Darwin, and centenaries have been celebrated on slighter grounds than the publication of this work.

THE chief characteristics of the weather during the past week have been its general mildness and dampness; the day temperatures have at times exceeded 50° in most parts of the kingdom, but at night slight frosts occurred towards the end of last week in Scotland and the south-eastern parts of England. The distribution of pressure has been complex, a series of depressions have passed over the coast of Norway from the westward, while an anticyclone lay over the south-western parts of our islands, the reading of the barometer in the south-west being about an inch higher than in the north of Scotland. The passage of the low-pressure systems in the north was accompanied by strong north-westerly winds and gales in Scotland, with hail or sleet in many places. Owing to the disappearance of the anticyclone from the continent, north-westerly winds became prevalent over western Europe, and a rapid rise of temperature occurred there, amounting to 30° in Germany between the 20th and 21st instant. During the last few days fresh depressions have approached our north-western coasts, with increasing winds from the south-west, and a continuance of mild, unsettled weather appeared probable. The *Weekly Weather Report* shows that for the week ending the 21st instant there was a large deficiency of rainfall in the west of Scotland, south-west of England, and south of Ireland. The percentage of possible duration of sunshine ranged from 28 in the south-west of England to 7 in the south of England and to 3 in the north of Scotland.

THE *Repertorium für Meteorologie*, vol. xv. recently issued by the Imperial Academy of Sciences of St. Petersburg, contains a discussion by P. A. Müller, of the Ekaterinburg Observatory, at the foot of the Ural Mountains, in the Government of Perm, on the question of the evaporation from a snow surface. Several writers, among whom are Drs. Brückner and Woeikof, differ in opinion as to whether the evaporation from a snow surface exceeds the condensation of the aqueous vapour of the air immediately above it. The method generally adopted for the decision of the question is to find whether the temperature of the snow surface is above or below the dew-point of the surrounding air; in one case there would be evaporation, and in the other condensation. The paper occupies forty-seven small folio pages, and the observations were made hourly from December 21, 1890, to February 28, 1891. The result of the investigation shows that according to the temperatures of the dew-point and of the surface of the snow, the evaporation of the snow greatly exceeds the condensation of the aqueous vapour, for the condensation occurred at only 27 per cent. while the evaporation occurred at 73 per cent. of the hourly observations.

PROF. FLINDERS PETRIE, to whose introductory lecture at University College, Gower Street, we referred last week, delivered on Saturday the first of his regular course of lectures on the Edwards Foundation. He said the Egypt of the early monuments was a mere strip of a few miles wide of green, amid boundless deserts, and beneath a sky of the greatest brilliancy; a land of extreme contrasts of light and shadow, of life and death. These conditions were reflected in the art. On the one hand was the most massive and overwhelming construction, and, on the other, the most delicate and detailed reliefs. On the one hand, the most sublime and stolid statuary; on the other, the course and accidents of daily life freely treated. On the one hand, masses of smooth buildings that far outdo the native hills on which they stand, gaunt and bare, and, on the other, the vivid and rich colouring in the interiors. In consequence of the climate also Egypt is a land of great simplicity of life, and simplicity is especially the characteristic of the oldest Egyptian buildings. Speaking of the early Egyptian statues, Prof. Petrie said that the race represented by them appears as "one of the noblest that ever existed."

AT Leeds, on Monday, Lord Playfair presided at a public dinner, held in support of the Yorkshire College. In proposing the principal toast—"The Yorkshire College"—he spoke of the efforts made half a century ago to secure for science the place which rightly belongs to it in the educational system. He was glad, he said, that these efforts had met with a temporary resistance, because if the Universities had at once yielded there would have been no colleges now in our great provincial towns. The colleges, he thought, were adapting themselves rapidly and well, upon the whole, to the genius of their several localities. Of the Yorkshire College he said that she had fitted herself for the liberal culture and life-work of a great industrial centre. "No doubt her technical courses are peculiar. Actual laboratories for spinning, for dyeing, for tanning, for engineering, are novel adjuncts to a college. What does it mean? That you are trying to strengthen and embellish industrial pursuits, as the Universities acted upon the professions when they were obliged to include them. Surely a great town like Leeds is right when it imbues its producers with intellectual knowledge, as well as with technical expertness. Such men in future carve out industrial professions for themselves, and illumine them by appropriate culture."

THE interesting address lately delivered by Sir Henry Roscoe on the occasion of the prize distribution at the Birmingham Municipal Technical School has now been issued separately. He describes the report of the first year's work as "more than encouraging." Speaking of the building which is to be erected for technical training at Birmingham, he says:—"You in Birmingham have, in my judgment, taken the right course. You are not going to squander your money by using it for a thousand different purposes. You are, I hope, going to do a good thing, and a big thing, in building and equipping a really great institution, worthy of your city and of your well-earned renown as being foremost amongst our towns in educational matters. You will have a place of higher technical instruction to which all the Midlands will look up. It will be the gathering ground for all the youthful talent of the busy millions of the district. It will be here that the future Faradays, and Priestleys, and Watts will get that sound though elementary scientific training which will enable them to pursue that training to its highest point at the Mason College here, or in other colleges elsewhere, which may in the end make both them and their country great."

THE new technical schools connected with University College, Nottingham, which were formally opened the other day, promise to be of immense service, not only to Nottingham itself, but to the wide district of which it is the educational centre. A remarkably clear description of the buildings, with plans, is given in a pamphlet prepared for the ceremonial opening. The pamphlet also includes an interesting summary of the facts relating to the history of the Nottingham College and its technical department.

MR. C. F. JURITZ, Senior Analyst in the Department of Lands, Mines, and Agriculture, Cape Colony, announces in the *Agricultural Journal*, issued by the Department, that a comprehensive series of investigations with reference to the chemical composition of the various soils of the colony is about to be undertaken. The samples of soil are to be collected by one of the officers of the analytical branch of the Department. In the first instance the southern part of the Malmesbury district will be visited, and soils will be taken from several localities representative (*a*) of primary and (*b*) of alluvial soils belonging to the Malmesbury beds of clay slate. Mr. Juritz proposes next to collect soils from the more northerly portion of the same district, in the vicinity of Hopefield, for instance, after which the Caledon district will be taken in hand. These analyses when completed will afford, he points out, an insight into the general composition of the clay slate soils, lying around the south-western coast of the Colony between Donkin's and Mossel Bays. The Government of Cape Colony look upon the proposals that have been made as "a move in the right direction," and have promised their warmest support.

MR. KEDARNATH BASU, describing in *Science* some relics of primitive fashions in India, says he does not see the same profusion as he saw ten or twelve years ago, of tattoo-marks and red-ochre or red oxide of lead (*sindur*) over the forehead and crown among the women of Bengal. The rapid progress of female education and the consequent refinement in æsthetic taste are, he says, the causes of the decline of this rude and savage adornment. The people of Behar, the North-western provinces, and other districts, however, still cling to these remnants of savagery. The up-country women, besides tatooing their bodies and painting the head with red paint, bore the lower lobes of their ears, and insert big and heavy wooden cylindrical plugs, which almost sever the lobes from the ears. The plugs are sometimes as big as two inches in length with a diameter of an inch and a half, and as much as two ounces in weight. These heavy plugs pull down the lobes of the ears as far as the shoulders, and give the wearers a hideous look.

MR. F. J. BLISS contributes to the new "Quarterly Statement" of the Palestine Exploration Fund a most interesting report on the excavations at Tell-el-Hesy during the spring season of 1892. Speaking of the now famous tablet discovered in the course of these excavations, he says:—"On Monday, May 14, ten days before we closed the work, I was in my tent at noon with Ibrahim Effendi, when my foreman Yusif came in with a small coffee-coloured stone in his hand. It seemed to be curiously notched on both sides and three edges, but was so filled in with earth that it was not till I carefully brushed it clean that the precious cuneiform letters were apparent. Then I thought of a day, more than a year before, when I sat in Petrie's tent at the pyramid of Meydûm, with Prof. Sayce. He told me that I was to find cuneiform tablets in the Tell-el-Hesy, which as yet I had never seen; and gazing across the green valley of the slow, brown Nile, and across the yellow desert beyond, he seemed to pierce to the core, with the eye of faith, the far away Amorite mound. As for me, I saw no tablets, but I seemed to be seeing one who saw them!" Mr. Bliss also notes that the discovery was a triumphant vindication of Mr. Flinders Petrie's chronology—established, not by even a single dated object, but by pottery, mostly plain and unpainted. It is announced in the "Quarterly Statement" that the excavations at Tell-el-Hesy are now being vigorously carried on by Mr. Bliss, who has recovered from his serious illness.

IT seems that in Yucatan and Central America, as in Egypt and other countries, ancient monuments are held in small respect by certain classes of travellers. According to Mr. M. H. Saville, assistant in the Peabody Museum, who writes on the subject in *Science*, enormous damage is being done to many of the most interesting antiquities in these regions. The magnificent "House of the Governor" in Uxmal, described as probably the grandest building now standing in Yucatan, is almost covered with names on the front and on the cemented walls inside. These names are painted in black, blue, and red, and among them are the names of men widely known in the scientific world. The "House of the Dwarfs" in the same city has suffered in like manner, and many of the sculptures which have fallen from the buildings in Uxmal have been wilfully broken. In Copan, when the Peabody Museum Honduras Expedition compared the condition of the "Idols" to-day with the photographs taken by Mr. A. P. Maudslay seven years ago, it was found that during that time some of the very finest sculptures had been disfigured by blows from machetes and other instruments. The Stela given as a frontispiece in Stephens' "Incidents of Travel in Central America," vol. i., has been much marred by some one who has broken off several ornaments and a beautiful medallion face from the northern side. One of the faces and several noses have been broken off from the sitting figures on the altar figured by Stephens in the same volume, opposite page 142; and on some of the idols and altars names have been carved. While excavating in one of the chambers of the Main Structure, members of the Expedition uncovered a beautiful hieroglyphic step, but before they had time to secure a photograph of it, some visitor improved the opportunity while no one was about to break off one of the letters. In Quirigua a small statue, discovered by Mr. Maudslay and removed by him to a small house near the rancho of Quirigua, had the head and one of the arms broken from it during the interval between two visits. This statue was of the highest importance, as it very much resembled the celebrated "Chaac-mol" now in the Mexican Museum, but discovered by Le Plongeon at Chichen Itza. Much mischief is also done by natives, who think nothing of tearing down ancient structures in order to provide themselves with building materials. The authorities of the Peabody Museum, to whom the care of the antiquities of Honduras has been granted for a period of ten years, deserve much credit for the efforts they make to cope with the evil. They have caused a wall to be built round the principal remains in Copan, and a keeper has been placed in charge with strict orders to allow nothing to be destroyed or carried away.

WHAT is the true Shamrock? Most Irishmen are probably of opinion that they can answer the question correctly, but unfortunately they do not all give the same reply. Mr. Nathaniel Colgan, who has been investigating the subject, collected thirteen specimens from the following eleven counties—Derry, Antrim, Armagh, Mayo, Clare, Cork, Wexford, Wicklow, Carlow, Queen's County, and Roscommon. Shamrocks were thus secured from northern, southern, eastern, western, and central Ireland, Mr. Colgan's correspondents in the various counties taking pains to have each sample selected by a native of experience who professed to know the genuine plant. All the specimens were planted and carefully labelled with their places of origin, and flowering within some two months later gave the following results: eight of the specimens turned out to be *Trifolium minus* of Smith, and the remaining five *Trifolium repens* of Linnæus. Cork, Derry, Wicklow, Queen's County, Clare, and Wexford declared for *Trifolium minus*; Mayo, Antrim, and Roscommon for *Trifolium repens*; and Armagh and Carlow, each of which had sent two specimens, were divided on the question, one district in each county giving *T. repens*, while the other gave *T. minus*. These results are set forth by Mr. Colgan in an interesting paper in the first volume of the *Irish Naturalist*, to which we referred last week. Elsewhere in the same volume Mr. R. L. Praeger suggests that authentic specimens of shamrock should be obtained from every county in Ireland, and he adds that he has no doubt Mr. F. W. Moore would gladly grow them at Glasnevin Gardens, if Mr. Colgan did not care to undertake so large an order. Mr. Praeger notes that in his own district, North Down, *Trifolium minus* is always regarded as the true shamrock, but that a luxuriant specimen, or one in flower, is generally discarded as an impostor.

THE waters of the Great Salt Lake, Utah, are known to vary in salinity at different times. Dr. Waller, of Columbia College, gives the results of his recent determination of the dissolved salts in the *School of Mines Quarterly*. A comparison of his results with those obtained by Gale, Allen, Bassett, and others, shows a constant change of salinity, and a closer examination reveals a variation from place to place. This is due to local differences in the amount of evaporation, and to the influx of water, fresh or saline, in many cases from subterranean springs which give no indication of their presence. For some of the constituents the water is nearly at saturation point, and differences of temperature are also apt to cause slight differences of composition. The presence of lithium and bromine strengthens Captain Bonneville's conclusions with regard to the basin of the ancient lake called after his name, and now represented by the Great Salt Lake and its lesser neighbours. The benches of sand and gravel seen high up on the flanks of the Wahsatch mountains and the Oquirrh range indicate the eastern and western shores of the old lake, whose waters must have covered an area equal to that of Lake Huron, or ten times that of the Great Salt Lake. Successive lowerings of level finally cut off its outlet to the north, by which it used to flow into the Pacific Ocean.

A BEAUTIFUL optical phenomenon, which has not yet been satisfactorily explained, is described by M. F. Folie in the *Bulletin* of the Belgian Academy. It was observed about a mile from Zermatt on August 13 at 8.30 a.m. "On our right, towards the east, on the steep flanks of the mountains which enclose the valley of the Viège, rose a group of fir trees, the highest of which projected themselves against the azure of the sky, at a height of 500 m. above the road. Whilst I was botanising my son exclaimed: 'Come and look: the firs are as if covered with hoar-frost!' We paid the most scrupulous attention to the phenomenon. To make sure that we were not misled by an illusion we made various observations, both with the naked eye and with an excellent opera-glass." It was observed that not only the distant trees, but those lining the road, glittered in a silvery light, which seemed to belong to the trees themselves, and that the insects and birds playing round the branches were bathed in the same light, forming an aureole round the tops of the trees, somewhat resembling the light effects observed in the Blue Grotto. It is suggested that the light was reflected from the snow. Since it disappeared as soon as the sun rose above the hill, and has never been seen except in the presence of snow, this explanation appears plausible, but it is highly desirable that further and more detailed observations should be made of this *spectacle féerique*.

THE Tasmanian Official Record is henceforth to be issued tri-annually instead of annually, and a handbook has been issued to take its place during the intervening years. This handbook (which is described on the title-page as "for the year 1892") contains a brief epitome of the historical portion of the Official Record, and summarises in a convenient form the more important statistical information contained in the detailed tables of the last volume of the general statistics of the colony.

MESSRS. ASHER AND CO. will publish shortly an English translation of the "Recollections of the Life of the late Werner von Siemens," the well-known electrician, and brother of Sir William Siemens. Two editions of the German original, published in December last, were issued in the course of a few weeks.

THE course of four winter lectures in connection with the London Geological Field Class will this year be delivered by Prof. H. G. Seeley, F.R.S., on Tuesday evenings, at the Memorial Hall, Farringdon Street, the subject being "The Fossil Reptiles of the Thames Basin." All particulars may be had of the Hon. Sec. Mr. J. H. Hodd, 30 and 31, Hatton Garden, E.C.

THE bacterial purification which takes place in a river during its flow has been recently attributed in part to the process of sedimentation which the micro-organisms in the water undergo, but it would seem that yet another factor must be taken into account. Buchner, in some investigations which he has recently published ("Ueber den Einfluss des Lichtes auf Bakterien," *Centralblatt für Bakteriologie*, vol. 11, 1892, also vol. 12, p. 217) shows that this diminution of the numbers present may be also assisted by the deleterious action which light exercises upon certain micro-organisms. A systematic series of experiments was made by introducing typhoid bacilli, *B. coli communis*, *B. pyocyaneus*, Koch's cholera spirilla, also various putrefactive bacteria, into vessels containing sterilized and non-sterilized ordinary drinking water. As a control, in each experiment one vessel thus infected was exposed to light, whilst a second was kept under precisely similar conditions, with the exception of its being covered up with black paper, by means of which every particle of light was excluded. The uniform result obtained in all these experiments was that light exercised a most powerful bactericidal action upon the bacteria in the water under observation. For example, in one water in which at the commencement of the experiment 100,000 germs of *B. coli communis* were present in a c.c., after one hour's exposure to direct sunlight *none* were discoverable, whilst in the darkened control flask during the same period a slight increase in the numbers present had taken place. Even the addition of culture fluid to the flasks exposed to sunlight could not impair in the least the bactericidal properties of the sun's rays. In the flasks exposed to diffused daylight the action was less violent but still a marked diminution was observed. In his later experiments Buchner has employed agar-agar, mixing a large quantity of particular organisms, pathogenic and others, with this material in shallow covered dishes and then exposing them to the action of light and noting its effect upon the development of the colonies. For this purpose strips of black paper cut in any shape (in the particular dish photographed by Buchner letters were used) were attached outside to the bottom of the dish, which was then turned upwards and exposed to direct sunlight for one to one and a half hours and to diffused daylight for five hours. After this the dish was incubated in a dark cupboard. At the end of twenty-four hours the form of the letters fastened to the bottom of the dish was sharply defined, the development of the colonies having *taken place in no part of the dish, except in those portions covered by the black letters*. Some interesting experiments on the same subject have also recently been made by Kotljar (*Centralblatt für Bakteriologie*, December 20, 1892). In the course of these investigations the author found that of the coloured rays of the spectrum the red favoured the growth of those bacteria experimented with, whilst the violet rays acted prejudicially, although less so than the white rays. The exceedingly interesting observation was made that the violet rays actually favoured the sporulation of the *Bac. pseudo anthracis*.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♂) from India, presented by Mr. A. Sandbach; a Triton Cockatoo (*Cacatua triton*) from New Guinea, presented by Mr. Arthur Harter; a Gannet (*Sula bassana*) British, presented by Mr. F. W. Ward; two Tuatera Lizards (*Sphenodon punctatus*) from New Zealand, presented by Mr. W. H. Purvis; two Wanderoo Monkeys (*Macacus silenus*) from the Malabar Coast; a Straw-necked Ibis (*Carphibis spinicollis*) from Australia; four Snow Buntings (*Plectrophanes nivalis*); six Wild Ducks (*Anas boschas*, 3 ♂ 3 ♀) British, purchased; a Meadow Bunting (*Emberiza cia*) European, received in exchange; two Shaw's Gerbilles (*Gerbillus shawi*) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

COMET HOLMES.—*Edinburgh Circular*, No. 37, announces that Palisa, telegraphing from Vienna, states that Comet Holmes now resembles an 8 m. star with a nebulous envelope 20″ of arc in diameter.

A further observation made by Prof. Schur in Göttingen on January 19 showed that the nucleus was of the 10th magnitude, and could not be considered at all brighter than that magnitude. For the latter observation the air, as regards clearness, was all that could have been desired.

At South Kensington, on January 18, the comet was observed as a hazy star and estimated to be about the 8th magnitude.

The following ephemeris is that given by Schulhof:—

Date.		R.A. app.			Decl. app.		
		h.	m.	s.	°	′	″
Jan.	26 ...	1	35	33·0 ...	+33	42	3
	27 ...		36	58·7 ...		42	51
	28 ...		38	25·1 ...		43	44
	29 ...		39	52·1 ...		44	43
	30 ...		41	19·8 ...		45	46
	31 ...		42	48·1 ...		46	55
Feb.	1 ...		44	17·0 ...		48	8
	2 ...	1	45	46·5 ...	33	49	26

On January 30 the comet will lie very nearly between β Andromedæ and β Trianguli, about one-third of the distance from the latter star.

COMET BROOKS (NOVEMBER 19, 1892).—The following ephemeris of Comet Brooks is due to Ristenpart, and is given in *Astronomische Nachrichten*, No. 3142:—

1893.	R.A. (app.) h. m. s.	Decl. (app.) ° ′	Log *r*.	Log Δ.	Br.
Jan. 26	23 35 8	+40 34·3	0·0921	0·0471	2·94
27	38 53	39 34·1			
28	42 22	38 36·8	0·0950	0·0688	2·62
29	45 37	37 42·2			
30	48 40	36 50·2	0·0981	0·0898	2·35
31	51 32	36 0·5			
Feb. 1	54 14	35 13·1	0·1015	0·1101	2·11
2	23 56 48	34 27·7			

This comet, which will be found to be in the constellation of Andromeda, will lie about 3½° to the south of *i* Andromedæ on January 27.

PHOTOGRAPHIC ABSORPTION OF OUR ATMOSPHERE.—The question of the degree with which our atmosphere absorbs photographic rays has become very important owing to the adoption of photography, so that any work enlightening us on this subject is anxiously listened to. Prof. Schaeberle, who has been making investigations in this direction, has just completed a memoir which is being published by the University of California, but in the meanwhile he has issued a table setting forth simply the final results. The absorption in the following table is expressed in photographic magnitudes, and must be added to the unknown atmospheric absorption at the zenith.

Z. D. °	Phot. Absorp.	Z. D. °	Phot. Absorp.
5	0·00	50	0·44
10	0·01	55	0·56
15	0·04	60	0·71
20	0·07	65	0·89
25	0·11	70	1·12
30	0·16	75	1·45
35	0·21	80	1·94
40	0·28	85	2·68
45	0·35	90	5·00

HARVARD COLLEGE OBSERVATORY.—The forty-seventh annual report of this Observatory, by Prof. Pickering, opens with a reference to the death of Mr. George B. Clark, to whose "genius for mechanical devices, indomitable perseverance, and devotion to the interests of the observatory, we are indebted for the success of many of our most useful instruments." Of the most important matters mentioned in the report are the permanent establishment of an observing station in South America, where the unsteadiness of the air is for the most part eliminated, the construction of a suitable building for the housing of photographs and the approaching completion of the Bruce photographic telescope. The work done with the various instruments during this period has been considerable. With regard to the Draper telescope, as many as 2777 photographs have been taken, while those taken with the Bache instrument number nearly 2000. The Boyden department, which is situated at Arequipa, in Peru, has been making great progress, the results of which have been frequently inserted in *Astronomy and Astrophysics*. The eight surfaces of the objective of the Bruce telescope have, as Prof. Pickering informs us, been ground and polished, and the results up to the present, according to tests made on a star, are very satisfactory. This instrument, when finished, is destined fo. the Arequipa station.

SOLAR OBSERVATIONS AT ROME.—Prof. Tacchini has issued the results of the observations made with regard to the distribution in latitude of the solar phenomena at the Royal Observatory during the third semester in 1892. From the tabulated statement which he gives the following facts may be gathered.

With regard to the eruptions, these phenomena seem to be quite local to the equatorial regions, the relative frequency being 0·667 and 0·333 for the north and south latitudes respectively. The spots, faculæ, and eruptions have their maxima nearly at the same distance from the equator both north and south, the zones being (± 20°, ± 30°), but the maxima for the prominences extend further north, about latitudes 60° north and south. Prof. Tacchini remarks that in the equatorial zone (+20° − 20°), where the maxima of faculæ, spots, and eruptions are observed, a feeble relative frequency in the prominences is noted, which shows us that we must consider a large number of prominences as the result of conditions "bien différentes par rapport à celles qui déterminent la production des taches dans la photosphère," whilst the prominences are formed simply in the solar atmosphere. As a case in point, he mentions an observation made on August 1 of last year, of a cloud which, starting at a distance of 264″, rose to 364″ without any corresponding alteration at the surface.

THE TOTAL SOLAR ECLIPSE, APRIL 15–16, 1893.—Writing to M. Flammarion about the scientific expedition sent by the Brazilian Government to study the region of the central plateau and to select a site for the proposed new capital, Dr. Cruls, the Director of the Observatory at Rio de Janeiro, adds the following note:—"About the total eclipse of April 16. Will France send any one to observe it? I beg you to make known through the *Review* (*L'Astronomie*) that the Brazilian Government is willing to send a warship to Ceara, on which foreign astronomers who wished to observe the phenomenon could find a passage."

GEOGRAPHICAL NOTES.

A CHANGE has been made in the arrangements for the expedition to Lake Rudolf referred to on p. 235, vol. xlvii. The expedition is to travel by the Tana river instead of the Juba, although its ultimate destination is the same, and Lieutenant Villiers, instead of accompanying it, has joined Sir Gerald Portal's mission to Uganda.

MR. H. J. MACKINDER, M.A., Reader in Geography at Oxford, delivered the first of a course of ten educational lectures, under the auspices of the Royal Geographical Society, on the relation of geography to history, on the 20th inst. The attendance was largely composed of teachers and University Extension students, to whom special terms were offered. The lecturer treated of "the Theatre of History," tracing the development of accurate geographical knowledge from the earliest times in a series of brilliant generalisations. He dwelt upon the contrast between the knowledge of early Greek geographers regarding the true shape of the earth, and their habitual representation of the regions known to them in a circular form. In the middle ages, amongst the half-learned, the map of the known world was elevated to the highest place, the figure of the globe was forgotten, and the doctrine of a flat earth gained currency. At the geographical *renaissance* the map was adapted once more to the sphere, and the discoveries of Columbus and his contemporaries resulted directly.

THE suggestion of Mr. Joseph Thomson to bestow the name of Livingstonia (vol. xlvii. p. 160) on the British sphere of influence north of the Zambesi, in spite of its singular propriety, has, we fear, failed to convince the authorities in charge of the region, who, it appears, have decided to adopt the cumbrous and scarcely accurate title of British Central Africa.

M. MIZON'S second expedition to Adamawa has been stopped on the Benué by the breakdown of his steamers, and the sudden falling of the water, he being left without means of progress about two-thirds of the way between Lukoja and Yola.

THE French flag has been formally hoisted on the little islands of St. Paul and New Amsterdam in the South Indian Ocean, midway between the Cape of Good Hope and Australia. St. Paul is an interesting instance of a volcanic island, the extinct crater of which forms a wide sheltered harbour communicating with the sea by means of a single narrow channel. It was one of the French stations for observing the transit of Venus in 1874. French fishermen from Reunion had practically taken possession of the islands in the early part of the century, but the fishing-grounds have long been abandoned.

MR. B. V. DARBISHIRE, M.A. (Oxon.), has been appointed Cartographer to the Royal Geographical Society. He has had the advantage of preliminary training in Germany, and under the Reader in Geography at Oxford.

THE APPROACHING ECLIPSE OF THE SUN, APRIL 16, 1893.[1]

I HAD the honour, two and a half years ago, of describing to you the total eclipse of the sun of December 22, 1889, which I had been to observe in the Salut Isles, French Guiana. In spite of very unfavourable atmospheric conditions I was then

[1] Address to the Astronomical Society of France, on November 2, 1892, by M. De la Baume Pluvinel, translated by A. Taylor.

able to take some photographs of the phenomena and to measure the actinic intensity of the corona. Two years previously I had been to Russia to observe the eclipse of August 18, 1887, but the bad weather prevented any observations. If these expeditions did not succeed as well as I had hoped, they were at least useful in showing me all the difficulties to be met with in such undertakings, and of convincing me that if one wishes to thoroughly avail one's self of the precious moments during which the eclipse lasts, it is necessary to gain a large experience of these phenomena by omitting no opportunity of observing them, and by making a speciality of these expeditions. Therefore, after the eclipse of 1889 I determined to go to observe the following eclipse, that of the 16th of next April.

This time the phenomenon is visible under particularly favourable conditions. On the African coast to the south of Dakar, where the expedition sent by the Bureau des Longitudes will observe, and where I also propose to instal myself, the duration of totality is four minutes thirteen seconds. Moreover, a very important consideration is that we are certain of fine weather. At a time when expeditions are being organised in every country in view of this astronomical event, I think it will be useful to draw your attention to the chief questions which should be the object of astronomical study during the next eclipse.

You are aware that the passing of the moon before the sun has the inestimable advantage of allowing us to see the circumsolar regions which, under ordinary circumstances, because of their faint light are lost in the general illumination of our atmosphere. The regions thus revealed consist of a layer in immediate contact with the sun, the chromosphere, in which are the rosy flames which form the protuberances; and a more or less extensive luminous aureole, the corona. But since the celebrated eclipse of 1868, which marks an epoch in the history of solar physics, we are able, thanks to the great discovery of Messrs. Janssen and Lockyer, to study the protuberances at any time, and consequently it is only to the corona that the attention of astronomers turns during total eclipses.

An invariable part of the day's programme is the study of the structure of the corona, and the luminous intensity of its various parts. We need to have recourse to photography for trustworthy evidence as to this, for photography alone can give a faithful representation of the phenomena; even the best drawings always leave much to be desired. Indeed, it is impossible in the space of a few minutes to exactly represent a nebulous mass as complicated as the corona, and without any definite outlines. We can judge of the difficulty presented by the drawing of the corona, by remembering that even the most skilful draughtsmen have never yet been able to give us similar drawings of the great Orion nebula, although this may be studied at leisure. The brilliancy of the corona varies in intensity so much from one eclipse to another, that it is difficult to determine beforehand the length of exposure needed with given apparatus to obtain as satisfactory a representation of the phenomenon as possible. Moreover, the different parts of the corona differ in brilliancy, so that when the photographic action is sufficient to give a good image of the middle part, the lower portions which form the interior corona are over-exposed, while the extreme limits of the aureole are not reproduced. To satisfy all the conditions it is necessary to take several photographs with different exposures.

To advantageously discuss the results obtained it is very important that astronomers should place upon each plate a uniform scale to measure the intensity of the photographic action upon it. This intensity is equal to the product of three factors; the effectiveness of the object glass, the length of exposure, and the sensitiveness of the plate. If we indicate the useful diameter of the object glass by a and the focus by f, the effectiveness, defined by the international congress of photography, is $100\frac{a^2}{f^2}$. If we take plates of gelatinobromide of silver of normal sensitiveness as our unit, and let t be the length of exposure in seconds, we have the following formula to express the photographic action:—$100\frac{a^2}{f^2}t$. In working with wet collodion plates it is necessary to multiply this expression by $\frac{1}{50}$, and with plates of dry collodion it must be multiplied by $\frac{1}{100}$. The first photographs of the corona, taken with wet collodion, from 1868 to 1878, were obtained with a photographic action not greater than 2. Later, thanks to rapid plates, much greater action could be obtained. Thus in 1883 a photograph obtained by M. Janssen had received a photographic action equal to 918. On the negative thus obtained, the corona extended to between 30′ and 40′ from the limb of the moon, but on the other hand, details of the parts near the sun were completely wanting.

We might ask whether by still further increasing the photographic action we should also extend the limits of the corona? Certainly not! for if the photographic action is too intense, the faint contrast between the extreme parts of the corona and the more or less illuminated sky is no longer appreciable on the negative. We know, indeed, that if we wish to produce the maximum contrast between two half tones we must only use just enough light for the faintest of the half-tones to give a perceptible image. In America, Mr. Burnham has been engaged in determining the maximum length of exposure necessary to obtain the best representation of the corona, and he has made experiments on this subject by photographing the moon and white clouds on a faintly lighted sky.

In 1889, at the Salut Isles, I used five instruments, giving photographic actions varying from 185 to 13, but, doubtless on account of the peculiarly intense illumination of the atmosphere due to the short duration of totality, and the great abundance of water vapour in the atmosphere, the most satisfactory negative was that corresponding to a photographic action of 30. It is very probable that an equally good result might have been obtained with much less photographic action. Mr. Barnard, to whom we owe the best photographs of the eclipse of January 1, 1889, worked with a photographic action equal to 0·58. This result proves that with the sensitive plates now in use it should be possible to obtain good images of the corona on a large scale by using secondary magnifiers to increase the size of the image given by the object glass. In any case we can employ object glasses of two or three metres focal length, which would give images sufficiently large to show all the details of the corona without having resort to enlargement of the plates. Nevertheless, in spite of the use of a long focus, the instrument must remain luminous enough for the time of exposure to be short. The displacements of the image on the plate, caused by the imperfect adjustment of the equatorial mounting, or by an irregularity in the clockwork movement, or by the movement of the sun in declination are thus rendered inappreciable.

The photographs, when obtained, should be examined from the following different points of view. First of all we wish to find if the corona, which will be observed in the month of April, 1893, at a period of great solar activity, and at an epoch when the south pole of the sun is projected on the visible part of its disc, resembles, as we have every reason to think it does, the corona of 1883, which was studied under the same conditions. A great resemblance between the forms of the corona in 1889 and 1878, at the periods of minimum sun-spots, has already been noted, and if it can be established that the corona, seen under similar conditions, presents the same appearance, it will be proved that the diversity of appearance hitherto noticed depends solely upon the state of agitation of the solar surface, and the position of the observer with respect to the solar equator.

If the corona should present an axis of symmetry it must be ascertained whether the poles of this axis coincide with the poles of the axis of rotation of the sun; or if, as is more usually the case, the poles of the corona are inclined at some degrees from the poles of the sun, thus resembling the position of the magnetic poles of the earth with regard to its geographical poles. If the corona shares in the movement of rotation of the sun, it must be the same with its axis of symmetry, and therefore if we once observe the northern pole of the corona to the east of the northern pole of the sun, we ought to find it after an uneven number of half-revolutions, of the sun to the west of the north pole of the sun. To ascertain if this is so or not, it is of the greatest importance to know exactly the orientation of the images upon the photographic plates. The most exact and simple method to orient the images is to place the photographic apparatus in the position which it occupied at the moment of the phenomena, and, in the night, to photograph the trails which the stars leave in passing across the field of the lens.

If the photographs should show the structure of the corona clearly, we shall be able to study the form of those luminous rays which we notice at the poles of the sun, and of those curvilinear structures which seem to extend from the middle latitudes of the sun. The study of the curvature of these structures will be very useful in verifying the exactness of one of the most favoured theories of the corona, which explains the phenomena by supposing that matter is thrown out by the sun normally at its surface, and that its projection is turned on one side by the rotation of the sun. Mr. Schaeberle, of the Lick Observatory, has mathematically studied this theory, and on applying it to eclipses already observed, has been able to report that the curvature of the structures has always conformed with the theory.

We must also examine the photographs taken with the longest exposure, to determine whether the dark parts which sometimes separate the luminous structures, and which we can trace to the base of the corona, are entirely destitute of light. The existence of these *rifts*, as the English call them, is difficult to explain, if we suppose that the coronal atmosphere entirely surrounds the sun, for in that case we should always see, projected on the plane perpendicular to the line of sight, the coronal matter all round the sun. According to Prof. Hastings, the presence of these rifts is a confirmation of his theory which ascribes the corona to diffraction, and not to the existence of a material mass.

Is the aspect of the corona quickly modified, or are the changes which we notice from one eclipse to another effected slowly? Hitherto we have never proved the difference between the photographs of the same eclipse taken at several hours' interval, and at stations very distant from each other. The English astronomers thought of testing this question in December, 1889, and the two English expeditions sent, one to the Salut Isles, and the other to the west coast of Africa, were furnished with photographic outfits as identical as possible, in order to obtain, at an interval of two and a half hours, comparable negatives of the corona. Unfortunately the complete failure of the expedition on the African coast did not permit the carrying out of this programme; otherwise it is very probable they would have proved no sensible difference between the negatives at the two stations, for photographs show that the corona of December 22, 1889, was almost identical with that of January 1 of the same year. We may say, then, that during the year 1889, a year of quietude on the solar surface, the appearance of the corona did not appreciably change.

However, the experiment attempted by the English expeditions needs to be repeated; if not to study the internal movements of the corona, to obtain identical photographs at two different angles, which would enable us, with the aid of the stereoscope, to judge of the *relief* of the corona.

Photographs of a total eclipse will not only inform us as to the structure of the corona, but will permit us to measure its actinic brightness. We can estimate the relative intensity of different parts of the corona by superposing several photographs, made on the same scale, but obtained with very different photographic actions. The outlines of each image would give lines of equal actinic intensity of the corona. The absolute intensity may be measured by comparing the opacity of the image on the plate with the opacities produced on the same plate from a source of standard light. For this purpose the plates destined for photography are previously exposed on their borders to a standard light for varying periods of time. When these plates are developed, a scale of tones which allows a comparison of opacities is obtained at the same time as the image of the phenomena.

The spectroscopic examination of the corona confronts us with still more complex and more interesting problems. When we keep the slit of the spectroscope on the crescent of the sun, which narrows more and more in proportion as the moon advances, the spectrum darkens and the dark lines become less and less apparent; then all at once the field of sight is covered with an infinite number of bright lines, which seem to be substituted for each dark line of the Fraunhofer spectrum. This phenomenon only lasts two or three seconds. Such is the remarkable observation made by Prof. Young in 1870. In the preceding year he had tried to prove this transformation of dark into bright lines, but failed because he had arranged the slit of his spectroscope as a radius to the sun, which gave the bright lines too little length to be perceptible. With a tangential slit, however, the lines were long enough to be easily recognised.

Prof. Young's observations revealed to us the existence round the sun of a layer of incandescent vapours, of relatively low temperature, which, conformably to Kirchoff's theory, produce by their absorbing power reversal of the lines of the solar spectrum. It is very probable that the vapours to which the reversal is due are not all situated in the atmosphere which Prof. Young has revealed to us, and which has a thickness of only 1000 kilometres. If it were so, absorption must be infinitely more intense at the edge of the sun than it is at the centre. Nevertheless, the borders of the sun show no trace of this abnormal absorption. The observations of M. Janssen in 1867 showed this, and it is also proved by photographs of the spectrum of the sun which I took at the annular eclipse of 1890 at Canœ in the island of Crete.

It is probable that the reversal of lines is produced in a series of layers whose total thickness is great enough to make the difference of absorption between the centre and limb of the sun inappreciable. According to Prof. Lockyer the sun should be surrounded by concentric layers of vapours arranged in order of density, which, according to his own expression, envelope the sun like "the leaves of an onion." Prof. Young's layer of vapours would comprehend only some of these layers. This hypothesis seems confirmed by the observation made by M. Trépied in 1882: although he saw "a veritable rain of bright lines in the spectrum," he proved that the coincidence of the bright and dark lines was not complete.

Prof. Lockyer's theory involves also, as another consequence, the unequal length and width of these bright lines; indeed, the layer nearest to the sun should give short lines corresponding to the thickness of this layer, and as the temperature here must be very high the lines should be rather wide. The following layer being seen by projection, and having a thickness equal to the two layers, should give lines twice as long; moreover, this second layer being cooler than the preceding the lines should be narrower. The same reasoning applies to the succeeding layers, so that we ought to find, soon after the beginning of the total eclipse, short and wide lines, then long and narrow ones. The observations of 1882 confirmed these predictions, and English astronomers wished to repeat the experiment in 1886. Unfortunately the observations of Father Perry and Mr. Turner were made under conditions too unfavourable for us to draw any certain conclusion from them. To fully elucidate the question it is necessary to obtain instantaneous photographs of these bright lines. The experiment was indeed attempted by English observers in 1883, but they seem to have obtained no result. Prof. Lockyer proposes during the approaching eclipse to photograph these lines as well as the bright lines of the corona. He intends to use not only an analytic spectroscope, but a prismatic object glass. This apparatus will give the monochromatic images of the corona, that is to say, the kind of rings corresponding to each elementary radiation emitted by the coronal light.

When we turn the spectroscope towards the corona itself we observe a continuous spectrum, crossed by a bright green line which does not belong to any known element. This line, near the line E, corresponds to the division 1474 of Kirchoff's scale, and was at first believed to coincide exactly with a line of iron; but in 1876 Prof. Young was able to separate the line 1474 with powerful dispersion, and proved that one of its two components belongs to iron while the other belongs to the coronal matter. This line 1474 has been shown in every total eclipse, but its intensity has been very variable and seems always to have followed the fluctuations of solar activity. Thus in 1878, a period of maximum spots, the green line was so faintly visible that it escaped all observers except two. On the other hand, in 1882, when the solar activity was almost at its maximum, the green line was visible to within 40' from the limb. However, we must remember that these estimations made by different observers, observing with very dissimilar instruments, are scarcely comparable, and trustworthy evidence can only be obtained from photographs of the spectrum of the corona. The new plates sensitive to the green will no doubt allow the line 1474 to be photographed in the approaching eclipse.

It would be interesting to know whether the intensity of the green line varies with the brilliancy of the different parts of the corona, whether it is completely wanting in the rifts, whether it extends further than the visible corona, whether it has the same width in its whole extent, &c. These observations can only be made by associating with the spectroscope a telescope serving as a finder, in which cross wires have been arranged to indicate at each moment towards what part of the corona the slit is directed. The spectrum is observed with one eye while with the other the corona is examined. This is the arrangement which M. Janssen has always adopted in his spectrum observations of the corona.

If the terrestrial atmosphere is loaded with water vapour, we must expect a general diffusion of the coronal light, and this is doubtless the reason that on some occasions, as in 1870, the green line is seen beyond the corona—even upon the lunar disc.

Prof. Hastings, in 1883, examined simultaneously with a special arrangement the spectra of east and west portions of the corona, and proved, conformably to the theory that he propounds, that the green line varied in length during the duration of the eclipse, and that it always extended furthest on the most illuminated side of the edge of the moon. Mr. Keeler repeated the experiment in 1889, and also noted that the length of the green line depends upon the position of the sun with respect to the moon. The question would be worth studying further.

The green line is not the only bright line in the spectrum of the corona, the hydrogen lines have also been discovered in it, but these never extend further than about 10′ from the sun's limb. Other bright lines in the red and in the violet were observed by M. Tacchini and by Thallon in 1882. It was in 1882 also that Prof. Schuster obtained the first photograph of the coronal spectrum upon which some thirty bright lines may be counted.

In addition to the incandescent solid or liquid matter producing the spectrum of the corona, and the incandescent gases, which give rise to bright lines, there must also be in the circumsolar regions matter reflecting the light of the photosphere, as our own atmosphere does. This is proved by the polarisation of the light of the corona, and by the presence in its spectrum of the dark lines of the Fraunhofer spectrum. We owe the discovery of these dark lines to M. Janssen. In 1871 he observed only the lines D and *b*, but, since, in 1883, he has recognised some hundred dark lines, thus showing that the complete Fraunhofer spectrum is found in the coronal spectrum. These dark lines are necessarily very faint, for they are drowned in the continuous spectrum. As a rule the line D is most conspicuous, although, according to Prof. Hastings, if a faint solar spectrum is projected on to the continuous spectrum of a gas flame, it is not the line D, but rather the group *b*, which is by far the most apparent. Prof. Hastings concludes from this experiment that the continuous spectrum of the corona is richer in green than in orange radiation, since it causes the group *b* to disappear before the line D.

In conclusion I must quote a remarkable observation made by Prof. Tacchini in 1883, which, should it be confirmed, would suggest a very fascinating theory of the corona. Upon examining the spectrum of one of the sheaves (panaches) of the corona with a considerable dispersion and a wide slit, Prof. Tacchini thought he recognised two or three bright bands characteristic of the hydrocarbons, which are always present in the spectra of comets. Father Perry in 1886 proposed to verify the observation of Tacchini, but unfortunately could not re-observe the bands in question. Certainly he used a spectroscope with slightly illuminated cross wires, and when the period of great solar activity had already passed. It would be well in future eclipses to devote some seconds to the search for these bands, for, if the presence of carbon were recognised in the coronal atmosphere, it would be a new proof of the analogy which exists between the corona and cometary masses. Like comets the corona seems formed of matter subject to a repulsive force on the part of the sun, indeed it is probable that solar gravity does not act upon the corona, for unless this were so, the lower parts, having to support the weight of the upper, would be much more dense than the latter. It would thus result that the lines of the coronal spectrum, the line 1474 for instance, would be wider at their bases than at their upper extremities; but nothing of the kind has hitherto been observed. Moreover, so that the corona may be visible at 30′ or 40′ from the sun, the coronal matter must necessarily not be too rare in these extreme regions; but even in ascribing an extremely low density to this, we should find upon allowing for solar gravity that the pressure near the sun would have a considerable value, although it is proved that the pressure at the base of the corona does not exceed some millimetres of mercury.

It is also sought to prove the slight density of the middle corona by the fact that it has never offered any resistance to comets, which, on several occasions, have passed through it; but as comets themselves experience no appreciable resistance when they encounter a body it is impossible to tell whether the absence of resistance is due to the comets or to the corona.

The repulsive force which expels the coronal matter from the sun would act in the same manner as electrical force; indeed Prof. Bigelow has noticed that the arrangement of plumes and sheaves round the solar disc, and the ircurvilinear forms exactly recall the lines of force of an electric field. Let us complete the parallel between comets and the corona by noting that the tails of comets sometimes assume the curvilinear form found in the sheaves of the corona. The dark parts which divide the tails of comets have also their analogues in the rifts of the corona. To push the comparison still further, it would be very interesting to be able to prove that the corona, like cometary masses, is transparent, and that bright stars can be seen through it. Unfortunately it will be impossible to attempt this experiment at the time of the next eclipse.

An exact photometric study of the solar surface would perhaps detect the transparency of the corona, indeed if we suppose that the corona presents a certain opacity the parts of the photosphere on which the large sheaves are projected must be less luminous than the parts covered by the polar rays.

If the corona is not subject to solar gravity it is scarcely probable that it shares the movement of rotation of the sun; however, it would be useful to try in the coming eclipse to study the question by the spectroscopic method, as M. Trouvelot wished to do in 1883. It would be desirable to conduct all spectroscopic observations of the corona by means of photography. The instruments which must be used for this purpose should be very luminous (*i.e.* give bright images), for there is little light available, and the exposures are necessarily short. In studying the effectiveness of a spectroscope in the case of an object presenting a large apparent diameter, like the corona, it is seen that the intensity of the spectrum depends entirely upon the width of the slit, and the effectiveness of the object glass which forms the image of the spectrum. As to the collimator and the condenser their dimensions are of no importance, provided that the collimator can well receive all the light of the condenser. As the object glass which forms the image of the spectrum must have an image long enough to give sufficient length to the spectrum, one is led, in order to obtain great effectiveness, to give this object glass a large aperture, and consequently to use a prism of large size.

The visibility of the bright lines depending not only on their brightness, but also on their width, a wide slit must be employed to obtain a good image of these lines; on the other hand, a narrow slit will give a spectrum of great purity, and will show the dark lines. The employment of two different spectroscopes is then plainly indicated.

It remains for us to speak of the photometric measuring of the corona by optical photometers. Bunsen's photometer has already been used for this purpose, but I think that we must henceforth turn to photography to obtain exact results. The question should not be neglected, for it is certain that the brilliancy of the corona varies considerably from one eclipse to another. Thus Prof. Lockyer estimates that in 1878, at a period of quiescence on the surface of the sun, the corona was ten times less brilliant that in 1871.

Let us end by pointing to the polariscope observations which hitherto have been far from giving concordant results as to the proportion of polarised light in the various parts of the corona. Here also there are new inquiries to be made.

Such, gentlemen, are the different problems suggested by the study of the solar corona. We will hope that the next eclipse will largely contribute to their solution.

MEMORIAL OF SIR RICHARD OWEN.

A MEETING was held at the rooms of the Royal Society, on Saturday, to make preparations for the provision of a suitable memorial of the late Sir Richard Owen. The Prince of Wales took the chair, and was supported by the Duke of Teck, the President, the Treasurer, and the Secretary of the Royal Society, Lord Kelvin, Sir John Evans, and Professor Michael Foster; the President of the British Association, Sir A. Geikie; the President of the Royal College of Physicians, Sir A. Clark; the President of the Royal College of Surgeons, Mr. T. Bryant; the President of the Royal Academy, Sir F. Leighton; the Bishop of Rochester, the Dean of Westminster, Lord Playfair, Prof. Huxley, Sir H. Roscoe, M.P., Sir F. Abel, Sir F. Bramwell, Sir G. Stokes, Sir H. Acland, Sir Joseph Lister, Mr. Ericsen, Dr. Priestley, Dr. Günther, Dr. H. Woodward, Dr. Maunde Thompson, Sir W. H. Flower, Sir Erasmus Ommanney, Sir James Paget, Sir Henry Thompson, Sir Spencer Wells, Sir Edwin Saunders, Sir John Fowler, Dr. E. A. Bond, Dr. P. L. Sclater, Mr. Carruthers, and Mr. W. P. Sladen. There were also present, among others, Sir G. M. Humphry, Mr. Holman Hunt, Mr. Ernest Hart, Dr. Michael (President of the Royal Microscopical Society), Prof. R. Meldola, Mr. O. Salvin, and Prof. T. Wiltshire.

The Prince of Wales, in opening the proceedings, said,—I have the great privilege conferred upon me of being asked to take the chair to-day, upon this very special occasion. We are assembled together for the purpose of paying a mark and tribute of respect and appreciation to the memory of a great man of science who has lately passed away from us. The name of Sir Richard Owen must always go down to posterity as that of a great man—one who was eminent in the sciences of anatomy, zoology, and palæontology. Perhaps I may be allowed to say a word of my own personal knowledge of him. It is now thirty-five years since I had the advantage of knowing him. When I lived as a boy at the White Lodge, Richmond Park, now occupied by my illustrious relative on my right (the Duke of Teck), I had opportunities of visiting him and knowing him. His geniality and his charm of manner to all those who knew him have, I am sure, left a deep and lasting impression. Whether he was explaining to you the mysteries of some old fossil bone that had been given him, or whether he was telling one of his vivid ghost stories, one felt that one was under the charm of his presence. His method of teaching, as you all know, was earnest and clear in every respect; and it even derived a measure of force from a certain hesitation in his manner. His great repute was gained as a zoologist, and in the study, not only of living animals, but of those long extinct, and following the same large range of work as Cuvier, to whom, in the history of science, he may be regarded as a successor. One of the great works and interests of his life was the formation of the Natural History Museum, which is now safely established in South Kensington under the able guidance of our friend Sir William Flower. It may be within your recollection what great difficulties Sir Richard Owen encountered when he was first appointed Superintendent of the Department of Natural History at the British Museum in 1856. He himself saw in getting that appointment that it was quite impossible that these large collections could be adequately seen unless they were removed to some other sphere. In 1862 a Bill was brought in by Mr. Gladstone, who took the greatest interest in the matter, while it was vigorously opposed, strange to say, by no less great a man than Mr. Disraeli. The Bill was lost, though it was eventually, ten years later, carried, and now we have that fine building that we all know and deeply appreciate. I may also mention that he took the greatest interest with regard to the colonies, and in trying to obtain from them specimens that would be worthily represented in the Natural History Museum. In sanitary matters also he was not behindhand, as was shown by his long intimacy with that distinguished man, Sir Edwin Chadwick. There are several resolutions to be proposed, and you will hear far better and more eloquent remarks from the distinguished gentlemen who will move and second them. That is the reason why on this occasion I shall not trouble you with more remarks. Allow me only to repeat the assurance of the deep interest I take in this movement for a suitable memorial to the memory of this great man, and how deeply I appreciate having been asked to take the chair on this interesting and important occasion.

Lord Kelvin moved :—"That it is desirable that the eminent services of the late Sir Richard Owen in the advancement of the knowledge of the sciences of anatomy, zoology, and palæontology should be commemorated by some suitable memorial." He said that, if there was no other reason but the part that Sir R. Owen took in the establishment of the Natural History Museum, and the success that ultimately attended his efforts, he deserved the gratitude of the nation. There was scarcely any branch of the whole of natural history that he had not touched and enriched with the results of his investigations. Three hundred and sixty papers, every one of them valuable, were to be found under his name in the Royal Society catalogue of scientific papers. From these contributions, however, he came back to the Natural History Museum, and he held that every subject of the Queen, in these islands or in the colonies, and every visitor to this country, must feel that he was benefited by the existence of that museum and by the splendid arrangement of its contents.

Prof. Huxley, in seconding the resolution, said that, if he mistook not, there were very few men living who had had occasion to follow the work of the remarkable man whose career they had met to celebrate with more carefulness and attention than he had done. It was a career remarkable for its length, for the rapid rise to eminence, and the long retention of high position of the person who was the subject of it. It was more than forty years ago since he, as a young man, had occasion to look abroad upon the scientific world of London, in which he was then a complete novice, and to see whether, perhaps, in some small and insignificant corner of it room might be found for him. At that time there were four persons whose names stood out amongst the first in the galaxy of scientific men of this country. They were Sir John Herschel, Mr. Faraday, Sir Charles Lyell, and, last, though by no means least, the famous Hunterian Professor, Owen. If he looked abroad amongst the lights of biological science, with which he was principally concerned, there were Johannes Müller in Berlin, Milne Edwards in Paris, Von Baer in St. Petersburg; but for quantity, general excellence, and variety of work there was no one who could be regarded as the superior of Owen. It was a common impression that Owen was the successor and continuator of Cuvier, and that was largely true. The memoirs on the pearly nautilus, on the marsupials, on the anthropoid apes were fully worthy of the author of the "Memoires sur les Mollusques" or the "Leçons d'Anatomie Comparée," while the "Ossemen fossies" had a full equivalent in the vast series of papers upon fossil remains, contained in the publications of the Royal, the Geological, and the Palæontographical Societies. But it was also to be remembered that, in another field, Owen was the successor and continuator of the school to which Cuvier was most vehemently opposed—that of St. Hilaire and Oken. The remarkable contributions to morphology embodied in the works on the archetype of the vertebrate skeleton and on the nature of limbs were able developments of speculative views of another order than Cuvier's. Readers of Goethe would remember that he thought the news of the controversy between Cuvier and St. Hilaire far more interesting than that of the Revolution of July, which broke out about the same time. Whether that was a just estimate of the relative importance of things or not might be left an open question; but it was the peculiar irony of history to show us in so many quarrels that right and wrong were on both sides. And in this particular controversy it had turned out that the right lay neither with Cuvier nor with St. Hilaire, but partly with both and partly with a third party, which at that time hardly existed. Whatever might be the ultimate verdict of science in this particular matter, there could be no doubt that it was a distinct aid to progress to have one view of the case stated and illustrated with the unrivalled wealth of knowledge which Owen brought to bear upon it. If history confirmed, as he believed it would, the estimate of the broad features of Sir Richard Owen's work, which he had suggested, then it would justify them in endeavouring to preserve the memory of the great results achieved by his stupendous powers of work, his remarkable sagacity in interpretation, and his untiring striving towards the ideal which he entertained.

The resolution was then put and agreed to unanimously, as were also those which followed.

The Duke of Teck moved :—"That the memorial shall consist primarily of a marble statue which shall be offered to the Trustees of the British Museum to be placed in the hall of the Natural History Museum." His Royal Highness said,—There is no doubt, in my mind at least, that this would be the most appropriate place and the most appropriate form in which to erect the likeness of our admired friend. It is, so to say, his second home, the home of his later labours, and no better place could be found. Besides, I think it is a very nice idea that every one who enters the hall should see first of all the man to whom we owe this inheritance. Others have said so much about Sir Richard Owen that it is needless for me to go over the ground again. As all of us know so well, what he has been and what he has done will remain in the minds of all who survive him, and, therefore, I will only say that in my opinion the hall, which is a very fine interior, of the Natural History Museum should be the place where the memorial of this great man should be erected.

Sir William Flower, in seconding the resolution, said that having twice in his life succeeded Sir Richard Owen, he had had special opportunities of judging of his work, and he might, therefore, be expected to say something about the general character and extent of that work on the present occasion, but after what had been said in the introductory remarks of His Royal Highness, and the speech of Prof. Huxley, than whom no one was more competent to give an opinion upon the scientific side of the question, there was no necessity for doing so. He could not refrain, however, from speaking upon one point. Among the various characteristics of Sir Richard Owen, one of the most remarkable was his untiring industry, which enabled him to produce an amount of work which was truly prodigious. It could hardly be expected that such a vast series of memoirs on so many diverse subjects, as that which he had given forth to the world during his long life, could all be equal in quality, or that the merits of some of them should not have been the occasion of controversy. He would only refer to one instance of this kind. As long ago as 1837, Sir R. Owen read a paper before the Society in whose rooms they were now assembled, which was published in the Philosophical Transactions, and in which certain remarkable characteristics were stated to exist in the brain of marsupial animals, widely distinguishing them from other members of the class to which they belong. The conclusions apparently established by this paper were generally accepted for nearly thirty years, but in 1865 another memoir was read before the same society, and also published in the Philosophical Transactions, in which a different view was taken both of the nature of the structural peculiarities and of their significance in classification. The views of the author of this second paper have generally found favour until within a few months since, when an independent investigation of the subject, carried on with all the improved methods of modern research, by Dr. J. Symington, has resulted in a declaration in favour of the accuracy of Owen's original description and conclusions. These observations may still require confirmation by others, but as he (Sir W. Flower) was the author of the second paper, he considered it only fitting that he should, at a meeting assembled to do honour to the memory of the great anatomist, from whom, on this point, he had differed so long, call attention to them. He thought this the best contribution he could make to the object for which they had gathered together.

Dr. P. L. Sclater suggested that, in addition, a memorial catalogue of the late professor's writings should be issued, with a portrait and biographical memoir.

Sir James Paget moved that a committee be formed to carry out the preceding resolutions. It would be impossible, he said, to have any better evidence that the resolutions just passed were right than the number and position of those who had offered to serve on the committee, for there was never a more representative list. Headed by the Prince of Wales, the Duke of Teck, the Archbishop of Canterbury, and the Lord Chancellor, it contained nearly 150 of the most prominent workers in all branches of science and many who were the best judges of the influence of science on the general well-being of the nation. He was the oldest person present who had worked with Sir R. Owen, and could remember him on entering St. Bartholomew's Hospital as a student in 1834. He could testify to the influence Owen had exercised in promoting the study of science by showing to all around him how keen his delight was in it, and how in itself alone it might be a sufficient reward. He resisted all temptations to leave science, though he might have been a very successful medical practitioner; and he was one of the first by whom the real reform of sanitary matters was begun in this country.

Sir J. Evans briefly seconded the motion.

Sir A. Clark moved—"That the following list of gentlemen constitute the executive committee: His Royal Highness the Prince of Wales (chairman), His Serene Highness the Duke of Teck, the President of the Royal Society, the President of the Royal College of Physicians, the President of the Royal College of Surgeons, the President of the Linnæan Society, the President of the Zoological Society (treasurer), Sir John Evans, Prof. Michael Foster, Dr. A. Günther, Prof. Huxley, Sir F. Leighton, Sir James Paget, Dr. P. L. Sclater, Mr. W. Percy Sladen (secretary), Lord Walsingham, Mr. A. Waterhouse, R.A., and Mr. Henry Woodward." Sir Andrew remarked that this memorial movement reminded them that nations no more than individuals can live by bread alone. Material prosperity did not constitute the true abiding life of a nation; it was necessary that it should live by ideas: and the nation honoured those who, like Owen, communicated new ideas which spurred others to new courses of activity.

Mr. T. Bryant, in seconding the motion, said the College of Surgeons felt the loss that science had sustained in the death of him who unquestionably was the grand expounder of John Hunter and who, more than any one else, demonstrated the value of the materials John Hunter left behind him. He did more than any one else to call the attention of the scientific world to the museum in Lincoln's Inn, and by additions to it to make it what it is. More than that, at a time when comparative anatomy and biological studies were little thought of he called attention to the value of them, the necessity for them, and the pleasures they would yield. As a young man he attended Owen's lectures, and felt the full force of his quiet enthusiasm, which was altogether independent of the materials embodied in the lectures.

Lord Playfair, in supporting the motion, said that he was the last surviving member of the Health of Towns Commission of 1844, upon which he was brought into continual intercourse with Sir R. Owen, and therefore he knew how much Sir Richard had at heart the advancement of sanitary science. This interest in it he maintained throughout his whole career. He lived close to Sir Edwin Chadwick, and although no two men could be more unlike, they were most intimate friends, and were constantly discussing how to advance the health of the nation. When Sir Richard returned from his interesting expedition to Egypt he told the speaker that he had come back in an unforgiving spirit towards Moses, because though skilled in the learning of the Egyptians, and having derived his chief commandments from those of that ancient race, he missed one important one, "Thou shalt not pollute rivers." Owen, like Prof. Huxley, exercised great influence outside the domain of science. Prof. Huxley had benefited the education of the country, and Prof. Owen had considerable influence in improving the sanitary condition of the country.

Sir W. Flower read a first list of donations, headed with one of £25 by the Prince of Wales.

Sir Henry Acland moved, and Prof. Michael Foster seconded, a vote of thanks to his Royal Highness for consenting to become chairman of the committee, and for presiding on the present occasion.

The Prince of Wales, in responding, said,—I beg to return my warmest thanks to my kind and valued old friend, Sir Henry Acland, for the way he has proposed, to Mr. Michael Foster for the way in which he seconded, and to you all for the kind manner in which you have received this resolution. It has indeed been a labour of love to me to-day to preside on this very interesting occasion, and I think that it has seldom been my good fortune to listen to more interesting or eloquent addresses than those which have fallen from the lips of those eminent gentlemen who have spoken. Nobody will take a deeper interest in the carrying out of this memorial of our lamented friend Sir Richard Owen than myself, and most sincerely do I hope that the great work that is to adorn the Natural History Museum will be worthy of a great sculptor and of the great man that it represents.

SCIENTIFIC SERIALS.

Bulletin de l'Académie Royale de Belgique, Nos. 9 and 10. Classe des Sciences.—On some new *Caligidei* of the coast of Africa and the Azores Archipelago, by P. J. van Beneden.—On an optical atmospheric phenomenon observed in the Alps, by F. Folie (see Notes).—On a state of matter characterised by the mutual independence of the pressure and the specific volume, by P. de Heen. It is easily shown that the density of saturated vapour at the critical temperature is variable, and depends, at constant pressure, upon the proportion of liquid enclosed in the tube. Experiments were made in order to decide whether this independence of pressure and volume was shown also at other temperatures. The liquid chosen was ether, and the volume of liquid and vapour contained in a sealed tube was read by means of a cathetometer. A series of results showed that during condensation by pressure the density of unsaturated vapour was greater than that of saturated vapour, or that the specific volume increased with the pressure. This is an experimental verification of Prof. James Thomson's pseudo-gaseous state of matter.—On the most complete reduction of invariant functions, by Jacques Deruyts.—Ex-meridian observations made at the Royal Observatory of Belgium from March to October, 1892, by L. Niesten and E. Stuyvaert.—On a new fluorine-derivative of carbon, by Frédéric Swarts. This is a liquid, of the formula CCl_3F, boiling at 24°·7, insoluble in water, and unaffected by sulphuric and nitric acids. Its density is 1·4944; an alcoholic solution of potash destroys it gradually, forming potassium chloride, fluoride, and carbonate. It was obtained by treating carbon tetrachloride with a mixture of antimony trifluoride and bromine in equal molecular proportions. It is notable that the bromofluoride produced by the mixture acts not as a bromising but a fluorising agent.—On a simplification of some of Tesla's experiments, by H. Schoentjes. Like some recent workers in England, Prof. Schoentjes has found that most of the experiments can be produced, although with lesser intensity, without the bobbin immersed in oil, the discharge exciter, and the condenser, simply by the first Rhumkorff coil, whose dimensions need not exceed 7 × 17 cm.—On a process of sterilisation of albumin solutions at 100° C., by Émile Marchal. Albumin can be easily sterilised at 100° C., without coagulation, by first adding 0·05 gr. per litre of borax, or 0·005 of ferrous sulphate in a 2 to 5 per cent. solution, or 4 to 5 gr. nitrate of urea per litre of 10 per cent. solution. The "incoagulable albumin" thus obtained is perfectly suitable for cultivations.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, November 24, 1892.—"Memoir on the Theory of the Compositions of Numbers," by P. A. MacMahon, Major R.A., F.R.S.

In the theory of the partitions of numbers the order of occurrence of the parts is immaterial. Compositions of numbers are merely partitions in which the order of the parts is essential. In the nomenclature I have followed H. J. S. Smith and J. W. L. Glaisher. What are called "unipartite" numbers are such as may be taken to enumerate undistinguished objects. "Multipartite" numbers enumerate objects which are distinguished from one another to any given extent; and the objects are appropriately enumerated by an ordered assemblage of integers, each integer being a unipartite number which specifies the number of objects of a particular kind; and such assemblage constitutes a multipartite number. The 1st Section treats of the compositions of unipartite numbers both analytically and graphically. The subject is of great simplicity, and is only given as a suitable introduction to the more difficult theory, connected with multipartite numbers, which is developed in the succeeding sections.

The investigation arose in an interesting manner. In the theory of the partitions of integers, certain partitions came under view which may be defined as possessing the property of involving a partition of every lower integer in a unique manner. These have been termed "perfect partitions," and it was curious that their enumeration proved to be identical with that of certain expressions which were obviously "compositions" of multipartite numbers.

The generating function which enumerates the composition has the equivalent forms—

$$\frac{h_1 + h_2 + h_3 + \dots}{1 - h_1 - h_2 - h_3 - \dots},$$

$$\frac{a_1 - a_2 + a_3 - \dots}{1 - 2(a_1 - a_2 - a_3 - \dots)},$$

where h_s, a_s represent respectively the sum of the homogeneous products of order s and the sum of the products s together of quantities

$$a_1, a_2, a_3, \dots, a_n,$$

and the number of compositions of the multipartite

$$\overline{p_1 p_2 \dots p_n}$$

is the coefficient of $a_1^{p_1} a_2^{p_2} \dots a_n^{p_n}$ in the development according to ascending powers.

It is established that

$$\frac{1}{2}\frac{1}{\{1 - s_1(2a_1 + a_2 + \dots + a_n)\}\{1 - s_2(2a_1 + 2a_2 + \dots + a_n)\} \dots \{1 - s_n(2a_1 + 2a_2 + \dots + 2a_n)\}}$$

is also a generating function which enumerates the compositions; the coefficient of

$$s_1^{p_1} s_2^{p_2} \dots s_n^{p_n} a_1^{p_1} a_2^{p_2} \dots a_n^{p_n},$$

being the number of compositions possessed by the multipartite

$$\overline{p_1 p_n \dots p_n}.$$

The previous generating function may, by the addition of the fraction $\frac{1}{2}$ and the substitution of $s_1 a_1$, $s_2 a_2$, &c., for a_1, a_2, &c., be thrown into the form

$$\frac{1}{2}\frac{1}{1 - 2(\Sigma s_1 a_1 - \Sigma s_1 s_2 a_1 a_2 + \dots (-)^{n+1} s_1 s_2 \dots s_n a_1 a_2 \dots a_n)},$$

and hence these two fractions, in regard to the terms in their expansions which are products of powers of $s_1 a_1$, $s_2 a_2$, …, $s_n a_n$, must be identical. This fact is proved by means of the identity—

$$\frac{1}{2}\frac{1}{\{1 - s_1(2a_1 + a_2 + \dots + a_n)\}\{1 - s_2(2a_1 + 2a_2 + \dots + a^n)\} \dots \{1 - s_n(2a_1 + 2a_2 + \dots + 2a_n)\}}$$

$$= \frac{1}{2}\frac{1}{1 - 2(\Sigma s_1 a_1 - \Sigma s_1 s_2 a_1 a_2 + \dots (-)^{n+1} s_1 s_2 \dots s_n a_1 a_2 \dots a_n)}$$

multiplied by

$$1 + \Sigma \frac{2(A_{\kappa_1} + a_{\kappa_1}) \dots (A_{\kappa t} + a_{\kappa t}) - (A_{\kappa_1} + 2a_{\kappa_1}) \dots (A_{\kappa t} + 2a_{\kappa t})}{(1 - S_{\kappa_1}) \dots \dots (1 - S_{\kappa t})} s_{\kappa_1} s_{\kappa_2} \dots s_{\kappa n},$$

where

$$S_\kappa = s_\kappa(2a_1 + \dots + 2a_\kappa + a_{\kappa+1} + \dots + a_n) = s_\kappa(A_\kappa + 2a_\kappa),$$

and the summation is in regard to every selection of t integers from the series

$$1, 2, 3, \dots n,$$

and t takes all values from 1 to $n - 1$.

This remarkable theorem leads to a crowd of results which are interesting in the theory of numbers.

The geometrical method of "trees" finds a place, and, lastly, there is the fundamental algebraic identity—

$$\frac{1}{k}\frac{1}{\{1 - s_1(ka_1 + a_2 + \dots + a_n)\}\{1 - s_2(ka_1 + ka_2 + \dots a_n)\} \dots \{1 - s_n(ka_1 + ka_2 + \dots + ka_n)\}}$$

$$= \frac{1}{k}\frac{1}{1 - k\Sigma s_1 a_1 + k(k-1)\Sigma s_1 s_2 a_1 a_2 - \dots + (-)^n k(k-1)^{n-1} s_1 s_2 \dots s_n a_1 a_2 \dots a_n}$$

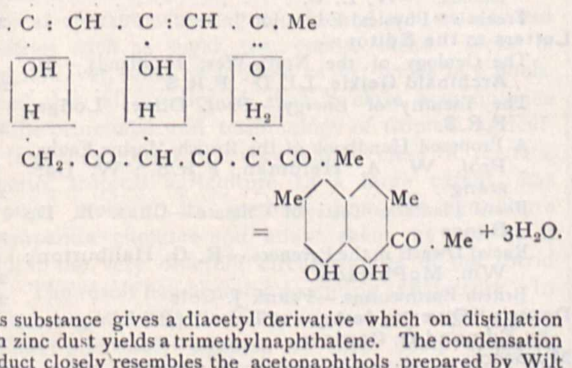
multiplied by

$$1 + \Sigma \frac{k(A_{t_1} + a_{t_1}) \dots (A_{t_n} + a_{t_n}) - (A_{t_1} + ka_{t_1}) \dots (A_{t_n} + ka_{t_n})}{(k-1)(1 - S_{t_1})(1 - S_{t_2}) \dots (1 - S_{t_n})} s_{t_1} s_{t_2} \dots s_{t_n},$$

which reduces to that formerly obtained when k is given the special value 2.

Chemical Society, December 13.—Mr. W. Crookes, Vice-President, in the chair.—The Stas Memorial Lecture, by J. W. Mallet, was read (see this vol. p. 248).

December 15.—Dr. W. J. Russell, Vice-President, in the chair.—The following papers were read:—The identity of caffeine and theine and the interactions of caffeine and auric chloride, by W. R. Dunstan and W. F. J. Shepheard. Various physiologists have concluded that differences exist between theine from tea and caffeine from coffee; the authors have compared the products from the two sources, and consider that their identity is beyond question. The differences in physiological action observed by Mays, Brunton, and Cash can only mean that the alkaloids employed were either impure or administered under non-comparable conditions. On heating an aqueous solution of caffeine aurichloride, a yellow precipitate of aurochlorocaffeine $C_8H_9(AuCl_2)N_4O_2$ separates; the production of this substance is better explained by Medicus' formula for caffeine than by that of E. Fischer.—Studies on isomeric change, ii. Orthoxylenesulphonic acids, by G. T. Moody. 1 : 2 : 3-orthoxylenesulphonic acid, when heated at 115–120° in a current of dry air, undergoes quantitative conversion into the isomeric 1 : 2 : 4-sulphonic acid. The former acid is prepared by sulphonating dibromorthoxylene and reducing the resulting dibromorthoxylenesulphonic acid with zinc dust and sodium hydroxide. A number of derivatives are described.—Studies on isomeric change, iii. Phenetoilsulphonic acids, $C_6H_4(OEt)SO_3H$, by G. T. Moody. Bromophenetoilsulphonic acid, prepared by ethylating parabromophenol and sulphonating the bromophenetoil so obtained, is readily reduced by zinc dust and sodium hydroxide with formation of orthophenetoilsulphonic acid. The latter is completely converted into the isomeric parasulphonic acid on heating for several hours at 100°. Lagai's observations, contradicting the author's previous results, are shown to be erroneous.—Formation and nitration of phenyldiazoimide, by W. A. Tilden and J. H. Millar. Phenyldiazoimide, $N_3.Ph$, is readily obtained by the interaction of nitrosyl chloride and phenylhydrazine in glacial acetic acid solution; on nitration it yields about two-thirds of its weight of the paranitro-derivative (m.p. 74°). Nitrophenyldiazoimide is a convenient source from which to prepare diazoimide.—The production of naphthalene derivatives from dehydracetic acid, by J. N. Collie. The author concludes that the yellow substance which he has previously obtained by the condensation of diacetylacetone (see this vol. p. 238) is probably formed in accordance with the following equation:—



This substance gives a diacetyl derivative which on distillation with zinc dust yields a trimethylnaphthalene. The condensation product closely resembles the acetonaphthols prepared by Wilt and Erdmann.—A new synthesis of hydrindone, by F. S. Kipping. Contrary to the statement of Hughes, hydrindone may be easily prepared in large quantities by the action of aluminium chloride on phenylpropionic chloride; 50–60 per cent. of the theoretical yield is obtained, the reaction being represented by the following equation: $Ph.CH_2.CH_2.COCl = Ph\left\langle{}^{CH_2}_{CO}\right\rangle CH_2 + HCl$. The ketone prepared in this way is identical with that obtained from other sources by several chemists; its hydrazone, hydroxime and a nitro-derivative are described. On heating hydrindone with moderately concentrated sulphuric acid a condensation product, $C_{18}H_{14}O$, is obtained; it forms yellowish plates melting at 141·5–142·5°. Phosphoric anhydride converts hydrindone into a yellow crystalline substance, which is apparently identical with the hydrocarbon of the empirical composition C_9H_6, which the author has previously obtained by the action of phosphoric anhydride on phenylpropionic chloride.—The resolution of methoxysuccinic acid into its optically active components, by T. Purdie and W. Marshall. Synthetical methoxysuccinic acid can be resolved into its optically active constituents by crystallisation of the acid cinchonine salts, the salt of the dextro-acid being less soluble in water than that of its lævo-isomeride. The separation effected in this way is, however, only partial, the metallic salts obtained after removal of the alkaloid being a mixture of the active and inactive compounds; by taking advantage of the fact that the inactive calcium or acid potassium salt is less soluble in water than its active isomeride, the optically active acids may be isolated. The active acids have a specific rotatory power of about 33° in 5–10 per cent. aqueous solutions and melt at 88–90°, whilst the inactive acid melts at 108°. The rotation of the normal ammonium or potassium salt is of the same sign as that of the parent acid; the rotation of the calcium or barium salt is of opposite sign to that of the acid, but varies greatly with change of concentration. The rotation of the barium salt changes sign in very dilute solutions.—Optically active ethoxysuccinic acid, by T. Purdie and I. W. Walker. If fed with nutritive mineral salts the spores of *Penicillium glaucum* flourish in a solution of inactive hydrogen ammonium ethoxysuccinate and consume the lævogyrate acid, leaving the dextro-acid unaltered. On crystallising the cinchonidine salt of the inactive acid, a separation into the lævo- and dextro-modifications may be effected, and the oppositely active acid ammonium salts prepared in this way resemble that obtained by means of *Penicillium glaucum*. Close parallelism exists between the methoxy- and ethoxy-succinates, with respect to optical activity.—The formation of benzyldihydroxypyridine from benzylglutaconic acid, by S. Ruhemann. Ethyl benzylglutaconate slowly dissolves at 100° in concentrated aqueous ammonia, yielding a solution, from which acids separate benzyldihydroxypyridine. This substance exhibits both basic and acid properties and melts at 184°.—The action of nitrous acid on 1-α-amido-2-β-naphthol; a correction, by R. Meldola. The author agrees with the statement of Grandmougin and Michel that β-naphthaquinone results from the interaction of nitrous acid and 1-α amido-2-β-naphthol.—Note on the action of phenylhydrazine on mono- and di-carboxylic acids at elevated temperatures, by W. R. Hodgkinson and A. H. Coote. On distilling a mixture of phenylhydrazine and phenylacetic acid in equivalent proportion, benzene, aniline and a liquid of the composition $C_{14}H_{12}O_2$ distil over; nitrogen and ammonia are also evolved. As has been previously shown, the hydrazide of the composition $Ph.CH_2.CO.NH.NH.Ph$ is the first product of the reaction; on distilling this substance, NH . NH is split off, and reduces the phenylhydrazine present to aniline and benzene. Somewhat similar reactions occur in the cases of orthotoluic, phenylpropionic, and succinic acids, and are now under investigation.

SYDNEY.

Royal Society of New South Wales, September 7, 1892.—Prof. Warren, President, in the chair.—Paper read: The effect which settlement in Australia has produced upon the indigenous vegetation, by A. G. Hamilton [Part I.].

October 5.—Prof. Warren, President, in the chair.—The second part of paper on the effect which settlement in Australia has produced upon indigenous vegetation, by A. G. Hamilton, was read, after which the society's bronze medal and a cheque for £25 were presented to the author.

November 2.—Prof. Warren, President, in the chair.—Dr. William Huggins, F.R.S., was elected an honorary member of the Society. The following papers were read:—Preliminary note on limestone occurring near Sydney, by H. G. Smith.—On a cyclonic storm near Narrabri, by H. C. Russell, F.R.S.—Some folk-songs and myths from Samoa, translated by the Rev. G. Pratt, with introduction and notes by Dr. John Fraser.

PARIS.

Academy of Sciences, January 16.—M. de Lacaze-Duthiers in the chair.—Swimming movements of the ray-fish, by M. Marey. These were investigated by means of chronophotography, ten exposures being made per second. The fish was fixed in position by the head and tail, and the views were taken from the front and the side respectively, the fins being left free to move. The photographs show the successive phases of one entire motion of the fins, which consists of a wave-like motion beginning in front. Shortly after the anterior portion has been lifted it is depressed, the motion being meanwhile propagated to the lateral portions, and growing in amplitude as the fin grows in breadth. Just before the movement dies out near the tail the process recommences in front. The periodic time was 0·8 seconds. The photographs show a striking likeness to those obtained by chronophotography applied to the flight of birds. M. Marey intends to study the mechanical effect of the action of the fins upon the water, also by the aid of photography.—Microscopic researches on the contractility of the blood-vessels, by M. L. Ranvier. The pericœsophagian membrane of the frog was placed on the disc of the slide-cell in one or two drops of peritoneal serum. It was kept extended by a platinum ring; electrodes of tinfoil were placed in connection, and a cover glass was fixed over the whole with paraffin. Thus mounted, the smooth muscular fibres and the internal elastic sheath are well seen. On connecting the induction coil with the electrodes, the muscular fibres contract as soon as the current is strong enough. At the same time, the folds of the internal sheath become more pronounced and finally touch, thus effacing the passage through the small artery. On breaking the current, the artery gradually regains its original diameter. If the current is not sufficiently strong for producing a regular contraction, some of the segments contract, while others are at rest. But the zone of contraction is never displaced, and, if interrupted, will reappear at the same place on reestablishing the current. Nothing corresponding to a peristaltic motion can be produced by direct electrical excitement. In none of the experiments, even with the strongest currents, was it possible to detect any signs of contraction in the capillaries.—On the sum of the logarithms of the first numbers not exceeding x, by M. Cahen.—On differential equations of a higher order, the integral of which only admits of a finite number of determinations, by M. Paul Painlevé.—On linear differential equations with rational coefficients, by M. Helge von Koch.—Electric waves in wires; depression of the wave propagated in conductors, by M. Birkeland (see *Wiedemann's Annalen*, abstract).—On the minimum perceptible amount of light, by M. Charles Henry. This was estimated by Aubert at $\frac{1}{300}$th of the light of the full moon. This is about a thousand times too great, as proved by some measurements made with the zinc-sulphide (phosphorescence) photometer previously described. The corrected formula for the rate of loss of luminosity of the sulphide is $i^{0.5}(t-18{\cdot}5)=1777{\cdot}8$, which agrees even with the longest observations, and is theoretically justified by M. Henri Becquerel. The minimum perceptible amount of light was determined by noticing the time at which the eye, previously kept in the dark for one hour, could only just distinguish the light emitted by the phosphorescent substance, taking care to test for illusions by the successive interposition of ground-glass screens. The time thus found was four hours, giving an amount of light of 29×10^{-9} standard candles at 1 m. If the eye is previously kept in the dark during varying periods, the minimum varies inversely as the square of the time during which it is kept dark.—On phosphorescent sulphide of zinc, considered as a photometric standard, by the same. Careful tests showed that the light emitted by zinc sulphide at a given instant is independent of the distance of the illuminating magnesium ribbon, of the time of illumination, and of the thickness of the layer, and is also uniform in samples prepared under different conditions, thus exhibiting all the requisites of a secondary photometric standard.—On an acid plato-nitrite of potassium, by M. M. Vèzes.—Decomposition of chloroform in presence of iodine, by M. A. Besson.—On some ethers of homopyrocatechine, by M. H. Cousin.—On the determination of phosphorus in iron and steel, by M. Adolphe Carnot. The new method, based like most others on the employment of ammonium molybdate, differs from them in the mode of separation of the silicon, which is effected by sulphuric acid; in the process of destruction of the carbon compounds, brought about by chromic acid; and in the nature of the final compound, which is not magnesium pyrophosphate, but dry phosphomolybdate of ammonia, which only contains 1·628 per cent. of phosphorus, thus ensuring a greater accuracy in the quantitative estimation.—Losses of nitrogen in manure, by MM. A. Muntz and A. Ch. Girard.—Researches on the localisation of the fatty oils in the germination of seeds, by M. Eugène Mesnard. It appears that, except in the grasses, the fatty oil is not specially localised. It is in all cases independent of the starch and the glucose, but it appears superposed upon the albuminoid materials in the reserves of ripe seeds.

BERLIN.

Physical Society, December 16, 1892.—Prof. Kundt, President, in the chair.—Dr. Lummer spoke on the principles involved in the use of half-shade polarimeters. He showed that the difference in brightness of the two halves of the field of the instrument depends first on the angle between the two polarising prisms, the less this is the greater being the difference produced by a minimal rotation of the analyzer, and secondly on the power of perceiving minute differences of brightness. In connection with the latter he had made some changes in the Lippich instrument which presented some distinct advantages.—Prof. Goldstein gave an account of some experiments made many years ago, but not yet published. He first dealt with the light which appears at the anode, and which, as compared with that of the kathode, has as yet been but little investigated. As is well known, a kathode consisting of two metals emits rays of different brightness from its two parts, thus for instance the aluminium emits brighter rays than does the silver. When this electrode is used as an anode, the reverse holds good, inasmuch as the anodic light of silver is brighter than that of aluminium. The difference is, however, only observed in rarefied oxygen, and does not exist in a hydrogen tube, and is hence due to oxidation of the silver. The second set of experiments dealt with Crookes' supposed reciprocatory deflection of kathodic rays of similar direction. The speaker had shown, by shielding one of the electrodes, that the deflection is apparent, not real. The change in the path of the kathodic radiation is due entirely to the effect of the second electrode upon the rays emitted by the first.

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